

# **Health Insurance and the Labor Supply Decisions of Older Workers: Evidence from the U.S. Department of Veterans Affairs**

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Updated for the 9th Annual Joint Conference of the Retirement Research Consortium  
“Challenges and Solutions for Retirement Security”  
August 9-10, 2007  
Washington, D.C.

The research reported herein was pursuant to a grant from the U.S. Social Security Administration (SSA) funded as part of the Retirement Research Consortium (RRC). The findings and conclusions expressed are solely those of the authors and do not represent the views of SSA, any agency of the Federal Government or the RRC. We are grateful to Elizabeth Ananat, David Autor, Dora Costa, Amy Finkelstein, Jon Gruber, Jim Poterba and participants in the MIT Public Finance lunch for helpful comments and suggestions and to Alejandro Bras, Erin Harrison, and Rebecca Willis for excellent research assistance. This research was also supported by National Institute on Aging Grant T32-AG00186. All remaining errors are our own.

## I. Introduction

Most existing programs in the U.S. cannot provide the kind of policy experiment needed in order to determine the effect of universal health insurance on the labor supply of older workers. In general, social insurance programs that increase income conditional on non-work, such as unemployment insurance (Coile and Gruber 2000) and disability insurance (Bound and Burkhauser 1999), have been found to decrease employment. The theoretical predictions, and the results of previous research are mixed, however, for the employment effect of government-provided health insurance programs. These programs are often structured so that they provide a mixture of income shocks, employment subsidies and/or taxes, and improvements in human capital (via health), leading to ambiguous net effects on labor supply.

First, national health insurance that is not linked to employment acts as a positive income shock for those with low earnings or high health costs, because it is paid for via taxes and the employed subsidize the not employed; theory implies that universal insurance will therefore decrease employment. Empirical evidence for Medicaid (Winkler 1991, Moffitt and Wolfe 1992, Yelowitz 1995, Meyer and Rosenbaum 2001, Borjas 2003), which is need-based and provides a positive income shock to those eligible for it, is nonetheless mixed. Studies find either no effect, small decreases, or large decreases in working based on the population studied and the methodology used.

In addition, other effects of universal insurance might lead to increases in labor supply. Gruber and Hanratty (1995) argue that the introduction of national health insurance may increase employment overall by improving health and therefore productivity. Meanwhile, evidence on insurance such as COBRA that de-links health insurance from employment (but does not increase income, since recipients must pay their health premiums) may increase productivity through enabling improved job matches.

Medicare is a health-care income shock that is not linked to employment, and so could shed light on questions of labor supply and health. Some studies (CITE) suggest that Medicare improves health, though evidence is mixed depending on the time period studied (Finkelstein and McKnight 2005). The empirical effects of Medicare on labor market outcomes, however, are difficult to disentangle from those of Social Security and

other programs linked to the Normal Retirement Age. Most papers on Medicare use dynamic programming or structural estimation to suggest that an expansion of Medicare will increase retirement (Rust and Phelan 1997, Blau and Gillespie 2003, Johnson et al. 2003, French and Jones 2004).

A unique opportunity to better understand the effects of universal coverage on older workers' employment has been provided by a major expansion in both the services offered and the population covered by the Department of Veterans Affairs health care system (VA), which occurred during the mid-1990s. This change allows us to study the effect of a program that provides an income shock and may have health effects for some recipients, but that is not linked to employment. The effects of this program change are probably most comparable to the effects of expanding Medicare to Americans under 65, a plan often proposed by politicians.

Examining this program expansion, we find that it decreases employment, increases retirement, and increases part-time work. In addition, it decreases self-employment, an outcome which is inconsistent with a job-lock story in which de-linking health care from employment should increase transitions from paid work to more flexible but uninsured self-employment but is consistent with the effect of an income shock in which the uninsured no longer need additional income to self-insure against adverse health shocks. Additionally, we find suggestive evidence that veterans from disadvantaged groups actually increase their employment outcomes, suggesting that for these groups the health effects of this insurance expansion allow people on the margin to continue working. Finally, we posit that health insurance may be one reason that retirement rates are higher in countries with national health insurance.

The paper is organized as follows: Section II describes the VA program in detail, Section III describes the dataset and empirical strategies, Section IV provides results, Section V discusses and provides implications and Section VI concludes.

## II. Description of VA program

Historically, the Department of Veterans Affairs (VA) health care system was a network of hospitals, established over 70 years ago for the purpose of providing specialty care to veterans with conditions resulting from their military service. Over time, the system was expanded to also include care for low-income veterans. VA provided mainly

inpatient care, with outpatient services for non-service-connected conditions available only as follow-up to an inpatient stay.

In 1996, the U.S. government began a major overhaul of this health care system. In an effort to catch up with progress in private-sector medicine, VA health care began a shift from an emphasis on hospital-based specialty services to a focus on primary care and preventive medicine. The total number of patients treated in VA hospitals dropped 44 percent between 1989 and 1999, while the total number of outpatient visits increased 66 percent over the same time period (Klein & Stockford, 2001). In addition to this change, VA's resource allocation system was redesigned. Following the HMO model, VA began distributing its health care budget using a capitated, patient-based formula.<sup>1</sup>

As a result of these changes, VA anticipated that increased efficiency would result in significant reductions in costs per patient and in necessary staff. With this in mind, VA felt that it would have the resources available to be accountable to the entire veteran population. VA therefore changed its rules on eligibility for care. Prior to the reform, VA guaranteed care only to veterans with service-connected conditions or low incomes; following the restructuring, all veterans became eligible for VA health care (GAO/T-HEHS-99-109). As a result of the changes in the system, VA's patient load increased from 2.5 million veterans in 1995 to 4.5 million in 2002 (CITE).

Boyle (2005) examines the impact of the VA overhaul on veterans' health care utilization and health outcomes. That study finds that between 35 and 70 percent of new VA health care users are individuals who drop private health insurance plans, something that may have been linked to their leaving full-time employment. In addition, she finds that while utilization of health care services increased, there were not net improvements in average veteran health, potentially because healthier veterans may crowd out sicker veterans.

The VA restructuring affects the availability of health care for the entire veteran population. For non-poor, non-disabled veterans, the policy change constitutes the introduction of a form of non-employer-provided health insurance that was previously unavailable. Even for the previously-eligible (i.e., low-income or disabled) segment of

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<sup>1</sup> In a capitated payment system, the health care provider is reimbursed a flat dollar amount for each patient regardless of the services provided.

the veteran population, this policy change results in a significant, exogenous change in health insurance status. The VA system following the reorganization became a health care provider much more similar to what was available in the private sector. Thus, even for previous users of VA care, the policy change resulted in the introduction of health care benefits that are much more substitutable for private care than anything provided under the old system. We therefore utilize this exogenous introduction of an outside health insurance option for U.S. veterans to estimate the impact of publicly provided health insurance on individuals' labor supply choices.

### III. Data and Empirics

We use data from the Census Bureau's March Current Population Survey (CPS) for the years 1992 through 2002. We utilize a difference-in-differences estimation strategy to compare the labor supply choices of veterans and non-veterans before and after the restructuring of VA health care. Because of the small number of female veterans we restrict our sample to include only males. Additionally, since we are interested in workers approaching retirement, we limit the sample to individuals ages 55 through 64<sup>2</sup>. With these restrictions, the treated population is therefore male veterans age 55 to 64, and the control group is male non-veterans in the same age group. Since changes in VA health care were implemented throughout 1996 and 1997, we define 1992-1995 as the pre-policy period and 1998-2002 as the post-policy period<sup>3</sup>.

The CPS allows us to study labor market outcomes such as retirement, labor force exit, and movement into part-time work or self-employment. In addition to information about employment in the current year, the survey questions individuals about their labor market participation in the previous year. In order to isolate the effect of the policy change on individuals' decisions to alter their labor market behavior, we restrict our sample to those who report working at least one week in the previous year.<sup>4</sup> We use a probit model to estimate the following equation:

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<sup>2</sup> Although it is not uncommon for individuals to continue work past age 64, eligibility for Medicare at age 65 will alter the impact of other public health insurance on the work decision.

<sup>3</sup> In January 2003, VA again revised the rules for obtaining health care. We therefore end our study period in 2002.

<sup>4</sup> This strategy is consistent with that used by Gruber and Madrian (1995). We find that restricting our sample to individuals who report working at least 10 weeks in the previous year produces very similar results. Regressions on the whole sample (i.e. including individuals that did not work in the previous year) also produce results that are qualitatively similar, although of smaller magnitude.

$$(1) \quad y_{it} = \beta_0 + \beta_1 \text{veteran}_i + \beta_2 \text{veteran}_i * \text{post}_t + \beta_3 \mathbf{X}_{it} + \delta_t + \mu_{it}$$

where:

$y_{it}$  = labor supply outcomes including: retired, not working, self-employed, working part time

$\text{veteran}_i = 1$  if individual has been honorably discharged from active military duty, 0 otherwise

$\text{post}_t = 1$  in the post-policy period, 0 otherwise

$\mathbf{X}_{it}$  = vector of individual characteristics: age, race, marital status, education, and state dummies, age \* veteran dummies, industry and occupation dummies, and indicators for employer-provided health insurance and pensions

$\delta_t$  = year dummies

and,

$\mu_{it}$  = a random error term.

Summary statistics are shown in Table 1. These statistics demonstrate that the veteran and non-veteran samples are reasonably comparable in the pre-period. The average veteran is slightly older, more educated, and slightly more likely to have employer provided health insurance than the average non-veteran. There is a somewhat different trend in average age for the two samples from the pre-period to the post-period – veterans on average grow older over the study period while non-veterans are slightly younger on average in the post-period. For this reason, we include an age\* veteran interaction term in the regressions, allowing age to enter separately for the two populations.<sup>5</sup> The age difference likely accounts for at least some of the average differences in labor supply outcomes between the two groups. Veterans are more likely to be retired or not working than the non-veterans in the pre-period sample.

#### IV. Results

##### A. Main Results

Our primary results are detailed in Tables 2 and 3. Reported coefficients for all regressions are probit marginal effects. All regressions are reported with and without controls for characteristics of the employer in the previous year. These characteristics include dummies for the industry and occupation of employment last year, as well as indicators for whether the individual received health insurance or a pension through his

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<sup>5</sup> One concern with our estimation strategy is the possibility of systematic differences between the treatment and control groups. For this reason, we have also run all reported regressions including veteran interaction terms for every control variable. When we allow all controls to enter for veterans and non-veterans separately, the coefficients on the veteran interactions are typically insignificant, and our coefficient of interest is virtually unchanged.

employer. Results are qualitatively similar with and without these controls, although the magnitude of the coefficient of interest (the coefficient on  $\text{veteran*post}$ ) varies slightly across the two specifications. In the remainder of the paper we discuss the regressions with the full set of controls.

As theory would predict, providing free health insurance outside of employment decreases full-time work for older workers and increases retirement. As a result of gaining VA coverage, the probability of working drops by 2.43 percentage points for an individual with average characteristics. Relative to the pre-period average, this is about a 10% increase in the probability that an older worker ceases work. The introduction of the VA health care benefit increases the probability of entering retirement for older workers by .38 percentage points, a 2.3% increase relative to the pre-period veteran average. While the magnitudes of these estimates are not particularly large, this is likely in part because while we measure the effect on the entire veteran population, only about a quarter of U.S. veterans actually enrolled in the VA system during our study period<sup>6</sup>. The effects are therefore likely to be diluted by the large number of veteran non-users, some of whom may have been unaware of their eligibility to use the VA system.

As reported in Table 3, our results also suggest an increase in the use of bridge jobs, which are jobs (often part-time) that people take after retiring from a main job (Ruhm 1994), with a positive interaction effect on our treatment for the part-time outcome. We estimate a 1.3 percentage point increase in the probability of working part-time, which is a 12.3% increase relative to the pre-period veteran average.

A story consistent with job-lock, or labor market stickiness caused by workers being afraid to change to a more productive employment match because they are afraid of losing health insurance, would also increase or have no effect on self-employment since people who preferred self-employment might be stuck with job-lock. An income shock, however, could decrease self-employment as people no longer need that income to self-insure (or pay for) health risks. In Table 3, columns (3) and (4), we find a negative effect

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<sup>6</sup> Any veteran wishing to use VA care must first sign-up for benefits or “enroll” in the system. During our study period, some veterans enrolled but did not actually subsequently use VA care. The fact that these individuals enrolled indicates awareness of their eligibility and a potential desire to access the system at a later point in time. It is not clear what proportions of unenrolled veterans are unaware of their eligibility, not interested in ever using VA care, or relying on the option of enrolling at a later date should they desire VA care.

of health insurance treatment on self-employment. We estimate a 1.0 percentage point decrease in the probability of self-employment, which is a 5% decrease relative to the pre-period veteran average. This result suggests that the option of outside health insurance acts as an income shock rather than solely as a decrease in job-lock.

However, it is important not to take these results as an indication that providing health insurance to these older workers is simply a productivity diminishing transfer to that group. There are potential distributional differences in how people are affected. Unmarried men in this age group are more likely in poor health than married (CITE). Additionally, being below the means test may be highly correlated with poor health (CITE). We find some positive work outcomes for these disadvantaged vets after they receive the health insurance offer. Table 6b provides results for single men. Single veterans are less likely to claim they are retired, less likely to be self-employed, and less likely to be working part-time as a result of the policy change. Table 5b provides results for those below the means test. Although the result is not significant at conventional levels, low-income veterans are less likely to be not working after the health insurance offer and expansion. A caution must be offered with the means test results; veterans below the means test already had access to VA health insurance, but as described earlier, this insurance was not comprehensive. Nevertheless, combined, these results are consistent with a situation in which increased medical care for more economically disadvantaged groups leads to health improvements and a corresponding increase in the ability to work. This result fits in with some Medicaid literature that find health increases and positive labor market effects from Medicaid among the poorest populations (Currie and Gruber 1996, 2001, Moffit and Wolfe 2002).

#### B. Robustness Checks

In interpreting our results, we have assumed that the differential changes in veteran labor supply are directly attributable to the acquisition of public health insurance. This causal interpretation is legitimate as long as no pre-existing veteran-specific trend exists. We therefore must ensure that veteran and non-veteran labor market outcomes do not move relative to one another as a result of unobservables that are unrelated to VA policy. In order to confirm that the changes in veteran labor supply actually result from gaining access to public health care, we check for pre-existing trends by estimating the



same difference-in-differences regressions on pre-policy data. We choose the years 1992-1995 because this is a period when no major changes took place in the VHA. We code the years 1992 and 1993 as the “pre” years, and 1994 and 1995 as “post” years. In Table 4, we present a set of specification checks for the results reported in Tables 2 and 3. These falsification tests reveal no pre-existing trend in veterans’ labor supply choices relative to their non-veteran counterparts. The coefficient of interest (post\*veteran) in these regressions is consistently small and statistically insignificant at standard levels. In the single case where the pre-trend is significant at the 10% level, the sign on this coefficient is the opposite of what we find in our main results and the magnitude is small.

Another worry is that even with a differences-in-differences strategy controlling for differences, there may be systematic differences between veterans and non-veterans. When we allow all controls to enter for veterans and non-veterans separately, the coefficients on the veteran interactions are typically insignificant, and our coefficient of interest is virtually unchanged. Additionally, in a specification not reported, we use propensity score matching to match veterans and non-veterans on observable characteristics. Using this strategy also provides results that are qualitatively the same and quantitatively very similar.

As discussed above, certain veterans were eligible for VA health care prior to the policy change. Previously-eligibles (those with service-connected disabilities or low incomes) still have the potential to be affected by the change, since the types of health services available became much more comparable to those covered by employer-provided health insurance. Even so, we would expect to see stronger effects of the policy change on newly-eligible veterans, who go from having no outside insurance to full coverage under the public program. In Table 5, we report results for regressions run on individuals whose household income in the previous year was above or below the VA-established means test cutoffs. All regressions include controls for employer characteristics. In general, we find stronger results for the group above the means test. None of the results are significant for the below means test group, the coefficients for not working and self employed outcomes switch signs from the main results, the coefficient for the part-time outcome is attenuated, and although the magnitude on the coefficient for the retired result is larger for the below means test, it is only significant at the 10% level.

Caution should be taken in interpreting these results, however, as the sample size for the below means test group is less than a fifth of the above means test.

Finally, VA health care covers only the veteran and not the veteran's spouse or dependents.<sup>7</sup> For this reason, married veterans whose wives have access to employer-provided health insurance may not experience increased labor mobility as a result of receiving this insurance if their spouses depend on health insurance provided through the veteran's employer.<sup>8</sup> As reported in Table 7, the coefficients of *veteran\*post* for those whose wives have this insurance is of larger magnitude than for those whose wives do not, although only the results for part-time are significantly different from each other. The magnitudes are as expected. The results may not be significantly different because the effect is clouded by the fact that wives with health insurance of their own often must continue working to keep that health insurance and the decision to retire is often jointly determined between husband and wife (Coile 2003).

## V. Implications and Discussion

In order to be able to compare the labor market effects of this insurance shock to other social insurance receipt, we calculate elasticities. In order to calculate these elasticities, we make a number of assumptions. We estimate the value of VA insurance to be equivalent to the single coverage health insurance premium for workers in 2002, or \$3270.60<sup>9</sup> multiplied by 102% since COBRA allows the employer to charge individuals 102% of these costs in order to cover administrative fees, giving a value of \$3336. In future work, we hope to get the actual average COBRA continuation cost for workers age 55-64 in 1998-2002, as that should be closest to the premium faced by our sample. The average income of full-time workers in 2002 in our sample, dropping those with negative income, is \$59,913.62. By this calculation, VA provides an income shock equivalent to  $(3336/59913.62)=.06$  or 6% of the average individual's income.

We find that individuals are 10% more likely to be not-working as a result of gaining VA coverage, implying a non-participation elasticity of 1.67, this is more elastic

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<sup>7</sup> In cases where the veteran is catastrophically disabled or dies as a result of military service, the spouse and other dependents do become eligible for VA care under the CHAMPVA program. This is not relevant in our study, however, as catastrophically disabled veterans will not be in the work force.

<sup>8</sup> 57% of veterans in the sample have wives who are employed.

<sup>9</sup> According to *National Compensation Survey: Employee Benefits in Private Industry in the United States, 2002-2003*, U.S. Dept. of Labor and Bureau of Labor Statistics.

than the result of .6 found for Social Security (Coile and Gruber 2000) and the range of .63 to .81 found for Disability Insurance (Chen and van der Klaauw 2004). Individuals are 2.3% more likely to label themselves as “retired” as a result of gaining VA coverage providing an elasticity of .38. They are 12.3% more likely to report working part-time as a result of gaining VA coverage providing an elasticity of 2.05. Finally, they are 5% less likely to be self employed; this provides an elasticity of -.833.

[Enter comparison to other countries once we have the retirement rate for people in these age groups in other countries here]

As final cautions, these results do not prove that offering health insurance will decrease employment overall. We are only looking at the effects on men close to the ends of their full-time work-lives. Social norms may keep prime-aged males in the labor force regardless of the offer of outside health insurance.<sup>10</sup> Indeed, our results are not inconsistent with Gruber and Hanratty (1995) that find that employment goes up with the introduction of national health insurance. Productivity may increase overall since this insurance allows the unhealthy to cut down on full-time work which may be optimal. Given that current US labor market laws protecting older workers reduce job separations for older men (Lahey 2007), insurance may encourage older workers who are less happy with their jobs to retire and be replaced by less-experienced (and thus less costly under an assumption of Lazear contracts) and potentially more productive matches. Additionally as discussed above, health insurance may improve the productivity of the unhealthy on the margins of working by increasing their health capital.

## VI. Conclusion

In conclusion, we find that providing free comprehensive health insurance outside of employment decreases full-time work for older workers and increases the use of bridge jobs. Our results are consistent with the income effect of newly provided insurance decreasing work, rather than through non-employer linked insurance increasing job mobility. To the extent that younger workers subsidize national health insurance for older workers, the income effect from universal coverage may be a reason that non-employment is

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<sup>10</sup> Regressions on prime-aged males could not be presented utilizing our strategy because these data show a pre-trend between veteran and non-veteran populations.

higher for older people in countries with national health coverage. However, lower employment in these groups may not be a bad thing to the extent that it allows for more productive sorting into work and retirement.

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**Table 1. Summary Statistics, CPS 1992-2002\***

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	<b><u>Veterans</u></b>		<b><u>Non-Veterans</u></b>	
	<b>Pre</b> (N=7684)	<b>Post</b> (N=8150)	<b>Pre</b> (N=6195)	<b>Post</b> (N=10692)
Age	59.364	58.849	58.474	58.652
Married	0.812	0.804	0.803	0.791
White	0.934	0.913	0.852	0.851
No HS	0.144	0.063	0.294	0.209
HS	0.352	0.352	0.309	0.299
Some College	0.238	0.295	0.151	0.178
College Grad	0.160	0.172	0.112	0.152
Grad School	0.107	0.118	0.134	0.163
Pension Plan	0.429	0.486	0.404	0.440
Empl. HI Plan	0.627	0.652	0.581	0.595
Northeast	0.238	0.221	0.262	0.229
Midwest	0.260	0.249	0.248	0.234
South	0.289	0.281	0.294	0.306
West	0.213	0.281	0.196	0.231
Not Working	0.250	0.225	0.229	0.198
Retired**	0.163	0.148	0.120	0.106
Self-Employed	0.201	0.166	0.209	0.192
Part Time	0.106	0.104	0.093	0.090
<b>Occupations:</b>				
Prof/Management	0.259	0.280	0.256	0.298
Tech/Sales/Cleric	0.186	0.187	0.148	0.148
Service	0.070	0.074	0.087	0.081
Farming	0.046	0.032	0.064	0.051
Craftsman	0.145	0.149	0.138	0.136
Operator	0.144	0.140	0.168	0.161
<b>Industries:</b>				
Agric/Mining	0.045	0.034	0.060	0.052
Construction	0.068	0.070	0.078	0.082
Manufacturing	0.160	0.147	0.182	0.151
Transport/Commun	0.078	0.098	0.064	0.067
Trade	0.137	0.125	0.149	0.131
Finance/Real estate	0.051	0.053	0.045	0.050
Business/Repair	0.045	0.051	0.044	0.055
Personal	0.032	0.028	0.030	0.037
Public	0.051	0.065	0.032	0.033
Professional	0.140	0.133	0.142	0.173

\*Sample includes males ages 55-64 and employed last year

\*\*Number of observations for Retired is 3628 for pre veterans and 3196 for pre non-veterans because the variable does not exist in 1992-1993

**Table 2. Not Working and Retired Outcomes**

	(1)	(2)	(3)	(4)
	Not Working	Not Working	Retired	Retired
veteran	0.0281 (0.0224)	0.0125 (0.0164)	0.0199+ (0.0106)	0.0123** (0.0038)
<b>veteranpost</b>	<b>0.0184**</b> <b>(0.0061)</b>	<b>0.0243**</b> <b>(0.0042)</b>	<b>0.0141**</b> <b>(0.0032)</b>	<b>0.0038**</b> <b>(0.0012)</b>
married	0.1099** (0.0036)	0.0251** (0.0046)	0.0763** (0.0039)	0.0143** (0.0026)
nonwhite	0.0494** (0.0067)	0.0224** (0.0067)	0.0172* (0.0073)	-0.0018 (0.0035)
pensincl2		-0.1327** (0.0055)		-0.0386** (0.0025)
hiemp2		-0.0374** (0.0082)		0.0165** (0.0015)
Observations	32721	32721	25666	25666

Coefficients estimates are taken from a probit regression of veteran and veteran x post as described in eq. (1). Marginal effects are reported. Regressions include age, age\*veteran, state, year and education dummies and a constant. Robust standard errors in parentheses are clustered on veteran and year. Regression universe is restricted to men who were employed at least one week in the year prior to the survey year.

+ significant at 10%; \* significant at 5%; \*\* significant at 1%

**Table 3. Part-Time and Self-Employed Outcomes**

	(1)	(2)	(3)	(4)
	Part Time	Part Time	Self Employed	Self Employed
veteran	-0.0022 (0.0138)	-0.0024 (0.0121)	-0.0164 (0.0196)	-0.0020 (0.0189)
<b>veteranpost</b>	<b>0.0100*</b> <b>(0.0043)</b>	<b>0.0125**</b> <b>(0.0041)</b>	<b>-0.0143**</b> <b>(0.0043)</b>	<b>-0.0102**</b> <b>(0.0035)</b>
married	-0.0252** (0.0051)	-0.0162** (0.0042)	0.0152** (0.0048)	0.0199** (0.0048)
nonwhite	-0.0102 (0.0073)	-0.0155* (0.0064)	-0.0730** (0.0092)	-0.0507** (0.0072)
penplan2		-0.0408** (0.0036)		
hiemp2		-0.0485** (0.0045)		-0.1235** (0.0052)
pensincl2				-0.1229** (0.0048)
Observations	23978	23978	32721	31250

Coefficients estimates are taken from a probit regression of veteran and veteran x post as described in eq. (1). Marginal effects are reported. Regressions include age, age\*veteran, state, year and education dummies and a constant. Robust standard errors in parentheses are clustered on veteran and year. Regression universe is restricted to men who were employed at least one week in the year prior to the survey year.

+ significant at 10%; \* significant at 5%; \*\* significant at 1%



**Table 4. Specification Checks: “Pre” = 1992-1993, “Post” = 1994-1995**

	(1)	(2)	(3)	(4)	(5)	(6)
	Not Working	Not Working	Self Employed	Self Employed	Part Time	Part Time
veteran	0.0334 (0.0358)	0.0169 (0.0238)	-0.0484 (0.0297)	-0.0145 (0.0299)	0.0308** (0.0098)	0.0264** (0.0099)
<b>veteranpost</b>	<b>-0.0162+</b> <b>(0.0096)</b>	<b>-0.0020</b> <b>(0.0085)</b>	<b>0.0003</b> <b>(0.0081)</b>	<b>-0.0066</b> <b>(0.0070)</b>	<b>0.0026</b> <b>(0.0063)</b>	<b>0.0027</b> <b>(0.0063)</b>
married	0.1188** (0.0045)	0.0345** (0.0081)	0.0085 (0.0067)	0.0215* (0.0092)	-0.0376** (0.0088)	-0.0199** (0.0064)
nonwhite	0.0515** (0.0093)	0.0243 (0.0149)	-0.0946** (0.0103)	-0.0638** (0.0066)	-0.0072 (0.0096)	-0.0130 (0.0082)
pensincl2		-0.1494** (0.0080)		-0.1224** (0.0048)		
hiemp2		-0.0429** (0.0119)		-0.1308** (0.0108)		-0.0469** (0.0073)
penplan2						-0.0451** (0.0050)
Observations	13879	13879	13879	13292	13750	13750

Coefficients estimates are taken from a probit regression of veteran and veteran x post as described in eq. (1). Marginal effects are reported. Regressions include age, age\*veteran, state, year and education dummies and a constant. Robust standard errors in parentheses are clustered on veteran and year. Regression universe is restricted to men who are currently employed in the survey year.

+ significant at 10%; \* significant at 5%; \*\* significant at 1%

**Table 5. Results by Estimated Means Test Cutoff**

<b>a. Above Means Test</b>				
	(1)	(2)	(3)	(4)
	Not Working	Retired	Self Employed	Part Time
veteran	0.0104 (0.0157)	0.0110** (0.0042)	-0.0033 (0.0182)	-0.0083 (0.0110)
<b>veteranpost</b>	<b>0.0275**</b> <b>(0.0042)</b>	<b>0.0043**</b> <b>(0.0012)</b>	<b>-0.0113**</b> <b>(0.0028)</b>	<b>0.0112**</b> <b>(0.0035)</b>
pensincl2	-0.1142** (0.0052)	-0.0384** (0.0028)	-0.1285** (0.0047)	
hiemp2	-0.0231** (0.0068)	0.0130** (0.0018)	-0.1279** (0.0059)	-0.0325** (0.0042)
penplan2				-0.0348** (0.0039)
Observations	27677	21781	26281	21066

  

<b>b. Below Means Test</b>				
	(1)	(2)	(3)	(4)
	Not Working	Retired	Self Employed	Part Time
veteran	0.0242 (0.0568)	0.0203 (0.0155)	0.0162 (0.0383)	0.0711 (0.0634)
<b>veteranpost</b>	<b>-0.0189</b> <b>(0.0205)</b>	<b>0.0069+</b> <b>(0.0038)</b>	<b>0.0005</b> <b>(0.0095)</b>	<b>0.0102</b> <b>(0.0220)</b>
pensincl2	-0.1588** (0.0236)	-0.0239** (0.0061)	-0.0738** (0.0124)	
hiemp2	-0.0627* (0.0252)	0.0338** (0.0081)	-0.1006** (0.0076)	-0.1222** (0.0182)
penplan2				-0.0362* (0.0164)
Joint Sig?	Yes	No	No	No
Observations	5044	3885	4969	2912

Coefficients estimates are taken from a probit regression of veteran and veteran x post as described in eq. (1). Marginal effects are reported. The regression universe in panel (a) is restricted to those persons who are above the income means test (given number of children under the age of 18) needed to meet the VA requirement prior to the reform. The regression universe in panel (b) is restricted to those below the same income means test. Columns (1) - (5) are restricted to those who worked at least one week in the year prior to the survey. Columns (6) - (7) are restricted to those currently employed in the survey year. Regressions include age, age\*veteran, state, year and education dummies and a constant. Robust standard errors in parentheses are clustered on veteran and year. "Joint Sig?" reports whether the veteran\*post coefficients for the two populations are statistically significantly different from one another at the 5% level.

+ significant at 10%; \* significant at 5%; \*\* significant at 1%

**Table 6. Results By Marital Status**

**a. Married**

	(1)	(2)	(3)	(4)
	Not Working	Retired	Self Employed	Part Time
veteran	0.0039 (0.0216)	0.0122* (0.0052)	-0.0062 (0.0206)	-0.0201 (0.0139)
<b>veteranpost</b>	<b>0.0325**</b> <b>(0.0042)</b>	<b>0.0079**</b> <b>(0.0015)</b>	<b>-0.0032</b> <b>(0.0051)</b>	<b>0.0224**</b> <b>(0.0046)</b>
Observations	26221	20528	25049	18704

**b. Single**

	(1)	(2)	(3)	(4)
	Not Working	Retired	Self Employed	Part Time
veteran	0.0228 (0.0234)	0.0050 (0.0074)	0.0271 (0.0234)	0.0643** (0.0232)
<b>veteranpost</b>	<b>0.0055</b> <b>(0.0070)</b>	<b>-0.0061*</b> <b>(0.0026)</b>	<b>-0.0278**</b> <b>(0.0072)</b>	<b>-0.0209*</b> <b>(0.0087)</b>
Joint Sig?	Yes	Yes	Yes	Yes
Observations	6500	5138	6201	5274

Coefficients estimates are taken from a probit regression of veteran and veteran x post as described in eq. (1). Marginal effects are reported. The regression universe in panel a is restricted to married men. The universe in panel b is restricted to not married men. Columns (1) - (5) are restricted to those who worked at least one week last year. Columns (6) - (7) are restricted to those currently employed. Regressions include age, age\*veteran, state, year and education dummies and a constant. Robust standard errors in parentheses are clustered on veteran and year. "Joint Sig?" reports whether the veteran\*post coefficients for the two populations are statistically significantly different from one another at the 5% level.

+ significant at 10%; \* significant at 5%; \*\* significant at 1%

**Table 7. Results by Wife's Health Insurance Status**

**a. Wife Has Employer-Provided Health Insurance**

	(1)	(2)	(3)	(4)
	Not Working	Retired	Self Employed	Part Time
veteran	-0.0113 (0.0267)	0.0249+ (0.0130)	-0.0030 (0.0043)	-0.0378+ (0.0224)
<b>veteranpost</b>	<b>0.0510**</b> <b>(0.0070)</b>	<b>0.0063</b> <b>(0.0054)</b>	<b>-0.0029**</b> <b>(0.0009)</b>	<b>0.0361**</b> <b>(0.0081)</b>
Observations	12603	12603	10983	8673

**b. Wife Without Employer-Provided Health Insurance**

	(1)	(2)	(3)	(4)
	Not Working	Retired	Self Employed	Part Time
veteran	0.0215 (0.0262)	0.0195** (0.0071)	-0.0029 (0.0061)	-0.0030 (0.0160)
<b>veteranpost</b>	<b>0.0186*</b> <b>(0.0087)</b>	<b>-0.0012</b> <b>(0.0025)</b>	<b>-0.0029*</b> <b>(0.0014)</b>	<b>0.0140*</b> <b>(0.0056)</b>
Joint Sig?	No	No	No	Yes
Observations	13197	13197	10955	9672