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Abstract

This paper exploits a major mid-1990s expansion in the U.S. Department of Veterans Affairs health care system to provide evidence on the labor market effects of expanding health insurance availability. Using data from the Current Population Survey, we compare the labor market behavior of older veterans and non-veterans before and after the VA health benefits expansion to test the impact of public health insurance on labor supply. We find that older workers are significantly more likely to decrease work both on the extensive and intensive margins after receiving access to non-employer based insurance. Older workers are also more likely to leave self-employment, a result inconsistent with "job-lock" effects of employer-based insurance, but consistent with a positive income effect from new access to public insurance. Some relatively disadvantaged subpopulations, however, may increase their labor supply after gaining greater access to public insurance, consistent with complementary positive health effects of health care access for these groups. We conclude that this reform has affected employment and retirement decisions, and suggest that future moves toward universal coverage or expansions of Medicare are likely to have significant labor market effects. To illustrate, we calculate that as much as 10% of the difference in retirement rates in the US and Canada may be due to Canada's provision of universal health care.

JEL Classification Codes: J2, I18

Keywords: labor supply, job-lock, retirement, older workers, health insurance, VA, Medicare, veteran

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I. Introduction

Retirement policy formation requires a clear understanding of the factors influencing the labor force participation of older individuals. Reforms encouraging work at later ages may prove ineffective if public health insurance incentives interfere. Economic theory predicts that public health insurance may affect job choice, income, and health. However, the magnitude and direction of the net effects of public provision on labor supply are ex ante ambiguous. Moreover, even where theory makes a clear prediction of the effect, empirical evidence has not always supported it. In this paper, we use new evidence from the United States Department of Veterans Affairs to estimate the labor market effects of expanding public health insurance availability. By examining a health insurance expansion that is tied neither to employment nor to other public programs, we isolate the impact of an insurance offer on labor supply for older workers.

Previous research examining the relationship between public health care and work behavior has not provided clear answers to this question. For example, government-provided health insurance that is not linked to employment acts as a positive income transfer for those with low earnings or high health costs because it is paid for via taxes, and the employed subsidize the not-employed. Theory therefore implies that universal insurance will likely decrease employment for these individuals. Empirical evidence for Medicaid, however, which not only is not conditional on employment but also is means-tested and therefore taxes earnings, is mixed (Winkler 1991, Moffitt and Wolfe 1992, Yelowitz 1995, Meyer and Rosenbaum 2001, Borjas 2003). Depending on the population studied and the methodology used, studies find a range of outcomes, from large decreases in working to no change at all.

Adding to the theoretical complexity, other effects of government-provided insurance might lead to increases in labor supply or labor productivity. Health insurance may increase employment overall by improving health, which may also result in increased labor productivity. In line with this prediction, Gruber and Hanratty (1995) find that employment increased in Canada after the introduction of national health insurance. Additionally, studies examining the introduction of the U.S. continuation-of-coverage mandates, such as COBRA (e.g. Gruber and Madrian 1995), find resulting increases in job switching. By de-linking health insurance and employment (but not increasing income, since recipients must pay their own health premiums), these mandates may increase productivity not only by improving health but by enabling improved job matches, that is, reducing "job-lock".[1]

Most existing programs in the U.S. cannot provide the kind of policy experiment needed to distinguish the effects of expanding 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. However, the theoretical predictions and the results of previous research are mixed for the employment effect of government-provided health insurance programs. These programs are often structured so that they provide a mixture of income transfers, employment subsidies and/or taxes, and improvements in human capital (via health), leading to ambiguous net effects on labor supply.

Medicare is a health-care income transfer that is not linked to employment, and so could shed light on the relationship between labor supply and health. Some studies

[1] For more information on job-lock, see Gruber and Madrian (2002).

(Lichtenberg 2002) 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 that study the Medicare-work relationship use dynamic programming or structural estimation to suggest that an expansion of Medicare will increase retirement (Rust and Phelan 1997, Blau and Gilleskie 2003, Johnson et al. 2003, French and Jones 2004).

A unique opportunity to better understand the effects of universal coverage on older workers' employment is provided by a major mid-1990s expansion in both the services offered and the population covered by the Department of Veterans Affairs health care system (VA). This change allows us to study the labor supply impact of a program that provides an income transfer and may have health effects for some recipients, but that is not tied to employment or income, and is not bundled with other program changes. From a policy standpoint, the effects of this program change are likely comparable to the effects of expanding Medicare to Americans under age 65, a plan often proposed by politicians.

We find that the VA expansion decreases employment, increases retirement, and increases part-time work among older recipients. In addition, it results in a drop in self-employment. This outcome is inconsistent with a job-lock reduction (in which de-linking health care from employment would increase transitions from paid work to more flexible but uninsured self-employment), but is consistent with the effect of an income transfer (in which recipients no longer need either the insurance or the earnings from employment to protect against adverse health shocks). Additionally, we find suggestive evidence that

veterans from disadvantaged groups actually increase their labor supply as a result of gaining public insurance, implying that for these groups, health improvements from this insurance expansion complement work. 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 provides a theoretical background for the effects of health insurance on employment, Section III describes the VA program in detail, Section IV describes the dataset and empirical strategies, Section V provides results, Section VI discusses and provides implications and Section VII concludes.

II. Predicted Effects

The impact of VA health insurance on labor supply is theoretically ambiguous. On the one hand, an offer of public health insurance acts as an income transfer. With higher income but the same underlying wage rate, theory predicts that on average, labor hours will fall. Some workers may move from full- to part-time work, because they no longer need the income to pay for insurance premiums or out-of-pocket medical costs, and thus substitute leisure for work. Similarly, in response to the income transfer, workers may drop out of the labor force entirely, either temporarily or permanently (i.e. earlier retirement). Finally, the income transfer would potentially lead to a movement out of self-employment, as individuals who were previously working in order to pay for health costs out-of-pocket will no longer need to do so.

Along with acting as an income transfer, the offer of public health insurance should reduce job-lock. Workers are no longer reliant on their employers for insurance coverage, and thus fluidity in the labor market should increase. Workers have the

flexibility to change to job positions offering higher wages but lower benefits, and more productive employer-employee matches may result. Older workers who are no longer job-locked because of insurance coverage will have the option of retiring earlier or transitioning to retirement by moving to part-time work without benefits. Workers who prefer self-employment but were previously unable to afford insurance in the non-group market or payment of health costs out-of-pocket now have the flexibility to become self-employed. Thus, the reduction in job-lock may lead to an increase in self-employment that runs counter to the decrease caused by the income transfer.

While both a job-lock reduction and an increase in underlying wealth due to an income transfer would predict a drop in overall labor hours on average, it is also theoretically possible that labor supply will increase for some groups. An uninsured (or inadequately insured) worker with a chronic health condition that may previously have forced him out of the labor force may be able to continue working if the newly-acquired insurance improves his health. The addition of health insurance may also allow workers on the margin of applying for SSDI (and receiving Medicare after two years) to stay in the labor force.[2] Hence, labor supply might increase for some groups after the expansion.[3]

III. 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

[2] Thanks to David Autor for recognizing this possibility.
[3] Note that an increase in health is equivalent to an increase in the relative wage since work is no longer as painful. As such, for groups not at the margin, the attractiveness of leisure may increase and hours may go down.

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.[4]

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 2.6 million veterans in 1995 to 4.3 million in 2002 (GAO/T-HEHS-96-134, GAO-03-1103R).

[4] In a capitated payment system, the health care provider is reimbursed a flat dollar amount for each patient regardless of the services provided.

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 who were previously the focus of VA care.

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 the veteran population, this policy change results in a significant, exogenous change in health insurance status because the reorganized VA is 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.

IV. 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 during this time, we restrict our sample to include only males. Additionally, since we are interested in workers on the margin of not working (i.e. approaching retirement), we limit the sample to individuals ages 55 through 64[5]. 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[6].

The CPS allows us to study labor market outcomes such as labor force or employment exit, retirement, 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.[7] We use a probit model[8] to estimate the following equation:

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[5] 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.

[6] In January 2003, VA again revised the rules for obtaining health care. We therefore end our study period in 2002. Due to concern that particular Vietnam Era veterans are affected by a 2002 change that allowed diabetes to be considered a war-related injury for veterans who may have been exposed to Agent Orange (Duggan, Rosenheck and Singleton 2006; Autor and Duggan 2007) we have also estimated all equations restricting our post-period to 1998-2001. Results of this exercise for the *not-working* outcome are shown in Table 5, Column (3). Coefficent magnitudes are nearly identical when 2002 observations are removed from the dataset and significance does not change.

[7] 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, as shown in Table 5, Column (5). 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.

[8] We have also estimated the model using multinomial logit to account for the fact that individuals choose among multiple alternative employment scenarios in the post-period. Marginal effects corresponding to

$$(1) \qquad y_{it} = \beta_0 + \beta_1 veteran_i + \beta_2 veteran_i * post_t + \beta_3 \mathbf{X}_{it} + \delta_t + \mu_{it}$$

The dependent variable, y_{it}, includes indicator variables for labor supply outcomes including retired, not working, self-employed, and working part-time. The variable *not working* is 0 if the individual is employed and 1 otherwise. The *retired* variable is self-reported retirement and is not available prior to 1994; retirement regressions are therefore limited to the years 1994-1995 in the pre-period.[9] Several part-time variables were tested. The part-time variable reported is coded as 1 if the number of hours worked is between 0 and 35 hours, and 0 if the individual works more than 35 hours. Alternate specifications for part-time provide similar results. *Self-employed* is an indicator that is equal to 1 if the class of worker is self-employed (either incorporated or not incorporated) and 0 otherwise.

Among the independent variables, $veteran_i$ is a dummy equal to 1 if the individual has been honorably discharged from active military duty, $post_t$ is a dummy equal to 1 in the post-policy period, $\boldsymbol{X}_{it}$ is a vector of individual characteristics including age, race, marital status, education, state dummies, industry and occupation dummies, and indicators for employer-provided health insurance and pensions and δ_t is a full set of year dummies. Part-time regressions include an indicator of whether or not the employer offers a pension; all other regressions include an indicator of whether or not the

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this estimation technique are very similar to the reported probit marginals, as shown in Table 5, Column (6).

[9] Self-reported retirement is not exclusive of working and often includes individuals who are still working full or part-time but no longer in a career job, still working at the career job but receiving retirement benefits, or involuntarily unemployed but over some age threshold, among other possibilities. Unemployed older workers hoping to return to the labor force may or may not claim to be retired. For more discussion of definitions of retirement see for example Gustman and Steinmeier (1995) or Araiza (2004). Results for the *not-working* outcome limited to the same years as the *retired* outcome produce nearly identical results to those in the full sample, as shown in Table 5, Column (4).

individual is included in the pension plan.[10] Standard errors are adjusted for non-independence of the errors within the veteran*year group.[11]

Summary statistics are reported in Table 1. These statistics demonstrate that the veteran and non-veteran samples are reasonably comparable in the pre-period. The average veteran is more educated, and slightly more likely to have employer-provided health insurance than the average non-veteran. Veterans are more likely to be retired or not working than non-veterans in the pre-period sample. [12]

V. Results

A. Main Results

Our primary results are detailed in Tables 2 and 3. Reported coefficients for all regressions are probit marginal effects. These regressions estimate equation (1) and are reported with and without controls for characteristics of the employer in the previous year. Results are qualitatively similar with and without these controls, although the magnitude of the coefficient of interest (the coefficient on *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 work for older workers and increases retirement. As a result of gaining VA

[10] There is no consistent variable indicating whether or not a firm offers health insurance, so regressions include an indicator for whether or not an individual is included in a health insurance plan.
[11] Results are robust to different clustering specifications including no clusters, age*year clusters, and vet*age*year clusters.
[12] 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. This is demonstrated in Table 5, Column (1).

coverage, the probability of working drops by 2.47 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 .40 percentage points, a 2.5% 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.[13] The effects are therefore likely to be attenuated 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 2, our results also suggest an increase in the use of bridge jobs, which are jobs (often part-time) that people transition to after retiring from a main job (Ruhm 1994). We estimate a 1.24 percentage point increase in the probability of working part-time, which is an 11.7% increase relative to the pre-period veteran average.

We also examine the effect of public insurance receipt on the probability of self-employment. A story consistent with "job-lock," or labor market stickiness caused by workers' reluctance to change jobs because they are afraid of losing health insurance, would predict an increase in (or at least no effect on) self-employment. This is because prior to gaining public insurance, some individuals who preferred self-employment might have remained in a current full-time employment situation in order to retain health

[13] 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.

benefits. On the other hand, since the public insurance is an income transfer for beneficiaries, the program could decrease self-employment as people potentially no longer need the extra income to self-insure (or pay for) health risks. In Table 2, columns (7) and (8), we find a negative effect of health insurance receipt 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 income transfer effect dominates any reduction 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; in particular, those in poor health might be more likely to see their health improve and their labor supply increase as a result of the health care expansion. To test for this possibility, we separately examine the outcomes for subgroups who typically exhibit worse health than average. For example, unmarried men in this age group are more likely to be in poor health than married men (Lillard and Panis 1996). Additionally, being below the means test may be highly correlated with poor health (Kiuila and Mieszkowski 2007). We find some positive work outcomes for these disadvantaged (i.e. unmarried or low-income) vets after they receive the health insurance offer. The first panel of Table 3 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. The second panel of Table 3 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 is consistent with some Medicaid literature that finds health increases and positive labor market effects from Medicaid among the poorest populations (Currie and Gruber 1996, 2001, Moffitt and Wolfe 2002). Additionally, we may be underestimating these effects because although some conditions will improve immediately with treatment, some conditions may take time to show improvement.

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 prior 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. Confirming this lack of movement establishes that changes in veteran labor supply actually result from gaining access to public health care.

To test for pre-existing trends that differ between veterans and non-veterans, we estimate 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 (*veteran*post*) in these regressions is consistently small and statistically insignificant at standard levels. In the single case where the coefficient of interest 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.[14]

Another concern is that there may be systematic differences between veterans and non-veterans that change over time. 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, as demonstrated in Table 5, Column (1). Additionally, we use propensity score matching to draw veteran and non-veteran samples that are comparable based on observable characteristics. Results for the *not working* outcome are shown in Table 5, Column (2). This strategy also produces results that are qualitatively the same and quantitatively very similar to those in our main regressions.

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

[14] The self-reported retirement variable does not exist prior to 1994 so we cannot run this falsification check for that outcome.

change on newly-eligible veterans, who go from having no outside insurance to full coverage under the public program. Consistent with this expectation, in Table 3 we report results for individuals whose household income in the previous year was above the VA-established means test cutoff and find that they are generally stronger than results for the group below the means test. Moreover, the coefficients for the *not working* and *self-employed* outcomes actually switch signs from the main results. These differences support the conclusion that the behavioral changes consistent with an income effect from newly available insurance are concentrated among those for whom it is, in fact, newly available.

Finally, VA health care covers only the veteran and not the veteran's spouse or dependents.[15] For this reason, the income shock is relatively smaller for married veterans whose spouses depend on health insurance provided through the veteran's employer — they still lose insurance coverage for their wives if they reduce their labor supply. As reported in the third panel of Table 3, the coefficients on *veteran*post* in predicting non-work, retirement, and part-time work for those whose wives have insurance through their own employers (i.e. do not depend on their husbands' employer-provided insurance)[16] are of larger magnitude than for those whose wives do not. Thus, the magnitudes are as expected, although only the results for the part-time outcome are significantly different across the two groups.[17]

[15] 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.
[16] 57% of veterans in the sample have wives who are employed.
[17] 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).

VI. Implications and Discussion

To facilitate a comparison of the labor market effects of this insurance transfer to other changes in social insurance, we calculate labor supply elasticities. To do so, we must make several assumptions. First, we estimate the value of VA insurance to be equivalent to the single-coverage health insurance premium for workers in 2002, or $3270.60[18] multiplied by 102% (since COBRA allows employers to charge individuals 102% of these costs in order to cover administrative fees), giving a value of $3336. 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 transfer 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 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 2007). Individuals are 2.5% more likely to label themselves as "retired" as a result of gaining VA coverage, implying an elasticity of .42. They are 11.7% more likely to report working part-time as a result of gaining VA coverage, which corresponds to an elasticity of 1.95. Finally, they are 5% less likely to be self employed; this implies an elasticity of -.833.

Our methodology can also be used to make back-of-the-envelope comparisons about the likely effect of national health insurance on employment for this age group. If, instead of using the hazard rate (that is, instead of limiting to people who worked in the previous year), we estimate equation (1) on the full set of men aged 55-64, the coefficient

[18] 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.

on *veteran*post* for the not working outcome is equal to .0086. Given that the not-working rate for men in this age group in Canada is .4333 and in the United States is .3450, the gap between the two countries is .0883.[19] Using this rough estimation, we find that .0088/.0883, or 9.96% of that gap, can be explained by the availability of non-employer-linked health insurance for that age group.

As final cautions, these results do not prove that offering health insurance will decrease employment overall. Indeed, our results are not inconsistent with Gruber and Hanratty (1995), which finds that total employment rises with the introduction of national health insurance. Note that we are only examining the effects on men close to the end 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. Given that US labor market laws protecting older workers reduce job separations for older men (Lahey 2007), insurance may, by encouraging older workers on the margin to retire and be replaced by less-experienced (and thus less costly under an assumption of Lazear contracts) workers, result in more productive job matches. Additionally, as discussed above, health insurance may improve the productivity of the unhealthy on the margin of working by increasing their health capital.[20] Finally, overall social welfare may increase even with declining labor supply among some groups, e.g. if this insurance allows unhealthy workers with liquidity constraints to cut down on full-time work.

[19] Thanks to Kevin Milligan for the relevant statistics for Canada. These statistics were calculated from the Statistics Canada Labour Force Survey for the years 1998 to 2002 for men ages 55-64.

[20] Additional general equilibrium effects may also increase the average age of workers at small firms and decrease it at large firms since small firms are less likely to offer health insurance. If these effects are large enough, the compensating wage differential at small firms may decrease.

VII. Conclusion

In conclusion, we find that providing free comprehensive health insurance outside of employment decreases full-time work for older workers and increases both part-time work and non-work. Our finding of a decrease in self-employment implies that the income effect of public insurance receipt dominates the reduction in job-lock. To the extent that younger workers subsidize national health insurance for older workers, the income effect on older people from universal coverage may be a reason that non-employment is higher for older people in countries with national health coverage. However, lower employment in these groups may be efficient to the extent that it allows for more productive sorting into work and retirement.

Table 1. Summary Statistics, CPS 1992-2002*

	Veterans		Non-Veterans	
	Pre (N=7684)	**Post** (N=8150)	**Pre** (N=6195)	**Post** (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	0.16	0.172	0.112	0.152
Grad School	0.107	0.118	0.134	0.163
Pension Plan	0.429	0.486	0.404	0.44
Empl. HI Plan	0.627	0.652	0.581	0.595
Northeast	0.238	0.221	0.262	0.229
Midwest	0.26	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	0.25	0.225	0.229	0.198
Retired**	0.163	0.148	0.12	0.106
Self-Employed	0.201	0.166	0.209	0.192
Part Time	0.106	0.104	0.093	0.09
Occupations:				
Prof/Management	0.259	0.28	0.256	0.298
Tech/Sales/Cleric	0.186	0.187	0.148	0.148
Service	0.07	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	0.14	0.168	0.161
Industries:				
Agric/Mining	0.045	0.034	0.06	0.052
Construction	0.068	0.07	0.078	0.082
Manufacturing	0.16	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	0.05
Business/Repair	0.045	0.051	0.044	0.055
Personal	0.032	0.028	0.03	0.037
Public	0.051	0.065	0.032	0.033
Professional	0.14	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

Effect of Insurance Receipt on Labor Supply Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Not Working	Not Working	Retired	Retired	Part Time	Part Time	Self Employed	Self Employed
veteran*post	0.0179**	0.0247**	0.0142**	0.0040**	0.0099*	0.0124**	-0.0144**	-0.0102**
	(0.0064)	(0.0043)	(0.0032)	(0.0012)	(0.0043)	(0.0041)	(0.0043)	(0.0035)
veteran	0.0140**	0.0078**	0.0161**	0.0094**	-0.0004	0.0029	-0.0275**	-0.0065+
	(0.0048)	(0.0028)	(0.0018)	(0.0008)	(0.0039)	(0.0037)	(0.0040)	(0.0039)
married	0.1098**	0.0251**	0.0764**	0.0145**	-0.0254**	-0.0163**	0.0151**	0.0199**
	(0.0036)	(0.0045)	(0.0039)	(0.0026)	(0.0051)	(0.0042)	(0.0048)	(0.0049)
nonwhite	0.0494**	0.0225**	0.0174*	-0.0015	-0.0103	-0.0157*	-0.0731**	-0.0508**
	(0.0068)	(0.0068)	(0.0074)	(0.0036)	(0.0073)	(0.0064)	(0.0092)	(0.0072)
pension		-0.1327**		-0.0388**		-0.0409**		-0.1230**
		(0.0055)		(0.0025)		(0.0037)		(0.0048)
health ins		-0.0376**		0.0166**		-0.0486**		-0.1234**
		(0.0082)		(0.0015)		(0.0044)		(0.0053)
Observations	32721	32721	25666	25666	23978	23978	32721	31250

Note: Coefficient 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, state, year and education dummies and a constant. Health insurance denotes whether or not an individual is included in a health insurance plan in the previous year. Part-time regressions include a control for whether the firm offers a pension plan, all other regressions include a control for whether or not the individual has a pension. 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

Results by Group Status

I. By Marital Status

	Married				Single			
	Not Working	Retired	Self Employed	Part Time	Not Working	Retired	Self Employed	Part Time
veteran*post	0.0329**	0.0080**	-0.0033	0.0225**	0.0039	-0.0060*	-0.0276**	-0.0218*
	(0.0042)	(0.0015)	(0.0051)	(0.0046)	(0.0072)	(0.0030)	(0.0067)	(0.0085)
veteran	0.0011	0.0051**	-0.0114*	-0.0019	0.0219**	0.0192**	0.0173**	0.0198**
	(0.0027)	(0.0010)	(0.0049)	(0.0034)	(0.0059)	(0.0043)	(0.0062)	(0.0089)
Sig. Different?					Yes	Yes	Yes	Yes
Observations	26221	20528	25049	18704	6500	5138	6201	5274

II. By Estimated Means Test Cutoff

	Above Means Test				Below Means Test			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Not Working	Retired	Self Employed	Part Time	Not Working	Retired	Self Employed	Part Time
veteran*post	0.0280**	0.0043**	-0.0113**	0.0113**	-0.0183	0.0060	0.0011	0.00083
	(0.0043)	(0.0012)	(0.0028)	(0.0035)	(0.0205)	(0.0038)	(0.0100)	(0.0218)
veteran	0.0047+	0.0072**	-0.0065*	-0.0019*	0.0559**	0.0176**	-0.0070	0.0331**
	(0.0028)	(0.0008)	(0.0030)	(0.0030)	(0.0170)	(0.0038)	(0.0092)	(0.0138)
Sig. Different?					Yes	No	No	No
Observations	27677	21781	26281	21066	5044	3885	4969	2912

III. By Wife's Health Insurance Status

	Wife Has Employer-Provided Health Insurance				Wife Without Employer-Provided Health Insurance			
	Not Working	Retired	Self Employed	Part Time	Not Working	Retired	Self Employed	Part Time
veteran*post	0.0514**	0.0064	-0.0029**	0.0365**	0.0190*	-0.0012	-0.0030*	0.0137*
	(0.0073)	(0.0054)	(0.0009)	(0.0078)	(0.0087)	(0.0025)	(0.0014)	(0.0056)
veteran	-0.0143**	0.0114*	0.0010+	-0.0176**	0.0109*	0.0160**	0.0009	0.0105*
	(0.0049)	(0.0052)	(0.0006)	(0.0054)	(0.0050)	(0.0028)	(0.0008)	(0.0034)
Sig. Different?					No	No	No	Yes
Observations	12603	12603	10983	8673	13197	13197	10955	9672

Note: Coefficient estimates are taken from a probit regression of veteran and veteran x post as described in eq. (1). Marginal effects are reported. Regressions are restricted to those who worked at least one week in the year prior to the survey. Regressions include age, race, marital status, whether the individual is included in a health insurance plan in the previous year and a full set of state, year, industry, occupation, and education dummies and a constant. Part-time regressions include a control for whether the firm offers a pension plan, all other regressions include a control for whether or not the individual has a pension. Robust standard errors in parentheses are clustered on veteran and year. "Sig. Different" reports whether the veteran*post coefficients for the two populations are statistically significantly different from one another at the 5% level. The regression universe in "By Estimated Means Test Cutoff - Above" 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 "By Estimated Means Test Cutoff - Below" is restricted to those below the same income means test. The regression universe in "Marital Status - Married" is restricted to married men. The universe in "Marital Status - Single" is restricted to not married men.

+ 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*post	-0.0161+	-0.0017	0.0001	-0.0068	-0.0079	-0.0066
	(0.0095)	(0.0084)	(0.0079)	(0.0069)	(0.0061)	(0.0062)
veteran	0.0212*	0.0098+	-0.0272**	-0.0006	0.0042	0.0070*
	(0.0091)	(0.0054)	(0.0049)	(0.0045)	(0.0032)	(0.0033)
married	0.1186**	0.0344**	0.0085	0.0217*	-0.0337**	-0.0218**
	(0.0044)	(0.0079)	(0.0067)	(0.0092)	(0.0081)	(0.0061)
nonwhite	0.0522**	0.0249+	-0.0946**	-0.0640**	-0.0098	-0.0153
	(0.0096)	(0.0151)	(0.0103)	(0.0069)	(0.00107)	(0.0097)
pension		-0.1500**		-0.1226**		-0.0344**
		(0.0078)		(0.0047)		(0.0059)
health insurance		-0.0432**		-0.1306**		-0.0542**
		(0.0118)		(0.0108)		(0.0068)
Observations	13879	13879	13879	13292	9863	9863

Note: Coefficient 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, state, year and education dummies and a constant. Health insurance denotes whether or not an individual is included in a health insurance plan in the previous year. Part-time regressions include a control for whether the firm offers a pension plan, all other regressions include a control for whether or not the individual has a pension. 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

Further Specification Checks, Dependent Variable = Not Working

	(1)	(2)	(3)	(4)	(5)	(6)
	fully interacted	propensity score matching	no 2002	post 1994	10+ weeks last year	multinomial logit
veteran*post	0.0232**	0.0225*	0.0259**	0.0225**	0.0231**	0.0326**
	(0.0045)	(0.0095)	(0.0053)	(0.0070)	(0.0042)	(0.0050)
veteran	-0.0081	0.0081	0.0071*	0.0096+	0.0052+	0.0068*
	(0.0244)	(0.0050)	(0.0029)	(0.0053)	(0.0029)	(0.0031)
married	0.0304**	0.0331**	0.0236**	0.0234**	0.0365**	0.0403**
	(0.0073)	(0.0109)	(0.0054)	(0.0054)	(0.0047)	(0.0053)
nonwhite	0.0217**	0.0093	0.0203*	0.0168*	0.0215**	0.0095
	(0.0058)	(0.0124)	(0.0081)	(0.0069)	(0.0065)	(0.0082)
Observations	32721	32574	27446	25666	31680	31675

Notes: Coefficient estimates are taken from a probit regression as described in eq. (1), except column (6) which provides the results for a multinomial logit regression. Marginal effects are reported. Regressions are restricted to those who worked at least one week in the year prior to the survey, except column (5) which is restricted to those who worked at least 10 weeks in the year prior to the survey. Regressions include age, race, marital status, whether the individual is included in a health insurance plan in the previous year and a full set of state, year, industry, occupation, and education dummies and a constant. Robust standard errors in parentheses are clustered on veteran and year. Column (1) reports results with all controls interacted with veteran. Column (2) reports the results for a propensity score-matched sample. Columns (3) and (4) change the universe to not include years after 2002 or before 1994 respectively. Column (6) reports results from a multinomial logit regression that includes outcomes for full-time for an employer, part-time for an employer, self-employment (full or part time) and not employed. Reported results are marginal effects for the not employed outcome.

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

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