

Health and Utilization Effects of Expanding Public Health Insurance

by

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Submitted to the Department of Economics
in partial fulfillment of the requirements for the degree of

Doctor of Philosophy in Economics

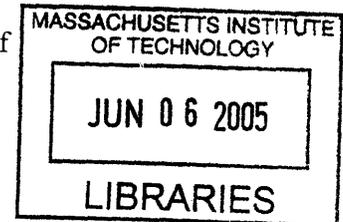
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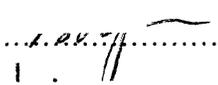
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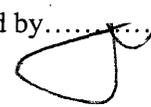
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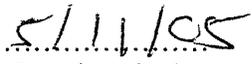
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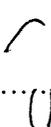


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Abstract

This thesis exploits a major overhaul in the U.S. Department of Veterans Affairs health care system to answer various questions about publicly-provided health care. The VA restructuring involved the adoption of a capitated payment system and treatment methods based on the managed care model. This reorganization was accompanied by a major expansion in the population eligible to receive VA care.

Chapter one analyzes both the efficiency of providing public health care in a managed care setting and the effectiveness of expanding coverage to healthier and wealthier populations. I estimate that between 35 and 70 percent of new take-up of VA care was the result of individuals dropping private health insurance. While utilization of services increased, estimates indicate that the policy change did not result in net health improvements. Regions providing more care to healthier, newly-eligible veterans experienced bigger reductions in hospital care and larger increases in outpatient services for previously-eligible veterans. This shift away from specialty care may help to explain the aggregate health declines.

Chapter two examines the impact of the introduction of a VA-sponsored drug benefit on Medicare-eligible veterans. Results suggest that a drug benefit does not result in changes in the quantity of drugs consumed, but does lead to an increase in spending and a shift in who pays for the prescriptions. The benefit appears to have a larger effect on lower-income individuals. Results also show suggestive evidence of positive health effects as a result of the drug benefit, an outcome which could be cost-saving in the long run.

Chapter three utilizes the change in government health care coverage for veterans to test whether employer-provided insurance leads to inefficiencies in the labor market, and the degree to which such inefficiencies might be alleviated by expanding public health insurance programs. We examine the impact of health care coverage on labor force participation and retirement by comparing veterans and non-veterans before and after the VA expansion. Results indicate that workers are significantly more likely to cease working as a result of becoming eligible for public insurance, and are also more likely to move to part-time work.

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This dissertation is dedicated to my grandfather, the veteran who inspired this research and the smartest person I know.

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Chapter One

Health and Utilization Effects of Increased Access to Publicly Provided Health Care: Evidence from the U.S. Department of Veterans Affairs

1.1. Introduction

In recent years there has been much focus in the political arena on the need for an improved system of public health care in the United States. With 45 million Americans currently uninsured, politicians often insist that the U.S. government should provide the same type of universal coverage for health expenditures that exists in other industrialized nations. In spite of years of national debate, however, a consensus has yet to be reached on a public insurance model for the entire United States. Often overlooked in these discussions is the fact that the United States government already owns and operates one of the largest health care systems in the world – the Veterans Health Administration (VHA).

The VHA is the principal agency of the U.S. Department of Veterans Affairs and the largest integrated health care system in the United States, with a budget of \$25 billion for 2003. Sweeping changes in VA health care over the past decade have resulted in a system designed in the spirit of the U.S. managed care model. These changes were meant to increase both the quality and availability of health care provided to United States veterans. VA's 1,300 care facilities include 163 hospitals, 850 ambulatory care and

community-based outpatient clinics, 206 counseling centers, 137 nursing homes and 43 domiciliary facilities (VA Fact Sheet, 2002).¹

Historically, the VA health care system was a network of hospitals, established over 70 years ago to provide specialty care to veterans with injuries or conditions directly resulting from their military service. Over time, the system expanded to include care for low-income veterans with non-service-connected conditions. VA provided mainly inpatient care, with outpatient services for non-service-connected conditions available only as a follow-up to an inpatient stay.

In 1995 the VHA began restructuring, shifting from hospital-based specialty care to an emphasis on primary care and prevention. 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 (Klein & Stockford, 2001). In addition to this change, the VHA's resource allocation system was redesigned. Following the HMO model, VA began distributing dollars using a capitated, patient-based formula.²

As a result of these changes, VA anticipated that increased efficiency would lead to significant reductions in costs per patient and in necessary staff. With this in mind, VA also 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).

The reorganization of the VHA has similarities to reforms in other parts of the U.S. health care sector. The literature evaluating these reforms does not provide a clear

¹ These provide shelter, food and necessary medical care to veterans disabled by age or disease but not in need of skilled nursing care or hospitalization.

² In a capitated payment system, the health care provider is reimbursed a flat dollar amount for each patient regardless of the services performed.

picture of the expected impact of such changes in the VHA. Within the private sector, managed care methods appear to have been successful at reducing costs per patient without negatively impacting health outcomes (e.g. Cutler and Sheiner 1997, Cutler, McClellan and Newhouse 2000). In public health care, however, it remains unclear whether managed care might achieve the same success. In a study of Medicaid HMOs, Duggan (2004) finds increases in spending and worsening health outcomes as a result of the switch to managed care.

Studies of public insurance expansions focusing on the increases in Medicaid eligibility in the late 1980s and early 1990s (e.g. Cutler and Gruber 1996, Dubay and Kenney 1997) have estimated that between 10 percent and 50 percent of new program enrollees are individuals who drop private health insurance in order to take up the public program. The expansion in VA coverage provides another opportunity to study the impact of offering public health care to a larger number of individuals, when newly-eligibles are drawn from a less vulnerable population in terms of income and health.

This paper is the first comprehensive study of these issues within the VA health care system. Papers that have examined the shift from hospital-based to outpatient-based care within the VHA (Ashton et al. 2003 and Thibodeau 2003) look at simple time trends in various health outcomes and conclude that the reorganization did not lead to health declines and may have resulted in health improvements along some dimensions. These studies, however, fail to control for overall health trends in the United States (for example, declining mortality and morbidity rates) and look at extremely short post-reform periods which do not capture the expansion in coverage. Therefore, while these findings may be suggestive, they are not conclusive.

Anecdotal evidence suggests that increased eligibility has resulted in severe overcrowding, particularly in regions serving a higher than average proportion of newly-eligible veterans (*New York Times*, 9/4/02). Even with the potential increases in the quality of care delivered, it is uncertain what the health effects of the reforms have been for the veteran population as a whole. Various competing effects make it difficult to predict whether aggregate veteran health should improve or decline as a result of these changes. On the one hand, as a result of the eligibility expansion, a large group of veterans has access to a previously unavailable form of health insurance. This, coupled with the fact that VA-users are now enrolled in a health care system that offers many more services than the old hospital-based care model, could lead to significant improvements in access to care and health outcomes. It is unclear however, whether newly-eligible veterans who may switch to VA care from another form of insurance are receiving superior care as a result. Additionally, if capacity has not expanded enough to compensate for the increase in demand for VA-provided care, veterans who were reliant on the VHA prior to the reforms may now be receiving less than adequate care because of overcrowding.

The goals of this paper are therefore twofold. I first evaluate the impact of the policy change on the health and health care utilization of the entire U.S. veteran population. I find increases in total health care utilization by veterans but evidence of net health declines. I therefore test whether the amount of care provided by the VHA to veterans in the previously-eligible categories is lower in regions which have proportionally more newly-eligible veterans in the patient load. This allows me to determine whether the declines in health may be the result of healthier, higher-income

veterans crowding-out care to VA's more vulnerable veteran populations. I find no evidence of crowd-out in total services provided, but instead find that regions with more newly-eligibles are shifting more of their care provision from inpatient to outpatient settings. Taken together, these findings suggest that financial constraints may have led to excessive reliance on outpatient systems for some veterans, lowering their health.

1.2. VHA Reform Details

By the early 1990s, the structure of the VA health care system had come under scrutiny. Critics of government-run health care pointed out that the U.S. government was already managing a very large health care system with a reputation for outdated, sub-par methods of care provision. The U.S. government determined that an overhaul of the VHA was necessary in order to repair the reputation of the health care system and to keep up with progress in American health care in general.

One of the biggest steps in the reorganization of the VHA was the passage of the Veteran's Health Care Eligibility Reform Act of 1996. Passed in October, this legislation was designed to restructure VA health care for increased efficiency. It led to the creation of the Medical Benefits Package, a health benefits plan available to all enrolled veterans. The plan covers services such as primary health care, diagnosis and treatment, surgery, mental health and substance abuse treatment, home health care, respite and hospice care, urgent and limited emergency care, drugs and pharmaceuticals, and preventive services such as immunizations and screening tests (VA Fact Sheet, 2002).

The Act also created a priority-based enrollment system for veterans using VA health care. All veterans who wish to receive VA care must enroll in the system, with the exception of those with a service-connected disability rating of 50 percent disabled or higher, those seeking care only for a service-connected condition, and those discharged from active duty for a disability incurred within the prior 12 months who have not yet received a disability rating. Veterans who enroll are placed in one of seven priority groups; veterans assigned to group one are considered the highest priority for treatment while veterans in group seven are considered lowest priority. (Group one veterans are those with service-connected conditions resulting in disability of 50 percent or higher; group seven veterans are those with incomes above VA determined means-test thresholds and no service-connected disabilities who agree to pay certain co-payments.) During the time period examined in this study, priority groups were used only for enrollment purposes. For all those enrolled, routine care was to be provided on a first-come first-served basis.

VA is required to enroll only those veterans for whom it has sufficient resources to provide timely health care, but for the years relevant to this study VA determined that its resources were adequate to enroll all priority groups.³ According to the 1996 legislation, VA could not provide hospital care or medical services to unenrolled veterans after October 1, 1998. VA began accepting applications for enrollment in October 1997, and applications were automatically processed for any veteran who had received care since January 1996. By 2002, the total number of veterans enrolled in the system was 6.6 million. In that same year, VA treated 4.5 million veterans, up from 2.5 million in 1995.

³ As of January 2003 VA began denying care to new enrollees in the lowest priority group. This is not a concern for this analysis, however, as the data only extend through 2002.

Prior to the legislation, VA was required to provide care only to veterans in priority groups 1-6, but could provide care to those in group 7 if resources allowed. The number of priority group 7 veterans treated by VA increased from 107,000 in FY1996 to 828,000 in FY2001 when they accounted for 22 percent of VA's workload (GAO-03-161).

In 1997, VA reformed its resource allocation method through the creation of the Veterans Equitable Resource Allocation (VERA) system. VERA was implemented to improve the distribution of resources among VA facilities. Most funds distributed through VERA are allocated based on expected patient load, and are adjusted according to case-mix. Funds are distributed to the administrations of each of the VHA's 21 regional networks. These Veterans Integrated Service Networks (VISNs) are then able to distribute their budgets across facilities as they see fit. VISNs receive a fixed dollar amount for each patient, so VERA provides incentives to increase the number of cases treated while minimizing the costs per case.⁴ Most priority 7 veterans are excluded from the patient load calculation, however.⁵ Thus, networks serving a higher than average proportion of priority group 7 veterans have fewer budgeted resources per patient than networks with a lower proportion (RAND report, 2001).

VA rationalized that excluding the healthier (less costly) group 7 individuals from the formula would eliminate the incentive to treat these veterans preferentially, to the detriment of veterans in the other priority groups. They also reasoned that co-payments by the priority 7 veterans would defray the costs of treating these individuals. However,

⁴ Under the case-mix adjustment, expected patients are classified as being either "Basic Care" or "Complex Care." Vested Basic Care patients (those with routine health needs) were allotted \$3,126 in 2001 while each Complex Care patient was allotted \$42,765. Adjustments are also made for variations in labor costs across regions (RAND report, 2001).

⁵ Only those in the complex care category are included – about 8 percent of priority 7 veterans were included in 2000.

networks with higher proportions of priority 7 enrollees consistently complain about resource shortages and serious appointment backlogs. The United States General Accounting Office (GAO) and VA's Office of Inspector General have both recommended that priority 7 veterans be included in the workload calculation (GAO-02-744T).

1.3. Predicted Effects of VHA Reforms

The VHA reforms have potentially competing effects on health care utilization and health outcomes. Following the policy change, all U.S. veterans became eligible for VA care, and therefore had to choose between the VHA and other health care providers. Such a decision is based on the relative value of treatment in each system, determined by factors such as the cost of care, wait time for care, and time to recovery following treatment. For an individual choosing between VA and the private sector, these factors may vary substantially before and after the policy change.

Prior to restructuring, low-income and disabled veterans could receive VA care for free, but the services provided were limited by the hospital-based nature of the health care system. For some priority 1-6 veterans, the value of VA hospital care was lower than the cost of privately purchased comprehensive insurance. For others, the cost of private care was prohibitive and if their particular needs were not met by available VA services, these individuals went without any care.

Previously-eligible veterans had more incentive to take up VA care following the reorganization because of the benefits expansion. Services remained free for veterans in groups 1-6, and the scope of VA coverage widened substantially. For poor and disabled veterans, the expected impact of the increase in benefits is increased utilization and improved health outcomes.

At the same time, as a result of the expansion in eligibility, newly-eligible veterans began queuing for care with previously-eligibles. If capacity did not expand enough to meet the increased demand for services, wait times for care provided to priority 1-6 veterans would have increased. As a result, these individuals may have received fewer services relative to the pre-period. In some cases, the cost imposed by increased wait times may have induced previously-eligibles to seek care elsewhere or not at all. The broader range of services may not have benefited these veterans if they had to wait longer for appointments. The eligibility expansion therefore would be expected to result in decreased utilization and potential health declines for the previously-eligible population. For veterans in priority groups 1-6, the expected advantages of the expansion in services combined with the disadvantages of longer wait times make it impossible to predict the net effect on utilization and health.

Veterans in priority group 7 were not eligible to receive VA care prior to the reforms. Unless they qualified for some other public insurance program, these individuals had to choose between purchasing private coverage and foregoing medical care. As a result of open enrollment, these veterans gained access to a form of comprehensive health insurance that was formerly unavailable.

For newly-eligible veterans who were otherwise uninsured or who had difficulty paying for care, the expected result would be an increase in health care utilization which could in turn have significant health benefits. Other veterans, however, may have dropped private insurance in order to take up VA care. For individuals shifting from private to public health care coverage, the impact on health and utilization would depend on the quantity and quality of care provided in the two systems. It is likely that the effects of merely switching from one form of coverage to another would be negligible.

The impact of the VA policy change on aggregate veteran health is determined by a complicated set of factors, many of which have opposing predicted effects on health care utilization and health outcomes. The consequences of this reform for the previously- and newly-eligible subsets of the veteran population are uncertain, and it is therefore difficult to predict the implications for access to care and health outcomes of veterans overall.

1.4. Aggregate Health and Utilization Effects

Because the implications of the VA reforms are unclear, I turn to empirical evidence to determine the effect on health care utilization and health outcomes for the veteran population. In assessing the impact of this policy change it is important to examine the effects on the aggregate veteran population rather than just VA-users. If care to previously-eligibles is crowded out by newly-eligibles, some individuals may receive no treatment and thus will not show up in the VA system. An analysis examining

only users of the VHA would therefore miss the potential negative impact on these individuals.

1.4.1. Data and Empirical Model

I use data from the National Health Interview Survey (1992-2001) to examine the impact of the changes in the VA system on aggregate veteran health. This survey is a nationally representative sample of repeated cross-sections containing information on individuals' self-reported health and utilization of health services. The NHIS contains an indicator for whether the individual in question is a veteran as well as data on an individual's health insurance coverage, but no information about health care providers (i.e. whether a particular veteran actually sought VA treatment).⁶ I therefore utilize this survey to examine the effects of the policy change on the health of the entire veteran population.

I use a difference-in-differences estimation strategy to compare the health of veterans and non-veterans before and after the enactment of the 1996 legislation. Because of the small number of female veterans and very young veterans in the data, I restrict my sample to include all surveyed males age 25 and over. The treated population is therefore male veterans age 25 and older, and the control group is male non-veterans over the age of 25. Since changes in the VHA are implemented throughout 1996 and 1997, I define 1992-1995 as the pre-policy period and 1998-2001 as the post-policy period. I estimate the following equation:

⁶ For the years 1992-1995 I also make use of the NHIS Health Insurance Supplement. Health insurance information was incorporated into the main surveys in 1997.

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

where:

y_{it} = measures of health outcomes and utilization

veteran_i = 1 if individual has been honorably discharged from active military duty,
0 otherwise

post_t = 1 in post-policy period, 0 otherwise

\mathbf{X}_{it} = vector of individual characteristics: age group dummies, age group*veteran status, race, marital status, years of education, income group, employment status, region, and an urban-rural indicator

δ_t = year dummy variables

and,

μ_{it} = a random error term.

Summary statistics are shown in Table 1. Comparing these statistics for the veteran and non-veteran populations reveals that the veteran population is older than the non-veteran population. For this reason, I include an age*veteran interaction term in my regressions, allowing age to enter separately for the two populations. The age difference likely accounts for at least some of the differences in average characteristics between the two groups. Veterans are less likely to be currently employed but have slightly higher average income and are more likely to be married. In addition, a smaller proportion of the veteran sample is Hispanic or black.

I employ a variety of health and health care utilization measures to assess the impact of the policy change on the veteran population. The utilization measures include hospital nights in the past year, hospital stays in the past year, an indicator for whether the individual has visited a doctor in the past year and a count of doctor visits in the past two weeks. It is unclear whether changes in utilization indicate a change in health status or a change in access to care, but examining the effect on these variables and the health

measures simultaneously will provide evidence of the impact of the policy change on both overall health and care availability. In addition to examining the utilization of particular health care services, I test measures of health insurance coverage, to see whether veterans drop private health insurance as a result of the policy change.

Health outcome measures include a self-report of the individual's health status (poor, fair, good, very good or excellent) and three 0-1 indicators of physical limitation based on activities of daily living (ADL) measures. These variables indicate whether an individual is limited in the ability to work, needs help with personal care, or is limited in any way. While it is difficult to quantify health, ADLs have been shown to be excellent predictors of morbidity and mortality.⁷ Since the NHIS does not contain mortality information, the ADL measures are the best available means of assessing the health effects of the policy change. In general, the average non-veteran in the sample reports slightly better health and spends fewer days in bed than the average veteran. In addition, the average non-veteran is less likely to report physical limitations and uses fewer health care services (hospital and outpatient).

1.4.2. Crowd-Out of Private Insurance

I first examine the impact of the policy change on health insurance coverage for the average veteran. The cost effectiveness of any public health insurance expansion will depend in part on how much of the newly-covered population was previously uninsured. Such a policy is expected to have a smaller impact on public health if a large portion of new users drops other forms of health insurance in order to take up the new program.

⁷ Wiener et al. (1990) provide a list of papers which give evidence of the predictive power of ADLs in determining health.

Table 2 presents the results of probit estimations of equation 1, where the dependent variables are various forms of health care coverage. The reported coefficients are probit marginal effects.

Veterans are less likely to hold private health insurance as a result of the VA expansion. Now that they have access to free (or very low cost in the case of those paying co-payments) and comprehensive health care services through the VHA, some veterans appear to no longer value coverage purchased through a private insurer. As shown in column 1, the average veteran's probability of being covered by private health insurance drops by 5 percent as a result of the policy change. In addition, column 2 shows that veterans are about 1 percent more likely to have no health insurance coverage other than VA as a result of restructuring.⁸

In columns 3-5, I test whether veterans drop other public programs as a result of the VA expansion. I check for declines in Medicare Part B coverage (Part B is the buy-in portion of Medicare which covers outpatient services) and whether there is any effect on take-up of Medicaid or other public insurance programs. Nearly all Medicare-eligibles in the United States buy into Part B. I test the impact on take-up for this program because of the fact that the VA Medical Benefits Plan is more generous than Medicare Part B coverage along some dimensions (for example, VA covers prescription drugs and routine physicals). Although the coefficient of interest is negative in the Part B equation, it is not

⁸ There is not perfect offset between the increase in no coverage and the drop in private coverage mainly because individuals dropping private coverage may be simultaneously covered by public programs other than VA. Tests on Medicare-eligibles in the sample show a significant drop in private insurance by veterans in this category. These individuals, who held private insurance in addition to their Medicare coverage, may have determined that Medicare combined with VA was sufficient, and that supplemental private insurance was no longer needed after the VA expansion.

statistically significantly different from zero. In addition, there appears to be no impact of the policy change on Medicaid or other forms of public insurance.

Prior to the policy change, about 80 percent of the veteran population was privately insured. The 5 percent decline in the probability that the average veteran has private insurance is therefore about a 4 percentage point drop in the veteran insurance rate, implying that 1.4 million veterans drop their private coverage. At the same time, about 2 million veterans take up VA care as a result of the policy change. Based on these numbers, about 70 percent of new take-up is offset by individuals who drop their private coverage. This is significantly higher than even the largest estimates of private insurance crowd-out in the Medicaid literature. This estimate is an upper-bound, because some veterans may drop their private coverage planning to use VA should the need arise, but then never actually take up the public program. Additionally, this case differs from the Medicaid expansions because it is likely that some of those dropping private insurance may not be individuals taking up the program for the first time. The switch to a much more comprehensive coverage of health care services may lead previous users who had private coverage to switch to relying only on VA care.

A lower-bound on the crowd-out estimate can be obtained by looking at the number of veterans who enrolled in the VHA following the policy change. By 2002, 6.6 million veterans had signed up to receive care. The patient load prior to restructuring was about 2.5 million, implying that after the reform about 4 million new veterans had indicated an interest in the VA program. During the same year, however, only about 2 million new veterans were treated. It is likely that some of the individuals dropping private insurance were among the 2 million veterans who enrolled but did not use VA

care. Based on the fact that about half of the new enrollees actually take up, it is reasonable to assume that an equivalent proportion of those dropping their insurance coverage are actually using VA care. This implies a lower-bound crowd-out estimate of about 35 percent.

1.4.3. Effects on Health Care Utilization

I next turn to estimating the effect of the policy change on utilization of health care services. In light of the fact that a non-trivial proportion of VA users appears to be substituting away from private insurance, it is uncertain whether the reforms will have a significant impact on utilization. For veterans dropping private insurance as a result of the policy change, the expected impact on total health care utilization is not large. For other groups of veterans, however, the effects could be substantial. Fully 18 percent of the veteran sample was without health insurance in the pre-period. These individuals now have access to a new form of health care, which could have a noticeable impact on average utilization. Additionally, previously-eligible veterans now have a different range of services at their disposal, but could also potentially be waiting in longer lines for care. The expected impact on their health care utilization is uncertain.

Table 3 reports the effect of the policy change on the utilization of health care inputs by veterans. The tested inputs in the equations reported in this table include number of hospital stays in the past 12 months, number of nights spent in the hospital in the past 12 months, an indicator for whether the individual had a doctor visit in the past year, an indicator for whether the individual had a doctor visit in the past two weeks, and the number of doctor visits in the past two weeks. Column 1 shows a negative and

significant drop in the number of nights the average veteran spent in the hospital in the past year as a result of the policy change. The number of hospital nights for the average veteran falls by $-.1$, which is approximately a 9 percent decrease in length of hospital stays relative to the pre-period.

With the exception of the number of hospital nights, all coefficients are positive and highly significant, indicating an overall increase in health care utilization by veterans following the policy change. Although the coefficient on number of hospital stays is positive, it is also small, indicating very little change in the number of admissions. This is not surprising; anecdotal evidence indicates that veterans with conditions serious enough to warrant hospitalization continued to receive timely care after the reforms (Twombly, 2003). This finding is also consistent with the estimated effects of managed care on the private sector. In general, managed care payment methods result in shorter hospital stays but no significant change in the total number of hospital admissions (Cutler & Sheiner, 1997).

While the length of hospital stays declines, use of outpatient services increases substantially. As a result of the policy change, the average veteran is 3 percent more likely to have had a doctor visit in the past year and 1.4 percent more likely to have visited the doctor in the past two weeks. The average number of doctor visits in the past two weeks increases by $.04$, a 14 percent increase in two-week outpatient visits for veterans. In spite of substantial shifting from private insurance, it does appear that overall utilization of health services increased for the veteran population as a result of restructuring.

1.4.4. Effects on Health Outcomes

Results for the effect of the reforms on health outcomes are reported in Table 4. All coefficients indicate a decline in veteran health as a result of the policy change, although the coefficient of interest in column 1 (the indicator for needing help with personal care) is insignificant. The policy change has a small positive effect on the probability that a veteran is limited in the ability to work. In addition, there is a larger positive effect on the probability that a veteran reports being limited in any way, with the policy change increasing this probability by 2 percent. In column 4, I test whether veterans' self-reported health is affected. I re-code the self-reported health measure as being equal to 1 if an individual reports excellent or very good health, and equal to zero if health is reported as being good, fair or poor. Following restructuring, the average veteran is 2 percent less likely to report either excellent or very good health.

It appears that while there are increases in health care utilization for the veteran population as a whole, the aggregate impact on veteran health is negative. In considering what may cause this result, it is important to observe that the change in VA benefits increased the covered population from roughly 40 percent of the veteran population to all veterans in the United States. Many veterans who were not receiving care previously may now have had access to care. The newly-covered individuals, however, tend to be relatively healthier, and their health improvements may be small when compared to the magnitude of the negative effects of crowd-out or longer wait times on the previously-eligible population. This possibility therefore invites a more detailed consideration of whether benefits to the newly-eligible population outweigh costs to previously-eligible VA-users.

1.4.5. Specification Checks

Before turning to an in-depth analysis of the impact of the policy change on various subsets of the veteran population, I perform a number of specification checks. In interpreting the coefficients in the above equations, I have assumed that the health and utilization effects are indeed a result of the changes in VA health care. The history behind the restructuring of the VHA and literature on similar changes in other health care systems supports this assumption. It is hypothetically possible, however, that the policy change arose from some pre-existing trend in veteran health, and that the changes in the post-period do not reflect changes in care provision. For example, veteran and non-veteran health may be moving relative to one another as a result of unobservables that are unrelated to VA policy and are not captured by the controls included in the regressions.

In order to confirm that the changes in veteran health and utilization actually result from the changes in VA health care, I check for pre-existing trends by estimating the same diff-in-diff regressions on pre-policy data. I choose the years 1991-1994 because this is a period when no major changes took place in the VHA. I code the years 1991 and 1992 as the “pre” years, and 1993 and 1994 as “post” years. The results of these falsification tests are reported in Tables 5a and 5b. In general, coefficients are quite small and insignificant, implying no previously existing trends impacting veteran health.

Most striking are the health outcome results reported in Table 5b. These coefficients are consistently the opposite sign from those found in the main regressions, indicating that veteran health was slightly improving prior to the policy change, whereas it declines sharply thereafter. The only highly significant, same-signed coefficient in these falsification checks is that on the indicator for visiting the doctor in the last two

weeks, and even this is quite small – about 2.5 times smaller than the coefficient in the main regressions. Evidence from these regressions therefore overwhelmingly suggests that the effects in the main specifications are in fact the result of the VA overhaul and not caused by pre-existing trends.

As a further test, I also predict self-reported health for a veteran and non-veteran with the same characteristics. I do this by running the following regression in each year of the sample separately for the veteran and non-veteran populations:

$$(2) \quad y_i = \beta_0 + \beta_1 X_i + \mu_i,$$

where y_i is the self-reported health measure (ranging from 0 to 4, where 0 is poor and 4 is excellent) and X_i contains the same controls as in equation 1.

I use these point estimates to predict veteran and non-veteran health in each year by calculating the fitted values of the dependent variable for a veteran and non-veteran, using the same characteristics for both predictions. The characteristics I choose are the average for the non-veteran population. I then difference predicted veteran and non-veteran health and calculate the standard error of the difference. Results are shown in Figure 1. As the figure demonstrates, there is a positive but insignificant difference between predicted veteran and non-veteran health (where a higher self-report indicates better health) in the pre-period. This difference becomes negative and significant in the post-period. This is further evidence that the veteran health declines are a result of the policy change and not some other unobservable trend.

1.4.6. Which Veterans Are Affected?

The coefficients reported in Tables 2-4 show the impact of the policy change on the aggregate veteran population. Although these results give evidence of the effects on veterans overall, it is unclear whether these effects are the result of take-up by the newly-insured or changes in care provision to the previously-eligible population. For this reason, I split the sample into two groups: expected previously-eligibles and expected newly-eligibles.

I categorize a veteran as previously-eligible (i.e. priority 1-6) if the individual has an income below VA-established means-test cutoffs (adjusted for the number of dependents in the household) or the individual reports being limited in any way. Since there is no indicator for service-connected disability in the survey, I assume that any disabled veteran is service-connected disabled. I therefore overestimate the number of priority 1-6 veterans in the sample. I define a veteran as newly-eligible (i.e. priority 7) if the individual reports no activity limitation and has an income above the means-test cutoff. I then estimate the same equations as reported in Tables 2-4 for each subset of the veteran population, where my control group for previously-eligible veterans is low-income or disabled non-vets and the control for the newly-eligible veterans is higher-income and non-disabled non-vets.

Results are reported in Tables 6a and 6b. The last row of Table 6b indicates whether the coefficients for previously and newly eligibles are significantly different from one another. I do not report results from regressions for the two samples where the dependent variables are indicators of health insurance coverage, because the coefficients for the two groups are never significantly different. Among the reported regressions, the two veteran samples are impacted differentially in three out of the six cases. The decline

in hospital nights in the main results turns out to fall entirely on the previously-eligible population. This is as expected, and is encouraging evidence that the methodology for splitting the sample results in good estimates of the two subsets of the veteran population. The previously-eligible population is subject to the change in the nature of VA care provision from hospital-based to outpatient-based services. The newly-eligible population, on the other hand, was (for the most part) not in the VA system before the change and did not experience this shift. It is therefore not surprising that hospital nights fall for the previously-eligible population only. In all other cases, both groups are affected similarly by the policy change, although some effects are slightly larger for the previously-eligibles.

The results in Table 6 establish that both subsets of the veteran population are affected by the policy change. This demonstrates that the results in Tables 2 through 4 are, for the most part, picking up effects for both newly- and previously-eligible veterans. It does not allow for a determination of which group is impacted more, however. While the effects on doctor visits seem a bit larger for the previously-eligible population, this is likely in part because these veterans are receiving more outpatient services in exchange for fewer inpatient services under the new VA system. Additionally, in spite of the increase in services, veterans in both groups still report a decline in health.

1.5. Are More Vulnerable Veterans Crowded Out?

The NHIS results show that for the average veteran, health care utilization, particularly of outpatient services, increases as a result of the changes in the VHA. They also demonstrate that this increase is not coupled with health improvements, but rather appears to be associated with declines in various measures of self-reported health. The declines in health for the previously-eligible population are of a particular concern, because these are the most vulnerable veterans – those VA is most concerned with serving. Anecdotal evidence on overcrowding (e.g. *The Boston Globe*, 1/1/03) suggests that the health declines for priority group 1-6 veterans may reflect crowd-out of services to these veterans by those in priority group 7. In spite of VA's expansion in services, if veterans in priority groups 1-6 are competing for care with those in group 7, these veterans may be treated less intensively than they were prior to the expansion. In order to check whether this can explain the demonstrated decline in health, I utilize data that will allow me to look specifically at the impact of the expansion on users of VA health care.

1.5.1. Data and Empirical Model

For this section of the paper, I turn to two additional data sources: the Current Population Survey Veterans Supplements (1993, 1997 and 1999) and VA Patient Treatment File (PTF) and Outpatient Care (OPC) claims records (1993-2002). The PTF and OPC claims records are large administrative files containing detailed information on every treatment episode in every VA facility. Because of the sheer size of these files, I aggregate to the VISN-year level. I use the PTF and OPC records to calculate treatment intensity for veterans in priority groups 1-6 and 7 in each VISN-year. I calculate the total

number of hospital nights, hospital stays, surgeries, inpatient procedures, clinic visits, and total contacts⁹ for group 1-6 and group 7 veterans in each VISN-year.

The CPS Veterans Supplements contain an indicator of service-connected disability status for the veteran sample, and are therefore used to provide population estimates of the number of veterans in priority groups 1-6 and 7 within each VISN. Because an individual must enroll in VA health care in order to be assigned to a priority group, the actual number of these individuals in the population is not known and therefore must be estimated. I use geographic identifiers in the CPS to assign veterans to a VISN. I separate priority group 7 veterans from those in groups 1-6 based on income (where the means test threshold is adjusted for number of dependents) and service-connected disability status. I also use the CPS to calculate the proportion of veterans in a VISN falling into each of six age groups.

I again employ a difference-in-differences empirical strategy, this time comparing regions providing a high number of services (measured as total contacts) to priority 7 veterans to those regions providing a low number of services to veterans in group 7. If it is true that newly-eligible veterans crowd out services to previously-eligibles, regions that provide more services to veterans in group 7 should therefore provide fewer services to those in groups 1-6 relative to other regions after the policy change. This possibility is made especially likely by the nature of the resource allocation system. Since VERA does not reimburse VISNs for treating priority 7 veterans, a region treating more of these individuals relative to its 1-6 patient population will have fewer total resources per patient. Regions providing more services to group 7 veterans may, as a result, provide fewer services to everyone else in the patient load.

⁹ Total contacts are defined as hospital stays plus clinic visits.

I define 1993-1996 as the pre-policy period and 1998-2002 as the post-policy period, since open enrollment was announced in 1997 but phased in beginning in 1998.

The equation that I estimate is as follows:

$$(3) \quad \text{services1-6}_{vt}/\text{pop1-6}_v = \beta_0 + \beta_1(\text{contacts7}/\text{pop1-6})_v * \text{post}_t + \beta_2 \mathbf{X}_{vt} + \mu_{vt}$$

where:

$\text{services1-6}_{vt}/\text{pop1-6}_v$ = Number of services to 1-6 veterans/Population 1-6 in a VISN-year, where population 1-6 is estimated from the 1999 CPS

post_t = 1 in post-policy period, 0 otherwise

$(\text{contacts7}/\text{pop1-6})_v$ = Average number of contacts to priority 7 veterans in a VISN in the post-period/Population 1-6 in a VISN, where population 1-6 is estimated from the 1999 CPS

\mathbf{X}_{vt} = VISN and year fixed effects, and VISN-level age group controls

and,

μ_{vt} = a random error term.

Tables 7a and 7b provide summary statistics on the services variables from the VA claims data. Table 7a reports the average number of treatments in a region provided to priority 1-6 and priority 7 veterans in the pre- and post-periods. Table 7b reports the average number of treatments to each priority group divided by the number of priority group 1-6 veterans living in the region. Comparing the pre- and post-period means reveals the significant shift from inpatient to outpatient services, as well as the large increase in treatments provided to veterans in priority group 7.

1.5.2. Results

I first estimate equation 3 by OLS. Results are reported in Table 8. A clear pattern emerges in the coefficients of the OLS regressions. Regions providing a higher number of contacts to veterans in priority group 7 do not appear to provide fewer total contacts to those in priority groups 1-6 – in fact the exact opposite is true. At the same time, these regions do provide fewer inpatient services to 1-6 veterans. It therefore appears that regions with greater influxes of new patients are shifting more of their care to outpatient provision than less crowded regions. In order to interpret these results, it is helpful to think about the difference in services provided to 1-6 veterans in the region providing the highest average number of contacts to priority 7 veterans versus the region providing the lowest number. The region providing the lowest average number of contacts to priority 7 veterans relative to the 1-6 population is VISN 10 (Ohio), while the region providing the highest number is VISN 23 (North and South Dakota, Iowa, Minnesota and Nebraska). The $(\text{contacts}_7/\text{pop}_{1-6})$ measure varies from .67 to 2.73, so the difference between the highest and lowest regions is around 2. Based on this, the OLS results imply that as a result of the policy change, the region providing the most contacts to priority 7 gives priority 1-6 veterans around 7.5 more clinic visits and total contacts per 1-6 population than the region providing the fewest contacts to group 7. A similar analysis for hospital nights indicates that the region with the highest concentration of group 7 veterans in its patient load provides group 1-6 veterans with .62 fewer hospital nights per population 1-6 than the region with the lowest concentration of priority 7 veterans.

A potential problem with equation (3) is the endogeneity of the $(\text{contacts}_7/\text{pop}_{1-6})$ term. Within a given region, the services provided to veterans in groups 1-6 and group

7 will be determined jointly. Thus, $(\text{contacts7}/\text{pop1-6}) \cdot \text{post}$ may be correlated with the error term, in which case OLS will result in a biased estimate of β_1 . In order to solve this problem, I instrument for $(\text{contacts7}/\text{pop1-6}) \cdot \text{post}$ using two different measures. The first instrument that I use is the post term interacted with the population of priority group 7 veterans in a VISN divided by the population of group 1-6 veterans ($\text{pop7}/\text{pop1-6}$), as estimated in the CPS.¹⁰ The location of veterans can be assumed exogenous (i.e., the population at large is not determined by VA health care within a VISN).¹¹ Therefore, regions with more priority 7 veterans relative to priority 1-6 veterans can reasonably be expected to have more priority 7 veterans seeking treatment.

An alternative instrument is the post term interacted with the number of priority 7 veterans who are Medicare-eligible (where any individual over age 65 is coded as Medicare-eligible) divided by the CPS measure of the total population of group 1-6 veterans ($\text{pop7} + \text{Mcare}/\text{pop1-6}$). The rationale behind this instrumental variable is the strong interest in take-up of VA health care by Medicare-eligible veterans following the policy change. As noted above, the VA Medical Benefits Package includes more generous coverage than traditional Medicare for particular services, especially pharmaceuticals. Consequently, Medicare-eligibles in group 7 have taken up VA care at very high and increasing rates. While about 30 percent of priority group 7 veterans in the overall veteran population are Medicare-eligible, 52 percent of treated priority 7 veterans

¹⁰ For this variable, population 7 is measured in the 1999 CPS, while population 1-6 is measured in the 1997 CPS. This denominator is estimated from a different sample in order to eliminate the division bias that could result from correlated measurement error in the denominators of the dependent and independent variables.

¹¹ This reasoning was supported by examining CPS estimates of the fraction of the total U.S. veteran population living in each VISN before and after the policy change. Veterans do not appear to relocate as a result of the health care reorganization.

were Medicare-eligible in 1999. By 2001, this proportion had increased to 65 percent (GAO-03-161).

While many Medicare-eligibles are interested in VA care primarily for the purpose of obtaining low-cost prescription drugs, VA pharmacies cannot fill prescriptions written by private physicians. These individuals must therefore receive care from a VA primary care physician in order to take advantage of the drug benefit. Since Medicare users are a subset of the veteran population that is highly likely to take up VA care as a result of the policy change, I assume that regions with a higher than average number of Medicare-eligible 7s relative to the total population of 1-6s will have to treat proportionately more group 7 veterans.

Results from the first stage regressions are reported in Table 9. In both cases, the coefficient on the instrument is positive and significant, demonstrating the expected relationship between the population of priority 7 veterans in a region and the number of services provided to those veterans. Tables 10a and 10b contain the results from the second stage. As in the OLS results, the number of clinic visits and total contacts to priority group 1-6 veterans rise relatively more in regions providing more services to group 7 after the policy change. Most of the inpatient measures are small and positive and statistically insignificantly different from zero, although the coefficients on hospital nights remain negative and in one case, significant. In table 10a, instrumenting for $(\text{contacts7/pop1-6}) \cdot \text{post}$ with $(\text{pop7/pop1-6}) \cdot \text{post}$, the coefficients imply that as a result of the policy change, the region providing the most contacts to group 7 veterans relative to priority 1-6 veterans will provide 1-6 veterans with 17 more clinic visits per total 1-6 population and .4 fewer hospital nights (although the hospital measure is insignificant)

than the region providing the fewest contacts to group 7 veterans. In table 10b, using the (pop7+Mcare)/pop1-6 instrument, the results imply that the policy change leads to .6 fewer hospital nights and 8 more clinic visits per population of 1-6 veterans in the highest contact 7 region versus the lowest.

As with the OLS results, 2SLS estimation suggests that regions serving more priority 7 veterans provide more total contacts to veterans in groups 1-6. At the same time, although the second-stage results for many of the inpatient measures are insignificant, evidence remains that regions serving more priority 7 veterans shorten hospital stays more and provide more of their treatment on an outpatient basis than regions serving fewer 7s relative to 1-6s. While there is no crowd-out in total services to previously-eligible veterans by newly-eligibles, it appears that the presence of newly-eligible veterans results in more substitution toward outpatient-based care for the previously-eligible population. Therefore, one possible explanation for the declines in veteran health found in the NHIS results is that the substitution of outpatient for inpatient services negatively impacts veterans in the previously-eligible group.

1.6. Conclusion

Analysis of the reforms in the Veterans Health Administration is important both for evaluating the VA program itself, and because these changes provide an excellent test case for studying government provision of health care in general. This policy change offers an opportunity to examine the efficiency of providing public health care in a

managed care setting, as well as the impact of extending health care coverage to individuals higher up in the health and income distributions.

As has been the case with other public health insurance expansions, some of the new take-up in VA health care appears to come from individuals who drop their private coverage in favor of VA. Crowd-out of private insurance by the public program is estimated to be between 35 and 70 percent. This is a larger effect than the 10 to 50 percent crowd-out measures found in the case of the expansions in Medicaid eligibility. The magnitude of the crowd-out of private insurance could stem from two factors. First, a large proportion of the total veteran population was insured prior to the policy change (about 80 percent), so much of the increased eligibility was for individuals with other forms of health insurance coverage. Second, both previously- and newly-eligible veterans have an incentive to drop private coverage in favor of VA after restructuring because of the major expansion in services that accompanied the eligibility expansion.

Although a potentially large proportion of new VA take-up is by individuals who were previously privately insured, there are significant utilization and health effects for both the newly-eligible and previously-eligible populations. As in the case of private insurance, VA care provision under the managed care model results in shorter hospital stays, but more services provided in an outpatient setting. While in the private sector this type of shift does not generally result in significant health declines or in changes in actual services provided, this appears not to be the case within the VA. Although health care utilization increases, veteran health declines according to every tested measure. This is more in line with the findings for Medicaid HMOs than private health care, although the

reasons for declining health in the VHA may be different since Medicaid coverage was contracted out to private HMOs whereas VA is entirely government operated.

Anecdotal evidence, as reported in the popular press and also by the Federal government (for example, see the Report on the Budget of the United States Government, 2003) indicates that the reforms have left veterans in priority groups 1-6 competing for care with veterans in group 7. Tests for crowding-out of services to previously-eligible veterans by the newly-eligible population do not indicate that veterans in more crowded regions receive fewer total visits, but rather that these regions shorten hospital stays more and alternatively provide more outpatient services relative to less crowded regions. It is possible, therefore, that VA's attempt to serve a larger (and on average, healthier) population has had a detrimental impact on veterans with service-connected conditions. Whereas, under the specialty care-based system, VA focused particularly on the needs of the most vulnerable veterans, the primary care-based system may substitute away from this type of care to the point that particular veterans with serious conditions related to their military service are no longer being treated with the same intensity.

Further research is needed to determine whether declines in specialty care are the cause of the negative health effects. The VA claims data contain diagnosis and treatment codes which will enable me to follow individuals with particular conditions over time in order to assess the impact of the policy change on treatment of specific health problems. I will additionally be able to test whether there is a change in the health composition of the previously-eligible population. This could result if VA specifically attempts to select the healthiest (least expensive) veterans. Bias in managed care towards healthier patient

populations has been well-documented in the case of Medicare HMOs.¹² Constructing measures of illness severity using the diagnosis codes in the PTF and OPC files will allow me to determine whether the policy change resulted in VA providing more care to healthier previously-eligibles, while sicker previously-eligibles wait longer for treatment.

In the case of the Department of Veterans Affairs, the coverage expansion and capitated payment system appear to negatively impact average health. Further evidence is needed, however, to determine what changes would be necessary to achieve health improvements. It is possible that adjustments in the resource allocation system allowing VA to shift more of its resources back to specialty services, or the end of open enrollment in 2003 halting the influx of healthier patients will result in a system which balances preventive medicine and specialty care in a manner more beneficial to the entire veteran population.

¹² For an overview of this literature, see Hellinger and Wong, 2000.

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**TABLE 1. SUMMARY STATISTICS, NHIS 1992-2001
(Sample Restricted to Males age 25+)**

| | Veterans | | Non-Veterans | |
|------------------------|--------------------------|---------------------------|--------------------------|---------------------------|
| | Pre (N=43218) | Post (N=28388) | Pre (N=89047) | Post (N=88329) |
| Age | 55.452 (14.049) | 57.928 (14.692) | 43.190 (14.5583) | 44.010 (13.725) |
| Hispanic | .044 | .065 | .132 | .221 |
| Black | .102 | .109 | .117 | .116 |
| Married | .801 | .738 | .741 | .690 |
| Years Education | 12.928 (2.783) | 13.081 (2.601) | 12.629 (3.538) | 12.562 (3.485) |
| Employed | .611 | .564 | .787 | .803 |
| Midwest | .249 | .235 | .238 | .214 |
| South | .337 | .369 | .327 | .350 |
| West | .220 | .218 | .228 | .252 |
| Northeast | .193 | .177 | .206 | .184 |
| MSA | .762 | .777 | .790 | .817 |
| Income \$0-\$4999 | .010 | .012 | .018 | .016 |
| Income \$5000-\$9999 | .037 | .023 | .045 | .029 |
| Income \$10000-\$14999 | .061 | .038 | .065 | .039 |
| Income \$15000-\$19999 | .079 | .043 | .074 | .041 |
| Income \$20000-\$24999 | .082 | .055 | .075 | .049 |
| Income \$25000-\$34999 | .147 | .104 | .137 | .090 |
| Income \$35000-\$44999 | .121 | .091 | .115 | .081 |

TABLE 1. SUMMARY STATISTICS, NHIS Cont.

| | Veterans | | Non-Veterans | |
|------------------------------------------------------------------------------------|--------------------------|---------------------------|--------------------------|---------------------------|
| | Pre (N=43218) | Post (N=28388) | Pre (N=89047) | Post (N=88329) |
| Income \$45000+ | .300 | .367 | .291 | .363 |
| Health (=0 if poor, 1 if fair, 2 if good, 3 if very good, 4 if excellent) | 2.641 (1.164) | 2.587 (1.119) | 2.839 (1.117) | 2.842 (1.061) |
| Hospital Nights Last Year | 1.112 (6.636) | 1.096 (6.590) | .552 (3.958) | .492 (4.292) |
| Hospital Stays Last Year | .156 (.540) | .187 (.892) | .088 (.410) | .090 (.480) |
| Doctor Visit Last Year? | .767 | .844 | .670 | .718 |
| #Doctor Visits Past 2 Weeks | .285 (.890) | .366 (1.240) | .190 (.750) | .201 (.941) |
| Limited in Ability To Work | .179 | .168 | .135 | .095 |
| Need Help With Personal Care | .047 | .045 | .039 | .027 |
| Limited in Any Way | .244 | .219 | .167 | .120 |
| Private Health Insurance | .799 | .742 | .733 | .696 |
| No Health Insurance | .175 | .138 | .262 | .217 |
| Medicare | .321 | .359 | .121 | .104 |
| Medicare Part B (Conditional on Medicare Eligibility) | .964 | .939 | .968 | .937 |

Table 2. Effects of VA Coverage Expansion on Other Forms of Health Insurance (Probit)

| | (1) | (2) | (3) | (4) | (5) |
|-----------------------|--------------------------------|------------------------------|------------------------------|-----------------------------|-----------------------------|
| | Private Coverage | No Outside Coverage | Medicare Part B | Medicaid | Other Public |
| Post x veteran | -0.04863** (0.00336) | 0.00697* (0.00307) | -0.00189 (0.00436) | 0.00146 (0.00191) | 0.00071 (0.00059) |
| veteran | 0.01321 (0.01934) | 0.00659 (0.00673) | -0.00983 (0.00601) | -0.00481** (0.00161) | -0.00062 (0.00079) |
| years education | 0.02509** (0.00103) | -0.01488** (0.00111) | -0.00020 (0.00037) | -0.00206** (0.00008) | -0.00012** (0.00005) |
| Employed | 0.23777** (0.01050) | -0.04332** (0.00555) | -0.02360** (0.00494) | -0.06690** (0.00233) | -0.00538** (0.00060) |
| Midwest | 0.03252** (0.00519) | -0.01038+ (0.00582) | 0.01805** (0.00616) | -0.00488** (0.00071) | -0.00117* (0.00052) |
| South | -0.05047** (0.00797) | 0.05741** (0.00751) | 0.01954** (0.00387) | -0.00720** (0.00049) | -0.00090+ (0.00047) |
| West | -0.06699** (0.00810) | 0.04643** (0.00612) | 0.01833** (0.00553) | 0.00114 (0.00093) | 0.00055 (0.00066) |
| Urban | 0.00921* (0.00361) | -0.00161 (0.00261) | -0.00685** (0.00258) | -0.00196** (0.00059) | -0.00055 (0.00053) |
| Hispanic | -0.13504** (0.00773) | 0.11958** (0.00921) | -0.00908 (0.00671) | 0.00215** (0.00061) | -0.00110** (0.00033) |
| Black | -0.07772** (0.00312) | 0.04614** (0.00279) | -0.01645* (0.00676) | 0.00810** (0.00098) | 0.00010 (0.00051) |
| Married | 0.12343** (0.00397) | -0.08929** (0.00671) | 0.01256** (0.00409) | -0.00652** (0.00029) | -0.00057* (0.00027) |
| Observations | 219014 | 227790 | 18208 | 219181 | 219078 |

Results from estimating equation (1) with Probit regressions.

Dependent variables are indicators for various sources of insurance coverage.

Coefficients are probit marginal effects.

Robust standard errors in parentheses are clustered on veteran and year.

Controls also include year, age and income group dummies, age group*veteran interaction terms and a constant.

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

Table 3. Effects of VA Reforms on Health Care Utilization

| | (1) | (2) | (3) | (4) | (5) |
|-----------------------|--------------------------------------|-------------------------------------|--------------------------------------|------------------------------------------|----------------------------------------|
| | Hospital Nights (OLS) | Hospital Stays (OLS) | Dr. Visit in Past Yr. (Probit) | Number Dr. Visits Last 2 Wks (OLS) | Dr. Visit Last 2 Weeks? (Probit) |
| Post x veteran | -0.09921+ (0.04990) | 0.01191* (0.00424) | 0.03106** (0.00644) | 0.03966** (0.00425) | 0.01423** (0.00224) |
| veteran | 0.12845* (0.04395) | -0.00129 (0.00675) | 0.04120** (0.00740) | 0.00405 (0.01102) | 0.01726** (0.00567) |
| years education | -0.01770** (0.00490) | -0.00172** (0.00037) | 0.01075** (0.00064) | 0.00436** (0.00078) | 0.00268** (0.00023) |
| Employed | -1.34508** (0.06329) | -0.15975** (0.00739) | -0.09372** (0.00366) | -0.22679** (0.01180) | -0.08269** (0.00328) |
| Midwest | -0.05659+ (0.02972) | 0.00562* (0.00258) | -0.02900** (0.00435) | -0.00898 (0.00682) | -0.00537+ (0.00281) |
| South | -0.02998 (0.03368) | 0.01296** (0.00332) | -0.03325** (0.00403) | -0.01126 (0.00767) | -0.00725** (0.00278) |
| West | -0.19408** (0.03511) | -0.01449** (0.00300) | -0.03305** (0.00724) | -0.00494 (0.01030) | -0.00310 (0.00328) |
| Urban | 0.01579 (0.02124) | -0.01025** (0.00268) | 0.01281** (0.00376) | 0.02335** (0.00592) | 0.00853** (0.00158) |
| Hispanic | -0.07849** (0.02190) | -0.01858** (0.00274) | -0.06397** (0.00421) | -0.03257** (0.00808) | -0.02625** (0.00252) |
| Black | 0.15903** (0.03029) | 0.00138 (0.00307) | 0.01148** (0.00289) | -0.00997 (0.00633) | -0.00849** (0.00199) |
| Married | -0.13149** (0.03086) | -0.00456+ (0.00243) | 0.04741** (0.00470) | -0.01763* (0.00630) | -0.00321 (0.00208) |
| Observations | 241550 | 241615 | 176195 | 241648 | 241650 |

Results are from estimating Equation (1). OLS coefficients are reported in columns (1), (2) and (4).

Probit coefficients in columns (3) and (5) are marginal effects.

Dependent variables are measures of health care utilization.

Robust standard errors in parentheses are clustered on veteran and year.

Controls also include year, age and income group dummies, age group*veteran interaction terms and a constant.

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

Table 4. Effects of VA Reforms on Health Outcomes

| | (1) | (2) | (3) | (4) |
|-----------------------|-----------------------------------------|----------------------------------------|--------------------------------------|-----------------------------------------|
| | Needs Help Personal Care (Probit) | Limited in Ability to Work (Probit) | Limited Any Way (Probit) | Health Excell./Very Good (Probit) |
| Post x veteran | 0.00081 (0.00072) | 0.00716** (0.00161) | 0.02092** (0.00205) | -0.02212** (0.00199) |
| veteran | -0.00654** (0.00179) | 0.00275 (0.00420) | 0.00336 (0.00492) | 0.03717** (0.00942) |
| years education | -0.00128** (0.00008) | -0.00611** (0.00019) | -0.00667** (0.00016) | 0.02606** (0.00054) |
| Employed | -0.08088** (0.00195) | -0.27913** (0.01554) | -0.27555** (0.01262) | 0.21792** (0.00549) |
| Midwest | 0.00387** (0.00073) | 0.01217** (0.00264) | 0.01859** (0.00355) | -0.01301** (0.00285) |
| South | 0.00348** (0.00069) | 0.01543** (0.00311) | 0.01980** (0.00424) | -0.03151** (0.00458) |
| West | 0.00318** (0.00082) | 0.01660** (0.00393) | 0.02537** (0.00466) | -0.01704** (0.00540) |
| Urban | 0.00069* (0.00031) | -0.00920** (0.00301) | -0.01577** (0.00354) | 0.02658** (0.00405) |
| Hispanic | -0.00503** (0.00050) | -0.04517** (0.00174) | -0.05445** (0.00286) | -0.02361** (0.00441) |
| Black | 0.00045 (0.00066) | -0.01463** (0.00249) | -0.01396** (0.00246) | -0.08142** (0.00436) |
| Married | -0.00754** (0.00099) | -0.03608** (0.00270) | -0.04210** (0.00377) | 0.03035** (0.00418) |
| Observations | 241650 | 214890 | 241650 | 241650 |

Results from estimating equation (1) with Probit regressions.

Coefficients are probit marginal effects.

Robust standard errors in parentheses are clustered on veteran and year.

Controls also include year, age and income group dummies, age group*veteran interaction terms and a constant.

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

Table 5a. Falsification Tests: Utilization
(“pre”= '91-'92, “post”='93-'94)

| | (1) | (2) | (3) | (4) | (5) |
|------------------|-----------------------------|-------------------------|---------------------------------------|-------------------------------------|-------------------------------------------|
| | Hospital Nights (OLS) | Hospital Stays (OLS) | Dr. Visit in Past Yr.? (Probit) | # Dr. Visits Last 2 Wks (OLS) | Dr. Visit Last 2 Weeks? (Probit) |
| “post” x veteran | -0.05991 (0.05382) | -0.00315* (0.00103) | 0.00841+ (0.00441) | 0.00774 (0.00709) | 0.00544** (0.00105) |
| Observations | 135443 | 135443 | 133321 | 135443 | 135443 |

Results from estimating equation (1) on 1991-1994 sample with “post” redefined as 1993-1994 and “pre” redefined as 1991-1992.

OLS coefficients are reported in columns (1), (2) and (4).

Probit coefficients in columns (3) and (5) are marginal effects.

Robust standard errors in parentheses are clustered on veteran and year.

Controls also include year, age and income group dummies, an age group*veteran interaction terms and a constant.

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

Table 5b. Falsification Tests: Health Outcomes
(“pre”= '91-'92, “post”='93-'94)

| | (1) | (2) | (3) | (4) |
|------------------|--------------------------------------|----------------------------------------|-----------------------------|--------------------------------------|
| | Needs Help Personal Care (Probit) | Limited in Ability to Work (Probit) | Limited Any Way (Probit) | Health Excell./Very Good (Probit) |
| “post” x veteran | -0.00206** (0.00032) | -0.00237 (0.00199) | -0.00714** (0.00182) | 0.01052+ (0.00552) |
| Observations | 135443 | 120234 | 135443 | 135443 |

Results from estimating equation (1) on 1991-1994 sample with “post” redefined as 1993-1994 and “pre” redefined as 1991-1992.

Coefficients are probit marginal effects.

Robust standard errors in parentheses are clustered on veteran and year.

Controls also include year, age and income group dummies, an age group*veteran interaction terms and a constant.

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

Figure 1. Predicted Health Difference, Vet vs. Nonvet, Same Average Characteristics

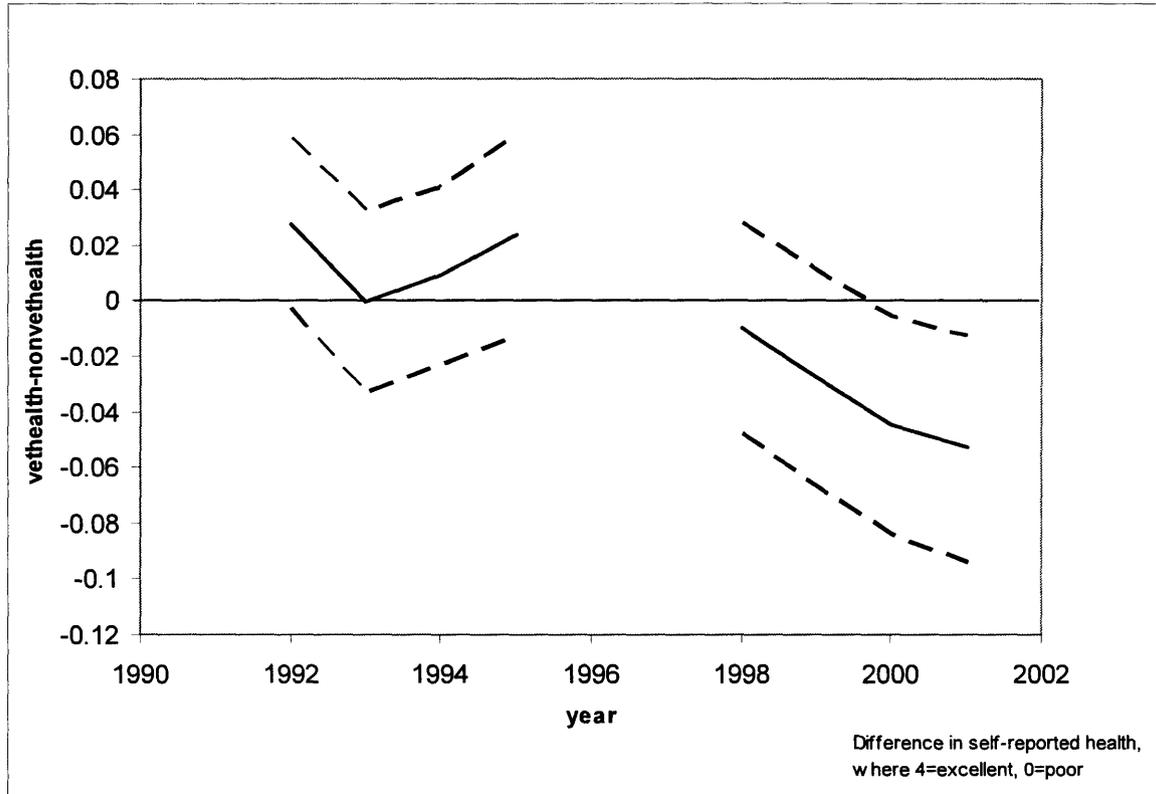


Table 6a. Effects on Expected Newly Eligibles

| | (1) | (2) | (3) | (4) | (5) | (6) |
|----------------|-----------------------------|----------------------------|---------------------------------------|-------------------------------------|----------------------------------------|-----------------------------------------|
| | Hospital Nights (OLS) | Hospital Stays (OLS) | Dr. Visit in Past Yr.? (Probit) | # Dr. Visits Last 2 Wks (OLS) | Dr. Visit Last 2 Weeks? (Probit) | Health Excell./Very Good (Probit) |
| Post x veteran | 0.00996 (0.01568) | 0.00952** (0.00284) | 0.01970* (0.00811) | 0.02679** (0.00360) | 0.01082** (0.00191) | -0.01339** (0.00344) |
| Observations | 125840 | 125843 | 92539 | 125843 | 125843 | 125843 |

Results are from estimating Equation (1) for the non-poor, non-disabled portion of the sample.

OLS coefficients are reported in columns (1), (2) and (4).

Probit coefficients in columns (3) and (5) are marginal effects.

Dependent variables are measures of health care utilization.

Robust standard errors in parentheses are clustered on veteran and year.

Controls also include year, age and income group dummies, age group*veteran interaction terms and a constant.

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

Table 6b. Effects on Expected Previously Eligibles

| | (1) | (2) | (3) | (4) | (5) | (6) |
|----------------|-----------------------------|----------------------------|---------------------------------------|-------------------------------------|----------------------------------------|-----------------------------------------|
| | Hospital Nights (OLS) | Hospital Stays (OLS) | Dr. Visit in Past Yr.? (Probit) | # Dr. Visits Last 2 Wks (OLS) | Dr. Visit Last 2 Weeks? (Probit) | Health Excell./Very Good (Probit) |
| Post x veteran | -0.22490* (0.10264) | 0.01173 (0.00938) | 0.04421** (0.00837) | 0.05154** (0.00973) | 0.01867** (0.00445) | -0.02786** (0.00572) |
| Observations | 115710 | 115772 | 83656 | 115805 | 115807 | 115807 |
| Sig Diff? | Yes | No | Yes | Yes | No | No |

Results are from estimating Equation (1) for the poor and/or disabled portion of the sample.

OLS coefficients are reported in columns (1), (2) and (4).

Probit coefficients in columns (3) and (5) are marginal effects.

Dependent variables are measures of health care utilization.

Robust standard errors in parentheses are clustered on veteran and year.

Controls also include year, age and income group dummies, age group*veteran interaction terms and a constant.

“Sig Diff?” reports whether the post*veteran 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 7a. Mean Number of Treatments in a Region-Year

| Pre-1997 | | Post-1997 | |
|---------------------------------------------|---------------------------------------|-----------------------------------------------|-----------------------------------------|
| Hospital Nights 1-6 444,752 (232,164) | Hospital Nights 7 9,853 (6,899) | Hospital Nights 1-6 226,955 (85,964) | Hospital Nights 7 10,898 (4,223) |
| Surgeries 1-6 13,879 (7,127) | Surgeries 7 315 (203) | Surgeries 1-6 7,757 (2,659) | Surgeries 7 529 (230) |
| IP Procedures 1-6 58,384 (40,294) | IP Procedures 7 1,308 (1,040) | IP Procedures 1-6 49,741 (18,020) | IP Procedures 7 2,772 (1,133) |
| Discharges 1-6 32,019 (16,932) | Discharges 7 685 (445) | Discharges 1-6 23,427 (7,407) | Discharges 7 1,296 (518) |
| Clinic Visits 1-6 663,959 (514,274) | Clinic Visits 7 20,269 (21,056) | Clinic Visits 1-6 5,214,600 (2,100,890) | Clinic Visits 7 546,784 (273,391) |
| Contacts 1-6 695,900 (524,492) | Contacts 7 21,032 (21,439) | Contacts 1-6 5,232,077 (2,105,674) | Contacts 7 554,030 (276,005) |

Table 7b. Mean Number of Treatments Per Pop. 1-6 In a Region-Year

| Pre-1997 | | Post-1997 | |
|---------------------------------------------------|-------------------------------------------------|---------------------------------------------------|-------------------------------------------------|
| Hospital Nights 1-6/Pop 1-6 0.9940 (0.5001) | Hospital Nights 7/Pop 1-6 0.0246 (0.0207) | Hospital Nights 1-6/Pop 1-6 0.5050 (0.1665) | Hospital Nights 7/Pop 1-6 0.0266 (0.0106) |
| Surgeries 1-6/Pop 1-6 0.0304 (0.0126) | Surgeries 7/Pop 1-6 0.0007 (0.0005) | Surgeries 1-6/Pop 1-6 0.0172 (0.0048) | Surgeries 7/Pop 1-6 0.0013 (0.0006) |
| IP Procs 1-6/ Pop 1-6 0.1343 (0.0975) | IP Procs 7/ Pop 1-6 0.0033 (0.0030) | IP Procs 1-6/ Pop 1-6 0.1171 (0.0565) | IP Procs 7/ Pop 1-6 0.0071 (0.0038) |
| Discharges 1-6/Pop 1-6 0.0711 (0.0345) | Discharges 7/Pop 1-6 0.0016 (0.0011) | Discharges 1-6/Pop 1-6 0.0522 (0.0127) | Discharges 7/Pop 1-6 0.0031 (0.0013) |
| Clinic Visits 1-6/Pop 1-6 1.4771 (1.0255) | Clinic Visits 7/Pop 1-6 0.0490 (0.0510) | Clinic Visits 1-6/Pop 1-6 11.5125 (3.3497) | Clinic Visits 7/Pop 1-6 1.3161 (0.6643) |
| Contacts 1-6/Pop 1-6 1.5481 (1.0441) | Contacts 7/Pop 1-6 0.0509 (0.0520) | Contacts 1-6/Pop 1-6 11.5511 (3.3560) | Contacts 7/Pop 1-6 1.3341 (0.6720) |

Table 8. OLS: Dependent Variables = Services to Priorities 1-6/Pop1-6

| | (1) | (2) | (3) | (4) | (5) | (6) |
|-----------------------------|---------------------|---------------------|-------------------------|---------------------|--------------------|--------------------|
| | Discharges | Hospital Nights | Inpatient Procedures | Surgeries | Clinic Visits | Total Contacts |
| (Contacts7/ pop1-6)*post | -0.016** (0.001) | -0.321** (0.031) | -0.011 (0.007) | -0.007** (0.001) | 3.766** (0.643) | 3.741** (0.642) |
| Observations | 189 | 189 | 189 | 189 | 189 | 189 |

Results from estimating equation (3) by OLS.

Dependent variables are VISN-year level measures of the number of services provided to priority 1-6 veterans divided by the CPS estimate of the total population of 1-6 veterans in the VISN.

Robust standard errors in parentheses are clustered on VISN pre and post.

Regressions also include year and VISN fixed effects and age group controls and a constant.

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

Table 9. First Stage

| | (1) | (2) |
|--------------------------|-------------------------|-------------------------|
| | (Contacts7/pop1-6)*post | (Contacts7/pop1-6)*post |
| (pop7/pop1-6)*post | 0.428** (0.162) | |
| (pop7+Mcare/pop1-6)*post | | 1.757** (0.437) |
| Observations | 189 | 189 |
| R-squared | 0.91 | 0.92 |

Dependent variables are VISN-year level measures of the total number of contacts (office visits + hospital stays) provided to group 7 veterans relative to the group 1-6 population in the VISN, interacted with the post dummy.

Regressions also include year and VISN fixed effects and age group controls and a constant.

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

Table 10. 2SLS: Dependent variables = Services to Priorities 1-6/Pop 1-6

10a. Instrument: (pop7/pop1-6)*post

| | (1) | (2) | (3) | (4) | (5) | (6) |
|-----------------------------|------------------|--------------------|-------------------------|------------------|-------------------|-------------------|
| | Discharges | Hospital Nights | Inpatient Procedures | Surgeries | Clinic Visits | Total Contacts |
| (Contacts7/ pop1-6)xpost | 0.002 (0.012) | -0.194 (0.170) | 0.065 (0.074) | 0.012 (0.013) | 8.724* (3.783) | 8.698* (3.780) |
| Observations | 189 | 189 | 189 | 189 | 189 | 189 |

Results from estimating equation (3) by 2SLS.

Instrument is VISN ratio of total population of group 7 veterans to total population of group 1-6 veterans, interacted with the post dummy.

Dependent variables are VISN-year level measures of the number of services provided to priority 1-6 veterans divided by the CPS estimate of the total population of 1-6 veterans in the VISN.

Robust standard errors in parentheses clustered on VISN pre and post.

Regressions also include year and VISN fixed effects and age group controls and a constant.

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

10b. Instrument: (pop7+Mcare/pop1-6)*post

| | (1) | (2) | (3) | (4) | (5) | (6) |
|-----------------------------|------------------|---------------------|-------------------------|------------------|-------------------|-------------------|
| | Discharges | Hospital Nights | Inpatient Procedures | Surgeries | Clinic Visits | Total Contacts |
| (Contacts7/ pop1-6)xpost | 0.004 (0.009) | -0.290** (0.098) | 0.029 (0.038) | 0.010 (0.008) | 4.344* (1.784) | 4.329* (1.777) |
| Observations | 189 | 189 | 189 | 189 | 189 | 189 |

Results from estimating equation (3) by 2SLS.

Instrument is VISN ratio of population of Medicare-eligible group 7 veterans to total population of group 1-6 veterans, interacted with the post dummy.

Dependent variables are VISN-year level measures of the number of services provided to priority 1-6 veterans divided by the CPS estimate of the total population of 1-6 veterans in the VISN.

Robust standard errors in parentheses clustered on VISN pre and post.

Regressions also include year and VISN fixed effects and age group controls and a constant.

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

Chapter 2

Should Medicare Cover Drugs?

The Impact of a Prescription Drug Benefit on Drug Utilization and Health Outcomes of Medicare-Eligible Veterans

2.1. Introduction

Since the 1965 inception of the Medicare program, the utilization and importance of prescription drug therapies has increased dramatically. When Medicare was first established, the role of pharmaceuticals in medicine was still fairly limited. Because drug spending made up only a small share of total medical expenditures in the U.S., outpatient prescription drug coverage was not a standard feature of most insurance programs, including Medicare. As drugs have become an increasingly critical component of modern health care, the vast majority of private insurance plans have incorporated prescription drug coverage into their standard benefits packages. The continued lack of such coverage by Medicare has therefore been a topic of extreme political importance.

The discussion of prescription drug coverage for the elderly is of particular significance because these individuals spend more on drugs than any other segment of the U.S. population. While 13 percent of the U.S. population is Medicare-eligible, Medicare beneficiaries account for 36 percent of prescription drug expenditures in this country (Goldman et al., 2002). According to Congressional Budget Office (CBO) estimates, drug spending for this population will rise at an average rate of 10 percent per year over the next ten years – far outstripping the anticipated growth in the U.S. economy (CBO,

Oct. 2002). The enactment of the Medicare Prescription Drug Improvement and Modernization Act of 2003 only partly addresses the concerns over the lack of prescription drug coverage for the elderly in the United States. Beginning in 2006, the Medicare program will offer a drug benefit providing partial coverage of prescriptions. This coverage is designed to protect Medicare-eligibles with catastrophically high prescription expenditures, but will still require fairly high out-of-pocket payments for many beneficiaries.¹³

The anticipated effects of prescription drug eligibility through Medicare are not yet clearly established. Thus, prior to the start of the benefit, the expected impact on spending, drug utilization and health outcomes is not obvious. In addition, proponents of a Medicare drug benefit commonly suggest that such a benefit, although expensive initially, could be cost-saving in the long run. It has been argued that the availability of drugs may improve the health of the elderly such that their use of other, more expensive Medicare services will decline.¹⁴ Yang, Gilleskie and Norton (2004) additionally point out that these types of health improvements may lead to increased life expectancy, and therefore increased lifetime consumption of medical care.

This paper examines the impact of a drug benefit on elderly Medicare beneficiaries by utilizing an exogenous change in eligibility for prescription drug coverage for a subset of the Medicare population. During the mid-1990s, the U.S.

¹³ For most individuals, these costs include a \$250 annual deductible, a 25% coinsurance rate on the first \$2250 of drug spending, no coverage for spending between \$2250 and \$5100, and a 5% coinsurance rate for all drug expenditures above \$5100.

¹⁴ Evidence from the medical literature lends some support to this claim. Soumerai et al. (1991) find that introducing a cap on the number of monthly prescriptions available to elderly Medicaid-eligibles in New Hampshire increased the risk of entering a nursing home over the course of one year. Soumerai et al. (1994) find that the same prescription cap resulted in an almost immediate increase in emergency mental health care and hospitalizations among mental health patients, leading to spending increases that exceeded the savings in prescription costs. Tamblyn et al. (2001) find that greater prescription cost-sharing in Canada led to higher rates of related adverse health events and emergency room visits for the elderly.

Department of Veteran's Affairs revamped its health care system, expanding both the population covered and the menu of available services. As part of this overhaul, VA established its first clearly defined health benefits package. This package was made available to all U.S. veterans, and includes a prescription drug benefit. This unique setup allows for estimation of the impact of drug coverage on Medicare-eligible veterans, utilizing a difference-in-differences strategy with non-veteran Medicare-eligibles as the control group.

2.2. Background

2.2.1. Prescription Drugs and the Elderly

Prescription drug spending in the United States has increased rapidly in recent years. These increases in spending can be attributed to a number of factors, including the introduction of new and increasingly more effective drugs with fewer side effects, and the higher cost of new brand-name drugs relative to older, generic alternatives. Older Americans have had particularly large increases in demand for these costly drug therapies because they tend to be in poorer health, with higher rates of disability and chronic illness than their younger counterparts (Yang et al., 2004). The distribution of prescription drug spending by the Medicare population is skewed, with the majority of beneficiaries spending under \$2000 per year (CBO, 2002). A large proportion of drug spending by Medicare-eligibles is concentrated in a relatively small share of the population – mainly individuals with chronic conditions. CBO predicted that only 17 percent of the Medicare

population would spend more than \$5000 on drugs in 2005, but that their spending would comprise more than 54 percent of total drug costs for the group. (CBO, 2002)

In spite of the lack of prescription drug coverage in traditional Medicare, many elderly individuals have some coverage from other sources including retiree health plans, individually purchased supplemental insurance, and Medicare HMOs. Even so, the generosity of these supplemental insurance plans is variable, and on average, 40 percent of drug expenditures for these individuals are out-of-pocket (CBO, 2002). Additionally, the likelihood that individuals have access to outside coverage varies by income level. Low-income elderly are often eligible for drug coverage through Medicaid, while individuals with higher incomes are the most likely to have retiree health benefits.¹⁵ Medicare beneficiaries with incomes between these two groups (in particular, those with incomes between one and three times the poverty level) are the most likely to have no prescription drug coverage (CBO, 2002).

Most studies examining the impact of prescription drug coverage on drug utilization by Medicare-eligibles rely on cross-sectional comparisons of beneficiaries without any drug coverage to those with prescription drug coverage from an outside source.¹⁶ These papers find, overall, that the presence of prescription insurance is associated with increased drug utilization. While these correlations are of interest, they do not provide direct evidence for the impact of a universal prescription benefit on drug utilization for the elderly. Because these studies cannot control for the endogenous

¹⁵ Because of increases in drug costs and spending, retiree health plans have also begun scaling back their generosity. Additionally, as a result of the Balanced Budget Act of 1997, reimbursement to Medicare HMOs became less generous and plans began withdrawing from the market.

¹⁶ For example, Lillard et al. (1999), Poisal et al. (1999), Blustein (2000), Federman et al. (2001), Poisal and Murray (2001).

selection of individuals into such supplemental insurance plans, they do not adequately simulate the introduction of a benefit for the entire Medicare population.

Several authors take other approaches to studying the effect of a drug benefit on Medicare-eligibles. Yang, Gilleskie and Norton (2004) use a dynamic simulation model to examine the effects of prescription drug benefits for Medicare-eligibles. Their simulations account not only for changes in drug utilization, but also for subsequent changes in health, mortality, and utilization of other health services. They find that expanding drug coverage would increase drug expenditures by between 12 percent and 17 percent, while improving mortality rates and only slightly increasing other health expenditures.

Hall (2005) uses variation in the market shares of Medicare HMOs to examine the demand for prescription drug coverage by individuals with access to Medicare managed care. She finds that the willingness to pay for a prescription drug benefit is \$20 per month, but that Medicare HMO enrollees spend \$146 per month when they receive a drug benefit. The author concludes that this is either caused by adverse selection into Medicare HMOs, or is a result of moral hazard causing individuals to spend more than their own willingness to pay when covered by the benefit. She concludes that if moral hazard is present, the provision of a prescription drug benefit to all Medicare beneficiaries “could be costly and inefficient.” The VA experiment will allow for further consideration of this issue, by measuring changes in the consumption of prescription drugs as a result of introducing a drug benefit.

2.2.2. The VA Reforms and Medicare-Eligible Veterans

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 to 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.¹⁷

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-

¹⁷ In a capitated payment system, the health care provider is reimbursed a flat dollar amount for each patient regardless of the services provided.

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.

For newly-eligible (non-poor and non-disabled) veterans, VA charges modest co-payments for use of services (\$2 per 30-day supply of each prescription drug), while all previously-eligible (poor and/or service-connected disabled) veterans may use VA services free of charge. If a veteran has private insurance, VA is also authorized to bill the insurance company for any services rendered that are not related to a service-connected condition, but VA cannot seek reimbursement from Medicare.

The Medicare-eligible veteran population has been particularly interested in taking up VA care as a result of this policy change. As part of the VA reforms, the government created the Medical Benefits Package, the first health benefits plan for veterans. This plan covers a number of services including primary care and preventive services, and, most notably, prescription drugs. This drug benefit has resulted in very high and increasing take-up rates among veterans over the age of 65. In 2002, 26 percent of the veteran population was Medicare-eligible, but 50 percent of VA-users were Medicare-eligible. Among newly-eligible users, the proportion over age 65 grew from 52 percent in 1999 to 65 percent in 2001. Over the same period, the number of 30-day prescription equivalents provided to newly-eligible veterans increased from 11 million to 26 million. The rate of growth of VA pharmacy expenditures for newly-eligible veterans between 1999 and 2001 was more than four times that for all other treated veterans (GAO-03-161).

While the VA drug benefit was particularly important to newly-eligible veterans, the overhaul in the system and expansion in services attracted many previously-eligibles

as well. Although spending on prescription drugs increased at a faster rate for the newly-eligible segment of the treated population, the net increase in spending for veterans in the newly-eligible group accounted for only 28 percent of the total increase in drug spending between 1999 and 2001 (GAO-03-161). VA health care may have been particularly attractive to low-income veterans following the policy change. These previously-eligibles were less likely than their disabled counterparts to have been aware of their eligibility for VA care prior to the reforms. Because the reforms were fairly well publicized and laid out much clearer eligibility rules, they may have affected some previously-eligible veterans in similar ways to the newly-eligible population. Boyle (2005) estimates the impact of the policy change on health outcomes and health care utilization for the aggregate veteran population. That study finds an impact on both the newly- and previously-eligible segments of the veteran population, showing an increase in doctor visits, but a decline in health for both groups. I now turn to examining the impact on the Medicare population in particular, focusing on utilization of prescription drug services.

2.3. Data and Empirical Strategy

In order to estimate the impact of a prescription drug benefit on Medicare-eligible veterans, I utilize data from the Medicare Current Beneficiary Survey (MCBS) for the years 1992-2000. The MCBS is a rotating panel of Medicare beneficiaries, with an over-sampling of older individuals. This data combines a survey component with Medicare

claims records, resulting in a dataset containing demographics for each survey participant, as well as detailed information about the individual's health status, utilization of medical care and medical spending.

I employ a difference-in-differences estimation strategy to compare prescription drug utilization and payments for veterans and non-veterans before and after the policy change. This strategy assumes that changes in drug coverage for Medicare beneficiaries that are unrelated to the VA expansion (such as the exit of many Medicare HMOs from the market) have the same impact on both veterans and non-veterans. Because of the very small number of female Medicare-eligible veterans, I restrict my sample to males. Additionally, because of the significant differences between the elderly and younger individuals receiving Medicare because of disability, I limit my sample to individuals age 65 and over.

Since changes in the VA system were implemented throughout 1996 and 1997, I define 1992-1995 as my pre-period and 1998-2000 as my post-period. I estimate the following equation by OLS:

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

where:

y_{it} = measures of prescription drug utilization and spending, measures of health, hospital stays and doctor visits

veteran_i = 1 if individual has been honorably discharged from active military duty, 0 otherwise

post_t = 1 in the post-policy period, 0 otherwise

\mathbf{X}_{it} = vector of individual characteristics: age, race, marital status, education, income, urban-rural and state dummies, age*veteran dummies

δ_t = year dummies

and,

μ_{it} = a random error term.

Although respondents may remain in the sample for as long as five years, because of the omission of 1996 and 1997, a very small number of pre-period respondents remain in the data for the post-period. For this reason, individual fixed effects are not included in the model and the dataset is treated as though it consists of repeated cross sections.

Summary statistics are reported in Table 1. In comparing the two populations, it is important to note the difference in average age across the groups. While the non-veteran sample is slightly older, the average age of the non-veterans falls over time while the average veteran age increases. These age differences are likely to explain at least some of the demographic differences between the two samples. In order to control for this factor, I allow age to enter into my regressions separately for veterans and non-veterans, by including age*veteran interaction terms.

2.4. Results

I first examine the impact of the VA policy change on prescription drug utilization of Medicare-eligible veterans age 65 and over. Since the policy change results in an exogenous change in drug coverage for a large number of veterans, the expectation is that Medicare-eligible veterans will increase their consumption of prescription drugs. Table 2 reports results from OLS difference-in-differences regressions. The outcomes considered are total (annual) spending on prescription drugs, the logged number of prescriptions filled during the year, and an indicator variable for any drug spending that year. As expected, total spending on prescription drugs for Medicare-eligible veterans

(where total spending is measured in 2000 dollars as the sum of spending by all payers in a given year) increases as a result of the introduction of a drug benefit. On average, total spending increases by \$66 as a result of the policy change, a 12 percent increase relative to the average total spending for a veteran in the pre-period.

More surprising, however, are the results for the other two outcomes. While spending on drugs does appear to increase, there is no significant effect on either the number of prescriptions filled or on the probability that a veteran fills any prescriptions during the year. It therefore does not appear that the elderly are increasing their consumption of drugs, measured in terms of the number of prescriptions, as a result of gaining VA insurance coverage. Thus, it does not appear that the elderly were unable to fill the same number of prescriptions previously because of a lack of drug coverage through Medicare. The increase in spending may instead represent the ability to switch to newer, more expensive drugs. If these drugs are more effective or have fewer side effects, this switch may have significant benefits.

The lack of an effect of the VA prescription drug benefit on the quantity of drugs consumed calls into question whether the increase in spending for veterans in the post-policy period is in fact a result of the VA benefit, or is a result of some other unobservable phenomenon. In order to better understand the effects of the benefit on drug spending by the veteran population, I next consider the effect of the policy change on the composition of payment for prescription drugs. For each medical service provided during the year, the MCBS contains information on payment, broken down by individual payers. This allows me to consider whether the policy change has an impact on the amount spent by each individual payment source.

The results by composition of spending are reported in Table 3. What is immediately noticeable is the distinct shift in the source of payment once the benefit is in place. As a result of the policy change, drug spending by VA increases by \$60 for the average veteran in the post-period, a 220 percent increase relative to the pre-period average. Simultaneously, spending by veterans out of pocket, and through insurance plans purchased in the private individual market both decline. Thus, while there is not an effect on the number of prescriptions filled, there is a marked change in who pays for these prescriptions.

Of additional note, is the fact that spending by private, employer-provided insurance plans and HMOs increases as a result of the policy change. It is possible that retirees with these types of benefits may still switch to using the VA drug benefit, because VA requires very little (if any) cost sharing by the veteran.¹⁸ Thus, even if a veteran has outside insurance, he can still save money by filling prescriptions through a VA doctor. If these prescriptions are not related to a service-connected condition, however, VA can bill the veteran's insurance company for its share of the cost. It is therefore possible that privately insured veterans can afford to switch to costlier drugs as a result of the policy change, and that the cost of the switch is partly borne by the private insurance company.

As has been noted by researchers and policy-makers alike, a prescription drug benefit for Medicare-eligibles has the potential to impact not only the use of prescription drugs, but the utilization of other medical services as well. In addition, given the importance of drug therapies in modern medicine, increased availability of such therapies

¹⁸ As mentioned earlier, during the time period considered in this study, newly-eligible veterans paid a co-payment of \$2 per prescription, while previously-eligible veterans paid nothing for prescription drugs.

has the potential to impact health outcomes. In Table 4, I consider the effects of the VA policy change on health care utilization and also on health outcomes.

The VA policy change had many components, and I am therefore unable to specifically isolate the impact of the drug benefit on the outcomes reported in Table 4. Instead, this table shows the impact of the policy change as a whole on the health and health care utilization of Medicare-eligible veterans. Even so, there is a great deal of evidence that the prescription drug benefit accounted for a significant portion of the increase in VA's patient load. The VA Office of Inspector General estimates that as many as 90 percent of newly-eligible users of VA care are primarily interested in using the system to fill prescriptions (Office of Inspector General, 2000). Thus, while the effects reported in Table 4 are not a result of drug coverage alone, they may be suggestive of the potential impact of such a benefit.

As shown in the table, the average number of hospital stays falls by .04 as a result of the policy change, a 14 percent decrease relative to the pre-period. The coefficient on the number of office visits is positive, but smaller (relative to the pre-period mean) and insignificant. These effects are, at least in part, a mechanical result of the shift in the nature of care provided by VA. Since VA shifted from an inpatient to an outpatient emphasis, many services that were previously provided in a inpatient setting were shifted to clinics. Even so, these results for Medicare-eligible veterans are slightly different from those found by Boyle (2005) for the entire veteran population. When examining veterans of all ages together, that study finds that while length of hospital stay declines, the number of hospital admissions is unchanged.

Table 4 also reports the effect of the policy change on a number of health outcome measures. These include a health indicator, coded as 1 if a veteran reports excellent, very good or good health, and 0 if health is described as fair or poor. Other tested measures include a dummy for whether the individual's social activities are limited by health, and an indicator for whether the individual dies during the year in which he is interviewed. While the effect on mortality is small and insignificant, health appears to improve according to the other two measures. As a result of the policy change, veterans are 3 percent more likely to report their health as good, very good or excellent, and 2 percent less likely to report that their health limits their social activities. These results are again in contrast to findings for the entire veteran population, for whom the policy change has a negative effect on self-reported health and activity limitation status (Boyle, 2005).¹⁹ Since younger veterans are less likely than older veterans to gain drug coverage as a result of the policy change (because more of them have access to employer-provided coverage), it is not unreasonable to assume that the drug benefit may be at least partly responsible for the differential positive impact on health for older veterans.

¹⁹ Although not reported in the paper, the same regressions in Boyle (2005) were run for the Medicare-eligible sample in the NHIS. For those individuals, self-reported health improved and activity limitation declined by magnitudes similar to those found with the MCBS data. Thus, results across the two samples are quite consistent for the elderly population.

2.5. Which Veterans Are Affected?

Tables 2-4 report the impact of the VA policy change on all veterans over the age of 64. While it is important to consider the effect of the policy change on all Medicare-eligible elderly veterans, these individuals fall into two distinct groups, each of which may be impacted differently by the change. As mentioned above, veterans may be classified as either newly-eligible for care (i.e. non-poor and non-disabled) or were eligible previously because of low-incomes or service-connected conditions. Newly-eligible individuals are the only segment of the population that undergoes a true shift from no coverage to a full benefit. Thus, isolating the effect on this group is of particular importance.

At the same time, previously-eligible veterans have the potential to be just as strongly affected, for a number of different reasons. First, these individuals became eligible for use of a health care system with a much wider scope of available services. Prescriptions, under the rules of the previous system, were available only for treatment of service-connected conditions, unless they were issued following a hospitalization. Thus, the prescription benefit became much more available to previously-eligible veterans with the shift to outpatient-based care and preventive medicine. In addition, the publicity surrounding the policy change may have raised awareness among veterans regarding the availability of VA care. Low-income veterans with no service-connected disabilities may have been previously unaware of their eligibility to receive care through the VA system, but may have learned of this option as a result of the policy change. In order to examine the impact of the drug benefit on these two populations separately, I split my veteran

sample into newly-eligibles and previously-eligibles, and test for a differential impact on the two groups.

2.5.1. Sampling Previously- and Newly-Eligibles

For each veteran in the sample, the MCBS contains sufficient detail to determine eligibility status. Using information about the veteran's income (a variable which also accounts for the income of the veteran's spouse, if applicable) I can verify whether or not the veteran is below the VA-established means test cutoff in a given year. Additionally, the data contains information about the veteran's service-connected disability rating. Following military service, every veteran who is injured or disabled in the line of duty is assigned a rating for the severity of that disability. Any veteran with a rating higher than 0 percent was eligible for VA care prior to the policy change. I can therefore establish whether a veteran was previously-eligible as the result of a service-connected condition.

In order to measure the impact of the policy change on newly- and previously-eligible veterans, I must first choose comparable controls from my sample of non-veterans. In order to do this, I divide my treated (i.e. post-period) veteran sample into two groups – those with and those without service-connected conditions. I then use propensity score matching to draw groups of individuals with comparable characteristics from the pre- and post-period non-veteran samples, as well as from the pre-period veteran sample. I calculate the propensity score (probability of treatment) using a logit model and controlling for a set of characteristics that includes year of birth, income, education, state of residence, residence in an MSA, race and marital status. Additionally, I include a number of activities of daily living (ADL) measures. These measures are indicator

variables coded to 1 if the individual reports having a lot of difficulty with or being unable to perform the following actions: kneeling, lifting, reaching, walking and writing.

Once the propensity score is calculated, I match each treated veteran to the individual in each of the other three groups (pre- and post-period non-veterans, and pre-period veterans) with the closest propensity score. This matching is done without replacement, and I impose a common support, meaning that treated individuals with propensity scores either above or below the scores for all non-treated individuals are dropped.²⁰ This leaves me with comparable control groups for both the disabled and non-disabled veterans in my sample. I then code each individual as being “newly-eligible” or “previously-eligible” based on income and disability group (where non-veterans matched to disabled veterans are considered “disabled” and therefore “previously-eligible.”)

The benefit of performing such a match is that it allows me to select a group of observably similar individuals to serve as controls for veterans with and without service-connected disabilities. The major drawback, however, is that unmatched individuals must be dropped from the sample. In general, this can result in large reductions in sample size (Blundell & Costa Dias, 2000). In this case, I retain about half of my original sample.

Summary statistics for the matched samples are reported in Table 5. The characteristics of veterans and non-veterans in these groups are much more similar than in the unmatched sample. Additionally, there are distinct differences between the newly- and previously-eligible populations. Previously-eligibles are less educated, less likely to

²⁰ The matching is accomplished with a Stata module, `psmatch2`, written by Leuven and Sianesi (see references).

be married, more likely to be black or Hispanic, and on average report poorer health than newly-eligibles.

2.5.2. Results By Eligibility Status

Results for newly- and previously-eligibles are reported in Tables 6-8. Table 6 reports the impact of the drug benefit on prescription drug utilization for the two groups. For the previously-eligible population, the results (reported in panel b) are consistent with those for the entire Medicare veteran population. Total spending on drugs increases significantly, with the drug benefit resulting in an increase in total spending of \$88 on average. There is not any significant effect on the number of prescriptions filled, nor is there an effect on the probability of any drug spending.

The results for newly-eligibles are surprising, however, in that they indicate a large and statistically significant decline in the number of prescriptions filled by veterans in this group after the policy change. The coefficient on *post*vet* in the total spending regression is also negative, although it is very imprecisely estimated. As in the results for the previously-eligibles, there does not appear to be any change in the probability that a newly-eligible veteran receives any drugs.

While these results are somewhat puzzling, a potential explanation emerges when examining changes in spending patterns across payers. Results by payer type are reported in Table 7. Both newly- and previously-eligible veterans experience higher drug payments by VA as a result of the policy change. The magnitude of the increase is enormous for the previously-eligibles, and quite a bit smaller for newly-eligibles. For the previously-eligible population, the policy change results in a \$126 increase in average

drug spending per person by VA, while the increase is only \$24 for newly-eligible veterans. As in the results for the entire sample, out-of-pocket spending on drugs falls quite a bit for both newly- and previously-eligibles, and spending through insurance plans purchased on the individual market also seems to fall, although these coefficients are not significant.

In addition to the difference in the magnitude of the increase in VA spending, there is one other result in Table 7 that differs markedly across the two populations. For newly-eligibles, there is a large, significant decline in prescription drug spending by Medicare HMOs, while this effect is much smaller, positive, and insignificant for the previously-eligible population. Since the end of the post-period coincides with the exit of many HMOs from the Medicare market, it is possible that there is, in fact, a differential effect of this exodus on newly-eligible (wealthier) veterans versus their non-veteran counterparts. While the VA drug benefit does appear to be providing an increase in drug spending to the newly-eligibles, they appear to be losing even more, on average, through a drop in coverage by Medicare HMOs. Based on this result, it appears possible that the drop in total drug spending reported in Table 6 is actually a result of this phenomenon rather than the VA policy change.

In the final table, Table 8, I examine the effects of the policy change on health care utilization and health outcomes for newly- and previously-eligible veterans. For the most part, these effects do not differ significantly across the two populations. The only case in which there are differential effects for the two groups is in the number of hospital stays, which decline for previously-eligibles but do not change significantly for newly-eligibles. This result is as expected, given that only the previously eligible population

was subject to the shift from inpatient-focused to outpatient-focused care that accompanied the VA policy change. As in the results for the unmatched sample, Table 8 indicates that the policy change had a health-improving effect on Medicare-eligible veterans. Veterans in both eligibility groups are more likely to report excellent, very good or good health, and are less likely to report that their health limits their social activities. Thus, while the drug benefit appears to have a differential impact on the two groups in terms of the amount spent, both groups appear to experience equal health benefits from the VA policy change as a whole.

2.6. Conclusion

In a 2002 report on the issues surrounding the design of a Medicare prescription drug benefit, the Congressional Budget Office stated that “the fundamental issue inherent in the debate about adding a drug benefit to Medicare may not be one of providing for use of prescription drugs so much as one of redistributing the cost of drugs away from the people, companies and government entities that now pay for them” (CBO, 2002). The results of this study appear to support that hypothesis. While utilization of prescriptions does not appear to change, on average, when Medicare-eligible veterans are offered a prescription drug benefit, there is a distinct shift in the composition of who pays for the drugs. Out-of-pocket spending on prescriptions drops sharply when the benefit becomes available, and spending by VA increases a great deal, especially for veterans in the previously-eligible group, who tend to have lower incomes and poorer health.

Such a shift in payers may be welfare-enhancing. If beneficiaries spend the money that they would otherwise have spent on drugs on other health-improving consumption goods (better food, for example), the policy change may improve average health even if it does not change drug consumption. Additionally, while the quantity of drugs consumed does not appear to be affected by this policy change, total spending on drugs does increase as a result. This could potentially indicate that in the presence of such a benefit, veterans are able to receive newer and more expensive drugs, which may be more effective and have fewer side-effects.

While this study cannot isolate the health effects specific to the drug benefit, it is interesting to note that the overall impact of the VA reorganization on health for Medicare-eligibles is positive. This is in direct contrast to previous findings which show that the policy change results in health declines for the entire veteran population. Since individuals in the Medicare-eligible segment of the veteran population are far more likely than their younger counterparts to be without outside drug coverage, it is probable that the differential impact of the drug benefit on older veterans at least partly accounts for this difference. The health-improving impact of the VA policy change on elderly veterans therefore provides suggestive evidence that a drug benefit for all Medicare beneficiaries will have positive effects on health outcomes.

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**TABLE 1. SUMMARY STATISTICS, MCBS 1992-2000
(Sample Restricted to Males age 65+, 2000\$)**

| | Veterans | | Non-Veterans | |
|-----------------------------------------------------------------|----------------------|----------------------|----------------------|----------------------|
| | Pre (N=9122) | Post (N=8180) | Pre (N=7156) | Post (N=4963) |
| Age | 73.0163 (6.1421) | 74.8115 (6.0283) | 79.2026 (7.8107) | 77.6453 (8.2830) |
| Hispanic | .0044 | .0093 | .0187 | .0520 |
| Black | .0669 | .0667 | .1055 | .1112 |
| Married | .7620 | .7439 | .6840 | .6604 |
| No HS Diploma | .3532 | .2624 | .5910 | .5104 |
| HS Diploma | .2935 | .2726 | .1373 | .2149 |
| Some College | .2524 | .3641 | .1433 | .2040 |
| College Degree | .1009 | .1009 | .1284 | .0707 |
| MSA | .7400 | .7328 | .6948 | .6933 |
| Income | 33246 (43000) | 36536 (47889) | 23999 (40872) | 26037 (40044) |
| Mortality | .0487 | .0550 | .0851 | .0784 |
| Health (=0 if poor or fair, 1 if good, very good, or excellent) | .4665 | .4595 | .3778 | .3565 |
| Activity Limit? | .2848 | .2754 | .3964 | .3694 |
| Any Drug Spending | .8144 | .8694 | .7884 | .8303 |
| Number Prescriptions | 14.4239 (18.1585) | 19.7278 (20.9696) | 15.0704 (19.0228) | 20.0480 (21.8646) |
| Total Drug Spending | 542.49 (753.14) | 944.29 (1243.39) | 522.18 (733.91) | 828.35 (1327.49) |

TABLE 1. SUMMARY STATISTICS, MCBS Cont.

| | Veterans | | Non-Veterans | |
|-----------------------------------------|-------------------------|--------------------------|-------------------------|--------------------------|
| | Pre (N=9122) | Post (N=8180) | Pre (N=7156) | Post (N=4963) |
| Drug Spend OOP | 279.4051 (447.7445) | 347.2216 (505.073) | 302.3481 (492.8668) | 389.2937 (632.5777) |
| Drug Spend HMO | 27.45849 (184.287) | 53.2734 (286.5586) | 22.3321 (181.9288) | 40.8195 (235.1832) |
| Drug Spend Empl Provided | 131.6622 (408.4726) | 302.2984 (751.1672) | 89.0975 (335.2317) | 175.6303 (828.4119) |
| Drug Spend Private Individual Market | 16.2199 (124.4327) | 25.2922 (163.5979) | 14.0011 (116.7878) | 28.6112 (173.8432) |
| Drug Spend Medicaid | 8.8100 (132.0418) | 14.0321 (265.5114) | 55.5231 (264.9589) | 88.3589 (408.7363) |
| Drug Spend Medicare HMO | 9.6788 (99.0804) | 48.6515 (445.449) | 10.7821 (92.9900) | 40.8199 (235.1832) |
| Drug Spend VA | 26.6342 (199.2345) | 85.02072 (432.4217) | n/a | n/a |
| Hospital Stays | .2857 (.7599) | .2855 (.7580) | .3931 (.9149) | .3996 (.9844) |
| Doctor Visits | 4.1505 (5.3520) | 4.5258 (5.9531) | 4.7594 (5.7140) | 4.5704 (5.9684) |

Table 2. Prescription Drug Utilization

| | (1) | (2) | (3) |
|---------------------|------------------------------|---------------------------|---------------------------|
| | Total Spending | Ln(#Prescriptions) | SpendAny0-1 |
| postxveteran | 66.4408* (22.6660) | 0.0077 (0.0243) | 0.0038 (0.0089) |
| veteran | -30.5403** (9.7011) | -0.0201 (0.0213) | -0.0025 (0.0075) |
| metro | 24.3090 (21.1225) | -0.0505+ (0.0255) | 0.0196* (0.0065) |
| hispanic | 54.0130 (44.7853) | 0.0532 (0.0486) | 0.0646** (0.0203) |
| black | -80.3982** (21.9276) | 0.0133 (0.0300) | -0.0067 (0.0054) |
| marital status | 68.3973** (14.8454) | 0.0330 (0.0197) | 0.0341** (0.0053) |
| Observations | 28911 | 24105 | 28911 |
| R-squared | 0.05 | 0.05 | 0.03 |

Results from estimating equation (1) by OLS. Dependent variables include a measure of total annual spending on prescription drugs, log number of prescriptions filled during the year, and an indicator for any drug spending during the year.

Robust standard errors in parentheses are clustered on veteran and year.

Spending is measured in 2000\$.

Controls also include age, age*veteran, state, year, income group and education dummies and a constant.

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

Table 3. Composition of Spending

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|----------------|---------------------------|-------------------------|-------------------------|--------------------------|--------------------------------------|--------------------------------------|--------------------------|-------------------------|
| | Medicaid | Medicare HMO | HMO | VA | Private Insurance, Employer Provided | Private Insurance, Individual Market | OOP | Other |
| postveteran | -15.32085* (7.07229) | -7.90262 (6.03904) | 11.02050** (2.42556) | 59.54179** (11.70615) | 67.54156** (8.40772) | -8.99263** (1.82854) | -44.11341** (7.17334) | 3.29311 (3.53357) |
| veteran | -28.75669** (3.79024) | -4.57226* (1.99097) | -4.43080* (1.78308) | 31.72580** (3.73352) | -5.04140 (5.72997) | -0.61713 (1.78719) | -31.39983** (4.83366) | 13.33012** (1.75160) |
| metro | -6.11537 (4.29521) | 19.25317** (5.42972) | 18.30426** (4.48273) | -1.07355 (3.78135) | 12.83689 (10.71986) | -4.54739+ (2.33547) | -21.43074** (6.94116) | 2.74389 (2.22948) |
| hispanic | 138.73713** (36.86216) | 22.19083 (14.86134) | -13.56111 (8.32820) | 3.31614 (5.91350) | -60.75227** (6.84915) | -10.88108** (2.24238) | -13.31437 (34.01586) | -9.94819+ (4.73077) |
| black | 10.80920 (7.82403) | 4.33477 (3.32038) | 0.35529 (4.29192) | 11.71299+ (6.46676) | -22.36447* (8.94641) | -7.25516** (1.55618) | -75.11581** (7.53664) | -3.27586 (8.98080) |
| marital status | 11.07189+ (5.85904) | 7.25249+ (3.77522) | 2.00857 (2.95797) | 2.92173 (5.36632) | 16.56093* (5.92652) | 1.58308 (1.70485) | 20.58371* (7.35798) | 4.47976+ (2.41748) |
| Observations | 28911 | 28911 | 28911 | 28911 | 28911 | 28911 | 28911 | 28911 |
| R-squared | 0.07 | 0.02 | 0.02 | 0.03 | 0.05 | 0.01 | 0.04 | 0.02 |

Results from estimating equation (1) by OLS.

Dependent variables are annual amount of spending by payer type.

Robust standard errors in parentheses are clustered on veteran and year.

Spending is measured in 2000\$.

Controls also include age, age*veteran, state, year, income group and education dummies and a constant.

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

Table 4. Utilization and Health

| | (1) Hospital Stays | (2) OfficeVisits | (3) Health0-1 | (4) ActLimit0-1 | (5) Died |
|---------------------|-------------------------------|-----------------------------|-------------------------------|-------------------------------|-----------------------------|
| postxveteran | -0.04131* (0.01560) | 0.11854 (0.10045) | 0.02852** (0.00580) | -0.01574* (0.00601) | 0.00212 (0.00286) |
| veteran | -0.00730 (0.00790) | -0.46071** (0.06204) | 0.00391 (0.00595) | 0.00570 (0.00582) | 0.00323 (0.00200) |
| metro | -0.01138 (0.01817) | 0.09105 (0.09580) | 0.00652 (0.00730) | -0.00212 (0.00585) | -0.00028 (0.00416) |
| hispanic | -0.05569 (0.03803) | 0.58431 (0.34512) | -0.03640* (0.01545) | -0.01248 (0.01864) | -0.02435* (0.01116) |
| black | -0.04029* (0.01857) | -1.07641** (0.10218) | -0.01005 (0.01243) | 0.02417+ (0.01207) | -0.00829 (0.00637) |
| marital status | -0.01763 (0.01271) | 0.18576+ (0.09412) | -0.01694** (0.00340) | 0.00338 (0.00718) | -0.00435 (0.00283) |
| Observations | 28911 | 28911 | 28824 | 28701 | 28911 |
| R-squared | 0.03 | 0.05 | 0.07 | 0.08 | 0.04 |

Results from estimating equation (1) by OLS.

Health is an indicator =1 if individual is in excellent, very good or good health.

ActLimit is an indicator =1 if individual reports that health limits social activity.

Robust standard errors in parentheses are clustered on veteran and year.

Controls also include age, age*veteran, state, year, income group and education dummies and a constant.

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

**TABLE 5A. SUMMARY STATISTICS, MATCHED SAMPLE OF NEWLY ELIGIBLES
MCBS 1992-2000
(Sample Restricted to Males age 65+, 2000\$)**

| | Veterans | | Non-Veterans | |
|-----------------------------------------|---------------------|----------------------|----------------------|----------------------|
| | Pre (N=1041) | Post (N= 1469) | Pre (N=1209) | Post (N=1591) |
| Age | 74.8953 (6.3998) | 75.0926 (7.6287) | 73.6791 (5.8201) | 75.3859 (7.0152) |
| Hispanic | .0058 | .0109 | .0091 | .0063 |
| Black | .0528 | .0470 | .0447 | .0402 |
| Married | .8738 | .8529 | .8677 | .8297 |
| No HS Diploma | .3045 | .2939 | .2366 | .2231 |
| HS Diploma | .2917 | .3035 | .3143 | .2810 |
| Some College | .2574 | .2306 | .2928 | .3476 |
| College Degree | .1464 | .1721 | .1563 | .1483 |
| MSA | .7017 | .7442 | .7411 | .7165 |
| Income | 43777 (44111) | 48727 (54587) | 48302 (60050) | 48042 (40461) |
| Mortality | .0548 | .0606 | .0463 | .0402 |
| Health | .4595 | .5014 | .4809 | .4710 |
| Activity Limit? | .2972 | .2612 | .2672 | .2587 |
| Any Drug Spending | .8415 | .8707 | .8553 | .8774 |
| Number Prescriptions | 15.4486 (17.38) | 19.0688 (20.1023) | 15.1357 (19.1873) | 20.1597 (21.3892) |
| Total Drug Spending | 609.50 (739.01) | 931.62 (1144.29) | 616.98 (809.43) | 1019.27 (1845.07) |
| Drug Spend OOP | 322.74 (478.24) | 366.529 (527.19) | 343.94 (512.62) | 468.54 (759.44) |
| Drug Spend HMO | 33.25 (201.99) | 59.68 (277.84) | 42.23 (212.64) | 58.26 (264.16) |
| Drug Spend Empl Provided | 160.92 (396.55) | 325.46 (819.46) | 165.13 (468.93) | 314.99 (1297.58) |
| Drug Spend Private Individual Market | 21.74 (155.22) | 35.99 (201.45) | 25.13 (170.50) | 47.29 (253.08) |
| Drug Spend Medicaid | .96 (27.65) | 7.23 (123.97) | 5.07 (126.78) | 6.31 (147.48) |
| Drug Spend Medicare HMO | 13.14 (144.93) | 47.31 (229.45) | 9.49 (93.62) | 84.95 (725.18) |
| Drug Spend VA | 13.02 (100.74) | 35.86 (206.72) | n/a | n/a |
| Hospital Stays | .2978 (.7543) | .2859 (.7591) | .2986 (.8375) | .2910 (.8089) |
| Doctor Visits | 4.9760 (5.7932) | 4.6535 (6.0041) | 4.8172 (5.4380) | 4.7769 (6.0853) |

**TABLE 5B. SUMMARY STATISTICS, MATCHED SAMPLE OF PREV ELIGIBLES
MCBS 1992-2000
(Sample Restricted to Males age 65+, 2000\$)**

| | Veterans | | Non-Veterans | |
|-----------------------------------------|----------------------|----------------------|----------------------|----------------------|
| | Pre (N=2241) | Post (N=2819) | Pre (N=2029) | Post (N=2551) |
| Age | 74.7711 (6.6379) | 75.9237 (6.8259) | 72.6925 (5.8258) | 76.0992 (6.8385) |
| Hispanic | .0138 | .0213 | .0138 | .0212 |
| Black | .1356 | .1146 | .1084 | .1015 |
| Married | .6206 | .6229 | .6461 | .6370 |
| No HS Diploma | .5580 | .4755 | .5111 | .4700 |
| HS Diploma | .2363 | .2586 | .2509 | .2536 |
| Some College | .1544 | .2084 | .1897 | .2215 |
| College Degree | .0512 | .0575 | .0483 | .0549 |
| MSA | .6851 | .6891 | .7068 | .7072 |
| Income | 18314 (15278) | 20174 (31679) | 20701 (35179) | 19818 (21375) |
| Mortality | .0785 | .0780 | .0582 | .0639 |
| Health | .3226 | .3391 | .3574 | .3373 |
| Activity Limit? | .4317 | .3815 | .3621 | .3867 |
| Any Drug Spending | .7876 | .8390 | .8029 | .8456 |
| Number Prescriptions | 16.6943 (21.0855) | 21.1721 (22.7671) | 15.8980 (18.9860) | 21.5312 (22.8735) |
| Total Drug Spending | 536.81 (771.87) | 926.59 (1360.11) | 560.58 (769.95) | 852.66 (1073.12) |
| Drug Spend OOP | 281.31 (449.47) | 313.97 (490.92) | 306.69 (483.31) | 401.83 (561.13) |
| Drug Spend HMO | 23.50 (160.23) | 53.71 (321.12) | 22.67 (159.37) | 46.11 (256.03) |
| Drug Spend Empl Provided | 99.29 (403.52) | 198.56 (588.49) | 97.78 (348.36) | 180.42 (615.66) |
| Drug Spend Private Individual Market | 12.56 (92.15) | 16.69 (132.40) | 15.48 (140.55) | 24.27 (133.97) |
| Drug Spend Medicaid | 22.89 (199.68) | 35.40 (441.20) | 68.52 (312.60) | 95.22 (432.60) |
| Drug Spend Medicare HMO | 10.20 (105.50) | 54.53 (691.81) | 13.36 (109.15) | 50.83 (242.73) |
| Drug Spend VA | 38.62 (221.59) | 168.38 (556.82) | n/a | n/a |
| Hospital Stays | .3958 (.9112) | .3491 (.8592) | .3494 (.8819) | .3701 (.9353) |
| Doctor Visits | 4.0522 (5.6010) | 3.9535 (5.8667) | 4.6437 (5.7493) | 4.5076 (6.0125) |

Table 6. Drug Utilization By Eligibility Status

| a. Newly Eligible | | | |
|-------------------------------|--------------------------------------|-------------------------------------|----------------------------------|
| | (1) | (2) | (3) |
| | Total Spending | Ln(#Prescriptions) | SpendAny0-1 |
| postxveteran | -68.5023 (42.0496) | -0.1204** (0.0362) | 0.0066 (0.0105) |
| veteran | 13.2087 (17.7664) | 0.0980** (0.0155) | -0.0030 (0.0058) |
| Observations | 5262 | 4558 | 5262 |
| R-squared | 0.06 | 0.07 | 0.03 |
| b. Previously Eligible | | | |
| | (1) | (2) | (3) |
| | Total Spending | Ln(#Prescriptions) | SpendAny0-1 |
| postxveteran | 88.3916** (24.8323) | 0.0071 (0.0382) | 0.0021 (0.0095) |
| veteran | 3.9338 (11.0259) | -0.0053 (0.0271) | 0.0023 (0.0092) |
| Sig Diff? | Yes | Yes | No |
| Observations | 9495 | 7860 | 9495 |
| R-squared | 0.06 | 0.05 | 0.05 |

Results from estimating equation (1) by OLS, for matched samples. Dependent variables include a measure of total annual spending on prescription drugs, log number of prescriptions filled during the year, and an indicator for any drug spending during the year.

Robust standard errors in parentheses are clustered on veteran and year.

Spending is measured in 2000\$.

Controls also include race, marital status, urban, age, state, year, income group and education dummies and a constant.

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

“Sig Diff?” reports whether the post*veteran coefficients for the two populations are statistically significantly different from one another at the 5% level.

Table 7. Composition of Spending By Eligibility Status

a. Newly Eligible

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|--------------|-----------------------|--------------------------|-----------------------|------------------------|--------------------------------------|--------------------------------------|---------------------------|-------------------------|
| | Medicaid | Medicare HMO | HMO | VA | Private Insurance, Employer Provided | Private Insurance, Individual Market | OOP | Other |
| postxvet | 1.98766 (3.16377) | -45.00117** (4.30879) | 5.22984 (7.90451) | 23.85820* (8.28031) | 17.97936 (23.30137) | -8.15607 (6.24620) | -60.39117** (15.50978) | 3.28261 (5.97404) |
| vet | -2.87979 (2.82181) | 12.11037* (4.95684) | -5.66927 (6.29893) | 10.33836* (3.73533) | -0.01218 (8.53574) | -0.00345 (3.56988) | -22.15217 (13.50303) | 19.41979** (5.09627) |
| Observations | 5262 | 5262 | 5262 | 5262 | 5262 | 5262 | 5262 | 5262 |
| R-squared | 0.03 | 0.05 | 0.04 | 0.06 | 0.04 | 0.03 | 0.06 | 0.02 |

b. Previously Eligible

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|--------------|--------------------------|-----------------------|-----------------------|---------------------------|--------------------------------------|--------------------------------------|--------------------------|-------------------------|
| | Medicaid | Medicare HMO | HMO | VA | Private Insurance, Employer Provided | Private Insurance, Individual Market | OOP | Other |
| postxvet | -12.40408 (11.84913) | 8.53725 (8.90341) | 2.78714 (3.68278) | 125.91059** (22.12296) | 11.05660 (12.86173) | -4.21929 (4.83693) | -59.97564** (9.97147) | 9.44953 (6.24554) |
| vet | -48.20026** (8.20204) | -3.09727 (2.58715) | 7.30146+ (3.82977) | 44.02481** (5.01692) | 9.90821** (2.47659) | -2.90823 (2.30940) | -20.79318** (6.57465) | 22.01209** (1.92869) |
| Sig Diff? | No | Yes | Yes | Yes | No | No | No | No |
| Observations | 9495 | 9495 | 9495 | 9495 | 9495 | 9495 | 9495 | 9495 |
| R-squared | 0.06 | 0.02 | 0.03 | 0.07 | 0.06 | 0.02 | 0.06 | 0.03 |

Dependent variables are annual amount of spending by payer type.

Robust standard errors in parentheses clustered on veteran and year.

Spending is measured in 2000\$.

Controls also include race, marital status, urban, age, state, year, income group and education dummies and a constant.

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

“Sig Diff?” reports whether the post*veteran coefficients for the two populations are statistically significantly different from one another at the 5% level.

Table 8. Utilization and Health By Eligibility Status

a. Newly Eligible

| | (1) | (2) | (3) | (4) | (5) |
|--------------|------------------------------------|-------------------------------------|--------------------------------------|-------------------------------------|------------------------------------|
| | Hospital Stays | Office Visits | Health0-1 | ActLimit0-1 | Died |
| postxvet | 0.00279 (0.02875) | -0.15099 (0.19466) | 0.05002** (0.00973) | -0.02290 (0.01424) | 0.01767 (0.01018) |
| vet | -0.01343 (0.01857) | 0.11012 (0.12552) | -0.00345 (0.00780) | 0.01087 (0.00842) | -0.00054 (0.00805) |
| Observations | 5262 | 5262 | 5244 | 5233 | 5262 |
| R-squared | 0.03 | 0.08 | 0.06 | 0.06 | 0.04 |

b. Previously Eligible

| | (1) | (2) | (3) | (4) | (5) |
|--------------|--------------------------------------|------------------------------------|--------------------------------------|---------------------------------------|------------------------------------|
| | HospStays | OfficeVisits | Health0-1 | ActLimit0-1 | Died |
| postxvet | -0.05698* (0.02500) | 0.13045 (0.07738) | 0.03160** (0.01026) | -0.05709** (0.00847) | 0.00335 (0.00598) |
| vet | 0.02793 (0.01877) | -0.59209** (0.06581) | -0.02184* (0.00826) | 0.05083** (0.00330) | 0.00546 (0.00563) |
| Sig Diff? | Yes | No | No | No | No |
| Observations | 9495 | 9495 | 9467 | 9403 | 9495 |
| R-squared | 0.03 | 0.05 | 0.05 | 0.05 | 0.03 |

Results from estimating equation (1) by OLS.

Health is an indicator = 1 if individual is in excellent, very good or good health.

ActLimit is an indicator = 1 if individual reports that health limits social activity.

Robust standard errors in parentheses are clustered on veteran and year.

Controls also include race, marital status, urban, age, state, year, income group and education dummies and a constant.

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

“Sig Diff?” reports whether the post*veteran coefficients for the two populations are statistically significantly different from one another at the 5% level.

Chapter 3

Alleviating Job-Lock? Evidence from a Public Health Care Expansion

(with Joanna N. Lahey, MIT)

3.1. Introduction

In the United States, unlike in other industrialized nations, the provision of health insurance is characterized by a pooling mechanism that ties insurance to employment. Over ninety percent of private health insurance in the U.S. is employer-provided (Gruber and Madrian, 2002). While firms provide a convenient means of grouping employed individuals in a manner not systematically related to health, it has long been a concern that tying health insurance to employment may have unintended negative consequences. If workers alter their labor supply and retirement decisions because of fears over losing health insurance coverage, or if firms are reluctant to hire certain types of workers (for example, older workers) because of expectations about these workers' insurance costs, inefficiencies may result. If health insurance does impede job mobility, a result that has been termed "job-lock," the most productive employer-employee matches may not be achieved.

Since the early 1990s, a large literature has emerged examining the economic consequences of tying health insurance to employment. While this literature has established a clear relationship between health insurance and labor supply choices, it has suffered from a particular limitation. Because such a substantial proportion of the group insurance market in the United States is tied to employment, it is difficult to find

individuals with outside sources of health insurance that are not in some way related to the individuals' employment decisions. Thus, while the consensus in the literature is that reductions in labor mobility result from tying health insurance to employment, the magnitude of this problem has yet to be clearly established.

In addition, the literature has not fully considered whether the introduction of government-sponsored health care may alleviate job-lock. The expansion of public health care programs in the United States will potentially affect the labor supply decisions of program beneficiaries. If workers have an alternative source of health care rather than depending solely on employer-provided insurance, their job mobility may increase.

This paper exploits a change in health care coverage for United States veterans to examine the impact of health insurance on labor supply. A major expansion in both the services offered and the population covered by the Department of Veterans Affairs health care system during the mid-1990s presents a unique opportunity to study the introduction of an exogenous source of health care coverage that is unrelated to employment. This setup both provides a means of cleanly identifying the extent of job-lock, and also demonstrates the potential labor supply effects of expanding other public health insurance programs.

3.2. Predicted Effects of Insurance on Labor Supply

If health insurance were a homogeneous good, firms providing such non-monetary compensation could uniformly reduce wages by the cost of the insurance for all workers choosing to accept such a benefit. In this case, workers could receive the same benefit at the exact same cost in any employment situation, and no labor market distortions would arise. In reality, however, insurance packages vary considerably across firms. This variation arises, in part, from differences across employers in the cost of providing health care coverage. These costs typically vary according to firms' experience ratings or projected future health expenditures, which depend on factors such as firm size and worker characteristics. Additionally, while health care costs vary substantially across individuals, employers only qualify for favorable tax treatment on health insurance if most workers within the firm are offered equivalent benefits packages. Firms are therefore typically constrained to offering the same package (at the same price) to all employees. For these reasons, health insurance benefits are often not comparable across firms, and workers may not switch to more productive employment situations because of preferences across insurance packages.

For prime-aged workers, health insurance therefore has the potential to impact labor supply in a number of ways. Reluctance to change jobs may result from preferences for the current employer's insurance benefits, and may also arise from fears about the potential effects of a temporary loss of insurance coverage. If coverage lapses while individuals are between jobs, this leaves workers vulnerable to pre-existing conditions exclusions. Additionally, some firms that provide employee health insurance

have a waiting period from the start of employment until the time that health insurance benefits become available.

In addition to slowing job mobility, these factors will likely reduce the number of individuals moving into self-employment (since insurance coverage purchased outside the group market is much more expensive). Workers may also be less likely to choose part-time work as a result of the tying of health insurance benefits to employment. Because most part-time jobs do not provide the same benefits as full-time jobs, workers may choose to work full-time to retain health care coverage, even if they would prefer to move into part-time work.

For older workers, health insurance has the same implications as described for younger individuals, and also potentially impacts retirement decisions. As workers age, there are two opposing influences which may affect such a decision. Because older individuals tend to encounter more health problems, work may become more difficult, strengthening the incentive to retire. At the same time, with the increased likelihood of declining health, these individuals may be more concerned about maintaining health insurance coverage in case of large medical expenses. In surveys, older individuals frequently state that they will postpone retirement until they become Medicare-eligible at age 65, even though they would actually prefer to retire earlier (Gruber & Madrian 2002). Additionally, older workers who wish to transition into retirement through part-time work or self-employment may be less likely to do so because they do not want to lose their employer-provided health insurance coverage.

Eligibility for public health care programs has the potential to alleviate many of the possible consequences described above. Public health care will provide beneficiaries

with coverage if they are between jobs or if they choose to move into part-time work or self-employment. It will allow individuals wishing to retire before age 65 to do so without sacrificing their health care coverage. Finally, depending on the generosity of a particular public plan, such insurance may be used to fill gaps in employer-provided plans, if an individual's optimal productivity match does not provide the preferred insurance package.

An extensive literature has examined the impact of employer-provided health insurance on the various labor supply decisions described above.²¹ Papers examining the impact on prime-aged workers often focus on married individuals (most often wives), and compare those with health insurance through their spouses' employers to those without. These studies generally find that the availability of spousal health insurance reduces labor force participation, with the estimated reduction typically falling between 6 and 12 percent.²² One difficulty with these papers, however, is the required assumption that spousal health insurance is exogenous to an individual's labor supply decisions. Gruber and Madrian (1997) overcome this difficulty by exploiting the introduction of continuation of coverage mandates. These state and federal laws require that employers offer employees the opportunity to continue to purchase health insurance through the employer's plan for up to 18 months after leaving the job.²³ Gruber and Madrian find that these mandates reduce labor force participation by around 15 percent.

²¹ For a complete review of the literature, see Gruber and Madrian (2002).

²² e.g. Buchmueller and Valetta (1999), Olson (1998), Schone and Vistnes (2000), Wellington and Cobb-Clark (2000)

²³ The federal law, commonly known as COBRA, was passed in 1986, and requires that individuals be allowed to purchase 18 months of coverage at the average group rate.

Studies that have examined the impact of public insurance on labor supply choices have, thus far, focused on the Medicaid program²⁴. The results of this literature are mixed, but in general seem to indicate that for low-income single mothers, the availability of public health care does not have much impact on labor supply. There are a number of difficulties with these studies however, and they cannot clearly identify the potential effects of other expansions in government health care for a number of reasons. First, the necessary focus on the Medicaid-eligible population – mainly low-income single mothers – makes the results less generalizable to other populations. Additionally, the historical tying of Medicaid benefits to cash welfare programs resulted in a unique incentive system under which it is extremely difficult to disentangle the effects of Medicaid and welfare.²⁵

A number of papers have also studied the impact of employer-provided health insurance on the retirement decision.²⁶ The majority of these studies suggest a significant effect of health insurance on retirement. Like the literature examining labor supply outcomes for prime-aged workers, the retirement literature has struggled with endogeneity issues. By utilizing continuation of coverage mandates as an exogenous form of outside health care coverage, Gruber and Madrian (1995, 1996) find that the retirement hazard increases by 30% when such coverage becomes available.

The COBRA mandates provided the opportunity to study an exogenous change in outside health insurance coverage, and papers which utilized this control group present

²⁴ For example, Winkler (1991), Moffit and Wolfe (1992), Yelowitz (1995), Yazici (1997), Ham and Shore-Sheppard (2000)

²⁵ Beginning in the 1980s, legislation was introduced to weaken the ties between the two programs. In 1996, the replacement of AFDC with TANF fully decoupled welfare and Medicaid.

²⁶ For example, Madrian (1994a), Karoly and Rogowski (1994), Hurd and McGarry (1996), Gustman and Steinmeier (1994), Rust and Phelan (1997), Blau and Gilleskie (2001).

the best evidence to date on the effects of health insurance on labor mobility. Even so, the continuation of coverage mandates suffer from two particular shortcomings for this purpose: their relatively short duration and high out-of-pocket costs. The opportunity to study a separate case of an exogenous health care benefit is therefore important not only because of the chance to confirm the results from the COBRA studies, but also to obtain a clearer picture of the magnitude of the effects. Such a case is provided by a radical change in the health care system of the U.S. Department of Veterans Affairs. As an added benefit, this case also mimics the expansion of other public health insurance programs, and therefore provides evidence on the impact of expanding public health care programs on U.S. labor markets.

3.3. Reforms in the VA Health Care System

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.²⁷

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.

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. 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

²⁷ In a capitated payment system, the health care provider is reimbursed a flat dollar amount for each patient regardless of the services 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 employer-provided health insurance on labor supply. More generally, this policy change allows us to investigate the effects of increasing the scope and availability of public health insurance programs on individuals' labor supply choices.

3.4. Data and Empirical Model

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 and very young veterans in the data, we restrict our sample to include all surveyed males age 25 and over. The treated population is therefore male veterans age 25 and older, and the control group is male non-veterans over the age of 24. 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.

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.²⁸ We use a probit model to estimate the following equation:

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

where:

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

veteran_i = 1 if individual has been honorably discharged from active military duty, 0 otherwise

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

δ_t = year dummies

and,

μ_{it} = a random error term.

Summary statistics are shown in Table 1. Comparing these statistics for the veteran and non-veteran populations reveals that the veteran population is older than the non-veteran population and is aging more rapidly. For this reason, we include an age* veteran interaction term in the regressions, allowing age to enter separately for the two populations.²⁹ The age difference likely accounts for at least some of the differences in

²⁸ 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 on the previous year) also produce results that are qualitatively similar, although of smaller magnitude.

²⁹ One concern with this 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.

average characteristics between the two groups. Veterans are more likely to be retired or not working and more likely to be married.

3.5. Results

3.5.1. Effects on Labor Force Participation

We begin by examining the effects of the VA policy change on individuals' decisions about whether to participate in the labor force. As mentioned above, the previous literature has found that tying employment to health insurance results in lower retirement rates and less labor mobility. We therefore expect the public insurance expansion implemented by VA to result in a lower probability of labor force participation by veterans.

Table 2 reports results for the labor force participation of both age groups. Reported coefficients 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 employer. Results are similar with and without these controls, although the magnitude of the coefficient of interest (the coefficient on veteran*post) is generally slightly smaller when employer characteristics are included.

As expected, results consistently show that individuals are more likely to move into not working as a result of the VA policy change. As a result of gaining VA

coverage, the probability of working drops by .34 percentage points for a prime-aged worker with average characteristics and by 2.43 percentage points for the average older worker. Relative to the pre-period veteran averages for these two groups, this is about a 4% increase in the probability that a prime-aged worker leaves employment, and 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.³⁰ 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.

The results in Table 2 demonstrate a clear relationship between employer-provided health insurance and the choice to participate in the labor market. They also indicate that expanding public insurance programs is likely to lead to earlier retirements and more labor mobility. We next turn to estimating the effect of health insurance on job choice, by examining movements into self-employment and part-time work.

³⁰ 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.

3.5.2. *Effects on Job Choice*

Because self-employed workers face much higher insurance costs than those whose insurance is employer-provided and purchased in a group market, individuals who may otherwise wish to move into self-employment may be reluctant to leave jobs with employer-provided insurance. A small number of papers have examined whether this is in fact the case, although the results have been mixed. Holtz-Eakin, Penrod and Rosen (1996) find no effect of employer-provided health insurance on the probability of moving from employment into self-employment, but Madrian and Lefgren (1998) find some evidence that the availability of outside coverage increases movement into self-employment. As reported in Table 3, we find that for prime-aged workers, the probability that the average veteran is self-employed increases by .14 percentage points, an increase of 3.2% relative to the pre-period mean for veterans in this age group. Therefore, as expected, it appears that individuals in this age group are more willing to move into self-employment once relinquishing employer-provided health insurance no longer implies paying high insurance costs out of pocket. At the same time, the sign on the coefficient of interest for older workers is the opposite of expected and, when controls are included for employer characteristics last year, highly significant. Thus, it appears that as a result of the policy change, the average veteran between the ages of 55 and 64 is actually less likely to become self-employed. One possibility is that individuals in this age group were previously using self-employment as a bridge to retirement, so that they could continue to afford medical care expenses until becoming Medicare-eligible at age 65. Since the rate of retirement increases for this group as a result of the policy change, a

potential explanation for these unexpected results could be a substitution of early retirement for self-employment.

In addition to studying the impact of public insurance availability on the probability of self-employment, we also examine the effects of insurance on part-time work. Because most part-time jobs do not provide workers with benefits such as health insurance, workers who place a high value on these benefits may avoid moving into part-time work in order to maintain their health insurance coverage. In surveys, older workers often state that they would prefer to transition into retirement by moving first to part-time employment (Abraham & Houseman 2004). If moving to part-time work means losing health insurance, however, older workers may be reluctant to do so. COBRA may aid in such transitions for individuals within 18 months of attaining Medicare coverage, but COBRA will still not alleviate the full extent of job lock, both because of its high out-of-pocket costs and limited duration. The literature examining the labor supply decisions of prime-aged, married women tests the impact of spousal coverage on the decision to work part time. As mentioned earlier, however, these tests suffer from potential endogeneity bias, since the labor supply decision by husbands and wives is likely to be a joint consideration. Buchmueller and Valletta (1999) find that spousal insurance increases the probability of working in a part-time job by 2.8 to 3.3 percentage points.

Table 4 reports our estimates of the impact of employer-provided insurance on the probability of working part-time. In these regressions, the sample is restricted to individuals employed in the current year. Controls for employer characteristics are therefore current year controls (as opposed to controls for the previous year, as in the regressions discussed previously). Additionally, we control for whether an employer

offers pensions and health insurance, as opposed to whether an individual receives such benefits, because of the fact that many individuals may lose these benefits if they move from full-time to part-time work. As predicted, we find that the average veteran is more likely to work part-time as a result of gaining outside health insurance coverage. For older workers, we estimate a 1.2 percentage point increase in the probability of working part-time, which is a 6.6% increase relative to the pre-period veteran average. Our result for prime-aged workers is also positive and highly significant, but considerably smaller. This result is not surprising, however, since the sample is restricted to men. While the previous literature has found fairly large effects for married women, these individuals are also typically found to have a higher elasticity of labor supply than males. Since prime-aged men are less likely to be secondary earners than prime-aged women, it therefore makes sense that they would be less likely to work part time.

3.5.3. Which Veterans Are Affected?

The previous results consider the effects of the VA policy change on the labor supply of all veterans in particular age groups. We now turn to examining the effects on specific segments of the veteran population, to investigate whether certain groups of veterans are impacted differentially. We test for differences in the impact on low-income and high-income veterans, married and single veterans, and veterans with and without employer-provided health insurance.

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. There appears to be no effect for low-income veterans on the probability of transitioning to not working, while the effect is strong and positive for higher-income individuals. At the same time, however, 55 to 64 year old veterans in both groups are significantly more likely to retire as a result of the policy change, and the magnitude of the effect is not significantly different across the two groups. Effects on the probability of moving into self-employment or working part-time are similar in most cases for low- and high-income veterans. One exception is that prime-aged low-income veterans are much more likely to move into part-time work as a result of the policy change than their wealthier counterparts, a difference which is significant at the 6% level. This could be, in part, because these individuals are more likely to be disabled, and to therefore desire shorter work hours once they do not need to rely on their employer for insurance.

Table 6 reports results for married and single veterans. VA health care covers only the veteran and not the veteran's spouse or dependents.³¹ For this reason, married veterans may still be job-locked if their spouse depends on health insurance provided

³¹ 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.

through the veteran's employer.³² We therefore expect to find stronger effects of the availability of this public insurance on single veterans than married. As reported in the table, however, this is not always the case. Married veterans are significantly more likely to move into not working or retirement than single veterans. These effects are large, positive and highly significant for married veterans, while the coefficients on veteran*post are insignificant for singles when the outcome is not working, and become significant and negative (although very small) for the retired outcome. The effect of the policy change on the probability of being self-employed is not significantly different across the two groups at either age. Married and single prime-aged workers are also equally likely to move into part-time work, but effects on this outcome are very different for married and single workers approaching the normal retirement age. For older, married workers, the probability of working part time increases by 1.7 percentage points as a result of the policy change. Older, single workers on the other hand, are significantly less likely to be working part time. The probability that these workers hold part-time positions drops by 1.83 percentage points.

The single versus married results are puzzling but have several possible explanations. One factor of importance may be the relationship between marital status and household income. As these characteristics are likely to be highly correlated, we may be picking up effects that are not directly related to marital status but rather to wealth. Single veterans may also differ systematically from married veterans according to other unobservable characteristics. For example, these individuals may have different tastes for work than married veterans who are more likely to have families (Coile 2003).

³² It is important to note, however, that a substantial number of married veterans in our sample have working spouses who are more likely to have their own health insurance coverage. Approximately 74% of prime-aged veterans and 57% of older veterans in the sample have wives who are employed.

Finally, we cut our sample by individuals who had employer-provided health insurance last year and those that did not. For individuals without employer-provided health insurance, there are not the same potential job-lock effects as for those who have this benefit. At the same time, individuals without group coverage through an employer are still indirectly affected by the U.S. convention of tying insurance to employment, as they pay higher costs to insure out-of-pocket in the individual market than their counterparts with group insurance. Workers without employer-provided insurance may alter their labor supply choices when they become eligible for VA care, because they no longer need to work as much in order to pay for health insurance or out-of-pocket medical expenses. Thus, eligibility for public health insurance may still affect employment behavior for individuals without employer-provided coverage, even though this is not the same job-lock effect typically examined in the literature. It is therefore not clear, a priori, whether to expect significantly different effects across the two insurance status groups.

Table 7 reports results by employer-provided health insurance status in the previous year. For older workers, there is not a statistically significant difference by insurance status in the probability of moving into retirement or not working. There is also no significant difference across the two groups of older workers in the probability of being self-employed. Older workers without employer-provided health insurance are, however, significantly more likely to be working part-time as a result of the policy change than older workers with a health insurance benefit. For the insured group, the coefficient on *post*vet* is positive but small and insignificant for the self-employed

outcome, while this same coefficient is large, positive, and highly significant for the group without employer-provided health insurance.

For prime-aged workers, there do appear to be differential effects by employer-provided insurance status on both not working and self-employment. The probability that workers in this age group are not working increases by 2 percentage points for those without employer provided insurance, but does not change significantly for those with insurance coverage through their jobs. Workers without insurance coverage are more likely to be self-employed as a result of gaining VA coverage – this probability increases by .2 percentage points. For those with insurance, this coefficient is positive but is extremely small and not highly significant. Finally, for prime-aged workers, there is an increase in the probability of working part-time as a result of the policy change, and this effect is not significantly different by insurance status. It therefore appears overall that the effects of gaining public insurance on labor supply are at least as strong if not stronger for those with no previous insurance coverage through an employer as for those who do have coverage from their place of employment. These results must be interpreted with caution, however, because of potential selection issues. Workers who remain in jobs with health insurance may be less sensitive to the incentives from the policy change than those that do not. For this reason, it would be more ideal to cut the sample according to health insurance status prior to 1996. Since this information is not available, however, we are only able to base our samples on health insurance status in the previous year.

3.6. Conclusion

This paper demonstrates a strong relationship between health insurance and labor supply choices. As with the literature that examines the introduction of COBRA and other continuation of coverage mandates, we utilize an exogenous change in outside health insurance status to show that tying health insurance to employment reduces job mobility, resulting in potential inefficiencies in the labor market. By utilizing a major organizational change in VA health care, we are also able to estimate the effects of expanding public health insurance availability on labor supply choices.

Our results demonstrate a significant effect of public health insurance on work decisions. We find particularly strong results for those workers in the 55-64 year old age group, who are approaching the normal retirement age. For this age group, our results suggest a positive and significant increase in early retirement with the availability of outside health care coverage. Our effects appear smaller than those found by Gruber and Madrian (1995), which is likely at least partially explained by the different populations considered by the two studies.³³ In addition, our results can generally be considered to be a lower bound on the effects of other public insurance expansions on labor supply, because while the VA expansion potentially extends benefits to a huge population of individuals, only about 25% of eligibles expressed an interest in using the program during our study period.

For veterans in both age groups, effects on labor force participation appear slightly stronger for higher-income individuals who are more likely to be newly-eligible

³³ Because they are estimating the effects of continuation of coverage mandates, Gruber and Madrian restrict their sample to individuals with employer-provided health insurance in the previous period.

for VA care. Effects also appear somewhat stronger for married than for single veterans. Finally, the availability of public insurance affects labor supply choices of individuals with and without employer provided health care coverage.

Overall, our study confirms the job-lock effects of tying health insurance to employment, and suggests that public health insurance expansions have the potential to alleviate some of the reductions in job mobility caused by this type of health insurance regime. While the magnitudes of our results are relatively small, they are likely diluted because we consider the impact on the labor supply of all veterans, many of whom may never consider using the VA program. This evidence therefore suggests even larger potential for the alleviation of job-lock through publicly provided health care.

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Table 1A. Summary Statistics, CPS 1992-2002
(Sample restricted to men between 55 and 64 employed last year)

| | Veterans | | Non-Veterans | |
|-----------------------|-----------------|------------------|-----------------|-------------------|
| | Pre (N=7775) | Post (N=8242) | Pre (N=6258) | Post (N=10820) |
| age | 59.365 | 58.843 | 58.480 | 58.662 |
| married | 0.813 | 0.804 | 0.803 | 0.791 |
| white | 0.927 | 0.907 | 0.845 | 0.844 |
| no hs | 0.142 | 0.062 | 0.294 | 0.209 |
| hs | 0.352 | 0.352 | 0.310 | 0.298 |
| some coll | 0.240 | 0.294 | 0.151 | 0.178 |
| coll grad | 0.160 | 0.171 | 0.112 | 0.152 |
| grad sch | 0.106 | 0.120 | 0.133 | 0.163 |
| pension plan | 0.480 | 0.545 | 0.448 | 0.490 |
| included in pen plan | 0.429 | 0.487 | 0.403 | 0.439 |
| inc. in emp HI plan | 0.628 | 0.653 | 0.583 | 0.596 |
| Northeast | 0.236 | 0.219 | 0.259 | 0.226 |
| Midwest | 0.257 | 0.247 | 0.246 | 0.231 |
| South | 0.286 | 0.278 | 0.292 | 0.303 |
| West | 0.222 | 0.257 | 0.204 | 0.240 |
| not working | 0.251 | 0.224 | 0.227 | 0.199 |
| part time employed* | 0.182 | 0.175 | 0.171 | 0.155 |
| retired** | 0.163 | 0.148 | 0.119 | 0.107 |
| self-employed | 0.039 | 0.040 | 0.033 | 0.039 |
| Occupations: | | | | |
| prof/manag | 0.254 | 0.267 | 0.249 | 0.288 |
| tech/sales/clerical | 0.175 | 0.174 | 0.143 | 0.139 |
| service | 0.065 | 0.069 | 0.082 | 0.077 |
| farming | 0.041 | 0.030 | 0.058 | 0.048 |
| craftsman | 0.135 | 0.140 | 0.132 | 0.127 |
| operator | 0.136 | 0.127 | 0.163 | 0.151 |
| Industries: | | | | |
| agriculture/mining | 0.045 | 0.034 | 0.060 | 0.053 |
| construction | 0.068 | 0.071 | 0.079 | 0.082 |
| manufacturing | 0.159 | 0.145 | 0.181 | 0.149 |
| transport/commun | 0.078 | 0.098 | 0.064 | 0.067 |
| trade | 0.136 | 0.125 | 0.149 | 0.131 |
| financial/real estate | 0.050 | 0.053 | 0.046 | 0.050 |
| business/repair | 0.045 | 0.051 | 0.045 | 0.055 |
| personal | 0.033 | 0.028 | 0.031 | 0.038 |
| public | 0.052 | 0.066 | 0.032 | 0.033 |
| professional | 0.139 | 0.134 | 0.140 | 0.173 |

*Part-time statistics are based on being currently employed. There are 3712 observations for pre-veterans, 8181 post-veterans, 3407 pre-non-veterans, and 11225 post-non-veterans.

** Number of observations for Retired is 3678 for pre-veterans and 3233 for pre-non-veterans, because variable does not exist for 1992-1993.

Table 1B. Summary Statistics, CPS 1992-2002
(Sample restricted to men between 25 and 51 employed last year)

| | Veterans | | Non-Veterans | |
|-----------------------|------------------|-------------------|------------------|-------------------|
| | Pre (N=19091) | Post (N=16199) | Pre (N=74844) | Post (N=99620) |
| age | 41.507 | 41.395 | 36.091 | 37.683 |
| married | 0.699 | 0.669 | 0.630 | 0.627 |
| white | 0.884 | 0.845 | 0.865 | 0.859 |
| no hs | 0.047 | 0.033 | 0.131 | 0.125 |
| hs | 0.379 | 0.371 | 0.326 | 0.321 |
| some coll | 0.355 | 0.396 | 0.246 | 0.256 |
| coll grad | 0.144 | 0.142 | 0.198 | 0.206 |
| grad sch | 0.075 | 0.057 | 0.099 | 0.092 |
| pension plan | 0.615 | 0.662 | 0.537 | 0.580 |
| included in pen plan | 0.536 | 0.566 | 0.453 | 0.495 |
| inc. in emp HI plan | 0.647 | 0.673 | 0.612 | 0.626 |
| Northeast | 0.197 | 0.169 | 0.242 | 0.212 |
| Midwest | 0.249 | 0.248 | 0.240 | 0.241 |
| South | 0.308 | 0.317 | 0.286 | 0.284 |
| West | 0.246 | 0.266 | 0.232 | 0.264 |
| not working | 0.092 | 0.082 | 0.080 | 0.064 |
| part time employed* | 0.116 | 0.104 | 0.117 | 0.106 |
| retired** | 0.005 | 0.005 | 0.001 | 0.002 |
| self-employed | 0.044 | 0.043 | 0.037 | 0.032 |
| Occupations: | | | | |
| prof/manag | 0.241 | 0.237 | 0.271 | 0.289 |
| tech/sales/clerical | 0.202 | 0.185 | 0.192 | 0.177 |
| service | 0.095 | 0.102 | 0.086 | 0.087 |
| farming | 0.023 | 0.018 | 0.040 | 0.036 |
| craftsman | 0.208 | 0.218 | 0.190 | 0.194 |
| operator | 0.189 | 0.189 | 0.190 | 0.182 |
| Industries: | | | | |
| agriculture/mining | 0.033 | 0.028 | 0.049 | 0.044 |
| construction | 0.098 | 0.106 | 0.111 | 0.123 |
| manufacturing | 0.204 | 0.187 | 0.207 | 0.185 |
| transport/commun | 0.135 | 0.140 | 0.085 | 0.089 |
| trade | 0.144 | 0.138 | 0.183 | 0.180 |
| financial/real estate | 0.040 | 0.037 | 0.048 | 0.049 |
| business/repair | 0.064 | 0.073 | 0.066 | 0.079 |
| personal | 0.026 | 0.031 | 0.036 | 0.038 |
| public | 0.095 | 0.093 | 0.045 | 0.044 |
| professional | 0.121 | 0.117 | 0.139 | 0.133 |

*Part-time statistics are based on being currently employed. There are 9393 observations for pre-veterans, 17040 post-veterans, 40197 pre-non-veterans, and 109688 post-non-veterans.

** Retired information is not available for 1992 and 1993. Therefore there are 8948 veterans and 37064 non-veterans in the pre-period.

Table 2. Not Working and Retired Outcomes by Age Group

| | (1) | (2) | (3) | (4) | (5) | (6) |
|-----------------------|---------------------------------------------|---------------------------------------------|---------------------------------------------|---------------------------------------------|--------------------------------------|--------------------------------------|
| | Not Working, Employed Last Yr., 55-64 | Not Working, Employed Last Yr., 55-64 | Not Working, Employed Last Yr., 25-51 | Not Working, Employed Last Yr., 25-51 | Retired, Employed Last Yr., 55-64 | Retired, Employed Last Yr., 55-64 |
| veteran | 0.0281 (0.0224) | 0.0125 (0.0164) | 0.0114 (0.0086) | 0.0116+ (0.0061) | 0.0199+ (0.0106) | 0.0123** (0.0038) |
| veteran x post | 0.0184** (0.0061) | 0.0243** (0.0042) | 0.0061* (0.0026) | 0.0034* (0.0015) | 0.0141** (0.0032) | 0.0038** (0.0012) |
| married | 0.1099** (0.0036) | 0.0251** (0.0046) | -0.0160** (0.0020) | -0.0133** (0.0011) | 0.0763** (0.0039) | 0.0143** (0.0026) |
| non-white | -0.0494** (0.0067) | -0.0224** (0.0067) | -0.0414** (0.0029) | -0.0185** (0.0022) | -0.0172* (0.0073) | 0.0018 (0.0035) |
| pension | | -0.1327** (0.0055) | | -0.0413** (0.0010) | | -0.0386** (0.0025) |
| healthins | | -0.0374** (0.0082) | | -0.0487** (0.0022) | | 0.0165** (0.0015) |
| industry/occ? | No | Yes | No | Yes | No | Yes |
| Obs | 32721 | 32721 | 207611 | 207611 | 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. Self-Employed Given Employed Last Year

| | (1) | (2) | (3) | (4) |
|----------------|------------------------------------|-------------------------------------|------------------------------------|------------------------------------|
| | 55-64 | 55-64 | 25-51 | 25-51 |
| veteran | -0.0043 (0.0093) | -0.0012 (0.0026) | -0.0016 (0.0045) | -0.0020* (0.0010) |
| veteran x post | -0.0030+ (0.0016) | -0.0014** (0.0005) | 0.0046** (0.0013) | 0.0014** (0.0004) |
| married | -0.0040 (0.0026) | -0.0008 (0.0007) | -0.0001 (0.0006) | -0.0011** (0.0002) |
| non-white | -0.0087** (0.0031) | -0.0012 (0.0008) | -0.0110** (0.0012) | -0.0028** (0.0003) |
| pension | | 0.0135** (0.0017) | | 0.0060** (0.0005) |
| healthins | | 0.0040** (0.0007) | | 0.0038** (0.0004) |
| industry/occ? | No | Yes | No | Yes |
| Obs | 32721 | 32721 | 207611 | 207611 |

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. Part-Time Work if Employed

| | (1) | (2) | (3) | (4) |
|-----------------------|-----------------------------------|------------------------------------|----------------------------------|------------------------------------|
| | 55-64 | 55-64 | 25-51 | 25-51 |
| veteran | 0.0070 (0.0090) | 0.0060 (0.0080) | -0.0082+ (0.0046) | -0.0052 (0.0035) |
| veteran x post | 0.0104* (0.0043) | 0.0118** (0.0042) | 0.0032 (0.0020) | 0.0043** (0.0015) |
| married | -0.0286** (0.0052) | -0.0151** (0.0040) | -0.0392** (0.0012) | -0.0241** (0.0010) |
| non-white | 0.0036 (0.0060) | 0.0117* (0.0050) | -0.0159** (0.0015) | -0.0065** (0.0011) |
| pension | | -0.0487** (0.0034) | | -0.0119** (0.0009) |
| healthins | | -0.0450** (0.0041) | | -0.0472** (0.0011) |
| industry/occ? | No | Yes | No | Yes |
| Obs | 32102 | 32102 | 218928 | 218928 |

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

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|----------------|---------------------------------------------|---------------------------------------------|-----------------------------------------|-----------------------------------------------|-----------------------------------------------|-------------------------------------------|-------------------------------------------|
| | Not Working, Employed Last Yr., 55-64 | Not Working, Employed Last Yr., 25-51 | Retired, Employed Last Yr., 55-64 | Self-Employed, Employed Last Yr., 55-64 | Self-Employed, Employed Last Yr., 25-51 | Part-time, Employed this Yr., 55-64 | Part-time, Employed this Yr., 25-51 |
| veteran | 0.0104 (0.0157) | 0.0092* (0.0046) | 0.0110** (0.0042) | -0.0021 (0.0033) | -0.0019+ (0.0011) | 0.0012 (0.0083) | 0.0008 (0.0036) |
| veteran x post | 0.0275** (0.0042) | 0.0048** (0.0016) | 0.0043** (0.0012) | -0.0016** (0.0006) | 0.0012** (0.0004) | 0.0099** (0.0037) | 0.0012 (0.0009) |
| Obs | 27677 | 16775 | 21781 | 27677 | 16775 | 27554 | 176316 |

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|----------------|---------------------------------------------|---------------------------------------------|-----------------------------------------|-----------------------------------------------|-----------------------------------------------|-------------------------------------------|-------------------------------------------|
| | Not Working, Employed Last Yr., 55-64 | Not Working, Employed Last Yr., 25-51 | Retired, Employed Last Yr., 55-64 | Self-Employed, Employed Last Yr., 55-64 | Self-Employed, Employed Last Yr., 25-51 | Part-time, Employed this Yr., 55-64 | Part-time, Employed this Yr., 25-51 |
| veteran | 0.0242 (0.0568) | 0.0389 (0.0339) | 0.0203 (0.0155) | 0.0017 (0.0041) | -0.0020* (0.0009) | 0.0636+ (0.0372) | -0.0480** (0.0133) |
| veteran x post | -0.0189 (0.0205) | -0.0074 (0.0071) | 0.0069+ (0.0038) | -0.0019 (0.0016) | 0.0011* (0.0004) | 0.0054 (0.0166) | 0.0185* (0.0079) |
| Sig Diff? | Yes | Yes | No | No | No | No | No |
| Obs | 5044 | 39836 | 3885 | 2907 | 39836 | 4548 | 42612 |

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. "Sig Diff?" 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) | (5) | (6) | (7) |
|-----------------------|---------------------------------------------|---------------------------------------------|-----------------------------------------|-----------------------------------------------|-----------------------------------------------|-------------------------------------------|-------------------------------------------|
| | Not Working, Employed Last Yr., 55-64 | Not Working, Employed Last Yr., 25-51 | Retired, Employed Last Yr., 55-64 | Self-Employed, Employed Last Yr., 55-64 | Self-Employed, Employed Last Yr., 25-51 | Part-time, Employed this Yr., 55-64 | Part-time, Employed this Yr., 25-51 |
| veteran | 0.0039 (0.0216) | 0.0068 (0.0055) | 0.0122* (0.0052) | -0.0015 (0.0022) | -0.0013 (0.0010) | -0.0013 (0.0093) | 0.0004 (0.0042) |
| veteran x post | 0.0325** (0.0042) | 0.0057** (0.0015) | 0.0079** (0.0015) | -0.0017** (0.0007) | 0.0011* (0.0005) | 0.0174** (0.0045) | 0.0035* (0.0015) |
| Obs | 26221 | 132607 | 20528 | 26221 | 132607 | 26760 | 152739 |

b. Single

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|-----------------------|---------------------------------------------|---------------------------------------------|-----------------------------------------|-----------------------------------------------|-----------------------------------------------|-------------------------------------------|-------------------------------------------|
| | Not Working, Employed Last Yr., 55-64 | Not Working, Employed Last Yr., 25-51 | Retired, Employed Last Yr., 55-64 | Self-Employed, Employed Last Yr., 55-64 | Self-Employed, Employed Last Yr., 25-51 | Part-time, Employed this Yr., 55-64 | Part-time, Employed this Yr., 25-51 |
| veteran | 0.0228 (0.0234) | 0.0209 (0.0173) | 0.0050 (0.0074) | 0.0010 (0.0057) | -0.0030+ (0.0016) | 0.0568** (0.0209) | -0.0274** (0.0065) |
| veteran x post | 0.0055 (0.0070) | -0.0043 (0.0033) | -0.0061* (0.0026) | 0.0016 (0.0022) | 0.0013* (0.0006) | -0.0183* (0.0093) | 0.0076* (0.0038) |
| Sig Diff? | Yes | Yes | Yes | No | No | Yes | No |
| Obs | 6500 | 75004 | 5138 | 5909 | 75004 | 5342 | 66189 |

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. "Sig Diff?" 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 Health Insurance Status

| a. Employer-Provided Health Insurance Last Year | | | | | | | |
|--------------------------------------------------------|---------------------------------------------|---------------------------------------------|-----------------------------------------|-----------------------------------------------|-----------------------------------------------|-------------------------------------------|-------------------------------------------|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| | Not Working, Employed Last Yr., 55-64 | Not Working, Employed Last Yr., 25-51 | Retired, Employed Last Yr., 55-64 | Self-Employed, Employed Last Yr., 55-64 | Self-Employed, Employed Last Yr., 25-51 | Part-time, Employed this Yr., 55-64 | Part-time, Employed this Yr., 25-51 |
| veteran | 0.0129 (0.0148) | 0.0187** (0.0042) | 0.0060 (0.0077) | -0.0032 (0.0052) | -0.0029+ (0.0018) | 0.0132 (0.0097) | 0.0002 (0.0024) |
| veteran x post | 0.0195** (0.0049) | -0.0018 (0.0013) | 0.0037* (0.0018) | -0.0027** (0.0008) | 0.0009+ (0.0005) | 0.0003 (0.0024) | 0.0023* (0.0011) |
| Obs | 20092 | 129917 | 15822 | 20092 | 129917 | 21857 | 144906 |

| b. No Employer-Provided Health Insurance Last Year | | | | | | | |
|-----------------------------------------------------------|---------------------------------------------|---------------------------------------------|-----------------------------------------|-----------------------------------------------|-----------------------------------------------|-------------------------------------------|-------------------------------------------|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| | Not Working, Employed Last Yr., 55-64 | Not Working, Employed Last Yr., 25-51 | Retired, Employed Last Yr., 55-64 | Self-Employed, Employed Last Yr., 55-64 | Self-Employed, Employed Last Yr., 25-51 | Part-time, Employed this Yr., 55-64 | Part-time, Employed this Yr., 25-51 |
| veteran | 0.0080 (0.0276) | -0.0083 (0.0184) | 0.0240* (0.0097) | 0.0000 (0.0025) | -0.0004 (0.0008) | -0.0162 (0.0216) | -0.0244* (0.0101) |
| veteran x post | 0.0276** (0.0077) | 0.0195** (0.0064) | 0.0040* (0.0017) | -0.0002 (0.0011) | 0.0019* (0.0008) | 0.0394** (0.0124) | 0.0102+ (0.0060) |
| Sig Diff? | No | Yes | No | No | Yes | Yes | No |
| Obs | 12629 | 77694 | 9844 | 9338 | 77694 | 10245 | 74022 |

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 men with employer-provided health in the year prior to the survey. The universe in panel b is restricted to men without employer-provided health in the year prior to the survey. 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. "Sig Diff?" 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%