LABOR MARKET EFFECTS OF EMPLOYMENT-BASED HEALTH INSURANCE

by

Brigitte Condie Madrian

Submitted to the Department of Economics
in Partial Fulfillment of the Requirements
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ABSTRACT

This dissertation assesses the impact of employer-provided health insurance on the labor market behavior of individuals. Specifically, I examine the effect of health insurance on both the turnover decisions of younger workers and the retirement decisions of older workers. Chapter 1 explores the extent to which workers are "locked" into their current jobs because the prevalence of preexisting conditions exclusions may make it very expensive for individuals with medical problems to give up the insurance they have on their current job. Job-lock should only affect those who have health insurance, and the effect should be greater for those who have high expected medical expenses. Therefore, if job-lock is significant, the difference in mobility rates between those with high and low expected medical expenses should be greater for those with employer-provided health insurance than for those without it.

I consider three different "experimental" groups: married men who have an alternative source of coverage in addition to employer-provided health insurance, heads of large families who are more likely to have high expected medical expenses simply because of the size of their family, and married men whose wives are pregnant. Using data from the 1987 National Medical Expenditure Survey I estimate a probit model of voluntary mobility. My results suggest that insurance-related job-lock reduces the voluntary turnover rate of those who have employer-provided health insurance by 25 percent, from 16 percent to 12 percent per year.

Chapter 2 examines whether those whose employers provide post-retirement health insurance retire earlier than those whose employers do not provide such insurance. Because individuals are not eligible for Medicare until age 65, they must often turn to the private market, where insurance is quite expensive, if they retire early and their employers do not

provide retiree health insurance. The lack of such insurance should therefore make early retirement much less attractive. Using data from the 1987 National Medical Expenditure Survey and the Survey of Income and Program Participation, I find that those with retiree health insurance retired 6-12 months earlier than those without such insurance. This result is robust to various definitions of retirement and to whether or not individuals receive a pension. It is less clear, however, whether this is the effect of actually having retiree health insurance or whether there is a correlation between firm provision of retiree health insurance and overall job quality, the generosity of pension benefits, and the incentives for early retirement that are a part of the pension plans in these firms.

Chapter 3, which was coauthored by Jonathan Gruber of MIT, also looks at the relationship between health insurance and retirement. In this paper, we consider the impact of state and federal "continuation of coverage" mandates which grant individuals the right to continue purchasing health insurance through a previous employer for a specified number of months after leaving the firm. We exploit variation in the timing and generosity of these laws to identify the effect of the availability of continuation coverage on retirement decisions. Using data on 55-64 year-old males from the Current Population Survey (CPS) and the Survey of Income and Program Participation (SIPP), we estimate that one year of continuation coverage raises retirement rates by over 15%. This finding is robust to a number of specification checks and is very similar in both data sets.

Thesis Supervisor: James M. Poterba
Title: Professor of Economics

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I vividly remember the afternoon over fourteen years ago when, as I sat in Mr. Day's 8th grade algebra class, I decided I would get a Ph.D. At the time, I could not have told you that it would be in economics, only that it would be a Ph.D. If I had known then what I now know, how much time and effort I would expend in meeting this goal, I might have been dissuaded. But the committment I made to myself that afternoon so many years ago has motivated me to press forward through those inevitable intervals of doubt and despair. Fortunately, it was not a journey I made alone.

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INTRODUCTION

It has been long recognized that health insurance distorts the demand for medical services. This dissertation explores two further margins along which health insurance may affect individual behavior: the decision to change jobs, and the decision about when to retire. These distortions arise from the current system of health insurance provision in the U.S. in which employers are the primary source of coverage for the nonelderly. As rising medical costs make health insurance an increasingly valuable component of employee compensation, we should expect it to be an important consideration in the labor market decisions of individuals. I find that health insurance has significant effects on both the turnover decisions of younger workers and the retirement decisions of older workers.

The outline of this introduction is as follows. Section I gives some background on the importance of employer-provided health insurance in the United States. This is followed in Section II by a brief summary of the results presented in the three chapters of this dissertation. Finally, Section III discusses the importance of my findings for the current policy debate over health care reform.

I. Background on Employer-Provided Health Insurance

The wide-spread availability of employment-based health insurance is a post-war phenomenon. The growth in its provision has been typically attributed to two factors. The first is the war-time adoption of wage and price controls which limited the ability of firms to directly increase the monetary compensation of their employees. This motivated employers to find alternative non-wage methods of compensation, including the provision of benefits such as pensions and health insurance, which were deemed excludable from taxable income in a 1943 IRS ruling. The second is the post-war expansion in both the tax base and marginal tax rates which served to increase the attractiveness of paying compensation in the form of benefits rather than wages even after wage and price controls had been abandoned.

The tax incentives to provide benefits are not inconsequential. They allow employers to provide a dollar's worth of compensation at a price of $1-\tau$ where τ is the marginal tax rate faced by the firm's employees. Although individuals would in general prefer to receive cash rather than in-kind income because they will not necessarily value benefits at the cost to the firm of providing them, cash income is taxable while benefits are not. Therefore, as long as the employee values the firm's expenditure of an additional dollar on benefits at more than $1-\tau$, the employee would prefer to be compensated with non-taxable benefits rather than with taxable income. In 1992, the federal tax expenditure generated by the deductibility of employer contribution for health insurance premiums and medical care was almost $40 billion.

While these tax benefits encourage the provision other benefits as well as health insurance, employers have two additional incentives to provide health insurance in particular. The first is that by pooling many individuals, they can reduce the risks of adverse selection which make purchasing individual health insurance in the private market quite expensive. The second is that they are able to realize savings on administrative costs through economies of scale. As shown in Table 1, these two factors together are estimated to reduce the cost of providing health insurance in large relative to small firms by close to 35%.

Given the substantial incentives that firms have to provide health insurance, how prevalent is such coverage? Table 2 illustrates the importance of employer-provided health insurance as a source of coverage. The first column shows, by age, what fraction of the population has any health insurance. The second column gives the fraction with private (not government-provided) health insurance, and the third column gives the fraction with employment-based health insurance. Overall, about two-thirds of the nonelderly population are covered by employer-provided health insurance, and this represents almost 95% of those with private health insurance.

Not only is employer-provided health insurance the predominant source of health insurance coverage that individuals actually receive, it also constitutes a sizeable component of their compensation as well. Employer expenditures on health insurance averaged $3605 per employee in 1991 (Employee Benefit Research Institute 1992), a figure which represents over 10% of median family income ($35,939) in 1991. Individuals place an even greater value on these benefits. When asked how much extra wage income they would have to receive in order to give up their employer-provided health insurance benefits, the mean response was $4,850, a number substantially greater than the actual cost to employers of providing these benefits (Employee Benefit Research Institute 1991). Because health insurance represents such a significant component of compensation, it should be an important parameter in the labor market decisions of individuals.

II. Health Insurance and the Labor Market

When considering the possible labor market effects of employer-provided health insurance, a natural case to consider for the sake of comparison is a world in which *nobody* is covered by employer-provided health insurance. Obviously, since health insurance and employment are completely divorced in this case, health insurance will have no impact on individual labor market decisions. Similarly, if everyone were covered by health insurance (employer-provided or otherwise) and this coverage was equivalent regardless of whether or where one worked, health insurance would have no influence on labor market decisions.

The potential labor market distortions arise from differences in the coverage that individuals receive based on where they are employed and whether or not they work at all that are not fully offset by differences in the wage payments that individuals receive. Some employers do not offer health insurance, and the policies offered by those employers who do provide health insurance differ along a variety of dimensions. The distortions generated by these differences in coverage are further exacerbated by the differential value that individuals place on health insurance coverage based on their perceived need for health insurance and how willing they are to take risks.

One key difference in coverage is whether or not a policy excludes preexisting conditions. These exclusions make changing the source of one's health insurance very costly if an individual (or one of his or her dependents) has a chronic medical condition which would be excluded. Chapters 1 and 3 discuss in more detail the nature of preexisting conditions exclusions, differences in individual versus group health insurance, and how the perceived compensation received by employees depends on the generosity of the coverage they receive and the value that they place on this coverage.

Preexisting conditions exclusions are often cited as one of the causes of "job-lock," a problem which I explore in Chapter 1. Job-lock is the tendency of individuals to stay in jobs they would really rather leave for fear of losing their health insurance coverage. While the popular press has on several occasions cited job-lock as a major problem with the current health care system, there is little empirical evidence on the magnitude of this problem. This is due in part to the difficulty of identifying exactly what job-lock is and when it occurs.

Chapter 1 uses three different empirical "experiments" to estimate the extent of job-lock. Although I cannot directly observe whether or not individuals are locked into their jobs, I can infer the extent of job-lock in the population as a whole by comparing the job turnover rates of those who are more likely to be affected by job-lock with the turnover rates of those who should not be affected by job-lock. The key identifying assumption is that job-lock should only affect those who have health insurance, and the effect should be greater for those who have high expected medical expenses. Therefore, if job-lock is significant, the difference in mobility rates between those with high and low expected medical expenses should be greater for those with employer-provided health insurance than for those without it.

To measure the extent of job-lock, I consider three different "experimental" groups. I first examine whether married men with a secondary source of coverage in addition to their own employer-provided health insurance have greater mobility rates than men whose sole source of coverage is through their employer. Because having an alternative source of coverage mitigates the costs of relinquishing health insurance upon changing jobs, we should not expect those individuals with two sources of coverage to be affected by job-lock. I then compare the mobility of workers with large families to that of workers with small families.

Heads of large families are likely to have higher expected medical expenses than those with small families and should therefore be less likely to change jobs, and this effect should be larger for those who have employer-provided health insurance than for those without it. Finally I look at married men whose wives are pregnant. Pregnancy is a preexisting condition which I can actually identify in my data, and it is a condition for which expected medical expenses are known and likely to be large. Among those with employer-provided health insurance, those men whose wives are expecting a child should be less likely to change jobs than other married men whose wives are not pregnant.

Using data from the 1987 National Medical Expenditure Survey, I find evidence of substantial insurance-related job-lock among all three groups. The increased likelihood of changing jobs from having other health insurance is 25 percent greater for those who have employer-provided health insurance than for those without employer-provided health insurance. Similarly, having a large family implies a 30 to 50 percent reduction in mobility rates for those with employment-based health insurance relative to those who do not have this type of health insurance. And finally, while having a wife who is pregnant increases mobility among those without employer-provided health insurance, it substantially reduces mobility for those with employer-provided health insurance. These results suggest that job-lock is an important consequence of the current system of predominantly employment-based health insurance.

An issue closely related to job-lock is how health insurance affects the retirement behavior of individuals. The underlying issues are the same--health insurance in the private market is much more expensive than the health insurance provided by employers, and individuals with preexisting conditions may find themselves unable to receive equivalent coverage if they retire from their job and give up the accompanying health insurance. The incentives facing older worker contemplating retirement are somewhat different, however, than those faced by younger workers changing jobs. First, all individuals become eligible for Medicare upon reaching age 65. Although Medicare is much less generous than most employer-provided policies, coverage is conditional only upon age and does not exclude preexisting conditions. The costs of relinquishing employer-provided health insurance are therefore diminished after reaching 65. Second, many employers provide post-retirement health insurance to their retirees. Thus, the possibility of losing health insurance coverage should not be a deterrent to retirement for individuals who work in firms that offer this type of coverage.

Chapter 2 examines whether the lack of employer-provided post-retirement health insurance induces individuals to postpone retirement. Using data from the 1987 National Medical Expenditure Survey and the Survey of Income and Program Participation, I find that those with retiree health insurance retired 6-12 months earlier than those without such insurance. This result is robust to various definitions of retirement and to whether or not individuals receive a pension. It is less clear, however, whether this is the effect of actually having retiree health insurance or whether there is a correlation between firm provision of retiree health insurance and overall job quality, the generosity of pension benefits, and the incentives for early retirement that are a part of the pension plans in these firms.

Chapter 2 also briefly addresses whether or not the spike in the retirement hazard at age 65 which has been only partially explained by previous research on the retirement effects of pension plans and Social Security can be explained by the availability of Medicare at age 65. If the spike at age 65 is due to Medicare eligibility, we would expect it to be larger for those without employer-provided retiree health insurance than for those with such insurance. I find little evidence that this is true.

Because the data I use in Chapter 2 do not enable me to completely disentangle the effects of pensions and health insurance, Chapter 3, written jointly with Jonathan Gruber, takes an alternative approach to examine the effect of health insurance on retirement. We use variation in state and federal "continuation of coverage" mandates which grant retirees (and other individuals) the right to continue purchasing health insurance through a previous employer for a specified number of months after leaving the firm to identify the effect of health insurance on retirement. These laws were passed at different times and in different states from the late 1970s through the mid 1980s before being federally mandated in 1986. By allowing individuals to continue coverage after retirement, they decrease the cost of early retirement for those who do not have retiree health insurance. We are able to separate the effect of health insurance from the effect of pensions by comparing the retirement behavior of individuals before and after the various law changes (our assumption is that the effect of pensions on retirement did not change in a way correlated with the passage of these laws).

Using data on 55-64 year-old males from the Current Population Survey and the Survey of Income and Program Participation, we estimate that one year of continuation coverage raises retirement rates by over 15%. This finding is robust to a number of specification checks and is very similar in both data sets. A somewhat surprising result, however, is that the effect appears to be uniform at all ages rather than larger near the age of Medicare eligibility. Because our model of how continuation coverage affects retirement does not predict that the effects at one age should be stronger or weaker than those at another, this result is not a contradiction of our theory, although it may seem somewhat counterintuitive.

The result that the effect of continuation coverage is roughly similar at all ages is also consistent with the finding in Chapter 2 that Medicare does not seem to account for the spike in the retirement hazard at age 65. That Medicare does not appear to affect individual retirement behavior may be construed as evidence that health insurance is not an important parameter in the retirement decision. However, it may also be evidence that Medicare is a vastly inferior source of health insurance and therefore does not affect retirement even though the availability of more generous health insurance might. While the average monthly individual premiums for employer-provided health insurance and Medicare Supplementary Medical Insurance are about equal,[1] the coverage provided by Medicare is much less generous. Only 5% of employees in medium and large firms faced a deductible of more than $300 in 1988; in contrast, the combined Medicare inpatient hospital and SMI deductible was $615. Similarly, 69% of those in medium and large firms have their annual out-of-pocket expenses limited to less than $1500 while Medicare has no out-of-pocket maximum. Medicare also excludes coverage for prescription drugs, something that is covered by 87% of employer-provided health insurance plans.

III. Policy Implications

The results of this dissertation strongly suggest that the coupling of health insurance with employment leads to labor market distortions. Individuals are both less likely to change

[1] In 1988, the average employee contribution required for individual coverage was $19.29 while the Medicare SMI premium was $24.80 (Employee Benefit Research Institute 1992).

jobs and less likely to retire when doing so entails a reduction in the extent of their health insurance coverage. Several proposals have been advanced to expand the scope of health insurance coverage in the U.S. Some of these remove the link between health insurance and employment, while others serve to strengthen it. The relative merits of these plans depend in part on how they affect the labor market behavior of individuals.

The only way to completely eliminate the labor market distortions generated by employment-based health insurance is to sever the link between employment and health insurance. Two proposals have been advanced which would do this. The first is a Canadian-style national health care system in which the government would insure everybody publicly and set the budgets and fees for hospitals, physicians, and other health care providers (Grumbach et al. 1991). The second would have the government auction off the right to insure large groups of the population to insurance companies on the condition that these insurers provide a minimum set of benefits (Diamond 1992). Under both of these proposals, everybody would receive continuous and unconditional medical care.

Because an individual's coverage would not depend on whether or where he or she was employed, the problem of job-lock would be completely eliminated. Among those currently without employer-provided retiree health insurance, the retirement hazard will increase and the average age at retirement will fall. The finding that one year of continuation coverage increases the retirement hazard by roughly 15% suggests that giving what is essentially unlimited coverage will have much greater effects on the retirement hazard. Using the results in Chapter 2 that employer-provided retiree health insurance decreases the average age at retirement by one year, I estimate that these proposals would decrease the labor force participation rate of 60-64 year-olds by 7.7%.

A second type of reform proposal calls for mandating that employers either provide health insurance for their employees or pay taxes to support government-sponsored health care for the uninsured (Rockefellar 1991; Bronow et al. 1991). These proposals also typically call for eliminating preexisting conditions exclusions, a move which would supposedly reduce job-lock. However, this effect is uncertain as many small employers would likely cease to offer health insurance absent an incentive for individuals with medical problems to seek employment elsewhere. The effect on job-lock resulting from the transfer of individuals from employer-provided to government-sponsored health insurance will depend on the relative quality of the two insurance programs. If the generosity of Medicare is any indication of the benefits that would be provided by the government, it is not clear that a proposal which guarantees coverage through either an individual's employer or the government will eliminate job-lock.

The effects of mandated benefits on retirement are also uncertain as these proposals do not typically address coverage of early retirees not yet eligible for Medicare. If the government also mandates that firms provide retiree health insurance, individuals who previously did not have access to this type of coverage will probably retire earlier. This is likely to be true even if, as a result of the mandate, firms decide to pay the tax and put their employees on the government program rather than to provide health insurance themselves because in this case, the threat of losing one's employer-provided health insurance will not be a deterrent to retirement.

The last type of proposal would replace the current tax deduction available to companies who provide health insurance with an individual refundable tax credit for health care expenses (Butler 1991). Because large firms have incentives beyond those that are tax-related to provide health insurance, it is likely that many large employers will continue their health plans under this type of proposal. Small firms, on the other hand, will likely cease to offer health insurance absent tax incentives to do so. Many individuals would therefore find themselves in the market for private health insurance, although they would receive a partial tax credit for their health insurance expenditures. In this type of two-tiered system, the effects on job-lock are uncertain. The fact that fewer firms offer health insurance may increase the degree of job-lock among those who actually work in firms that continue to provide health insurance. On the other hand, the fact that more individuals would have policies which were not employment-based would serve to reduce job-lock.

The effects of this proposal on retirement are more clear. The retirement incentives of those who continue to be eligible for retiree health insurance will not change, while the incentive for everyone else to postpone retirement in order to maintain their health insurance will be eliminated. We would therefore be likely to see an increase in the retirement hazard for those individuals buying health insurance in the private market and a corresponding reduction in the average age at retirement.

Given the distortions that currently exist, this discussion suggests that any health care reform proposal is likely to have some labor market impacts. The extent to which these proposals eliminate the labor market distortions generated by employment-based health insurance depends on their ability to sever the link between employment and health insurance. As noted, not all of the proposals currently under consideration will be successful in doing this. It is likely that all of the proposals outlined above will increase the retirement hazard and reduce the expected age of retirement. Their effects on job-lock, however, differ. While actually eliminating these distortions is only one dimension along which any health care reform proposal should be judged, the labor market effects of these proposals should at least be evaluated as the results in this paper suggest that they could be substantial.

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TABLE 1

Health Insurance Loading Factors

Firm Size (Number of Employees)	Claims and General Administration	Risk and Profit	Commissions and Premium Taxes	TOTAL
1-4	21.8%	8.5%	11.2%	41.5
5-9	19.8	8.0	8.7	36.5
10-19	16.4	7.5	7.6	31.5
20-49	14.0	6.8	5.8	26.5
50-99	9.1	6.0	4.4	19.5
100-499	8.1	5.5	3.9	17.5
500-2499	7.1	3.5	2.9	13.5
2500-2499	5.2	1.8	2.5	9.5
10,000+	3.7	1.1	2.2	7.0

Source: Congressional Research Service (1988).

TABLE 2

Employer-Provided Health Insurance Coverage

	Males			Females		
Age	Any	Private	Employer	Any	Private	Employer
0-18	81.0	62.6	60.7	80.7	62.3	60.3
19-25	62.2	57.3	50.7	73.1	57.4	53.3
36-35	76.1	71.8	66.4	83.3	69.6	67.6
36-55	83.5	78.2	74.3	84.7	74.5	71.7
56-65	88.2	76.9	66.3	86.8	74.3	59.1

Source: Author's calculation using data from the 1987 National Medical Expenditure Survey.

CHAPTER 1

Employment-Based Health Insurance and Job Mobility: Is There Evidence of Job-Lock?

The majority of privately insured Americans obtain their health insurance through their own or a family member's employment. The rationale for employers to provide health insurance is straightforward. By pooling the risks of many individuals, employers can reduce adverse selection and lower administrative expenses. In addition, they can take advantage of tax laws that allow businesses to deduct their health insurance costs. Employers can therefore provide health insurance at a price much lower than the price their employees would face individually. These advantages of employer provision must be weighed, however, against the distortions they may generate in individual labor market decisions. In particular, health insurance may distort job mobility if employees decide to keep jobs they would rather leave for fear of losing coverage for preexisting conditions,[1] a possibility that has been termed "job-lock". This paper attempts to quantify the effect of employer-provided health insurance on the labor market mobility of individuals.

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[1] A preexisting condition is generally defined as any medical problem which has been treated or diagnosed within the past six months to two years. In some cases it may be more broadly defined as any medical problem for which an individual has *ever* received care. It may also be extended to include medical conditions for which a prudent person would have sought care even if no physician was actually consulted.

The link between employer-provided health insurance and labor market mobility is a potentially important factor in evaluating several competing proposals to reform the U.S. health care system. Some proposals involve either replacing or supplementing employer-provided insurance with individual tax credits or subsidies (Butler 1991; Enthoven and Kronick, 1989); others mandate that all firms provide health insurance for their employees (Rockefeller 1991; Bronow et al. 1991); and some eliminate employment-based insurance in favor of national health insurance (Grumbach et al. 1991). To the extent that these proposals affect the link between employment and health insurance, they could have substantially different effects on the degree of job-lock, yet there is little empirical evidence on the relationship between health insurance and job mobility.

Job-lock may also be an important concern if there is a match-specific component of productivity that makes workers more productive in some jobs than in others (Jovanovic 1979). The productivity of the economy as a whole will suffer if individuals who would like to move to more productive jobs are constrained to keep their current positions simply to maintain their health insurance. If firms make investments in worker human capital, they may actually adopt policies (such as providing pensions) with the explicit purpose of lowering mobility. The reduction in mobility associated with job-lock, however, is different than that associated with pensions because job-lock is not created by the firm but is an externality imposed by the benefit policies of other firms.

To test for the presence of job-lock, I examine the relationship between turnover, health insurance status, and expected medical expenses. If job-lock is important, individuals with employer-provided health insurance should be less likely to leave their jobs the higher are their expected medical expenses. However, job-lock should only affect those who actually have group employment health insurance. The effect of job-lock is therefore identified using a difference-in-difference approach: the mobility difference between those with high and low expected medical expenses should be greater for those with employer-provided health insurance than for those whose jobs do not include insurance.[2] This test allows me to distinguish the effect of employer-provided health insurance on mobility from other factors related to mobility. I estimate a probit equation for one-year voluntary turnover using data from the 1987 National Medical Expenditure Survey (NMES) which has information on job mobility, sources of health insurance coverage, and a set of characteristics that allow me to construct proxies for expected medical expenses.

In the empirical work I consider three different "experimental" groups. First, I examine whether married men with a secondary source of coverage in addition to employer-provided health insurance have greater mobility than men whose sole source of coverage is through their employer. This is to be expected if alternative coverage mitigates some of the costs of relinquishing insurance upon changing jobs. The second test compares the mobility of workers with large families to that of workers with smaller families, controlling for health insurance status. Heads of large families are likely to have higher expected medical expenses and should therefore be less likely to change jobs than heads of small families; this effect should be larger for those who have employer-provided health insurance than for those without it. The third test looks at married men whose wives are pregnant, a group whose anticipated medical expenses are known and likely to be large. Among those with health

[2] This type of identification strategy has been used by others to examine the effect of worker's compensation on injury duration (Krueger 1990; Meyer, Viscusi and Durbin 1990), the effect of mandated maternity benefits on wages (Gruber 1992a), and the effect of minimum wage legislation on employment (Card 1990).

insurance, men who are expecting a child should be less likely to change jobs than other married men whose wives are not pregnant.

I find evidence of substantial insurance-related job-lock among all three groups. Having other health insurance increases mobility for those with group employment health insurance and is associated with a 25 percent differential in the mobility rate between married men with employer-provided health insurance and those without. This result is robust to measures of family income or spouse's employment status. Similarly, having a large family implies a 30 to 40 percent reduction in mobility rates for those with employment-based health insurance relative to those who do not have this type of insurance. Having a pregnant wife tends to reduce mobility significantly for those with employer-provided health insurance, but expecting a child increases mobility for those without insurance. The mobility differential among those with employer-provided health insurance between those who are expecting a child and those who are not is between 30 and 50 percent. These results suggest that for some workers, job-lock may be an important consequence of the current system of employer-provided health insurance.

The paper is organized as follows. Section I provides some background on the link between health insurance and worker mobility. Section II details the methodology I use to identify job-lock, and Section III describes the data. Section IV presents the empirical results. The potential welfare consequences of job-lock are discussed in Section V, and the paper concludes with a discussion of the policy implications of my results.

I. Background and Motivation

There is abundant anecdotal evidence in support of insurance-related job-lock. In a recent *CBS/New York Times* poll, 30% of the respondents answered "Yes" to the question "Have you or anyone else in your household ever decided to stay in a job you wanted to leave mainly because you didn't want to lose health coverage?" (*New York Times*, September 6, 1991). That so many individuals feel constrained by the need for health insurance is telling evidence on the importance of health insurance in job decisions. If employees knew that all of their illnesses would receive identical coverage regardless of whether they worked, where they worked, or how long they had been on the job, health insurance would not be a deterrent to worker mobility.

The problem, however, is that employees do not necessarily receive identical coverage when they change jobs. Employers and insurance companies, who are fearful of adverse selection into their risk pools, know that for some individuals, health insurance benefits are a primary consideration in deciding which job to take. This has led many insurers to exclude so-called "preexisting conditions" for an extended period (typically six months to two years). Small firms are increasingly requiring medical examinations for new employees in order to medically underwrite serious ailments and exclude them from coverage entirely.[3] The New York benefits consulting firm Foster Higgins found that in 1987, 57% of employers excluded preexisting conditions in their health plans (Cotton 1991). Although small firms are more likely to impose these exclusions (64% of firms with under 500

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[3] Medical underwriting occurs when an insurance company requires medical exams of all new employees in order to exclude certain medical conditions on an individual basis for the life of the insurance policy. For example, if an individual has had cancer, the insurance company may underwrite the policy to exclude any further expenses related to cancer for that individual. Such underwriting is often a precondition to providing insurance to small firms.

employees), 45% of firms with more than 10,000 employees had them as well. In addition to denying coverage for preexisting conditions, many companies also impose a length-of-service requirement before employees are eligible for *any* insurance. In 1989, 49% of full-time workers in firms employing more than 250 workers faced such a requirement (see Table 1). Although the typical length-of-service requirement is less than four months, preexisting conditions may be excluded for a further period of time.

Length-of-service requirements and preexisting conditions exclusions imply that employees may be locked into their current jobs because changing jobs entails losing benefits for existing medical problems. Congress attempted to ease this burden in its 1985 COBRA (Consolidated Omnibus Budget Reconciliation Act) legislation by mandating that employers provide the option of continuation coverage to terminating employees.[4] Any employee who leaves a firm for any reason other than gross misconduct must be allowed the opportunity to purchase health insurance from the company for up to 18 months.[5] However, the cost of COBRA to the employee (102% of the employer's premium) may be prohibitively high at a time when individuals can least afford it. Spencer Associates (1991) reports that the average monthly health insurance premium for family coverage under COBRA was about $300 in 1990. The annual cost of $3,600 is 10% of the 1990 median family income of $36,000.

Job-lock may be further exacerbated by the importance of experience rating in setting a firm's health insurance premiums. For small employers who have only a few employees

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[4] Prior to 1985, 23 states already had continuation provisions in place, however, they were much less generous than the federal continuation mandates under COBRA.

[5] COBRA also gives the right to purchase insurance for 36 months to dependents and spouses after the death of the employee or a divorce from the employee.

over whom to spread risks, one major illness may significantly increase the firm's premiums for several years. To avoid this possibility, employers may simply discriminate by refusing to hire employees with health problems. Or, when such events occur, employers will cancel their policies altogether. Although the Americans with Disabilities Act, which recently took effect, states that employers may not screen for health in hiring, it places no constraints on the insurance company. A firm's insurance company may exclude an individual from coverage or drop the plan entirely if the firm hires an employee with sufficiently high medical costs. Fear of this event may discourage individuals from moving to small firms or leaving a job where they know their insurance premiums will not fluctuate.

II. Identifying Job-Lock

To study the phenomenon of job-lock, one would ideally like information on individual and family health status, worker mobility, and the health insurance plans of both the firm for which an individual works *and* to which an individual could move. Unfortunately, information on health status and health insurance is not widely available in labor force surveys, information on worker mobility is not typically available in health surveys, and information on insurance plans of companies for which an individual *could* have worked is nonexistent. An alternative approach is to identify two groups of workers who are similar in all respects except for either their health status or their insurance status and then compare the mobility of these two groups. I consider three factors associated with health and insurance status which should affect the cost of relinquishing health insurance upon changing jobs. Using the 1987 National Medical Expenditure Survey (NMES), I then examine the mobility rates of individuals affected by these cost factors for evidence of job-lock.

A. *Cost Factor 1: Having Other Health Insurance*

The first division is between those who have an alternative source of coverage as well as their own employer-provided health insurance and those who do not. In addition to group employment health insurance, there are several alternative (or supplementary) sources of coverage including Medicaid, CHAMPUS,[6] unions, and individual nongroup policies. Table 2 lists the fraction of married men who report coverage from various sources of insurance using data from the NMES.[7] Although employers are the predominant provider of health insurance, more than one-third of the men with employer-provided insurance have an alternative source of insurance not attached to their own employment. For most men, this secondary source is the employer-provided insurance received by their working wives.

Job-lock is based on the premise that individuals lose their coverage for preexisting conditions when they leave their job and relinquish the accompanying health insurance. But if an individual has additional coverage not attached to his own employment, then there is no insurance loss from changing jobs. Therefore, an individual with coverage through both his own employment *and* an outside source should be more likely to change jobs than an individual whose sole source of insurance is through his employer.

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[6] CHAMPUS/CHAMPVA (Civilian Health and Medical Program of the Uniformed Services/Veterans Administration) is the health insurance provided to dependents of individuals on active military duty, recipients of military retirement benefits and their dependents, and individuals receiving more than $1,000 per month in veterans' benefits.

[7] The NMES has quarterly information about whether an individual is covered by insurance through Medicare, Medicaid, group employment, union, CHAMPUS, other group, or nongroup health insurance. It also reports whether an individual actually holds a group employment, union, other group or nongroup policy.

To see this, consider the following matrix of mobility rates by employer-provided health insurance and other health insurance status, where M represents the probability of changing jobs in each cell.

		Employer-Provided Health Insurance	
		No	Yes
Other Health Insurance	No	M_{00}	M_{01}
	Yes	M_{10}	M_{11}

The test of whether those with employer-provided health insurance and other coverage are more likely to turnover than those without alternative coverage is simply the test of M_{11} - $M_{01} > 0$. This will provide a consistent estimate of job-lock as long as individuals with other health insurance are not more likely to change jobs for reasons unrelated to job-lock. There may, however, be grounds to believe that mobility will be greater for those with other health insurance for reasons other than job-lock. For example, a man whose wife has employer-provided health insurance also has a secondary source of income, something which might increase mobility as well. A second test for job-lock, therefore, is whether having other health insurance increases mobility *more* for those who have employment-based health insurance than for those who do not, or

$$(M_{11} - M_{01}) - (M_{10} - M_{00}) > 0 .$$

This difference-in-difference estimate for the effect of job-lock is consistent under the assumption that the independent effect of other health insurance on mobility is the same for those with employer-provided health insurance as it is for those without employer-provided health insurance.

It is important to note that looking at the effect of health insurance on mobility (M_{00} - M_{01} or M_{10} - M_{11}) *cannot* be construed as a test for job-lock, as health insurance could be correlated with other unobserved job attributes that also tend to reduce mobility. For example, jobs which include health insurance benefits may also be "better" along other dimensions, such as providing a pension or paid vacation days. The difference estimators that I use avoid this objection.

B. Cost Factor 2: Expected Medical Expenses and Family Size

A second test for job-lock compares mobility rates for those with and without high expected medical expenses; we should be more likely to see job-lock in families who most need health insurance because their medical expenses are high enough to be burdensome without insurance. Although the data which I have do not include good measures of health status,[8] I do have one variable that should be correlated with expected medical expenses--family size. There are two reasons to expect a positive correlation between medical expenses and family size. First, a larger family will have higher absolute medical expenses simply because it will make more routine visits to the doctor; second, it is more likely that there will be a considerable medical expense in a larger family because there are more people who might have something go wrong.

[8] In addition to the household information which I use in my analysis, the 1987 NMES did collect detailed information on health care utilization, medical expenditures, and health status in a supplemental Medical Provider Survey. These data, however, will not be publicly released for several months.

If the expected medical expenses associated with family size decrease mobility, then, in a mobility matrix

	Employer-Provided Health Insurance	
	No	Yes
Small Family	M_{00}	M_{01}
Big Family	M_{10}	M_{11}

we should see that $M_{01} - M_{11} > 0$. This simple test, however, may not be appropriate as this could merely reflect an independent effect of family size on mobility. To control for family size, therefore, an additional test for job-lock is whether the differential mobility rate between small and large families is greater for those who have employer-provided health insurance than for those who do not. This hypothesis is

$$(M_{01} - M_{11}) - (M_{00} - M_{10}) > 0 .$$

C. Cost Factor 3: Expected Medical Expenses and Pregnancy

There is another easily identifiable group with large anticipated medical expenses--families who are expecting the birth of a child. Changing jobs during pregnancy is very costly if it entails losing insurance coverage. The Health Insurance Association of America reported in 1989 that average costs for a normal pregnancy and delivery were $4,334 while average cesarean costs were $7,186. Furthermore, it is very likely that an employee who does change jobs will have to bear many of these expenses because of the significant number of firms that exclude preexisting conditions and impose length-of-service requirements. Pregnancy also accounts for a large fraction of the total medical expenses incurred by young workers. In the NMES, two-thirds of the hospital admissions in married families with a husband aged 24-30 were for the birth of a child. Based on this information we can make two predictions about worker mobility. First, for workers who have insurance, those who are expecting should be less likely to change jobs than those who are not. Second, among those workers who are pregnant, those currently insured should be less likely to change jobs over the nine months during which they are pregnant than workers who are *not* currently insured.

While looking at the mobility of pregnant women may be problematic since many women choose to leave the labor force (at least temporarily) when they have a baby, there is a natural alternative group of workers to consider--husbands with pregnant wives. Indeed, for most women this is the person whose mobility matters most, since two-thirds of insured women aged 15-44 are covered as dependents (Farley 1986). For married men, using pregnancy as a "preexisting condition" has several appealing characteristics. First, it is a condition which does not directly affect worker productivity (with other ailments, one could argue that the condition itself would affect a worker's mobility decision regardless of insurance considerations). Second, the costs of a normal pregnancy are well-established, as are the risks of complications, making it easy to measure the expected cost of having a child with and without insurance.

In a mobility matrix, the hypothesis that pregnancy-related expected medical expenses reduce mobility among those with health insurance is $M_{01} - M_{11} > 0$. But, as with family size, looking purely at the effect of pregnancy among those with health insurance may not be sufficient to identify job-lock because there are many reasons why individuals who are expecting a baby may have different mobility patterns than everyone else.[9] We can control for this, however, by looking also at the effect of a wife's pregnancy on the mobility of men without employer-provided health insurance.

	Employer-Provided Health Insurance	
	No	Yes
Wife Not Pregnant	M_{00}	M_{01}
Wife Pregnant	M_{10}	M_{11}

Having a wife who is pregnant should reduce mobility more for men who have employer-provided health insurance than for men who do not, and we should therefore expect to see

$$(M_{01} - M_{11}) - (M_{00} - M_{10}) > 0 .$$

III. Model

The model underlying job-lock is a simple variant of standard models of labor mobility (Mortensen 1986; Mitchell 1983). If there are no costs associated with changing jobs and all compensation is paid in the form of wages, workers will change jobs if the wage on an alternative job (w_a) exceeds that of the current job (w_c). Now suppose that compensation (C) may also include health insurance so that $C = w + VHI$ where VHI is the value of health insurance *to the employee.*[10] In this case the employee will change jobs only

[9] For example, the onset of fatherhood may have a "settling" effect on an individual's lifestyle, or individuals may not want to cope with the stress of changing jobs *and* having a baby at the same time.

[10] Note that the value of health insurance to the employee need not equal the cost to the employer of providing health insurance.

if perceived compensation on the alternative job exceeds that of the current job, or

$$w_a + VHI_a > w_c + VHI_c .$$

This can be rewritten as

$$w_a > w_c + D_{HI} = w_r$$

where D_{HI} equals VHI_c - VHI_a, the difference between the value of health insurance on the current job and that on an alternative job, and w_r is the employee's reservation wage.

To the worker, the value of health insurance depends critically on expected medical expenses as well as on other characteristics such as the employee's degree of risk aversion. This implies that if employees value health insurance differently, total compensation may vary across employees even if they have equivalent health insurance coverage and the wage component of compensation is the same. In principle, an employer could offset the wages of an employee by his valuation of health insurance so that the value of total compensation equalled the opportunity cost of the worker. In practice, however, this does not occur, perhaps because there is asymmetric information between the firm and the employee about the employee's prospective medical expenditures.[11] Most firms either purchase health insurance or use third-parties to administer their health plans if they self-insure. Consequently, the firm may actually know very little about its employees' health problems because it is the insurance company that tracks health care utilization and expenditures. Even

[11] It is this inability of the firm to set the compensation the company "pays" equal to the compensation the worker "receives" that may generate a suboptimal allocation of workers across firms. If all compensation is paid as wages and wages equal the employee's productivity, workers will move to the firms where their productivity is highest and all turnover will be optimal. But, if part of compensation is paid as health insurance and employees value health insurance differently from firms, workers may fail to move to firms where their productivity is highest (see Gruber 1992b for a lengthier discussion of this issue).

if the firm did have this information, using it to offset wages could be construed as discrimination which is generally unacceptable and in some cases illegal.[12,13]

Like the firm, we have no direct measure of the value of health insurance to the worker. However, we do have information on things which should affect it. If both jobs provide the employee with equivalent health insurance coverage, $D_{HI}=0$ and, just as in the case where all compensation is paid as wages, the worker will change jobs if $w_a > w_c$. If a worker currently has health insurance and the insurance policy of an alternative job imposes a length-of-service requirement or a preexisting conditions exclusion, then the value of health insurance on the current job will be greater than that on an alternative job, $D_{HI}>0$. Furthermore, the magnitude of D_{HI} will increase with the severity of the employee's preexisting conditions and the duration of the exclusion or the length-of-service requirement.

Now suppose that each employee faces a known wage offer distribution, *F(w)*, and that the employee receives one alternative wage offer each period (the offer distribution may be different for each employee). The probability that an employee changes jobs in any period is simply the probability that the alternative wage offer exceeds the employee's reservation wage,

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[12] Discrimination in hiring or wages because of pregnancy or other disabilities is illegal by statute (1978 Pregnancy Discrimination Act and the 1991 Americans with Disabilities Act).

[13] There is some evidence that firms may adjust wages based on the medical history of a broader group to which an individual belongs. For example, Gruber (1992a) finds that the wages of married women in their child-bearing years fell after the passage of the 1978 Pregnancy Discrimination Act which mandated that firms provide insurance coverage for pregnancy and delivery equivalent to that provided for other similar "illnesses". This, however, is different than lowering the wages of a pregnant woman by $4,500 in the year she is expecting a child.

$$\begin{matrix}\textit{Probability of}\\ \textit{Changing Jobs}\end{matrix} = 1 - F(w_r).$$

Unfortunately, we do not observe either w_a or w_r. Assume that w_r takes the following form,

$$w_r = w_c + \mathbf{z}'\gamma + \mathbf{x}'\beta + \varepsilon$$

where $\mathbf{z}$ is a vector of observable demographic characteristics (age, education, etc.) which may affect the employee's general valuation of health insurance, $\mathbf{x}$ is a vector of attributes which directly affect D_{HI}, and ε is a normally distributed error term. For the sake of simplicity, we can also assume that an individual's wage offer distribution depends on the same individual characteristics that affect w_r,

$$w_a = \mathbf{z}'\delta + \nu$$

where ν is a normally distributed error term uncorrelated with ε. This expression simply says that certain characteristics of the employee (such as his education) affect the mean value of the employee's wage offer distribution.

The worker will now change jobs if

$$\nu - \varepsilon > w_c + \mathbf{x}'\beta + \mathbf{z}'(\gamma - \delta)$$

and the probability of changing jobs is given by

$$\begin{matrix}\textit{Probability of}\\ \textit{Changing jobs}\end{matrix} = Pr\big(\nu - \varepsilon > w_c + \mathbf{x}'\beta + \mathbf{z}'(\gamma - \delta)\big).$$

With the assumption that both error terms are normally distributed, $\nu\text{-}\varepsilon \sim N(0,\sigma^2_{\nu\text{-}\varepsilon})$ and we can normalize by dividing through by $\sigma_{\nu\text{-}\varepsilon}$. The probability of changing jobs can now be expressed directly as

$$\begin{aligned}\begin{matrix}\textit{Probability of}\\ \textit{Changing jobs}\end{matrix} &= 1 - \Phi\left(\frac{w_c}{\sigma_{\nu-\varepsilon}} + \mathbf{x}'\frac{\beta}{\sigma_{\nu-\varepsilon}} + \mathbf{z}'\frac{(\gamma - \delta)}{\sigma_{\nu-\varepsilon}}\right)\\ &= 1 - \Phi\left(\alpha^{*}w_c + \mathbf{x}'\beta^{*} + \mathbf{z}'(\gamma^{*} - \delta^{*})\right)\\ &= 1 - \Phi\left(A_i\right)\end{aligned} \tag{1}$$

where $\Phi(\cdot)$ is the standard normal distribution function and A_i is defined implicitly. This is simply a probit model which can be easily estimated.[14] In the empirical work, I use current employer-provided health insurance coverage, other non-employment-related health insurance coverage, family size, and pregnancy as measures of $\mathbf{x}$.

Although the model is straightforward, the actual estimation is complicated by the fact that in my data, I observe individuals at two points in time separated by intervals of between 7 and 15 months.[15] The only information I have on turnover is whether the individual is on the same job at the end of the interval as at the beginning. Thus, I know whether or not an individual changed jobs at least once.

Let P_{it} denote the probability that individual i changes jobs in any given month t. This implies that the probability that individual i does *not* change jobs over an interval of m months is

$$\begin{pmatrix}\textit{Probability of}\\ \textit{Not Changing Jobs}\end{pmatrix}_i = \prod_{t=1}^{m}\left(1 - P_{it}\right). \tag{2}$$

Similarly, the probability of at least one job change over the same interval is

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[14] Although the structural parameters can be identified from the coefficient on w_c, these parameters are not directly relevant because I am interested in how the variables are related to turnover probabilities and not how they are related to the underlying latent variable that determines these probabilities.

[15] This is a problem of other panel data sets as well. In the PSID, time between interviews also varies from 7 to 15 months, while in the NLSY it varies from 9 to 20 months. Variation is less extreme in the SIPP because the sample is divided into groups which are interviewed every four months.

$$\begin{pmatrix} \textit{Probability of} \\ \textit{Changing Jobs} \end{pmatrix}_i = 1 - \prod_{t=1}^{m} (1 - P_{it}) . \tag{3}$$

If the probability of job change in any month is independent of that in any other month, these two probabilities reduce to

$$\begin{aligned} \begin{pmatrix} \textit{Probability of} \\ \textit{Not Changing Jobs} \end{pmatrix}_i &= (1-P_i)^m &&= \Phi(A_i)^m \\ \begin{pmatrix} \textit{Probability of} \\ \textit{Changing Jobs} \end{pmatrix}_i &= 1-(1-P_i)^m &&= 1-\Phi(A_i)^m . \end{aligned} \tag{4}$$

If, however, individuals have different underlying propensities to change jobs (i.e., there are "movers" and "stayers"), these probabilities may not be independent. To explicitly account for this I also include an individual specific random effect in the error term. In this case an individual will change jobs in period t if the unobserved latent variable Y_{it} is positive where

$$\begin{aligned} Y_{it} &= \alpha^* w_c + \mathbf{x}'\beta^* + \mathbf{z}'(\gamma^* - \delta^*) + \theta_i + \varepsilon_{it} + \nu_{it} \\ &= A_i + \theta_i + \varepsilon_{it} + \nu_{it} . \end{aligned} \tag{5}$$

In this expression, θ_i is an individual-specific fixed factor which I assume is distributed normally with mean 0 and variance σ_θ^2 (a parameter which will also be estimated). The respective probabilities of changing jobs and not changing jobs are now given as

$$\begin{aligned} \begin{pmatrix} \textit{Probability of} \\ \textit{Not Changing Jobs} \end{pmatrix}_i &= \prod_{t=1}^{m} (1-P_{it}) &&= \prod_{t=1}^{m} \Phi(A_i + \theta_i) \\ \begin{pmatrix} \textit{Probability of} \\ \textit{Changing Jobs} \end{pmatrix}_i &= 1 - \prod_{t=1}^{m} (1-P_{it}) &&= 1 - \prod_{t=1}^{m} \Phi(A_i + \theta_i) . \end{aligned} \tag{6}$$

For those who change jobs, their individual contribution to the likelihood function is

$$L_i = \int_{-\infty}^{\infty} [1 - \Phi(A_i + \theta_i)^m] \cdot f(\theta)\, d\theta \;, \tag{7}$$

while for those who do not change jobs,

$$L_i = \int_{-\infty}^{\infty} \Phi(A_i + \theta_i)^m \cdot f(\theta)\, d\theta \;. \tag{8}$$

If a logistic distribution for the differences error terms (ν-ε) is assumed rather than a normal distribution, the model would imply a logit specification for turnover. Most empirical studies of turnover based on cross-sectional data have in fact estimated either a logit or probit specification. These include several which examine the impact of fringe benefits, particularly pensions, on turnover (Mitchell 1982, 1983; McCormick and Hughes 1984; Bartel 1982; Bartel and Borjas 1977; Schiller and Weiss 1979). Generally these studies conclude that pensions and other fringe benefits are associated with lower mobility rates, although it is not clear whether this is because pensions are typically nonportable or because pensions are correlated with other favorable aspects of a job (Gustman and Steinmeier 1987, 1990). Other studies have found that unionization tends to decrease turnover (Freeman 1980) and that "negative" job characteristics such as injury risk lead to higher turnover (Viscusi 1979; Bartel 1982).

III. Data: 1987 National Medical Expenditure Survey

The data I use come from the 1987 National Medical Expenditure Survey (NMES) conducted by the Agency for Health Care Policy and Research. This survey of approximately 14,000 households (38,446 individuals) collected detailed information about health status, health insurance, and medical care utilization in 1987 through a series of four interviews. Additionally, several questions relating to employment were asked during each of the four interview rounds which make it possible to identify if an individual changed jobs between the first and the fourth interview.[16]

The sample I look at is restricted to married men ages 20-55 who were full-year eligible respondents, employed but not self-employed at the first interview, and married to the same individual at the first and fourth interviews.[17] The final sample consisted of 2978 individuals; Table 3 gives the specific selection criteria used to reach this final sample.

These data are used to estimate a probit model of job turnover where the dependent variable measures *voluntary* job mobility. In my sample, 16% of individuals changed jobs and 12% changed voluntarily. These numbers are not out of line with one year mobility rates reported elsewhere. In the May 1988 CPS, 12% of married men age 24-55 report having tenure less than one year. My mobility rates may appear high relative to the CPS, but since the CPS question about tenure is only asked of individuals who are employed, it understates mobility because individuals who quit their jobs or who have been laid-off but have not yet found new employment are not included in the sample. In the NMES, the total mobility rate *conditional* on having a job at the fourth round interview is 12.2% for men 24-55, a fraction almost identical to that reported in the CPS.

The dependent variable used in all specifications equals 1 if the individual changed jobs voluntarily. The data include an indicator variable for whether an individual held a

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[16] Specifically, the data contain an indicator variable for whether or not the main job which an individual held at the fourth interview was the only main job that an individual held during the year.

[17] Military personnel are not included in the sample because they are considered "out-of-scope" while they are in the military.

different job at the last interview than in previous interviews. I code these individuals as well as those who are not employed at the final interview as job changers (everyone in the sample is employed at the first interview). There are also three questions in each round regarding whether an individual is currently laid-off or spent any time during the previous round on layoff. If the individual changed jobs and answered yes to any of these layoff questions after the first round, I assume that the individual changed jobs involuntarily. Therefore, voluntary job-changers are coded as those who either changed jobs between the first and the fourth interview or who became unemployed and who did not spend any time on layoff after the first interview.[18]

Table 4 presents some descriptive statistics of the variables used in the analysis. Some details of their construction follow. In addition to other demographic variables such as race, union status and education, experience is included as an independent variable in all specifications. Measures of labor market experience are typically prone to error because the usual calculation is age-education-6. If all time after age 6 is spent either working or going to school, there is nothing wrong with this construction of an experience variable. The 1987 NMES allows one to construct a more accurate measure of experience, however, because it asks how many years an individual spent not working after age 21 for several reasons including school, caring for children, and poor health. I use this information to construct

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[18] This measure may slightly overstate the degree of voluntary mobility if there are individuals who were laid-off but did not spend any time unemployed (since questions regarding layoff were only asked of those who were or had been unemployed). Data from the January 1987 Current Population Survey suggest that 23% of those who lost the job they held a year previously found a new job within 2 weeks. If none of these individuals experienced any unemployment, this would lower fraction of those who left their jobs voluntarily by 1% at most (from 12% to 11%).

a measure of experience which subtracts the number of years spent out of the labor force for reasons other than education from the traditional measure of experience.[19]

The wage variable used was constructed by the Agency for Health Care Policy Research (AHCPR) using information on wage and salary payments, the time period covered by the payment (i.e. hourly, weekly, monthly), and the usual number of hours and days worked. The family and individual income variables were also constructed by AHCPR.

All three experiments used to test for job-lock include a dummy variable for whether or not the individual actually *holds* an employment-related health insurance policy. 72.5 percent of my sample are coded as holding group employment health insurance. The first experiment, which uses other health insurance to identify job-lock, also includes several dummy variables which correspond to sources of other health insurance coverage (union, CHAMPUS, nongroup, and spousal health insurance). These variables equal 1 if an individual is *covered* by such a policy; another family member could actually hold the policy.

The second experiment uses family size to identify job-lock. Family size should only matter, however, if an individual's health insurance policy actually covers others in the family. While the 1987 NMES does contain information about both whether an individual is covered by a particular type of insurance policy and whether an individual actually holds the policy in his or her name, it does not give information about the source of coverage for individuals who are covered but do not actually hold a policy. For example, if a child is reported as being covered by employment-related health insurance and both parents hold

[19] Because most men do not typically spend much time out of the labor force for reasons other than education, my measure of experience and the more traditional measure are not that different. For women, however, the difference can be substantial.

employer-provided health insurance, I do not know if the child's coverage comes from the father, the mother, or whether both insure the child.[20]

I have constructed a conservative and a liberal measure of whether a husband's employer-provided health insurance covers others. In both cases, I have assumed that if the husband is the only family member with group employment health insurance and his spouse or children are covered by employment-related health insurance but do not hold a policy, then the husband's policy covers everyone. In my conservative estimates, I have further assumed that if both parents hold employer-provided health insurance, the husband only covers himself. This will obviously understate the extent to which an individual covers others. Using this criterion, 51.3% of the sample (and 68% of those who have employer-provided health insurance) have health insurance that covers others.

In the liberal estimates I have assumed that if the children and wife are covered and the husband holds a group employment policy, then this policy covers everyone, regardless of whether or not the wife also holds group employment insurance. With this definition, 62.8% of the sample (83.4% of those with employer-provided insurance) have insurance that covers others. This estimate will overstate the coverage of others (especially to the extent that individuals do not have the option of family coverage), but is likely closer to the truth than the conservative estimate. A comparison with similar data from the May 1988 Current Population Survey suggests that this bias is likely to be small.[21] Even if individuals do not

[20] In practice, such coverage could also come from someone outside the family unit, for example a divorced parent. The NMES does report whether or not coverage comes from outside the home, although very few individuals are affected.

[21] In a similar sample of married men from the May 1988 CPS, 64.9% have employer-provided health insurance which covers others and this is 79.1% of those with such insurance. These numbers are very close to the numbers I have calculated with the liberal estimate of covering others.

actually elect family coverage, they may usually add other family members to their policy outside the open-enrollment period if other family members have lost their insurance due to a change in the spouse's employment.[22]

In determining coverage from a wife's health insurance policy I have assumed that if the wife holds employer-provided health insurance, her husband is also covered.[23] This corresponds to the liberal measure for covering children just described. In principle I could also make a conservative measure of coverage by a wife's policy analogous to that for covering children, but it would not be possible to identify job-lock in the estimation. With a conservative measure, only those who do not have employer-provided health insurance could be coded as having coverage through a wife's policy. An interaction between having your own employer-provided health insurance and being covered by a wife's health insurance would therefore equal zero for everyone.

The third experiment identifies job-lock using pregnancy as a preexisting condition. Because I only observe births and not pregnancy, I construct two measures of pregnancy. The first is simply a dummy variable for whether or not a baby was born between the first interview and December 31, 1987. The second is the fraction of time between the first interview and the end of the year during which an individual's wife was pregnant. Using this second measure gives a stronger test of job-lock. Among those who have employer-provided health insurance, individuals whose children are born shortly after the first interview should

[22] Neither measure, however, accounts for the possibility that an individual could have coverage through his or her employment but does not even elect individual coverage because he or she already has coverage elsewhere.

[23] Using this definition, 33.5% of my sample are coded as having health insurance through their spouse's employment. In the May 1988 CPS, 33.9% of married men have wives with employer-provided health insurance. Of these women, 80% have insurance which covers others in the family, a figure roughly similar to that for men in both the NMES and the CPS.

be more likely to change jobs than individuals whose children are born at the end of the year. This is because after the baby is born, the deterrent to mobility which kept the individual from changing jobs is gone (assuming the baby is healthy).[24] Because the relationship between the timing of births and the timing of job changes is somewhat complicated, Appendix A goes into greater detail about the implementation of this test.

IV. Empirical Results: 1987 National Medical Expenditure Survey

Tables 5-10 present the empirical results from estimating the probability of changing jobs as a function of the cost factors outlined previously:

$$\mathit{\begin{matrix}Probability\ of\\ Changing\ Jobs\end{matrix}} = f\left(\beta_0 + \beta_1 \cdot \mathit{\begin{matrix}Health\\ Insurance\end{matrix}} + \beta_2 \cdot \mathit{\begin{matrix}Cost\\ Factor\end{matrix}} + \beta_3 \cdot \mathit{\begin{matrix}Health\\ Insurance\end{matrix}} * \mathit{\begin{matrix}Cost\\ Factor\end{matrix}} + \mathbf{z}'\gamma\right).$$

In all specifications, the vector $\mathbf{z}$ includes the demographic variables described previously as well as 5 industry and 4 occupation dummies (although these coefficients are not reported). Except where noted, all probits are run on a sample of 2,978 men.

The relationship between the estimated βs and the mobility matrices outlined earlier is straightforward. Consider once again the mobility matrix associated with the other health insurance experiment. The βs which correspond to each cell's mobility rate are listed below. The estimated constant term, β_0, corresponds to the mobility rate (conditional on $\mathbf{z}$) for individuals who have no health insurance coverage, either by themselves or through someone else. β_1 and β_2 give the marginal impact on mobility associated with holding employer-

[24] Mobility may be lower after birth than it was before pregnancy if family size has an independent effect on mobility; however, if it is the expense associated with pregnancy that deters mobility, the likelihood of changing jobs after birth should be greater than it was during pregnancy (if the baby is healthy).

provided health insurance (β_1) and having other health insurance (β_2); and β_3 gives the extra impact on mobility generated by having both sources of health insurance coverage. The tests of job-lock, therefore, are tests about the sign and magnitude of the estimated βs.

		Employer-Provided Health Insurance	
		No	Yes
Other Health Insurance	No	M_{00} β_0	M_{01} $\beta_0+\beta_1$
	Yes	M_{10} $\beta_0+\beta_2$	M_{11} $\beta_0+\beta_1+\beta_2+\beta_3$

The first column in Table 5 lists the coefficients from a simple probit equation for turnover which does not include any of the variables used to identify job-lock. Wages, union status, and experience are all negatively associated with turnover, while the effects of education and race are insignificant.[25] As expected, the time between interviews increases the likelihood of turnover.

The second column of Table 5 adds a dummy variable for whether or not the individual has employer-provided health insurance. This variable is negative and highly significant (health insurance is actually the most significant predictor of turnover in this specification). The estimated coefficient on health insurance implies that workers in jobs with health insurance have a 60% lower likelihood of turnover than equivalent workers in jobs without health insurance. Note that when health insurance is included as a regressor,

[25] Previous studies of turnover have also found little effect of education or race (Gustman and Steinmeier 1987, 1990; Mitchell 1982).

the impact of wages falls substantially, by about one half.[26] Moreover, the coefficient on health insurance is substantially larger than that on wages. This suggests that increasing employer spending on health insurance has a greater effect on turnover than an equivalent increase in wages.[27] The effect of health insurance alone, however, cannot be construed as evidence of job-lock because jobs which provide health insurance typically provide many other fringe benefits as well (such as pensions, paid vacations, or simply better working conditions).[28]

A. Cost Factor 1: Having Other Health Insurance

The first test of job-lock comes in the third column of Table 5 in which the cost factor *Other HI* is included as a regressor where *Other HI* denotes coverage from any one of four alternative sources of coverage (CHAMPUS, union, nongroup or spouse's group employment health insurance). There are two hypotheses concerning mobility and alternative sources of coverage which fall from the theory of job-lock. The first is that among those

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[26] Although this reduction of the wage coefficient may seem large, Mitchell (1982) finds a similar result for pensions. In her study, including a dummy variable for whether or not an individual has a pension reduces the wage coefficient by 40 percent.

[27] A recent poll by the Employee Benefits Research Institute asked how much extra wage income an individual would have to receive in order to give up his or her employer-provided health insurance (Employee Benefit Research Institute 1991). The mean response was $4,850 which is about one-sixth of the median family income in the U.S. (In contrast, the average cost to firms of providing health insurance was $2,748 per employee in 1989.) If employees value health insurance just as they value wages, then the effect of health insurance should be similar to the effect of increasing income by $5,000 per year. Because most income is wage income, we would therefore expect the coefficient on health insurance to be smaller than that on wages if employees value wages and health insurance equivalently.

[28] The Employee Benefits Research Institute reports that 75% of private-sector workers receive employer-provided health insurance, 57% have pensions, and 54% have both. Among public-sector employees, 93% have health insurance, 93% have pensions, and 90% report having both benefits. These numbers suggest that almost all employees who have pensions also have health insurance (Piacentini and Anzick 1991).

with employer-provided health insurance, those who also have other health insurance should be more likely to change jobs than those who do not have alternative coverage. The test of this hypothesis is $\beta_2+\beta_3>0$. The second hypothesis is that having other health insurance will increase mobility more among those with employer-provided health insurance than among those without it. The test of this hypothesis is simply $\beta_3>0$.[29] Because these are both one-sided hypotheses, the reported p-values correspond to a one-tailed test.

The bottom panel of Table 5 presents both tests of job-lock. The statistic for the first test, $\hat{\beta}_2+\hat{\beta}_3$, is positive (.171) with a p-value of .017. The second test statistic, which is simply $\hat{\beta}_3$ (the coefficient on the interaction between employer-provided health insurance and other health insurance), is also positive (.211) with a p-value of .058. Both of these tests give strong evidence of insurance-related job-lock.

The actual effect of job-lock may be more easily seen, however, by once again considering a mobility matrix, this time with the estimated probability of changing jobs over a 12 month period in each cell (standard errors are in parentheses).[30] The turnover probability is calculated for a representative individual: a white, 38-year old man with 13 years of schooling and 19 years of experience who works in a non-union manufacturing job as a craftsman, earns an hourly wage \$11.50, and has a total family income of \$36,000. These characteristics correspond roughly to the averages in the sample (or the mode for

[29] Using the mobility matrix, the first test, $M_{11}-M_{01}>0$, is equivalent to $\beta_2+\beta_3>0$. Likewise, the second test, $(M_{11}-M_{01})-(M_{10}-M_{00})>0$, is equivalent to $\beta_3>0$. These are simply algebraic manipulations of the βs which correspond to the mobility rates of each cell in the matrix.

[30] The variance for the predicted probabilities, $\hat{P}=P(x'\hat{\beta})$, is computed as

$$Var[\hat{P}] = \left(\frac{\delta\hat{P}}{\delta\hat{\beta}}\right)' Var[\hat{\beta}] \left(\frac{\delta\hat{P}}{\delta\hat{\beta}}\right).$$

categorical variables). The average probabilites for everyone in the sample look very similar to those computed for the representative individual.

		Employer-Provided Health Insurance		Difference-in-Difference	
		No	Yes	Right-Left	Adjusted
Other HI	No	.256 (.032)	.085 (.012)		
	Yes	.244 (.032)	.115 (.017)		.081 (.016)
Row Difference (2-1)		-.012 (.034)	.030 (.012)	.042 (.037)	.034 (.018)
Percentage Difference		-5.1% (14.4)	26.0% (10.4)	31.1% (17.7)	29.6% (13.8)

The probability of turnover for an individual with no sources of health insurance is .256, the first entry in the matrix. Similarly, the turnover probability for an individual with employer-provided insurance but no other source of coverage is .085 (as expected, mobility is much lower for those with employer-provided health insurance than for those without). The third row of the table gives the absolute difference in the mobility rate for those with and without other health insurance (the second row minus the first row), while the last row shows the percentage difference in the mobility rates between the two rows where those with other health insurance are taken as the base group.

The striking feature of this matrix is that while individuals with other health insurance only are slightly (5.1%) *less* likely to change jobs than individuals with no health insurance, individuals with both sources of health insurance are much (26.0%) *more* likely to change jobs than those who only have employer-provided health insurance. If we look only at those with employer-provided health insurance (column 2), we can use the increased mobility of those with both sources of health insurance over those who only have employer-provided health insurance (the row difference in column 2) as a measure of job-lock. This suggests that job-lock is responsible for a 26% reduction in mobility rates among those with employer-provided health insurance.

To account for any independent effect of other health insurance on mobility, we should also consider a difference-in-difference estimator for job-lock, presented in columns 3 and 4 of the matrix. A simple difference-in-difference estimate of job-lock, presented in the third column, is the row difference in the second column minus that in the first column. This gives an estimate for job-lock of 31.1% (26.0-(-5.1)). An alternative difference-in-difference estimate of job-lock can be obtained by comparing the actual mobility rate of those with both sources of health insurance to the counterfactual mobility rate of this group if the effect of other health insurance were the same as for those without employer-provided health insurance. This is calculated as follows. The row difference in column 1 suggests that other health insurance reduces mobility by 5.1% among those who do not have employer-provided health insurance. If the effect is similar for those who do have employer-provided health insurance, then the mobility rate of those with *both* sources of health insurance would be .081 rather than .115.[31] This is the number given in the second row of the last column of the mobility matrix. The true magnitude of job-lock is then a 29.6% ((.115-.081)/.115) reduction in mobility among those with employer-provided health insurance. The last row of column 4 in the matrix gives this adjusted difference-in-difference estimate of job-lock. Because other health insurance alone does not have a substantial impact on mobility (as suggested by the small row difference in column 1), the measure of job-lock computed from

[31] The number .081 is derived by dividing .085 (the mobility rate of those with only employment-based insurance) by 1.051 because the mobility rate of those with only other health insurance is 5.1% lower than that of individuals with no health insurance.

the simple row difference among those with employer-provided health insurance and both difference-in-difference estimates are quite similar.[32] The last row of Table 5 gives the range of these estimates as the degree of job-lock.

The last column in Table 5 gives the results from estimating a random effects probit model for turnover (obtained by maximizing the likelihood function specified in equations 7 and 8). The coefficients in columns 3 and 4 are not directly comparable because those for the simple probit give the effect on between-interview turnover while those for the random effects probit correspond to monthly turnover. The relative magnitudes, however, are very similar (i.e, the coefficient on health insurance is roughly twice that on wages in both specifications), as are the predicted probabilities of job change over a 12-month interval. While the standard errors are slightly larger using the random effects specification, the underlying results are very similar: job-lock accounts for a 25-30% reduction in mobility.

Table 6 breaks down other health insurance into its component parts to determine whether they have similar effects on mobility. It appears that the results in columns 3 amd 4 of Table 5 are driven mostly by the effect of having a wife with employer-provided health insurance. Both tests of job-lock in Table 6 based on wife's health insurance give evidence of job-lock similar to that in Table 5, and the estimated magnitude of job-lock is only slightly larger at 28% to 37%. For union coverage and CHAMPUS, both tests of job-lock are inconclusive. Nongroup health insurance gives more evidence of job-lock, although the

[32] Given the similarity between the two difference-in-difference estimates of job-lock, some may question the need for an adjusted estimate. The adjusted estimate is actually preferable because it is possible for the simple estimate to exceed 100%, and a reduction in mobility greater than 100% does not make sense. The two estimates are similar here because the row difference in column 1 is so small. It will matter, however, when we come to the pregnancy "experiment".

standard errors on the coefficient estimates $\hat{\beta}_2$ and $\hat{\beta}_3$ are still quite large (even so, Test 2 is significant at the 85% level).

These results are not completely unexpected. Union coverage is indirectly linked to employment and perhaps should not be counted as "other" health insurance. The fraction of individuals in the sample covered by CHAMPUS and non-group health insurance is quite small (2.5% in both cases), and the percentage with both employer-provided health insurance and these other sources of coverage is even smaller (1.5% for CHAMPUS and .5% for nongroup health insurance). With such small cell sizes, it is not surprising that it is difficult to precisely identify any effect. Although the coefficients for the various sources of health insurance coverage appear to be quite different, the standard errors are large enough that a likelihood ratio test fails to reject the hypothesis that they are equal.[33]

Table 7 examines further the relationship between wife's health insurance and job mobility. The first column looks solely at wife's health insurance, and the results are very similar to those for wife's health insurance in Table 6. Because a wife with employer-provided health insurance is obviously a wife who is working, it is possible that the effect of wife's health insurance is in reality the effect of having a working wife. Certainly having a second source of income in the family would make it easier for an individual to give up his current job if he had not yet lined up another. The last two columns in Table 7 control for family income (column 2) and wife's income (column 3). Higher family income has a negative impact on turnover, although this could be partly the effect of wages since wages and income are highly correlated. Indeed, when family income is included as a regressor,

[33] The likelihood ratio statistic comparing the restricted estimates in column 3 of Table 5 with the unrestricted estimates in Table 6 is 3.22, while the critical value for a χ^2 with 6 degrees of freedom is 12.59.

the magnitude of the wage coefficient falls by 30%. Higher wife's income, however, does increase the likelihood of changing jobs. Even so, the tests for job-lock are much the same. The estimates of β_3 in columns 2 and 3 of Table 7 are almost the same as in column 1, and the estimates of the magnitude of job-lock are also very similar. These results suggest that the increased mobility for men whose wives also have health insurance is not merely capturing the impact of having a working spouse.

This conclusion is further supported by the results in Table 8, which look only at men whose wives are working. Both tests of job-lock are actually more significant than those for the full sample despite a 40% reduction in sample size, and the estimated magnitude of job-lock is larger (36% to 51%). Controlling for family income, wife's income or wife's wages do not alter the results substantially.[34]

B. Cost Factor 2: Expected Medical Expenses and Family Size

Table 9 moves to the second job-lock experiment in which family size is used as a proxy for expected medical expenses. The actual equation estimated is the same as before except that β_2 now corresponds to family size (rather than other health insurance) and β_3 to the interaction between having employer-provided health insurance that covers others and family size. As before, we can consider two tests of job-lock. We can first look at whether having health insurance that covers others reduces mobility more for individuals with large families, or $\beta_2+\beta_3<0$. If family size has an independent effect on mobility, a stricter test would be that the differential mobility between small and large families should be greater for

[34] Although the results are not presented, controlling for family income, wife's income and wife's wages together does not change the tests for job-lock either.

those with employer-provided health insurance than for those without it. This test is $\beta_3 < 0$.[35]

As mentioned in the description of the data, I use both a conservative and a liberal measure of whether the husband's health insurance covers others in the family. In both cases, the tests for $\beta_2+\beta_3$ and for β_3 alone suggest evidence of job-lock. Although the effects are much more significant for the conservative test, the actual estimates are almost identical. Using the conservative measure of covering others gives a stronger test of job-lock because when using the liberal measure, the effect of covering others will be partially offset by the fact that having a wife with employer-provided health insurance reduces job-lock. The third column of Table 9 looks only at families for whom the wife does not have employer-provided health insurance and, as could be expected, the results on job-lock in column 3 are stronger and of a greater magnitude than those in column 2. In all three cases, family size has a negative impact on mobility, but this effect is insignificant. The last column in Table 9 gives the results from estimating a random effects probit using the full sample and the conservative estimate of covering others. As was the case with other health insurance, the qualitative results for the simple and the random effects probit are very similar.

The magnitude of job-lock can once again be derived from the predicted probabilities in a mobility matrix. The estimates come from the results in column 1 of Table 9. The probabilities in the first row correspond to an individual with one child, while those in the second row correspond to an individual with 5 children. As shown earlier, the probability

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[35] Using a mobility matrix, the test M_{10}-$M_{11}>0$ is equivalent to $\beta_2+\beta_3<0$ while the test (M_{10}-M_{11}) - (M_{00}-M_{01})>0 is the same as $\beta_3<0$. The predicted signs are opposite those in the other health insurance case because having other health insurance should increase mobility for those with employer-provided health insurance while having a large family should decrease mobility.

of turnover is substantially lower for those with employer-provided health insurance, regardless of family size. Similarly, family size decreases the probability of changing jobs regardless of health insurance status (the row differences for both columns are negative). However, the negative effect of family size on turnover is much larger for those with employer-provided health insurance (the second column). Not only is the relative reduction in mobility larger (44.5% versus 11.6%), but the absolute reduction in mobility is larger as well (.041 versus .029).

		Employer-Provided Health Insurance		Difference-in-Difference	
		No	Yes	Right - Left	Adjusted
Family Size	1 Child	.253 (.027)	.092 (.012)		
	5 Children	.224 (.041)	.051 (.014)		.081 (.015)
Row Difference (2-1)		-.029 (.036)	-.041 (.012)	-.012 (.027)	-.030 (.010)
Percentage Difference		-11.6% (14.1)	-44.5% (13.2)	-33.0% (25.0)	-37.3% (11.1)

Looking only at the difference in mobility rates of large and small families among those with health insurance (column 2), the estimated effect of job-lock is a 44.5% reduction in mobility among those with employer-provided health insurance. Accounting for the negative (albeit insignificant) effect of family size using a simple difference-in-difference estimate gives a more conservative measure of job-lock (33%). If having a large family reduces mobility by 11.6% for those without employer-provided health insurance and we assume that the pure effect of family size is similar for those with employer-provided health insurance, then the adjusted difference-in-difference effect of job-lock from having four additional children would be to reduce mobility by 37%. These numbers are presented in the last row of Table 9 as the degree of job-lock. All of these estimates of job-lock obviously depend on the arbitrarily chosen family size for the small and large family. If the "large" family had 4 children rather than 5 children, the estimate of job-lock would naturally be smaller. Comparing a family of 2 children with a family of 4 children gives a difference-in-difference estimate of job-lock of about 25%.

C. Cost Factor 3: Expected Medical Expenses and Pregnancy

Results using pregnancy as a preexisting condition are presented in Table 10. The first two columns use the percent of time pregnant as the measure of pregnancy, while the last two columns use a dummy variable for whether or not the individual had a baby. In the estimated equation, β_2 now corresponds to pregnancy while β_3 corresponds to the interaction between pregnancy and employer-provided health insurance. The two tests for job-lock are 1) whether pregnancy reduces mobility among those who have health insurance ($\beta_2+\beta_3<0$); and 2) whether health insurance reduces mobility more for those who are expecting a child than for those who are not expecting ($\beta_3<0$).

As columns 1 and 3 of Table 10 show, both measures of pregnancy suggest evidence of job-lock and, as expected, using the fraction of time pregnant does give stronger results. Looking only at the individuals most likely to have children, those aged 20-39, does not alter the results significantly (columns 2 and 4). The last column of Table 10 presents the results from estimating a random effects probit corresponding to the simple probit in column 1. As before, the results from the random effects probit and the simple probit are qualitatively similar.

The tests of job-lock in the pregnancy experiment are less compelling than those from the other health insurance and family size experiments. While the test of $\beta_3 < 0$ is significant, the simple test of $\beta_2+\beta_3<0$ is only significant at the 70% to 80% level. The significance of the difference-in-difference estimator β_3 is due largely to the fact that among individuals who do not have employer-provided health insurance, pregnancy actually *increases* mobility ($\beta_2 > 0$). This result may seem counterintuitive, however, it should not seem too surprising that these individuals may be motivated to find better jobs precisely because they are expecting a child. Since not all firms exclude preexisting conditions, there is a chance that an individual in a job which does not currently provide health insurance will find a new job which provides health insurance that will pay for the delivery.

Even though the test $\beta_2+\beta_3$ is not as significant as that from the other health insurance and family size experiments, its magnitude is still large enough that it gives evidence of job-lock within the range of the previous two experiments. This is shown in the mobility matrix below based on column 1 of Table 10.

		Employer-Provided Health Insurance		Difference-in-Difference	
		No	Yes	Right - Left	Adjusted
Wife Pregnant	No	.242 (.026)	.097 (.012)		
	Yes	.502 (.147)	.067 (.0397		.202 (.064)
Row Difference (2-1)		+.261 (.147)	-.030 (.012)	-.291 (.151)	-.135 (.073)
Percentage Difference		107.8% (35.4)	-30.9% (37.8)	-138.7% (51.8)	-66.7% (20.7)

Note that among those who do not have employer-provided health insurance, the predicted mobility rate of individuals who are expecting a child is more than twice that of individuals who are not expecting (the row difference in column 1). In contrast, among those who do have employer-provided health insurance, individuals who are expecting have a predicted mobility rate 31% lower than those who are not expecting. The effect of job-lock using the row difference in column 2 is therefore 31%. The simple difference-in-difference estimate of the effect of job-lock is 139%, while the adjusted difference-in-difference estimate of job-lock is 67%. In this case, the adjusted difference-in-difference estimate makes much more sense than the simple difference-in-difference estimate because a reduction in mobility rates greater than 100% is impossible. The last row of Table 10 gives the row difference and the adjusted difference-in-difference measures of job-lock for the other specifications which look at pregnancy.

Because the effect of pregnancy on mobility is positive for those without group employment health insurance and negative for those with such insurance, the rationale for using the difference-in-difference measure of job-lock is less compelling than when looking at family size for evidence of job-lock. This is particularly true if part of the mobility differential among those without insurance between those who are expecting and those who are not is motivated by the former group trying to find jobs with health insurance (this would be a kind of reverse job-lock).

I attempted to confirm my results of pregnancy-related job-lock by estimating a hazard model of voluntary mobility using data from the National Longitudinal Survey of Youth (NLSY). This dataset has the advantage of precisely dating (month, day and year) both job changes and births. The measured effect of job-lock using the NLSY was the "wrong" sign, although the standard error on the interaction between pregnancy and employer-provided health insurance was so large that it precluded making any inferences. An attempt to reconcile the differing results from these two datasets was not particularly fruitful, although there is some suggestion that the lack of evidence for job-lock in the NLSY is due partly to the fact that most of the births were first births.[37]

D. Specification Checks

There is one variable which should perhaps be included in all of the mobility equations that is missing, and that is tenure. Unfortunately, the NMES did not survey participants about their job duration. Studies which include tenure as a dependent variable consistently find a negative and significant relationship between tenure and voluntary turnover, suggesting that individuals who have been on their jobs longer are less likely to quit.[38] If having health insurance is correlated with tenure, the coefficients which I use to identify job-lock could be biased by the omission of tenure. Applying standard omitted variable formulas,[39] I calculated the magnitude of the potential bias using estimates of the

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[37] Controlling for income, we might expect to find more evidence of job-lock among workers expecting their second child rather than their first if workers learn about provisions such as preexisting conditions exclusions through trial and error. A more detailed description of the results from the NLSY is available from the author upon request.

[38] It is unclear, however, whether this is a causal relationship or the outcome of a selection process in which we discover that individuals who had a lower chance of leaving initially are less likely to change jobs simply because they are the ones more likely to have greater tenure.

[39] If the relationship between *tenure* and the variables that are included is linear, i.e.,

$$tenure_i = x_i'\gamma + \nu_i ,$$

where **x** denotes the included variables and ν is a normally distributed error term, then the k elements of the vector β^* estimated from a standard probit equation for turnover converge to

$$plim\ \beta_k^* = \frac{\beta_k + \delta\gamma_k}{\sqrt{\delta^2\sigma_\nu^2 + 1}}$$

where δ is the true effect of tenure on mobility (Yatchew and Griliches 1985). Note that the numerator in this expression is simply the standard OLS formula for the effect of an omitted variable on the coefficient of an included variable. Using a sample of married men from the May 1988 CPS, I estimate that γ_{HI} ranges from 1.7 to 3.4 and σ_ν equals 6.3. I assume that the true effect of tenure on mobility, δ, equals -.09 (Mitchell 1982). Using these numbers, the true estimate of the coefficient on *Health Insurance* in column 2 of Table 5 would be -.448, a bias of 28%. Similar calculations were made for the remaining variables pertaining to job-lock. (These calculations, of course, are not entirely accurate because estimates of tenure from other studies will be biased by omitting variables related to job-lock).

effect of tenure on mobility from other studies. The omission of tenure biases the coefficient on health insurance by at most 30%. Correcting for the bias on the interaction terms does not change the conclusion that there is substantial insurance-related job-lock.

Table 11 compares the estimated impact of job-lock from the three different experiments. As noted previously, we should expect to see more evidence of job-lock among individuals with higher expected medical expenses. Columns 3 and 4 give the estimated impact of job-lock, while column 5 lists the expected family medical expenses for the group specified in each experiment. While there appears to be little relationship between the effect of job-lock based on $\hat{\beta}_2+\hat{\beta}_3$ and expected medical expenses, the effect using $\hat{\beta}_3$ and expected medical expenses are highly correlated. For example, the effect of job-lock based on $\hat{\beta}_3$ from the other health insurance experiment is 66% that from the family size experiment, and expected medical expenses for other health insurance are 80% those of family size. Similarly, the effect of job-lock from the family size experiment is 51% the effect of using $\hat{\beta}_3$ from the pregnancy experiment, while expected medical costs are 54% of those for an expectant family. This suggests that the difference in the mobility rates of the control and the experimental group between those with health insurance and those without health insurance is largely accounted for by differences in expected medical costs faced by these groups.

As presented, the measures of job-lock derived by looking at the three cost factors separately are not independent. That is, the same sample of individuals is used to look at the effect of other health insurance, the effect of family size, and the effect of pregnancy, and it is possible that those individuals who are pregnant could also be the ones with large families and the ones who have other health insurance. To get around this, one could, in each case, restrict the sample to only those individuals not affected by the other two cost factors. For example, the family size probits could be run only on those individuals who do not have other health insurance and who are not expecting. When this is done, the results (not reported) are not markedly different (the third column of Table 9 restricts the sample to only those without wife's health insurance, although those who are expecting are still included).

Making the other health insurance and pregnancy "experiments" independent is a little more difficult. While one can restrict the sample to those not expecting to look at the effect of other health insurance and similarly restrict the sample to those without other health insurance to look at the effect of pregnancy, it is difficult to make the other health insurance or pregnancy "experiments" independent of family size because family size can take several values. The results, however, are very similar if the analysis of other health insurance is done only for those who are not expecting, and likewise if we look at the effect of pregnancy only among those without other health insurance.

The estimates of job-lock found in all three experiments are robust to general changes in specification. Estimating a logit or a linear probability model of turnover rather than a probit does not change things substantively. Using education dummies rather than a linear education variable does not change the estimates of job-lock. Likewise, adding an experience-squared term or weighting the data do not change the estimates of job-lock.

V. Welfare Implications

By distorting the mobility decisions of individuals, job-lock will affect the distribution of workers across jobs. In evaluating the implications of job-lock for economic efficiency, there are three factors to consider. The first is whether an important component of productivity is match specific or whether productivity is largely invariant to the characteristics of workers. There is considerable evidence that voluntary job changes are associated with large wage increases.[40] It is unclear, however, how much of these wage increases result from better job matches and how much represent a redistribution of rents accruing to individuals who change jobs.

Suppose, for example, that workers are completely homogeneous (except for their expected medical expenses) and there are two sectors, a low productivity sector paying low wages and a high productivity sector paying high wages, so that all mobility in the economy is from the low productivity to the high productivity sector. While workers who are "locked" into the low productivity sector by their medical expenses will experience a utility loss, another worker in the low productivity sector will move to the high wage job leaving total economic output unaffected. When workers are "replaceable," therefore, job-lock will only affect the distribution of output. In contrast, if the wage increases associated with turnover are due to increased match quality between workers and firms, job-lock will result in a loss of economic efficiency.

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[40] Using the National Longitudinal Survey of Youth, Minter (1992) finds that young workers who change jobs have wage increases 18% higher than those who stay. Similarly, in a study of older (45-49) workers, Bartel and Borjas (1977) estimate that individuals who leave their jobs voluntarily have wage increase 18% higher than those who stay. Mincer (1986) calculates that there is a 16% wage increase for young workers who quit one job to take another, while older workers who quit experience a wage increase of 10%. And finally, Topel (1986) places the amount of total wage growth due to mobility at 67%.

As a prediction of the wage increase that job-locked individuals would receive had they changed jobs, these estimates are biased. Because the sample of individuals who choose to change jobs is disproportionately composed of those who receive large wage gains from doing so, the wage increase experienced by those who move overstates the wage increase that individuals who stayed on their jobs would have received had they changed jobs. Similarly, the within-job wage increase of those who stay will overstate the wage increase that individuals who moved would have received had they stayed because individuals who received large wage increases from staying are overrepresented in the sample of stayers.

To the extent that job-lock does lower productivity, a second important consideration is whether these losses are temporary or permanent. While pregnancy is a preexisting condition that comes and goes in a matter of months, some of the individuals facing job-lock will be affected by chronic conditions that last for years. Their wage losses will be compounded over time and will be much greater than the loss in a single year.

Finally, some might ask whether job-lock is a benefit, rather than a cost, for firms. If firms make job-specific investments in worker human capital, they may want to reduce turnover among their employees. This is a commonly cited reason for employer provision of pensions, and providing health insurance may also be an effective way to reduce turnover (the evidence presented in this paper does suggest that health insurance is associated with greatly reduced turnover). The effect of job-lock, however, is separate from the general mobility reducing effect of health insurance because it is the workers with high expected medical expenses who will be most likely to stay. Presumably the firm would rather reduce turnover among workers with low expected medical expenses than among those with high expenses. More importantly, job-lock is not created by the firm but is an externality imposed by the benefit policies of other firms, either because other firms exclude preexisting conditions or, less frequently, because they do not offer health insurance.

While this paper sheds some light on the degree to which job-lock reduces mobility, evaluating the associated welfare losses requires additional assumptions about the permanent versus transitory components of job-lock and about the importance of match specific human capital. While better data on health status might enable one to say something about the former, there is little empirical evidence on the latter. An explicit welfare calculation is therefore beyond the scope of this paper, although this clearly is an area which warrants further research.

VI. Conclusions and Policy Implications

The evidence presented above suggests that there is substantial health insurance-related job-lock. The change in mobility from having other health insurance is 25% greater for those with employer-provided health insurance than for those without employer-provided health insurance. In addition, individuals with larger families are less likely to leave their jobs if they have health insurance than if they do not. And finally, while having a wife who is pregnant increases mobility among those with no health insurance, it reduces mobility substantially (30% to 40%) for those who have employer-provided health insurance. These results are robust to changes in specification and in the sample over which they are estimated.

The impediment to mobility resulting from employer-provided health insurance has important implications for the current health care reform debate. Several proposals have been advanced to expand access to medical care for the currently uninsured. The first proposal calls for a Canadian-style national health care system in which the government would insure everybody publicly and set the budgets and fees for hospitals, physicians, and other health care providers (Grumbach et al. 1991). A second proposal would have the government auction off the right to insure large groups of the population to insurance companies on the condition that these insurers provide a minimum set of benefits (Diamond 1992). A third type of plan, which has been termed "Play-or-Pay", mandates that employers either provide health insurance for their employees or pay taxes to support government-sponsored health care for the uninsured (Rockefeller 1991; Bronow et al. 1991). Finally, a fourth proposal would replace the current tax deduction available to companies who provide health insurance with an individual refundable tax credit for health care expenses (Butler 1991). The relative merits of these plans depend in part on how they affect the extent of job-lock.

The only way to completely eliminate job-lock is to sever the link between employment and health insurance. The two plans which accomplish this are national health insurance and the government auction plan. Under both of these proposals, everybody would receive continuous and unconditional medical care. Individuals could therefore change jobs at will without compromising their coverage for serious medical conditions.[41]

Mandating that employers provide insurance actually strengthens the link between employment and insurance, and if this plan succeeded in giving more workers employer-provided health insurance, the extent of job-lock would likely increase. There are several factors which make the effect ambiguous, however. Many proposals for mandated benefits also call for eliminating preexisting conditions clauses, a move which would supposedly reduce job-lock. But this effect is uncertain, as many small employers would likely cease to offer health insurance absent an incentive for individuals with medical problems to seek employment elsewhere. As far as job-lock is concerned, a firm which offers no health insurance is worse than a firm which excludes preexisting conditions. Fewer firms may also offer health insurance if, as Zedlewski et al. (1992) estimate, up to 30% of employers opt to pay taxes rather than provide health insurance. The effect on job-lock resulting from the transfer of individuals from employer-provided to government-provided health insurance will

[41] In Canada there is some role for employers to provide supplemental coverage for things not covered by the government such as dental or vision care. It is unlikely, however, that these types of coverage would give rise to substantial job-lock.

depend on the relative quality of the two insurance programs. Mandated benefits could also increase the extent to which individuals have other health insurance which will mitigate some of the effect of job-lock.

The effect on job-lock of replacing the current tax deduction to businesses which provide health insurance with an individual refundable tax credit for health care expenses is also ambiguous. Even without the tax incentives currently available, large employers may still find it advantageous to offer health insurance because these firms can realize sizable gains by pooling risks. Small firms, however, do not have enough employees to significantly reduce risks by pooling and face administrative loading factors six times greater than those for large firms (Congressional Research Service 1988). If these small firms ceased to offer insurance absent a tax break, many Americans would be forced into the market for private health insurance where concern over adverse selection has given rise to policies which are typically less generous and more expensive than those provided by employers. These policies are also much more likely to exclude preexisting conditions or to require medical underwriting. This would almost surely increase the extent of job-lock, although this effect would be partially offset by the fact that more individuals would have policies which were not employment-based.

In conclusion, the results of this paper suggest that the current relationship between health insurance and employment creates significant deterrents to job mobility. While mitigating the extent of job-lock is certainly only one of the many factors that should be weighed in any proposal to reform the U.S. health care system, it warrants serious consideration. This would be a move welcomed not only by the individuals who have experienced job-lock, but by many employers as well.

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APPENDIX

Using Pregnancy to Identify Job-Lock in the NMES

There are two pieces of information one would ideally like to know in order to identify job-lock using pregnancy as a preexisting condition: 1) the date of job changes, and 2) when an individual was pregnant. Unfortunately the NMES does not provide either type of information with the precision desired. The following time-line will help illustrate the information that is available and how I use this information.

```
      A                                           C
Jan. 1, 1987                                Dec. 31, 1987
      |----------------|-------------------------|---------------------|
                First Interview                               Last Interview
                Sometime in 1987                              Sometime in 1988
                       B                                             D
```

Because the NMES is designed to collect information about medical care utilization during 1987, I only have records for children born before December 31, 1987. Although birthdays are not reported in the NMES, I can identify the date of birth for children born after January 1, 1987 and before December 31, 1987 because they are only eligible for the survey once they are born and I do know the number of days for which an individual is eligible. I therefore date the birth of infants by subtracting their days of eligibility from the last day of the year.[41] With the exception of adoption, individuals who experienced the

[41] Adults can be eligible for less than the full year if they do not reside in a surveyed household for the entire year. So, individuals who move in or out of a household will have less than 365 days of eligibility. Because my sample of married men consists only of individuals who were eligible for the entire year, I have assumed that the date of birth for children less than a year old is defined by the number of days they were eligible. If these children were moving in or out of the household for other reasons, I will measure their date of birth incorrectly. This, however, is unlikely to be a large problem because the sample consists of men who were eligible the entire year, who were married to the same spouse in the first and fourth interviews, and whose spouse's were present in both the first and last rounds.

birth of a child will have also experienced a pregnancy. However, I cannot identify individuals who were pregnant but who did not give birth until 1988. My measure of pregnancy will therefore understate the true number of individuals who were pregnant during 1987.

Unfortunately, the knowledge of my timing about job changes does not coincide directly with my knowledge of the timing of births. I can identify whether an individual changed jobs between the first interview (**B**), which occurred sometime after January 1, 1987, and the last interview (**D**), which occurred sometime in 1988. So, I do not know about job changes that occurred after January 1 but before the first interview; likewise, I do not know about births that occurred after December 31 but before the last interview (actually, I would like to know about births up until 9 months after the last interview).

In using pregnancy to identify job-lock, I ignore the information that I have on births before the first interview because the cause of job-lock for these individuals will have already passed and should therefore not affect mobility decisions between **B** and **D**. Although I would like to have information about births after December 31 through 9 months after the last interview because these pregnancies will affect mobility between **B** and **D**, the absence of this information actually strengthens my test. This is because the test will then be comparing the mobility of individuals who definitely experienced a pregnancy between **B** and **D** with a second group, most of whom were not pregnant but some of whom were. If job-lock is significant, my measured effect will by biased downward because the mobility of the control group will be contaminated by some individuals who are also actually affected by job-lock.

My first measure of pregnancy is a dummy variable for whether or not a child was born after the first interview and before December 31, 1987. My second measure is the fraction of time between the first interview and December 31, 1987 before the child was born.

TABLE 1

Fraction of Full-Time Employees with a Length-of-Service Requirement for Participation in an Employee Medical Plan

	1981	1985	1989
With Service Requirement	59%	54%	49%
Service Requirement			
1 month	18	16	13
2 months	13	8	7
3 months	19	19	17
4+ months	9	11	12

Source: U.S. Department of Labor, Bureau of Labor Statistics. *Employee Benefits in Medium and Large Firms*, various issues.

TABLE 2

Sources of Health Insurance Coverage

	Fraction who have coverage through	Fraction with employer coverage who also have
Own employment	75.0%	100%
Spouse's employment	33.5	36.0
Union	4.5	0.5
Other group policy	0.4	0.3
Non-group policy	2.3	0.6
CHAMPUS	2.1	1.7
Medicaid	0.5	0.0
Any non-employer source	41.0	37.5

Author's calculations using a sample of 2978 married men from the 1987 National Medcial Expenditure Survey.

TABLE 3

Sample Selection Criteria
1987 National Medical Expenditure Survey

Sample of Married Men	
Initial Sample	38,446
Individuals Deleted:	
Females	20,352
Initial Age <20 or >55	9,759
Not a full-year respondent or full-year eligible	1,226
Missing at round 1 or round 4 interview	2
Not married at round 1 and round 4 interview	2,888
Different spouse at round 1 and round 4 interview	24
Spouse absent or ineligible at round 1 interview	41
Not household respondent or spouse of respondent	56
Not employed at round 1 interview	369
Self-employed	568
Full- or part-time student	152
Miscellaneous problems with matching spouses	4
Missing data	28
Final Sample of Married Men	2,978

TABLE 4

Descriptive Statistics
1987 National Medical Expenditure Survey

Variable	Mean	Standard Error	Minimum	Maximum
Union	.248	.432	0	1
Black	.151	.359	0	1
Education	12.88	2.93	0	18
Experience	19.18	9.11	0	47
Hourly wage	$11.53	$7.23	$1.06	$192.31
Log hourly wage	2.30	.554	.058	5.26
Log family income	10.51	.808	0	12.99
Log wife's income	7.72	.332	0	12.52
Health Insurance	.752	.432	0	1
Other Health Insurance	.406	.491	0	1
Family Size	3.36	.121	2	12
Pregnant	.064	.246	0	1

Author's calculation using a sample of 2978 married men from the 1987 National Medical Expenditure Survey.

TABLE 5

Effect of Health Insurance on the Turnover Probability of Married Men

	Simple Probit						RE Probit	
A. Coefficient Estimates								
Union	-.357	(.0842)	-.345	(.0861)	-.342	(.0878)	-.287	(.1054)
Black	-.031	(.0874)	-.022	(.0893)	-.041	(.0898)	-.032	(.0750)
Education	-.019	(.0139)	-.007	(.0142)	-.007	(.0143)	-.006	(.0122)
Experience	-.018	(.0037)	-.016	(.0038)	-.016	(.0038)	-.014	(.0050)
Log hourly wage	-.164	(.0619)	-.080	(.0639)	-.078	(.0644)	-.067	(.0570
Months b/t interviews	.071	(.0256)	.074	(.0281)	.077	(.0282)	–	–
Health Insurance (β_1)	–	–	-.626	(.0696)	-.715	(.0950)	-.586	(.1694)
Other Health Ins. (β_2)	–	–	–	–	-.039	(.1075)	-.029	(.0852)
HI * Other HI (β_3)	–	–	–	–	.211	(.1339)	.167	(.1106)
σ_ϵ	–	–	–	–	–	–	.536	(.3693)
Log Likelihood	-1040.55		-997.05		-994.73		-996.01	
B. Magnitude of Job-Lock								
Test 1: $\beta_2+\beta_3>0$ $\hat{\beta}_2+\hat{\beta}_3$ [p-value]	–	–	–	–	.171	[.017]	.138	[.031]
Test 2: $\beta_3>0$ $\hat{\beta}_3$ [p-value]	–	–	–	–	.211	[.058]	.167	[.066]
Degree of Job-Lock	–		–		26% to 30%		25% to 28%	

The table shows estimates of the equation:

$$\textit{Probability of Changing Jobs} = f(\beta_0 + \beta_1 \cdot HI + \beta_2 \cdot Other\ HI + \beta_3 \cdot HI * Other\ HI + z'\gamma).$$

The random effects probit is described in the text. The top panel gives coefficient estimates, while the bottom panel gives the estimated impact of job-lock. Coefficients for 5 industry and 4 occupation dummies are not reported. The sample consists of 2978 married men from the 1987 National Medical Expenditure Survey. The degree of job-lock is the reduction in the mobility rate of those who have only employer-provided health insurance relative to those who have both employer-provided and other health insurance. More detail about this calculation is given in the text.

TABLE 6

Sources of Other Health Insurance and the Turnover Probability of Married Men

Independent Variables	Coefficient Estimates		Magnitude of Job-Lock				
			Test 1: $\beta_2+\beta_3>0$ $\hat{\beta}_2+\hat{\beta}_3$	[p-value]	Test 2: $\beta_3>0$ $\hat{\beta}_3$	[p-value]	Degree of Job-Lock
Union	-.327	(.0924)					
Black	-.044	(.0901)					
Education	-.007	(.0144)					
Experience	-.016	(.0038)					
Log hourly wage	-.071	(.0646)					
Months b/t interviews	.077	(.0283)					
Health Insurance (β_1)	-.761	(.0931)					
Wife's Health Insurance (β_2)	-.096	(.1241)					
HI * Wife's HI (β_3)	.285	(.1480)	.188	[.010]	.285	[.027]	28% to 37%
Union Health Insurance (β_2)	-.137	(.1704)					
HI * Union HI (β_3)	.221	(.5688)	.084	[.439]	.221	[.349]	14% to 28%
CHAMPUS (β_2)	.066	(.2892)					
HI * CHAMPUS (β_3)	-.002	(.4243)	.064	[.419]	-.002	[.502]	4% to 11%
Nongroup HI (β_2)	-.171	(.2052)					
HI * Nongroup HI (β_3)	.517	(.4789)	.346	[.212]	.517	[.140]	44% to 55%
Log Likelihood	-993.12						

The estimated equation is a probit as in Table 5. The first column gives coefficient estimates; the remaining columns show the estimated impact of job-lock. Coefficients for 5 industry and 4 occupation dummies are not reported. The sample consists of 2978 married men from the 1987 National Medical Expenditure Survey. The degree of job-lock is the reduction in the mobility rate of those who have only employer-provided health insurance relative to those who have both employer-provided health insurance and other health insurance.

TABLE 7

Effect of Wife's Health Insurance on the Turnover Probability of Married Men

Independent Variables	Full Sample					
	Wife's HI		Add Family Income		Add Wife's Income	
A. Coefficient Estimates						
Union	-.347	(.0863)	-.336	(.0864)	-.348	(.0863)
Black	-.043	(.0899)	-.050	(.0900)	-.052	(.0902)
Education	-.007	(.0143)	-.003	(.0144)	-.010	(.0144)
Experience	-.016	(.0037)	-.015	(.0038)	-.016	(.0038)
Log hourly wage	-.079	(.0642)	-.052	(.0659)	-.080	(.0642)
Months b/t interviews	.077	(.0282)	.076	(.0282)	.076	(.0282)
Log family income	–	–	-.080	(.0378)	–	–
Log wife's income	–	–	–	–	.019	(.0109)
Health Insurance (β_1)	-.720	(.0838)	-.697	(.0847)	-.913	(.0839)
Wife's Health Ins. (β_2)	-.069	(.1203)	-.032	(.1238)	-.115	(.1230)
HI * Wife's HI (β_3)	.260	(.1446)	.236	(.1453)	.264	(.1447)
Log Likelihood	-994.12		-991.93		-992.50	
B. Magnitude of Job-Lock						
Test 1: $\beta_2+\beta_3 > 0$ $\hat{\beta}_2+\hat{\beta}_3$ [p-value]	.191	[.009]	.204	[.006]	.148	[.039]
Test 2: $\beta_3 > 0$ $\hat{\beta}$ [p-value]	.260	[.036]	.236	[.052]	.264	[.034]
Degree of Job-Lock	28% to 38%		30% to 34%		23% to 39%	

The estimated equation is a probit as in Table 5. The top panel gives coefficient estimates, while the bottom panel gives the estimated impact of job-lock. Coefficients for 5 industry and 4 occupation dummies are not reported. The full sample consists of 2978 married men from the 1987 National Medical Expenditure Survey. The degree of job-lock is the reduction in the mobility rate of those who have only employer-provided health insurance relative to those who have both employer-provided and wife's health insurance.

TABLE 8

Effect of Wife's Health Insurance on the Turnover Probability of Married Men

Independent Variables	Men Whose Wives Work							
	Wife's HI		Add Family Income		Add Wife's Income		Add Wife's Wage	
A. Coefficient Estimates								
Union	-.273	(.1070)	-.258	(.1070)	-.273	(.1070)	-.269	(.1072)
Black	.026	(.1034)	.010	(.1040)	.026	(.1034)	.028	(.1036)
Education	-.006	(.0177)	-.001	(.0179)	-.007	(.0178)	-.003	(.0180)
Experience	-.014	(.0048)	-.013	(.0048)	-.014	(.0048)	-.014	(.0048)
Log hourly wage	-.228	(.0807)	-.199	(.0820)	-.229	(.0810)	-.213	(.0819)
Months b/t interview	.023	(.0353)	.019	(.0354)	.024	(.0353)	.023	(.0353)
Log family income	--	--	-.109	(.0463)	--	--	--	--
Log wife's income	--	--	--	--	.009	(.0414)	--	--
Log wife's wage	--	--	--	--	--	--	-.095	(.0849)
Health Insurance (β_1)	-.867	(.1161)	-.840	(.1169)	-.867	(.1161)	-.868	(.1163)
Other Health Ins. (β_2)	-.189	(.1330)	-.155	(.1341)	-.195	(.1353)	-.161	(.1355)
HI * Other HI (β_3)	.441	(.1674)	.415	(.1680)	.441	(.1674)	.436	(.1675)
Log Likelihood	-643.33		-631.66		-634.81		-633.71	
B. Magnitude of Job-Lock								
Test 1: $\beta_2+\beta_3 > 0$ $\hat{\beta}_2+\hat{\beta}_3$ [p-value]	.251	[.007]	.260	[.006]	.247	[.009]	.274	[.004]
Test 2: $\beta_3 > 0$ $\hat{\beta}$ [p-value]	.441	[.004]	.415	[.007]	.441	[.004]	.435	[.005]
Degree of Job-Lock	37% to 51%		38% to 49%		36% to 51%		40% to 51%	

The estimated equation is a probit as in Table 5. The top panel gives coefficient estimates, while the bottom panel gives the estimated impact of job-lock. Coefficients for 5 industry and 4 occupation dummies are not reported. The sample consists of 1858 married men whose wives work. The degree of job-lock is the reduction in the mobility rate of those who have only employer-provided health insurance relative to those who have both employer-provided and wife's health insurance.

TABLE 9

Using Family Size as a Proxy for Expected Medical Expenses in Examining Turnover

Independent Variables	Probit						RE Probit	
	Conservative		Liberal		No Wife's HI		Conservative	
A. Coefficient Estimates								
Union	-.345	(.0865)	-.342	(.0864)	-.491	(.1132)	.283	(.1129)
Black	-.040	(.0899)	-.012	(.0894)	-.009	(.1188)	-.032	(.0735)
Education	-.009	(.0144)	-.008	(.0144)	.014	(.0175)	-.007	(.0119)
Experience	-.018	(.0038)	-.018	(.0038)	-.018	(.0048)	-.014	(.0061)
Log hourly wage	-.075	(.0644)	-.079	(.0642)	-.116	(.0770)	-.064	(.0570)
Months b/t interviews	.078	(.0283)	.077	(.0282)	.097	(.0356)	–	–
Health Insurance (β_1)	-.507	(.0864)	-.491	(.1111)	-.463	(.1735)	-.407	(.1692)
Family Size (β_2)	-.024	(.0299)	-.019	(.0349)	-.012	(.0402)	-.018	(.0252)
HI * Family Size (β_3)	-.053	(.0232)	-.047	(.0303)	-.088	(.0467)	-.043	(.0237)
σ_θ	–	–	–	–	–	–	.508	(.5065)
Log Likelihood	-992.44		-993.86		-636.20		-993.74	
B. Magnitude of Job-Lock								
Test 1: $\beta_2+\beta_3<0$ $\hat{\beta}_2+\hat{\beta}_3$ [p-value]	-.077	[.004]	-.066	[.009]	-.100	[.007]	-.061	[.028]
Test: $\beta_3<0$ $\hat{\beta}_3$ [p-value]	-.053	[.011]	-.047	[.062]	-.088	[.030]	-.043	[.035]
Degree of Job-Lock	33% to 45%		30% to 39%		48% to 54%		32% to 43%	

The estimated equation is the same as in Table 5 except that *Other HI* is replaced with *Family size*. Coefficients for 5 industry and 4 occupation dummies are not reported. The sample in columns 1, 2 and 4 consists of 2978 married men from the 1987 National Medical Expenditure Survey, while that in column 3 includes only the 1981 men whose wive's do not have employer-provided health insurance. The bottom panel gives the estimated impact of job-lock.

TABLE 10

Using Pregnancy as a Proxy for Expected Medical Expenses in Examining Turnover

Independent Variables	Measure of Pregnancy								RE Probit Time Pregnant Full Sample	
	Percent of Time Pregnant				Had a Baby					
	Full Sample		20-39		Full Sample		20-39			
A. Coefficient Estimates										
Union	-.345	(.0862)	-.362	(.1169)	-.344	(.0861)	-.361	(.1168)	-.298	(.1361)
Black	-.020	(.0892)	.111	(.1119)	-.018	(.0892)	.113	(.1118)	-.016	(.0766)
Education	-.007	(.0142)	-.032	(.0203)	-.006	(.0142)	-.031	(.0203)	-.006	(.0124)
Experience	-.016	(.0038)	-.043	(.0086)	-.016	(.0039)	-.042	(.0086)	-.014	(.0063)
Log hourly wage	-.081	(.0639)	-.023	(.0867)	-.082	(.0639)	-.026	(.0866)	-.071	(.0618)
Months b/t interviews	.073	(.0282)	.068	(.0359)	.072	(.0282)	.067	(.0359)	--	--
Health Insurance (β_1)	-.597	(.0715)	-.600	(.0914)	-.596	(.0722)	-.588	(.0930)	-.507	(.2089)
Pregnant (β_2)	.707	(.3705)	.557	(.4004)	.323	(.1889)	.321	(.1999)	.575	(.4092)
HI * Pregnant (β_3)	-.906	(.4628)	-.825	(.4979)	-.401	(.2435)	-.451	(.2595)	-.746	(.5163)
σ_θ	--	--	--	--	--	--	--	--	.579	(.5237)
Log Likelihood	-994.94		-630.95		-995.46		-630.74		-996.06	
B. Magnitude of Job-Lock										
Test 1: $\beta_2+\beta_3<0$ $\hat{\beta}_2+\hat{\beta}_3$ [p-value]	-.200	[.241]	-.268	[.186]	-.078	[.311]	-.130	[.219]	-.171	[.252]
Test 2: $\beta_3<0$ $\hat{\beta}_3$ [p-value]	-.906	[.025]	-.825	[.049]	-.401	[.050]	-.451	[.041]	-.746	[.074]
Degree of Job-lock	31% to 67%		42% to 71%		15% to 40%		23% to 50%		30% to 66%	

The estimated equation is the same as in Table 5 except that *Other HI* is replaced with *Pregnant*. Coefficients for 5 industry and 4 occupation dummies are not reported. The full sample consists of 2978 married men from the 1987 National Medical Expenditure Survey, while there are 1708 individuals in the 20-39 sample. The bottom panel gives the estimated impact of job-lock.

TABLE 11

Calibrating the Magnitude of Job-Lock

Experiment	Family	Estimated Effect[a]		Expected Medical Expenses (1984 $)[b]
		$\beta_2+\beta_3$	β_3	
Other HI	2 children	.171	.211	$2318
Family Size	4 children	.462	.318	$2892
Pregnancy	1 child, expecting another	.201	.619	$5371

[a] The estimated effect of job-lock for other health insurance is taken from column 3 of Table 5. For family size, the coefficients from column 1 of Table 9 are multiplied by 6, the family size of the base group. In the case of pregnancy, the coefficients from column 2 of Table 10 are multiplied by .75 (the fraction of a year for which an expectant mother is pregnant).

[b] For other health insurance and family size, expected medical expenses of $287 for children and $872 for adults are taken from Table 5 of Manning et al. (1987). For pregnancy, expected medical expense is calculated as the cost of 1 child and 2 adults from Manning et al. plus the average cost of pregnancy and delivery of $3340 (Health Insurance Association of America 1989; deflated by the medical care CPI between 1984 and 1988).

CHAPTER 2

Post-Retirement Health Insurance and the Decision to Retire

The interest over the past decade in post-retirement health insurance benefits (PRHBs) has been confined largely to their impact on corporate financial statements. Recent changes in the acceptable methods of accounting for these benefits have affected both the profits and the value of firms which provide post-retirement health insurance (Mittelstaedt and Warshawsky 1991). Given the importance of health insurance in labor market decisions (Madrian 1992), another relevant but relatively unstudied issue is the relationship between post-retirement health insurance and the worker's retirement decision. All retirees receive health insurance coverage through Medicare after age 65, but unlike Social Security, there is no early retirement age before 65 when one qualifies for Medicare. For many individuals this is not a problem because their employer provides post-retirement health insurance benefits. The majority of workers, however, do not receive such benefits. Although health insurance will not be a concern for individuals who want to retire after age 65, there could be many individuals who would like to retire before age 65 but who do not want to give up their employer-provided health insurance. This paper attempts to quantify the effect of post-retirement health insurance on the date of retirement.

Previous research on the age at retirement has typically found a spike in the retirement hazard at age 65 which is only partially explained by pension plan and Social Security incentives which encourage retirement at specific ages. One potential explanation for this spike is that Medicare eligibility creates an additional incentive not to retire until age 65 (Lumsdaine, Stock and Wise 1992).

From a policy perspective, the relationship between health insurance and retirement is relevant to both firms and the government for several reasons. The link between retirement and health insurance has important implications for Medicare. There have been several calls to change the age of Medicare eligibility. One recently introduced legislative proposal would make Medicare available to all individuals over age 60.[1] Another proposal would extend coverage to all Social Security retirement recipients and to spouses (over age 62) of Medicare beneficiaries.[2] Yet another would postpone eligibility until age 67.[3] The availability of Medicare as a source of health insurance coverage for early retirees will certainly affect federal government expenditures on Medicare which currently amount to $120 billion, over 8% of total government expenditures and 20% of national health care expenditures.

From the firm's perspective, the availability of post-retirement health insurance may be a significant factor in its ability to encourage early retirement among its older employees. It will also directly affect the firm's expenditures on medical care, wages and

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[1] See the Health Insurance Coverage and Cost Containment Act of 1991 (H.R. 3205) introduced by Rep. Dan Rostenkowski.

[2] See the Medicare Eligibility Expansion Act of 1991 (H.R. 1444) introduced by Rep. Pete Stark.

[3] "Proposals to Avoid Insolvency in Medicare Trust Fund Bring Sharp Debate," *New York Times*, 22 January 1989, p. 23.

pensions. The relationship between health insurance and retirement will be an important decision parameter as many companies reevaluate their decision to offer post-retirement health insurance in the wake of more stringent accounting standards.

Anecdotal evidence suggests that health insurance is an important determinant in the decision concerning when to retire. A recent Gallup poll reports that 63 percent of working Americans "would delay retirement until becoming eligible for Medicare (age 65) if their employers were not going to provide health coverage" despite the fact that 50 percent "said they would prefer to retire early--by age 62" (Employee Benefit Research Institute 1990). A simple tabulation of post-retirement health insurance coverage by age at retirement (Table 1) gives further evidence that this is an important factor in the decision to retire. As Table 1 shows, workers who retire at younger ages are much more likely to have post-retirement health insurance than workers who retire at older ages.

Using data from the 1987 National Medical Expenditure Survey and the Survey of Income and Program Participation, I estimate a truncated regression model to explain the age at retirement. I find that those whose employers provided post-retirement health insurance retired 6-12 months earlier than those whose employers did not provide such insurance. This result is robust to a variety of different empirical specifications. It is not clear, however, whether the estimated effect is really that of having retiree health insurance or whether it is explained by a correlation between the provision of retiree health insurance and job quality, the generosity of pension benefits, or other incentives for early retirement that exist in these firms.

The paper will proceed as follows. Section I provides some background on the availability and structure of post-retirement health insurance benefits. The estimation strategy is outlined in Section II, and is followed in Section III by a description of the data. Section IV presents the empirical results. The paper concludes in Section V with a discussion of the results as they relate to government and firm provision of health benefits for the retired.

I. The History and Structure of Post-Retirement Health Insurance

Before the inception of Medicare, private health insurance for the elderly was not widely available. To meet the health care needs of their retired employees, firms first began providing post-retirement health insurance benefits in the late 1940s and 1950s. At that time the labor force was growing, there were relatively few retirees, and real medical costs were substantially lower than they are today. The costs of pr viding these benefits were thus, for the most part, inconsequential. When Medicare assumed the major portion of health care expenditures for the elderly in 1965, providing such benefits became more attractive and many firms began to furnish supplemental health care benefits for their retirees to cover deductibles and other costs not paid by Medicare.

Although there is no consistent time series data available which shows the rise in the availability of post-retirement health insurance, this growth can be seen in Tables 2 and 3 which look in a cross-section at coverage rates by age and year of retirement. Table 2 shows that the young retired (those 65-75) are much more likely to have PRHBs than the old retired (those over age 80). This suggests growth in the availability of PRHBs from about 20% in the 1960s when the old retired left their jobs to about 45% in the late 1970s and early 1980s when the young retired left their jobs. When we look in Table 3 at coverage rates by year of retirement, we see a similar pattern. Less than 30% of those retiring in the 1960s received post-retirement health insurance, while around 45% of those retiring during the late 1970s and 1980s received such coverage.

Information on the availability of post-retirement health insurance among the currently employed support these figures. Data from the August 1988 Current Population Survey supplement on retiree health insurance suggest that 48.7% of employees over age 40 have health insurance benefits that continue beyond retirement (Employee Benefit Research Institute 1992, Table 6.28). Firm level data paint a similar picture: in 1988, 45% of full-time employees in medium and large firms were eligible for retiree health benefits upon retirement (U.S. Department of Labor 1988, Table 60).

There are significant differences in the characteristics of firms which provide post-retirement health insurance benefits and in the characteristics of individuals who receive them. As might be expected given the relationship between firm size and the provision of other employee benefits, large firms are much more likely to provide PRHBs than small firms. Only 15% of firms with less than 100 workers provide such benefits, while over 80% of firms with more than 2500 employees supply post-retirement health insurance benefits (Chollet 1989; U.S. Department of Labor 1990a).

Table 4 illustrates the considerable variation within industry and occupation in the receipt of post-retirement health insurance. While over two-thirds of the individuals who retire from public administration receive post-retirement health insurance, less than 15% of those in sales, personal services, and agriculture do so. As might be expected, industries which are more likely to provide pensions and health insurance to their active employees are more likely to provide post-retirement health insurance as well.

One surprising feature of many post-retirement health insurance benefit plans is that eligibility is not necessarily tied to the receipt of other retirement benefits such as a pension or to an employee's length of service (Table 5).[4] For only one-quarter of workers are post-retirement health insurance benefits contingent on pension recipiency, while over one-third of employees face no prerequisites other than retirement for the receipt of benefits. Another significant difference between PRHBs and pension benefits is that PRHBs are rarely portable. That is, an employee's entitlement to post-retirement health insurance does not become vested and is forfeited upon departure from a firm for reasons other than retirement.

The coverage of post-retirement health insurance plans is typically identical to that provided to active employees, with nearly 90% of firms providing the same coverage to retirees under age 65 as to current employees (A. Foster Higgins & Co. 1990) . Differences in coverage, where they exist, are usually minor, such as reduced coverage for vision care, prescription drugs, or other "noncore" medical benefits. Some companies also require retiree contributions to health insurance premiums although their plans for active employees are noncontributory. In general, however, these contributions are "reasonable", with monthly premiums in over 90% of firms not exceeding $25 in 1985 (Hewitt 1990).

For retirees over age 65, employer-coverage must be coordinated with the coverage provided by Medicare (de Lissovoy, et al. 1990). The least generous plans simply cover the employee's Medicare Part B premium or provide supplemental "Medi-Gap" type coverage which pays for deductibles, copayments and items such as prescription drugs not covered by

[4] The receipt of pensions and post-retirement health insurance benefits are nevertheless highly correlated. Data from the 1984 Survey of Income and Program Participation suggest that among men age 65 and over who are not in the labor force, 88% percent of those with post retirement health insurance also receive pension benefits.

Medicare. About 14% of the employers which offer retiree health insurance provide this type of coverage.[5] Typically, however, firms also include their retirees over age 65 in their regular health plan for active employees (and retirees under age 65). There are three general ways in which firms coordinate their own coverage with the duplicate coverage offered by Medicare.

The most popular method, adopted by 39% of firms, is known as "Medicare carve-out". Under this type of plan, the firm reduces the amount it would pay in the absence of Medicare by the amount that Medicare pays and the retiree pays the same deductible and copayment as would have been paid without Medicare coverage. "Medicare exclusion" plans, used by 21% of employers, subtract Medicare's payment from the cost of the claim filed by the retiree and then use this "residual" amount as the claim to be covered by the firm. Under this plan, the employee's deductible will be the same as under the "Medicare carve-out" plan but the amount the employee pays as coinsurance will be lower. The third and most generous method is known as "Coordination of Benefits". This type of plan is offered by 26% of employers. In this case the firm calculates the benefits payable under its own plan and then applies the amount Medicare would pay to the coinsurance the employee would face in the absence of Medicare. This plan often leaves the retiree with little or no medical expenses not paid for either by Medicare or the former employer.

An example best illustrates the differences in the amount paid by the retiree and the firm under these three methods of coordinating benefits. Suppose the retiree has a medical expense for which the billed charge is $1,500 and the Medicare allowed charge is $1,000 and

[5] The fraction of firms which use the various methods of coordinating retiree health insurance with Medicare is taken from A. Foster Higgins (1990).

assume that both the firm and Medicare have a $100 deductible and require a 20% copayment. In the absence of Medicare, the individual would pay a $100 deductible and $280 in coinsurance on the remaining $1400 of the billed charge. Altogether, the individual would pay $380 and the firm $1,120. In the absence of the firm's retiree health insurance plan, the individual would pay a $100 deductible, $180 in coinsurance on the remaining $900 of the Medicare allowed charge, and the $500 difference between the billed charge and the Medicare charge. In this case, Medicare pays $720 and the individual pays $780.

Under a "Medicare carve-out" plan, the individual pays a $100 deductible and a $280 copayment as in the absence of Medicare, Medicare pays $720, and the firm pays the remaining $500. With a "Medicare exclusion" plan, the individual copayment is calculated on the $780 of the claim not paid by Medicare, leaving the individual to pay $236 ($100 deductible plus a $136 copayment) while the firm pays the remaining $544. Under "Coordination of Benefits", the employee is responsible only for the $100 deductible while the firm pays $680 and Medicare pays $720.

The cost to employers of providing post-retirement health insurance depends on several factors including the method used to coordinate benefits and the generosity of the company's health plan. Because Medicare pays some of the costs for individuals over age 65 that would otherwise be paid by firms, the most significant determinant of a firm's total expenditures on retirement health insurance is the number of retirees under age 65. Expenditures on pre-65 retirees are typically double those for retirees over age 65; in 1989, the median cost to firms of providing retiree health insurance to those under age 65 was $2,246 while that for retirees over age 65 was $1,033 (Hewitt 1990).

II. Estimating the Effect of Retiree Health Insurance on Age at Retirement

The rationale for why the availability of post-retirement health insurance benefits should affect the retirement decision is straightforward. As Table 6 shows, the vast majority of individuals in the U.S. receive their health insurance coverage through their own or a family member's employment. This insurance, however, is not typically portable. That is, when an individual quits his or her job, the insurance coverage associated with that job ceases as well.[6] Although individuals have the option of purchasing health insurance in the private market, there are several reasons why this may be unattractive for older individuals contemplating retirement (and younger individuals as well).

The first is that employers can provide insurance at a price much lower than the price faced by individuals in the private market. By pooling the risks of many individuals, employers can reduce adverse selection and lower administrative expenses. These two factors alone reduce the cost of providing insurance in large firms by 45% (Congressional Research Service 1988). In 1989, firms in Massachusetts spent an average of $3882 on health insurance for each employee, which is equivalent to $5050 in current dollars. In contrast, the cost of a basic individual policy from Blue Cross/Blue Shield exceeds $6000. Not only is this substantially more than the cost faced by employers, but it represents 18% of the average family income of early retirees in Massachusetts.[7] A potentially more important factor than cost, however, is the fact that the Blue Cross/Blue Shield policy comes with a 3-year exclusion on preexisting conditions. Such exclusions are typical of most

[6] Federal legislation under COBRA (1985) now allows individuals to maintain their health insurance through a former employer for up to 18 months; the individual, however, must pay the full cost of the coverage. Gruber and Madrian (1993) examine the effects of this legislation and similar state laws on the age at retirement of older workers.

[7] Author's calculation using data from the 1990 Current Population Survey.

employer-provided policies and almost all individual policies. For individuals with health problems, they make the relative cost of health insurance in the private market even greater.

The cost and risk associated with buying health insurance in the private market may therefore serve as substantial deterrents to early retirement. Employer provision of retiree health insurance increases the attractiveness of early retirement along two dimensions. First, it augments the opportunity set available to the retired worker (a wealth effect). Second, it reduces the variance in post-retirement consumption associated with the uncertainty about medical expenditures. We would therefore expect that among those covered by employer-provided health insurance, those whose coverage extends beyond retirement will be more likely to retire than those whose coverage does not. In other words, the availability of post-retirement health insurance should increase the retirement hazard. This effect should be weaker after age 65 when all individuals become eligible for Medicare but should not be expected to go away entirely unless Medicare is a perfect substitute for employer-provided health insurance.[8]

To test for these effects, one would ideally like to follow a group of not yet retired individuals over time, some of whom had health insurance that would continue past retirement and some of whom did not, to see whether those with retiree health insurance were more likely to retire than those without such insurance. Unfortunately, there is no longitudinal data currently available with information about whether or not the health

[8] This is unlikely to be true as Medicare coverage is less generous than most employer-provided policies. On average, Medicare covers only about half of the medical expenses of the elderly.

insurance coverage of working individuals will continue after retirement.[9] There are several sources of data, however, that allow one to identify whether or not the health insurance coverage of those currently retired comes from a former employer. Because I cannot follow those not yet retired, I look at the retirement decision retrospectively and consider the effect of post-retirement health insurance on the age at retirement among those currently retired. The actual equation estimated is of the following form

$$y_i = \beta' \mathrm{x}_i + \gamma \cdot RHI_i + \varepsilon_i \qquad \varepsilon_i \sim N(0,\sigma^2) \tag{1}$$

where y_i is age at retirement, x_i is a vector of demographic characteristics, and RHI is an indicator variable equal to 1 if the individual has retiree health insurance.

This estimation strategy has one obvious problem: we do not observe the age of retirement for those who are not retired. As is typical with this type of censoring, the forced exclusion of these individuals from the regression will tend to bias the estimated coefficients. With the upper tail of the retirement age distribution truncated, the average retirement age in the remaining sample will be too low. In a univariate setting (where post-retirement health insurance is the only variable which explains retirement) and with only one cohort of individuals, this truncation will lead to an understatement of the effect of retiree health insurance if such insurance actually does raise the retirement hazard at all ages. This is because at every age, a higher fraction of those without retiree health insurance will be excluded relative to those with retiree health insurance leading to a greater downward bias in the average retirement age for those without retiree health insurance. In a multivariate

[9] The new Heath and Retirement Survey (HRS) is collecting this type of information. The first wave of data is scheduled to be released in late 1993. To conduct a longitudinal analysis of the retirement decision, however, one would have to wait until at least late 1995 when the second wave of the data becomes available as well.

setting, however, and with many cohorts each truncated at different ages, the direction of the bias is not unambiguous.

Table 7 illustrates the potential severity of sample truncation in looking at early retirement. In the 1987 National Medical Expenditure Survey, only 20% of those aged 55-59 report having ever retired; the remaining 80% would therefore have to be excluded from any estimation of the average age at retirement. Because the magnitude of the bias decreases with the age at which the truncation takes place, one way to reduce the bias is to restrict the sample to cohorts for which a substantial fraction of individuals have already retired. For example, one could look only at individuals over age 70, an age at which 85% of individuals report ever having retired.

There are, however, econometric methods that explicitly account for truncation in the estimation (Hausman and Wise 1977; Maddala 1983). In the context of retirement, the point of truncation for individuals not yet retired is their current age. The density function of y_i, the age at retirement, is now that of a truncated normal:

$$f(y_i | y_i \leq Age_i) = \frac{(1/\sigma)\phi[(y_i - \beta' \mathrm{x}_i - \gamma \cdot RHI_i)/\sigma]}{\Phi[Age_i - \beta' \mathrm{x}_i - \gamma \cdot RHI_i)/\sigma]} \quad . \tag{22}$$

The corresponding log-likelihood function is given by

$$\begin{aligned} \log L = -N \log[(2\pi)^{1/2}\sigma] - \frac{1}{2}\sum_i \left[\frac{y_i - \beta' \mathrm{x}_i - \gamma \cdot RHI_i}{\sigma}\right]^2 \\ -\sum_i \log\Phi\left[\frac{Age_i - \beta' \mathrm{x}_i - \gamma \cdot RHI_i}{\sigma}\right] \quad . \end{aligned} \tag{23}$$

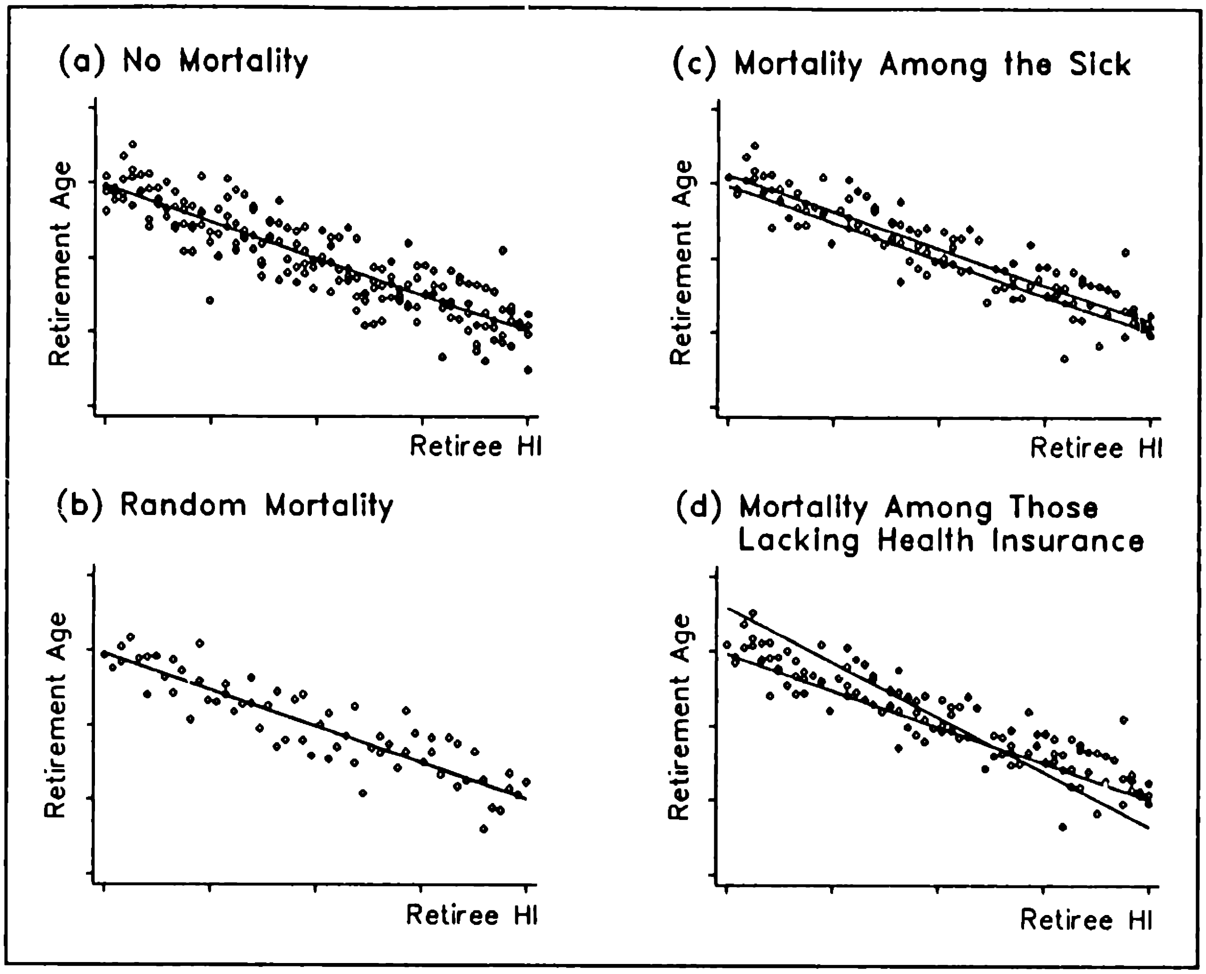
Note that it is not possible to estimate the more commonly used censored-regression model in this context because for individuals who have not retired, the values of *both* the left- and some right-hand-side variables are missing; not only do we not observe a retirement age for those not yet retired, but we also cannot infer whether or not these individuals have retiree health insurance until they retire.

A second problem potential problem, for both OLS and the truncated regression model, is that of mortality. Only those who are alive when the data are collected will be included in the sample. In other words, the sample is selected on the basis of not having already died. With an older sample, this type of selection is considerable: of those individuals who are 55 today, 83% are expected to reach 65, 54% are expected to reach 75, and only 21% are expected to reach 85 (Faber 1982).[10]

Figure 1 illustrates how mortality could affect the estimated effect of retiree health insurance on the age of retirement (this Figure assumes that retiree health insurance is a continuous rather than a discrete variable). The baseline case of no mortality is shown in (a). If the mortality that occurs is completely random, as in (b), this will make the data more sparse everywhere but should not bias the estimated coefficients. Of course, mortality might not be random. In particular, we might expect that those in poor health will be more likely to die, and we know that individuals in poor health tend to retire early. This is shown in (c), in which those with low values of ε are excluded from the sample. If mortality is only related to ε and not to retiree health insurance, this will bias the estimate of the constant but will not affect the estimated slope parameters. The potential problems arise in (d), in which

[10] It is interesting to note that the mortality bias is less severe at younger ages but this is the part of the sample where truncation is most likely to be a problem. Conversely, at older ages where the selection due to mortality is substantial there is much less truncation.

FIGURE 1: Mortality Bias in Estimating the Effect of Retiree Health Insurance



mortality is more likely among those who retire early *and* who don't have retiree health insurance. This type of differential mortality could be possible if retiree health insurance improves the access to health care and this subsequently reduces the risk of dying. In this case, the estimated effect of retiree health insurance on age at retirement will be too large.[11]

McClellan and Skinner (1993) find some evidence that income and mortality are correlated. Insofar as retiree health insurance and income are also correlated, this bias may

[11] If this bias is present, the growth over time in the availability of retiree health insurance shown in Tables 2-3 will be understated.

be present. Explicitly accounting for this bias, however, would entail estimating the relationship between retiree health insurance and mortality, something that is beyond the scope of this paper. The results, therefore, should be interpreted with a note of caution.

A third bias could arise if there is selection by individuals into jobs that provide retiree health insurance on the basis of individual preferences for early retirement. There are two factors, however, that mitigate the potential severity of this bias. The first is that a large fraction of individuals simply do not know whether or not their job provides retiree health insurance until they actually retire. The August 1988 Current Population Survey asked individuals over age 40 with employer-provided health insurance whether or not their health insurance would continue should they retire. One-third of those aged 40-44 did not know, and, even more striking, one-quarter of those aged 60-64 didn't know either. The second factor is that, as shown below, individuals have typically been on the job from which they eventually retire for literally decades.[12]

Fraction of Individuals Who Retire with Tenure of:

<10 yrs	11-15 yrs	16-20 yrs	21-25 yrs	26-30 yrs	31-35 yrs	36-40 yrs	40+ yrs
6.5%	4.6%	9.6%	10.7%	13.0%	18.9%	18.5%	22.9%

While possible, it is unlikely that individuals are selecting into jobs on the basis of retiree health insurance, a benefit they will likely not receive for 20 or 30 years and which, in contrast to pension benefits, the firm has no legal entitlement to provide.

[12] Author's calculation using data from the SIPP-CJR sample described below.

III. Data

The empirical work uses data from two different sources. The first is the 1987 National Medical Expenditure Survey (NMES). This survey of approximately 14,000 households (38,446 individuals) collected detailed information about health status, health insurance, and medical care utilization in 1987. It also collected some information about current and past labor force participation. In particular, all individuals over age 55 were asked whether or not they had ever retired from a job or business. If so, they were then asked the date on which they retired, the industry of their former employer and their occupation while working, and whether or not they currently received any health insurance benefits through their former employer. Through these questions, I can therefore identify the age at which an individual retired and if the job from which an individual retired offered retiree health insurance.[13] The disadvantage of the NMES is that there is no information on either pension or Social Security recipiency.

The second source of data is the Survey of Income and Program Participation (SIPP). The SIPP is a nationally representative survey of households designed to collect information on the economic and demographic characteristics of individuals and their families. Sample members are interviewed every four months for roughly 2½ years and asked to provide information about their labor market activity, income, and participation in welfare and transfer programs over the previous four months. These quarterly interviews are referred to as "waves." A certain set of "core" questions are asked at every interview including whether or not an individual received health insurance through a current or former employer. In addition, supplemental questions which change from wave to wave are asked in what are

[13] This assumes that all individuals who had the option of receiving retiree health insurance did so.

called "topical modules". I use information on retirement from two different topical modules.

Wave 3 of the 1984 SIPP Panel included a topical module on Education and Work History (EWH). As part of this topical module, individuals who did not work during the four-month reference period were asked in what year they last worked, the industry of their former employer and their occupation while working, and the reason (including retirement) for leaving their former job. Additional information on retirement is found in the topical module on Characteristics of Job from Which Retired (CJR) which was incorporated into the 1984, 1985 and 1986 Panels of the SIPP.[14] In this topical module, questions concerning retirement were asked only of those receiving retirement income other than Social Security. Much more information on the former job is available, however. In addition to the year last worked, industry, and occupation, this topical module includes questions on firm size, usual hours worked, earnings before retirement, whether pension benefits were calculated based on contributions or years of service and pay, and whether an individual is now covered by a health plan provided by a former employer.

Because the sample of individuals covered by these two topical modules differs, I treat them separately. I therefore have two SIPP datasets: the SIPP-EWH dataset based on the 1984 Education and Work History topical module, and the SIPP-CJR dataset based on the 1984, 1985 and 1986 Characteristics of Job From Which Retired topical module.[15] The SIPP-CJR sample is larger because it pools individuals from three different panels. It is

[14] Specifically, 1984 Wave 4, 1985 Wave 7, and 1986 Wave 4.

[15] Note that those individuals in the 1984 Panel who were not working in wave 3 and who received some pension income in wave 4 will be in both the SIPP-EWH and the SIPP-CJR samples.

confined, however, to those who have received pension income. The SIPP-EWH sample, in contrast, includes everyone who did not work in the previous four months and is therefore more representative. For both samples, I know from the core questionnaire whether or not an individual is covered by health insurance through a former employer and the amount of pension and Social Security income an individual is receiving. Although the two SIPP samples do have some information on pensions, the data do not contain the type of detail on pension incentives I would ideally like to have. In particular, I do not know the annual pension accrual rate, i.e., the ages at which the pension provides strong incentives to retire.

As in any study of the retirement decision, the first question to be addressed is how to define retirement. In the previous literature, which has mostly looked at retirement in a dynamic framework, all of the following have been used: 1) a "permanent" departure from the labor force; 2) a substantial reduction in the usual number of hours worked; 3) self-reported retirement; and 4) receipt of pension or Social Security benefits. The definition chosen is often guided by the information available in the data at hand, and I am similarly constrained. Unfortunately, it is not possible to get a definition of retirement that is completely consistent across all three datasets. This means that the results from the different datasets are not strictly comparable. It does, however, give a way to check the robustness of the results to various definitions of retirement.

In the NMES, I have information on the date of retirement for those who report ever having retired--a self-reported definition of retirement. In addition, I also know whether or not an individual is currently working; if so, I know how many hours, and if not, I know whether the stated reason for not working is retirement. This makes it possible to consider alternative (stricter) definitions of retirement, such as reported having retired *and* not currently working. In the estimation, implementing this particular stricter definition amounts to truncating individuals who are currently working even if they report a retirement date.

The SIPP-CJR sample is confined to those who are receiving pension income, and the primary definition of retirement is the date at which an individual left the job from which he is receiving pension benefits. I can also further restrict the sample to those not currently in the labor force or who work less than a specified number of hours per week. In the SIPP-EWH sample, which is comprised of individuals who did not work at all in the previous quarter, retirement corresponds to the date last worked. Some of these individuals, however, may be not working for reasons other than retirement, so I also consider an alternative definition of retirement which is not working *and* not in the labor force. This topical module included a question about the reason for leaving the last job, so I can also confine the sample to those individuals who report retirement as the reason for having left. In addition, I can confine the sample to those receiving pension income to make the sample comparable to the SIPP-CJR sample.

In this paper, the key variable of interest in explaining the retirement decision is whether or not an individual has retiree health insurance. All three datasets have information about whether or not an individual is covered by any private health insurance, whether this policy is held in the individual's own name, and whether or not it comes from a current or former employer. If an individual is not working, it is therefore likely that this coverage is from a retiree health insurance policy.[16] The NMES and SIPP-CJR datasets also include a question specifically about retiree health insurance following the other questions related to

[16] For some retirees, this coverage could be COBRA coverage (or continuation coverage under a state law in effect before COBRA). Individuals cease to be eligible for COBRA, however, once they are entitled to Medicare. So for older retirees, this definition should be fairly accurate.

retirement. As you might expect, there are some discrepancies in the answers to these two different sets of questions. This is shown in the tables below.

NMES:

		Health insurance from current/former employer	
		Yes	No
Retiree health insurance	Yes	37.7%	7.4%
	No	3.9%	51.0%

SIPP-CJR:

		Health insurance from current/former employer	
		Yes	No
Retiree health insurance	Yes	56.8%	10.2%
	No	6.2%	26.8%

In both samples, the vast majority of individuals (85-90%) give consistent responses to both questions. I do, however, use both definitions in the empirical results, although the results are generally not sensitive to the definition of retiree health insurance.

The other explanatory variables used in the regressions include race, education, industry and occupation (although industry and occupation are not available for the 1984 panel in the SIPP-CJR sample). I also try to make use of some of the pension information in the SIPP-CJR dataset. All regressions include a full set of age dummies to account for any cohort effects that might be present. (Without these age dummies, the coefficient on retiree health insurance could just be picking up a spurious correlation between a secular trend toward earlier retirement and the increased availability of retiree health insurance over the same time period). The sample for all three datasets is restricted to men aged 55-84.

IV. Empirical Results

A. 1987 National Medical Expenditure Survey

The first set of empirical results using data from the NMES are presented beginning with Table 8. The basic specification, a truncated regression of the age at retirement (see equation (3)), is constant across all three datasets and includes a constant term, whether or not the individual has retiree health insurance, education, race, and a set a cohort dummies. This specification is presented for the sake of comparison; results including other regressors are also presented, although the other explanatory variables of interest are not necessarily the same across the three datasets.

Table 8 compares the results obtained using OLS and those from the truncated regression model. The first column of the top panel, which includes the full sample, shows that in the OLS specification, retiree health insurance reduces the age of retirement by a little over a year. This effect is statistically significant with a t-statistic of 6.7. Education has a positive and significant effect on the age at retirement, with an additional year of school increasing the retirement age by about a month, while being black has a positive but insignificant effect on the age at retirement.

Explicitly accounting for the truncation in the data, as in column 2, increases the magnitude of all the coefficients. This is consistent with our expectation that truncation leads to a downward bias in the coefficients. The impact of retiree health insurance on the age at retirement is 20% greater than under OLS, reducing the age at retirement by 1.4 years. As might be expected, a specification test rejects OLS in favor of the truncated regression model.[17]

To further illustrate the effects of truncation, the second and third panels of Table 8 restrict the sample to older ages: those age 65 and older and those age 70 and older. The problem with truncation is less severe at older ages, and as expected, the OLS and truncated regression coefficients are less disparate in the 65+ sample than for the full sample. When the sample is restricted to those age 70 and older, the two sets of coefficients are very similar, although the likelihood ratio test still rejects the OLS model in favor of the truncated regression model. The effect of retiree health insurance increases slightly when the sample is restricted to these older ages, reducing the age at retirement by 1.5 years.

Using the full sample, Table 9 compares the estimated effects from a truncated regression of the two different definitions of retiree health insurance across three different definitions of retirement. The results are not substantially different from those presented in Table 8. The effect is slightly larger using the first definition of retiree health insurance rather than the second, and slightly larger with a stricter definition of retirement.

One concern with interpreting the coefficient on retiree health insurance in these specifications as the effect of health insurance on the age at retirement is that there are many things omitted from the model that are possibly correlated with retiree health insurance. Chief among them, of course, are pensions. As Table 4 shows, the industries which tend

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[17] The test of whether the truncation in the data creates a substantial bias in the OLS coefficient estimates is a test of the significance of the inverse Mill's ratio ($-\phi(\alpha)/\Phi(\alpha)$) in the OLS regression

$$y_i = \beta' \mathrm{x}_i + \gamma \cdot RHI_i + \delta \cdot \frac{\phi(\alpha_i)}{\Phi(\alpha_i)} + \varepsilon_i$$

where $\alpha_i = (Age_i - \beta' \mathrm{x}_i - \gamma \cdot RHI_i)/\sigma$. In this regression, the t-statistic on the coefficient δ equals 3.57, which suggests that the truncation does indeed matter.

to offer retiree health insurance also tend to offer pensions. However, as noted previously, none of the datasets with good health insurance information contains the detailed data on pension incentives one would like to include in a retirement model. The NMES, with absolutely no information on pensions, is the worst offender.

Table 10 attempts to partially address this concern by including a set of industry and occupation dummies. Not surprisingly, the coefficient on retiree health insurance falls when industry and occupation controls are present. The effect, however, is still large and statistically significant, reducing the age at retirement by 1.2 years. Once again, the results do not differ substantially using different definitions of retirement or of retiree health insurance.

An alternative approach to examine the effect of health insurance on retirement would be to estimate the effect of retiree health insurance on the likelihood of "early" retirement. In the retirement literature, "early" typically refers to retirement before age 65, the Social Security "normal" retirement age. Age 65 is also a natural breakpoint to consider when looking at the effect of health insurance on retirement because this is the age at which individuals become eligible for Medicare.

The results from estimating a probit equation for whether or not an individual retires before age 65 are shown in Table 11. Because truncation is still an issue, the sample is restricted to individuals age 70 and over. The sign on the retiree health insurance coefficient is positive because health insurance now serves to *increase* the likelihood of early retirement (whereas in the previous tables it decreased the age at retirement). In all cases the coefficient is significant and implies an increase in the retirement probability of about 15%.[18]

B. Survey of Income and Program Participation--EWH Sample

Table 12 presents results analogous to those in Table 8 using the SIPP-EWH sample. Once again, the effects of truncation are readily apparent. For the full sample, the coefficient on retiree health insurance is -.39 under OLS and -.65 in the truncated regression model. In both cases, these effects are significant, although they are substantially lower than those obtained from the NMES. The coefficients become larger, however, as the sample is restricted to older ages, and the coefficient in the truncated regression model for those age 70 and over is close to that obtained in the NMES (-1.24 versus -1.49). This suggests that in this sample, the truncated regression model may not be completely capturing the effects of the truncation.

This may be partially due to the fact that the baseline definition of retirement (did not work in the previous quarter) is probably less accurate in the SIPP-EWH sample. As the first column in Table 13 shows, the coefficient on retiree health insurance gets much larger (-.90 versus -.65) using the strict definition of retirement for this sample: not working and claiming retirement as the reason for having left the last job. The signs and magnitudes on the other coefficients in the regressions generally accord with those found using the NMES.

The second column on Table 13 restricts the sample to those who are receiving pension income. This is done both for comparability with the SIPP-CJR sample, which only

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[18] The marginal probability is calculated as the average across all individuals in the sample of the difference between the predicted probabilities of early retirement with and without retiree health insurance.

includes those with pension income, and as an attempt to control for the effect of pensions. For the first two definitions of retirement, this restriction on the sample does not substantively change the coefficients. For the third definition of retirement, however, the coefficient falls by 60%. Although these results are not presented, it should be noted, however, that when the sample is restricted to those age 70 and over, this fall in the coefficient is much smaller (-1.24 to -.84). These results suggest that the coefficient on retiree health insurance is at least partially picking up the effect of having a pension. Among those with pensions, however, retiree health insurance nevertheless decreases the age at retirement by between 4 and 10 months.

Table 14 presents results from a probit equation for early retirement in the SIPP-EWH sample. The coefficients on retiree health insurance are once again positive and significant and imply an increase in the marginal probability of retirement of approximately 7.5%. In this case, the effect does not appear to differ across the different definitions of retirement or on the basis of receiving pension income.

C. Survey of Income and Program Participation--CJR Sample

Table 15 compares the OLS and truncated regression results using the SIPP-CJR sample. Once again, the truncated regression coefficients on retiree health insurance are larger than the OLS coefficients and increase slightly as the sample is restricted to older ages. Overall, they imply a reduction in the age of retirement of 5-6 months, a result smaller than the that from the NMES but consistent with that from the SIPP-EWH sample. As Table 16 shows, the results are not sensitive to the definition of retirement chosen although the second definition of retiree health insurance gives a slightly larger coefficient.

Industry and occupation controls are added in Table 17. The results here are somewhat puzzling. The industry and occupation codes in the CJR topical module are not available in the 1984 panel of the SIPP, so the sample sizes in this table are substantially smaller than in Table 16. The results in column 1, which includes the 1985 and 1986 panels, seem to suggest that when industry and occupation controls are included, the effect of retiree health insurance goes away; the coefficients are half those in Table 16 and no longer significant. In column 2, using the second definition of retiree health insurance, the coefficient is once again smaller than in Table 16, although marginally significant, and implies a reduction in the age at retirement of 5.5 months.

The puzzle is revealed in the bottom half of the table which restricts the sample to only the 1986 panel. The coefficients on retiree health insurance are larger and once again significant for both definitions (despite a much smaller sample size). Although the results are not presented, further investigation suggests that the relationship between retirement and retiree health insurance, while strong for the 1984 and 1986 panels, is extremely weak in the 1985 panel. As in the NMES, the inclusion of industry and occupation controls only slightly reduces the effect of retiree health insurance in the 1986 panel. Unfortunately, I have no good reason for why the results in the 1985 panel are contrary to those in the other two years of the SIPP. In all other respects, the data appear to be the same. I merely note that the 1985 SIPP-CJR results seem to be somewhat peculiar.

Results from the probit equation for early retirement are shown in Table 18. The results generally agree with the results from the NMES and the SIPP-EWH samples, although the anomalies with the 1985 panel are still present. The coefficients imply that retiree health insurance increases the marginal probability of retirement by about 6%.

Although the result that retiree health insurance has a negative and significant impact on age at retirement (and probability of early retirement) even among those receiving pensions gives some evidence that the effect of retiree health insurance is not simply a pension effect, this evidence is by no means conclusive. It is possible that within the set of firms that provide pensions, those who provide retiree health insurance have pension incentives that induce early retirement as well. Without information on the specific details of these pension plans, I cannot directly test for this in my data. I can, however, examine whether the pensions received by individuals with retiree health insurance are different, and if so, test to see whether these differences affect the impact of retiree health insurance on the age at retirement.

Table 19 presents some summary statistics on the pension benefits and Social Security payments received by individuals in the SIPP-CJR sample (all of whom have pensions). The first row shows that average Social Security payments received by individuals with retiree health insurance are virtually identical to those received by individuals without retiree health insurance. While Social Security is not a perfect indicator of pre-retirement income (and is somewhat endogenous to the decision of when to retire), these numbers suggest that the wage component of compensation is similar for jobs with and without retiree health insurance. The non-wage component of compensation, however, may be quite different. The second row of Table 19 shows that the average pension received by individuals with retiree health insurance is \$250-\$350 higher than that received by individuals without retiree health insurance. Moreover, individuals with retiree health insurance are more likely to have received a cost-of-living-adjustment to their pension (this will partially account for the difference in pension amounts in row 2). These last two findings suggest that firms that offer retiree health insurance provide more generous pension benefits as well.

One possible way to skirt the problem of whether or not firms that provide retiree health insurance also provide incentives for early retirement is to look at individuals who participated in defined contribution plans. In the SIPP-CJR data, I know whether or not your pension was based on "years of service and pay" (defined benefit) or on the "amount contributed to [the] plan" (defined contribution). Defined contribution plans do not typically include the incentives for retirement at particular ages characteristic of many defined benefit plans (Samwick 1993).

Table 20 compares the effect of retiree health insurance on age at retirement for those in defined benefit and defined contribution plans. While the coefficient on retiree health insurance is negative and significant for those in defined benefit pension plans, it is actually positive and significant for those in defined contribution plans (despite a 90% reduction in sample size). This result is somewhat troubling. It is difficult to believe that retiree health insurance actually *increases* the age at retirement by almost 1½ years. More likely, the positive sign of this coefficient is picking up the fact that jobs which provide retiree health insurance are "better" jobs and individuals are therefore less likely to leave them. To the extent that this bias is present in the previously presented empirical work, it makes the negative coefficient on retiree health insurance in all of the other specifications all the more striking.

Taken as a whole, the results from these three different datasets suggest that individuals in firms that provide retiree health insurance retire, on average, 6-12 months earlier than individuals who work in firms that do not provide retiree health insurance. This result is robust to various definitions of both retirement and retiree health insurance, to the inclusion of industry and occupation controls, and to whether or not individuals receive any income. It is less clear, however, whether this estimated effect is the effect of actually having retiree health insurance or whether it is picking up a correlation between the provision of retiree health insurance and overall job quality, the generosity of the pension benefits, and the incentives for early retirement that are a part of pension plans in these firms. Clearly, more research into the relationship between retiree health insurance and pension benefits is warranted before a firm conclusion about the effect of health insurance on retirement can be reached.

V. Implications and Policy Relevance

The previous cautions aside, can we draw any implications from these results about the potential labor market effects of various health care reform proposals? The most natural reform to consider would be a move to universal health care. Table 21 illustrates the consequences of such a proposal based on the assumption that the effect of universal health care is to reduce the retirement age of those currently without retiree health insurance by one year.[19] The effect on the labor force participation rate of men is shown in column 3, and the absolute decline in employment in column 4. The effect is largest, both in percentage and absolute terms, for those aged 60-64. Universal coverage would reduce the labor force

[19] The effect on the labor force participation rate in Table 23 is computed as follows. I calculated the actual distribution of retirement ages for individuals over age 70 in the NMES for those with and without retiree health insurance and the age-specific labor force participation rates that these distributions implied. I then shifted the retirement age distribution forward by one year for those without retiree health insurance and recalculated the labor force participation rate. The decline in the labor force participation rate was then recalibrated to be consistent with the official labor force participation rate published in the June 1987 Employment and Earnings (which is reported in column 3 of Table 23).

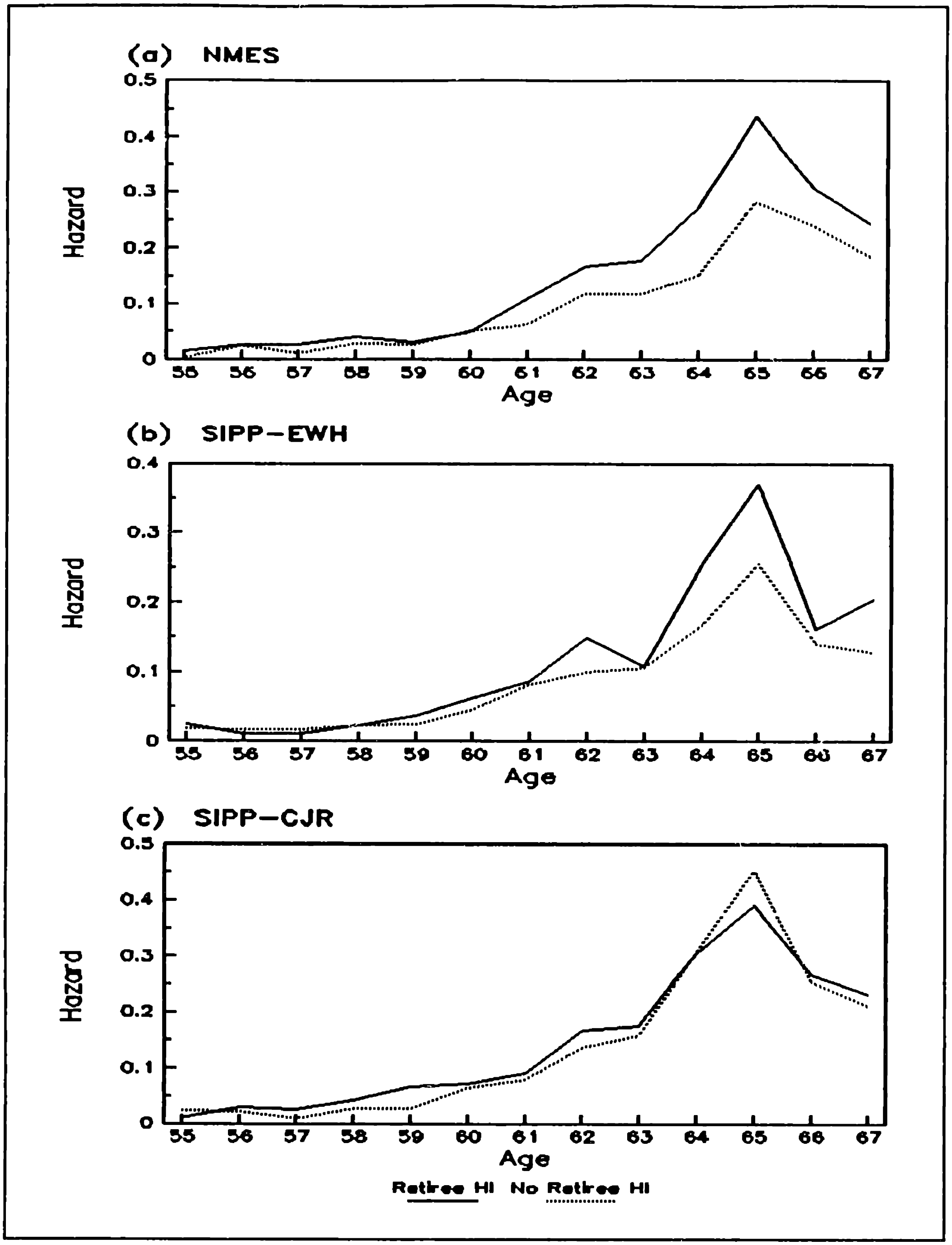
participation rate of this group by 4.27 percentage points, a 7.7% reduction from the baseline rate of 55.6%. The overall reduction in employment for the 50-69 year old male population would be 450,000. These effects are quite large, although a more conservative estimate of the effect of retiree health insurance on retirement (for example, 6 months rather than a year) would naturally imply a smaller reduction in employment.

Another natural question to ask is whether health insurance can explain the "excess" peak in the retirement hazard at age 65 that exists even after financial incentives for retirement are appropriately accounted for (Lumsdaine, Stock and Wise 1992). To directly address this question, one really needs an estimate of how retiree health insurance affects the retirement hazard at each age. Although the data used in this paper do not lend themselves to estimation of such a hazard, Figure 2 plots the empirical hazard based on the actual retirement ages for each of the three datasets. Although these figures do not remove the effects of any of the other factors that influence retirement, I think they are nonetheless illustrative of the differences in the hazard rate for those with and without retiree health insurance.[20]

Note that the overall shapes of the hazards are consistent with those in the previous literature and exhibit the characteristic spikes at ages 62 and 65 associated with Social Security eligibility. The shape is not markedly different for those with and without retiree

[20] In particular, the removal of cohort effects could alter the hazards to the extent that older cohorts are both less likely to have retiree health insurance and less likely to have retired early for other reasons. The data, however, are too sparse to actually estimate a probability of retirement at each age which adequately controls for these and other effects.

FIGURE 2: Post-Retirement Health Insurance and the Retirement Hazard



health insurance, and, in particular, the spike at age 65 is not diminished for those with retiree health insurance. In two of the three datasets, the spike at age 65 is actually larger for those with retiree health insurance. If entitlement to Medicare were a substantial explanation of the spike in the retirement hazard at age 65, we would expect that individuals with retiree health insurance would not exhibit such a spike or, to the extent that the spike exists for other reasons (such as Social Security and pension incentives), it should be smaller for those with retiree health insurance. To the extent that Medicare is inferior to employer-provided health insurance, this finding may not be quite so surprising. Employer-provided health plans are typically much more generous than Medicare, and individuals may simply place a sufficiently low value on Medicare that it does not impact their retirement decision. The relative valuation of Medicare and employer-provided health insurance is clearly another area which warrants further research.

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TABLE 1

Post-Retirement Health Insurance Coverage by Age at Retirement

	NMES	SIPP
50-54	57.1%	32.7%
54-59	63.9	55.4
60-61	47.7	51.4
62-63	45.1	48.8
64-65	40.0	38.9
66-69	32.1	32.7
70-74	18.0	28.1
75+	11.2	21.7

Sources: 1987 National Medical Expenditure Survey, men age 55+ who report having ever retired (weighted); 1984 Survey of Income and Program Participation, men age 55+ not working and who report retirement as the reason for leaving their last job (weighted).

TABLE 2

Post-Retirement Health Insurance Coverage by Age

	NMES	SIPP	CPS
Age 65-69	45.1%	44.6%	46.1%
Age 70-74	42.7	38.9	39.0
Age 75-79	34.7	35.6	30.4
Age 80-84	27.6	28.1	26.2
Age 85+	25.0	16.0	20.4

Sources: 1987 National Medical Expenditure Survey, men age 55+ who report having ever retired (weighted); 1984 Survey of Income and Program Participation, men age 55+ not working and who report retirement as the reason for leaving their last job (weighted); August 1988 Current Population Survey, men age 55+ not in the labor force (weighted).

TABLE 3

Post-Retirement Health Insurance Coverage by Year of Retirement

	NMES	SIPP
1960-1964	32.9%	24.4%
1965-1969	39.7	29.2
1970-1974	47.4	37.6
1975-1979	47.7	43.7
1980-1987	48.7	50.4

Source: 1987 National Medical Expenditure Survey, men age 55+ who report having ever retired (weighted); 1984 Survey of Income and Program Participation, men age 55+ not working and who report retirement as the reason for leaving their last job (weighted).

TABLE 4

Fraction of Individuals with Post-Retirement Health Insurance, Health Insurance, and Pension Benefits by Industry and Occupation

	Retiree Health Ins. (NMES)	Health Insurance (CPS)	Pension (CPS)
A. Industry			
Agriculture, Forestry and Fishing	5.7%	27.4%	10.14%
Construction	31.0	49.9	28.6
Manufacturing	46.8	80.8	59.6
Transportation, Utilities and Communication	56.5	78.7	64.3
Sales	13.8	55.1	31.0
Finance, Real Estate, and Insurance	32.7	69.3	50.4
Personal Services	7.4	51.4	26.3
Professional Services	35.2	64.5	54.3
Public Administration	66.7	81.8	84.0
B. Occupation			
Professional and Technical	50.1%	73.6%	61.1%
Clerical	42.6	66.1	52.2
Sales	16.7	58	33.6
Managerial and Administrative	38.9	74.2	53.9
Craftsmen and Foremen	45.9	67.5	46.9
Operatives	35.3	72.1	49.5
Transportation operatives	41.7	66.7	46.8
Service workers	21.5	37.8	23.8
Nonfarm laborers	37.4	59.9	54.2

Sources: 1987 National Medical Expenditure Survey, individuals age 55+ who report having ever retired (weighted); March 1988 Current Population Survey, working individuals aged 25-50.

TABLE 5

Eligibility Requirements and Benefits for
Post-Retirement Health Insurance

	Medium and Large Firms, 1989	State and Local Governments, 1990
Eligibility Requirements		
All retirees eligible	39%	32%
Subject to service requirement	29	39
Must qualify for pension	26	20
Benefit Level		
No change in coverage	84%	87%
Reduced coverage	16	13

Source: U.S. Department of Labor (1989), *Employee Benefits in Medium and Large Firms*; U.S. Department of Labor (1990), *Employee Benefits in State and Local Governments*.

TABLE 6

Sources of Health Insurance Coverage

Fraction of Individuals with Coverage from:	All Individuals				Full-Time Workers			
	Age 25-54		Age 55-64		Age 25-54		Age 55-64	
	Men	Women	Men	Women	Men	Women	Men	Women
Employer-Provided								
Any	72.5	70.8	67.5	61.9	77.7	79.4	78.4	73.7
Own Name	65.0	37.8	60.8	31.0	70.4	53.1	71.7	52.6
Other Group	1.1	1.2	4.1	4.1	1.0	1.2	4.4	3.5
Nongroup	6.3	5.6	12.0	16.7	6.1	5.6	11.1	14.6
CHAMPUS/VA	2.6	8.6	5.5	9.6	2.3	8.2	5.0	8.1
Medicare/Medicaid	3.7	7.4	12.1	8.8	0.7	1.8	0.6	1.0
Uninsured	17.1	13.7	11.4	12.6	15.2	11.4	9.1	11.3

Source: 1987 National Medical Expenditure Survey, Round 1 (weighted). Full-time is defined as working 35+ hours per week. Totals may exceed 100% because some individuals have more than one source of coverage.

TABLE 7

Retirement Rates by Age

Age	1987 NMES	1987 Current Population Survey	
	Ever Retired	Currently Retired	Not in the Labor Force
55	20.2%	10.5	20.2
60	43.7	33.0	44.2
65	76.8	62.6	73.6
70	86.9	73.6	84.9
75	84.1	76.7	90.2
80	83.2	78.1	94.6
85	91.2	76.7	97.3

Sources: Author's calculations from the 1987 National Medical Expenditure Survey and the 1987 Merged Outgoing Rotation Group of the Current Population Survey.

TABLE 8

Post-Retirement Health Insurance and the Age at Retirement (NMES)
OLS vs. Truncated Regression Model

Sample (N=Sample Size)	OLS		Truncated Regression	
A. Full Sample (N=1816)				
Constant	56.184	(.6754)	62.274	(1.541)
Retiree HI	-1.134	(.1701)	-1.388	(.2078)
Education	.0954	(.0253)	.1157	(.0309)
Black	.2653	(.2823)	.3315	(.3522)
σ	--	--	3.824	(.0789)
R^2	.277		--	
ln L	-4822.3		-4686.3	
B. Age >= 65 (N=1551)				
Constant	61.284	(.8363)	63.110	(1.219)
Retiree HI	-1.271	(.1907)	-1.456	(.2171)
Education	.1045	(.0286)	.1186	(.0325)
Black	.3192	(.3273)	.3588	(.3751)
σ	--	--	3.868	(.0817)
R^2	.141		--	
ln L	-4193.8		-4138.0	
C. Age >= 70 (N=1006)				
Constant	61.830	(.8470)	62.085	(.9107)
Retiree HI	-1.422	(.2604)	-1.487	(.2700)
Education	.1629	(.0386)	.1699	(.0400)
Black	.3019	(.4525)	.3204	(.4691)
σ	--	--	4.039	(.0970)
R^2	.104		--	
ln L	-2809.4		-2801.9	

The dependent variable is age of retirement. The data come from the 1987 National Medical Expenditure Survey, and the sample consists of men aged 55-84 who reported ever having retired. All specifications include a complete set of cohort dummies.

TABLE 9

Post-Retirement Health Insurance and the Age at Retirement (NMES)

Definition of Retired	Definition of Retiree Health Insurance: Health Insurance from Retirement Job		Hold Employer-Provided Health Insurance	
A. Ever Retired				
Constant	62.274	(1.541)	62.248	(1.544)
Retiree HI	-1.388	(.2078)	-1.251	(.2086)
Education	.1157	(.0309)	.1211	(.0310)
Black	.3315	(.3522)	.3120	(.3523)
σ	3.824	(.0789)	3.832	(.079)
ln L	-4686.3		-4710.5	
Sample Size	N = 1816		N = 1824	
B. Ever Retired and Not Working				
Constant	61.456	(2.032)	61.406	(2.032)
Retiree HI	-1.400	(.2167)	-1.378	(.2175)
Education	.1132	(.0330)	.1185	(.0331)
Black	.2366	(.3679)	.2203	(.3671)
σ	3.756	(.0812)	3.756	(.0810)
ln L	-4007.4		-4028.9	
Sample Size	N = 1539		N = 1547	
C. Ever Retired, Not Working b/c Retired				
Constant	62.534	(2.312)	62.627	(2.305)
Retiree HI	-1.462	(.2265)	-1.366	(.2272)
Education	.1123	(.0347)	.1159	(.0348)
Black	.4563	(.3951)	.4330	(.3945)
σ	3.753	(.0848)	3.759	(.0847)
ln L	-3689.2		-3717.1	
Sample Size	N = 1424		N = 1434	

The table presents results from estimating a truncated regression model in which the dependent variable is age of retirement. The data come from the 1987 National Medical Expenditure Survey, and the sample consists of men aged 55-84 who reported ever having retired. All specifications include a complete set of cohort dummies.

TABLE 10

Post-Retirement Health Insurance and the Age at Retirement (NMES)
Controlling for Industry and Occupation Effects

	Definition of Retiree Health Insurance			
Definition of Retired	Health Insurance from Retirement Job		Hold Employer-Provided Health Insurance	
A. Ever Retired				
Constant	61.731	(1.572)	61.707	(1.572)
Retiree HI	-1.190	(.2140)	-1.083	(.2150)
Education	.1158	(.0354)	.1184	(.0353)
Black	.2138	(.3525)	.2047	(.3524)
σ	3.791	(.0780)	3.796	(.0779)
ln L	-4673.2		-4696.5	
Sample Size	N = 1816		N = 1824	
B. Ever Retired and Not Working				
Constant	61.040	(2.064)	60.968	(2.062)
Retiree HI	-1.215	(.2239)	-1.220	(.2255)
Education	.1159	(.0376)	.1195	(.0376)
Black	.2083	(.3702)	.1987	(.6391)
σ	3.736	(.0806)	3.735	(.0804)
ln L	-4000.4		-4021.3	
Sample Size	N = 1539		N = 1547	
C. Ever Retired, Not Working b/c Retired				
Constant	62.030	(2.350)	62.068	(2.341)
Retiree HI	-1.312	(.2350)	-1.231	(.2357)
Education	.1088	(.0400)	.1112	(.0399)
Black	.3549	(.3986)	.3374	(3978)
σ	3.737	(.0843)	3.742	(.0842)
ln L	-3684.1		-3711.6	
Sample Size	N = 1424		N = 1434	

The table presents results from estimating a truncated regression model in which the dependent variable is age of retirement. The data come from the 1987 National Medical Expenditure Survey, and the sample consists of men aged 55-84 who reported ever having retired. All specifications include a complete set of cohort dummies as well as industry and occupation dummies.

TABLE 11

Post-Retirement Health Insurance and the Probability of Early Retirement (NMES)

Definition of Retired	Definition of Retiree Health Insurance: Health Insurance from Retirement Job			
A. Ever Retired				
Constant	1.056	(.2907)	.9069	(.3425)
Retiree HI	.4138	(.0846)	.3891	(.0890)
Education	-.0531	(.0126)	-0516	(.0145)
Black	-.1979	(.1466)	-.1762	(.1481)
Ind/Occ dummies	No		Yes	
ln L	-648.2		-641.2	
Sample Size	N = 1006		N = 1006	
B. Ever Retired and Not Working				
Constant	1.220	(.3292)	1.071	(.3864)
Retiree HI	.4275	(.0894)	.4209	(.0940)
Education	-.0506	(.0136)	-.0498	(.0157)
Black	-.2395	(.1576)	-.2425	(.1593)
Ind/Occ dummies	No		Yes	
ln L	-581.8		-577.3	
Sample Size	N = 914		N = 914	
C. Ever Retired, Not Working b/c Retired				
Constant	1.249	(.3405)	1.084	(.3946)
Retiree HI	.4247	(.0926)	.4266	(.0977)
Education	-.0573	(.0142)	-.0548	(.0165)
Black	-.3472	(.1697)	-.3326	(.1721)
Ind/Occ dummies	No		Yes	
ln L	-542.9		-538.7	
Sample Size	N = 850		N = 850	

The table presents results from estimating a probit for whether or not an individual retired before age 65. The data come from the 1987 National Medical Expenditure Survey, and the sample consists of men aged 70-84 who reported ever having retired. All specifications include a complete set of cohort dummies.

TABLE 12

Post-Retirement Health Insurance and the Age at Retirement (SIPP-EWH)
OLS vs. Truncated Regression Model

Sample (N=Sample Size)	OLS		Truncated Regression	
A. Full Sample (N=2060)				
Constant	56.060	(.5089)	67.501	(1.744)
Retiree HI	-.3904	(.1788)	-.6504	(.2754)
Education	.0819	(.0237)	.1190	(.0354)
Black	.1650	(.3203)	.3325	(.4898)
σ	--	--	4.649	(.1079)
R^2	.361		--	
ln L	-5654.1		-5301.9	
B. Age >= 65 (N=1645)				
Constant	60.835	(.5415)	64.157	(.9415)
Retiree HI	-.5821	(.2200)	-.7932	(.2920)
Education	.0978	(.0284)	.1284	(.0372)
Black	.2780	(.3911)	.4071	(.5198)
σ	--	--	4.722	(.1121)
R^2	.197		--	
ln L	-4656.3		-4522.7	
C. Age >= 70 (N=1082)				
Constant	62.686	(.6115)	63.742	(.7575)
Retiree HI	-1.072	(.3127)	-1.239	(.3577)
Education	.1180	(.0387)	.1348	(.4441)
Black	.4064	(.5468)	.4688	(.6316)
σ	--	--	4.950	(.1302)
R^2	.136		--	
ln L	-3187.7		-3156.7	

The dependent variable is age of retirement. The data come from the 1984 Survey of Income and Program Participation (Wave 3), and the sample consists of men aged 55-84 who did not work during the quarter before the survey. All specifications include a complete set of cohort dummies.

TABLE 13

Post-Retirement Health Insurance and the Age at Retirement (SIPP-EWH)

Definition of Retired	Full Sample		Those with Pension Income	
A. Not Working				
Constant	67.507	(1.744)	63.355	(1.616)
Retiree HI	-.6504	(.2754)	-.5913	(.2943)
Education	.1190	(.0354)	.1026	(.0411)
Black	.3325	(.4898)	1.137	(.5542)
σ	4.649	(.1079)	3.973	(.1162)
ln L	5301.9		2874.0	
Sample Size	N = 2060		N = 1166	
B. Not Working and Not in Labor Force				
Constant	66.627	(1.798)	63.307	(1.668)
Retiree HI	-.6272	(.2749)	-.6341	(.2937)
Education	.1146	(.0353)	.1087	(.0410)
Black	.3143	(.4899)	1.145	(.5560)
σ	4.625	(.1071)	3.954	.(1160)
ln L	5209.2		2833.0	
Sample Size	N = 2009		N = 1146	
C. Not Working b/c Retired				
Constant	66.840	(2.164)	63.886	(1.812)
Retiree HI	-.8987	(.2828)	-.3597	(.3022)
Education	.0846	(.0368)	.0414	(.0415)
Black	.5898	(.5410)	.6021	(.5816)
σ	4.243	(.1084)	3.723	(.1160)
ln L	3733.6		2268.4	
Sample Size	N = 1467		N = 931	

The table presents results from estimating a truncated regression model in which the dependent variable is age of retirement. The data come from the 1984 Survey of Income and Program Participation (Wave 3), and the sample consists of men aged 55-84 who did not work in the quarter before the survey. All specifications include a complete set of cohort dummies.

TABLE 14

Post-Retirement Health Insurance and the Probability of Early Retirement (SIPP-EWH)

Definition of Retired	Full Sample		Those with Pension Income	
A. Not Working				
Constant	.4119	(.1686)	.3142	(.2307)
Retiree HI	.2025	(.0862)	.1960	(.1106)
Education	-.0156	(.0107)	-.0221	(.0151)
Black	-.1240	(.1497)	-.1955	(.1932)
ln L	711.6		-370.7	
Sample Size	N = 1082		N = 564	
B. Not Working and Not in Labor Force				
Constant	.3890	(.1690)	.2925	(.2313)
Retiree HI	.2022	(.0865)	.2048	(.1108)
Education	-.0136	(.01[illegible]7)	-.0211	(.0151)
Black	-.1002	(.1509)	-.1553	(.1956)
ln L	707.7		-368.6	
Sample Size	N = 1076		N = 561	
C. Not Working b/c Retired				
Constant	.2542	(.1927)	-.0458	(.2506)
Retiree HI	.2224	(.0950)	.2041	(.1208)
Education	-.0076	(.0120)	.0024	(.0162)
Black	-.0863	(.1751)	.0407	(.2177)
ln L	557.2		-318.2	
Sample Size	N = 844		N = 483	

The table presents results from estimating a probit for whether or not an individual retired before age 65. The data come from the 1984 Survey of Income and Program Participation (Wave 3), and the sample consists of men aged 70-84 who did not work in the quarter before the survey. All specifications include a complete set of cohort dummies.

TABLE 15

Post-Retirement Health Insurance and the Age at Retirement (SIPP-CJR)
OLS vs. Truncated Regression Model

Sample (N=Sample Size)	OLS		Truncated Regression	
A. Full Sample (N=2657)				
Constant	56.719	(.3785)	62.744	(.8677)
Retiree HI	-.3458	(.1360)	-.4416	(.1785)
Education	.0044	(.0182)	.0065	(.0214)
Black	.4180	(.2614)	.5604	(.3586)
σ	--	--	3.686	(.0673)
R^2	.324		--	
ln L	-6832.6		-6513.0	
B. Age >= 65 (N=2011)				
Constant	61.571	(.4087)	63.406	(.5672)
Retiree HI	-.4327	(.1621)	-.5014	(.1888)
Education	.0216	(.0224)	.0250	(.0259)
Black	.6675	(.3317)	.7885	(.3909)
σ	--	--	3.748	(.0708)
R^2	.108		--	
ln L	-5352.0		-5264.6	
C. Age >= 70 (N=1270)				
Constant	62.954	(.4572)	63.257	(.4874)
Retiree HI	-.5057	(.2219)	-.5274	(.2301)
Education	.0356	(.0301)	.0369	(.0311)
Black	.9379	(.4574)	.9848	(.4780)
σ	--	--	3.870	(.0826)
R^2	.067		--	
ln L	-3493.9		-3485.1	

The dependent variable is age of retirement. The data come from the Survey of Income and Program Participation, and the sample consists of men aged 55-84 who reported receiving pension income. All specifications include a complete set of cohort dummies.

TABLE 16

Post-Retirement Health Insurance and the Age at Retirement (SIPP-CJR)

Definition of Retired	Definition of Retiree Health Insurance: Health Insurance from Retirement Job		Hold Employer-Provided Health Insurance	
A. Received Pension Income				
Constant	62.744	(.8677)	62.858	(.8644)
Retiree HI	-.4416	(.1785)	-.6688	(.1771)
Education	.0065	(.0241)	.0105	(.0241)
Black	.5604	(.3586)	.5672	(.3581)
σ	3.686	(.0673)	3.678	(.0671)
ln L	-6513.0		-6509.0	
Sample Size	N = 2657		N = 2657	
B. Received Pension Income and Not Working				
Constant	63.072	(1.158)	63.177	(1.153)
Retiree HI	-.5016	(.1958)	-.7230	(.1927)
Education	.0184	(.0269)	.0215	(.0268)
Black	.7298	(.3953)	.7335	(.3947)
σ	3.759	(.0731)	3.751	(.0729)
ln L	-5586.1		-5582.4	
Sample Size	N = 2243		N = 2243	
C. Received Pension Income and Work <20 HPW				
Constant	62.911	(1.075)	63.011	(1.071)
Retiree HI	-.4458	(.1893)	-.6515	(.1868)
Education	.0095	(.0258)	.0127	(.0257)
Black	.6597	(.3782)	.6589	(.3778)
σ	3.736	(.0708)	3.729	(.0707)
ln L	-5907.6		-5904.3	
Sample Size	N = 2379		N = 2379	

The table presents results from estimating a truncated regression model in which the dependent variable is age of retirement. The data come from the Survey of Income and Program Participation, and the sample consists of men aged 55-84 who reported receiving pension income. All specifications include a complete set of cohort dummies.

TABLE 17

Post-Retirement Health Insurance and the Age at Retirement (SIPP-CJR)
Controlling for Industry and Occupation Effects

Sample	Definition of Retirement: Received Pension Income			
	Health Insurance from Retirement Job		Hold Employer-Provided Health Insurance	
A. 1985 and 1986 Panels				
Constant	62.990	(1.333)	63.195	(1.334)
Retiree HI	-.2019	(.2420)	-.4627	(.2368)
Education	.0007	(.0391)	.0038	(.0389)
Black	.1999	(.5129)	.1935	(.5123)
σ	3.619	(.0887)	3.612	(.0885)
ln L	-3495.0		-3493.4	
Sample Size	N = 1429		1429	
B. 1986 Panel Only				
Constant	62.628	(1.658)	62.682	(1.670)
Retiree HI	-.7341	(.3371)	-.6071	(.3236)
Education	-.0426	(.0536)	-.0448	(.0537)
Black	-.0360	(.6524)	.1608	(.6540)
σ	3.360	(.1150)	3.364	(.1152)
ln L	-1635.6		-1636.3	
Sample Size	N = 678		678	

The table presents results from estimating a truncated regression model in which the dependent variable is age of retirement. The data come from the Survey of Income and Program Participation, and the sample consists of men aged 55-84 who reported receiving pension income. All specifications include a complete set of cohort dummies as well as industry and occupation dummies.

TABLE 18

Post-Retirement Health Insurance and the Probability of Early Retirement (SIPP-CJR)

	Definition of Retiree Health Insurance: Health Insurance from Retirement Job					
Definition of Retired	All Panels		1985 and 1986 Panels		1986 Panel	
A. Received Pension Income						
Constant	.6700	(.1570)	.8786	(.3354)	.1363	(.5006)
Retiree HI	.1482	(.0755)	.0924	(.1052)	.3595	(.1663)
Education	-.0174	(.0102)	-.0143	(.0175)	.0191	(.0267)
Black	-.4011	(.1524)	-.3095	(.2402)	-.0541	(.3290)
Ind/Occ dummies	No		Yes		Yes	
ln L	-802.2		-433.2		-192.4	
Sample Size	N = 1270		N = 705		N = 339	
B. Received Pension Income and Not Working						
Constant	.6053	(.1660)	.8153	(.3605)	.1928	(.5338)
Retiree HI	.1727	(.0792)	.1338	(.1107)	.4584	(.1739)
Education	-.0173	(.0109)	-.0207	(.0188)	.0020	(.0288)
Black	-.4095	(.1592)	-.4390	(.2487)	-.2019	(.3921)
Ind/Occ dummies	No		Yes		Yes	
ln L	-733.4		-397.7		-180.1	
Sample Size	N = 1163		N = 649		N = 319	
C. Received Pension Income and Work <20 HPW						
Constant	.6518	(.1620)	.8657	(.3455)	.1057	(.5107)
Retiree HI	.1560	(.0775)	.1003	(.1076)	.3946	(.1688)
Education	-.0169	(.0106)	-.0174	(.0180)	.0159	(.0271)
Black	-.3866	(.1557)	-.3700	(.2437)	-.0643	(.3768)
Ind/Occ dummies	No		Yes		Yes	
ln L	-764.3		-417.0		-188.4	
Sample Size	N = 1211		N = 680		N = 332	

The table presents results from estimating a probit for whether or not an individual retired before age 65. The data come from the Survey of Income and Program Participation, and the sample consists of men aged 70-84 who reported receiving pension income. All specifications include a complete set of cohort dummies.

TABLE 19

Differences in Pensions for Those With and Without Retiree Health Insurance

	Full Sample		Age 70+	
	With Retiree HI	Without Retiree HI	With Retiree HI	Without Retiree HI
Average social security amount	$545	$552	$555	$559
Average pension amount	$761	$424	$616	$363
Fraction ever received COLA	51.4%	35.0%	67.1%	41.6%
Fraction with DC pension	8.9%	13.3%	10.2%	13.9%

Source: Author's calculation using the SIPP-CJR data. Sample include men aged 55-84.

TABLE 20

Post-Retirement Health Insurance and the Age at Retirement (SIPP-CJR)
Defined Benefit vs. Defined Contribution Pension Plans

	Defined Benefit Pension		Defined Contribution Pension	
Constant	62.761	(.8805)	59.943	(3.162)
Retiree HI	-.6478	(.1860)	1.433	(.5799)
Education	.0040	(.0251)	.1391	(.0810)
Black	.2986	(.3757)	3.014	(1.096)
σ	3.619	(.0694)	3.830	(.2176)
ln L	-5807.8		-680.8	
Sample Size	N = 2381		N = 276	

The table presents results from estimating a truncated regression model in which the dependent variable is age of retirement. The data come from the Survey of Income and Program Participation, and the sample consists of men aged 55-84 who reported receiving pension income. All specifications include a complete set of cohort dummies.

TABLE 21

The Effect of Universal Coverage on Labor Force Participation

	Male Population (1987)	Labor Force Participation Rate (LFPR)	Decrease in LFPR from Universal Coverage	Decrease in Total Employment
Age 50-54	5.265 million	89.2%	0.35%	18,541
Age 55-59	5.247 million	80.4%	1.26%	65,875
Age 60-64	5.020 million	55.6%	4.27%	214,181
Age 65-69	4.393 million	25.5%	3.49%	153,267

Author's calculation; described in the text.

CHAPTER 3

Health Insurance and Early Retirement: Evidence From the Availability of Continuation Coverage

Joint with Jonathan Gruber, MIT

The vast majority of Americans receive their health insurance coverage through one of two sources. Among the non-elderly, the primary source of coverage is their own or a family member's employment. For those over 65, Medicare covers the majority of medical expenditures. One group of individuals which often finds itself excluded from both of these systems are those who retire before the age of 65. While many employers provide post-retirement health insurance benefits to their employees, the majority of workers do not receive such benefits. These non-working, non-elderly are often forced into the individual market for health insurance, or may go without insurance coverage at all. Given the high costs of medical care for these individuals and the correspondingly high cost of individual health insurance, a potentially important determinant in the retirement decision for older workers is the availability of post-retirement health insurance coverage. If the timing of retirement is very sensitive to insurance coverage, it could have major implications for the costs and consequences of a number of current policy proposals, such as extending the age of Medicare eligibility to 67, or providing government-sponsored health insurance to all citizens.

Despite its likely importance, the role of health insurance coverage has, until recently, been largely ignored in the literature on retirement behavior. In this paper, we attempt to address this gap by examining the effect of state and federal "continuation of coverage" mandates. These mandates grant individuals the right to continue purchasing health insurance through a previous employer for a specified number of months after leaving the firm. Although individuals must pay the full cost of their group insurance, the price to the early retiree is typically well below that of a policy purchased in the individual market. This is both because of the high loading factors on individual insurance (Congressional Research Service 1988), and because younger workers have lower expected medical costs and thus subsidize the cost of insurance for their older coworkers. Continuation of coverage benefits therefore act as a subsidy to the purchase of health insurance, and may therefore increase the likelihood of early retirement for those individuals whose employers do not already provide post-retirement health insurance.

We exploit variation in the timing and generosity of continuation of coverage laws to identify the effect of health insurance on the retirement decision. These laws were passed at different times and in different states from the late 1970s through the mid 1980s before being federally mandated in 1986 under COBRA (the Consolidated Omnibus Budget Reconciliation Act of 1985). In addition, some laws were quite liberal, allowing 15-20 months of coverage, while under others the extent of coverage was minimal. Using data on 55-64 year-old males from the Current Population Survey (CPS) and the Survey of Income and Program Participation (SIPP), we model the retirement decision in both a static and a dynamic framework. Our estimates suggest that one year of continuation coverage raises retirement rates by over 15%. This finding is robust to a number of specification checks and is very similar in both data sets. Furthermore, the effect appears to be uniform at all ages rather than disproportionately stronger near the age of Medicare eligibility.

The organization of the paper is as follows. Section I provides some motivation for why health insurance should matter in the early retirement decision. Section II then outlines the state and federal continuation of coverage laws which we use to identify the effect of health insurance on retirement. This is followed in Section III by a model which formalizes the effect of health insurance on retirement. The data are described in Section IV, and the empirical results are presented in Sections V and VI. Section VII then considers the impact of continuation mandates on insurance coverage, and the paper concludes in Section VIII with a discussion of the policy implications of our results.

I. Health Insurance and Retirement--Should It Matter?

A. Previous Literature on Retirement

A number of studies have examined the retirement behavior of older (primarily male) workers. This literature is largely motivated by the striking decline in post-war labor force participation among those over age 55. In 1950, 87% of men aged 55-64 were in the labor force as were 46% of those over age 65. By 1991, the labor force participation rates had fallen to 67% and 16% for these two groups.

Three factors have been extensively studied as explanations for this trend: the growth of the Social Security program, the increased availability and generosity of private pensions, and the expansion of federal disability insurance. This literature has generally concluded that the effect of Social Security on retirement is small. Burtless (1986) estimates that the 20% real increase in Social Security benefits in the early 1970s reduced the age at retirement by only 0.2 years. Similar results are reported in Burtless and Moffitt (1984), Hausman and Wise (1985), Sueyoshi (1989), and Diamond and Hausman (1984). Recent research has suggested that pensions, in contrast, may have a much larger effect on retirement. Stock and Wise (1990a and 1990b) find that increasing the pension early retirement age from 55 to 60 decreases the fraction of workers who retire before age 60 by one-third. The effect of disability insurance remains unclear; a recent study by Bound and Waidmann (1992) concludes that between one-quarter and one-third of early retirement among 55-64 year-olds can be attributed to the growth of this program.

One potentially important factor which has not received much attention is the expansion of health insurance coverage for retirees. The introduction of Medicare in the mid 1960s provided universal insurance coverage for those over age 65.[1] The postwar period has seen substantial growth in employer-provided insurance coverage for retirees as well. Madrian (1993) finds that while only 30% of men who retired in 1960-64 received retiree health insurance from their former employers, almost half of those retiring during the 1980s did. This oversight is especially surprising given the rather consistent evidence that health status is a significant predictor of early retirement (Diamond and Hausman 1984; Bazzoli 1985). If health status matters in the decision about when to retire, it seems quite natural that health insurance should matter as well.

Two recent papers have attempted to model the role of health insurance in the retirement decision. Lumsdaine, Stock and Wise (1992) incorporate the value of Medicare into an option value model of retirement and find no effect of Medicare eligibility on the

[1] Several of the articles cited above suggest that there appears to be "excess" retirement at age 65, even given the incentives provided by Social Security and private pensions at that age. Medicare eligibility may provide an explanation for this "excess".

retirement hazard. This result is not surprising, however, as they estimate their model on a sample of workers from the same firm, all of whom have employer-provided post-retirement health insurance which is much more generous than Medicare. Gustman and Steinmeier (1992) use information from the Retirement History Survey, a longitudinal survey from the 1970s, to ascertain whether individuals have employer-provided retiree health insurance, and data from the 1977 National Medical Care Expenditure Survey, to impute the value of that insurance based on individual characteristics. They also find very small effects of retiree health insurance on retirement decisions.

The results of these two studies are at odds with both intuition and with what individuals report about the importance of health insurance in the retirement decision. In a recent Gallup poll, 63 percent of working Americans reported that they "would delay retirement until becoming eligible for Medicare [age 65] if their employers were not going to provide health coverage" despite the fact that 50 percent "said they would prefer to retire early--by age 62" (Employee Benefits Research Institute 1990). The apparent contradiction between the importance of health insurance as stated by individuals and that estimated by these two previous studies provides a further motivation for our research.

B. Health Status of Older Individuals

That individuals should cite health insurance as an important consideration in the retirement decision is not surprising, as older persons are fairly likely to need expensive medical care. Tables 1-4 compare the health status of individuals by age along a number of dimensions. The simplest measure, self-reported health status, is shown in Table 1. The fraction of individuals who report being in fair or poor health increases markedly from ages

45-54 (19.7%) to ages 55-64 (31.3%). While recent research has suggested that self-reported health status may be a poor indicator of the actual severity of an individual's clinical conditions (Bazzoli 1985), it may be the most accurate measure of an individual's valuation of health insurance coverage. Thus, these figures suggest that insurance valuation will rise dramatically with age.

Furthermore, as Table 2 shows, health status as measured by doctor-diagnosed health problems deteriorates with age as well. The incidence of many of the health problems listed (stroke, cancer, heart attack, arteriosclerosis, emphysema, and heart disease) more than doubles between ages 45-54 and ages 55-64. Furthermore, almost three-quarters of those aged 55-64 have been diagnosed with at least one of the 11 conditions listed. Not surprisingly, relative to those aged 45-54, individuals 55-64 are more likely to be admitted to the hospital over the course of a year and spend more time there once admitted (Table 3).

The most direct evidence that health insurance should be valued relatively highly by older workers, however, is that the actual medical expenses incurred by those aged 55-64 are much higher than those of younger individuals (Table 4). In every category, not only do expenditures rise with age, but the variance increases as well. In 1990 dollars, total medical expenditures of those 55-64 averaged $2144. This represents 5.4% of average total family income for this age group, 6.9% of average total family income for retired individuals, and 30% of the average pension income of early retirees.[2] A one standard deviation increase in expenditures for a 55-64 year-old would represent an additional 16.5% of family income.[3]

[2] Expenditures as a fraction of income are calculated using income data from the March 1990 Current Population Survey.

[3] These numbers include expenditures by both insured and uninsured individuals. To the extent that insurance-induced moral hazard results in spending with low marginal benefits, the actual expenditures of early retirees wishing to maintain a given level of health may be lower.

Total family medical expenditures would naturally constitute a much higher fraction of income. Thus, it is easy to see why older individuals should be concerned about their health insurance coverage after retirement.

C. Health Insurance Coverage and Costs

Given the costs of health care for older workers, it should not be surprising that older individuals are no more likely to be uninsured than their younger counterparts as is shown in Table 5. The sources of health insurance coverage, however, differ with age. Even though employment-based health insurance is the predominant source of coverage regardless of age, older individuals are less likely than younger persons to have employment-based health insurance, and much more likely to be covered by a nongroup (individual) or other group policy. This suggests that individuals who retire early but who do not have access to employer-provided health insurance turn to the individual market for insurance.

The bottom two sections of Table 5 break down the sources of health insurance coverage by employment status. There are three major differences between the sources of health insurance coverage for those who are and are not employed. First, one-fifth of non-working older persons are insured through Medicare or Medicaid, while only 1% of the older employed receive coverage from one of these two sources. Second, older non-working individuals are 40% less likely to be uninsured than their younger counterparts. Third, relative to the young, the older non-working are six times more likely to be covered by employer-provided health insurance in their own name.

These last two differences are explained in large part by the availability of employer-provided post-retirement health insurance. 45% of individuals work in firms that provide retiree health insurance benefits[4]. The older non-working, who are more likely to be retired than the young non-working, are therefore more likely to be covered by employer-provided retiree health insurance.

There are, nevertheless, a substantial number of older individuals who are not covered by either employer or government-provided health insurance. It is these individuals who find themselves in the market for individual health insurance and who we would therefore expect to benefit from the availability of continuation coverage. The reason is simple--insurance in the individual market is typically quite expensive.

Employers have a significant cost advantage in providing health insurance. By pooling the risks of many individuals, they are able to lower administrative expenses and reduce adverse selection. These two factors alone are estimated to reduce the cost of insurance in large (10,000 or more employee) firms relative to small (1-4 employee) firms by 40% (Congressional Research Service 1988). In addition, employer provision of health insurance is completely tax deductible (that is, it is not counted as taxable income to the employee), while expenditures on health insurance by the individual are only deductible for individuals who itemize and only to the extent that they exceed 7.5% of adjusted gross income.[5] For older individuals, the cost differential between employer-provided and individual health insurance is exacerbated by the fact that policies in the individual market are typically age-rated, while within the firm younger workers subsidize the health insurance costs of their older co-workers. The Congressional Research Service (1988) reports that the

[4] See Madrian (1993) for background on the structure and availability of post-retirement health insurance.

[5] From January 1, 1986 to June 30, 1992, 25% of expenditures on health insurance in the private market were also deductible for those who were self-employed and who had no other source of health insurance.

cost to employers of providing insurance coverage for 55-64 year old males is three times that of providing coverage to males under 40; for females, the ratio is two to one.[6]

In Massachusetts, the average cost of family health insurance coverage per employee in 1989 was $3882.[7] When inflated by the medical care component of the Consumer Price Index, this is equivalent to $5047 in 1993 dollars. In contrast, a New England commercial insurance company is offering a family policy for a 58 year-old male with a 1-year preexisting conditions exclusion for $8640. This represents 26% of the average family income of retired individuals aged 55-64 in Massachusetts. Individual policies are also generally medically underwritten, so that sick individuals may face substantially higher prices. Alternatively, they may not be able to purchase a policy at all, or may face even stricter pre-existing conditions exclusions.

The coverage available in the private market not only is expensive, but is typically less generous than employer-provided health insurance as well. Table 6 compares the health insurance benefits of individuals covered under group and nongroup policies in 1977. In every category, those covered under nongroup policies receive more limited benefits. Relative to those with nongroup coverage, those with group policies are more than twice as likely to receive major medical coverage or coverage for physician office visits and prescription drugs, and more than 50 percent more likely to receive ambulance, mental health, and outpatient diagnostic service coverage. Furthermore, nongroup policies generally feature both higher deductibles and higher copayments. Thus, relative to the individual

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[6] Of course, to the extent that these employer costs can be shifted to the wages of employees in an age-specific fashion, older individuals will bear their higher costs. See the discussion in Section III.

[7] Authors' calculation using unpublished data from the Health Insurance Association of America.

market, continuation of group coverage offers individuals higher quality insurance coverage at a significantly lower price.

II. Continuation of Coverage Laws

For those individuals whose employers do not offer retiree health insurance, an alternative to purchasing health insurance in the individual market is provided by various state and federal continuation of coverage laws. These laws mandate that employers sponsoring group health insurance plans offer terminating employees and their families the right to continue their health insurance coverage through the employer's plan for a specified period of time. The laws generally apply to all separations (except those due to an employees gross misconduct), although in some states benefits are restricted to those who leave their jobs involuntarily.[8] They often also provide benefits to divorced or widowed spouses and their families. The first such law was implemented by Minnesota in 1974. More than 20 states passed similar laws over the next decade before the federal government, as part of its 1985 Consolidated Omnibus Budget Reconciliation Act (COBRA), mandated such coverage at the national level. Continuation coverage is now commonly referred to as COBRA coverage, a nomenclature we will also use.

The various state statutes are summarized in Table 7.[9] The length of coverage is generally quite short, from 3-6 months, although 10 states mandate coverage of nine months

[8] Because retirement is a voluntary separation, we treat those states whose laws apply only to involuntarily terminated employees as states without laws.

[9] Details on state laws are from Hewitt (1985) and Thompson Publishing (1992) and have been cross-checked against the actual statutes. Table 7 lists only those states with laws that apply to employees who terminate their employment voluntarily. There are, in addition, several states with laws that apply only to involuntarily terminated employees.

or more. Most state laws stipulate that an employee must have been covered by an employer's insurance for 3-6 months before being eligible for continuation coverage. This condition, however, is not likely to be binding of older workers, most of whom have been with their current employer for many years.[10] The state laws also apply only to firms that actually purchase insurance through an insurance company; self-insured firms, under the 1974 Employee Retirement Income and Security Act (ERISA), are not subject to these (or any other) state mandates.

Although similar in spirit, the state and federal laws differ in a number of important ways. First, the length of coverage mandated under the federal law, 18 months, equals or exceeds that mandated by all but one state (as of January 1987, Connecticut law provides for up to 20 months of coverage).[11] Second, there is no minimal length of time for which an employee must be covered under an employer's plan before being eligible for continuation benefits. Third, the federal law applies to self-insured firms, who are exempt from the state laws, as well as to those who purchase their coverage from insurers. The federal law, however, does not apply to small firms employing less than 20 workers. Finally, employees of religious organizations and the federal government were exempt from COBRA, although federal employees have subsequently been included (beginning in 1990). When the specific details of the state and federal statutes are at odds, firm provision of continuation benefits is governed by the law which provides for more generous coverage.

[10] Almost 95% of retirees have job tenure of at least ten years by the time they retire (Madrian 1993).

[11] 18 months is the maximum length of coverage available following the voluntary or involuntary termination of employment. COBRA also provides up to 36 months of coverage for family members who would otherwise lose their insurance coverage through events such as an employee's death, divorce from the employee, or the employee's eligibility for Medicare.

The effective dates of the state laws are listed in Table 7. The federal coverage mandated under COBRA was phased in. Beginning in July 1986, firms had to offer continuation benefits at the start of their next plan year. For workers provided with health insurance under union contracts, such benefits did not have to be offered until the next contract negotiation after January 1987.

Both the state and federal laws stipulate that the employee must pay the full cost of the coverage. At the federal level, this is defined specifically as 102% of the average employer cost of providing coverage. The coverage must be identical to that provided to similarly situated active employees, including the option to continue enrollment in supplemental insurance plans (such as for vision or dental care) if these are available. Although 102% of the employer's cost is typically much more than individuals pay as active employees,[12] it is, as already noted, substantially less than the cost of buying equivalent coverage in the private market, especially for older workers.

Because continuation coverage is a relatively new phenomenon (at least at the national level), information on the extent of continuation coverage is somewhat scare. Zedlewski (1993) estimates that in 1988, 5.2% of retired workers aged 55-64 were covered by COBRA health insurance. This figure must be interpreted relative to the number of individuals who could be expected to take up such coverage. The 52% of individuals aged 55-64 with retiree health insurance are not likely to be covered, and the 21% of individuals who were not insured through their former employer are not eligible. Similarly, those who have been retired for more than 18 months will have exceeded their potential eligibility. Tabulations

[12] In most firms the cost of insurance is highly subsidized. Of course, the full cost of health insurance will ultimately be borne in the form of lower wages whether or not an actual deduction for health insurance shows up on the employee's pay stub.

from the 1987 National Medical Expenditure Survey indicate that one-third of retired individuals aged 55-64 have been retired for less than 18 months. If we take the group who could potentially be affected by COBRA to be one-third of retired individuals between ages 55 and 64 who worked in firms that provided health insurance but did not provide retiree health insurance, we would expect at most 9% of early retirees to be covered. That 5.2% receive continuation benefits suggests that 58% of the retired population who would be at all likely to be covered by COBRA actually are. As knowledge about the availability of such coverage has become more widespread since 1988, this fraction may be higher today.

An alternative calculation is possible using figures reported in Flynn (1992). She uses data from a large firm that administers COBRA claims to estimate that 23% of individuals who qualified for COBRA coverage because of retirement elected to receive benefits. If we would only expect the 30% of individuals in firms that offer health insurance but do not offer retiree health insurance to even consider purchasing COBRA insurance, this take-up rate implies that 75% of those most likely to be covered by continuation benefits actually are. Both of these calculations, therefore, suggest that retirees without an alternative source of health insurance coverage are quite likely to elect continuation coverage.

For all COBRA beneficiaries, the average length of time on COBRA was 7 months (Flynn 1992). Individuals over age 61, however, maintained their coverage for a much longer period of time--about 12 months on average. This finding is not surprising. First, younger individuals are more likely to find alternative coverage through a new job or a spouse's employment. Second, COBRA coverage provides a larger subsidy for older workers; with a lower relative price, they should therefore demand more coverage.

Table 8 compares the distribution of health insurance coverage in 1984, two years before COBRA was first implemented, and in 1989, two years after it had been phased-in. Note that employment-based health insurance coverage is more prevalent after COBRA, and that this effect is confined to those who are not employed, exactly the group whom we would expect to be insured under COBRA. Table 9 compares health insurance coverage before and after COBRA for those states with pre-COBRA laws mandating 9 or more months of coverage and those states with no pre-COBRA law. Coverage by employer-provided health insurance increases after COBRA in those states that did not have any continuation mandates, once again the group that we would most expect to be affected by COBRA. In contrast, there is a decline in coverage by employer-provided health insurance after COBRA in the states that already had continuation mandates in place.

Rogowski and Karoly (1992) examine the primary source of insurance coverage after retirement based on the source of insurance coverage before retirement before and after COBRA. They find that in the pre-COBRA period, 72% of individuals who retired from jobs with employment-based health insurance continued to be covered by that insurance upon retirement. After COBRA, this figure rises to 78.5%. Taken together, the evidence on take-up rates and the increase in the extent of employer-provided health insurance coverage among early retirees after COBRA suggests that older workers who retire early and who do not have an alternative source of coverage actually avail themselves of the continuation benefits to which they are entitled.

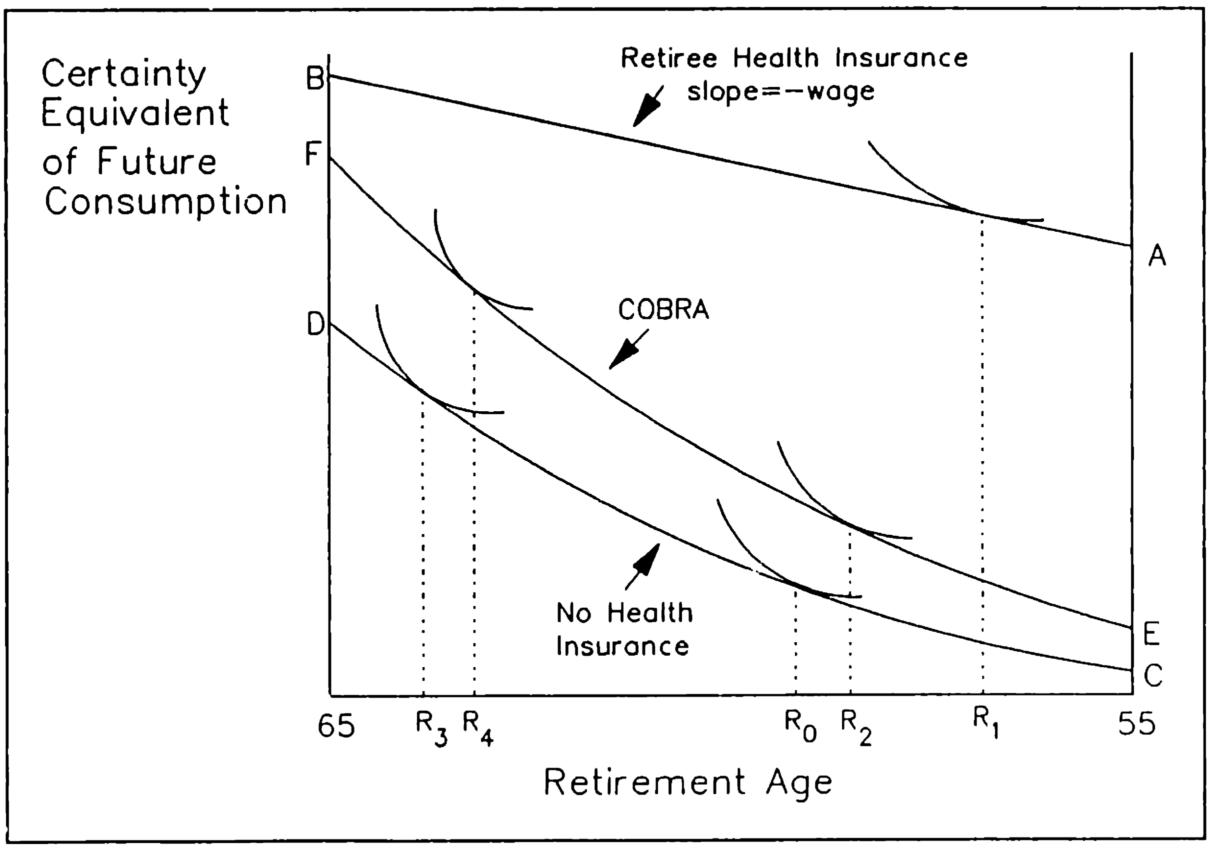
Our use of continuation of coverage regulations as the source of variation for identifying the effect of insurance coverage on retirement has both advantages and disadvantages relative to looking directly at workers with and without employer-provided retiree health insurance. One potential problem with the latter strategy is that the researcher is unable to control for job characteristics which may be correlated with both the generosity of retiree health coverage and the incentives that these jobs offer for retirement. An obvious example is pensions (which are accounted for in both Lumsdaine, Stock and Wise (1992) and Gustman and Steinmeier (1992)). There may be a number of other ways in which firms encourage or discourage retirement, however, such as through the tasks that they assign older workers or the wage profile that these workers are offered. Furthermore, there may be sorting of workers by retirement propensities into the types of firms that do or do not offer retiree health insurance. To the extent that these are unobserved to the econometrician but correlated with both the offering of retiree coverage and the retirement decision, they will bias the estimated effect of such coverage on retirement. What is needed to identify the effect of retiree health insurance is exogenous assignment of such coverage to individuals that is independent of these other job characteristics. Continuation mandates potentially provide such exogenous assignment.

The primary disadvantage of our strategy is that continuation benefits are more expensive to the early retiree than retirement health insurance and only provide coverage for a limited number of months. These differences may make it unreasonable to extrapolate our results to infer the effects of full retiree health insurance coverage. In the next section, we discuss how these differences affect the relative influence of full retiree coverage and continuation benefits on the retirement decision.

III. Modelling the Effect of Health Insurance on Retirement

We present a simple graphical exposition of the effect of health insurance benefits on the retirement decision, along the lines of Burtless (1986) and Burtless and Moffitt (1984). We consider both retiree health insurance in general and continuation benefits more specifically. Figure 1 shows the budget constraint facing an older worker between the ages of 55 and 65. The horizontal axis represents the age of retirement. The vertical axis measures the certainty equivalent (CE) of consumption from age 55 onward. This differs from the earlier literature which has typically considered the relationship between the age at retirement and the actual level of future consumption rather than certainty equivalent of future consumption. This departure is necessitated by our focus on the effect of insurance coverage.

FIGURE 1: Modelling the Effect of Continuation Coverage on Retirement



We assume that workers receive health insurance on their current job but that they may or may not have retiree health insurance coverage. Firms that provide post-retirement health insurance do so on the same basis for both workers and retirees, and these benefits cease upon eligibility for Medicare.[13] We also assume that once a worker leaves his current job, he will remain retired for the rest of his life. To simplify the analysis, we ignore the effects of both Social Security and pensions; they could, however, be easily incorporated into the analysis.

In the model, as in the real world, workers who retire without health insurance coverage have two options: they may purchase an individual policy, or go uninsured. In either case, their out-of-pocket medical expenditures will be significantly higher than if they receive retiree coverage or have the option of continuing their group coverage. For a worker with retiree health insurance, the slope of the budget constraint will be the after-tax wage, which is depicted by line line AB in Figure 1. Since medical expenditures are insured, there is no uncertainty about future consumption.

For the worker without retiree coverage, the relative position and slope of the budget constraint depends on two factors. First, because individuals are risk averse, those without retiree health insurance will have a lower level of CE consumption; this places the no insurance budget constraint below that of an insured worker.[14] Second, because both the mean and the variance of medical expenditures rise with age, a year of health insurance coverage is worth more at older ages. The cumulative reduction in CE consumption will be

[13] In reality, most retiree health insurance plans do "top off" Medicare to some extent; see Madrian (1993) for details. This does not alter the main conclusions of this section.

[14] Risk aversion in this model operates in a similar fashion to higher expected medical costs.

greater at younger retirement ages, but the incremental effect will be smaller. This latter effect gives curvature to the no health insurance budget constraint, line CD in Figure 1. At age 65 there is a jump in the no insurance budget constraint as Medicare equalizes the opportunities of all individuals.

If leisure is a normal good, retiree health insurance will lead to earlier retirement, at age $R_1 < R_0$, because such coverage makes individuals wealthier. As individuals are more risk averse, the wealth effect will increase as both the level of the no health insurance budget constraint falls and its slope becomes steeper.

Now consider the effect of a continuation mandate that provides one year of subsidized insurance coverage relative to having no health insurance. For the risk-neutral worker, this is simply equivalent to an increment to wealth equal to expected medical costs for a year minus the cost of the group policy.[15] This increment rises in value as the worker ages since expected medical expenditures increase with age. Thus, the budget constraint with a continuation option, line EF, lies above the no health insurance constraint but below the retiree coverage constraint. At younger ages, it is very close to the no insurance constraint; at age 64, it differs from the retiree coverage constraint by the cost of the group coverage. As workers become more risk averse and the no health insurance constraint becomes steeper, the distance between the no health insurance and the continuation coverage constraints will increase, and this increase will be greater at older ages. In this case, the value of one year of coverage will equal expected medical costs minus the cost of the group policy plus the increase in CE consumption implied by eliminating uncertainty in that year.

[15] Once again, this amount is presumably positive even for a risk-neutral worker due to cross-subsidization of the group policy by younger co-workers.

The value of both retiree health insurance and continuation benefits will rise with the cost of being uninsured or the cost of buying individual insurance in the private market. The important difference between these two sources of coverage, however, is their age patterns: while retiree insurance coverage is of highest value to very early retirees, continuation benefits are more valuable at older ages. Because of this, we might expect continuation benefits to be used primarily by older workers seeking a "bridge to Medicare" which allows them to retire a certain number of months before age 65 without losing group coverage. If this is true, we would expect the effect of continuation coverage on retirement to be greatest at older ages.

There are, however, a number of complications which cloud this basic intuition. The first is the heterogeneous nature of the population of early retirees. Individuals who retire at younger ages may have, by that very act, revealed themselves to be less attached to the labor force or more sensitive to retirement incentives. To the extent that there is a positive correlation between the level of attachment to the labor force and the responsiveness of individuals to retirement incentives, continuation benefits may have a larger effect, per dollar, on younger retirees. On the other hand, it may be that individuals who retire early reveal themselves to have a very high disutility of work so that they will be less responsive to any financial incentives. Whether the effect of continuation benefits should be greater or smaller at older ages is therefore unclear. In Figure 1, we have illustrated a case where the effect of continuation benefits is roughly equal for individuals of younger and older ages.

The analysis is further complicated by the empirical violation of one of our assumptions, namely that retirement is permanent. Diamond and Hausman (1984) report substantial reentry rates for early retirees; among 55-64 year olds, the one-year reentry rate is approximately 15%. Sueyoshi (1989) finds that one-third of the elderly "partially retire", moving from permanent employment to less than full-time work. To the extent that continuation mandates facilitate movement across jobs, rather than permanent retirement, they may have larger effects at younger ages than was depicted above.[16]

In this analysis, we have assumed that retiree health insurance offers pure rents to workers in the firms that offer this type of coverage. In labor market equilibrium, presumably at least a portion of these rents will be reflected in lower wages for workers with retiree coverage. The extent to which these compensating differentials offset the benefits of retiree health insurance at each age will be a function of the employer's ability to set relative age-specific wages freely,[17] the mobility of workers across firms at different ages, and the excess of the cost of continuation benefits over the group premium paid by the early retiree.[18] The existence of compensating differentials may affect both the location and the shape of the budget constraint facing the potential retiree; the net effect on retirement age will be a function of the nature of the compensating differential.[19]

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[16] One important consideration, of course, is whether this reentry is to jobs that offer health insurance; unfortunately, there is little evidence on this question.

[17] See Rosen (1986) for a discussion of the theory of compensating differentials. Gruber (1992) provides some evidence that shifting the costs of employer-provided benefits to distinct demographic groups in the workplace is feasible.

[18] Huth (1991) reports that the health insurance claims of COBRA recipients exceed those of active employees by 50%. This difference in costs is attributed to adverse selection; it is the sickest individuals who will find continuation coverage most attractive and they will therefore be the ones most likely to take it up. Long and Marquis (1992), who examine the actual health insurance claims in three firms, find similar evidence that expenditures of COBRA beneficiaries are higher than those for active employees.

[19] For example, if the entire cost of the benefits is shifted to older workers, this will lower the slope of the budget constraint with continuation benefits relative to the one without benefits (because wages for those with benefits fall), which will have both income and substitution effects on the retirement decision.

Finally, we have ignored the possibility that workers may be liquidity constrained in making their retirement decision. The fact that most retirees have few liquid assets (Diamond and Hausman 1984) implies that such liquidity constraints may be empirically important in determining retirement dates. This explanation is suggested in both Diamond and Hausman (1984) and Burtless and Moffitt (1984) in their discussion of why Social Security benefits do not seem to affect retirement until they actually become available at age 62. Samwick (1993) finds that much of the estimated increase in retirement probabilities attributed to Social Security occurs among those with pensions, suggesting that all workers would like to take advantage of these benefits early, but that only those with pensions can afford to do so. The presence of liquidity constraints could increase the effect of continuation benefits at younger ages, as the wealth increment which the benefits represent could be loosening these constraints.

IV. Data

The ideal data source for this project would have several characteristics. It would extend over a number of years before and after 1986 in order to exploit the variation in state and federal continuation of coverage legislation. Because these laws should only affect individuals in firms that provide health insurance but do not provide retiree health insurance, it would have a large sample size so that the effects of state law changes on older workers could be identified. It would be longitudinal in order to facilitate dynamic modelling of the transition into retirement. And it would contain detailed information on pension incentives which have been found to be central to the retirement decision. Unfortunately, there is no source of data which meets all of these qualifications. We therefore rely on two different data sets in an effort to exploit the relative strengths of each. The data set with the largest sample size and which covers the longest time period is the Current Population Survey (CPS) which interviews over 50,000 households each month. Unfortunately, this data is not primarily longitudinal and it contains only limited information on pension availability.

We take two approaches with the Current Population Survey data. First, we use the Merged Outgoing Rotation Group (MORG) file from 1980-1990 which includes information on demographic characteristics and labor force attachment during the survey week for one-quarter of each month's sample for each month of the year. This is the largest available annual data set on individual labor force behavior. With this data we can examine the effects of continuation mandates on the probability that an individual is retired at a point in time. Although recent work on retirement has focused on the transition into retirement, evidence on the stock of retired persons can still be useful in this context; if the laws are affecting flows, they should affect stocks as well.[20] Furthermore, this method avoids some of the potential econometric difficulties with our dynamic modelling strategies. The drawback of this data, however, is that we have no information on the previous jobs of those who are retired and are therefore unable to control for the characteristics of those jobs.

We also make use of the limited longitudinal capabilities of the 1980-1990 March Current Population Surveys, which ask both about current labor force attachment and labor force behavior during the previous year, to estimate a dynamic model of the transition into retirement. We examine the effect of continuation benefits on the probability that individuals

[20] This is not strictly true if the mandates affect the number of persons who decide to work at all; in this case, both the numerator and denominator of the labor force participation rate would be increasing, and the effect on the stock would be ambiguous. This is not likely to be a problem for the sample of older males on which we focus. We discuss other issues involved in relating stocks to flows below.

who worked at some point during the previous year are retired at the time of the survey. Focusing on individuals who retired during the year enables us to control for a number of characteristics of the individual's job, such as industry, occupation, and whether the worker received health insurance or was covered by a pension. The sample size, however, is approximately one-fourth that of our MORG sample.

This second approach has a drawback as well. Our sample selection rule, that individuals be working in the previous year, introduces a "dynamic self-selection" bias into our estimation, as noted by Diamond and Hausman (1984). In a bivariate setting, this would lead to a downward bias of our estimates of the effect of continuation provisions. Imagine that continuation of coverage availability does induce individuals to retire earlier. The set of individuals whom we observe actually working in a state after the law has been in place for a number of years will be less likely to retire in response to the law than would the entire population because those most likely to respond will have already retired. Thus, by selecting on those who are still working, we are less likely to find an effect of these provisions since that very group has shown itself less likely to respond.[21] In a multivariate context, however, the direction of the bias cannot be signed a priori.

With only time invariant covariates in our model, it would be possible to account for this "left-censoring" by modelling the retirement decisions of those already retired at the start of our sample (Amemiya 1985). Our key covariate, however, the months of continuation coverage available, varies over time. Without knowing its value at the point at which

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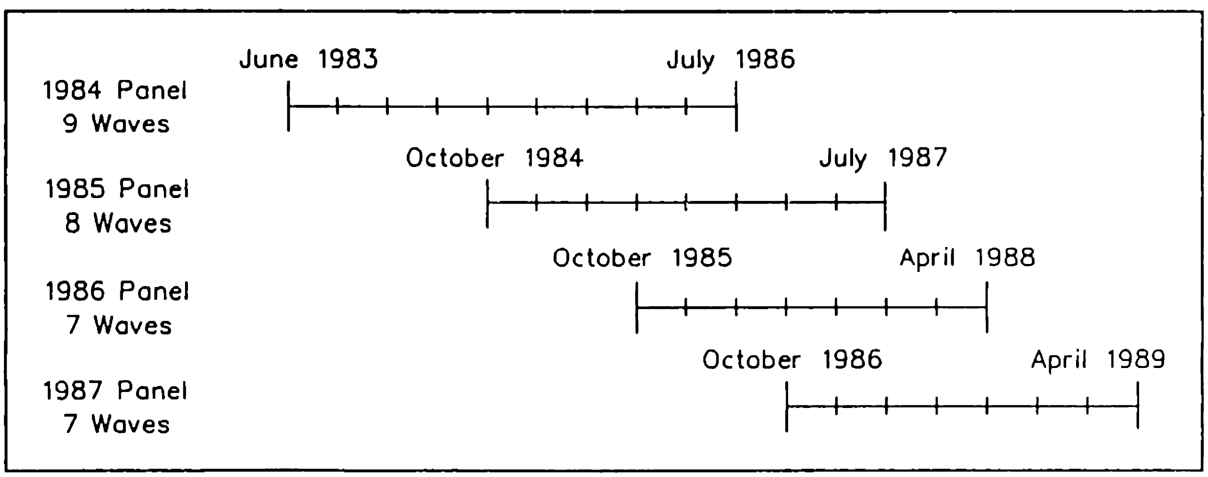
[21] An alternative way to see this point is to imagine a law that applied to a cohort, rather than to an age group. The individuals who were most likely to respond to this law would do so in the first year. In the next year, by selecting on the set of persons who have not yet responded, we will bias the result against finding an effect of the law. When the law applies to an age group, rather than a cohort, this effect is attenuated by the fact that new members arrive into the age group.

individuals made their retirement decisions, it is not possible to model those decisions. Thus, we are unable to account for this bias in our results.

To look at retirement in a more dynamic framework, we also use data from the 1984, 1985, 1986 and 1987 panels of the Survey of Income and Program Participation (SIPP). The SIPP is a nationally representative survey of households designed to collect information on the economic and demographic characteristics of individuals and their families. Sample members are interviewed every four months for roughly 2½ years and asked to provide information about their labor market activity, income, and participation in welfare and transfer programs over the previous four months. These quarterly interviews are referred to as "Waves". The first interviews of the 1984 Panel were conducted in October of 1983, while the initial interviews for subsequent panels commenced in February of the corresponding calendar year. Each panel is divided into four approximately equal groups ("Rotation Groups"), of which one is interviewed each month. Figure 2 illustrates the time span covered by the respective panels. Note that because the panels overlap, as many as three panels may be interviewed concurrently.

FIGURE 2: Reference Periods of the 1984-87 SIPP Panels



The primary advantage of using a longitudinal data set such as the SIPP is that we are able to observe the transition from work to retirement. We do so using a hazard framework described below. Relative to other panel data sets such as the PSID and the Retirement History Survey (RHS), the SIPP provides better information with which to look at retirement because it contains monthly measures on individual labor force status. The availability of true longitudinal data also allows us to implement a test for the importance of the dynamic self-selection bias discussed above. The panel is relatively short, however, and the sample size is much smaller than either CPS sample. We also lose some of the variation in state laws that we use to identify the effect of continuation coverage on retirement because the SIPP did not begin until October of 1983.

It should be noted that none of these data sets contains information on pension incentives. This will only affect our results if pension incentives changed systematically in a way which was correlated with the passage of continuation of coverage mandates. We do, however, control for industry, occupation, and the availability of health insurance in our empirical work using the SIPP and the March CPS; in the March CPS we also control for the presence of a pension.

V. Empirical Results--Current Population Survey

A. Merged Outgoing Rotation Group (MORG) Data: Probability of Being Retired

We first use the 1980-1990 CPS MORG data to examine the effect of continuation mandates on whether or not an individual is retired. We focus on two definitions of retirement, both based on a CPS question which asks about the major activity in which an individual was engaged during the week before the survey: whether or not an individual reports being retired, and whether or not an individual is out of the labor force. The latter definition is useful both because retirement may be a subjective term which takes on different meanings for different individuals, and because this measure corresponds most closely to the one we will use in the SIPP. These retirement definitions are clearly problematic along at least two dimensions. First, we are unable to contrast the effect of these regulations on both "full" and "partial" retirement, as is done in Burtless and Moffit (1984) or Sueyoshi (1989). Second, we are unable to account for reentry into the labor market, as discussed in Diamond and Hausman (1984). Nevertheless, these measures should provide reasonable estimates of the effect of continuation mandates on the propensity of older workers to remain employed. In future work, we plan to explore in more detail other measures of the transition to retirement.

Our sample consists of men age 55-64. Means of the sample are presented in the first column of Table 10. Overall, 20% of the sample report being retired and 35% are out of the labor force. The sample is fairly well-educated, and there are relatively few non-whites.

We estimate the following probit model of retirement:

$$Pr(Retired_{ijt}) = \Phi(\alpha + \beta_1 \cdot X_{ijt} + \beta_2 \cdot State_j + \beta_3 \cdot Time_t + \beta_4 \cdot Law_{jt}) \qquad (1)$$

where i indexes individuals, j indexes states, and t indexes time. X_{ijt} is a set of individual demographic characteristics, $State_j$ is a set of state dummies, $Time_t$ is a set of year and month dummies, and Law_{jt} is the number of months of continuation coverage available in state j at time t. The state fixed effects control for any time invariant characteristics of a state which may be correlated with the state's propensity to pass continuation legislation.[22] We include a set of year dummies to control for national trends in retirement behavior which may be correlated with the passage of these laws, and month dummies to control for seasonal patterns in retirement behavior. Thus, the effect of the laws is identified in this model by changes in retirement behavior in states which passed the laws (or which were affected by the Federal law), relative to those which did not, during the period after the laws were passed. Further identifying variation comes from differences across states in the number of months of eligibility which these laws allow. Since we have monthly data, we phase-in the federal law in 12 equal increments between July 1986 and June 1987.

The basic regression results are reported in Table 11. The first column reports the probit coefficients from the self-reported retirement equation while the second column gives the marginal probabilities implied by these coefficients.[23] The same is done in the third and fourth columns using not in the labor force as the definition of retirement. More education is associated with a slightly lower probability of being retired and a much lower probability of being out of the labor force. Being non-white is associated with a lower probability of retirement but a significantly higher probability of being out of the labor force. And individuals who are married are less likely to be either retired or out of the labor force. The age pattern of retirement propensities is familiar from the previous literature; there is a large

[22] We exclude two states from our sample: Hawaii, which has mandated health insurance for all employees, and West Virginia, for which we were unable to definitively date their continuation mandate.

[23] For dummy variables, the marginal probabilities are calculated by predicting the probability of retirement with the dummy equal to one for the entire sample, predicting the probability with the dummy equal to zero for the entire sample, and taking the average of the difference in these predictions across all individuals. For continuous variables, the marginal probability is calculated by predicting the probability at the current level of the variable, predicting the probability by adding one to the variable, and once again taking the average of the difference in these predictions across individuals.

jump in the probability of being retired at age 62, and individuals age 64 are 25% more likely to be retired than individuals age 55. This pattern is even more pronounced for being out of the labor force, as the probability at age 64 is 40% greater than the probability at age 55.

There is a sizeable and significant effect on the stock of retired persons of another month of continuation coverage. One year of coverage raises the probability that an individual is retired by 1.1 percentage points which is 5.4% of the baseline probability of being retired in this sample.[24] For the not in the labor force regressions, the estimated effect of a year of continuation coverage is of approximately the same magnitude as in the retired equation (although the coefficient is only significant at the 10% level), and suggests an increase in the baseline probability of being out of the labor force of 2.8%

The model described in Section III suggests the possibility that the effect of continuation mandates on retirement could vary with age; intuitively, it seemed that this effect should be strongest at older ages. In Table 12, therefore, we free up the effect of months of continuation coverage by age. In fact, we do see a very strong pattern of increasing effects of continuation of coverage availability on older workers. The effect in both specifications is statistically significant for workers age 62 and above. For 64 year-olds, one year of continuation coverage increases the baseline probability of being retired by 2% and of being out of the labor force by 2.5%. The last row of Table 12 reports the coefficient from an alternative specification which replaces the *Age*Months* interactions with a variable

[24] This marginal probability is evaluated for a one month change in coverage and then multiplied by 12 to get yearly values. This is not strictly correct when the function is nonlinear. When we evaluate the probit for a 12 month increment to coverage, the results are virtually identical; for the transition equations below, we slightly understate the effects by extrapolating from the one month coefficient.

equal to one if the months of coverage available to an individual is sufficient to ensure continuous health insurance coverage until the age of Medicare eligibility.[25] This variable, *Bridge to Medicare*, has a sizeable and significant coefficient and implies that the availability of continuation coverage increases the probability of retirement by 1.3% for those near the age of Medicare eligibility.

In these stock regressions, however, there is another reason why the effect may be stronger at more advanced ages. Even if the flow of individuals into retirement increases equally at every age in response to these mandates, in steady state the effect will accumulate by age so that the estimated effect on the stock of retired individuals will be largest at ages close to 65. This phenomena is illustrated by an example in Table 13. A five percentage point increase in the flow at each age beginning at age 60 increases the stock of retired by only 5% at age 60, but by 15% at age 65. On the other hand, when expressed as a fraction of the baseline retirement stock, the stock increase from a constant flow increase is declining with age rather than increasing.

Thus, we normalize the increment to the retired probabilities at each age by the baseline probability of being retired at that age (these are the bracketed numbers under the marginal probabilities in Table 12). For being retired, the effects now appear to *decline* with age, while for being out of the labor force they increase slightly. This suggests that the non-normalized findings represent both a cumulative stock effect and some increase in the retirement probabilities by age. We should note, however, that the relationship between stocks and flows described in Table 13 is a steady state calculation, while we are estimating

[25] This variable equals one if [age + (months of coverage + 6)/12] is greater than or equal to 65. Because we do not know an individual's month of birth, the construction of this variable is somewhat crude.

the effects during a period of transition. In transition, age accumulation will not operate to its full extent and we cannot separate its effect from that of retirement probability increments that rise with age. To help disentangle these two effects, we turn next to evidence on the transition into retirement.

B. March Current Population Survey Data: Transitions into Retirement

Each March, the CPS asks not only about current labor force attachment, but about labor force attachment in the previous year as well, including a number of questions about the characteristics of the longest job held during that year. We can therefore observe transitions into retirement over a one year interval. Once again we use two definitions of retirement, exactly analogous to those used in the MORG data. The first is having worked at least one week in the previous year but reporting being retired at the time of the survey; the second is having worked last year but being out of the labor force at the time of the survey. The estimated coefficients are very similar if we restrict the sample to individuals who worked at least 10 weeks in the previous year; they are somewhat weaker if we use a 26 week restriction, although this only allows us to examine retirement over an eight-month window.

In analyzing the transition into retirement of those who worked, we are able to control for a number of job characteristics. In addition to the previous set of regressors, we include 8 industry and 6 occupation controls, a control for having health insurance on the job, and a control for being covered by a pension on the job. The means for this data set are presented in the second column of Table 10. 7.3% of the sample retires each year, while 9.3% leaves the labor force; about 70% of workers have health insurance on the job, and

55% have a pension. The demographic characteristics are very similar to those in the MORG data.

The actual equation estimated is a probit as specified in equation (1) except that the variable to be explained is *retiring* rather than being retired. The results from this "flow" probit are presented in Table 14. While neither marital status nor education have a significant effect on the probability of retiring, they both substantially reduce the probability of leaving the labor force. Being non-white also decreases the likelihood of leaving the labor force, and has a negative impact on the probability of retiring as well. Workers who receive employer-provided health insurance on their job are also much less likely to retire or to leave the labor force. To the extent that these workers don't have retiree health insurance, this is supportive evidence for our contention that insurance affects retirement. However, it may also indicate that individuals are less likely to leave "good" jobs.[26] On the other hand, the coefficient on employer-provided pensions is completely insignificant. This is somewhat surprising given the strong evidence in the previous literature for the effect of pensions on retirement, and is probably due to the inability of a simple dummy variable to adequately capture pension incentives. Using data from the Survey of Consumer Finances, Samwick (1993) also finds that a simple pension dummy variable does not significantly predict the

[26] One important point to note in the CPS is that the definition of employer-provided health insurance changed in the March 1988 survey. Prior to that time, it measured coverage from the current employer only; it was then redefined to include coverage from a previous employer as well. This means that individuals with retiree coverage would be counted as uninsured before 1988 and insured thereafter. However, this should not bias our results. The question about insurance coverage in the CPS is whether you had coverage *at any time* during the previous year. Since we are only examining individuals who worked in the previous year, and having insurance on the job is generally a prerequisite for having retiree coverage, the number of individuals who had coverage at any time will measure the number with coverage from their previous job. Thus, while the number of 55-64 year-old males with employer provided heath insurance rose from 53% on average before 1988 to 62% after, for those who were working there was only a 0.05% change (actually a decline) in the rate of insurance coverage.

probability of retirement, although pension accrual rates do matter. Consistent with the results of the stock regression in Table 11, the age coefficients imply that the probability of retiring increases with age, once again with a spike at age 62. The probability of retiring at age 64 is approximately 15% higher than at age 55.

The coefficient on *Months of Coverage* is significant at the 5% level for the retirement equation and at the 8% level for the leaving the labor force equation. It suggests that another year of continuation coverage increases the probability of retiring by 1.4 percentage points which is 19% of the baseline retirement probability. The effect for leaving the labor force is of the same magnitude, although this represents a smaller 15% rise in the baseline probability. These are both very sizeable effects.

Tables 15A and 15B explore the age pattern of the effect of continuation coverage on retirement. In the first column of Table 15A, we see no obvious pattern to the age coefficients, and we cannot reject the restriction that the coefficients are the same at all ages between 55 and 64[27]. The three largest effects occur ages 58, 60, and 63; the effects at ages 57 and 61 are also significant at the 10% level. The first column in Table 15B repeats this exercise for leaving the labor force. The results are very similar, although the effect here is largest at age 60 and is significant at the 10% level at ages 59, 60, 63, and 64. The last row of Tables 15A and 15B present the *Bridge to Medicare* specification. As in the MORG data, the coefficients are positive, although only significant at the 15% level.

[27] The chi-squared test statistic is 7.65 for the retirement equation and 9.29 for the leaving the labor force equation. With 9 degrees of freedom, neither is significant at even the 10% level. In contrast, the analogous test statistics for the stock regressions using the MORG data are 16.44 for the retired equation and 27.2 for the not in the labor force equation.

These results are striking in that they are counter to the simple intuition that the effects of these mandates should be strongest at older ages. There are at least three reasons why this result may arise. The first is the set of theoretical issues we raised in Section III, such as the possibility that individuals who retire early are more sensitive to retirement incentives, or that individuals may face liquidity constraints which are loosened by this temporary health insurance. While we are unable to distinguish between these views, our findings suggest that, as a group, they are sufficient to counter the intuitive presumption that COBRA will act as a "bridge to Medicare".

The second reason is statistical: we may not have enough power in these transition probits to distinguish true larger effects at older ages from the effects at younger ages. In fact, if we include both the bridge to Medicare variable and the months of coverage variable in the retirement probit together, they are both positive; the months coefficient is significant at the 7% level, while the bridge to Medicare coefficient is only as large as its standard error. Furthermore, if the probit is run separately for those age 62 and above and those below age 62, the coefficient on *Months of Coverage* is three times as large for the older group. Thus, the results suggest, but do not statistically prove, that the result is stronger for those nearest the age of Medicare eligibility. However, there still remains the finding of some effect at the younger ages.

Finally, it may be that our result is spurious, either due to problems with our statistical model or our identifying assumptions. We previously noted the problem of dynamic sample selection bias. If this bias serves to increase the estimated effects of continuation coverage at younger ages, or to decrease them at older ages, it could explain our finding. Unfortunately, it is not possible to test for the significance of this bias without longitudinal data; we present some evidence on its importance in the SIPP data below.

One potential problem with our identification strategy is that the passage of these laws could be correlated with some other change in retirement behavior in these states. Alternatively, it could be that the laws themselves are endogenous responses to changes in retirement propensities among the population; that is, if more individuals are retiring, states may respond by mandating benefits that cover individuals after their retirement. There is a natural test for these alternative hypotheses, however. Note that only individuals who have health insurance on the job are eligible to receive continuation benefits. Thus, workers without health insurance on the job provide a natural control group for assessing the effects of continuation mandates. If these mandates are simply correlated with, or due to, exogenous changes in retirement propensities, then the laws should be correlated with the retirement propensities of workers both with and without health insurance on the job. However, if the laws are causing changes in retirement propensities, this should only affect workers with employer-provided health insurance.

The second and third columns in Tables 15A and 15B report the results separately for those with and without employer-provided health insurance on their job in the previous year. The coefficients on the number of months of coverage are 40% greater among those with employer-provided health insurance than for the full sample, while they are negative and insignificant for those without such health insurance. The coefficients are not statistically different, however, due to the higher degree of imprecision with which each is estimated in this split sample. When the effect of months of coverage is freed up by age, the results for those with health insurance are very strong at each age; they are significant at the 10% level for eight of the ten age interactions. On the other hand, the results for those without health insurance are uniformly insignificant and generally wrong-signed. While the coefficients once again are not statistically different from each other at any age, the pattern strongly supports our contention that the effect is only operating for those with health insurance on their previous job. The results for leaving the labor force in Table 15B are similar.

We also attempted several further specification checks on these results. First, as noted earlier, the effect of continuation of coverage regulations will be mitigated by factors such as the availability of retiree health insurance or the extent to which firms self-insure (as they are exempt from state mandates). In order to appropriately measure the effect of continuation coverage, we need to account for variation in these factors across the states. We do this by constructing a "corrected" months of coverage variable which adjusts for these various factors.

The Appendix details the calculation of this correction factor. Briefly, we use unpublished data from the Health Insurance Association of America (HIAA) and the Current Population Survey to estimate, by state, the fraction of workers actually offered employer-provided health insurance, the fraction in firms that offer retiree health insurance, the fraction in self-insured firms, and the fraction in small (<20 employee) firms. If a state law only is in effect, we adjust the months of coverage to reflect the fact that the state law applies only to those offered insurance and only to those who do not have retiree health insurance or who do not work in a firm that self-insures. If only a federal law is in effect, we adjust the months of coverage by the fraction of individuals who are offered insurance, the fraction who work in small firms, and the fraction in large firms that offer retiree health insurance. When both a state and federal law are in effect, the corrected months of coverage accounts for the fact that the state law will affect insured individuals working in small firms that do not offer retiree health insurance even though the federal law exempts small firms. When we adjust for these factors, the average length of continuation coverage in states that offer coverage falls from 14.4 months to 5.24 months.

In Tables 16A and 16B we include this "corrected" months of coverage variable in our transition probits. While the significance of the results does not change, there is a sizeable effect on the magnitude of the coefficients; they are generally more than twice as large as those in Table 15. In the second set of columns, the corrected probits are run for those with employer-provided health insurance only. In this case, the correction factor is increased, as described in the Appendix, to account for the fact that the full population under study has health insurance. Once again, the coefficients are much larger than before.

These corrected coefficients imply quite large effects. Using the coefficient from Model 1 of Table 16A, a year of continuation coverage now implies a 3.6 percentage point increase in the probability of retirement which is 50% of the baseline probability for 55-64 year olds. For 64 year-olds with health insurance (the second column of Table 16A), a year of coverage is estimated to raise retirement probability by almost 5% which is 25% of the baseline probability for this group. Thus, these corrected results confirm our earlier finding, although they suggest effects of the mandates which may be implausibly large.

As a further specification check, we restricted the regression to the period before 1987, in order to focus only on the state mandates, which may be more comparable to each other than to the Federal law. We also tried including in the regression not only state effects but state-specific trend terms; that is, we interact each state effect with a trend for the ten year period. In this way, we control for within-state trends in retirement which may be correlated with the propensity of legislatures to mandate continuation coverage.[28] In both cases, the estimated coefficients on *Months of Coverage* are similar to that in the basic specification, although the standard errors rise. When the estimation is restricted only to those with health insurance, both coefficients using retirement as the dependent variable are significant at the 10% level, while the estimates for leaving the labor force are significant at the 6% and 12% levels respectively. In the MORG data, when we include state-specific trends and restrict the sample period to the years before COBRA, the coefficients on *Bridge to Medicare* and on the older age interactions remain sizeable and highly significant.

Thus, to summarize the CPS results, we find a very large effect of continuation coverage availability on retirement propensities. These effects appear to be roughly uniform over ages 55-64, countering the intuition that they will only act as a "bridge to Medicare". To place these findings in perspective, it is useful to contrast them to the estimated effects of Social Security on retirement decisions. Samwick (1993) reports that a $1000 increment to Social Security wealth increases retirement propensities by 0.081%. In our basic specification above, we find that one year of continuation coverage raises the retirement probability by 1.4%, implying that one year of coverage is worth approximately $17,000 in Social Security wealth. Based on the cost information reported in Section II, a COBRA policy would save an older worker approximately $4,500 per year on the price of family coverage. Taken at face value, our results therefore suggest that workers value the insurance received from continuation policies at a much higher level than its associated cost savings. This may reflect the fact that the individual policy we priced, as with most individual

[28] This type of "random growth" or "fixed trend" estimator is suggested by Heckman and Hotz (1988) and is used by Jacobson, Lalonde, and Sullivan (1992).

policies, excluded pre-existing conditions for some period; this substantially raises the relative value of a continuation policy which does not exclude these conditions. Alternatively, it may be that a number of early retirees must pay substantially more for individual policies or are unable to obtain such policies at all.[29]

VI. SIPP Results

We turn now to a dynamic model of retirement using data from the Survey of Income and Program Participation. As with the CPS data, we restrict our sample to men aged 55-64. Individuals are included in the sample upon turning 55 and are censored upon reaching their 65th birthday. We exclude individuals who are in the sample for only one wave, individuals already retired when we first observe them, and those who report being self-employed or for whom data is missing. For previously cited reasons, we exclude individuals living in West Virginia and Hawaii. We also drop individuals from several other small states because, out of concern for confidentiality, the SIPP has grouped these states together thereby making it impossible to assign the appropriate state laws to individuals in these states.[30]

The SIPP does not ask individuals directly whether they have retired. We therefore use a measure of retirement based on length of time out of the labor force. This has the advantage, relative to point in time self-reported measures, of capturing transitions to non-work rather than partial (but perceived) retirement. It has the disadvantage, however, of not

[29] An obvious test of this proposition is to interact the presence of these laws with some measure of health status. We plan to do so in future work using data on health status in the SIPP.

[30] These states are Alaska, Idaho, Iowa, Maine, Mississippi, Montana, New Mexico, North Dakota, South Dakota, Vermont and Wyoming. The CPS results are similar if we restrict our CPS sample in the same fashion.

allowing us to disentangle retirement from other reasons for a temporary absence from the labor force. Following Rogowski and Karoly (1992), we define retirement as a departure from the labor force of 5 or more months.[31] Individuals who are not in the labor force for at least the first four months for which we observe them are excluded from the sample, and individuals who report being out of the labor force in the last 5 months of the panel are censored at the last month for which they are in the labor force.

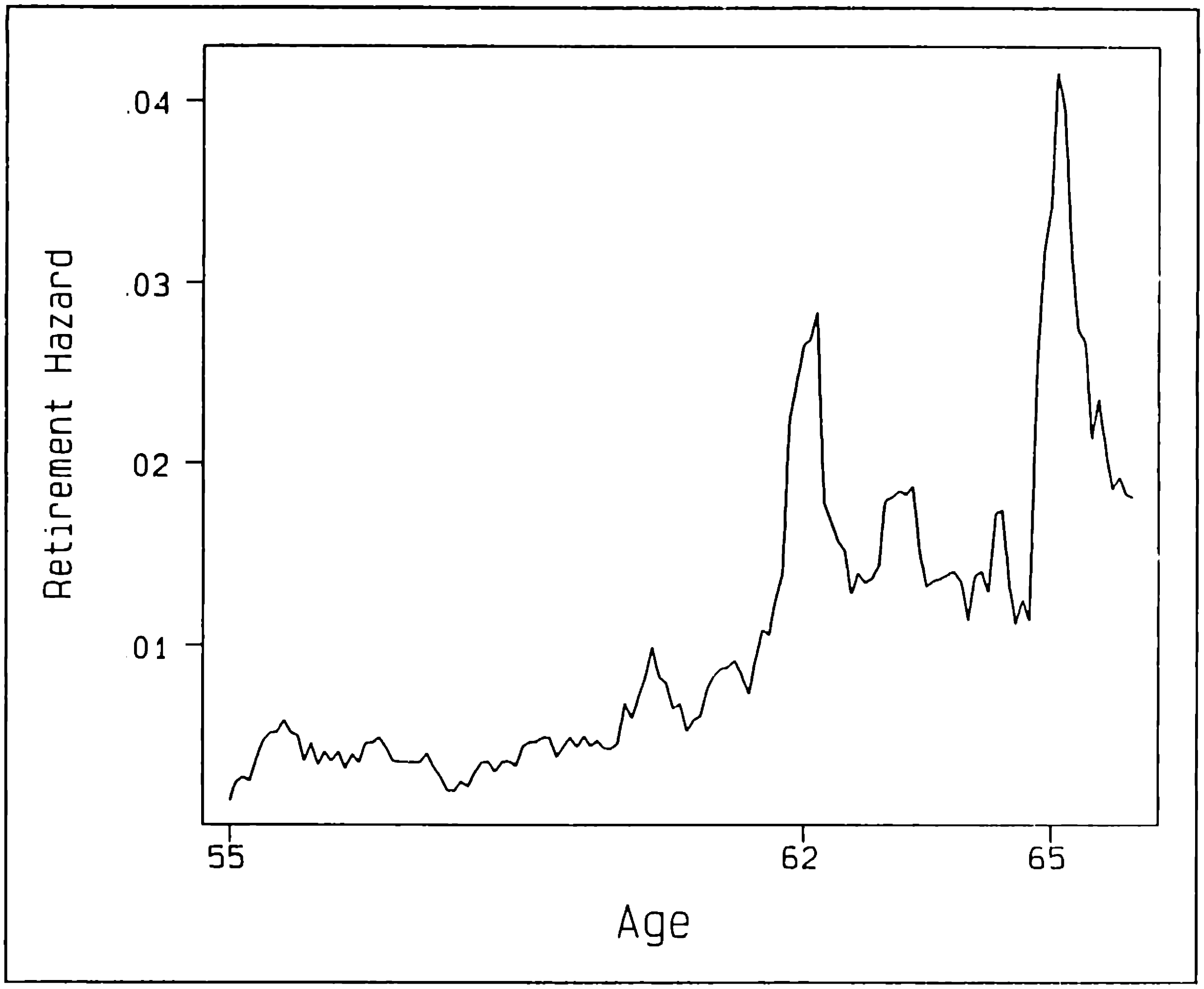
This definition of retirement helps alleviate the problem of measurement error in the reporting of individual labor force status. As Klerman and Rahman (1992) note, there is some tendency in the SIPP for respondents to "propagate their current status back through the entire reference period for the current interview," a phenomenon which has been termed "seam bias". A retirement window of longer than 4 months requires that an individual be out of the labor force for at least part of two consecutive waves before being counted as retired. Our 5-month retirement rule therefore reduces the likelihood that individuals reported as being out of the labor force for a full wave were really working for part of that period.

Table 17 shows, among those aged 55-64 in the SIPP, how many are excluded from our sample and, among those who are included, how many retire and how many are censored. About one-third of those aged 55-64 are excluded because they have already retired, and another third are excluded for other reasons, primarily because they were either self-employed or because they were only in the SIPP for one wave. The sample we actually use, about 3700 individuals, is much smaller than either of the CPS samples. Of these

[31] Rogowski and Karoly (1992) actually impose a 6-month rule for departure from the labor force. It turns out that almost all of the individuals who are out of the labor force for 5 months are actually out for 6 or more months.

individuals, about 15% retire. Figure 3 plots a 5-month moving average of the empirical retirement hazard for our sample. It looks very similar to retirement hazards based on the Retirement History Survey or labor force participation rates from the Current Population Survey and shows the characteristic peaks at age 62 and 65 usually attributed to Social Security eligibility (Hurd 1990).

FIGURE 3: Retirement Hazard in the SIPP (5-Month Moving Average)



As with other longitudinal data sets, attrition is a problem in the SIPP. Table 18 shows the distribution of participation time by panel. Only 50-65% of the individuals in each panel are included in all possible waves. The remaining 35-50% either entered the panel late, left early (attrition), or both. Note that the fraction of individuals included in 7 or more waves increases for successive panels suggesting that the problem of attrition has been getting less severe as the SIPP has matured. We treat individuals who leave the panel prematurely as censored observations. As long as the reasons for leaving the sample are not related to the factors that determine retirement, this type of censoring will not bias our results[32].

We use the SIPP to estimate a hazard model of retirement. This type of retirement model has been used previously by Diamond and Hausman (1984) and Hausman and Wise (1985). The retirement hazard, $\theta_i(t|\mathbf{x}_i)$, gives the probability that individual i retires in period t conditional on not having already retired:

$$\theta_i\,(t\,|\mathbf{x}_i) = \frac{f(t\,|\mathbf{x}_i)}{1 - F(t\,|\mathbf{x}_i)}\ . \tag{2}$$

We specify a proportional hazards model of retirement,

$$\theta_i\,(t\,|\mathbf{x}_i) = \theta_0(t)\,\exp\{\mathbf{x}_i'\beta\}\,, \tag{3}$$

in which age in months, t, is the relevant duration. $\theta_0(t)$, the baseline hazard common to all individuals at time t, is estimated nonparametrically, while the estimates of β are obtained by maximizing the partial likelihood function

[32] It is possible that attrition could be correlated with the retirement decision. For example, individuals who retire and move to Florida at the same time may be more likely to leave the sample. This would be a type of endogenous censoring. We are neither aware of the extent of this problem nor are able to sign the resulting bias.

$$L(\beta) = \prod_{h=1}^{k} \left[\frac{\exp\{x_h'\beta\}}{\sum_{l \in R(t)} \exp\{x_l'\beta\}} \right] \tag{4}$$

where there are k observed exit times and $R(t)$ is the set of all individuals at risk of retiring at time t (Kalbfleisch and Prentice 1980).

As in the CPS, the set of covariates which affect the hazard include marital status, race, education, industry, occupation, and whether or not an individual has health insurance on the job. Means for some of these variables are shown in Table 10. The educational attainment and marital status of this sample are quite similar to the CPS sample, although individuals in the SIPP are more likely to be non-white and more likely to have employer-provided health insurance in their own name.

Once again, we include two variables intended to capture the effect of continuation coverage on retirement. The first is simply the maximum months of coverage, from the state and federal laws, for which an individual is eligible. This varies by state and may vary over time for the same individual as state laws begin to take effect or are revised and as the federal law takes effect. As before, we phase-in the federal law in 12 equal increments between July 1986 and June 1987. The second variable, intended to measure whether or not continuation coverage acts solely as a bridge to Medicare, equals 0 until an individual reaches the age at which the maximum months of continuation coverage for which he is eligible would keep him completely covered by private health insurance until age 65. If the federal law is in effect, this variable will equal 1 after an individual turns 63½. Because we have monthly observations and know the month and year of an individual's birth, this variable contains much less error in the SIPP than the bridge to Medicare variable we used in the CPS.

Table 19 shows the coefficient estimates and corresponding hazard ratios from our basic hazard specification using data from the SIPP (the hazard ratio gives the effect on the hazard of a one unit change in the corresponding variable[33]). Marital status is an insignificant determinant in the retirement decision. Being non-white lowers the retirement hazard by 18%, and an additional year of school decreases the hazard by 2.5%. In contrast to our results from the CPS, health insurance increases the hazard, although the effect is completely insignificant. We also include a seam variable which equals one for the last reference month of any wave to allow for the potential effect of seam bias. There does indeed appear to be a tendency to propagate responses as the retirement hazard in seam months is over six times that in non-seam months.

The top panel of Table 19 also includes the months of continuation coverage for which an individual is eligible. The coefficient is positive and significant and implies that a year of continuation coverage raises the retirement hazard by approximately 15%. It is interesting to note that this is almost exactly the same coefficient we obtained using the March CPS data, despite the differences in the data sets and the modelling strategy. In the second column, which adds state effects to the hazard, the estimated effect of continuation coverage rises; one year of coverage implies a 22% increase in the hazard.

The bottom panel of Table 19 looks at whether continuation coverage is being used as a bridge to Medicare. The coefficient is actually the "wrong" sign (negative), although it is insignificant. This result also concurs with our results from the CPS, namely that the effect of continuation coverage is a general increase in the retirement propensities at all ages rather than an increase in the likelihood of retirement only among those who would then not

[33] If the estimated coefficient is β, the hazard ratio is e^{β}.

face a period without employer-provided health insurance by retiring before age 65. Although not reported, when both *Months of Coverage* and *Bridge to Medicare* are included, the results are not markedly different than when they are included separately.

Table 20, which does not include state dummies, allows the effect of continuation coverage to vary by age. In the first column, which uses the full sample, the coefficients are actually *decreasing* with age. Furthermore, the effects are negative, although insignificant, after age 62. In contrast to our March CPS results, we can reject the hypothesis that the coefficients are equal at all ages[34]. As a specification check, the second and third columns of Table 20 look separately at those who have employer-provided health insurance and those who do not. For the sample of those who have health insurance, the results are very similar to those for the full sample, although the magnitude of the coefficients is slightly larger. In five of the 10 cases, the coefficients are significant at the 10% level. For those without health insurance, however, the effects are generally negative and insignificant (the notable exception is age 61 which is both positive and significant). This result concurs with what we found in the CPS, namely that the effect of continuation coverage is confined to those with health insurance, a result we would expect.

We include state dummies in Table 21. Unfortunately, the sample of those without health insurance is too small to yield reliable estimate when state dummies are included so we present results only for the full sample and for those with health insurance. The state

[34] The likelihood ratio test-statistic equals 27.2 while the 95% critical value for a χ^2 with nine degrees of freedom is 16.9.

effects are jointly significant at the 10% level.[35] They have a small positive impact on the coefficients at each age.

Table 22 presents results analogous to those in Table 21 using the corrected months of coverage variable described earlier. As in the CPS, the coefficients are substantially larger when the months of coverage available is corrected to account for differences across states in firm-size, the rate of self-insurance, and the availability of retiree health insurance. The pattern of declining effects with age is, if anything, more pronounced with the corrected results.

The next two tables address several difficulties with our basic model. First, following our CPS methodology, we have assigned all persons in each state the number of months of continuation coverage mandated by that state (or the federal government). In reality, individuals cease to be eligible for continuation benefits once they are covered by Medicare. Thus, the effect of these regulations will vary with individiuals' ages as well as their state of residence and across time. In the first column of Table 23, we therefore set the months of coverage equal to the minimum of either the months of coverage available or the months until an individual turns 65 (which we can measure precisely since we have month and year of birth). So, if an individual is 64½ and the federal law is in effect, we assign to that individual 6 months of coverage rather than 18 because the individual will be eligible for Medicare in 6 months. This, obviously, has little effect on the coefficients at younger ages; at older ages, it makes them even more negative, although they remain insignificant. Thus,

[35] The likelihood ratio statistic for this test is 40.9 while the 95% critical value for a χ^2 with 30 degrees of freedom is 43.8 and the 90% critical is 40.3.

the age-specific pattern of these effects cannot explain our anomalous findings for the oldest workers.

The censoring of individuals who report being out of the labor force for less than five months at the end of the panel is somewhat problematic because the censoring is potentially endogenous to the retirement decision. That is, if these individuals are actually retiring, but we censor them because we cannot observe them for a sufficient length of time to definitively determine their status, then our censoring of them is correlated with the likelihood that they actually retired at the end of the panel. The natural solution to this problem, which we adopt in the second column of Table 23, is to censor all persons after the sixth-to-the-last month of the panel. Because everyone is treated equally, this censoring is exogenous and should not lead to a bias in the estimated parameters. We undertake this exogenous censoring in the second panel of Table 23. This doubles our estimated effect of the law, so that one year of coverage is found to increase the hazard rate by 33.6%; the effect also rises at every age. We are unclear as to why this correction has such a large effect on our findings, and are currently investigating the econometric properties of this "exogenous censoring" estimator. In the last column of Table 23, we combine these two corrections; the results are similar to those in the second column.

Finally, we return to the previously mentioned problem of dynamic sample selection. If those who are most likely to be affected by continuation mandates have already retired before we observe them and are therefore excluded from the estimation, our results could be biased. Diamond and Hausman (1984) suggest that a way to gauge the potential magnitude of this problem is to artificially left-censor the data and compare the coefficient estimates to those obtained without this censoring. In their work, they find that doing so radically alters the estimated effects of Social Security and health status on retirement, with some coefficients changing by a factor of 3. In Table 24 we compare the results from our basic specification (the first column of Table 20) with those we get when we left-censor our data before the seventh month at which we observe individuals. When *Months of Coverage* is constrained to be the same at all ages (Model 1), this left-censoring has little effect. When allowed to vary by age, the coefficients change slightly, with some increasing and some decreasing. In general, however, the left-censoring does not significantly alter the estimated coefficients, suggesting that dynamic sample selection is not a severe problem in our data.

Overall, the results from the SIPP generally accord with those from the CPS. The availability of continuation coverage increases the likelihood of early retirement and this effect is significant at most ages. While in the CPS we cannot reject the hypothesis that the effects are equal at all ages, this clearly is not true in the SIPP. Surprisingly, however, the reason for this rejection is that the effects weaken with age, and are insignificant above age 62. The dramatic dropoff in the effects from age 61 to age 62 suggests that there is potentially an interesting interaction with the availability of social security benefits. This would be consistent with either the liquidity constraints model or the model where the individuals who retire before age 62 are the most sensitive to financial incentives.

VII. Continuation Mandates and Insurance Coverage Among the Retired

An interesting corollary to the question of whether the availability of continuation coverage affects retirement is whether such coverage actually increases the number of insured early retirees in the population. Table 25 addresses this question. Using the sample of individuals who actually retire in the SIPP, we model the probability of being covered by employer-provided health insurance after retirement as a function of the months of continuation coverage available at the time of retirement. We find that an extra month of continuation coverage increases the probability of being insured after retirement by .5%; this implies that a year of coverage would increase the probability of being insured by 6%, while 18 months would increase the probability of coverage by 9%, a result consistent with that found by Rogowski and Karoly (1992). The results are slightly stronger if we restrict the sample to only those who were actually covered by employer-provided health insurance before retirement; the sample without health insurance before retirement is too small to yield a reliable estimate.

The results of Table 25 corroborate the evidence on take-up rates presented in Section II. If the take-up rates are about 75% among the 27% of the population who work in firms that offer health insurance but do not offer retiree health insurance, this would imply a 20% increase in coverage if continuation coverage lasted until age 65. However, COBRA expires after 18 months, so that the two-thirds of retirees who have been retired for longer than 18 months will no longer be eligible. One-third of 20% implies an increase in employer-provided health insurance coverage of 6.75%, which is somewhat below our estimate of 9% in Table 24.

Furthermore, we can reconcile this finding with our estimates of the effect of continuation mandates on retirement from the CPS and SIPP data. Our basic model in both data sets implied that one year of coverage raised retirement probabilities by 15-20%. The insurance coverage regression implies that one year of continuation benefits raises the likelihood of coverage by 6%.

These numbers cannot be directly compared, since some of those who have retired will have exceeded their months of continuation eligibility; thus, even if they retired in response to continuation availability, we won't see them being covered. However, given our relatively short window and the fact that most of our retirement spells happen after COBRA, this is not likely to be a very large problem. Thus, these insurance coverage results pose a bit of a mystery. It is unclear why the level of insurance coverage is rising by so much less than the propensity to retire if retirement is occurring in response to the insurance incentives.

VIII. Conclusion

A number of current public policy proposals in the U.S. would affect the health insurance coverage of early retirees. Thus, it seems especially important at this time to understand the interaction between insurance coverage and the retirement decision. Our strategy for doing so has been to examine the effect of state and federal continuation of coverage mandates on retirement propensities. This has the natural advantage that such coverage was assigned exogenously to workers in different states at different times, allowing for substantial variation with which to estimate these effects.

Our primary finding is that continuation mandates appear to have significantly increased retirement propensities. This result emerges from two different data sets and is robust to a variety of specification checks. Surprisingly, we find that this effect is not greater at older ages; if anything, it is smaller. This counterintuitive result is, however, consistent with a number of hypotheses which we posed in Section III. Disentangling these hypotheses is an important task for future research on this question.

The effects of complete retiree coverage may differ significantly from the effects of continuation benefits studied here. Future work in this area could fruitfully focus on the role of employer-provided retiree coverage. In particular, our findings suggest the value of focusing on the age-specific pattern of any uncovered effects of retiree insurance. An important problem to be addressed in this line of work, however, must be the potential endogeneity of this employer-provided benefit to unobserved characteristics of both workers and workplaces.

Taken at face value, our findings have important implications for public policy. They suggest that increasing the age of full Social Security benefit eligibility to 67 may not have a significant effect on retirement ages unless the age of Medicare eligibility is changed as well. Furthermore, these findings imply that policies to provide health insurance to all citizens in the U.S. could lead to a large increase in the rate of early retirement. This factor should be accounted for in considering the revenue impacts of such policies.

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APPENDIX

Calculating Correction Factors for the Impact of State and Federal Continuation Laws

In our basic regression specification, we assign to individuals the maximum number of months of continuation coverage mandated under either federal or state law. There are several factors, however, that lead to less than full coverage of these laws. First, those who are not covered by employer-provided health insurance should not be affected by any of these laws. Similarly, workers in firms that offer retiree health insurance should also be unaffected. Before the federal law took effect, employees of self-insured firms should not have been affected by the state laws because, under the 1974 Employee Retirement Income and Security Act (ERISA), self-insured firms are exempt from state mandates. And last, those who work in firms with fewer than 20 employees will not be affected by the federal law, which does not apply to small firms, although they will be influenced by state laws which pertain to all firms.

To the extent that these factors differ across states, we would expect laws which mandate equivalent months of coverage to have different effects. To account for the less than full coverage of these laws, we compute a "corrected" measure of months of coverage. As before, if there is no state or federal law in effect, we assign no months of coverage to an individual. If a state law is operative but the federal law has not yet taken effect, the corrected months of coverage equals

$$\begin{bmatrix} \textit{Months} \\ \textit{of State} \\ \textit{Coverage} \end{bmatrix} \cdot \left[\begin{bmatrix} \textit{Fraction} \\ \textit{Offered} \\ \textit{Insurance} \end{bmatrix} \cdot \begin{bmatrix} 1 - \begin{matrix} \textit{Fraction with} \\ \textit{Retiree HI or in} \\ \textit{Self-Insured Firm} \end{matrix} \end{bmatrix} \right] .$$

If the federal law is in effect but there is no state law, the corrected months of coverage equals

$$\begin{bmatrix} \textit{Months} \\ \textit{of Federal} \\ \textit{Coverage} \end{bmatrix} \cdot \left[\begin{bmatrix} \textit{Fraction} \\ \textit{Offered} \\ \textit{Insurance} \end{bmatrix} \cdot \begin{bmatrix} & \textit{Fraction with} \\ 1 - & \textit{Retiree HI and} \\ & \textit{in} > 20 \textit{ Firm} \end{bmatrix} \cdot \begin{bmatrix} & \textit{Fraction} \\ 1 - & \textit{Insured in} \\ & < 20 \textit{ Firm} \end{bmatrix} \right] .$$

Finally, if both a state and the federal law are in place, the corrected months of coverage equals

$$\begin{bmatrix} \textit{Months} \\ \textit{of Federal} \\ \textit{Coverage} \end{bmatrix} \cdot \left[\begin{bmatrix} \textit{Fraction} \\ \textit{Offered} \\ \textit{Insurance} \end{bmatrix} \cdot \begin{bmatrix} & \textit{Fraction with} \\ 1 - & \textit{Retiree HI and} \\ & \textit{in} > 20 \textit{ Firm} \end{bmatrix} \cdot \begin{bmatrix} & \textit{Fraction} \\ 1 - & \textit{Insured in} \\ & < 20 \textit{ Firm} \end{bmatrix} \right] +$$

$$\begin{bmatrix} \textit{Months} \\ \textit{of State} \\ \textit{Coverage} \end{bmatrix} \cdot \left[\begin{bmatrix} \textit{Fraction} \\ \textit{Offered} \\ \textit{Insurance} \end{bmatrix} \cdot \begin{bmatrix} & \textit{Fraction with} \\ 1 - & \textit{Retiree HI or in} \\ & \textit{Self-Insured Firm} \end{bmatrix} \cdot \begin{bmatrix} \textit{Fraction} \\ \textit{Insured in} \\ < 20 \textit{ Firm} \end{bmatrix} \right] .$$

We measure the various components of the corrected months of coverage using data from the Health Insurance Association of America (HIAA) 1989 employer survey and the May 1988 Current Population Survey (CPS) pension supplement. In using data from this late date, we are assuming that these factors are constant over time. This is clearly not true for self-insurance which grew dramatically during the 1980s. As long as self-insurance did not grow in a way correlated with the passage of these laws, however, this will not bias our results. Recent research suggests that mandates play little role in the firm's decision to self-insure.

The fraction of workers offered employer-provided health insurance and the fraction of insured workers in firms with less than 20 employees are measured directly, by state, from the CPS. The fraction with retiree health insurance and in self-insured firms are estimated from the HIAA data. In using the HIAA data, we have tried two different approaches. The first is to calculate the average of these quantities by state. This strategy, however, is problematic, as the state cell sizes in the HIAA data are quite small. Our second approach, therefore, is to use the HIAA data to run a regression predicting the rate of self-insurance and the fraction covered by retiree health insurance as a function of firm size, industry, and census division. We then use these estimated coefficients to impute values of these quantities for each individual in the May CPS. Our correction factors are state-wide averages of these imputed values.

We did this imputation for two different populations: all male workers, and all male workers over 45 years old. The former yields larger cell sizes in each state, while the latter is closer to the population under study. The correction factors calculated based on these two sample were very highly correlated, so we used only the latter for this analysis.

In our empirical work, for both the CPS and the SIPP, we use the corrected months of coverage computed as outlined above when the sample includes all workers. When the sample is restricted to only those with health insurance, we use a similarly constructed corrected months of coverage which does not adjust for the fraction of workers in the state with employer-provided health insurance.

TABLE 1

Self-Reported Health Status by Age

	Health Status			
Age	Excellent	Good	Fair	Poor
25-34	36.4%	53.1%	9.5%	1.1%
35-44	32.0	54.6	11.9	1.5
45-54	27.8	52.5	15.6	4.1
55-64	18.0	50.7	24.9	6.4
65+	9.3	43.1	36.1	11.4

Source: Authors' calculations using data from the 1987 National Medical Expenditure Survey. The numbers in the table give the fraction of individuals who report having the given health status.

TABLE 2

Incidence of Health Problems by Age

Condition	Age				
	25-34	35-44	45-54	55-64	65+
Stroke	0.4%	0.8%	1.6%	3.6%	7.4%
Cancer	1.6	2.4	4.7	9.7	13.3
Heart Attack	0.3	1.1	3.8	7.7	13.3
Gallbladder disease	1.6	3.6	7.3	9.4	14.6
High blood pressure	10.1	18.2	29.1	41.9	49.8
Arteriosclerosis	0.2	0.6	2.8	6.1	16.3
Rheumatism	0.8	1.6	5.2	8.2	16.4
Emphysema	0.4	1.0	2.6	5.2	8.0
Arthritis	5.1	11.6	24.9	41.2	54.9
Diabetes	1.7	3.0	5.7	9.8	14.7
Heart disease	0.8	2.2	6.1	11.9	22.2
Any of the above	18.2	31.7	51.8	72.3	84.2

Source: Authors' calculations using data from the 1987 National Medical Expenditure Survey. The numbers in the table give the fraction of individuals who report ever having had the listed medical condition.

TABLE 3

Annual Medical Care Utilization by Age

	Age				
	25-34	35-44	45-54	55-64	65+
A. Fraction admitted to hospital	9.2%	6.8%	8.7%	11.0%	20.1%
Number of admissions (if ever admitted)	1.17	1.24	1.39	1.5	1.5
Nights in hospital (if ever admitted)	5.5	6.8	9.3	11.8	13.8
B. Fraction with prescribed medicines	52.9%	55.6%	61.1%	71.1%	81.9%
Number of prescribed medicines (if any prescribed medicines)	5.2	6.6	11.5	14.7	18.5
C. Fraction who visited a doctor	64.1%	67.1%	71.1%	77.9%	85.8%
Number of doctor visits (if visited a doctor)	4.6	4.6	5.5	6.0	7.4

Source: Authors' calculations using data from the 1987 National Medical Expenditure Survey.

TABLE 4

Average Annual Medical Expenditures by Age ($1980)

	Age					
	25-34	34-44	45-54	55-64	65-74	75+
A. Average Expenditures						
Hospital/Inpatient	$794 ($3763)	$744 ($3186)	$894 ($36[illegible]8)	$1526 ($6211)	$2142 ($6567)	$3700 ($10,811)
Physician/Outpatient	$334 ($716)	$330 ($653)	$391 ($890)	$473 ($1176)	$543 ($1582)	$560 ($916)
Prescription Medication	$477 ($98)	$65 ($154)	$111 ($208)	$163 ($299)	$195 ($271)	$221 ($276)
Total	$1176 ($4025)	$1135 ($3537)	$1395 ($4001)	$2144 ($6532)	$2877 ($7070)	$4481 ($11,045)
B. Average Expenditure if Expenditure > 0						
Hospital/Inpatient	$2103 ($5900)	$2350 ($5323)	$2289 ($5557)	$3945 ($9502)	$4747 ($9151)	($7482) ($14,218)
Physician/Outpatient	$467 ($807)	$458 ($731)	$543 ($1002)	$592 ($1287)	$668 ($1732)	($662) ($959)
Prescription Medication	$80 ($117)	$111 ($189)	$178 ($243)	$230 ($332)	$258 ($284)	($269) ($282)
Total	$1454 ($4431)	$1428 ($3913)	$1699 ($4357)	$2461 ($6944)	$3270 ($7450)	($4820) ($11,383)

Source: Authors' calculation using data from the 1980 National Medical Care Utilization and Expenditure Survey (inflated to $1990 using the Medical Care Component of the Consumer Price Index). Standard deviation of expenditures is given in parentheses.

TABLE 5

Insurance Coverage by Age and Employment Status

	Employment-Based		Other	Nongroup	CHAMPUS/	Medicare/	Uninsured
	Any	Own Name	Group		CHAMPVA	Medicaid	
A. All Individuals							
25-54	71.6%	51.1%	1.2%	5.9%	5.7%	5.6	15.4%
55-64	64.5	44.8	4.1	14.5	7.7	10.4	12.0
B. Employed							
25-54	78.5%	62.7%	1.1%	5.8%	4.9%	1.2	13.5%
55-64	76.3	63.1	4.0	12.6	6.8	0.8	10.1
C. Not Employed							
25-54	44.2%	4.2%	1.3%	6.2%	8.8%	23.4	23.0%
55-64	51.6	24.7	4.3	16.6	9.2	20.9	14.1

Source: Authors' calculations using data from the 1987 National Medical Expenditure Survey.

TABLE 6

Group and Nongroup Health Insurance Benefits

	Fraction of Individuals with Specified Benefit	
	Group Plans	Nongroup Plans
A. Primary benefits		
Major medical coverage	86.9%	39.1%
Hospital room and board	98.4	91.4
Surgery	97.6	91.6
Physician office visit	87.9	40.4
B. Other benefits		
Ambulance	89.0	54.0
Outpatient diagnostic services	95.9	66.0
Prescribed medicines	87.3	30.3
Mental health	92.2	66.0
C. Generosity of benefits (conditional on having benefit)		
Major medical deductible < $100	94.3	61.6
Full semi-private room charge	77.8	38.2
80-100% of UCR surgical charge	70.6	60.0
80-100% of UCR physician charge	91.8	81.3

Source: Farley (1986), Tables 45-58.

TABLE 7

State Continuation of Coverage Laws

State	Effective Date	Months of Coverage	State	Effective Date	Months of Coverage
Arkansas	7/20/79	4	North Carolina	1/1/82	3
California	1/1/85	3	North Dakota	7/1/83	10
Colorado	7/1/86	3	New York	1/1/86	6
Connecticut	10/1/75	10	Oklahoma	1/1/76	1
	1/1/87	20			
Georgia	7/1/86	3	Oregon	1/1/82	6
Illinois	1/1/84	6	Rhode Island	/ /88	18
	8/23/85	9			
Iowa	7/1/87	9	South Carolina	1/1/79	2
				1/1/90	6
Kansas	1/1/78	6	South Dakota	7/1/84	3
				3/3/88	18
Kentucky	7/15/80	9	Tennessee	1/1/81	3
Minnesota	8/1/74	6	Texas	1/1/81	6
	3/19/83	12			
	6/1/87	18			
Missouri	9/28/85	9	Utah	7/1/86	2
Nevada	1/1/88	18	Vermont	5/14/86	6
New Hampshire	8/22/81	10	Virginia	4/17/86	3
New Mexico	7/1/83	6	Wisconsin	5/14/80	18

Sources: Hewitt (1985), Thompson Publishing Group (1992), and state statutes.

TABLE 8

Health Insurance Coverage Before and After COBRA

	All Individuals		Employed		Not Employed	
	25-54	55-64	25-54	55-64	25-54	55-64
I. Insurance Coverage in 1984						
Any private health insurance	82.1	83.7	89.1	92.5	60.1	74.1
Health insurance in own name						
Employment-based	52.1	47.4	66.7	68.9	5.9	23.6
Not employment-based	5.1	12.5	5.1	10.4	5.2	14.7
Covered as a dependent	24.2	23.4	16.8	12.8	47.7	35.0
II. Insurance Coverage in 1989						
Any private health insurance	82.4	84.3	88.6	92.1	57.3	74.9
Health insurance in own name						
Employment-based	54.7	49.2	66.4	68.1	7.1	26.6
Not employment-based	5.3	12.9	5.2	9.6	5.2	16.8
Covered as a dependent	22.0	21.8	16.4	14.4	43.7	30.6

Source: Authors' calculations using data from the Survey of Income and Program Participation, 1984 Wave 3 and 1987 Wave 7.

TABLE 9

Health Insurance Coverage Before and After COBRA

	States with Laws Before COBRA		States without Laws Before COBRA	
	25-54	60-64	25-54	60-64
I. Insurance Coverage in 1984				
Any private health insurance	88.9	90.8	83.0	86.3
Health insurance in own name				
Employment-based	57.8	57.8	51.7	48.0
Not employment-based	3.7	6.0	5.0	13.0
Covered as a dependent	27.2	23.7	25.7	24.9
II. Insurance Coverage in 1989				
Any private health insurance	88.3	87.3	82.6	86.1
Health insurance in own name				
Employment-based	55.8	53.4	54.1	51.1
Not employment-based	6.2	12.4	5.1	12.3
Covered as a dependent	25.9	20.6	22.6	22.4

Source: Authors' calculations using data from the Survey of Income and Program Participation, 1984 Wave 3 and 1987 Wave 7.

TABLE 10

Sample Means

	CPS MORG	CPS March	SIPP
Education	11.7	12.0	12.3
Non-white	.094	.077	.113
Married	.854	.878	.879
Employer health insurance	--	.696	.839
Employer pension	--	.552	--
Fraction Retired	.197	--	--
Fraction not in the labor force	.348	--	--
Fraction who retire	--	.073	.155
Fraction who leave the labor force	--	.093	--

Source: Authors' calculations using Current Population Survey and Survey of Income and Program Participation data.

TABLE 11

The Effect of Continuation Coverage on the Probability of Being Retired
(Current Population Survey MORG Data)

	Definition of Retired			
	Report Being Retired		Not in the Labor Force	
Independent Variable	Coefficient (st. error)	Marginal Prob. * 10^2	Coefficient (st. error)	Marginal Prob. * 10^2
Months of Coverage	.0036 (.0017)	.088	.0025 (.0015)	.081
Married	-.0154 (.0010)	-.373	-.0577 (.0009)	-1.872
Education	-.0655 (.0092)	-1.622	-.3427 (.0081)	-11.728
Non-white	-.1204 (.0121)	-2.823	.0918 (.0104)	3.053
55 Years Old	-1.205 (.0503)	-19.497	.1180 (.0443)	3.916
56 Years Old	-1.097 (.0502)	-18.533	.1935 (.0443)	6.458
57 Years Old	-1.016 (.0501)	-17.703	.2435 (.0443)	8.156
58 Years Old	-.9251 (.0499)	-16.686	.3157 (.0442)	10.627
59 Years Old	-.8115 (.0498)	-15.254	.4094 (.0442)	13.851
60 Years Old	-.6254 (.0496)	-12.540	.5302 (.0441)	18.038
61 Years Old	-.4903 (.0496)	-10.240	.6394 (.0442)	21.872
62 Years Old	-.0854 (.0494)	-2.025	.9977 (.0441)	34.405
63 Years Old	.1033 (.0494)	2.604	1.161 (.0442)	39.957
64 Years Old	.1938 (.0494)	5.037	1.262 (.0442)	43.243

The table gives estimates from a probit equation for whether or not an individual is retired using data from the 1980-1990 Merged Outgoing Rotation Groups of the CPS. The sample is comprised of 214 508 men aged 55-64. Coefficients for year, month and state dummies are not reported.

TABLE 12

The Age-Specific Effect of Continuation Coverage on the Probability of Being Retired
(Current Population Survey MORG Data)

Independent Variable	Definition of Retired: Report Being Retired — Coefficient (st. error)	Report Being Retired — Marginal Prob. * 10^2 [% of baseline]	Not in the Labor Force — Coefficient (st. error)	Not in the Labor Force — Marginal Prob. * 10^2 [% of baseline]
Model 1				
Months of Coverage	.0036 (.0017)	.088	.0025 (.0015)	.081
Model 2				
55*Months	.0028 (.0023)	.068 [1.09]	.0012 (.0019)	.039 [.202]
56*Months	.0013 (.0023)	.061 [.795]	.0022 (.0019)	.073 [.341]
57*Months	.0021 (.0022)	.051 [.571]	-.0005 (.0019)	.017 [.074]
58*Months	.0027 (.0022)	.066 [.629]	.0008 (.0019)	.026 [.103]
59*Months	.0046 (.0021)	.111 [.873]	.0019 (.0019)	.062 [.219]
60*Months	.0024 (.0021)	.059 [.347]	.0018 (.0018)	.057 [.175]
61*Months	.0020 (.0020)	.049 [.238]	.0021 (.0018)	.070 [.192]
62*Months	.0048 (.0020)	.117 [.345]	.0040 (.0018)	.133 [.264]
63*Months	.0041 (.0020)	.100 [.243]	.0045 (.0018)	.148 [.260]
64*Months	.0067 (.0020)	.164 [.366]	.0063 (.0018)	.207 [.339]
Model 3				
Bridge to Medicare	.0520 (.0149)	1.290	.0650 (.0147)	2.159

The table gives estimates from a probit equation for whether or not an individual is retired using data from the 1980-1990 Merged Outgoing Rotation Groups of the CPS. The sample is comprised of 214,508 men aged 55-64. Coefficients for year, month and state dummies are not reported.

TABLE 13

Comparing Stock Effects vs. Flow Effects

Age	Total Number	Fraction Retired		Difference: 15% Flow - 10% Flow	Difference/ 10% Flow
		10% Flow	15% Flow		
60	100	10.0%	15.0%	5.0%	50.0%
61	100	19.0%	27.7%	8.7%	46.1%
62	100	27.1%	38.6%	11.5%	42.4%
63	100	34.4%	47.8%	13.4%	39.0%
64	100	40.9%	55.6%	14.7%	35.8%

TABLE 14

The Effect of Continuation Coverage on Retirement Transitions
(March Current Population Survey Data)

	Definition of Retired			
	Report Being Retired		Not in the Labor Force	
Independent Variable	Coefficient (st. error)	Marginal Prob. *10^2	Coefficient (st. error)	Marginal Prob. * 10^2
Months of Coverage	.0093 (.0047)	.117	.0076 (.0043)	.116
Married	-.0035 (.0260)	-.044	-.1446 (.0225)	-2.354
Education	.0009 (.0030)	.011	-.0172 (.0027)	-.260
Non-white	-.1936 (.0363)	-2.166	-.0449 (.0303)	-.669
Health Insurance	-.1220 (.0233)	-1.568	-.2033 (.0211)	-3.247
Pension	.0621 (.0220)	.772	.0212 (.0203)	.323
55 Years Old	-1.581 (.1433)	-10.695	-.7798 (.1265)	-8.314
56 Years Old	-1.522 (.1431)	-10.431	-.7404 (.1267)	-8.016
57 Years Old	-1.511 (.1431)	-10.296	-.7140 (.1267)	-7.804
58 Years Old	-1.454 (.1433)	-10.157	-.6559 (.1269)	-7.386
59 Years Old	-1.338 (.1425)	-9.729	-.5652 (.1264)	-6..638
60 Years Old	-1.094 (.1419)	-8.751	-.3875 (.1261)	-4.948
61 Years Old	-.9772 (.1421)	-8.037	-.3015 (.1264)	-3.991
62 Years Old	-.4638 (.1414)	-4.656	.1497 (.1259)	2.455
63 Years Old	-.4648 (.1414)	-4.600	.1128 (.1261)	1.820
64 Years Old	-.4602 (.1418)	-4.517	.1175 (.1264)	1.904

This table gives estimates from a probit equation for whether or not an individual retired in the last year using data from the 1980-1990 March Current Population Surveys. The sample is comprised of 56,180 men aged 55-64. Coefficients for year, state, industry, and occupation dummies are not reported.

TABLE 15A

The Age-Specific Effect of Continuation Coverage on Retirement Transitions
(March Current Population Survey Data)

	Definition of Retired: Report Being Retired					
	Full Sample		Employer HI Only		No Employer HI Only	
Independent Variables	Coefficient (st. error)	Marginal Prob. $*10^2$	Coefficient (st. error)	Marginal Prob. $*10^2$	Coefficient (st. error)	Marginal Prob. $*10^2$
Model 1						
Months of Coverage	.0093 (.0047)	.117	.0143 (.0057)	.169	-.0028 (.0083)	-.035
Model 2						
55*Months	.0061 (.0220)	.076	.0089 (.0072)	.105	-.0018 (.0113)	-.023
56*Months	.0073 (.0060)	.090	.0132 (.0072)	.155	-.0087 (.0110)	-.111
57*Months	.0107 (.0060)	.134	.0156 (.0072)	.182	-.0030 (.0109)	-.038
58*Months	.0116 (.0059)	.144	.0170 (.0071)	.199	-.0032 (.0109)	-.041
59*Months	.0060 (.0057)	.075	.0138 (.0070)	.162	-.0122 (.0103)	-.156
60*Months	.0123 (.0055)	.151	.0166 (.0067)	.193	-.0006 (.0101)	-.008
61*Months	.0097 (.0054)	.120	.0133 (.0067)	.155	-.0007 (.0097)	-.009
62*Months	.0059 (.0052)	.073	.0107 (.0063)	.125	-.0050 (.0093)	-.064
63*Months	.0134 (.0053)	.167	.0167 (.0065)	.195	.0051 (.0093)	.066
64*Months	.0089 (.0054)	.110	.0181 (.0068)	.211	-.0048 (.0094)	-.062
Model 3						
Bridge to Medicare	.0529 (.0388)	.679	--	--	--	--

This table gives estimates from a probit equation for whether or not an individual retired in the last year using data from the 1980-1990 March Current Population Surveys. The sample is comprised of 56,180 men aged 55-64 of which 39,123 have employer-provided health insurance and 17,057 do not. Coefficients for year, state, industry and occupation dummies are not reported.

TABLE 15B

The Age-Specific Effect of Continuation Coverage on Retirement Transitions
(March Current Population Survey Data)

Independent Variables	Definition of Retired: Not in the Labor Force					
	Full Sample		Employer HI Only		No Employer HI Only	
	Coefficient (st. error)	Marginal Prob. *10²	Coefficient (st. error)	Marginal Prob. *10²	Coefficien t (st. error)	Marginal Prob. *10²
Model 1						
Months of Coverage	.0076 (.0043)	.116	.0129 (.0054)	.179	-.0028 (.0073)	-.045
Model 2						
55*Months	.0018 (.0054)	.027	.0059 (.0068)	.081	-.0072 (.0091)	-.118
56*Months	.0048 (.0053)	.073	.0124 (.0066)	.171	-.0097 (.0091)	-.159
57*Months	.0088 (.0053)	.135	.0134 (.0067)	.184	-.0005 (.0089)	-.009
58*Months	.0075 (.0052)	.114	.0139 (.0066)	.191	-.0035 (.0088)	-.058
59*Months	.0092 (.0052)	.140	.0165 (.0065)	.225	-.0054 (.0088)	-.088
60*Months	.0112 (.0050)	.171	.0138 (.0063)	.189	.0069 (.0087)	.113
61*Months	.0060 (.0050)	.090	.0111 (.0063)	.152	-.0051 (.0086)	-.084
62*Months	.0045 (.0049)	.068	.0096 (.0061)	.131	-.0050 (.0083)	-.082
63*Months	.0105 (.0050)	.160	.0161 (.0063)	.221	-.0010 (.0084)	-.016
64*Months	.0101 (.0051)	.154	.0174 (.0065)	.239	-.0006 (.0084)	-.001
Model 3						
Bridge to Medicare	.0594 (.0379)	.936	--	--	--	--

This table gives estimates from a probit equation for whether or not an individual retired in the last year using data from the 1980-1990 March Current Population Surveys. The sample is comprised of 56,180 men aged 55-64 of which 39,123 have employer-provided health insurance and 17,057 do not. Coefficients for year, state, industry and occupation dummies are not reported.

TABLE 16A

The Effect of Continuation Coverage on Retirement (Corrected)
(March Current Population Survey Data)

	Definition of Retired: Report Being Retired			
	Full Sample		Employer HI Only	
Independent Variables	Coefficient (st. error)	Marginal Prob. *10^2	Coefficient (st. error)	Marginal Prob. *10^2
Model 1				
Months of Coverage	.0238 (.0105)	.301	.0260 (.0112)	.310
Model 2				
55*Months	.0157 (.0146)	.196	.0097 (.0151)	.114
56*Months	.0176 (.0144)	.221	.0228 (.0148)	.269
57*Months	.0339 (.0143)	.428	.0338 (.0147)	.401
58*Months	.0353 (.0142)	.446	.0365 (.0146)	.433
59*Months	.0143 (.0140)	.179	.0255 (.0146)	.301
60*Months	.0292 (.0130)	.367	.0303 (.0135)	.357
61*Months	.0260 (.0130)	.326	.0231 (.0137)	.272
62*Months	.0132 (.0123)	.165	.0170 (.0129)	.199
63*Months	.0322 (.0126)	.406	.0303 (.0134)	.358
64*Months	.0216 (.0129)	.272	.0352 (.0139)	.417

This table gives estimates from a probit equation for whether or not an individual retired in the last year using data from the 1980-1990 March Current Population Surveys. The sample is comprised of 56,180 men aged 55-64 of which 39,123 have employer-provided health insurance. Coefficients for year, state, industry and occupation dummies are not reported. Months of coverage is "corrected" months as described in the text and the Appendix.

TABLE 16B

The Effect of Continuation Coverage on Retirement (Corrected)
(March Current Population Survey Data)

Independent Variables	Definition of Retired: Not in the Labor Force			
	Full Sample		Employer HI Only	
	Coefficient (st. error)	Marginal Prob. $*10^2$	Coefficient (st. error)	Marginal Prob. $*10^2$
Model 1				
Months of Coverage	.0165 (.0097)	.255	.0179 (.0106)	.249
Model 2				
55*Months	.0026 (.0129)	.039	-.0006 (.0142)	-.008
56*Months	.0100 (.0128)	.153	.0171 (.0137)	.236
57*Months	.0220 (.0128)	.339	.0217 (.0137)	.300
58*Months	.0236 (.0126)	.362	.0244 (.0136)	.338
59*Months	.0207 (.0125)	.317	.0262 (.0135)	.363
60*Months	.0242 (.0120)	.372	.0190 (.0128)	.261
61*Months	.0127 (.0121)	.194	.0146 (.0130)	.201
62*Months	.0053 (.0116)	..081	.0083 (.0124)	.115
63*Months	.0214 (.0119)	.329	.0238 (.0129)	.329
64*Months	.0242 (.0122)	.372	.0301 (.0134)	.417

This table gives estimates from a probit equation for whether or not an individual retired in the last year using data from the 1980-1990 March Current Population Surveys. The sample is comprised of 56,180 men aged 55-64 of which 39,123 have employer-provided health insurance. Coefficients for year, state, industry and occupation dummies are not reported. Months of coverage is "corrected" months as described in the text and the Appendix.

TABLE 17

SIPP Sample Composition

	Number of Individuals Age 55-64	Excluded from Sample		Included in Sample		
		Already Retired	Other Reasons	Total	Retire (%)	Censored (%)
1984 Panel	4325	1398	1448	1389	14.3%	85.7%
1985 Panel	2933	985	1051	897	15.6%	84.4%
1986 Panel	2325	788	838	699	15.5%	84.5%
1987 Panel	2381	855	808	718	17.4%	82.6%

Source: Authors' tabulations of data from the Survey of Income and Program Participation. The other reasons for exclusion from the sample include being in the survey for only 1 wave, being self-employed, and data inconsistencies which made it difficult to accurately determine an individual's age or retirement status.

TABLE 18

Distribution of Participation Time in the SIPP

Participation Time in the SIPP	SIPP Panel			
	1984	1985	1986	1987
1 Wave	5.2	5.8	6.5	6.5
2 Waves	4.7	6.8	7.6	6.2
3 Waves	6.2	14.8	6.3	6.0
4 Waves	11.5	8.2	6.7	5.6
5 Waves	11.2	5.1	5.7	5.6
6 Waves	4.8	4.3	5.9	4.6
7 Waves	5.4	4.1	61.5	65.4
8 Waves	2.7	51.7	--	--
9 Waves	48.5	--	--	--

Source: Authors' calculations. Participation time is the time spanned by the first and last wave in which an individual was included.

TABLE 19

The Effect of Continuation Coverage on the Retirement Hazard
(Survey of Income and Program Participation Data)

Explanatory Variables	No State Effects		State Effects	
	Coefficient (st. error)	Hazard Ratio	Coefficient (st. error)	Hazard Ratio
Model 1				
Months of Coverage	.0123 (.0062)	1.012	.0177 (.0066)	1.018
Married	-.0970 (.1321)	.908	-.1091 (.1334)	.897
Education	-.0253 (.0130)	.975	-.0256 (.0131)	.975
Non-white	-.2004 (.1502)	.818	-.2581 (.1526)	.773
Health Insurance	.1392 (.1323)	1.149	.1108 (.1336)	1.117
Seam Month	1.834 (.0929)	6.259	1.820 (.0930)	6.172
Model 2				
Bridge to Medicare	-.2653 (.2104)	1.304	-.2061 (.2142)	.814

The table shows estimates of a proportional hazard model of age at retirement for men aged 55-64 using data from the 1984, 1985, 1986 and 1987 Panels of the Survey of Income and Program Participation. Coefficients for industry, occupation, and month dummies are not reported.

TABLE 20

The Age-Specific Effect of Continuation Coverage on the Retirement Hazard
(Survey of Income and Program Participation Data)

	Full Sample		Have Health Insurance		No Health Insurance	
	Coefficient (st. error)	Hazard Ratio	Coefficient (st. error)	Hazard Ratio	Coefficient (st. error)	Hazard Ratio
55*Months	.0648 (.0233)	1.067	.0694 (.0260)	1.072	.0450 (.0548)	1.046
56*Months	.0271 (.0228)	1.027	.0488 (.0250)	1.050	-.0639 (.0654)	.938
57*Months	.0131 (.0254)	1.013	.0203 (.0259)	1.021	-.1349 (.1965)	.874
58*Months	.0522 (.0202)	1.054	.0461 (.0221)	1.047	.1023 (.0578)	1.108
59*Months	.0437 (.0196)	1.045	.0477 (.0201)	1.049	-.0077 (.0940)	.992
60*Months	.0021 (.0187)	1.002	.0070 (.0120)	1.007	-.0285 (.0551)	.972
61*Months	.0467 (.0136)	1.048	.0374 (.0154)	1.038	.1050 (.0333)	1.111
62*Months	-.0193 (.0135)	.981	-.0231 (.0143)	.977	.0117 (.0438)	1.012
63*Months	-.0014 (.0150)	.999	.0055 (.0156)	1.006	-.0596 (.0624)	.942
64*Months	-.0221 (.0167)	.978	-.0101 (.0178)	.990	-.0891 (.0550)	.915

The table shows estimates of a proportional hazard model of age at retirement for men aged 55-64 using data from the 1984, 1985, 1986 and 1987 Panels of the Survey of Income and Program Participation. Coefficients for industry, occupation, and month dummies are not reported.

TABLE 21

The Age-Specific Effect of Continuation Coverage on Retirement (State Effects)
(Survey of Income and Program Participation Data)

	Full Sample		Have Health Insurance	
	Coefficient (st. error)	Hazard Ratio	Coefficient (st. error)	Hazard Ratio
55*Months	.0687 (.0234)	1.071	.0722 (.0261)	1.075
56*Months	.0314 (.0229)	1.032	.0517 (.0251)	1.053
57*Months	.0167 (.0255)	1.017	.0220 (.0260)	1.022
58*Months	.0559 (.0204)	1.057	.0484 (.0222)	1.050
59*Months	.0480 (.0198)	1.049	.0505 (.0203)	1.052
60*Months	.0072 (.0188)	1.007	.0103 (.0200)	1.010
61*Months	.0512 (.0137)	1.053	.0407 (.0155)	1.052
62*Months	-.0125 (.0137)	.988	-.0168 (.0146)	.983
63*Months	.0026 (.0152)	1.003	.0097 (.0159)	1.010
64*Months	-.0153 (.0169)	.985	-.0049 (.0180)	.995

The table shows estimates of a proportional hazard model of age at retirement for men aged 55-64 using data from the 1984, 1985, 1986 and 1987 Panels of the Survey of Income and Program Participation. Coefficients for industry, occupation, state, and month dummies are not reported.

TABLE 22

The Effect of Continuation Coverage on Retirement (Corrected)
(Survey of Income and Program Participation Data)

	Full Sample		Have Health Insurance	
	Coefficient (st. error)	Hazard Ratio	Coefficient (st. error)	Hazard Ratio
55*Months	.1883 (.0570)	1.207	.2937 (.0658)	1.341
56*Months	.0652 (.0590)	1.067	.2297 (.0658)	1.258
57*Months	.0225 (.0673)	1.023	.1721 (.0662)	1.188
58*Months	.1200 (.0524)	1.127	.1196 (.0535)	1.127
59*Months	.1126 (.0499)	1.119	.1440 (.0501)	1.155
60*Months	.0031 (.0494)	1.003	.1149 (.0526)	1.122
61*Months	.1077 (.0356)	1.114	.1230 (.0431)	1.131
62*Months	-.0413 (.0358)	.960	.0225 (.0407)	1.023
63*Months	-.0129 (.0404)	.987	.0086 (.0448)	1.009
64*Months	-.0347 (.0443)	.966	-.0665 (.0541)	.936

The table shows estimates of a proportional hazard model of age at retirement for men aged 55-64 using data from the 1984, 1985, 1986 and 1987 Panels of the Survey of Income and Program Participation. Coefficients for industry, occupation, and month dummies are not reported.

TABLE 23

The Effect of Continuation Coverage on Retirement
(Survey of Income and Program Participation Data)

	Adjusted Months of Coverage		Exogenous Censoring		Adjusted Months/ Exogenous Censoring	
	Coefficient (st. error)	Hazard Ratio	Coefficient (st. error)	Hazard Ratio	Coefficient (st. error)	Hazard Ratio
Model 1						
Months of Coverage	.0148 (.0065)	1.015	.0276 (.0062)	1.028	.0310 (.0066)	1.031
Model 2						
55*Months	.0646 (.0233)	1.067	.0766 (.0241)	1.080	.0766 (.0241)	1.080
56*Months	.0270 (.0228)	1.027	.0463 (.0229)	1.047	.0463 (.0229)	1.047
57*Months	.0130 (.0254)	1.013	.0317 (.0254)	1.032	.0315 (.0254)	1.032
58*Months	.0521 (.0202)	1.053	.0684 (.0201)	1.071	.0682 (.0201)	1.071
59*Months	.0435 (.0196)	1.044	.0648 (.0197)	1.067	.0646 (.0120)	1.067
60*Months	.0018 (.0187)	1.002	.0155 (.0187)	1.016	.0152 (.0187)	1.015
61*Months	.0464 (.0136)	1.047	.0662 (.0137)	1.068	.0658 (.0137)	1.068
62*Months	-.0198 (.0135)	.980	-.0028 (.0135)	.997	-.0033 (.0135)	.997
63*Months	-.0046 (.0159)	.995	.0139 (.0149)	1.014	.0108 (.0158)	1.011
64*Months	-.0559 (.0338)	.946	-.0123 (.0167)	.988	-.0413 (.0333)	.960

The table shows estimates of a proportional hazard model of age at retirement for men aged 55-64 using data from the 1984, 1985, 1986 and 1987 Panels of the Survey of Income and Program Participation. Coefficients for industry, occupation, and month dummies are not reported.

TABLE 24

The Extent of Dynamic Sample Selection Bias in the SIPP

	Full Sample		Left-censored Sample	
	Coefficient (st. error)	Hazard Ratio	Coefficient (st. error)	Hazard Ratio
Model 1				
Months of Coverage	.0054 (.0066)	1.005	.0055 (.0066)	1.005
Model 2				
55*Months	.0648 (.0233)	1.067	.0876 (.0353)	1.092
56*Months	.0271 (.0228)	1.027	.0414 (.0255)	1.042
57*Months	.0131 (.0254)	1.013	.0366 (.0261)	1.037
58*Months	.0522 (.0202)	1.054	.0434 (.0210)	1.044
59*Months	.0437 (.0196)	1.045	.0352 (.0192)	1.036
60*Months	.0021 (.0187)	1.002	.0007 (.0198)	1.001
61*Months	.0467 (.0136)	1.048	.0372 (.0136)	1.038
62*Months	-.0193 (.0135)	.981	-.0251 (.0142)	.975
63*Months	-.0014 (.0150)	.999	-.0136 (.0156)	.986
64*Months	-.0221 (.0167)	.978	-.0273 (.0168)	.973

The table shows estimates of a proportional hazard model of age at retirement for men aged 55-64 using data from the 1984, 1985, 1986 and 1987 Panels of the Survey of Income and Program Participation. Coefficients for industry, occupation, and month dummies are not reported.

TABLE 25

Continuation Coverage and the Probability of Being Insured After Retirement

	Full Sample of Retirees		Retirees with Employer Health Insurance Before Retirement	
Independent Variable	Coefficient (st. error)	Marginal Probability	Coefficient (st. error)	Marginal Probability
Married	.0820 (.1764)	.026	.0460 (.2068)	.013
Black	-.8403 (.2227)	-.293	-.9563 (.2486)	-.326
Education	.0381 (.0122)	.012	.0386 (.0142)	.011
Age	.1788 (.0903)	-.020	.1343 (.1052)	-.019
Age^2	-.0001 (.00006)	--	-.0001 (.00007)	--
Months of Coverage	.0163 (.0084)	.005	.0201 (.0097)	.006
Log-likelihood	-288.72		-218.76	
N=Sample Size	524		441	

The table gives estimates of the probability of being insured after retirement using data from the 1984, 1985, 1986 and 1987 Panels of the Survey of Income and Program Participation. The sample is comprised of men aged 55-64 who retire over the sample period. Coefficients for industry and occupation dummies are not reported.