THREE ESSAYS ON HEALTH INSURANCE REGULATION AND THE LABOR MARKET

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By

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ABSTRACT

This dissertation continues the tradition of identifying the unintended consequences of the US health insurance system. Its main contribution is to estimate the size of the distortions caused by the employer-based system and regulations intended to fix it, while using methods that are more novel and appropriate than those of previous work.

Chapter 1 examines the effect of state-level health insurance mandates, which are regulations intended to expand access to health insurance. It finds that these regulations have the unintended consequence of increasing insurance premiums, and that these regulations have been responsible for 9-23% of premium increases since 1996. The main contribution of the chapter is that its results are more general than previous work, since it considers many more years of data, and it studies the employer-based plans that cover most Americans rather than the much less common individual plans.

Whereas Chapter 1 estimates the effect of the average mandate on premiums, Chapter 2 focuses on a specific mandate, one that requires insurers to cover prostate cancer screenings. The focus on a single mandate allows a broader and more careful analysis that demonstrates how health policies spill over to affect the labor market. I find that the mandate has a significant negative effect on the labor market outcomes of the very group it was intended to help. The mandate expands the treatments health insurance covers for men over age 50, but by doing so it makes them more expensive to insure and employ. Employers respond to this added expense by lowering wages and hiring fewer men over age 50. According to the theoretical model put forward in the chapter, this suggests the mandate reduces total welfare.

Chapter 3 shows that the employer-based health insurance system has deterred entrepreneurship. It takes advantage of the natural experiment provided by the Affordable Care Act's dependent coverage mandate, which de-linked insurance from employment for many 19-25 year olds. Difference-in-difference estimates show that the mandate increased self-employment among the treated group by 13-24%. Instrumental variables estimates show that those who actually received parental health insurance as a result of the mandate were drastically more likely to start their own business. This suggest that concerns over health insurance are a major barrier to entrepreneurship in the United States.

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CHAPTER 1

THE EFFECT OF HEALTH INSURANCE BENEFIT MANDATES ON PREMIUMS

This chapter examines the effects of laws mandating that health insurance cover specific conditions, procedures, providers, and beneficiaries. Unlike previous work, this chapter considers the market for employer-based health insurance rather than the much smaller individual market, and uses a panel data approach to account for unobserved heterogeneity among states. This means that relative to previous work, my estimates of the effects of mandates will be more accurate and will apply to a more relevant market. Using a fixed effects model, I find that the average mandate increases premiums by 0.44–1.11 percent annually. This implies that new mandates were responsible for 9–23 percent of all premium increases over the 1996–2011 period.

1.1 Introduction

Much work in health economics has tried to determine why health costs in the US have increased so rapidly in the last few decades. One under-explored mechanism for increasing costs is state-level health insurance regulation. In 1970, the average US state had less than one health insurance mandate. According to Laudicina et al. (2011), by 2011 the average US state had 37 mandates. Most mandates require health insurance plans to cover a specific procedure, such as mastectomy, or a specific condition, such as autism. Other mandates require insurance to cover certain types of health care providers, such as chiropractors. Finally, mandates may specify who is covered by health insurance, for instance the policyholder's grandchildren.

When a mandate is passed, more medical spending is channeled through insurers, rather than being paid directly out-of-pocket by consumers. This partly explains the long-term shift away from out-of-pocket spending in the US health care market, which is shown in Figure 1.1. As more medical spending uses insurance, health insurance costs and premiums rise. It is clear that health insurance premiums have been rising over time, but it is not clear exactly why this has been happening. This chapter estimates how much of the rising premiums can be attributed to insurance mandates.

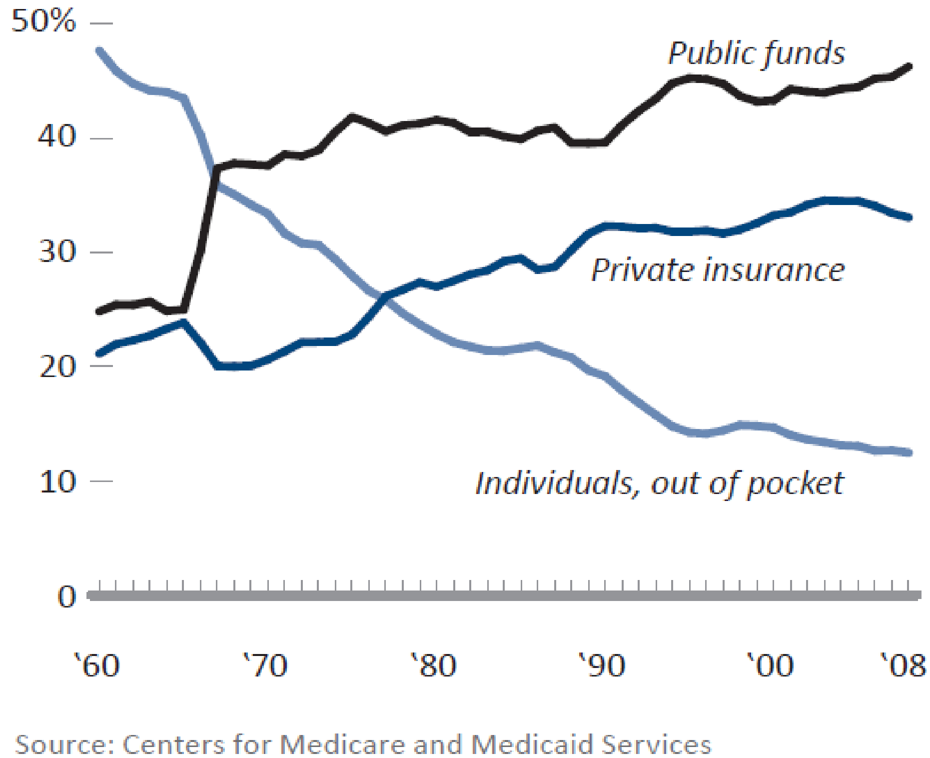

Figure 1.1: Source of Funds for US Health Care Spending 1960-2008

The main economic argument for mandates is the classic Rothschild and Stiglitz (1976) concern about adverse selection. Just as sicker people can push out healthier people by raising the cost of insurance generally, people with a high demand for a specific insurance benefit can raise costs for others. For example, an insurer may want to cover treatments for AIDS but may worry about attracting customers who reveal that they have AIDS after signing up at the normal premium. If AIDS coverage is mandated for all insurers, then none suffers so greatly from adverse selection.

The behavioral argument for mandates is that individuals and employers may not be able to correctly evaluate an insurance policy when enrolling in it. If people systematically undervalue some kinds of coverage, a mandate could make them better off. This may apply in particular to preventive care, or to treatments with positive externalities (such as reducing contagion).

Finally, individuals may support mandates because they believe that the costs will be borne by insurers and employers, giving individuals coverage at no cost. However, Gruber (1994a) found that the cost of a maternity care mandate was almost entirely passed on to individuals likely to use the coverage in the form of lower wages. Lahey (2012) found that infertility benefit mandates did not reduce wages for the affected group, but they did reduce employment.

The simple argument against mandates is that they reduce welfare by forcing people to pay for coverage that they were not willing to pay for voluntarily. The adverse selection argument suggests, however, that people may be more willing to pay for coverage if others are doing it too. Therefore, most arguments against mandates try to demonstrate that they raise insurance premiums and reduce the number of people with insurance. Most academic work has focused on the connection between mandates and the number of people without insurance. In one of the first papers on the subject, Jensen and Gabel (1992) developed a theoretical model of a firm's decision to offer health insurance, and showed that mandates make firms less likely to offer insurance.

Gruber (1994b) found that five specific mandates did not have a significant effect on the percentage of people with health insurance. He attributed this finding mainly to the fact that the mandates were not binding, since most insurance plans already covered the mandated services. Sloan and Conover (1998) found that a higher total number of mandates does reduce the number of people with insurance, while Cummins (2011) found that different types of mandates may increase or decrease insurance coverage.

This chapter proceeds as follows: Section 2 describes previous studies of the effect of mandates on premiums, Section 3 lays out a theoretical framework, Section 4 describes the data sources used, Section 5 presents the econometric results, Section 6 discusses the robustness and implications of the results, and Section 7 concludes.

1.2 Previous Work

Review articles by Jensen and Morrisey (1999) and Monheit and Rizzo (2007) describe much of the work that has been done on mandates in general. However, only a handful of studies have examined the effect of mandates on insurance premiums. These are described below.

1.2.1 Mandates in General

Kowalski et al. (2008) use data on premiums from high-deductible non-group plans sold by the insurer Esurance in 42 states in 2003, and Blue Cross Blue Shield Association data on the total number of mandates in each state. They find that the average mandate increases average premiums by 0.26-0.74%, and the effect is significant in most specifications. They suggest the importance of panel data for future work, noting "As with most cross-sectional work, we are also vulnerable to issues of endogeneity and omitted variable bias. In particular, it is possible that our regulation measures are correlated with unmeasured aspects of the insurance market within each state."

Gohmann and McCrickard (2009) also use premium data from non-group plans sold by Esurance. Their data are from 2006, and they focus on plans sold in Metropolitan Statistical Areas that cross state borders, leaving them with data for 108 cities in 35 states. By examining the difference in premiums on different sides of a state border within the same MSA, they are able to account for unobserved heterogeneity at the MSA level that could affect premiums. By assuming that all remaining differences in premiums are due to differences in mandates, however, they ignore all other sources of heterogeneity at the state level. For example, other state regulations and taxes besides mandates are likely to affect premiums. Their data on mandates come from the Council for Affordable Health Insurance, an industry association. They examine the effect of many specific mandates, finding that most mandates have a large positive effect on premiums while a few have a large negative effect. The problem with examining specific mandates is that there are over 100 kinds of mandates, but including more than 25 or so in a single regression causes perfect multicollinearity. When only a subset of mandates can be included, the mandates chosen may be unrepresentative. This is why most studies simply measure the effect of the total number of mandates.

LaPierre et al. (2009) use self-reported data on premiums from individuals in the Community Tracking Survey 1997-2003. Their data cover 3,552 families in 60 communities and 33 states, and they include only non-group plans. Their data on mandates are from the Blue Cross Blue Shield Association (BCBSA). They use separate variables for the number of benefit, provider, and coverage mandates rather than considering all mandates together. Using interval regression, they find coefficients suggesting that benefit mandates increase premiums while provider mandates decrease them. This could be because provider mandates encourage people to seek out lower-cost alternatives to doctors. In any case, the effect of mandates is not significant in any of their specifications.

1.2.2 Specific Mandates

The three papers described above are the most relevant to this chapter, as the only other papers to estimate the effect state-level mandates in general have on premiums. A related literature has investigated specific mandates that are expected to have especially large effects Bailey and Depew (2013) focus in depth on a single mandate, the federal dependent coverage mandate (described in depth in chapter 3 of this dissertation), and find that it led to a 1.9-2.5% increase in the premiums of family plans.

1.2.3 Why Premiums?

It is odd that so few papers have investigated the effect of mandates on premiums, given that there is a sizable literature on other effects of mandates. In fact, many of the other effects investigated depend on the assumption that mandates affect premiums. For instance, Gruber (1994a), Kaestner and Simon (2002), Meer and West (2011), and Lahey (2012) look for the effect of mandates and wages and employment, assuming that mandates made workers more expensive for employers to insure. Jensen and Gabel (1992) and van der Goes et al. (2011) investigate whether mandates cause employers to stop offering insurance altogether, while Jensen et al. (1995) look for firms self-insuring to avoid mandates. Again, these studies are premised on the assumption that mandates do in fact increase premiums.

1.3 Theory

While several previous papers have tried to estimate the effect of mandates on premiums, none has developed a theoretical framework for what we should expect mandates to do in various cases, and so how to interpret the empirical results. In particular, previous work has reported a null or even negative effect on premiums for some benefit mandates, a result that is difficult to reach with rational-choice theory.

Consider a simple supply and demand model of the market for health insurance as shown in Figure 1.2. A benefit mandate increases the services covered by health insurance, resulting in two effects. First, to the extent that the mandate increases the average cost C of providing health insurance, the supply curve shifts up from S to S'. Second, consumers value the mandated benefit at some fraction α of its cost C, causing the demand curve for health insurance to shift up by αC. I assume for now that $0 < \alpha < 1$; Figure 1.2 shows the case of full valuation, $\alpha = 1$. In this standard market, the quantity of health insurance decreases if consumers do not value the mandate at cost, and stays the same if they do. However, the effect on premiums

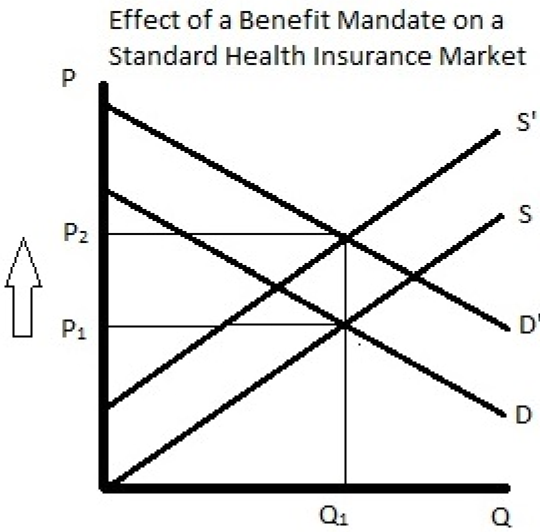

Figure 1.2: Supply and Demand for Health Insurance Following a Mandated Benefit: with Perfect Information

is almost certain: for all interior solutions they will increase.

A null effect on premiums is possible if the mandate is costless ($C = 0$); this could happen if the mandate was not binding because all firms already covered the benefit. In this case neither supply nor demand shift. Another, less likely way to achieve a null effect on premiums is if consumers place no value on the mandate ($\alpha = 0$) and have perfectly elastic demand.

Is it possible for benefit mandates to have a negative effect on premiums? In theory, consumers could value a mandate negatively ($\alpha < 0$), for instance by thinking it is immoral for their insurer to cover contraception. This would result in demand shifting downwards, which could lower premiums as long as the cost of the mandate (and so the supply shift) is relatively small. However, it is hard to imagine a mandate being passed if consumers overall value it negatively. Furthermore, this casts doubt on the idea that a mandate that has a zero or negative effect effect on premiums will increase consumer surplus.

It is also possible for consumers to value a mandate above its cost ($\alpha > 1$). In a standard market, insurers should already have offered the product without a mandate if consumers value it so highly. In a market characterized by adverse selection, however, individual insurers could refrain from offering a service valued above the cost to the average consumer, because they fear attracting only high-cost consumers. The adverse selection case is considered in Figure 1.3.

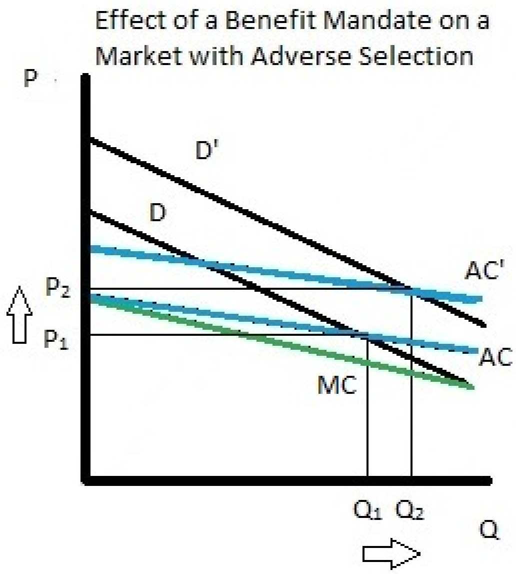

Figure 1.3: Supply and Demand for Health Insurance Following a Mandate Benefit: with Adverse Selection

Adverse selection in insurance can be represented most simply by a downward sloping average cost curve (see for instance Einav et al. (2010) and Einav and Finkelstein (2011)). The idea is that if only a small number of consumers are buying insurance, those choosing to buy will be those who value it most- those with the highest medical costs. This makes average costs (and so premiums) high when the quantity of health insurance bought and sold is small. As the quantity of health insurance increases, average and low-cost consumers are added to the risk pool, bringing down premiums and average costs.

On the demand side, the mandate has the same effect with adverse selection as without: demand shifts up by αC. On the supply side, the mandate increases costs by C in both markets. The difference is that, because supply curves downward in the market with adverse selection, an increase in costs that moves the curve "up" now means moving it to the right rather than the left. This means that the quantity of health insurance should increase rather than decrease following a benefit mandate in a market characterized by adverse selection. The effect on premiums, however, remains the same as in the standard market. They should increase except in the same odd corner solutions.

In sum, theory suggests we should expect premiums go up following a mandate. If they do not go up, it suggests that the mandate was not binding, and so had no effect on the market (or on welfare). This is true both in a standard market and in a market with adverse selection. In a market with moral hazard, the conclusion is even stronger. Moral hazard (increased use of the service once it is covered by insurance) should result in larger cost increases, and so larger supply shifts.

This theory informs the empirical work in two ways. First, premium movements cannot tell us anything directly about welfare in the market for health insurance. In particular, a zero or negative effect on premiums should not be interpreted as a good sign for mandates; in this model there is no way that a mandate can increase welfare without increasing premiums. Second, the magnitude of the premium change is important because it gives us a clue as to how big the distortions outside of the market might be. A sizable literature has looked for distortions from mandates on the assumption that premiums are being affected, without a solid idea of how much premiums are changing- or whether they are changing at all. The decisions that may be distorted in the wake of mandate-induced premium increases include whether to self-insure, whether to hire employees based on expected health costs, how many employees to hire, whether to hire full-time or part-time, and whether to offer health insurance to employees at all.

1.4 Data

This chapter uses data on insurance premiums from the Medical Expenditure Panel Survey Insurance Component (MEPS-IC). The MEPS-IC is an annual survey of employers conducted by the Agency for Healthcare Research and Quality, a part of the US Department of Health and Human Services. MEPS provides state-level information about the health insurance benefits offered by employers. The main variable used in this chapter is the annual premium paid for the average single-coverage employer-based health insurance policy. MEPS reports data on family and single-coverage plans separately; this chapter focuses on the single-coverage plans because they represented a slight majority of covered workers in 2011, and changes in their costs are less driven by changes in family structure. MEPS-IC also provides information about how employer-based health insurance premiums vary by firm size. This information is used as part of a robustness test. MEPS began collecting data on premiums in 1996, and the data currently run through 2011. There are some gaps in the data: MEPS did not collect any information about premiums in 2007. Before 2003, MEPS did not collect data on premiums in every state and year; in some years there are data on as few as 40 states. The original MEPS premium data are not inflation-adjusted, so I deflate MEPS premiums to 1996 dollars, using the Consumer Price Index for all urban consumers provided by the Bureau of Labor Statistics.

The data on mandates come from the Blue Cross Blue Shield Association, found in Laudicina et al. (2011). BCBS collects data on new mandates each year and classifies them as benefit, provider, or coverage mandates. They also provide the year in which each existing mandate was passed. I have generated a count of the total number of mandates from BCBS data by combining their count of benefit, provider, and coverage mandates, as well as "additional mandates" that were passed in only a handful of states. The effects of each kind of mandate are also investigated separately. According to BCBS data, there were 1896 total mandates in 2011, counting mandates in all states and the District of Columbia, an average of 37 mandates per state. This is an increase from 1031 total mandates (20.2 per state) in 1996. Virginia passed the most new mandates over the 1996-2011 period, with 32, while Ohio passed the fewest, with 5. Thirteen states have passed mandate waivers, which allow some insurers or businesses to offer "mandate-lite" plans exempt from some mandates. Data on mandate waivers comes from the Robert Wood Johnson Foundation's State Coverage Initiative.

Data for demographic control variables come from the U.S. Census Bureau's Current Population Survey (CPS). They were retrieved from the Integrated Public Use Microdata Series (IPUMS) compilation of CPS data. The control variables used are race, union membership, mean income, and mean age.

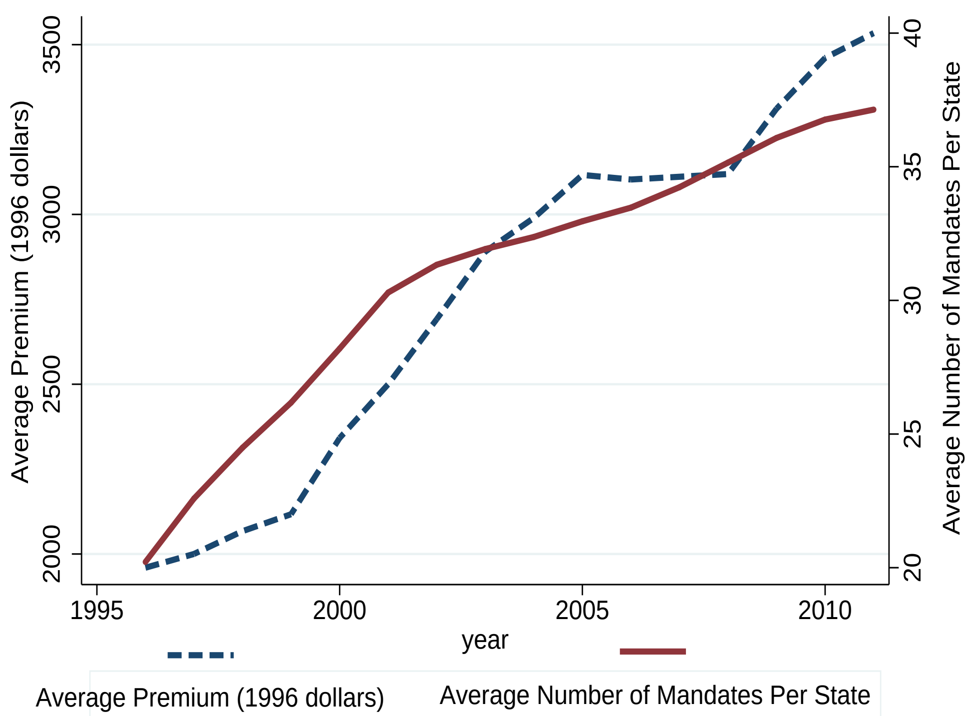

Figure 1.4: Average Mandates and Premiums 1996-2011
Sources: Data on premiums is from the Medical Expenditure Panel Survey- Insurance Component. Data on mandates is from Laudicina et al. (2011).

1.5 Rresults

I estimate fixed effects models of the form

$$lnPremium_{st} = \beta_0 + \beta_1 * Mandates_{st} + \beta_2 * Controls_{st} + \beta_3 Time_t + \varepsilon_i$$

where $lnPremium_{st}$ is the natural logarithm of the average group health insurance premium in state s in year t, $Mandates_{st}$ is the total number of mandates in force in state s in year t, and $Controls_{st}$ represents demographic information about state s in year t. All data are reported annually at the state level from 1996 to 2011. There are 693 state-year observations in the data. A linear time trend is included to control for the fact that medical costs are increasing over time for reasons other than changes in mandates and demographics. The fixed effects estimator is used, meaning that the regression controls for unobserved state-specific effects. Fixed effects estimators are consistent but may not be efficient. Hausman tests confirmed that the fixed effects estimator was more appropriate than random effects for these data.

I estimate several different specifications to check the robustness of the results. Column (1) of Table 1 shows the results of a simple fixed effects regression of mandates on premiums, which results in a very large estimated effect of mandates. Column (2) shows the results once time and demographic changes (in race, union membership, mean income, and mean age) are controlled for. The estimated effect is much smaller but remains strongly statistically significant.

The Wooldridge test described by Drukker (2003) suggests that the standard fixed effects models used to generate columns (1) and (2) suffer from serially correlated errors. Because of this, it may be more appropriate to use fixed effects with an AR(1) disturbance term, meaning that premiums that are higher than predicted in one year are likely to remain higher than predicted the next year. Baltagi and Wu (1999) explain how this model may be especially appropriate for panels with unequally spaced data; the MEPS premium data used has such gaps, with an average of 13.6 years of data for each state over the 16 years from 1996 to 2011. Column (3) shows the results when this fixed effects with AR(1) disturbances model is used.

Table 1.1: Effect of Mandates on Average Employer-Based Health Insurance Premiums

	(1)	(2)	(3)
$TotalMandates$	.0297***	.0044***	.0111***
	(.0007)	(.0009)	(.0027)
$PercentUnion$		1.015***	1.856***
		(.2252)	(.3981)
$MeanIncome$		-.0023	.0164***
		(.0017)	(.0034)
$PercentBlack$		-.4401*	.6667*
		(.2310)	(.3455)
$MeanAge$		.0151***	.1707***
		(.0042)	(.0039)
$MandateWaivers$		.0132	.0401
		(0110)	(.0280)
$Time$		.0379***	.0018
		(.0019)	(.0039)
Overall R^2	.18	.76	.34

*Indicates p-value < 0.10 **Indicates p-value < 0.05 ***Indicates p-value < 0.01
693 observations, standard errors given in parentheses

Because the dependent variable is the natural logarithm of premiums, the coefficients can be interpreted as the percentage change in premiums caused by a one-unit change in the independent variable. For instance, the .0044 coefficient of mandates in column (2) means that each mandate increases premiums by 0.44%. Under the alternative assumption of AR(1) disturbances reported in column (3), each mandate increases premiums by 1.12%.

The number of average number of mandates in each state increased by 17 in the period studied, from 20 to 37. Combined with the estimates found in columns (2) and (3) above, this suggests that mandates were responsible for premium increases of 7.5% to 19%. Over the same period, annual inflation-adjusted single-coverage premiums increased from $1960 to $3534, or 80.3%. This means that mandates seem to account for 9.3% to 23.6% of all premium increases from 1996 to 2011.

1.6 Discussion and Robustness

1.6.1 Varying Types of Mandates

One limitation of these results is that the effect of any given mandate may be very different from the average effect. Some mandates certainly cost much more or less, depending on the cost and popularity of the mandated benefit and the number of plans that already covered it before the mandate. Furthermore, this analysis estimates the average cost of mandates in the 1996-2011 period. This average is a function of the kind of mandates passed in the period. In the future, states may pass a mix of mandates that are more or less expensive.

In order to get a better idea of how different mandates may have different costs, I estimate the effect of three different categories of mandates separately. Table 1.2 shows the estimated effects of benefit mandates (which require insurance to cover a certain condition or procedure), provider mandates (which require insurance to coverage a certain kind of provider, such as marriage therapists) and person mandates (which require insurance to cover a certain kind of person, such as adult dependent children). I find that benefit and provider mandates significantly increase premiums. In fact, provider mandates appear to increase costs much more than benefit mandates. This is in sharp contrast to LaPierre et al. (2009), whose point estimates suggest that provider mandates reduce premiums.

I find that person mandates have no effect on premiums. This last should be seen as a robustness check, since the premium data used is for single-coverage plans as opposed to family plans. Most person mandate laws apply only to family plans, and so should have no effect on single-coverage premiums.

Table 1.2: Effect of Different Types of Mandates on Premiums, 1996-2011

	Benefit Mandates	Provider Mandates	Person Mandates
Estimated Coefficient	.0049***	.0171***	-.0025
Standard Error	(.0014)	(.0029)	(.0053)

*Indicates p-value < 0.10 **Indicates p-value < 0.05 ***Indicates p-value < 0.01

Results are from fixed effects regressions of the natural log of premiums on mandates, mandate waivers, demographic controls, and time with 693 observations

1.6.2 Firm Size and Mandate Exemptions

State-level mandates do not apply to all insurance plans. The Employee Retirement Income Security Act of 1974 (ERISA) is a federal law that preempts most state-level insurance regulation and allows employers to self-insure. Employer plans covered by ERISA are exempt from most state-level mandates, so mandates should not directly affect their premiums. According to 2011 MEPS data, 58.5% of all privately insured workers are enrolled in self-insured plans. Most large employers offer self-insured plans, since they are better able to pool risks. Table 1.3 displays data from MEPS, illustrating that employees at larger firms are much more likely to be enrolled in self-insured plans. Table 1.3 also shows the results of regressions estimating the effect of benefit mandates on firms of various sizes. The results clearly show the effect of self-insurance: large firms experience smaller and less significant increases in premiums as a result of mandates. Firms with over 1000 employees are estimated to see only a 0.43% premium increase following a mandate, compared to a 0.77% increase at small firms with 10 to 24 employees. It is important to keep self-insurance in mind when applying the results of this chapter. Any future federal mandates are likely to have larger effects than those estimated here because, unlike state mandates, they can also apply to self-insured firms.

Table 1.3: Effect of Mandates on Premiums by Firm Size

# of Employees in Firm	10 to 24	25 to 99	100 to 999	Over 1000
Employees in Self-Insured Plan	9.5%	13.2%	35.0%	86.3%
Benefit Mandates Coefficient	.0077***	.0061***	.0069***	.0043**
	(.0023)	(.0021)	(.0021)	(.0018)

*Indicates p-value < 0.10 **Indicates p-value < 0.05 ***Indicates p-value < 0.01

Data on self-insurance is the US average in 2011 according to MEPS. Coefficients are from a fixed effects regression controlling for mandate waivers, demographics and time with 693 observations from 1996 to 2011. Standard errors are in parentheses.

1.6.3 Endogeneity

A possible shortcoming of the econometric approach used in this chapter is that it does not fully account for endogeneity in the passage of mandates. If states with increasing premiums are more likely to pass mandates, then these results overestimate the true effect of mandates on premiums. If, on the other hand, states with rising premiums are less likely to pass mandates then these results underestimate the true effect. This shortcoming was greater in previous studies which did not control for state and year effects. The fixed-effects model used with panel data in this chapter reduces the extent of the problem by accounting for anything that regularly makes states have both high premiums and many mandates (perhaps a permanently strong doctor's lobby). However, it still misses the effect of year-to-year changes in variables that affect both premiums and mandates (such as an increase in lobbying efforts by doctors).

This potential bias in the results could, in principle, be overcome by using an instrumental variable for the passage of mandates, as was done in van der Goes et al. (2011). However, in practice, unbiased instruments that are correlated with mandates but uncorrelated with premiums except through mandates are hard to find. Most variables that could lead to the passage of mandates, such as a change in the majority political party or an increase in the number of people with a condition, are also likely to have direct impacts on health insurance premiums.

The finding of smaller and less significant results for large firms and person mandates in this chapter argues against a large bias from endogeneity or omitted variables. If endogeneity or an omitted variable were at work, we might expect them to tie all kinds of mandates and premiums together. However, if mandates are the main causal force at work, then we know when they should have little to no effect on premiums. Person mandates apply only to family plans and should not affect the premiums of single coverage plans, and in fact no significant relationship was observed between person mandates and single-coverage premiums. Mandates in general should have a smaller effect on large firms that can self-insure, and this was in fact found to be the case. There does appear to be a real causal effect of mandates on premiums, though there may still be some bias at work, and the question of how mandates get passed remains open.

A final caveat is that the reported estimates in this chapter may be biased downward. This is because plans can have ways to respond to a new mandate other than by simply adding coverage and passing the cost on in a premium. In order to keep the overall cost of the plan near the pre-mandate level, they could reduce coverage of other benefits or increase deductibles and co-payments. These changes would reduce the cost of the plan and reduce premiums, masking the full cost of the mandate.

1.7 Conclusion

This chapter finds that mandates have a substantial and strongly significant positive impact on health insurance premiums. The average mandate increases premiums by 0.44% to 1.11%. This result is based on premium data representing half of the employer-based market, and is therefore more general and robust than the results of previous work, most of which relied on data from a single insurer in a single year, and all of which focused on the small non-group market. Mandates cause larger premium increases at smaller firms. States considering new mandate legislation should keep in mind the substantial effect of mandates on health insurance premiums. Mandates do not provide a free lunch.

CHAPTER 2

WHO PAYS THE HIGH HEALTH COSTS OF OLDER WORKERS? EVIDENCE FROM PROSTATE CANCER SCREENING MANDATES

Between 1992 and 2009, 30 US states adopted laws mandating that health insurance plans cover screenings for prostate cancer. Because prostate cancer screenings are used almost exclusively by men over age 50, these mandates raise the cost of insuring older men relative to other groups. This chapter uses a triple-difference empirical strategy to take advantage of this quasi-random natural experiment in raising the cost of employing older workers. Using IPUMS data from the March Supplement of the Current Population Survey, I find that the increased cost of insuring older workers results in their receiving 3% lower hourly wages, being 2.2% less likely to be employed, and being 2% less likely to have employer-sponsored health insurance.

2.1 Introduction

Prostate cancer mandates are state laws that require most private health insurance plans to cover screening tests for prostate cancer. These mandates are now in place in 29 US states. There has been a steady growth in the number of prostate cancer mandates since Delaware and Georgia passed the first laws in 1992. This chapter uses a difference-in-difference-in-difference (henceforth triple difference or DDD) strategy to estimate the effects of these mandates on the labor outcomes of older men. Triple-difference analysis has been applied to maternity care mandates by Gruber (1994a) and to infertility treatment mandates by Lahey (2012), but has yet to be applied to measure the effect of prostate cancer mandates. The triple-difference strategy is similar to the standard difference-in-difference (henceforth double-difference or DD) strategy in that both evaluate the effect of a binary change such as the passage of a law by using comparison groups. Triple-difference estimation takes advantage of the fact that some situations offer additional comparison groups for a more robust control. Prostate cancer screening is used exclusively by men, and almost exclusively by men over age 50. Therefore, insurance companies and employers know that the cost of this mandate will be generated by one easily identifiable subgroup, rather than the whole population, and they can be expected to frame their responses to the mandate accordingly.

The passage of prostate cancer mandates raises the costs of insuring and employing men over age 50 relative to others. This gives us a window into how employers react to the fact that older workers generally have higher health costs, and how workers respond in turn to changing compensation. Previous work has found that the relatively poor health of older workers adversely affects their labor market outcomes both directly (Bound et al. (1999)) and through increased health insurance costs paid by employers (Scott et al. (1995)).

This chapter uses 1990 to 2009 data on labor market outcomes and demographic controls from the Integrated Public Use Microdata Series compilation of the March Current Population Survey (IPUMS-CPS), a dataset with approximately 200,000 individual-level observations per year. The particular labor market outcomes studied are employment, hourly wages, and whether or not an individual has employer-provided health insurance. I find that labor markets react strongly to the increased health costs of older workers, resulting in lower levels of money wages, employment, and employer-based health insurance among the older men that prostate cancer screening mandates were intended to help.

Section 2 gives more information on prostate cancer mandates, health insurance mandates in general, and the medical side of prostate cancer. Section 3 describes the data and the empirical strategy of triple-difference estimation. Section 4 gives the econometric results and discusses their robustness and implications. Section 5 concludes.

2.2 Background

2.2.1 Health Insurance Mandates

Health insurance mandates are common at the state level in the United States, and are applied to many benefits besides prostate cancer screening. Many other specific treatments or conditions receive mandated coverage, from maternity care to infertility treatments to diabetes. Laws mandating that health insurance cover specific treatments or conditions are known as benefit mandates. Other types of mandates may require insurance to cover certain types of providers, such as chiropractors, or certain types of beneficiaries, such as grandchildren. This chapter follows most academic work in focusing on benefit mandates, which are the most common type. Industry organizations such as the Council for Affordable Health Insurance and the Blue Cross Blue Shield Associations release annual reports tracking which mandates are in force in each state, the most recent reports being Laudicina et al. (2011) and Bunce and Wieske (2011). The number of mandates in force in the average state has greatly increased over the past 40 years. According to the Council for Affordable Health Insurance, the average number of mandates in each state has gone from 0 in 1960, to 17 in 1992, to 45 in 2011. Every year more mandates are passed, while they are almost never repealed.

There has been a fair amount of academic work on health insurance mandates, which is summarized in the survey articles by Jensen and Morrisey (1999) and Monheit and Rizzo (2007). One basic effect of mandates often predicted by these papers is an increase in overall insurance premiums, as treatments that were formerly paid out-of-pocket are now paid using insurance, while moral hazard increases total use of the service. Kowalski et al. (2008), LaPierre et al. (2009), and Gohmann and McCrickard (2009) test this hypothesis in the market for individual insurance. Kowalski et al. (2008) and Gohmann and McCrickard (2009) find that mandates tend to cause statistically significant increases in premiums, while LaPierre et al. (2009) find they do not. Gohmann and McCrickard (2009) and LaPierre et al. (2009) find large variations in the effects of different mandates, with some causing large increases in premiums while others cause premiums to decrease. Bailey (2013a) and Chapter 1 of this thesis test the effect of mandates on premiums for employer-based group health insurance (which represents the vast majority of the private insurance market), finding that the average mandate causes a statistically significant increase in premiums of 0.44-1.11%. Bunce and Wieske (2011) use actuarial data to estimate the cost of each mandate, finding that some mandates lead to insurance cost increases of over 5%, while most mandates (including prostate cancer screening) directly increase costs by less than 1%. It is important to keep in mind, however, that each paper cited above estimates the effect of mandates on the premium for an average person, rather than analyzing how mandates affect costs for different groups.

Most other academic work on mandates has examined the mandates' effects on the labor market, starting with Summers (1989). If mandates result in higher insurance premiums, then employers who offer health insurance may reconsider their compensation packages. They may stop offering health insurance, change the composition of insurance plans, or reduce other forms of compensation, such as wages. Gruber (1994b) found that unemployment did not rise after the passage of five particularly costly mandates. He speculated that the mandates did not actually increase the proportion of plans offering the mandated treatments, due to mandate exemptions and a high proportion of plans already offering the treatments. Kaestner and Simon (2002) also found that the average mandate does not have a statistically significant effect on labor market outcomes for the average person. Van der Goes et al. (2011), by contrast, found that the average mandate reduces the chance that an individual has employer-provided health insurance by 0.2%, and Jensen and Gabel (1992) found that mandates are a major reason that some firms do not offer health insurance. In summary, there is mixed evidence that the average health insurance benefit mandate has significant effects for the average person.

The evidence that mandates affect specific groups is much stronger. Gruber (1994a) found that the cost of mandated maternity care benefits was passed on in its entirety in the form of lower wages for women aged 20-40. Lahey (2012) found that infertility treatment mandates resulted in lower wages for women aged 28-42, which, in turn, decreased the women's labor supply. Bailey (2013b) found that diabetes mandates resulted in lower wages for obese workers, who are much more likely than average to have diabetes. The cost of a mandate may be too small to notice if it is spread out over all insured people, but it can be significant if it is passed on to one relatively small demographic group.

2.2.2 Prostate Cancer Screening

The basic facts about prostate cancer are well-summarized by the most recent report of the United States Preventative Services Task Force (USPSTF), an independent expert body within the Agency for Health Care Research and Quality. They state that

> "Prostate cancer is the most commonly diagnosed non-skin cancer in men in the United States, with a lifetime risk for diagnosis currently estimated at 15.9%. Most cases of prostate cancer have a good prognosis, even without treatment, but some are aggressive; the lifetime risk of dying of prostate cancer is 2.8%. Prostate cancer is rare before age 50 years and very few men die of prostate cancer before age 60 years. Seventy percent of deaths due to prostate cancer occur after age 75 years."

The most common screening test is the Prostate-Specific Antigen (PSA) test, which tests the antigen levels in blood serum. Digital Rectal Examinations (DRE) are also sometimes employed. The USPSTF gives the PSA test a grade D recommendation, meaning they recommend against it. Their methodology considers only medical harms and benefits, not financial costs. According to USPSTF (2012), "There is adequate evidence that the benefit of PSA screening and early treatment ranges from 0 to 1 prostate cancer deaths avoided per 1000 men screened... no study found a difference in overall or all-cause mortality." This possible benefit is weighed against the potential harm incurred from screening and treatment. Surgery and radiation used to treat prostate cancer cause enough morbidity and mortality that the USPSTF finds that "there is convincing evidence that PSA-based screening for prostate cancer results in considerable overtreatment and its associated harms."

It is an odd and concerning fact that people in the United States spend so many resources on, and in fact mandate insurance coverage for, medical care that may bring no net benefit. The argument of this chapter, however, does not rely on the still-debated claim that prostate cancer screening has no net medical benefit. The econometric strategy used to test the labor market effect of these mandates depends on two facts about prostate cancer screening. One is that it is primarily used by one demographic group, men over age 50. This point should be beyond dispute: prostate cancer affects only men, the vast majority of prostate cancer diagnoses and deaths are in men over age 50, and even the more pro-PSA-testing guidelines from the American Cancer Society recommend possible screening only for men over age 50. The other fact about prostate cancer screening necessary for this chapter's argument is that screening is expensive enough for insurers and employers to notice it and respond to it.

This may not be so obvious, since the PSA test is simple blood work, and Medicare pays only about $30 per test. However, the test can lead to additional treatments that would not have occurred otherwise. According to USPSTF (2012), "Over 10 years, approximately 15% to 20% of men will have a PSA test result that triggers a biopsy." Mitchell (2012) finds that in 2005 Medicare paid approximately $900 per prostate biopsy (including pathology lab services). This screening also leads to prostate cancer being treated earlier and more often; according to USPSTF (2012), "From 1986 through 2005, PSA-based screening likely resulted in approximately 1 million additional U.S. men being treated with surgery, radiation therapy, or both compared with the time before the test was introduced". This makes for an additional 50,000 cases of prostate cancer being treated every year. According to Jacobs et al. (2012), about 200,000 men are diagnosed with prostate cancer every year in the US, making screening-induced treatments 1/4 of the total.

According to Surveillance Epidemiology and End Results (SEER) data summarized by Howlader et al. (2013) the 50-64 age group accounts for 40% of prostate cancer diagnoses. Assuming that the age of the screening-induced cases matches that of the general prostate cancer population, this means 20,000 men age 50-64 are diagnosed with prostate cancer annually as a result of PSA screening. Because prostate cancer is so slow-growing, men commonly have the disease for decades before dying of another cause. From the perspective of an employer, screening may result in payments for surgery or radiation treatment now that otherwise would have been put off for years, often past retirement. Roehrig et al. (2009) estimate that in 2005, medical total spending related to prostate cancer treatment was $6.8 billion, or $34,000 per new prostate cancer patient. Each year, PSA testing leads to 20,000 addition prostate cancer diagnoses among men aged 50-64; this represents 0.1% of the age group, making for an expected cost of $34000*0.001=$34. Even combined with the costs of biopsies and and PSA tests, expenditures on prostate cancer are relative low.

This suggests that the direct monetary costs of prostate cancer may be dominated by lost time: absences due to attending screening and treatment, or early retirements due to receiving a cancer diagnosis.

One puzzle about prostate cancer screening mandates is described by Rathore et al. (2000), who note that "coverage for the PSA test is mandated in 18 states despite inconclusive evidence about its efficacy in reducing prostate cancer mortality.

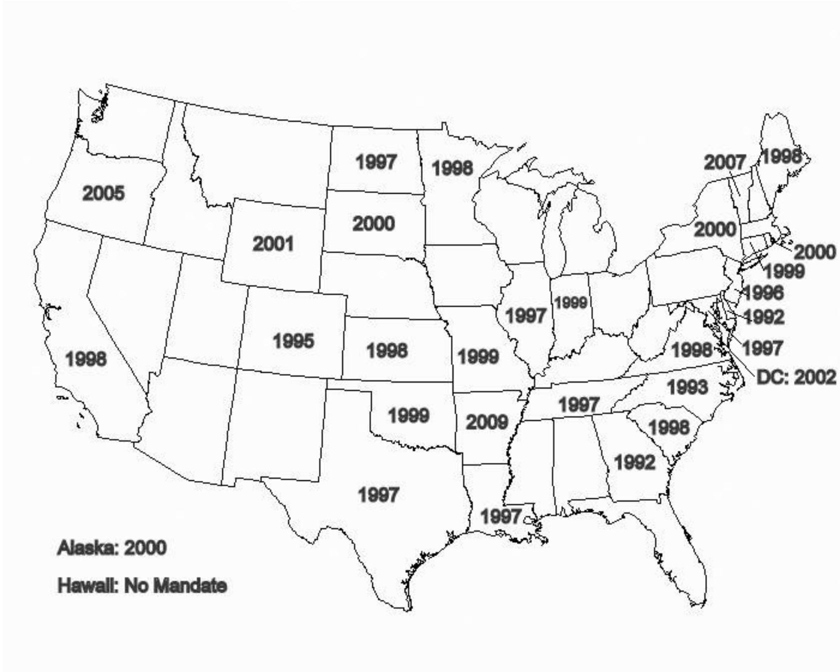

Figure 2.1: Date of Prostate Cancer Screening Mandate Passage in Each State

This finding is particularly troubling because the majority of PSA mandates were implemented after 1996, during the recent debate concerning the value of the PSA." States that choose to mandate screening are choosing to follow one set of medical guidelines, usually the consensus-based American Cancer Society guidelines, rather than another set such as that of the US Preventative Services Task Force. Moreover, after the law is passed, states often drift into a position where their mandate follows no current guidelines; Rathore et al. (2000) found that "ACS and USPSTF guidelines for both mammography and PSA testing have been updated in the past 3 years, yet few states have modified their screening coverage mandates accordingly. In fact, most of the mandates we evaluated were outdated by both current ACS and USPSTF recommendations."

2.2.3 Prostate Cancer Mandates

Figure 2.1 shows when each state passed its prostate cancer mandate; blank states have not passed any prostate cancer mandate as of 2013. Figure 2.2 shows the number

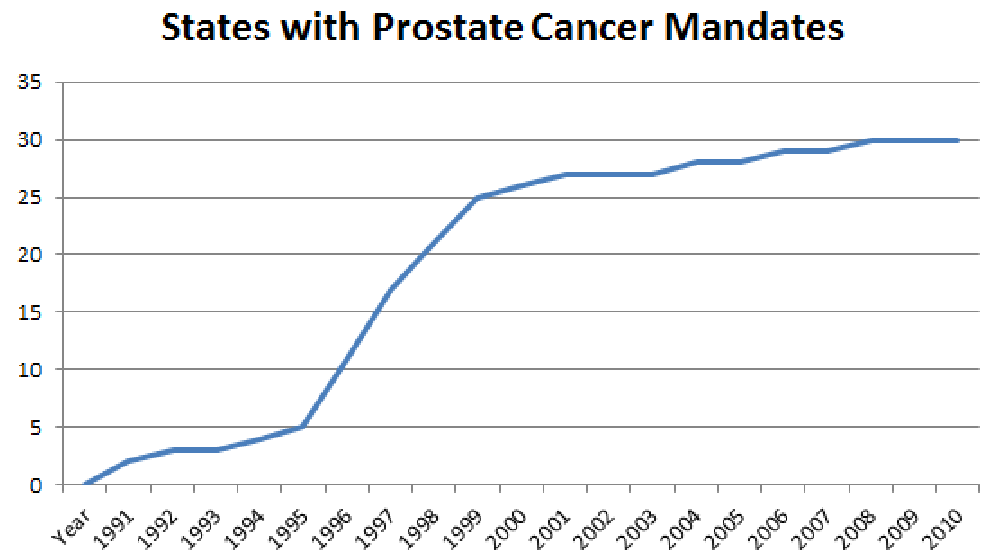

Figure 2.2: Number of States with Prostate Cancer Mandates by Year

of US states with prostate cancer mandates in force over time. The steady upward trend is clear, with rapid growth in the mid-1990s. There is some variation in the specific language used in each state's mandate law. Most states mandate coverage for men over age 50 and for men over age 40 who are in high-risk categories, while some simply mandate coverage for everyone. Many states specifically mention that they are mandating the Prostate-Specific Antigen test, some specify this as well as another test, and some do not specify the screening technique. Given that prostate cancer screening is mainly sought by men over age 50 in any case, this variation in state laws does not seem to be enough to require different codings of the mandate variable in the main regressions.

2.3 Model

The canonical model of a mandated benefit was developed by Summers (1989). In Summers' model, a mandated benefit applies to the whole population, and causes a shift from money wages to the mandated benefit. Hours worked will decrease if workers value the benefit at less than its cost. The effect of the mandate is the same as that of a tax set equal to the difference between the cost of the mandate to employers and its valuation by employees. For example, if the benefit costs \$1000 per year to provide, but employees value the benefit at only \$700, then the mandate is equivalent to a \$300 tax in its effect on employment and deadweight loss.

Gruber (1992) extended this model to account for group-specific mandates, mandates that only affect some workers. This model was also explained with graphs by Lahey (2012). To my knowledge, no published paper has equations modeling the labor market effects of group-specific health benefit mandates. Gruber (1992) is a working paper, and the theory section was cut out when it was published (Gruber 1994a). Lahey (2012) includes only supply and demand graphs. This thesis generalizes and extends the Gruber model.

Consider two groups of workers: Group A has a mandated benefit applied to it while group B does not. The mandated benefit costs employers C to provide, and workers value it at a fraction $0 \leq \alpha \leq 1$ of its cost. Utility increases linearly with consumption (financed by working L hours to earn hourly wages W, scaled by the elasticity of supply parameter ε^S) and decreases quadratically as leisure is lost. Workers in Group A face the mandate and have utility:

$$U_A = \varepsilon_A^S L_A(W_A + \alpha C) - \frac{1}{2}L_A^2 \tag{2.1}$$

While workers in group B have utility:

$$U_B = \varepsilon_B^S L_B(W_B) - \frac{1}{2}L_B^2 \tag{2.2}$$

Workers in each group choose their labor supply to maximize utility:

$$\frac{\partial U_A}{\partial L_A} = \varepsilon_A^S(W_A + \alpha C) - L_A = 0 \tag{2.3}$$

$$\frac{\partial U_B}{\partial L_B} = \varepsilon_B^S(W_B) - L_B = 0 \tag{2.4}$$

Resulting in the labor supply equations:

$$L_A^S = \varepsilon_A^S(W_A + \alpha C) \tag{2.5}$$

$$L_B^S = \varepsilon_B^S(W_B) \tag{2.6}$$

Firms maximize profits by choosing how many of each type of worker to hire. The market for labor is perfectly competitive, so firms and workers both take wages as given. In the simplest case, workers are perfect substitutes. Firms have a constant returns to scale production technology and face a constant price for their output. They earn profits as described by equation (7):

$$\pi = \varepsilon_A^D L_A + \varepsilon_B^D L_B - (W_A + C)L_A - W_B L_B \tag{2.7}$$

The first order conditions for the firm's problem are:

$$\frac{\partial \pi}{\partial L_A} = \varepsilon_A^D - (W_A + C) = 0 \tag{2.8}$$

$$\frac{\partial \pi}{\partial L_B} = \varepsilon_B^D - W_B = 0 \tag{2.9}$$

Solving the firm's problem for wages gives:

$$W_A = \varepsilon_A^D - C \tag{2.10}$$

$$W_B = \varepsilon_B^D \tag{2.11}$$

Plugging these equilibrium wages into the labor supply equations gives:

$$L_A = \varepsilon_A^S * \varepsilon_A^D + \varepsilon_A^S * (\alpha - 1)C \tag{2.12}$$

$$L_B = \varepsilon_B^S * \varepsilon_B^D \tag{2.13}$$

The model shows in equation (10) that wages decline by C, the cost of the mandate, for type A workers. How workers react to this decline in wages depends on their valuation of the mandated benefit, as is shown in equation (12). Employment of type A workers decreases when $\alpha < 1$, that is, when workers value the mandate at less than its cost. If workers value the mandate at cost ($\alpha = 1$) there is no employment effect. If workers place no value on the mandate, employment decreases by $\varepsilon_A^S * C$, where ε_A^S is the elasticity of labor supply for workers affected by the mandate. Employment could increase if workers value the mandate above its cost, or if the labor supply curve is backward bending.

2.4 Data and Identification Strategy

2.4.1 Data Sources

Data on the passage of mandates was gathered from several sources. The Blue Cross Blue Shield Association releases an annual report, "State Legislative Healthcare and Insurance Issues," which includes information on which health insurance benefit mandates are in force in each state. The National Council of State Legislatures maintains a database of states that have passed prostate cancer screening mandates, with a description of the specific components of each state law. The initial coding of the dummy variable for the passage of mandates was based on these two sources. When the BCBSA and NCSL were in conflict, Lexis Nexis was used to find the actual text of the state law and determine how to code the state.

All other data is from the Integrated Public Use Microdata Series release of the March Current Population Survey. There are 3.5 million observations covering every state and the District of Columbia from 1990 to 2009. Of these, 99,862 individuals observed are most affected by the mandates (men between 50 and 64 years of age who live in states and years where a mandate is in effect). All three dependent variables are from IPUMS: employment, hourly wages, and a dummy for whether individuals have employer-provided health insurance. The health insurance and employment variables come directly from IPUMS. Hourly wages are imputed from other IPUMS variables by dividing total annual income by hours worked per week and weeks worked per year, and adjusting for inflation using the Consumer Price Index. The natural log of hourly wages is used for the regressions so that the coefficients can be interpreted as percentage changes.

Control variables include dummies for the individual's age ($ageDum36_i - ageDum64_i$), race ($white_i$ and $black_i$), ethnicity ($hispanic_i$), education ($HighSchoolGrad_i$ and $CollegeGrad_i$), and marital status ($married_i$). The hourly wage and employer-based insurance regressions also control for job characteristics: the size of the firm ($SmallFirm_i$ and $MidFirm_i$) and whether the worker is in a union ($union_i$). The independent variables of interest for triple-difference regression are dummy variables generated from the IPUMS data: these include a dummy for whether an individual is male ($Male_i$), a dummy for whether he is over 50 ($AgeGroup_i$), and a dummy for whether his state has a prostate cancer mandate in effect ($Mandate_{st}$). The way that these variables and the interactions between them are used for triple-difference regression is explained in the following section.

2.4.2 Triple-Difference Estimation

Triple-difference estimation is a technique occasionally used in labor economics. It has been applied to health insurance mandates by Gruber (1994a) and Lahey (2012), who studied the effect of maternity care and infertility mandates respectively. According to Angrist and Pischke (2009), "this triple-differences model may generate a more convincing set of results than a traditional DD analysis that exploits differences by state and time alone." Triple-difference estimation is a way of adding additional control groups to traditional difference-in-difference (DD) estimation to increase its robustness. A common application of traditional DD estimation is to examine the effect of a policy change in one state by using other states as a control group. For example, one could compare the change in wages over time when one state passes a mandate (one difference) to the change in wages in states that didn't pass the mandate (second difference). However, traditional DD estimation would have trouble distinguishing between the effect of the policy and the presence of an unobserved state-specific shock, such as a decline in the state's major industry. Alternatively, traditional DD estimation sometimes compares groups within the same state or country when a policy is expected to affect only one group within the state. For example, one might use men as a control group when examining a policy like maternity leave that is expected to affect only women. However, DD estimation in this case is sensitive to the assumptions that the policy has no effect on the other group and that there are no other, unobserved shocks affecting women's wages.

Triple-difference estimation can achieve more robust results than difference-in-difference estimation by using several control groups at once. In this case, the primary treatment group for prostate cancer screening mandates is men over age 50 in states that have passed mandates. What is the best control group to use when estimating the effect of the mandate on this group? A mediocre DD estimation would compare everyone in the treated states to everyone outside them, or compare men in treatment states to women in the same states, or compare people over age 50 in treatment states to people under 50 in the same states. Each of these control groups has at least one thing in common with the treatment group. A better DD estimation would use a more relevant control group, one with at least two things in common with the control group: men over age 50 in control states, or men under 50 in the treatment states, or women over 50 in the treatment states. DD estimation would pick one of these good control groups and move along, throwing away the information from the other groups. Triple-difference estimation uses all three good control groups, thus controlling for many possible sources of unobserved variation. An unobserved shock to wages in treatment states, or to men, or even an unobserved shock specific to men in treatment states, will not bias the results.

In simple cases, triple-difference estimation can be performed without regression by comparing the changes in means of the treatment group and control groups. However, using DDD regression allows us to control for additional sources of heterogeneity, such as changes in the composition of the groups. The control groups are those who share two (but not three) relevant characteristics with the treatment group; for instance, they are men over age 50 who have no mandate in place in their state at the time they are surveyed.

In a DDD regression, the control groups are represented by double interaction terms such as $Male_i * AgeGroup_i$, and the treatment group of men over age 50 in states with mandates is represented by the triple interaction term $Mandate_{st} * AgeGroup_i * Male_i$. The treatment effect estimated is simply the coefficient of the triple interaction term.

The basic DDD regression equation is given by:

$$
\begin{aligned}
Y_{it} &= \beta_1 Mandate_{st} * AgeGroup_i * Male_i + \beta_2 Mandate_{st} * Male_i \\
&+ \beta_3 Mandate_{st} * AgeGroup_i + \beta_4 Male_i * AgeGroup_i + \beta_5 Mandate_{st} \\
&+ \beta_6 Male_i + \beta_7 AgeGroup_i + \beta_8 X_i + \theta_t + \sigma_s + \epsilon_{sti}
\end{aligned}
$$

Where Y_{it} is a variable measuring the outcomes of individual workers. Three different dependent variables Y_{it} are used in separate regressions: the natural log of hourly wages, a dummy indicating whether the individual is employed, and a dummy indicating whether the individual has employer-sponsored health insurance. The subscript i refers to individuals, s refers to states, and t refers to years. $Mandate_{st}$ is a dummy variable that is equal to 1 in states and years where mandates are in force and equal to 0 otherwise. $AgeGroup_i$ is a dummy variable set to 1 for individuals between 50 and 64 years old, and $Male_i$ is a dummy set to 1 for men. X_i is a vector of control variables that can be observed for individuals. These controls include measures of age, race, ethnicity, education, and marital status. θ_t indicates fixed effects for each year, and σ_s indicates fixed effects for each state. The coefficient β_1 gives the DDD estimate of the treatment effect, the change in the dependent variable for the group (men over age 50 in states with mandates) that should be most affected by the mandate.

2.5 Results

Table 2.1 shows the results of the three main triple-difference regressions. $Male * AgeGroup * Mandate$ is the estimate of the treatment effect on the main treatment group. Its coefficients represent the effect of mandates on the hourly wages, employment, and chance of having employer-provided health insurance for men aged 50 to 64. Each coefficient can be interpreted as a percentage change; for instance, the -0.028 coefficient for log wages can be interpreted as mandates causing a 2.8% reduction in the hourly wages of men over age 50.

The triple-difference regression estimates for the effect of prostate cancer mandates on the labor market outcomes of older men, therefore, are as follows: after the passage of a prostate cancer mandate, hourly wages decrease 2.8%, employment decreases 2.0%, and the chance of having employer-provided health insurance decreases 0.7%.

Table 2.1: Basic Estimates of the Effect of Prostate Cancer Screening Mandates on Labor Market Outcomes

	ln(HourlyWage)	Employed	Employer Insures
$Male * AgeGroup * Mandate$	-0.028***	-0.020***	-0.007***
	(0.010)	(0.006)	(0.003)
$Male * AgeGroup$	0.055***	-0.030***	0.011***
	(0.005)	(0.004)	(0.002)
$Male * Mandate$	-0.018**	0.013*	-0.002
	(0.006)	(0.007)	(0.002)
$AgeGroup * Mandate$	-0.001	0.021***	0.013***
	(0.007)	(0.004)	(0.002)
$Mandate$	0.013	-0.007*	-0.07**
	(0.0121)	(0.004)	(0.003)
$AgeGroup$	0.225***	-0.313***	-0.053***
	(0.019)	(0.005)	(0.005)
$Male$	0.271***	0.131***	0.015***
	(0.007)	(0.004)	(0.002)
State Fixed Effects	yes	yes	yes
Year Fixed Effects	yes	yes	yes
Observations	803,409	1,299,581	605,585

*Indicates p-values less than 0.10 **Indicates p-values less than 0.05 ***Indicates p-values less than 0.01; Values in parentheses are robust standard errors clustered by state. The coefficients for ln(Hourly Wage) are the results of an Ordinary Least Squares regression. The numbers reported for Employment and Employer Insurance are the marginal effects from a Logit regression, since the dependent variables are binary. Coefficients of demographic control variables are not shown (these include measures of age, race, ethnicity, education, and marital status for all regressions, as well as firm size and union membership in the wage and insurance regressions). The sample used is Americans aged 35-64. Person-level probability weights were used in all regressions to account for sampling bias.

Each result is significant at the 1% level. By contrast, the coefficient $Mandate_{st}$ gives an estimate of the effect of prostate cancer mandates on the general population. This effect is estimated to be much smaller and less significant.

Only data on individuals aged 35-64 were used in these regressions. Workers under 35 may not be a close control group for those over fifty, differing in unobserved ways. Individuals over 64 have access to Medicare, so changes in the private insurance market will have a less clear effect on them. However, a robustness check described in section 2.5.2 shows that the results remain significant with various specifications of age. The data used begin in 1990, two years before the first mandate was passed, and extends to 2009, the year the most recent mandate was passed. Probability weights are used to reflect the likelihood that each individual was sampled, as is standard in research using survey data. State and year fixed effects were included in each regression to control for the possibility of labor market shocks specific to any state or year.

2.5.1 Discussion and Welfare Analysis

Gruber (1992) developed a framework for the welfare analysis of a mandated benefit, which he applied to find that women put a value on maternity care coverage equal to its cost, a framework also used by Lahey (2012). Figure 2.3 demonstrates the idea. Mandates raise the total cost of compensating workers by C. The demand curve in terms of money wages for workers affected by the mandate will shift up by C, as in the case of a tax. This results in wages falling to W_2. If workers place no value on the mandate, then their labor supply curve remains the same, so the reduction in demand causes employment to fall to L_2. If instead workers value the mandate fully, their supply curve shifts down by the cost of the mandate C, balancing the shift in demand. Money wages fall to W_2', but hours worked remain steady. If workers value the mandate at cost, welfare is unchanged from before the mandate. If workers value the mandate below its cost C, welfare is reduced. The mandate functions like a tax equal to $(1-\alpha)*C$, where α is the worker's valuation of the mandate divided by its cost to employers. The left half of figure 2.3 represents the special case $\alpha = 0$, while the right half represents the special case where $\alpha = 1$.

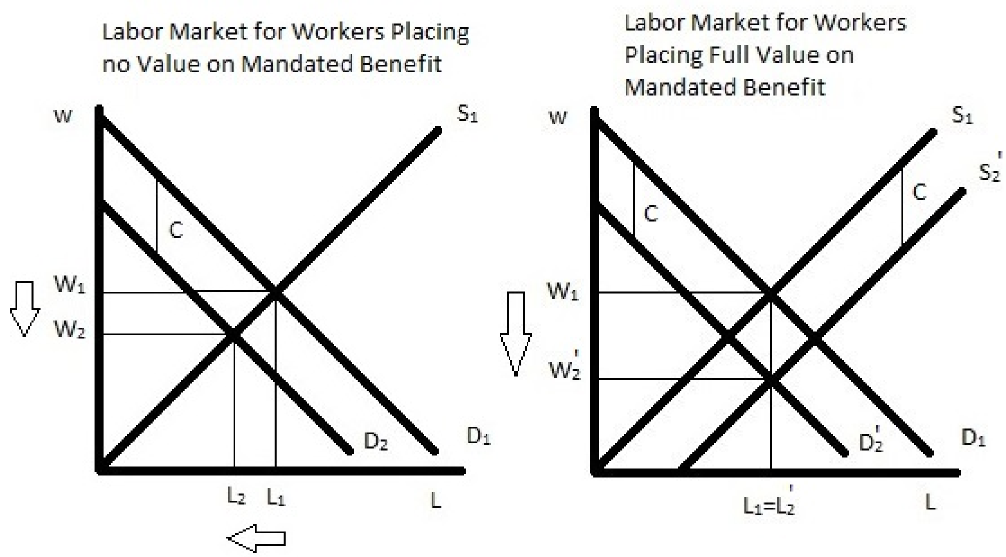

Figure 2.3: Labor Market Effects of Mandates with No Valuation vs Full Valuation

With this theoretical framework in mind, the first question the empirical work answers is whether the mandate is large and binding enough to noticeably raise compensation costs, and so shift demand and lower money wages. This paper found that this is indeed the case for prostate cancer screening mandates, which lower the wages of men over age 50 by 2.8%. The second question the empirical work can answer is how workers value the mandate and so what happens to welfare. If they value the mandate at its cost, then they perceive no change in the value of their compensation, only in how it is composed (more in health benefits and less in wages). However, if the workers value the mandated benefit at less than its cost, then the value of their total compensation has decreased (they lose more in wages than they gain in health benefits), and their hours worked and welfare decrease as a result.

The fact that there was a 2% decrease in the employment of men over age 50 after the passage of a mandate shows that workers do not value prostate cancer screening mandates at cost, and that welfare was reduced in their labor market. This, along with the significant reduction in employer-based health insurance coverage after a mandate, suggests that prostate cancer screening mandates are probably a poor policy, one which ends up hurting the very group it was intended to help.

However, there are two ways in which mandates could have better effects than these results seem to suggest. First, it is possible that men are incorrect to place such a low value on prostate cancer screening, and that the mandate actually improves their health enough to make them better off. But the medical research discussed in section 2.2 suggests otherwise: that men are probably correct in the low valuation they assign to screening. The mandates actually provide an excellent opportunity to study the disputed health effects of a marginal increase in screening; that question, however, is beyond the scope of this paper.

There is a second reason the broader welfare effects of the mandate may not be as bad as they may first appear, given the 2% drop in employment and the decrease in employer health insurance for men over age 50. The older men's jobs do not simply disappear; rather, so long as they are substitutes for other kinds of workers rather than complements, their place is taken by women and younger men. This paper found no significant effect of prostate cancer screening mandates on overall wages and employment for all workers (as measured by the coefficient of $Mandate_{st}$), suggesting that men over 50 are in fact close substitutes for women and younger men.

The fact that other workers are substitutes for older men also helps to explain why the estimated effects of the mandate are so large. Difference-in-difference estimation does not measure the absolute change in the wages and employment of older men, but rather the change *relative* to the comparison groups, the women and younger men whose wages and employment may increase following the mandate. The bulk of any negative welfare effect, then, likely comes from the decrease in the perceived value of compensation for the older men who kept their jobs, and from employers paying to make the transition to younger workers and women.

Further work using difference-in-difference estimation to study group-specific mandates must keep the possibility of complementary workers in mind. A paper could find no employment effect of mandates on the targeted group (as in Gruber (1994a)) either because there is no such effect, or because it is having an almost equal negative effect on the targeted workers and the complementary workers in the comparison group.

2.5.2 Robustness

Comparison Group

This chapter's triple-difference analysis has compared men aged 50-64 to women aged 50-64 and to men aged 35-49. It is possible that the results are sensitive to the choice of comparison group. Age 65 was chosen as an upper bound because it is the age when eligibility for Medicare begins, and age 35 was chosen as a lower bound to provide an age group spanning 15 years to mirror those aged 50-64. However, it is possible that younger people or people with Medicare actually are appropriate comparison groups. Alternatively, it is possible that the original analysis used too broad a range of ages to provide good control groups. A sensitivity analysis of the ages included is shown in Table 2.2. The estimated magnitudes of the coefficients experience moderate changes as new age groups are added or removed, but in each specification the effect of mandates on the labor market outcomes of older men remains significant. This suggests that the results are robust to various choices of comparison group.

Serial Correlation

Bertrand et al. (2004) describe how difference-in-difference estimation can lead to "significant" results much more often than is appropriate due to failures to account

Table 2.2: Estimated Effect of Mandates on Older Men when Various Age Groups are Used

	ln(HourlyWage)	Employed	Employer Insures
Ages 35-49, 50-64	-0.028***	-0.020***	-0.007***
	(0.010)	(0.006)	(0.003)
Ages 25-49, 50-64	-.036***	-.015***	-.009***
	(0.010)	(0.006)	(0.003)
Ages 40-49, 50-62	-.024***	-.020***	-.007*
	(0.008)	(0.005)	(0.004)
Ages 18-49, 50 and up	-.037***	-.011**	-.010***
	(0.011)	(0.005)	(0.003)

*Indicates p-values less than 0.10 **Indicates p-values less than 0.05 ***Indicates p-values less than 0.01; Values in parentheses are robust standard errors clustered by state. The coefficients for ln(Hourly Wage) are the results of an Ordinary Least Squares regression. The numbers reported for Employment and Employer Insurance are the marginal effects from a Logit regression, since the dependent variables are binary. Coefficients of control variables (including demographic controls, state and year fixed effects, and the terms needed for triple differencing) are not shown. Person-level probability weights were used in all regressions to account for sampling bias.

for autocorrelation. In fact, they find that uncorrected autocorrelation in difference-in-difference estimation can lead to false positives in as many as 62.5% of regressions. The critiques raised by Bertrand et al. (2004) are highly relevant to this analysis, since their focus was on other papers which also use DD techniques on Current Population Survey data covering many time periods. In response, this chapter has already taken several steps to avoid the pitfalls described by Bertrand et al. (2004). First and most obviously, this chapter uses triple-difference rather than double-difference estimation, and so has several close control groups. Second, clustered standard errors are used to account for serial correlation of outcomes within states. Bertrand et al. (2004) found that using standard errors clustered on states (and therefore allowing an arbitrary variance-covariance matrix) reduces the proportion of false positive findings of significance to only 1.3%. By using robust standard errors clustered on states, therefore, this chapter deals with the Bertrand et al. (2004) critique of difference-in-difference work and drastically reduces the probability that the significance of the results is

Table 2.3: Summary Statistics in 1990 for States Eventually Passing or Not Passing Mandate

	States with Mandate (29 States and DC)	States without Mandate (21 states)
Mean Age	34.1	35.1
Age 50-65	13.0%	13.7%
Male	48.7%	48.6%
Mean Hourly Wage	\$9.77	\$9.56
Mean Income	\$18,014	\$16,959
Employer Insures	82.4%	80.5%

Survey probability weights were used in calculations of means to account for sampling bias. Top-coded incomes were omitted in the calculation of mean income. Dollars are 1990 values unadjusted for inflation.

merely due to chance.

Endogeneity

Another possible concern is with endogeneity. The estimation strategy of this chapter effectively assumes that mandates were passed randomly. The estimates would be biased if the passage of mandates is in fact caused by the differences in the proportion of men over age 50 or in labor market outcomes across states. A simple, informal test of endogeneity is to look for systematic differences in demographics between states with and without mandates. The results of this comparison are shown in Table 2.3.

Overall, there seem to be small differences between the states that passed mandates and those that did not. States that passed mandates had a 1.9% higher rate of employer-based health insurance, which could indicate that more people in those states would benefit from a mandate. There is also a small difference in age between mandate and non-mandate states. However, the difference is in the opposite direction that one would expect if more people in the relevant interest groups lead states to pass mandates. States with prostate cancer mandates (which are intended to primarily benefit men over age 50) actually had a younger average population and fewer men age 50-65 as of 1990. It does not seem that demographic differences between states are large enough to cause major differences in the likelihood that a state would adopt a mandate. This informal comparison casts doubt on the possibility that endogeneity could be a major source of bias.

A slightly more formal method of assessing endogeneity is to include state-specific time trends in the main regressions, in addition to the state and year fixed effects already included. This allows for the fact that states may have already had a certain trend in wages, employment and employer-based health insurance before the passage of a mandate. Controlling for state-specific time trends should prevent the estimation from attributing to mandates what was really a pre-existing trend. The results of this exercise are shown in Table 2.4. The inclusion of state-specific time trends causes only very slight changes to the magnitude and significance of the coefficients, suggesting that the main results were not biased by endogeneity.

Another way of investigating the potential endogeneity problem is to examine the leading and lagged effects of the mandate laws. Angrist and Pischke (2009) describe how to perform a kind of Granger test on a difference-in-difference specification. If the mandates are estimated to cause significant changes before they are actually adopted, it would suggest that the results found in this chapter are simple pre-trends falsely attributed to the mandates. In general, we expect causes to happen before effects, though it is possible that employers could respond today to prepare for a law they expect to be passed the next year. Table 2.5 shows the estimated effects of mandates in the years before and after they are passed.

This test shows no significant evidence of a pre-trend where effects occur before their supposed cause, suggesting that there is no endogeneity problem nor any reaction in anticipation of the passage of a mandate. The test also shows that mandates have both an immediate and a persistent effect on employment. The estimated effect of the mandate on men over age 50 is significant in each year, and has a magnitude very

Table 2.4: Results with State-Specific Time Trends

	ln(HourlyWage)	Employed	Employer Insures
$Male * AgeGroup * Mandate$	-.028**	-.020***	-.007***
	(0.010)	(0.006)	(0.003)
$Male * AgeGroup$	.055***	-.030***	.011***
	(0.005)	(0.004)	(0.002)
$Male * Mandate$	-.018***	.012*	-.002
	(0.006)	(0.007)	(0.003)
$AgeGroup * Mandate$	.001	.021***	.013***
	(0.007)	(0.004)	(0.002)
$Mandate$	.015**	-.007	-.006**
	(0.007)	(0.005)	(0.003)
$AgeGroup$	0.225***	-.313***	-.053***
	(0.019)	(0.005)	(0.005)
$Male$	.271***	.131***	.015***
	(0.007)	(0.004)	(0.002)
State Fixed Effects	yes	yes	yes
Year Fixed Effects	yes	yes	yes
State-Specific Time Trends	yes	yes	yes
Observations	803,409	1,299,581	605,585

*Indicates p-values less than 0.10 **Indicates p-values less than 0.05 ***Indicates p-values less than 0.01; Values in parentheses are robust standard errors clustered by state. The coefficients for ln(Hourly Wage) are the results of an Ordinary Least Squares regression. The numbers reported for Employment and Employer Insurance are the marginal effects from a Logit regression, since the dependent variables are binary. Coefficients of demographic control variables are not shown (these include measures of age, race, ethnicity, education, and marital status for all regressions, as well as firm size and union membership in the wage and insurance regressions). The sample used is Americans aged 35-64. Person-level probability weights were used in all regressions to account for sampling bias.

Table 2.5: Leads and Lags Test

	ln(HourlyWage)	Employed	Employer Insures
$Male * Age * Mandate_{t-2}$	.005	.009*	.007**
	(0.017)	(0.005)	(0.004)
$Male * Age * Mandate_{t-1}$	.007	.010	.006
	(0.017)	(0.010)	(0.006)
$Male * Age * Mandate_{t}$	.012	-.019**	-.006
	(0.017)	(0.007)	(0.006)
$Male * Age * Mandate_{t+1}$	.018	-.016**	-.007
	(0.017)	(0.008)	(0.006)
$Male * Age * Mandate_{t+2}$	-.008	-.021***	-.011**
	(0.019)	(0.007)	(0.005)
$Male * Age * Mandate_{t+3forward}$	-.037***	-.019***	-.005**
	(0.011)	(0.006)	(0.003)

*Indicates p-values less than 0.10 **Indicates p-values less than 0.05 ***Indicates p-values less than 0.01; Values in parentheses are robust standard errors clustered by state. Law change dummies $Male * Age * Mandate$ from $t-2$ to $t+2$ are equal to one for only one year each, but $t+3$ is equal to one in every year beginning with the third year after adoption. The coefficients for ln(Hourly Wage) are the results of an Ordinary Least Squares regression. The numbers reported for Employment and Employer Insurance are the marginal effects from a Logit regression, since the dependent variables are binary. Coefficients of control variables (including demographic controls, state and year fixed effects, and the terms needed for triple differencing) are not shown. Person-level probability weights were used in all regressions to account for sampling bias.

close to the original estimate in the main specification. The test shows only a delayed effect on wages though, suggesting that they are somewhat sticky. The mandate has no significant effect on wages in the year it is passed or in the two years afterward, but does have a significant effect in the third and all subsequent years.

2.6 Conclusion

I find that prostate cancer mandates lead to statistically and economically significant declines in employment, wages, and insurance coverage for men over age 50. One lesson to take from this is that there are costs to health insurance benefit mandates, and that sometimes these costs fall precisely on those whom the mandate is intended to help. If mandated benefits increase costs to employers, they respond quickly by reducing other parts of the compensation package and substituting to mandate-exempt workers. A broader point is that prostate cancer mandates are one more example of labor market distortions caused by employer-provided health insurance. If health care were provided primarily through individual insurance, whether public or private, then mandates may result in inefficiently high medical spending but would not distort labor markets. But in the current US employer-based system, mandates may still lead to inefficiently high medical spending while also distorting labor markets. This paper demonstrates how prostate cancer mandates can distort the labor market for men over age 50. Prostate cancer treatment is only a small fraction of all medical care, but it demonstrates a general trend wherein employers are pushed away from any person or group predicted to have high health costs.

CHAPTER 3

HEALTH INSURANCE AND THE SUPPLY OF ENTREPRENEURS: NEW EVIDENCE FROM THE AFFORDABLE CARE ACT'S DEPENDENT COVERAGE MANDATE

Is the difficulty of purchasing health insurance as an individual or small business a major barrier to entrepreneurship in the United States? I answer this question by taking advantage of the natural experiment provided by the Affordable Care Act's dependent coverage mandate, which allowed many 19-25 year olds to acquire health insurance independently of their employment. This mandate provides a means to estimate the number of potential entrepreneurs discouraged by the current system of employer-based health insurance. A difference-in-difference strategy finds that the dependent coverage mandate led to a 13-24% increase in self-employment among the treated group. The effect is found to be larger for women and for unincorporated businesses. An instrumental variables strategy finds that those actually receiving health insurance coverage as dependents were much more likely to start businesses.

3.1 Introduction

The Affordable Care Act was signed on March 23rd, 2010. The dependent coverage mandate took effect six months later, requiring health insurance plans offering dependent coverage to extend coverage until the 26th birthday. Antwi et al. (2012) estimate that the mandate led 2 million young adults to gain health coverage through their parents. I use the mandate as a quasi-random natural experiment in breaking the link between health-insurance and employment for these young adults, a link that remains strong for most Americans. I investigate the extent to which Americans are reluctant to leave their wage-earning job and start their own business because it is relatively difficult and expensive to acquire health insurance as an individual or small business. I explain how health insurance enters into the decision to start a business with a simple theoretical model. Using a difference-in-difference approach and American Community Survey data from 2005-2011, I find that the dependent coverage mandate lead to a 13-24% increase in self-employment among the treated group (19-25 year olds).

This result is robust to a variety of tests novel to the small literature on health insurance and entrepreneurship, including the use of rare events estimators and a continuous definition of self-employment. I find that the result is largely driven by an increase in self-employment among women and an increase in unincorporated businesses. Using an instrumental variables strategy, I show the bias in simple approaches to estimating the effect of health insurance on self-employment. I show that receiving dependent coverage makes an individual two to three times more likely to start a business, a much larger effect than previous work has found.

3.2 Background

3.2.1 Health Insurance in the United States

Currently, most Americans receive health insurance through their employers. A large company is able to pool the risks of its employees, allowing it to self-insure without risking a high variance of claims, or to purchase insurance without insurers fearing adverse selection. Because of this, people who work for large businesses are more likely to have insurance and pay lower premiums for equivalent policies than those working for small businesses. Conversely, those looking for insurance as individuals or small businesses find insurance at significantly higher prices or not at all, as documented by Pauly and Lieberthal (2008). Because of this, Americans may be pushed toward working for large businesses and refrain from starting their own businesses out of concern for health insurance. A recent poll found that 9% of Americans under age 35 saw access to health insurance as a key barrier to starting a business

(Kauffman Foundation 2011).

3.2.2 The Affordable Care Act's Dependent Coverage Mandate

The Affordable Care Act (ACA) of 2010 provides a unique opportunity to estimate the causal effect of access to health insurance on entrepreneurship. The ACA introduced many changes to the US health care system, but most of its major provisions (the individual mandate, partial community rating, and guaranteed issue) do not take effect until 2014 or later. In contrast, the dependent coverage mandate (section 2714 of the Patient Protection and Affordable Care Act) was one of the first major provisions to go into effect, on September 23rd 2010. It requires insurers to offer coverage to the young adult (age 19-25) children of policyholders. Specifically, the law requires group plans that offer any coverage for children to extend coverage until their 26th birthday.

This dependent coverage mandate increased in the number of insured young adults by 2 million (Antwi et al. (2012)) to 3 million (Sommers (2012)). Many states had previously passed similar dependent coverage laws, but the ACA mandate has had a much larger effect. One reason for this is that self-insured plans (which cover roughly half of those with employer-based health insurance, according to the Medical Expenditure Panel Survey) are exempt from state mandates, whereas essentially all plans are subject to the federal mandate. A second reason is that the federal mandate is much broader. State laws had many restrictions: requiring the young adults to be full-time students, unmarried, or dependents for tax purposes, among other restrictions. The federal mandate applies to all those age 19-25 who have not been offered insurance through their own employer. Antwi et al. (2012) found that the ACA dependent coverage mandate led to a 30% increase in the likelihood that young adults were on their parents' insurance.

3.2.3 Previous Research

ACA Dependent Coverage Mandate

Three papers have used the Affordable Care Act's Dependent Coverage Mandate to study labor market outcomes. Slusky (2012) studies the effect of the mandate on labor supply and employment using a difference-in-difference approach on March Current Population Survey (CPS) data, and finds no significant effect. Depew (2012) also studies the effect of the mandate on labor supply and employment. He uses a difference-in-difference approach on data from the Survey of Income and Program Participation, and finds that the mandate significantly reduces labor supply and employment among 19-25 year olds. Bailey and Chorniy (2013) study the effect of the mandate on the job mobility of the non-self-employed. Using a difference-in-difference approach and CPS data, they find no significant effect.

Health Insurance and Entrepreneurship

Many previous papers have examined the effect of health insurance on job-to-job mobility. A survey by Gruber and Madrian (2002) showed that roughly half of these papers found significant evidence of "job-lock", reduced labor mobility due to the employer-based health insurance system. Most of these papers specifically excluded the self-employed from study. Only a handful of papers have examined the effect of health insurance on transitions to entrepreneurship specifically; this previous work has found mixed evidence for the hypothesis that lack of access to health insurance deters entrepreneurship.

Gruber and Madrian (2002) present a simple theoretical model explaining how employer-provided health insurance can lead to inefficiently low labor mobility. Holtz-Eakin et al. (1996), using data from the Panel Study of Income Dynamics and the Survey of Income and Program Participation, found that those who have health insurance through their spouses are not significantly more likely to start businesses than those who do not. Wellington (2001) used the same approach with data from the Current Population Survey and found that those with access to a spouse's health insurance are in fact 1.2-4.6% more likely to start a business. Fairlie et al. (2011), using data from the Current Population Survey, found that those with access to a spouse's health insurance are more likely to start businesses than those without. They also found that those just over age 65 (with access to Medicare) start more businesses than do those just under age 65.

One major flaw in the previous work on health insurance and entrepreneurship is the possibility of endogeneity and selection bias. Married people differ from non-married people in many ways, some of which are hard to observe and control for. Spouses who are willing and able to provide health insurance may be helping the entrepreneur in many unobserved ways. These studies may be attributing business creation to spousal health insurance when it is really due to other causes. This selection bias / omitted variable bias could lead to biased estimates of the effect of health insurance. The spousal insurance approach leaves the direction of causality unclear.

The Medicare approach used in Fairlie et al. (2011) addresses the endogeneity problem, since almost everyone obtains access to Medicare. But it is not clear to what extent their estimate of how many 65 year-olds start businesses when they get access to Medicare generalizes to tell us how people in general start businesses when they have access to health insurance. Younger entrepreneurs may differ in their decision-making process of whether to start a business. They are likely to care less about health insurance generally, since their short-term expected health costs are much lower. Furthermore, those with Medicare may act differently from those with access to private health insurance, which has different costs and benefits.

Another strand of the literature, exemplified by Heim and Lurie (2010) and Gruber and Poterba (1994), has examined the effect of changes in the tax deductibility of health insurance for the self-employed. Federal tax deductibility of health insurance for the self-employed increased in a series of steps from 0% before the Tax Reform Act of 1986 to 100% after 2003. These papers found that increases in tax deductibility led to substantial increases in self-employment. Their focus on changes in tax law does a great deal to overcome endogeneity problems. But taxes were only one of several barriers to self-employed individuals obtaining insurance. Others, such as adverse selection and search costs, remain even after tax treatment has equalized.

This chapter will estimate the size of the remaining barriers to health insurance, and will do so using a quasi-random natural experiment to overcome endogeneity.

3.3 Theory

3.3.1 A Simple Model of Self-Employment

Assume that workers place value on several kinds of compensation: wages W, employer health insurance H, and being a business owner B (which provides benefits such as not dealing with bosses). This results in the separable utility function:

$$U = \alpha H + \beta W + \gamma B \tag{3.1}$$

To simplify, assume that those starting their own firms get no health insurance ($H = 0$), earn the same wages W as those working for other firms, and get $B = 1$ higher other compensation from being their own boss. Those working as salaried employees get health insurance $H = 1$, earn the same wages W as the self-employed, and get no compensation in the form of being their own boss ($B = 0$). The value placed on wages, β, is the same for all workers. There is heterogeneity in the value placed on health insurance (α) and being a business owner (γ). These values are uniformly distributed with support from 0 to 1, and are distributed independently of each other.

Individuals face the choice of whether to start a business or work as an employee.

They choose the option that maximizes their own utility.

Employees get utility from their wages (W) and health insurance ($H = 1$):

$$U_E = \alpha + \beta W \tag{3.2}$$

While business owners get utility from their wages (W) and from the independence of owning their own business ($B = 1$):

$$U_B = \gamma + \beta W \tag{3.3}$$

Assuming that wages and the utility derived from wages are equal for both groups, the difference in utility for business owners is:

$$U_B - U_E = \gamma - \alpha \tag{3.4}$$

Therefore, an individual will start a business so long as their realization of the value of business ownership γ is greater than their realization of the value of employer health insurance α. If γ and α are independently and uniformly distributed between 0 and 1, then $\gamma > \alpha$ for half of individuals, and that half will start their own business.

3.3.2 Effect of the Mandate on Self-Employment

Now suppose the dependent coverage mandate makes it easier for some workers to get health insurance outside of the job. This reduces their value of employer health insurance by a factor δ where $0 < \delta < 1$. This results in utility:

$$U_M = \delta\alpha H + \beta W + \gamma B \tag{3.5}$$

The resulting difference in utility between business ownership and employment for individuals covered by the mandate is:

$$U_{MB} - U_{ME} = \gamma - \delta\alpha$$

Assume that δ, the discount in the valuation of employer health insurance caused by the mandate, is uniformly distributed, with an average value of $0 < \bar{\delta} < 1$. In this case, the proportion of self-employed people will increase by $1 - \bar{\delta}$ in the group affected by the mandate. For instance, if the mandate cuts the value of employer health insurance by half ($\delta = 0.5$), then self-employment for the group covered by the mandate will increase from 50% to 75%. If the mandate cuts the value of employer insurance by 10% ($\delta = 0.9$), then self employment will increase from 50% to 55% for the covered group. In this model, the mandate has no effect on the self-employment of those it does not directly apply to.

3.4 Data and Econometric Strategy

3.4.1 Data

This chapter uses several datasets to take advantage of each one's individual strengths. The primary dataset used is the Integrated Public Use Microdata Series (IPUMS) compilation of the American Community Survey (ACS) from 2005 to 2011. It has information about labor market outcomes (including self-employment) as well as extensive demographic controls. Its comparative advantage is its huge size: the ACS surveys over 3 million individuals per year. This is important because of my focus on a small subgroup: self-employed individuals aged 19-25. The full ACS dataset has over 21 million individuals but contains just under 50,000 self-employed individuals age 19-25. For some robustness checks I use the smaller Current Population Survey (CPS). One major advantage of the CPS is that it has information about the month in which individuals were surveyed. This allows me to look directly at the point in time when the law took effect (September 2010). The CPS also has a semi-panel structure, following individuals for a short time. This allows me to examine changes in self-employment at the individual level.

In both the ACS and CPS, individuals are coded as self-employed if they work more hours for their own business than for others.

For most specifications, the universe includes only 19-33 year olds. It is very rare for workers age 18 and under to be self-employed, and those over age 33 may be too different from 19-25 year olds to provide appropriate controls. Robustness checks show that the results are not sensitive to this narrowing of age groups.

3.4.2 Difference-in-Difference Estimation

The basic strategy of this chapter is to use difference-in-difference estimation to determine the effect of the dependent coverage mandate on self-employment. This means comparing people covered by the dependent coverage mandate (those age 19-25 after the mandate took effect in September 2010) to those not covered by the mandate. 26-year-olds are dropped from most regressions because they can be considered both treated and un-treated (the mandate does not apply to them, but at the same time as the mandate was implemented, tax deductibility of dependent coverage was extended until the 27th birthday). In effect, the difference-in-difference strategy uses control groups (19-25 year olds before the mandate took effect, and 27-33 year olds) to isolate the true effect of the mandates. This helps to prevent attributing to the mandate what is really due to changing economic conditions or due to young adults consistently starting fewer businesses than their older counterparts.

The first step is to generate a simple non-regression difference-in-difference estimate of the effect of the dependent coverage mandate. This is shown in Table 3.1. Following the implementation of the dependent coverage mandate in September 2010, 19-25 year-olds (who are covered by the mandate) increased their rate of self-employment by 0.062 percentage points relative to 27-33 year-olds (who are not covered by the mandate). However, this result could simply be due to the changing

Table 3.1: Basic Difference-in-Difference Effect of Dependent Coverage Mandate on Self-Employment

% Self-Employed	Before September 2010	After September 2010	Difference
19-25 year olds	2.358%	2.191%	-0.167%
% Self-Employed	Before September 2010	After September 2010	Difference
27-33 year olds	5.494%	5.265%	-0.229%
Difference	-0.167%	-0.229%	0.062%

Data from the 2009-2011 American Community Survey, retrieved using sampling weights.

composition of each age group. Therefore, the next step is to do a difference-in-difference logit regression which controls for additional variables such as race, gender and marital status. I use logit (and probit) because the dependent variable is whether someone is self-employed, which is a binary variable. The regression takes the form:

$$\begin{aligned} SelfEmployed_i = \beta_0 + \beta_1 * Mandate + \beta_2 * AgeGroup \\ + \beta_3 * Mandate * AgeGroup + \beta_4 * Controls + Error_i \end{aligned}$$

This logit regression gives the main result, where β_3 gives the estimate of the effect of the dependent coverage mandate on business creation by young adults. By using a difference-in-difference strategy that takes advantage of the natural experiment of the Affordable Care Act dependent coverage mandate, this chapter avoids the endogeneity and omitted variable problems that plague the previous literature. The natural experiment helps to overcome endogeneity: the Affordable Care Act extended the possibility of insurance uniformly, not only to those more or less likely to start businesses. The difference-in-difference strategy reduces the possibility of omitted variable bias by using similar control groups.

3.5 Results

The results of the main regressions are shown in Table 3.2. The coefficients for the variable *Treated* give the estimate of the treatment effect of the dependent coverage mandate. These specifications find that the mandate significantly increases the likelihood that 19-25 year olds are self-employed. Depending on the specification, the increase in self-employment is between 0.32 and 0.58 percentage points. The average rate of self-employment among 19-25 year olds over the entire period is about 2.4%, so the estimates imply a 13-24% increase. Because our dependent variable is binary (the ACS counts respondents as either self-employed or not), the linear probability model is less appropriate than the others, so more weight should be given to the logit and probit estimates of a 13-16% increase.

Table 3.2: Regression Difference-in-Difference Effect of Dependent Coverage Mandate on Self-Employment

	Linear Probability	Logit	Probit
Treated	.0058***	.0032**	.0038***
	(.0007)	(.0008)	(.0007)
After Mandate	-.0031***	-.0015*	-.0017**
	(.0009)	(.0008)	(.0008)
Age 19-25	-.0038***	-.0059***	-.0050***
	(.0006)	(.0006)	(.0006)
Observations	2,637,376		

Controls include age, number of children, state-year employment, and a time trend along with dummies for race, high school and college completion, marital status, and state fixed effects. Data is from the 2005-2011 IPUMS compilation of the American Community Survey. Data from 26 year olds and from the year 2010 have been dropped because they can be classified as being in both the treatment and control groups. The universe consists of 19-33 year olds who have ever worked. Robust standard errors clustered by household are given in parentheses. Coefficients reported for logit and probit regressions are the average marginal effects.

3.5.1 High-Growth Entrepreneurship

The results above simply estimate the effect of the dependent coverage mandate on the likelihood that the average 19-25 year old will become self-employed in a business of any kind. However, it is more reasonable to expect that the mandate has different impacts on various groups and various kinds of businesses.

When considering the benefits of entrepreneurship, researchers are especially interested in the kinds of businesses that are likely to bring growth and innovation. Previous research has found that incorporated businesses are more likely than unincorporated businesses to grow and hire additional employees (Henderson (2002)). About one-third of all businesses are incorporated. In order to determine how the dependent coverage mandate may have affected these two kinds of business differently, I re-run the main regression, but instead of the dependent variable being "self-employed," it is "self employed in incorporated business" or "self-employed in unincorporated business." Table 3.3 shows the results. The ACA dependent coverage mandate appears to have encouraged the formation of unincorporated, lower-growth businesses more than incorporated, higher-growth businesses. It is possible that as these new businesses grow they will eventually become incorporated.

A more direct way to examine high-growth entrepreneurship in the short run is to see how many of the new businesses have actually hired employees. This requires switching to Current Population Survey data, which asks about the size of the firm the employee runs or works for, and provides ranges such as "fewer than 11 employees" or "11 to 25 employees". Unfortunately, by not specifying exactly how many employees are in the smallest firms, the CPS conceals most of the variation for the self-employed. In the CPS data, only 15.9% of self-employed individuals report that their firm has 11 or more employees. The best option given the data limitations is to see whether workers treated by the dependent coverage mandate are more likely to have businesses with at least 11 employees. This appears to be the case: 20.5% of the self-employed in the treated group report having a business with at least 11 employees, compared to 15.8% of the control group.

Table 3.3: Effect of the Dependent Coverage Mandate on Incorporated vs Unincorporated Businesses

	Incorporated		
	Linear Probability	Logit	Probit
Treated	.0023***	.0003	.0006
	(.0003)	(.0005)	(.0004)
After Mandate	-.0009*	-.0002	-.0002
	(.0005)	(.0004)	(.0004)
Age 19-25	-.0010***	-.0017***	-.0013***
	(.0003)	(.0003)	(.0003)
Observations	2,637,376		
	Unincorporated		
	Linear Probability	Logit	Probit
Treated	.0035***	.0023***	.0026***
	(.0006)	(.0007)	(.0006)
After Mandate	-.0022***	-.0013*	-.0014***
	(.0007)	(.0007)	(.0007)
Age 19-25	-.0028***	-.0044***	-.0038***
	(.0005)	(.0005)	(.0005)
Observations	2,637,376		

Controls include age, number of children, state-year employment, and a time trend along with dummies for race, high school and college completion, marital status, and state fixed effects. Data is from the 2005-2011 IPUMS compilation of the American Community Survey. Data from 26 year olds and from the year 2010 have been dropped because they can be classified as being in both the treatment and control groups. The universe consists of 19-33 year olds who have ever worked. Robust standard errors clustered by household are given in parentheses. Coefficients reported for logit and probit regressions are the average marginal effects.

3.5.2 High-Tech Entrepreneurship

One popular image of entrepreneurship is a 20-something with an internet startup in Silicon Valley. This high-technology type of business is what many people hope for when they say they want to expand entrepreneurship. I use the occupation reported in the ACS to determine whether the dependent coverage mandate had a disproportionate effect on high-tech entrepreneurship. I count individuals as working in a "high-tech" occupation if their occupation code in the ACS is between 1000 and 1999. This range includes "Life, physical and health sciences," "architecture and engineering," and "computer and mathematical" occupations.

If anything, those affected by the mandate are actually less likely to start a high-tech business. Among self-employed 19-25 year olds after the passage of the mandate, 1.8% percent are in high-tech occupations. Among the control group, 2.4% are in high-tech occupations. While this difference is not statistically significant, it casts doubt on the idea that the mandate is leading to a surge in high-tech entrepreneurship.

3.5.3 Men and Women

The law may also have heterogeneous effects across men and women. Women are more risk-averse (see for instance Borghans et al. (2009)), have higher health insurance costs than men, and are less likely to start businesses (over the whole ACS sample, 6.2% of women are self-employed compared to 11.8% of men; among the group affected by the mandate, 2.0% of women are self-employed compared to 2.6% of men). Table 3.4 shows the results of the main regression when the sample is split into men and women. Women appear to have started 0.51-0.63 percentage points (25-32%) more businesses in the wake of the mandate. The results for men are about one third the magnitude in the logit and probit specifications, and not statistically significant for logit.

3.6 Robustness

3.6.1 Rare Events Estimation

Logit and probit are the most commonly used techniques for analyzing data with a binary dependent variable. But they work best when the data has close to equal numbers of 1's and 0's. They can be biased in small samples when one outcome is relatively rare. Because only about 2.5% of young adults are self-employed, logit and probit may be biased for this sample, even though it is not particularly small. This bias is reduced in the alternative techniques of rare events logit (relogit) and complementary log-log (cloglog) regression (see King and Zeng (2001) for one explanation).

Table 3.4: Effect of the Dependent Coverage Mandate on Self-Employment Among Men and Women

	Men		
	Linear Probability	Logit	Probit
Treated	.0054***	.0014	.0022**
	(.0010)	(.0012)	(.0011)
After Mandate	-.0036***	-.0017	-.0020*
	(.0013)	(.0012)	(.0012)
Age 19-25	-.0044***	-.0065***	-.0053***
	(.0009)	(.0009)	(.0009)
Observations	1,338,947		
	Women		
	Linear Probability	Logit	Probit
Treated	.0063***	.0051***	.0053***
	(.0009)	(.0010)	(.0009)
After Mandate	-.0026**	-.0012	-.0014
	(.0011)	(.0010)	(.0010)
Age 19-25	-.0034***	-.0052***	-.0047***
	(.0008)	(.0008)	(.0008)
Observations	1,298,429		

Controls include age, number of children, state-year employment, and a time trend along with dummies for race, high school and college completion, marital status, and state fixed effects. Data is from the 2005-2011 IPUMS compilation of the American Community Survey. Data from 26 year olds and from the year 2010 have been dropped because they can be classified as being in both the treatment and control groups. The universe consists of 19-33 year olds who have ever worked. Robust standard errors clustered by household are given in parentheses. Coefficients reported for logit and probit regressions are the average marginal effects.

Both assume that the error distribution follows an extreme value distribution. The results, given in Table 3.5, show that the magnitudes estimated by relogit and cloglog are very slightly smaller than the logit magnitude, and remain strongly statistically significant. It appears that the large sample size is able to reduce the rare event bias almost to zero.

3.6.2 State-Level Policies

By the time the federal dependent coverage mandate was passed, 29 states had implemented their own version of a dependent coverage mandate. Table 3.6 gives

Table 3.5: Effect of the Dependent Coverage Mandate on Self-Employment According to Rare Events Estimators

	Logit	Rare Events Logit	Complementary Log-Log
Treated	.0032**	0.0029***	.0031***
	(.0008)	(.0008)	(.0008)
After Mandate	-.0015*	-0.0013*	-.0014*
	(.0008)	(.0008)	(.0008)
Age 19-25	-.0059***	-0.0052***	-.0062***
	(.0006)	(.0005)	(.0006)
Observations		2,637,376	

Controls include age, number of children, state-year employment, and a time trend along with dummies for race, high school and college completion, marital status, and state fixed effects. Data is from the 2005-2011 IPUMS compilation of the American Community Survey. Data from 26 year olds and from the year 2010 have been dropped because they can be classified as being in both the treatment and control groups. The universe consists of 18-33 year olds who have ever worked. Robust standard errors clustered by household are given in parentheses. Coefficients reported are the marginal effects.

the state that passed these laws and the years they were implemented. State-level dependent coverage mandates have been studied by Depew (2012), Cantor et al. (2012), and Dillender (2013). The state mandates did not apply as widely as the federal mandate. States often applied their mandates only to students, or unmarried young adults, or those without their own children. In addition to states limiting the scope of their own mandates, the federal Employee Retirement Income Security Act of 1974 (ERISA) limits the reach of most state regulations to fully-insured firms. Firms which self-insure are exempt from the state dependent coverage mandate. These firms now cover just over half of all employees with employer-based health insurance. While the state mandates are narrower in scope than the federal mandates, it is reasonable to expect that they had some effect. This means the impact of the federal mandate should be larger in states which had not passed their own dependent coverage mandates. Table 3.7 shows that this is in fact the case. In states which had passed their own mandates, the estimated magnitude of the effect of the federal mandate is less than half as large, and is not statistically significant at conventional levels.

Table 3.6: Year State-Level Dependent Coverage Mandates Passed

State	Year	State	Year
Colorado	2006	New Hampshire	2007
Connecticut	2009	New Jersey	2006
Delaware	2007	New Mexico	2003
Florida	2007	New York	2010
Georgia	2006	Pennsylvania	2010
Idaho	2007	Rhode Island	2007
Iowa	2010	South Carolina	2010
Illinois	2004	South Dakota	2007
Indiana	2007	Tennessee	2008
Kentucky	2008	Texas	2005
Maine	2007	Utah	1995
Maryland	2008	Virginia	2007
Massachusetts	2007	Washington	2009
Minnesota	2008	West Virginia	2007
Missouri	2008	Wisconsin	2010
Montana	2008		

Source: Depew (2012)

3.6.3 Sensitivity to Ages Included

Table 3.8 shows the results of regressions where the age groups used are broadened and narrowed. The results are robust to including all age groups, but not to narrowing the age groups to 23-25 and 27-29 years olds in order to focus on more similar treatment and control groups. This may be because the value of the mandate is smaller to those close to the age cutoff at 26. A 25 year old only has one year of dependent coverage; the mandate should have a smaller effect on their decisions than those of a 19 year old who expects 6 years of dependent coverage.

3.6.4 What is Self-Employment?

Some individuals put much more effort into their businesses than others. Is the mandate encouraging people put many hours into self-employment, or to pursue hobby-style businesses? One way of accounting for this difference is to count people as self-employed only if they work a certain minimum number of hours per week, such as 30 or 40. In fact, these results are similar to the original specification.

Table 3.7: Effect of the Federal Mandate Based on Previous State Law

	States with Previous Mandate	States without Previous Mandate
Treated	.0016	.0038***
	(.0012)	(.0010)
After Mandate	.0001	-.0019*
	(.0014)	(.0010)
Age 19-25	-.0053***	-.0061***
	(.0012)	(.0007)
Observations	723,862	1,913,514

Controls include age, number of children, state-year employment, and a time trend along with dummies for race, high school and college completion, marital status, and state fixed effects. Data is from the 2005-2011 IPUMS compilation of the American Community Survey; data on state mandates is from Depew (2012). Data from 26 year olds and from the year 2010 have been dropped because they can be classified as being in both the treatment and control groups. The universe consists of 18-33 year olds who have ever worked. Robust standard errors clustered by household are given in parentheses. Coefficients reported are the average marginal effects of logit regressions.

But this binary approach still means ignoring most of the information about hours worked.

Self-Employment Defined on a Continuum

Although this thesis has considered several definitions of self-employment, the concept has still been treated in a black-and-white way, since people are coded as either self employed or not. One more nuanced definition is to consider the continuous variable, hours worked as self-employed. I construct this variable from ACS data by multiplying hours worked and whether individuals are self-employed. Table 3.9 shows the effect of the dependent coverage mandate on self-employed hours worked. The OLS results show that hours worked increased by about 0.25 (15 minutes). Given that hours worked as self-employed was initially very low (because almost everyone reports zero hours), this represents a 10% increase in total hours of self-employment. I also consider a tobit model that accounts for selection into self-employment. It shows that hours of self-employment increase by 2.67 per self-employed person.

Table 3.8: Robustness to Age Groups Used of Difference-in-Difference Effect of Dependent Coverage Mandate on Self-Employment

	Narrow Comparison (23-25, 27-29 year olds)		
	Linear Probability	Logit	Probit
Treated	.0012	-.0012	-.0007
	(.0010)	(.0010)	(.0010)
After Mandate	-.0012	-.0005	-.0006
	(.0008)	(.0009)	(.0009)
Age Group	-.0007	-.0006	-.0004
	(.0010)	(.0010)	(.0010)
Observations	1,129,146		
	Broad Comparison (All Ages)		
	Linear Probability	Logit	Probit
Treated	.0078***	.0008	.0026***
	(.0006)	(.0011)	(.0009)
After Mandate	-.0007**	.0003	.0002
	(.0003)	(.0003)	(.0003)
Age Group	-.0188***	-.0506***	-.0410***
	(.0003)	(.0005)	(.0004)
Observations	11,520,237		

Controls include age, number of children, state-year employment, and a time trend along with dummies for race, high school and college completion, marital status, and state fixed effects. Data is from the 2005-2011 IPUMS compilation of the American Community Survey. Data from 26 year olds and from the year 2010 have been dropped because they can be classified as being in both the treatment and control groups. The universe consists of 19-33 year olds who have ever worked. Robust standard errors clustered by household are given in parentheses. Coefficients reported for logit and probit regressions are the marginal effects.

3.6.5 Clustering, Weighting

Each previous specification has used robust standard errors clustered on households, a standard technique to account for intra-household correlations. The non-clustered standard errors reported in Table 3.10 are the same as the clustered standard errors used in the baseline specification to four significant digits. Bootstrapped standard errors, also reported in Table 3.10, are slightly smaller than traditional standard errors. Previous specifications have not used probability weights, because the universe consists of young adults and is not intended to represent to full population (see Solon et al. (2013)). However, the results are robust to the inclusion of

Table 3.9: Effect of Dependent Coverage on Hours Worked by Self-Employed

	Linear Regression	Tobit
Treated	.2482***	2.677***
	(.0265)	(.6356)
After Mandate	-.0807**	-1.027
	(.0349)	(.6876)
Age 19-25	-.1114***	-4.135
	(.0242)	(.5225)
Observations	2,637,376	2,637,376

Controls include age and a time trend along with dummies for race, high school and college completion, marital status, and state fixed effects. Data are from the 2005-2011 IPUMS compilation of the American Community Survey. Data from 26 year olds and from the year 2010 have been dropped because they can be classified as being in both the treatment and control groups. Robust standard errors clustered on households are given in parentheses.

probability weights.

Standard statistical software such as Stata (the program used for this thesis) uses approximations when computing the coefficient and standard errors of interaction terms in nonlinear models (such as logit and probit). Ai and Norton (2003) describe how the approximation can be misleading, and introduce their own Stata package, $Inteff$, which correctly computes interaction terms. Approximating the interaction term could be especially dangerous for this chapter, where the key variable of interest is an interaction term. Table 3.10 gives the results when $Inteff$ is used to precisely calculate the magnitude and standard errors of the treatment effect. The bias of the approximation turns out to be small in this case.

3.7 Effect on Individuals: Instrumental Variables

An instrumental variables approach brings together the theories underlying the previous estimation to make more general statements about dependent health insurance. The previous sections of this chapter examined the effect of the Affordable Care Act's dependent coverage mandate on self-employment. They assumed that the mechanism was that the ACA increased dependent coverage, which, in turn, increased

Table 3.10: Robustness to Clustering and Weighting

	No Clustering	Bootstrap cluster	Probability Weights	Inteff
Treated	.0032***	.0032***	.0020**	.0029***
	(.0008)	(.0007)	(.0010)	(.0007)
After Mandate	-.0015*	-.0015*	-.0017*	-.0015*
	(.0008)	(.0007)	(.0010)	(.0008)
Age 19-25	-.0059***	-.0059***	-.0047***	-.0059***
	(.0006)	(.0006)	(.0008)	(.0006)
Observations		2,637,376		

Controls include age, number of children, state-year employment, and a time trend along with dummies for race, high school and college completion, marital status, and state fixed effects. Data is from the 2005-2011 IPUMS compilation of the American Community Survey. Data from 26 year olds and from the year 2010 have been dropped because they can be classified as being in both the treatment and control groups. The universe consists of 19-33 year olds who have ever worked. Robust standard errors are given in parentheses. Coefficients reported are the average marginal effects of logit regressions.

self-employment. However, the difference-in-difference estimates did not actually incorporate any information on dependent health coverage. This means they suffer from two shortcomings. One is that they did not actually prove a direct link between health insurance and self-employment: they leave open the possibility that there was some other change affecting 19-25 year-olds around September 2010 that was the true cause of their self-employment. By contrast, an instrumental variables approach using the Affordable Care Act dependent coverage mandate as an instrument for health insurance can demonstrate the link directly. The second shortcoming of this chapter's previous estimates is the lack of generality of what they are estimating: the effect of the ACA dependent coverage mandate on self-employment. This is a good question to know the answer to, but in the end it is simply one policy change. It would be better to answer a more general question: what is the effect of acquiring dependent health coverage on self-employment? The instrumental variables approach can answer this question directly at the individual level in a way that the difference-in-difference approach could not.

The first stage of the the instrumental variables approach is to use the ACA dependent coverage mandate as an instrument for health insurance, as follows:

$$HealthInsurance_i = \beta_0 + \beta_1 * Treated_i + \beta_2 * Controls + Error_i$$

where $Treated_i$ is a dummy that is equal to one for 19-25 year-olds after September 2010 and equal to zero for all others. Ideally, $HealthInsurance_i$ would refer to whether people are covered by their parent's health insurance as a dependent. However, most datasets do not provide this level of specificity. The ACS says only whether an individual has health insurance (and whether it is public or private, and direct-purchase or employer based), not their relationship to the policyholder. The IPUMS compilation of the March CPS is more specific: it says whether an individual has group health insurance as a dependent. In the CPS data, 57% of those 19-25 have private health insurance of some kind, while 27% have coverage as a dependent. I first estimate the effect of having any private health insurance on self-employment using the ACS. The large size of the ACS is beneficial since there are relatively few treated subjects, and instrumental variables estimators are not very efficient. I then use the CPS to find the effect of dependent coverage on self-employment. The ACA dependent coverage mandate is a stronger instrument in the CPS case. Antwi et al. (2012) found that the mandate increased dependent coverage 2-3 times more than it increased total private insurance coverage among 19-25 year olds.

The second stage is as follows:

$$SelfEmployed_i = \beta_0 + \beta_1 * Health\hat{Insurance}_i + \beta_2 * Controls + Error_i$$

The results of the instrumental variables approach using ACS data are given in the second column of Table 3.11. The first column shows the results of an OLS regression. OLS finds a significant negative correlation between health insurance coverage and self-employment. Presumably the causation here is that self-employed people find it hard to get health insurance, rather than health insurance coverage somehow making people not want to start businesses. The instrumental variable results support this idea. They find that health insurance coverage has a significant positive effect on self-employment. Those who acquired health insurance as a result of the mandate were 3.51 percentage points more likely to be self-employed. This represents an increase of 77% relative to the 4.53% base rate of self-employment among 19-33 year olds in the ACS.

The IV equation is exactly identified. The F-statistic is 245, meaning the coefficients are strongly jointly significant.

Table 3.11: Instrumental Variable Estimates of the Effect of Health Insurance Coverage on Self-Employment

	OLS	Linear Instrumental Variables
$HealthInsurance_i$	-.033***	.0351***
	(.0004)	(.0123)
Observations	1,345,772	1,345,772

Controls include age and a time trend along with dummies for race, high school and college completion, marital status, and state fixed effects. Data are from the 2008-2011 IPUMS compilation of the American Community Survey; ACS began asking about insurance in 2008. Data from 26 year olds have been dropped because they can be classified as being in both the treatment and control groups. Data from individuals under age 19 and over age 33 have been dropped because they do not provide a close control for the treated group, individuals age 19-25. Clustered, robust standard errors are given in parentheses.

The instrumental variables results using CPS data are given in Table 3.12. The ordinary least squares results find a small but significant positive correlation between dependent health insurance coverage and self-employment. The instrumental variables approach finds a much larger effect: 7.81 percentage points (172%) instead of 1.67 percentage points (37%). These estimates suggest that de-linking health insurance from employment can have massive effects on the willingness to start a business.

As with the ACS estimates, the system of equations is exactly identified, and an F-test shows the first stage results to be strongly significant.

Table 3.12: Instrumental Variable Estimates of the Effect of Dependent Health Insurance Coverage on Self-Employment

	OLS	Linear Instrumental Variables
$HealthInsurance_i$	.0167***	.0781***
	(.0012)	(.0261)
Observations	249,454	249,454

Controls include state-year unemployment, age, and a time trend along with dummies for race, high school and college completion, marital status, and state fixed effects. Data are from the 2005-2013 IPUMS compilation of the Current Population Survey. Data from 26 year olds have been dropped because they can be classified as being in both the treatment and control groups. Data from individuals under age 19 and over age 33 have been dropped because they do not provide a close control for the treated group, age 19-25. Robust standard errors clustered by household are given in parentheses.

3.8 Policy Implications

The preponderance of evidence presented in this chapter suggests that a statistically and economically significant number of potential entrepreneurs are deterred from self-employment by the current employer-based health insurance system. When 19-25 year-olds gained access to health insurance unrelated to their employment, many chose to start businesses. But most people over age 25 do not get the same opportunity. This means that the health insurance system still discourages many people from starting their own businesses.

This thesis takes no stand on the optimal number of self-employed people. It is possible that there are other distortions in the economy pushing people toward self-employment, such as principal-agent problems and regulations that apply only to large firms, and that these outweigh distortions in the other direction, leaving overall self-employment too high. This would mean that the distortions caused by the health insurance system are actually beneficial. In the absence of other distortions though, the evidence in this chapter suggests that the health insurance system leads to too few self-employed Americans. This thesis leaves open the question of the best way to reduce the distortions of employer-based health insurance, except to say that the ACA dependent coverage mandate did increase self-employment. But there are many other policies that would allow people to find insurance outside of the employer-based system.

One commonly discussed alternative is government provision of insurance. This is already done for the poor (Medicaid) and elderly (Medicare). Fairlie et al. (2011) found that Americans are 13.8% more likely to own a business at age 65 than at age 64, and attributed this difference largely to the fact that Medicare eligibility starts at age 65, allowing people access to health insurance even when they leave their large-company jobs to start a business.

Another alternative is to make individual health insurance competitive with employer health insurance. One step toward this would be to equalize the tax treatment of individual and employer-based plans. Under current law employer-based plans are almost entirely exempt from income taxes, while most individual plans are not. Making individual and small-group plans competitive with large-group plans also means finding ways around the adverse selection problem. The Affordable Care Act of 2010 attempts to solve this problem with an individual mandate (everyone must buy health insurance, even if healthy) and guaranteed issue (insurers must sell policies to everyone, even if sick), which take effect in 2014.

A final alternative solution would be to enact health reforms that reduce the perceived necessity of health insurance. This could mean reductions in total health care spending, or the introduction of policies such as health savings accounts which make it easier to pay out of pocket. Any policy alternative that reduces the importance of employer-based health insurance is likely to increase self-employment, although this should be examined in detail for each proposed policy.

3.9 Conclusion

This chapter's main difference-in-difference specification finds that the Affordable Care Act's dependent coverage mandate led to a 13-24% increase in self-employment among 19-25 year olds. The estimate is robust to the use of several alternative estimators and definitions of self-employment. This result should be interpreted with caution for two reasons. One is that it is not robust to comparing only narrower age groups. The second is that even if this thesis did discover the true effect of extending insurance access for 19-25 year olds, the effect on other age groups may differ. Young adults have unusually low health care and individual health insurance costs, reducing the importance of large-group employer-based health insurance; the employer-based health insurance system is more likely to deter older individuals from starting businesses.

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