THREE ESSAYS ON THE ECONOMICS OF HEALTH REFORM

by

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Dedication

For Bacon and Isaac

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Table of Contents

List of Tables

List of Figures

Abstract

This dissertation presents evaluations of three pre-ACA policies resembling ACA reforms. In the first essay, "Entrepreneurship and job lock: the interaction between tax subsidies and health insurance regulations," I use variation across states and over time in state-level health insurance regulations and health insurance tax subsidies to show that the effect of a tax subsidy similar to the ACA's tax credits depends on the type on regulations that affect underwriting. I show that people with pre-existing conditions do not respond to tax subsidies in places where they cannot buy health insurance, but that they respond disproportionately in places where they can buy risk-rated health insurance. Most importantly for the ACA, in states in which people with pre-existing conditions face the same premium as healthy people, tax subsidies have no effect on either group. In the second essay, "How do minimum medical loss ratio rules affect insurance markets? Historical evidence from Michigan," I use the introduction of a minimum medical loss ratio rule in Michigan to show that a minimum MLR rule results in at least a six percentage point decrease in MLRs among for-profit commercial insurers. The reduction in commercial medical loss ratios was driven by a disproportionate increase in premium revenue. The findings of this paper suggest that the establishment of a minimum MLR rule can result in lower MLRs, possibly because of tacit collusion. Because the minimum MLR rule limits the price of health insurance, it is similar to a price ceiling, which has been shown to result in tacit collusion. In the third essay, "Do Pap smears reduce cervical cancer mortality? Evidence from federal family planning funding," I use quasi-experimental variation in the

availability of subsidized pap smears to estimate the effect of pap smears on cervical cancer mortality. I find that large increases in the likelihood of receiving a Pap smear in the last year do not correspond to changes in cervical cancer mortality or incidence, but the estimates are too imprecise to rule out existing estimates from the literature.

Chapter 1 Introduction

On March 23rd, 2010, President Barack Obama signed into law the Patient Protection and Affordable Care Act (ACA), the first successful attempt at major health reform in nearly half a century. At the time of its passage, the need for health reform was particularly urgent. The rate of employer-sponsored insurance coverage had steadily declined since it was first measured in 1987, covering only 55 percent of Americans in an employer-based health insurance system (DeNavas-Walt, Proctor, and Smith 2011). At the same time, the private alternative to employer-sponsored coverage—insurance purchased directly from insurers in the non-employer market—was often difficult to obtain. Because these markets are characterized by adverse selection, insurance premiums were high relative to comparable employer-sponsored plans, and many individuals with high medical costs were excluded from the market altogether. As a result, nearly 55 million Americans, or 16 percent of the population, lacked health insurance coverage, putting them at risk of bad health outcomes and financial insecurity (DeNavas-Walt, Proctor, and Smith 2011; Council of Economic Advisers 2011).

The main goal of the ACA is to expand coverage to 32 million people, and about half of this increase is expected to come from reforms that affect the private health insurance market (Congressional Budget Office 2010). The cornerstones of private health insurance market reform are large subsidies to reduce the price of health insurance for middle-

income Americans, and a host of new rules affecting the characteristics of plans insurers may offer, and to whom they must offer. These rules include limits on how insurers set premiums, a ban on health insurance denials, and requirements that insurers spend a minimum fraction of premiums on benefits and cover a basic set of benefits (Kaiser Family Foundation 2013b).

These aspects of the law are controversial and have major economic implications. Proponents of these reforms say they will increase access to medical care, improve health and financial stability, and create jobs (Council of Economic Advisers 2012, 2009a). Opponents of the reforms say they will increase spending, have no effect on health, and destroy jobs (John Boehner 2011). Because many of the ACA's key provisions did not go into effect until 2014, many of the health and economic impacts of the law remain an open question. Given uncertainty over the effects of the law, health policy researchers must find similar policy experiments in the past and use causal inference techniques to estimate their impact and predict what will happen under the ACA.

This dissertation is composed of three such policy evaluations. I am concerned with the effect of aspects of private health insurance market reform on labor markets, health insurance markets, and health. In each of three essays, I use comparable state- or local-level policy to evaluate three claims of policymakers: that the ACA will result in increased self-employment, that the ACA will lower premiums and increase the value of insurance, and that the ACA will result in better health.

In the first essay, "Entrepreneurship and job lock: the interaction between tax subsidies and health insurance regulations," I estimate the effect of health insurance tax subsidies for the self-employed on the probability of self-employment,

accounting for interactions with state health insurance regulations affecting both the price of insurance and whether insurance is offered at any price. Because people with health conditions often face higher prices and/or limits on coverage, or are denied health insurance coverage at any price in the individual, non-employer health insurance market, they cannot benefit from tax subsidies designed to encourage self-employment.

Using federal and state variation in health insurance tax deductions for the self-employed between 1999 and 2005, I show that the effect of subsidies depend crucially on state insurance market regulation, especially for people with pre-existing conditions. I derive a simple model of the choice between employment and self-employment as a function of the after-tax price of health insurance, the relationship between health status and premiums, and individual health expenditures to illustrate that the effect of tax subsidies depends on coverage options in the non-group market. The model predicts that tax subsidies have no effect on self-employment for people with pre-existing conditions in markets in which pre-existing conditions are grounds for coverage denial. In contrast, tax subsidies have a stronger effect for people with pre-existing conditions, compared to those without, when they can purchase coverage at a higher price. The model predicts that people with and without pre-existing conditions respond similarly to one another in markets in which premiums do not vary with health status.

I operationalize the theoretical model empirically with a specification that allows treatment effects of tax subsidies for self-employed health insurance to differ by the presence of a pre-existing condition and the price and accessibility of non-group market insurance. Empirical results using the Panel Study of Income Dynamics confirm the model's predictions: households with pre-existing conditions are more likely to be self-employed in response to tax deductions than are households with healthy members in states in which they can purchase risk-

rated health insurance. But, they do not respond at all in states in which they should expect to be denied a policy altogether. In states in which insurers are prohibited from denying insurance or using health information to set premiums, health insurance tax deductions appear to have no effect on self-employment for either type of household.

In the second essay, "How do minimum medical loss ratio rules affect insurance markets? Historical evidence from Michigan," I estimate the effect of a minimum medical loss ratio rule similar to that included in the ACA. The ACA requires all insurers to spend a minimum fraction of premium revenue on medical care and quality improvement. This fraction is called a medical loss ratio. Although policymakers predict that the rule will result in higher medical loss ratios, the rule creates complex incentives that could actually result in lower loss ratios. I use the introduction of a minimum medical loss ratio rule in Michigan in 1974 to measure the effect of this type of policy on loss ratios. I outline a conceptual framework that illustrates how minimum loss ratio rules can result in tacit collusion and why the medical loss ratio may fall among for-profit insurers as a result. Then I estimate the effect of the rule on Michigan's for-profit insurers using state-level data on average insurer loss ratios from Health Insurance Association of America member surveys. The results suggest that the average loss ratio fell by at least six percentage points among for-profit insurers in Michigan. Using state-level data on the components of the loss ratio, I show that for-profit insurers in Michigan achieved decreases in the loss ratio through a disproportionate increase in premiums.

In the third essay, "Do Pap smears reduce cervical cancer mortality? Evidence from federal family planning funding," I explore whether Pap smears, a cancer screening that the ACA requires insurers to cover without any cost-sharing, affect cervical cancer mortality. Although the Pap smear is widely credited for dramatic reductions in cervical cancer

over the 20th century, time-series evidence does not suggest it played a large role in these reductions. Due to the ethical challenges of withholding a presumed beneficial cancer screening, the test has never been evaluated in a randomized controlled trial, and observational studies that claim to support its effectiveness suffer from internal validity problems that preclude causal inference.

In this essay, I exploit a natural experiment in the availability of subsidized reproductive care to establish the true effect of Pap smears on cervical cancer mortality. Leveraging the chaotic nature in which initial federal grants for family planning clinics were administered to community organizations between 1965 and 1973, I find that an 8-13 percent increase in use of cervical cancer screening as a result of funding did not result in statistically significant changes in cervical cancer mortality. However, the results are inconclusive because the estimates are imprecise.

Chapter 2 Entrepreneurship and job lock: the interaction between tax subsidies and health insurance regulations

[The Affordable Care Act] will unleash tremendous entrepreneurial power into our economy. Imagine a society and an economy where a person could change jobs without losing health insurance; where they could be self-employed … or start a small business. Imagine an economy where people could follow their passions and their talent without having to worry that their children would not have health insurance -- that if they had a … pre-existing medical condition in their family, that they would [not] be job locked. Under this bill, their entrepreneurial spirit will be unleashed.

--Nancy Pelosi (2010)

Introduction

Economists have long speculated that the connection between employment and health insurance discourages the would-be self-employed from striking out on their own (Council of Economic Advisers 2009a; Government Accountability Office 2012b). This feature of the labor market is part of a larger phenomenon of reduced job separations associated with employer-sponsored benefits, called "job lock"(Madrian 1994). While job lock affects mobility between jobs and decisions to become retired (Gruber and Madrian 2002), it should especially inhibit transitions from jobs with health insurance to self-employment because the alternative to employer-sponsored insurance—"non-group" coverage purchased directly from an insurer—is both costly (Whitmore et al. 2011) and sometimes completely inaccessible for people with health conditions (Kaiser Family Foundation 2012). Since the 1990's, Federal and state policymakers have responded to self-employment related job lock with either insurance market reforms or tax-based health insurance subsidies. Both types of policies try to alleviate job lock by lowering the

cost of non-group coverage. One of the primary objectives of the Affordable Care Act (ACA) is to lower the cost of non-group coverage, and therefore reduce job lock, through a combination of both non-group insurance market reforms and health insurance tax subsidies for non-group coverage (Kaiser Family Foundation 2013b). The combination of these policies is expected to increase self-employment, especially among those with pre-existing health conditions (Council of Economic Advisers 2009b; Government Accountability Office 2012b), by 1.5 million people after 2014 (Blumberg, Corlette, and Lucia 2013).

In this paper, I measure the effect of health insurance tax subsidies for the self-employed on individuals' probability of self-employment. Using Federal and state variation in health insurance tax deductions for the self-employed between 1999 and 2005, I show that this effect depends crucially on state insurance market regulation, especially for people with pre-existing conditions. I derive a simple model of the choice between employment and self-employment as a function of the after-tax price of health insurance, the relationship between health status and premiums, and individual health expenditures to illustrate that the effect of tax subsidies depends on coverage options in the non-group market. The model predicts that tax subsidies have no effect on self-employment for people with pre-existing conditions in markets in which pre-existing conditions are grounds for coverage denial. In contrast, tax subsidies have a stronger effect for people with pre-existing conditions, compared to those without when they can purchase coverage at a higher price. The model predicts that people with and without pre-existing conditions respond similarly to one another in markets in which premiums do not vary with health status.

I operationalize the theoretical model empirically with a specification that allows treatment effects of tax subsidies for self-employed health insurance to differ by the presence of

a pre-existing condition and the price and accessibility of non-group market insurance. Empirical results using the Panel Study of Income Dynamics confirm the model's predictions: households with pre-existing conditions are more likely to be self-employed in response to tax deductions than are households with healthy members in states in which they can purchase risk-rated health insurance. But, they do not respond at all in states in which they should expect to be denied a policy altogether. In states in which insurers are prohibited from denying insurance or using health information to set premiums, health insurance tax deductions appear to have no effect on self-employment for either type of household.

This paper makes both theoretical and empirical contributions to the literatures on health insurance tax subsidies and self-employment, and non-group market regulations and self-employment. My theoretical framework is the first to formalize the connection between health insurance tax subsidies, non-group market regulations, and heterogeneous health status in a model of self-employment and health insurance. It also is the first in these literatures to incorporate a well-known feature of the non-group market: that people with pre-existing conditions are often denied health insurance altogether, even when insurers could charge them higher premiums. I show that state-based limits on the use of riders to exclude coverage for pre-existing conditions play a role in insurance denials. This paper's further, empirical contribution is to construct measures of pre-existing conditions and use them to estimate separately the treatment effect of non-group market tax subsidies for households that can and cannot purchase non-group market health insurance.

Furthermore, these findings have important policy implications. First, they provide an evaluation of the effects of a Federal and state tax policy intended to increase self-employment through health insurance tax deductions. The results suggest that the benefits of this tax subsidy

for the self-employed accrued unevenly across households by health status. In particular, households that contained a member with a pre-existing condition did not benefit from tax subsidies at all in places where they could likely not purchase health insurance. In effect, this policy magnified the disadvantages of being excluded from the non-group market for people with pre-existing conditions.

Second, the results can inform current policy debates in several ways. The results provide support for the ACA's two-pronged approach—non-group market reforms and health insurance tax subsidies for non-group insurance. Efforts to lower the cost of health insurance through tax policy must be paired with regulations that prevent insurers from denying coverage to people with pre-existing conditions. Otherwise, this group cannot benefit from the tax subsidy at all. Recent efforts to selectively repeal parts of the ACA should therefore be viewed with caution.

The results reported in this paper also help inform projections of the likely impact of the ACA's tax subsidies on self-employment. Because health insurance tax subsidies for the self-employed have little effect on self-employment in states with regulations that most closely mirror those enacted by the ACA, the estimates imply that the ACA's premium tax subsidies may also have little effect on self-employment.

Background

Job lock and self-employment

In the United States, 90 percent of private coverage among non-elderly adults is purchased as a fringe benefit of employment through an employer (DeNavas-Walt, Proctor, and Smith 2011). Purchasing coverage through an employer (group coverage) is significantly less

expensive than purchasing it directly from an insurance company (non-group coverage). The link between health insurance and employment reduces job separations as workers are reluctant to leave jobs with group coverage for fear of increased premiums or loss of coverage altogether (Madrian 1994). Along with reducing worker mobility between jobs, job lock especially deters transitions from employment to self-employment because of the considerable difference in cost between group and non-group coverage (Whitmore et al. 2011).

The price difference exists for three reasons: economies of scale in plan administration, greater tax advantages to employer-sponsored coverage, and favorable risk pooling for workers with health conditions in group coverage. Relative to the self-employed or employees of small firms, employees of large firms enjoy economies of scale in plan administrative costs. The difference between the expected payout and premiums, or load, depends on the size of the group purchasing coverage. If the firm is large, the administrative costs associated with administering the plan are small relative to individual premiums. However, if the firm is small, overhead costs may constitute a large fraction of the premium. Karaca-Mandic, Abraham, and Phelps (2011) estimate that loads represent only four percent of the premium for firms with greater than 10,000 employees, but roughly one-third of the premium for firms with fewer than 100 employees. Loads create an inverse relationship between premiums and firm size.

Secondly, while the employed enjoy a substantial tax advantage on their premium costs, the self-employed do not. In group coverage, the employer excludes its share of the premium payments from payroll tax and employees do not pay income tax on the employers' contributions. In most cases, employees may also pay for their share of premiums with pre-tax dollars if the employer sponsors a Section 125 plan (Thomasson 2003; Lang and Kang 2007). In contrast, the tax code provided no health insurance subsidy for the self-employed until 1986.

And while the introduction of Federal and state health insurance premium deductions for the self-employed between 1986 and 2004 lessened the disparity, the value of this tax subsidy today is much less than that of the tax advantage enjoyed by the employed (Selden 2009).

Finally, group coverage is relatively less expensive for workers with health conditions because it spreads risk across employees, subsidizing the care of high-cost employees with premiums from low-cost employees. In contrast, non-group coverage may directly reflect the health status of the individual purchasing coverage. This practice is called "medical underwriting" and it results in higher premiums or full denial of coverage for individuals with a history of high-cost medical conditions, or "pre-existing conditions." Since job lock appears to be a serious impediment to self-employment in the United States, policy makers have considered a variety of reforms to reduce it.

Remedies for job lock: non-group market reforms

One approach to reducing job lock seeks to decrease the cost of non-group coverage for self-employed people with pre-existing health conditions through reforms to medical underwriting practices. Such reforms make health insurance more affordable and accessible to individuals with pre-existing health conditions. For example, the adoption of community rating requires that insurers must charge everyone the same price for health insurance regardless of health status. Paired with guaranteed issue, insurers must also issue insurance to every person who applies. Medical underwriting reforms determine not only the price of insurance for people with health conditions but also whether it is offered at any price.

The research on non-group market reforms and self-employment suggests that such reforms have little effect. Comparing states that adopted community rating and guaranteed issue

in the 1990's to those that did not, Heim and Lurie (2013a) find no increase in self-employment on average. Using a similar strategy comparing New Jersey to other states in the region, DeCicca (2007) finds that a package of non-group market reforms—including community rating and guaranteed issue—increased self-employment by 15-25 percent in the first three years of the reform. But DeCicca's result appears to be temporary; with the inclusion of several more years of post-policy observations, Heim and Lurie (2013a) show that the increase in self-employment did not persist.

Remedies for job lock: health insurance tax deductions

The second type of policy tries to reduce the cost of non-group health insurance through Federal and state premium tax deductions for the self-employed. Deductions reduce the after-tax cost of health insurance. Before 1986, self-employed people who purchased non-group coverage paid the full cost of health insurance without any tax subsidies. Beginning with the Tax Reform Act of 1986, the Federal government allowed self-employed people without access to employer-sponsored coverage to deduct a fraction of health insurance costs from their income tax returns. Over the following years, most states followed suit. By 2004, the self-employed could deduct the full cost of health insurance from both Federal and state income tax returns in most states (Selden 2009).

Several papers find that tax deductions increase a variety of self-employment measures. Velamuri (2012) finds that the introduction of a partial Federal deduction in 1986 increased self-employment by 14 to 25 percent among women in the Current Population Survey. Using a panel of tax returns, (Heim and Lurie 2010) find that self-employment, measured by the amount of self-employment income reported on tax returns, increased by about 15 percent between 1999

and 2004. Gurley-Calvez (2011) finds that deductions decrease self-employment exit by seven percent during the same period.

Interaction of non-group market reforms and tax subsidies

Health insurance tax subsidies and non-group market regulations have generally been studied separately, but the primary contribution of this paper is to describe and estimate the ways in which these two policies interact. While tax-based reform provides a subsidy that lowers the after-tax cost for all self-employed taxpayers, the value of the subsidy depends on the pre-tax premium. People who pay higher premiums benefit more from deductions. Depending on the type of medical underwriting permitted by law, people with health conditions pay higher pre-tax premiums or are not offered a policy at all. The consequence of this interaction is that the benefits of health insurance deductions range from greater than average when people with pre-existing conditions pay risk-rated premiums, to zero when they cannot purchase a policy at any price.

Only one paper attempts to study the combined effect of both policies. Using a panel of tax returns, Heim and Lurie (2013b) separately estimate the effect of deductions on self-employment in states with heavy, moderate, and light levels of non-group underwriting reforms that affect the price of insurance. They find that the effect of tax subsidies is different in states with heavy regulation compared to those with light or moderate regulations. The authors attribute differences in the effect to differences in the pre-tax premiums induced by different amounts of regulation. Consistent with this observation, they find that the effect among taxpayers over age 40 is strongest in the heavily regulated states in which premiums are not permitted to vary substantially with age.

These results are suggestive, but cannot fully capture the interaction between insurance regulation and tax policy. First, state-level restrictions on non-group market underwriting practices lead to variation in not only the price, but also the probability of being offered a policy *at any price*. This feature of non-group markets is important because in states where underwriting practices result in denial of coverage for individuals with pre-existing conditions, tax subsidies have no benefit. Pre-existing conditions-based coverage denials are likely common because 60 million working Americans are estimated to have a pre-existing condition (Government Accountability Office 2012a) and one in seven applications to the four largest insurers is denied because of a pre-existing condition (Waxman and Stupak 2010). Second, identifying individuals who pay more for non-group coverage or are excluded from the non-group market altogether requires data on past health status, which are not available in most datasets. Although insurers use demographic information such as age to set premiums, they rely primarily on medical history when medical underwriting is permitted (National Association of Insurance Comissioners 2012).

I also incorporate differences across states in the treatment of the self-employed in the group market for small employers (small group). Although states may be classified as having heavy, moderate, and light levels of non-group underwriting reforms, within each group, there are some states that allow the self-employed to participate in the small group market as a "group-of-one." Beginning in 1996, the Health Insurance Portability and Accessibility Act established nationwide limits on medical underwriting in the small-group market. While the price of coverage was still allowed to vary with health status, the law required insurers to offer coverage to all applicants and prevented insurers from denying coverage to people with pre-existing conditions (Kaiser Family Foundation 2013a). For this reason, self-employed people who live in

states that 1) allow non-group market insurers to use medical underwriting, and 2) allow self-employed people to participate in the small-group market can be expected to purchase coverage in the small-group market. Because the small-group market has more protections to assure affordability and accessibility of coverage for people with health conditions than the states with light or moderate regulation of the non-group market, self-employment should increase more among this group. Including people who purchase insurance in the small-group market instead should overstate the effect of tax subsidies in lightly regulated states.

The following sections outline a theoretical model and empirical strategy that account for each of these issues. I model the self-employment response to tax subsidies in states with different kinds of insurance regulation, paying special attention to the legality of denying coverage. The model generates predictions for workers with and without pre-existing conditions in different regulatory environments. Next, I construct a unique dataset from the Panel Study of Income Dynamics that contains data on individual health histories as well as state insurance regulations, which allow me to test the predictions of the theoretical model.

Theoretical framework

Individuals with heterogeneous health risks should respond differently to health insurance tax subsidies in the presence of heterogeneous non-group market regulations. Heim and Lurie (2013b) suggest this idea without formalizing it, by noting that the decision to become self-employed should vary with the change in the price induced by regulation across states. I formalize the decision as a function of factors that affect both the price and accessibility of health insurance: the tax price of health insurance in each sector, presence of pre-existing conditions, state non-group market regulations affecting risk-rating and denials, and their interactions.

For the self-employed, the value of a tax subsidy depends on the pre-tax price of health insurance. More formally, assume individuals have preferences over wages net of health insurance costs in the self-employed and employed sectors ($j \in (se, e)$): $U_i^j = U(w_i^j - c_i^j)$. w_i^j is the wage individual i receives in each sector, j, and c_i^j is the individual's after-tax cost of health insurance. I assume that wages are fully offset by employer contributions and employers pay for the entire premium. Therefore w_i^e is the wage minus the employer-sponsored premium for employed workers. If all individuals purchase health insurance, then $w_i^j - c_i^j$ are the wages net of the total after-tax cost of health insurance. I assume increasing utility ($U' > 0$) and decreasing marginal utility ($U'' < 0$).

Tax prices

The after-tax cost to the individual is a function of the tax price of health insurance, p_i^j, and before-tax premiums in each sector, π_i^j:

(1) $c_i^j = p_i^j \pi_i^j$

The tax price describes the reduction in the unit after-tax price of a good as a function of marginal tax rates and tax subsidies for the good. For example, if the tax price of health insurance is 0.4, it means that the taxpayer pays only 40 cents for a dollar's worth of health insurance after health insurance tax subsidies have been taken into account. Put another way, if the tax code did not subsidize health insurance at all, the tax price would be equal to one. The tax price of health insurance in the self-employed sector is $1 - \tau_f \theta_f - \tau_s \theta_s$ where τ_f and τ_s are federal and state marginal tax rates, and θ_f and θ_s are the fractions of premiums self-employed

persons may deduct from federal and state income taxes. As the deductions increase, the tax price of health insurance for the self-employed falls for a given pair of τ_f and τ_s. Recall that θ_f and θ_s vary exogenously over time and across states. The tax price in the employed sector is $\frac{1-\tau_f-\tau_s-\tau_p}{1+\tau_p}$ where τ_p is the marginal tax rate the employer pays on benefits, or the payroll marginal tax rate. The employed tax price expresses the cost of health insurance to the employee in terms of foregone wages. Because employer-sponsored premiums are excluded from taxable income, employers are indifferent between paying one dollar of benefits and $\frac{1}{1+\tau_p}$ dollars in wages (Gruber and Poterba 1994). The employee's wages are reduced by $\frac{\pi^e(1-\tau_f-\tau_s-\tau_p)}{1+\tau_p}$, and therefore p_i^e expresses how the fraction of premiums paid changes with marginal tax rates.

Pre-tax premiums

While premiums for the employed (π^e) do not vary at the individual level, premiums in the non-group market (π_i^{se}) can, depending on the type of regulatory environment. Non-group insurance markets are at risk for adverse selection. This is a problem for insurers because adverse selection can result in large financial losses and destabilize markets (Rothschild and Stiglitz 1976). To mitigate adverse selection in the non-group market, insurers can charge higher prices for people with pre-existing conditions, exclude coverage related to those conditions, or refuse to offer these people coverage altogether. The legality of these tools is determined by three types of non-group market regulations: community rating, guaranteed issue, and prohibitions on elimination riders. Community rating and guaranteed issue (as described above) prohibit insurers in the non-group market from varying premiums with health risk and require insurers to offer health insurance to all who apply. While the effect of community rating and

guaranteed issue on a variety of non-group market outcomes has been studied previously (Herring and Pauly 2006; Heim and Lurie 2013a; LoSasso and Lurie 2009), the last type of policy—prohibitions on elimination riders—has not been adequately studied.

Elimination riders are limits on coverage written into a health insurance policy that carve out coverage related to a pre-existing condition (Kaiser Family Foundation 2012). Prohibitions on elimination riders prevent insurers from placing such limits on coverage. Anecdotal evidence suggests they play an important role in determining coverage accessibility for people with pre-existing conditions (Kaiser Family Foundation 2001). If elimination riders are permitted, insurers can exclude coverage related to the pre-existing condition either temporarily or permanently. With such exclusions, people with pre-existing health conditions can at least purchase coverage for other types of care unrelated to the condition. Recent work by Hendren (2013) suggests that insurers deny coverage to people with pre-existing conditions because they have more private information about their own health risks than people without such conditions. Therefore, insurers choose not to offer people with pre-existing conditions coverage, even at an actuarially fair premium. Elimination riders may allow insurers to sell insurance to people with pre-existing conditions while limiting the probability of large losses. Indeed, elimination riders are commonly used among the nation's four largest insurers (Waxman and Stupak 2010).

The interaction of these three types of regulations affects accessibility and affordability of insurance in the non-group market, especially for people with pre-existing conditions. I define three types of non-group market regulatory schemes. "Risk-rated" states are states that have an absence of all three types of regulations or only have guaranteed issue. In these states, insurers should offer coverage to people with pre-existing health conditions, but the premium should vary with health risk, and coverage for the pre-existing condition may be excluded. "Denial" states

are defined as states that prohibit elimination riders only, allowing insurers to risk-rate or deny policies but not to carve out expensive coverage from a policy. In these states, insurers should offer risk-rated coverage to moderate health risks but deny coverage altogether to those with pre-existing conditions because they would be forced to cover costly benefits if they did issue a policy. States that have all three regulations and thus prohibit insurers from using any adverse selection mitigation strategy are defined as "community-rated" states. In these states, insurers cannot vary premiums with health risks, nor can they deny or carve out costly coverage. Table one shows the distribution of these policies across states.

The function $g(m_i)$ relates individual expected medical expenditures (m_i) to premiums in each type of state. In risk-rated states in which people with high expected medical expenditures pay more for health insurance, $g'(m_i) > 0$. In denial states in which people with pre-existing conditions—a proxy for high expected medical expenditures—are denied insurance, $g(m_i)$ is a discontinuous function. For people with low values of m_i, $g'(m_i) > 0$. But, for people with values of m_i above a critical threshold (m^*), $g(m_i) = \infty$ because they are not offered a policy at any price. I define households with pre-existing conditions as having $m_i > m^*$. Finally, in community-rated states in which medical underwriting is prohibited, $g'(m_i) = 0$ because premiums do not vary across expected medical expenditures.

Choice between sectors

Individuals choose self-employment if and only if the utility of self-employment is greater than the utility of employed work: $U_i^{se} - U_i^e > 0$. Wages net of after-tax health insurance costs for the self-employed are $w_i^{se} - g(m_i)(1 - \tau_f\theta_f - \tau_s\theta_s)$. Substituting wages net of after-tax health insurance costs into the utility function and differentiating with respect to

deductions shows that, as the deductible fraction of premiums rises, so do the benefits of self-employment because the right-hand side of the equation is positive. This is true for all types of states.

(2) $\frac{\partial U_i^{se}}{\partial \theta_f} = U'(w_i^{se} - c_i^{se})g(m_i)\tau_f > 0$ $\frac{\partial U_i^{se}}{\partial \theta_s} = U'(w_i^{se} - c_i^{se})g(m_i)\tau_s > 0$

Note that I assume that all individuals purchase insurance, that $w_i^j > c_i^j$, and that m_i is strictly positive.

To see how non-group regulations matter, consider the effect of the premium function, $g(m_i)$, on the partial effect of deductions on self-employment and the necessary and sufficient condition for self-employment.

Risk-rated states:

Because $g'(m_i) > 0$ and $U'' < 0$,

(3) $\frac{\partial^2 U_i^{se}}{\partial \theta_f \partial m_i} = g'(m_i)\tau_f \left(U' + U''\left(\theta\tau_f - 1\right)g(m_i)\right) > 0$

Therefore people with higher expected medical expenditures should be more likely to switch to self-employment. Note that the same is true for $\frac{\partial^2 U_i^{se}}{\partial \theta_s \partial m_i}$.

Denial states:

For people with $g'(m_i) > 0$, the benefits of deductions increase in m_i as in risk-rated states. But for those who are denied because of a pre-existing condition, the necessary and sufficient condition for self-employment can never be satisfied because $g(m_i) = \infty$ and so,

(4) $U\left(w_i^e - \pi^e \frac{(1-\tau_f-\tau_s-\tau_p)}{1+\tau_p}\right) > U(w_i^{se} - g(m_i)\left(1 - \tau_f\theta_f - \tau_s\theta_s\right))$

Note that these individuals *would* benefit greatly from deductions because $lim_{g(m_i)\to\infty} \frac{\partial^2 U_i^{se}}{\partial \theta_f \partial m_i} = \infty$, but they never do because the utility of wage and salary work always exceeds that of self-employment.

Community-rated states:

In these states, premiums do not depend on m_i, so, $\frac{\partial^2 U_i^{se}}{\partial \theta_f \partial m_i} = 0$ and the benefits of deductions do not depend on m_i either.

Predictions

This model has three testable predictions:

Prediction 1: In risk-rated states, the effect of deductions is greater among people with pre-existing conditions than those without.

Prediction 2: In denial states, deductions have no effect on people with pre-existing conditions.

Prediction 3: In community-rated states, there is no difference in the effect between people with and without pre-existing conditions.

The following section describes how I test these predictions using data from the Panel Study of Income Dynamics.

Empirical approach

Previous literature approach

The preceding theoretical framework shows that the effect of tax deductions should be stronger for people with pre-existing conditions when health insurance is risk-rated but zero in markets where denial is likely. The effect of increasing health insurance tax subsidies should not differ between people with and without pre-existing conditions where medical underwriting is prohibited. As in previous work in this field, I estimate the effect of tax subsidies at the household level. My innovation relative to previous empirical work is to separately identify the treatment effect by both the presence of a household member with a pre-existing condition and the type of non-group market regulations in the state. The existing literature estimates the effect of the tax prices of health insurance in each sector on the likelihood of self-employment (Gumus and Regan 2009; Gurley-Calvez 2011; Heim and Lurie 2010, 2013b). To estimate my own model, I begin with the approach taken most frequently in previous work, regressing a measure of self-employment on the logged tax price of health insurance in each sector, a set of controls, and household-level fixed effects:

$$(5)\ \Pr(SE_{it}) = \alpha_0 + \alpha_1 \ln(p^{se}_{i,t-1}) + \alpha_2 \ln\left(p^{e}_{i,t-1}\right) + X_{it}\gamma + v_i + g_t + u_i$$

The dependent variable (SE_{it}) indicates self-employment at the household-level based on self-employment income reported on income tax returns (Heim and Lurie 2010, 2013b; Gurley-Calvez 2011) or self-reported self-employment (Gumus and Regan 2009). The key independent variables are the logs of the tax prices in the self-employed and employed sectors, $1 - \theta_f\tau_f - \theta_s\tau_s$ and $\frac{1-\tau_f-\tau_s-\tau_p}{1+\tau_p}$, which are represented by p_i^j. Ideally, the independent variables would be the after tax cost—the product of the premium and the tax price—but premiums are not observed in the data. Other researchers have dealt with this problem by assuming that differences between

households in the premium are a function of the covariates in the model. Both tax prices enter into the equation linearly because individuals weigh the cost of non-group and group coverage in choosing whether to be self-employed.

The tax prices are lagged because people do not know their marginal tax rate in the current year, so they use the previous year's rate as a best guess. The coefficients α_1 and α_2 are the semi-elasticities of self-employment with respect to each tax price. Because self-employment and non-group health insurance are complements, α_1 should be negative; as non-group health insurance becomes less (more) expensive, self-employment should increase (decrease). The sign of α_2 should be positive because non-group and employer-sponsored coverage are substitutes. Because $p^e_{i,t-1}$ varies little, the predicted sign of α_2 is ambiguous and the magnitude should be small.

In previous work of this kind, controls typically include rudimentary demographic information contained on a tax return such as family size, marital status, and age (X_{it}). Household-level fixed effects (v_i) are used to control for differences in health status that affect both the tax price and self-employment between households. But while these fixed effects are intended to control for differences across households in both the price and demand for insurance, health characteristics vary over time, such that v_i can only partially control for these differences. Other models also include year effects (g_t) to control for secular trends in self-employment.

Theory-motivated approach

The previously used econometric model estimates the average effect of tax deductions on self-employment but cannot capture important heterogeneity related to the presence of pre-

existing conditions within the household and the price and accessibility of non-group insurance across states. The theoretical framework shows that the effect of the tax prices differs across state groups and expected medical expenditures. Therefore, I allow the effect to vary over both characteristics by interacting a measure of pre-existing conditions with the tax price of health insurance in each sector and estimating the following model in each of the three types of states:

$$(6)\ \Pr(SE_{it}) = \beta_0 + \beta_1 \ln(p^{se}_{i,t-1}) + \beta_2 \ln\left(p^{e}_{i,t-1}\right) + \beta_3 PEC_{it}$$

$$+\beta_4\left[\ln(p^{se}_{i,t-1}) \times PEC_{it}\right] + \beta_5\left[\ln(p^{e}_{i,t-1}) \times PEC_{it}\right] + X_{it}\gamma + H_{it}\delta + g_t + v_i + \epsilon_i$$

In this model, PEC_{it} indicates that the head of the household or his wife has ever been diagnosed with a medical conditions that would be considered a pre-existing condition if they were to try to buy health insurance in the non-group market at time *t*. Because equation (6) is estimated separately for each type of state, the interaction terms give the differential effect of the tax price of health insurance on self-employment for households with a pre-existing condition in each kind of state. Therefore $\beta_1 + \beta_4$ is the semi-elasticity of self-employment with respect to the self-employed tax price among households that contain a person with a pre-existing condition in risk-rated, denial, or community-rated states. Assuming that tax deductions have an effect for households that do not contain a person with a pre-existing condition ($\beta_1 < 0$), the sign and statistical significance of β_4 in each of the three state regulatory environments amounts to a test of predictions (1)-(3). In risk-rated states, I expect $\beta_4 < 0$ because households that contain a person with a pre-existing condition (pre-existing condition households) should pay more for health insurance than those that do not. Therefore, pre-existing condition households benefit more from the tax deduction. In denial states, I expect $\beta_4 > 0$ because pre-existing conditions households should be denied health insurance altogether. Finally, in community-rated states, I

expect $\beta_4 = 0$ because both types of households pay similar prices regardless of expected medical expenditures.

My approach also differs from the existing literature in the use of controls. I supplement the rudimentary demographic and income controls previously used (X_{it}) with a rich set of time-varying household-level health controls (H_{it}). The vector H_{it} includes the self-reported health status of the head and wife, health-related absenteeism of the head and wife due to his/her own or a family member's illness, the presence of disabilities that limit the work of the head or wife, and measures of sedentary lifestyle, smoking status, and obesity for the head and wife. I also include household-level fixed effects (v_i), as in equation (5).

Synthetic tax price instruments

Health insurance tax prices are a function of both the exogenously changing deduction *and* marginal tax rates. Marginal tax rates are endogenous to self-employment because self-employment income and marginal tax rates are jointly determined; individuals decide how much self-employment income to make based on their expected marginal tax rate, but the marginal tax rate is also affected by self-employment income. A solution to this problem is to instrument for the tax prices using synthetic tax prices that do not reflect the simultaneity of self-employment income and marginal tax rates. The prevailing approach in the literature is to use the tax-filing characteristics from the first year the individual is observed in the panel to impute tax prices for the rest of the panel, omitting the first year. I choose not to do this for two reasons. First, I want to keep as many observations as possible because my sample is small, and second, past individual characteristics may still reflect the simultaneity of income and self-employment.

Instead, I construct synthetic tax price instruments using a two-step procedure similar to that used by Currie and Gruber (1996) to create simulated Medicaid eligibility. First, I calculate the marginal tax rate each household would face in each state in the estimation sample if it had the same income and household characteristics it had in the year of observation. Then I calculate the average marginal tax rate in that year by state, head's five-year age-category, marital status, head's sex, and number of dependents (censored at three). These synthetic tax prices therefore reflect federal and state-by-year variation in the policy without being contaminated by the simultaneity of household marginal tax rates and self-employment.

I use the synthetic tax prices as instruments in two-stage least squares regressions of self-employment on the tax prices. One concern with two-stage least squares is that the instruments are weak. If they are weak, the bias in the two-stage least squares estimator approaches that of ordinary least squares. In the case of one endogenous variable, the bias is inversely proportional to the *F*-statistic (Bound, Jaeger, and Baker 1995). Because both tax prices and their interactions are endogenous, the *F*-statistic on the excluded instruments in the first-stage regressions are no longer helpful because the *F*-statistic might be large even if only one instrument is a strong predictor of the endogenous variable and the rest of the instruments are weak. To test for weak instruments, I use a modification of the *F*-statistic for multivariate regressions proposed by Angrist and Pischke (2009a); (Angrist and Pischke 2009b). To obtain the *F*-statistic, I regress the endogenous tax prices on the instruments after partialling-out the variation in the endogenous variable due to the other instruments. In this way, I isolate the effect of the instruments on each of the endogenous variables. The results from first-stage regressions and the *F*-statistics are reported in Table 2.7. The first-stage coefficients on the synthetic tax prices are always positive and strongly predictive of the endogenous tax prices. The value of the *F*-statistic on the

excluded instruments ranges from 48 to 70 in regressions as specified in equation (5), and from 14 to 575 in my main results, as specified in equation (6). They are sufficiently large to suggest that the estimates do not suffer from bias due to weak instruments.

Data

There are very few datasets that contain information on self-employment, detailed income data necessary to calculate marginal tax rates, and retrospective information on health conditions. While tax return data can accurately capture self-employment and marginal tax rates, available controls for health status are limited to crude demographic variables such as age. Even surveys like the Current Population Survey and the Survey of Income and Program Participation, which contain information on current health status as well as self-employment measures and income characteristics, are not useful for the purpose of this study because current health status may be only weakly associated with past conditions. Datasets with rich health information such as the Medical Expenditure Panel Survey do not contain detailed income characteristics and so there is considerable measurement error in both the independent and dependent variables. Additionally, longitudinal data are desirable, and the Medical Expenditure Panel Survey's panel follows respondents for a maximum of two years.

A dataset that contains all three elements necessary to study this problem—precise measures of self-employment, detailed income and filing characteristics necessary to calculate marginal tax rates, and retrospective information about health conditions—is the Panel Study of Income Dynamics (PSID). The PSID is a longitudinal survey conducted by the University of Michigan starting in 1968 and based on a probability sample of 4,800 households in 1967. The purpose of the survey is to follow households over multiple generations in order to analyze

dynamic economic and demographic behavior (Hill 1991). Retrospective health data were added to the study in 1999.

The analysis uses a sample of 12,385 observations from 3,786 households from the 1999, 2001, 2003, and 2005 waves of the PSID. From an initial sample of 9,148 households with non-missing data, I restrict the sample to households that are observed at least twice during the study period (N= 7,438) with a prime working-age head aged 25 to 54 (N= 5,226). I chose this age range rather than what is used in the literature, 25-64 (Gumus and Regan 2009; Heim and Lurie 2010, 2013b, 2013a; Gurley-Calvez 2011) because I want to exclude households that are eligible for early-retiree health insurance. Because people who can retire from their current work might chose to become self-employed because of early retiree health insurance rather than tax deductions, I exclude them from the sample. I further restrict the sample to exclude the nine group-of-one states where the self-employed do not purchase health insurance in the non-group market (CO, CT, DE, FL, HI, MI, MS, NC, RI), the two states that changed regulations in the middle of the study period (KY, NH), and Oregon, which has community rating but no guaranteed issue (N= 3,786).

The original PSID sample was composed of two distinct subsamples: a nationally representative sample, and an over-sample of low-income households, called the Survey of Economic Opportunity (SEO) sample (Brown 1996). Because of problems with the sampling procedure used to construct the SEO sample, some have recommended disregarding the SEO sample (Shin and Solon 2011). Omitting the 3,715 SEO households from my estimation sample does not affect the results. Therefore I chose to use these observations in the analysis.

Table 2.1 shows that the PSID sample is fairly representative of the U.S. population. Each observation represents a household, and all variables except for age, sex, and race represent responses for both the head and his legal wife, if he is married. Note that in the PSID, household heads are always male unless the head is a single woman. Eighteen percent report that the head or wife is self-employed in their main job. The sample statistics are similar to published statistics from the Bureau of Labor Statistics and the U.S. Census: 15.3 million Americans were self-employed in 2009, or 18 percent of the population aged 25 to 44 (Hipple 2010; Howden and Meyer 2011).

The key independent variables are the tax prices of health insurance for the self-employed and employed households. They are continuous measures indicating the cost to the household of a dollar of health insurance after taxes are taken into account. The self-employed tax price is identified with Federal and state-by-year changes in the statutory health insurance tax deduction for the self-employed. Because the self-employed tax price is a non-linear function of both deductions and marginal tax rates, the interaction of deductions and marginal tax rates also provide identifying variation. Figure one illustrates both types of variation over the study period. Tax prices depend on marginal tax rates, which are imputed using the National Bureau of Economic Research TAXSIM model (Version 9). TAXSIM uses 22 inputs to impute federal, state, and payroll marginal tax rates. These inputs are created from the PSID following (Butrica and Burkhauser 1997) and are described in Table 2.8. For more details about the TAXSIM model, see Feenberg and Coutts (1993). The average household tax price for the self-employed over the period is 85 percent, falling roughly 10 percent from 0.9 to 0.8 over the study period. In contrast, the average household tax price for the employed is relatively constant over the period at 56 percent. Trends in the synthetic tax price instruments are similar to trends in the tax price.

One major advantage of the PSID is that I can identify households that contain an individual with a pre-existing health condition that would result in denial of non-group health insurance in denial states. Because the definition of a pre-existing condition is insurer specific, several definitions have been offered in the literature and are outlined in a report by the Government Accountability Office Government Accountability Office (2012a). I construct two measures based on these definitions and work by Zellers, McLaughlin, and Frick (1992), who survey insurance companies to identify conditions that result in denial. The first is a narrow definition of pre-existing conditions set to one if the head answers yes to the question "Has a doctor ever told you that you/[your wife] have or had one of the following conditions," which include heart disease, cancer, heart attack, or stroke. I also construct a broader measure of pre-existing conditions using the same question but expanding the conditions to include high blood pressure, diabetes, lung disease, emotional problems, memory loss, and obesity. By the narrow measure, 12 percent of households contain a person with a pre-existing condition and by the broad measure, 27 percent of households contain a person with pre-existing condition. For comparison, the Government Accountability Office estimates that the fraction of the adult population with a pre-existing condition is between 20 and 66 percent (Government Accountability Office 2012a).

A final key variable is the classification of states into one of three types of non-group market regulatory environments. I classify states into risk-rated, denial, and community-rated states using data from the Kaiser Family Foundation State Health Facts Website (Kaiser Family Foundation 2012) and a report by the Assistant Secretary for Evaluation and Planning (Assistant Secretary for Evaluation and Planning 2010). Table 2.2 illustrates the distribution of these

policies across states. Sixty percent of sample households reside in the 27 risk-rated states, 22 percent reside in the five denial states and 19 percent reside in the six community-rated states.

Two pieces of evidence support my state classification. First, a study conducted by the Kaiser Family Foundation and the Georgetown Institute for Health Policy found that insurance companies in states with prohibitions on elimination riders and no guaranteed issue (denial states) were more likely to reject applicants than those in other states (Kaiser Family Foundation 2001). Second, more recent data from the Department of Health and Human Services Center for Consumer Information and Oversight's Plan Finder show that application denials are highest in denial states, and risk-rating is prevalent in both risk-rated and denial states (Department of Health and Human Services 2013). Plan Finder is a national database of non-group and small group health insurance offerings designed to help people find coverage before the establishment of Exchanges in 2014 (Office of the Inspector General 2011). Plan Finder collects characteristics of insurance plans including information about benefits, cost-sharing, and premiums. The data also contain the fraction of applications that are denied in each quarter and the fraction of policies that are risk-rated because of health status in each quarter. Data from the three most recent quarters shows that application denial rates are highest for insurers in denial states: seven percent versus four percent in risk-rated states and less than one percent in community-rated states. Note that the observed denial rate might actually be too low if people with pre-existing conditions are anticipating denials and are therefore more likely to forgo applying altogether. The average fraction of plans that are risk-rated is as expected. The rate in risk-rated states is three percent, compared to almost zero in community-rated states. Ten percent of policies are risk-rated in denial states.

My analysis also includes a variety of time varying, household, and individual-level health characteristics that have not been previously used in the literature. They include self-reported health status, health-related work limitations, absenteeism, low physical activity, current smoking status, and obesity. Fourteen percent of households contain a head or wife who reports fair or poor health. Seventeen percent report the presence of a condition that limits the amount or type of work the head or wife can do in their job. The mean number of weeks the head reports having missed work in the last year due to personal illness or a family member's illness is 0.8. Fifty-three percent of heads and their wives report having a sedentary lifestyle measured by getting less than one hour of physical activity a week. Twenty-eight percent of households report having a head or wife who smokes and 34 percent of households report a head or wife who is obese.

Empirical evidence

Descriptive evidence

A simple plot of the data provides visual evidence for the testable implications of my theory. Figure two shows the trend in self-employment for both types of households using the narrow definition of a pre-existing condition. Each panel of the figure depicts a state type: risk-rated, denial, or community-rated. Each figure also plots the average tax price for each type of household in each type of state, showing that the tax price of self-employed health insurance fell by a similar amount across household and state types. In risk-rated states I find what is expected: self-employment rises more for pre-existing condition households than for households without a pre-existing condition, as tax prices fall. In denial states, I expect that pre-existing condition households should not respond to the tax price of health insurance for the self-employed. And

indeed, self-employment among non-pre-existing condition households increases by roughly five percentage points, but self-employment among pre-existing condition households remains constant at about 15 percent. In community-rated states, I expect that the effect of tax prices should not differ between the two types of households. I find partial evidence for this hypothesis. Both lines are upward sloping, but it appears that self-employment for pre-existing condition households increases slightly less over the period than does self-employment for non-pre-existing condition households.

Regression results: replication of previous findings

The first step of my formal analysis is to show that I can generate results consistent with previous findings using my PSID sample. Column one of Table 2.3 replicates Heim and Lurie (2010) although the magnitude of my estimate is nearly twice as large. In a model with demographic and income controls and household fixed effects, the estimated semi-elasticity of self-employment with respect to the self-employed tax price is −0.374. This implies that the ten percent reduction in the tax price of self-employment over the period resulted in a 3.74 percentage point increase in the self-employment rate between 1999 and 2005, holding the employed tax price constant. As a fraction of the average level of self-employment in the balanced panel, the point estimate implies a 21 percent increase. Heim and Lurie (2010) find that tax deductions increase self-employment by 15 percent over the same time period.

The addition of time-varying health controls in column two has little effect on the estimates. The magnitude of the semi-elasticity of self-employment with respect to the tax price falls slightly, but the statistical and economic significance of the estimate remains the same. The only time-varying health regressor with a statistically significant effect on self-employment is an

indicator of fair or poor self-reported health status for the head or his wife. Those who report a member of the household in fair or poor health are almost two percentage points less likely to be self-employed. The elasticity of self-employment with respect to the employed tax price has the expected sign and is statistically insignificant in both models.

Regression results: risk-rated states

The benefits of a tax deduction are greatest for households that pay higher premiums for the same amount of insurance. Therefore, pre-existing condition households should respond more to deductions than non-pre-existing condition households in states in which they can buy an insurance policy but it costs more (prediction 1). To test this prediction, I examine the sign and statistical significance of β_3, the interaction coefficient of the self-employed tax price with the pre-existing conditions indicator. The first column of Table 2.4 provides evidence consistent with this prediction using the narrow definition of pre-existing conditions. In a model with time-varying health and demographic controls as well as household fixed effects, $\beta_3 = -0.472$ and it is statistically significant (s.e.=0.207). This implies that the semi-elasticity of self-employment with respect to the self-employed tax price is larger among households with pre-existing conditions than among other households. The estimates imply that self-employment increased by 18 percent between 1999 and 2005 for non-pre-existing condition households but by about 45 percent for pre-existing condition households. The coefficients on the employed tax price and its interaction have the anticipated sign and are small and statistically insignificant, consistent with no effect.

Regression results: denial states

Those who cannot purchase health insurance in the non-group market cannot benefit from tax deductions for non-group health insurance. Therefore, pre-existing condition households should not respond to deductions at all in states in which they should be denied health insurance (prediction 2). A test of this prediction is equal to the sign, statistical significance, and magnitude of the coefficient on β_3: it should be equal in magnitude to β_1—the semi-elasticity of self-employment for non-pre-existing condition households—but have the opposite sign, such that $\beta_1 + \beta_3 = 0$. Evidence consistent with this prediction is provided in column two of Table 2.4. In it, β_3 is 0.665 (s.e.=0.391), almost exactly the same size as the coefficient on β_1 (–0.654) but with the opposite sign. These results imply that deductions increased self-employment by 36 percent between 1999 and 2005 for non-pre-existing condition households (0.0107, s.e.=0.527). Although it is consistent with my theory, the estimated zero effect is imprecise. The standard error implies I cannot rule out a ten percentage point increase in self-employment or a ten percentage point *decrease* in self-employment between 1999 and 2005. The coefficient on the employed tax price has the correct sign and is small and statistically insignificant.

Regression results: community-rated states

Finally, column three of Table 2.4 investigates the effect of the tax prices on self-employment in community-rated states. My theory predicts that the semi-elasticities of self-employment with respect to the self-employed tax price do not differ between the two types of households in these states. A test of this prediction is equal to the statistical significance of β_3. Although I find that $\beta_3 = 0$, the effect for non-pre-existing conditions households is small and statistically insignificant (–0.078, s.e.=0.634). Taken together, these results imply that the semi-

elasticity of self-employment with respect to the tax price is zero for both groups in community-rated states.

Robustness checks

My main results use a narrow definition of pre-existing conditions, but more broad definitions also exist. Table 2.5 reports my main results, re-estimated using the broad definition of pre-existing conditions. Column one shows the results for risk-rated states. My first prediction does not hold: the β_3 is not statistically different from zero (0.024, s.e.=0.0235), and the estimated semi-elasticities are about -0.45 for both types of households. In terms of the 10 percent decrease in the self-employed tax price, this effect implies a 25 percent increase in self-employment between 1999 and 2005 for both groups. The results for denial states in column two of Table 2.5 are qualitatively similar to the results using the narrow measure, but the semi-elasticity of self-employment no longer differs significantly between the two types of households (0.182, s.e.=0.273). The estimated semi-elasticity of self-employment with respect to the tax price for non-pre-existing condition households is -0.649 (s.e.=0.326) and -0.467 (s.e.=0.409) for pre-existing condition households. Although the interaction coefficient is not different from zero, I cannot reject that the effect among households with a pre-existing condition is zero. Column three illustrates the results for community-rated states. The results are similar to those using the narrow definition of pre-existing conditions although the magnitude of the effect for non-pre-existing condition households doubles in size and switches signs. It remains statistically indistinguishable from zero.

Employer-sponsored health insurance through a spouse increases self-employment (Fairlie, Kapur, and Gates 2011). Additionally, measurement error in the tax price is higher in

married households because married tax payers may file jointly or separately, but I do not observe their filing status. An important robustness check therefore is to estimate the results separately for single households. Column one of Table 2.6 shows the results from the replication results for single households. The semi-elasticity of self-employment with respect to the tax price is statistically significant, and the magnitude is larger than the results using both married and single households. This result suggests that the results are not driven by the availability of employer-sponsored insurance for one spouse. Ideally, I would be able to estimate my main results on a sample of single households, but small sample size prevents me from doing so. Thirty-seven percent of the sample is single, and only nine percent of the single sample has a pre-existing condition. This would result in very small cells when splitting the sample further by state non-group regulation type. For this reason, I do not do this robustness check on the main results.

The original PSID sample over-represents households that were low-income in 1967 (Gouskova et al. 2008). Although I control for current income in my results, the original selection procedure could introduce endogenous sampling if the error term is still correlated with self-employment after conditioning on current income. However, if controlling for current income sufficiently removes variation related to 1967 income from the error term, weighting will increase the standard errors but not affect the estimated parameters (Solon, Haider, and Wooldridge 2013). This is because weighted least squares is inefficient relative to ordinary least squares when the probability of being sampled is unrelated to the error term. Columns two through four of Table 2.6 re-estimate the main results using the PSID's longitudinal family weight. The magnitudes and signs of the estimated coefficients are qualitatively similar, but the

standard errors are much larger. These results suggest that my preferred specification without weights is acceptable.

Discussion

The results of this paper show that the effect of health insurance tax subsidies on self-employment depends on both the presence of pre-existing conditions and the affordability and accessibility of coverage in the non-group market. In states in which people with pre-existing conditions pay higher premiums for insurance, the effect of tax subsidies is twice as large among households with pre-existing conditions as those without. In states in which pre-existing conditions are valid grounds for denial, households that contain a person with a pre-existing condition do not respond to tax subsidies at all. Finally, in states in which people with pre-existing conditions face the same premium as healthy people, tax subsidies have no effect on either group.

This paper makes several theoretical and empirical contributions to the literature and has a number of policy-relevant findings. The paper's theoretical contributions are three-fold. First, I formalize the connection between non-group market regulations and tax policy through their combined effect on the after-tax price. Previous work has only informally alluded to this relationship. Second, the theory incorporates not just risk-rating but also the well-known feature of the non-group market that some individuals cannot obtain coverage at any price because of pre-existing conditions. To the best of my knowledge, no paper in the literature on either tax deductions or non-group market reforms has explicitly studied the effect of both affordability of and access to coverage on self-employment. Third, the paper illustrates the counterintuitive point that deductions should disproportionately benefit people with pre-existing conditions in

states in which risk-rated insurance is available. This finding runs contrary to the idea that medical underwriting makes people with pre-existing conditions worse off.

The paper makes four empirical contributions. First, it is the first to estimate separate treatment effects of deduction on self-employment by the presence of a pre-existing condition as well as the affordability and accessibility of insurance in the non-group market. Additionally, to the best of my knowledge, no other paper in the literature on non-group health insurance explicitly incorporates pre-existing conditions. Second, it is the first paper in this literature to investigate the role of prohibitions on elimination riders in non-group markets. Most papers have focused on community rating and guaranteed issue but this paper shows that the treatment of elimination riders matters for high-risk applicants in states in which elimination riders are prohibited but guaranteed issue is not required. Although prohibiting elimination riders was likely meant to protect people with pre-existing conditions, the presence of this policy when insurers are not required to offer coverage to all applicants results in fewer coverage options. Third, this paper is the first in the literature on non-group market regulations or tax deductions to recognize that group-of-one states in which the self-employed can purchase insurance outside of the non-group market should be excluded from studies of the non-group market. Including individuals from group-of-one states could overstate the effect of tax subsidies on self-employment in denial or risk-rated states. Fourth, the paper improves the synthetic tax price methodology because my instrument more adequately addresses simultaneity in the tax price of self-employment over time. My first-stage results show that these synthetic tax price instruments are strong predictors of the endogenous tax prices, and my multivariate F-statistics indicate that concerns about weak instruments bias are negligible.

Finally, this paper contains four policy-relevant findings. First, I show that job-lock affects self-employment for people with pre-existing conditions. Policymakers have consistently referred to people with pre-existing conditions as the quintessential case for job lock (Government Accountability Office 2012b), yet little work explores the effect of high-cost medical conditions on employment outcomes. Second, I provide evidence that the benefits of two decades of increased generosity in the tax code towards health insurance for the self-employed accrued unevenly across health status and geography. While people with high-risk health conditions benefited disproportionately in the majority of states, they did not benefit at all in states in which they were mostly likely to be denied health insurance. Third, I show that tax subsidies and non-group market regulations interact. This provides support for the ACA's two-pronged approach (tax subsidies and non-group market reforms) and casts doubt on recent proposals to repeal parts of the ACA or replace the ACA with tax-based subsidies alone (Collins, Kriss, and Fund 2008). Additionally, it illustrates the importance of accounting for both types of policies in future evaluations of the ACA on self-employment. Fourth, the results of this paper suggest that the ACA's tax subsidies might not have an effect on self-employment, contrary to predictions by researchers and policymakers (Blumberg, Corlette, and Lucia 2013; Council of Economic Advisers 2009b; Government Accountability Office 2012b). The ACA's non-group market reforms establish community rating and guaranteed issue, and prohibit elimination riders and therefore, look most similar to community-rated states. Note that, while these results show that tax subsidies do not have an effect on self-employment in community rated states, my results do not imply that community rating and guaranteed issue will not have a direct effect on self-employment.

Conclusion

Subsidizing health insurance for the self-employed through the tax code is a popular policy that has been shown to increase self-employment. This paper shows that the effect of tax subsidies on self-employment depends on expected medical expenditures and the treatment of pre-existing conditions in the non-group insurance market. Households that contain a person with a pre-existing condition respond more strongly to tax subsidies when they pay more for health insurance, but do not respond at all in states in which they would be denied health insurance because of a pre-existing condition. Finally, neither type of household responds to health insurance tax subsidies in states in which the price of health insurance is not allowed to vary with health risk.

Tables and figures

Table 2.1 State non-group market classification

State Type	Nothing	Guaranteed Issue	Riders Prohibited	Community-Rating	Totals
Risk-Rated	X				25
		X			2
Denial			X		5
Community-Rated		X	X	X	6
Not Appropriate					12
Total					50

The figure shows how the 50 states are classified into the three non-group market regulation types. Risk-rated states are defined as those that have none of the three regulations (AL, AK, AZ, AR, GA, IL, IA, KS, LA, MO, MT, NE, NV, NM, ND, OH, OK, SC, SD, TN, TX, UT, WV, WI, WY) or guaranteed issue alone (PA, VA). Denial states have prohibitions on elimination riders only (CA, ID, IN, MD, MN). Community-rated states have all three regulations (ME, MA, NJ, NY, VT, WA). States that allow the self-employed to purchase coverage in the small group market and have risk-rating in the non-group market are not included in the analysis (CO, CT, DE, FL, HI, MI, MS, NC, RI). States that made changes to non-group market regulations between 1999 and 2005 were also omitted (KY, NH). Finally, Oregon is excluded because it has community rating but does not require guaranteed issue.

Table 2.2 Panel Study of Income Dynamics Sample statistics

	Mean	Standard Deviation
Self-Employment Measures		
Self-Employment in Main Job	0.180	0.392
Key Independent Variables		
Self-Employed Tax Price	0.846	0.125
Wage and Salary Tax Price	0.560	0.144
Synthetic Self-Employed Tax Price	0.852	0.061
Synthetic Wage and Salary Tax Price	0.564	0.054
Pre-Existing Condition, Narrow Definition	0.117	0.322
Pre-Existing Condition, Broad Definition	0.277	0.448
State Non-Group Regulation Type		
Community-Rated	0.187	0.330
Risk-Rated	0.596	0.491
Denial	0.217	0.412
Health Controls		
Head or Wife in Fair or Poor Health	0.140	0.347
Head or Wife has a Work-Limiting Condition	0.166	0.372
Head Weeks of Work Missed due to Illness	0.816	2.615
Head or Wife is Sedentary	0.525	0.499
Head or Wife is a Current Smoker	0.281	0.449
Head or Wife is Obese	0.313	0.464
Other Controls		
Age of Head	40.3	8.4
Head is Married	0.630	0.483
Head is Male	0.813	0.390
Head or Wife is Non-White	0.250	0.433
Head or Wife is College-Educated	0.591	0.492
Total Number of Dependents	0.981	1.118
Total Household Income	71426	120680
Year		
1999	0.264	0.430
2001	0.282	0.451
2003	0.164	0.379
2005	0.290	0.458

The table provides sample means and standard deviations for the estimation sample which includes 12,385 household-year observations from 3,786 households with a working-age head (25-54) in the Panel Study of Income Dynamics.

Table 2.3 Effect of tax prices on self-employment in the Panel Study of Income Dynamics

	Fixed Effects	Fixed Effects and Health Controls
ln(self-employed tax price)	-0.374***	-0.360***
	(0.135)	(0.134)
ln(employed tax price)	0.0728	0.0604
	(0.193)	(0.190)
Fair or Poor Health Status		-0.0184*
		(0.0100)
Limitations at Work		0.0122
		(0.0100)
Weeks Missed Due to Own or Family Illness		-0.000321
		(0.000976)
Sedentary Lifestyle		-0.00659
		(0.00635)
Currently Smokes		0.00747
		(0.0119)
Obese		-0.00509
		(0.00968)
Constant	-0.182	-0.175
	(0.407)	(0.414)
Observations	12,385	12,385

The table replicates results from the Heim and Lurie (2010) as in equation (5) with data from the PSID. Self-employment is defined as self-reported, exclusive self-employment in the head or wife's main job. Column 1 is estimated as in equation (5) with rudimentary demographic and income controls and fixed effects. Column two adds time-varying health controls. All regressions are run on the full estimation sample of 38 states. All regressions use 2-stage least squares with synthetic tax price instruments for both the self-employed and wage and salary tax prices. The first stage F-statistics for the self-employed and employed tax prices are 56 and 70 for column 1, and 49 and 66 for column 2. Standard errors are clustered at the household level and are heteroskedasticity-robust. * indicates statistically significant at the 10% level, ** indicates statistically significant at the 5% level and *** indicates statistically significant at the 1% level.

Table 2.4 Heterogeneous effects of tax prices on self-employment by pre-existing conditions and state regulation type

	Risk-Rated States	Denial States	Community-Rated States
ln(self-employed tax price)	-0.33*	-0.65*	-0.08
	[0.17]	[0.34]	[0.63]
ln(employed tax price)	0.30	0.05	-0.87
	[0.19]	[0.50]	[0.65]
ln(self-employed tax price) ×Condition	-0.47**	0.67*	0.33
	[0.21]	[0.39]	[0.38]
ln(employed tax price) ×Condition	0.16	-0.52	0.51
	[0.20]	[0.34]	[0.64]
Pre-Existing Condition	-0.007	-0.15	0.40
	[0.092]	[0.18]	[0.44]
Constant	-0.15	-0.5	0.19
	[0.49]	[1.1]	[1.29]
Observations	8,108	2,549	1,728

The table reports regression coefficients from equation (6) for each type of state non-group regulatory environment, using the narrow definition of pre-existing conditions. The estimation sample in column 1, is risk-rated states, column 2 is denial states and column 3 is community-rated states. Self-employment is defined as self-reported exclusive self-employment in the head or the wife's main job. The tax prices are interacted with narrow measures of pre-existing conditions defined in the text. All regressions include demographic, income and health controls, as well as individual-level fixed effects. All regressions use 2-stage least squares with synthetic tax price instruments for both the self-employed and wage and salary tax prices and their interactions. First-stage results and multivariate F statistics are reported in appendix A. Standard errors are clustered at the household level and are heteroskedasticity-robust. *** indicates significance at the 1% level, ** indicates significance at the 5% level and * indicates significance at the 10% level.

Table 2.5 Heterogeneous effects of tax prices on self-employment by pre-existing conditions and state regulation type, alternative definition of pre-existing conditions

	Risk-Rated	Denial	Community Rated
ln(self-employed tax price)	-0.45***	-0.65**	0.15
	[0.17]	[0.33]	[0.49]
ln(employed tax price)	0.35*	0.08	-0.71
	[0.20]	[0.51]	[0.56]
ln(self-employed tax price) ×Condition	0.02	0.18	0.21
	[0.15]	[0.270]	[0.25]
ln(employed tax price) ×Condition	0.003	-0.020	-0.07
	[0.052]	[0.095]	[0.11]
Pre-Existing Condition	-0.027	0.023	-0.046
	[0.020]	[0.034]	[0.051]
Constant	-0.05	-0.3	0.08
	[0.50]	[1.1]	[1.20]
Observations	8,108	2,549	1,728

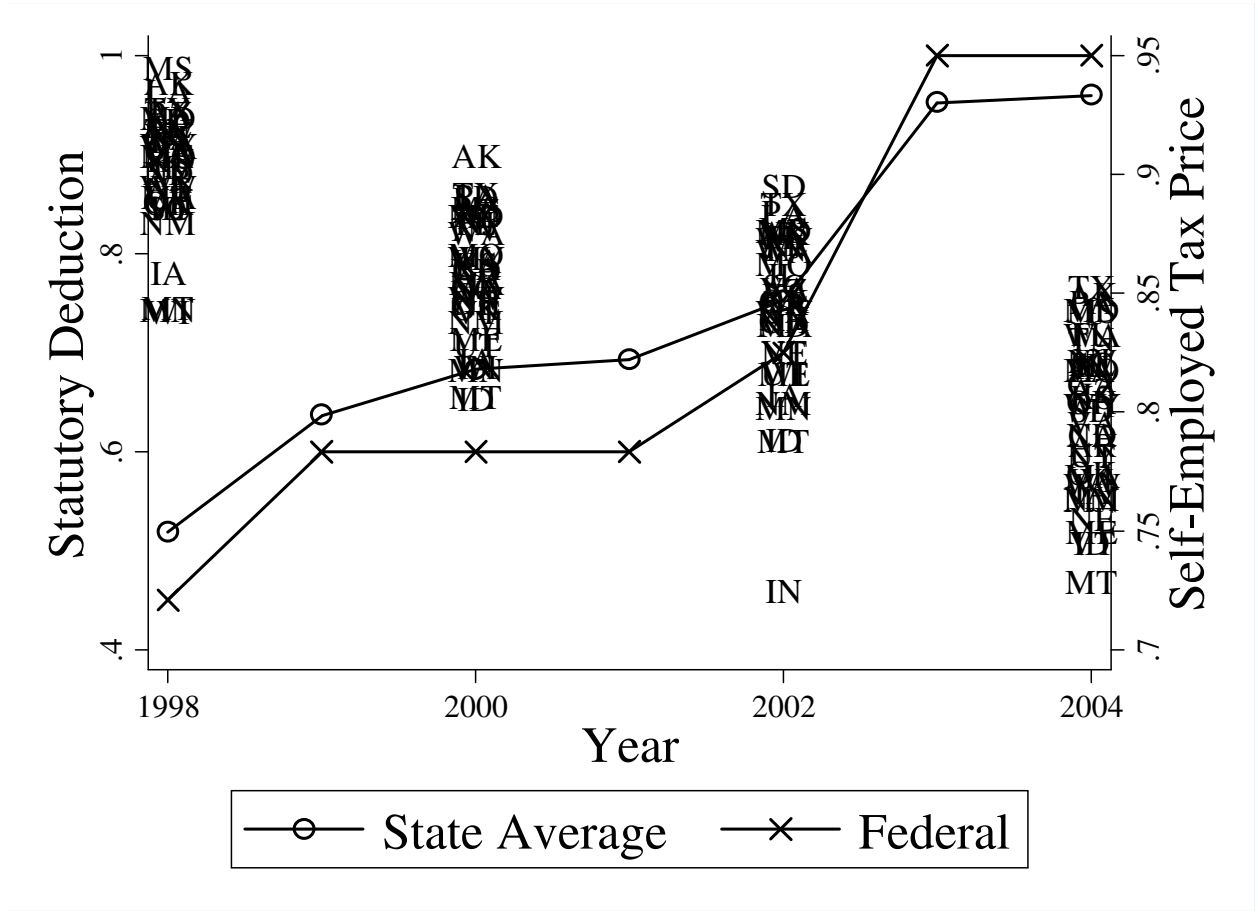
The table reports regression coefficients from equation (6) for each type of state non-group regulatory environment, using the broad definition of pre-existing conditions. The estimation sample in column 1, is risk-rated states, column 2 is denial states and column 3 is community-rated states. Self-employment is defined as self-reporting exclusive self-employment in the head or the wife's main job. The tax prices are interacted with the broad measure of pre-existing conditions defined in the text. All regressions include demographic, income and health controls as well as individual-level fixed effects. All regressions use 2-stage least squares with synthetic tax price instruments for both the self-employed and wage and salary tax prices and their interactions. First stage results and multivariate F statistics are reported in appendix A. Standard errors are clustered at the household level and are heteroskedasticity-robust. *** indicates significance at the 1% level, ** indicates significance at the 5% level and * indicates significance at the 10% level.

Table 2.6 Effect of tax prices on self-employment, robustness checks

	Single Households	Weighted Results		
		Risk-Rated States	Denial States	Community-Rated States
ln(self-employed tax price)	-0.51**	-0.17	-1.03	0.17
	[0.23]	[0.19]	[0.89]	[0.78]
ln(employed tax price)	0.18	0.22	0.45	-0.48
	[0.25]	[0.16]	[0.52]	[0.48]
ln(self-employed tax price) × Condition		-0.56*	0.67	0.05
		[0.29]	[0.47]	[0.29]
ln(employed tax price) × Condition		0.12	-0.58*	0.61
		[0.26]	[0.34]	[0.70]
Pre-Existing Condition		-0.06	-0.16	0.40
		[0.11]	[0.22]	[0.50]
Observations	4,582	8,108	2,459	1,728

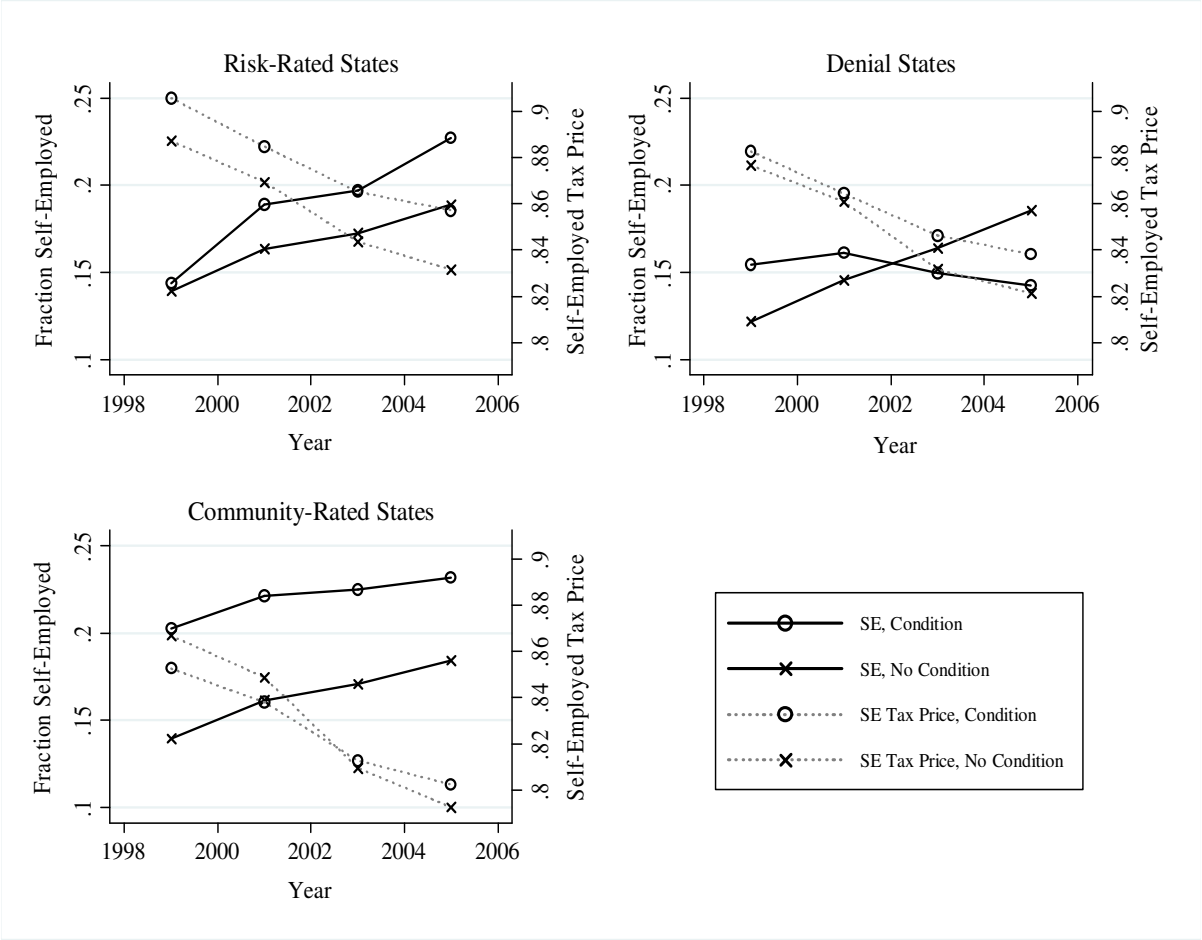
The table reports results from robustness checks. Column 1 presents the results from the replication for single households only and columns 2-4 present the results from the main regressions using the narrow definition of pre-existing conditions, weighting using the PSID family longitudinal weights. All regressions include demographic, income and health controls as well as individual-level fixed effects. All regressions use 2-stage least squares with synthetic tax price instruments for both the self-employed and wage and salary tax prices and their interactions. Standard errors are clustered at the household level and are heteroskedasticity-robust. *** indicates significance at the 1% level, ** indicates significance at the 5% level and * indicates significance at the 10% level.

Figure 2.1 Federal and average state statutory health insurance deductions for the self-employed and average self-employed tax price by state



The figure shows the trend in statutory health insurance deductions at both the Federal and average state level for the period of analysis. The figure also shows the average self-employed health insurance tax deduction in each state and each year of observation.

Figure 2.2 Heterogeneous trends in self-employment by the presence of a pre-existing condition and state non-group market regulation



The figure shows the trend in self-employment for households that contain a person with a pre-existing condition and those that do not in each type of non-group market regulatory environment. The dotted lines represent the average self-employed tax price of health insurance for each group.

Table 2.7 First-stage regressions and multivariate F-statistics

Panel 1: Narrow Pre-Existing Condition Definition

	Risk-Rated States				Denial States				Community-Rated States			
Endogenous Variable	ln(tpse)	ln(tpe)	ln(tpse) X Condition	ln(tpe) X Condition	ln(tpse)	ln(tpe)	ln(tpse) X Condition	ln(tpe) X Condition	ln(tpse)	ln(tpe)	ln(tpse) X Condition	ln(tpe) X Condition
ln(synthetic self-employed tax price)	0.791***	-0.0752	0.0328	0.0110	0.622***	-0.125	-0.0525	-0.0203	0.509***	-0.334	0.000135	0.158*
	(0.0564)	(0.0990)	(0.0256)	(0.0386)	(0.0922)	(0.162)	(0.0417)	(0.0611)	(0.120)	(0.228)	(0.0463)	(0.0809)
ln(synthetic employed tax price)	-0.104*	0.636***	-0.0486**	-0.0514	-0.212***	0.271**	0.0247	-0.0184	-0.210*	0.391*	0.0171	-0.0921
	(0.0532)	(0.0933)	(0.0241)	(0.0364)	(0.0638)	(0.112)	(0.0289)	(0.0423)	(0.113)	(0.214)	(0.0433)	(0.0758)
ln(synthetic self-employed tax price) ×Condition	-0.131*	0.0932	0.956***	0.156***	-0.242*	-0.212	0.762***	-0.224**	-0.0694	-0.167	1.056***	-0.507***
	(0.0757)	(0.133)	(0.0343)	(0.0518)	(0.134)	(0.236)	(0.0607)	(0.0888)	(0.136)	(0.259)	(0.0524)	(0.0916)
ln(synthetic employed tax price) × Condition	0.145**	-0.0124	0.0647**	0.913***	0.169	0.145	-0.0660	0.868***	0.421***	0.564**	-0.136***	0.963***
	(0.0702)	(0.123)	(0.0318)	(0.0481)	(0.112)	(0.197)	(0.0508)	(0.0743)	(0.127)	(0.241)	(0.0488)	(0.0853)
Constant	0.0519	-0.432	-0.0712	-0.156	0.137	-0.880	0.0443	-0.166	0.399	0.573	0.0357	0.162
	(0.191)	(0.336)	(0.0867)	(0.131)	(0.385)	(0.679)	(0.174)	(0.255)	(0.391)	(0.741)	(0.150)	(0.263)
Observations	8,108	8,108	8,108	8,108	2,549	2,549	2,549	2,549	1,728	1,728	1,728	1,728
R-squared	0.265	0.048	0.454	0.752	0.316	0.040	0.484	0.781	0.478	0.044	0.786	0.867
F-Statistic on Excluded Instruments	33.92	50.92	71.25	397.8	55.24	14.34	283.0	155.3	220.7	57.00	33.97	37.17

Note:

Panel 2: Broad Pre-Existing Condition Definition

	Risk-Rated States				Denial States				Community-Rated States			
Endogenous Variable	ln(tpse)	ln(tpe)	ln(tpse) X Condition	ln(tpe) X Condition	ln(tpse)	ln(tpe)	ln(tpse) X Condition	ln(tpe) X Condition	ln(tpse)	ln(tpe)	ln(tpse) X Condition	ln(tpe) X Condition
ln(synthetic self-employed tax price)	0.651***	-0.121	-0.0359	0.0745	0.586***	-0.295	-0.225***	-0.179	0.814***	-0.0563	0.0892***	0.110**
	(0.0923)	(0.163)	(0.0544)	(0.0886)	(0.122)	(0.231)	(0.0738)	(0.129)	(0.0568)	(0.0996)	(0.0340)	(0.0537)
ln(synthetic employed tax price)	-0.214***	0.278**	-0.0747**	-0.263***	-0.150	0.488**	0.0117	-0.141	-0.100*	0.633***	0.0874***	-0.116**
	(0.0639)	(0.113)	(0.0376)	(0.0613)	(0.113)	(0.213)	(0.0681)	(0.119)	(0.0530)	(0.0930)	(0.0318)	(0.0502)
ln(synthetic self-employed tax price) × Condition	-0.177*	-0.0954	0.739***	-0.324***	0.00487	0.0923	1.050***	-0.253**	-0.134**	0.00259	0.918***	-0.0200
	(0.0925)	(0.163)	(0.0545)	(0.0887)	(0.0971)	(0.183)	(0.0586)	(0.102)	(0.0564)	(0.0989)	(0.0338)	(0.0534)
ln(synthetic employed tax price) × Condition	0.0572*	0.0148	0.0801***	1.116***	-0.00960	-0.0338	0.0307	1.147***	0.0476**	-0.00161	0.0107	1.002***
	(0.0326)	(0.0574)	(0.0192)	(0.0313)	(0.0353)	(0.0667)	(0.0213)	(0.0372)	(0.0195)	(0.0342)	(0.0117)	(0.0184)
Constant	0.133	-0.896	-0.101	-0.537	0.366	0.548	0.176	0.214	0.0748	-0.423	-0.0646	-0.207
	(0.386)	(0.679)	(0.227)	(0.370)	(0.394)	(0.743)	(0.238)	(0.415)	(0.191)	(0.335)	(0.114)	(0.181)
Observations	8,108	8,108	8,108	8,108	2,549	2,549	2,549	2,549	1,728	1,728	1,728	1,728
R-squared	0.315	0.040	0.571	0.813	0.472	0.041	0.692	0.835	0.265	0.048	0.516	0.796
F-Statistic on Excluded Instruments	575.4	4659	29161	66.32	153.7	69.38	21.48	365.1	33.75	42.96	14.87	114.7

Calculation of marginal tax rates

The paper uses the methodology proposed by Butrica and Burkhauser (1997). Because the PSID does not contain information on which family members constitute a tax filing unit, I assign individuals to tax units using the following rules (See directions for line 6c on the 1040: http://www.irs.gov/pub/irs-pdf/i1040gi.pdf):

All married heads are assumed to file jointly with their wife

All unmarried children of the head or wife (under age 19 or a full-time student between ages 19 and 24) and relatives of the head or wife without income are assumed to be dependents of the head.

All other dwelling unit members who are not dependents are considered to be their own tax filing units.

The analysis includes only the tax-unit to which the head belongs. If the head is legally married, I assume his tax filing status is married, filing jointly. If the head is single with dependents, I assume his tax filing status is single head of household. If the head is single without dependents, the individual's tax filing status is single.

For the time period of this study, the PSID does not ask about whether individuals itemized or took the standard deduction. For this reason, I assume that all individuals take the standard deduction. This will produce conservative estimates of marginal tax rates because the self-employed are more likely to itemize so the marginal tax rate will be an upper bound (Therefore the tax price is a lower bound).

The TAXSIM program uses 22 inputs to calculate marginal tax rates. The following table shows the variables used to generate the inputs and their definitions:

Table 2.8 TAXSIM-Panel Study of Income Dynamics cross-walk

Taxsim Input	PSID Variables
Caseid	Family Identifier
Tax Year	Lagged Survey Year
State Code	PSID State Code
Tax Filing Status	Marital Status + Number of Dependents
Dependent Exemptions	Children of head or wife under age 19 or between 19 and 24 who are full-time students
Number of Taxpayers Aged 65+	Number of household members aged 65+
Wage and Salary Income of Taxpayer	Head Labor Income + Head Labor Income from Business
Wage and Salary Income of Wife	Wife Labor Income + Wife Labor Income from Business
Dividend Income	Dividend Income of the Head and Wife
Other Property Income	Head Asset Income from Business+ Wife Asset Income from Business+ Farm/Market Gardening Income + Alimony Received + Head Rent Income + Head and Wife Interest Income + Retirement Income- Alimony Paid
Taxable Pensions	Non-VA Retirement Income of Head + Retirement and Annuities Income of the Wife
Gross Social Security	Social Security Income of Family Unit
Other Non-Taxable Transfer Income	SSI Income of Head and Wife + VA Pension Income of Head + Workers Compensation Income of Head and Wife + TANF Income of Head and Wife + Other Welfare Income of Head and Wife + Child Support Income of Head and Wife
Rent Paid	Annual Rent Paid
Property Taxes Paid	Annual Property Tax Paid
AMT-Preferred Itemized Deductions	Itemized Charitable Deductions + Itemized Medical Deductions
Child Care Expenses	Annual Child Care Expenses Paid
Unemployment Compensation	Unemployment Income of Head and Wife
Number of Dependents Aged Less Than 17	Number of children aged 16 and lower
Non-AMT-Preferred Itemized Deductions	Annual Mortgage Interest
Short-Term Capital Gains	*Assume zero*
Long-Term Capital Gains	*Assume zero*

For details about how TAXSIM calculates marginal tax rates, see (Feenberg and Coutts 1993). To create tax prices, I use several data sources. I received the state-by-year statutory deduction amounts from Thomas Selden. Table 2.9 shows the statutory deduction by state and year. Note that 17 states adopt the Federal deduction in all years, and eight states do not have an

income tax during this period. I get the federal fraction deductible from Heim and Lurie (2010). Table 2.10 displays these data.

Table 2.9 State statutory deduction fraction

State	1998	2000	2002	2004
CA	0.35	0.6	0.7	1
GA	0.45	1	1	1
IA	1	1	1	1
ID	0.45	1	1	1
IL	1	1	1	1
MA	0.45	0.5	0.7	1
ME	0	0.6	0.7	1
MN	1	1	1	1
MS	0	0.6	0.7	1
MT	1	1	1	1
NJ	0	0	0	0
OH	1	1	1	1
PA	0	0	0	0
SC	1	1	1	1
UT	0.6	1	1	1
WI	1	1	1	1
AK	1	1	1	1
NV	1	1	1	1
SD	1	1	1	1
TN	1	1	1	1
TX	1	1	1	1
WA	1	1	1	1
WY	1	1	1	1
AL	0.45	0.6	0.7	1
AR	0.45	0.6	0.7	1
AZ	0.45	0.6	0.7	1
IN	0.45	0.6	0.7	1
KS	0.45	0.6	0.7	1
LA	0.45	0.6	0.7	1
MD	0.45	0.6	0.7	1
MO	0.45	0.6	0.7	1
ND	0.45	0.6	0.7	1
NE	0.45	0.6	0.7	1
NM	0.45	0.6	0.7	1
NY	0.45	0.6	0.7	1
OK	0.45	0.6	0.7	1
VA	0.45	0.6	0.7	1
VT	0.45	0.6	0.7	1
WV	0.45	0.6	0.7	1
DC	0.45	0.6	0.7	1

Data come from Selden (2009). Eight states do not have income tax during the period.

Table 2.10 Federal statutory deduction fraction

Time Period	Fraction
Before 1986	0.00
1986-1995	0.25
1996	0.30
1997	0.40
1998	0.45
1999-2001	0.60
2002	0.70
2003-present	1.00

Data are presented in Heim and Lurie (2010)

Chapter 3 How do minimum medical loss ratio rules affect insurance markets? Historical evidence from Michigan

For years, too many middle-class families saw their health care costs go up and up and up, without much explanation as to why or how their money was being spent. But today, because of the Affordable Care Act, insurance companies have to spend at least 80 percent of every dollar that you pay in premiums on your health care -- not on overhead, not on profits, but on you. Now, many insurance companies are… bringing down premiums and providing better value to their customers.

--President Barack Obama (2013)

Introduction

The ratio of medical spending to premium revenue is called a medical loss ratio (MLR). In an effort to reduce premiums and promote value in health spending, the Affordable Care Act (ACA) establishes a minimum medical loss ratio rule in health insurance markets. The new rule requires insurers to spend at least 80 percent (and 85 percent for large group plans) of premium revenue on medical care or quality improvement activities, limiting the share that goes to administrative expenses and profits to no more than 20 percent. If insurers do not meet the minimum, they must issue a rebate to consumers. Policymakers argue that the minimum MLR rule will benefit consumers by encouraging insurers to reduce premiums in order to comply with the law (Center for Consumer Information and Insurance Oversight 2012b, 2012a).

However, the minimum MLR rule creates complex incentives for insurers, some of which could reduce consumer welfare. Harrington (2013) argues that insurers with MLRs above the minimum might increase premiums in order to compensate for the risk of paying a rebate when

medical care expenditures are lower than expected. The rule could increase market concentration and reduce choice for consumers if insurers have to exit the market because they cannot comply with the rule (Harrington 2013). Consumers could also face fewer choices if insurers exit state health insurance markets where it is relatively more difficult to meet the MLR requirements because of state taxes and licensing fees (Kirchhoff and Mulvey 2012). The minimum MLR rule could exacerbate wasteful spending if insurers comply with the rule by increasing expenditures on medical care or quality improvements that are not valued by consumers.

Finally a potentially important, but as yet unstudied, unintended consequence of the law is that it could facilitate tacit collusion, resulting in lower MLRs. The minimum MLR rule establishes a mandated floor for the MLR that aims to cap the price of insurance for a given level of benefit payments. Because this limits the price of health insurance, the premium, it is similar to a price ceiling. Knittel and Stango (2003), as well as Ma (2007) show that the establishment of a price ceiling can result in prices below the ceiling but above what would be the equilibrium price in an unregulated market.

The probable impact of the ACA's minimum MLR rule is an open question. This paper provides evidence that minimum MLR regulations can result in aggregate reductions in the MLR, consistent with tacit collusion. I use the introduction of a similar rule in Michigan in 1974 to measure the effect of this type of policy on MLRs. I outline a conceptual framework that illustrates how minimum MLR rules can result in tacit collusion and why the MLR may fall among for-profit insurers as a result. Then I estimate the effect of the rule on Michigan's for-profit insurers using state-level data on average insurer MLRs from the Health Insurance Association of America member surveys. The results suggest that the average MLR fell by at

least six percentage points among for-profit insurers in Michigan as a result of the rule. I find suggestive evidence that the reduction in average MLRs resulted through an increase in premiums.

This paper makes a contribution to the literature on health insurance regulation, as well as the rapidly growing literature on the ACA's minimum MLR rule. Karaca-Mandic et al. (2013) show that insurers in highly concentrated markets have lower MLRs. Karaca-Mandic, Abraham, and Phelps (2011) use data from the decade before the introduction of the ACA rule to predict its effect on insurers. McCue and Hall (2012), McCue, Hall, and Liu (2013), and Abraham, Karaca-Mandic, and Simon (2014) use data from 2010 and 2011 to estimate the effect of the ACA's rule on MLRs and their components in the first year. These early evaluations of the ACA's minimum MLR rule generally find that, in its first year, the rule had little effect on group market insurers' MLRs but increased MLRs among some individual market insurers. These increases in the MLR appear to be driven by for-profit insurers (McCue, Hall, and Liu 2013) and insurers that were initially below the threshold. Abraham, Karaca-Mandic, and Simon (2014) find that insurers with pre-rule MLRs more than 10 percent below the minimum increased their MLRs, but insurers with pre-rule MLRs more than 10 percent above the minimum decreased their MLRs.

There are three challenges that might limit the conclusions that can be drawn about the effects of the ACA's minimum MLR rule. First, the ACA's rule has only been in effect since 2011, and the administrative data are only available after several years (National Association of Insurance Commissioners, personal communication 2013). Therefore it will not be possible to estimate longer-term effects of the rule for at least several years. Second, the ACA's rule alters the definition of an MLR from the traditional one, requiring new information not previously recorded in administrative data. This limits the pre-period for evaluation of the ACA's minimum

MLR rule to one year (Karaca-Mandic et al. 2013). Third, it is difficult to find control groups for an evaluation of the rule because the ACA establishes a national policy that applies to nearly all insurers. Without a control group, reductions in the MLR due to the rule cannot be distinguished from secular trends. I attempt to address all three challenges by focusing on one state's minimum MLR rule, which was implemented in the 1970s. Because the rule took effect several decades ago and uses the traditional definition of an MLR, I have many more years of pre- and post-rule data. And because the rule only affected one state, I can exploit both political boundaries and program features as controls.

The policy

In 1974, Michigan became one of the first states to implement a minimum MLR rule (America's Health Insurance Plans 2010). At the time, insurers in Michigan were required to submit proposed insurance rates to the insurance commissioner the year before the plan was to be offered (P. Austin, personal communication 2013). In 1974, Michigan's administrative code was amended to require all insurance companies selling individual or group health insurance policies to include in their annual rate filing "an actuarial certification that the medical care payments provided are reasonable in relation to the premium charged and [that] shall show the anticipated loss ratio" (Michigan Compiled Laws).

As defined by the Michigan rule, the MLR is the ratio of expected medical care payments to expected premium revenues, adjusted for any part of the premium that was returned to beneficiaries. Because insurers must demonstrate that plans to be sold in the next calendar year meet the rule's requirements, the MLR for the purposes of the policy is a prediction based on past data (Michigan Compiled Laws). Equation (1) is an expression for the MLR:

(1) $MLR_t^{MI} = \frac{PVb_{i(t-1)} - Div_{t-1}}{PV\pi_{i(t-1)} - Div_{t-1}}$

MLR_t^{MI} is the MLR in Michigan at time *t.* $PVb_{i(t-1)}$is the present value of benefits, defined as medical care payments, the year before the insurance plan is offered. $PV\pi_{i(t-1)}$ is the present value of premiums the year before the plan is offered. Present value is defined as the value today of an amount of money in the future. Because health insurance contracts are written on a yearly basis, the present value of premiums or benefits is the expected premium in year *t*, the year the plan will be sold, discounted back one year to year *t-1,* the year the plan is submitted to the insurance commissioner. $Div_{t-1}$are any payments returned to plan beneficiaries by insurer *i,* called dividends. Because insurance is typically offered on a yearly basis, such dividends are rare. Equation (1) means that the MLR submitted to the Michigan insurance department is a predicted MLR for next year based on data from this year or last year with payments and premium revenue expressed in today's dollars.

According to the law passed in 1974, any MLR below 65 percent for group insurance or 55 percent for individual insurance is considered "unreasonable," and the insurer is not allowed to sell the proposed plan in Michigan (Michigan Compiled Laws). In addition, approval was withdrawn on September 30th, 1974 for all insurance plans that were approved for sale in 1975 if they did not meet the new requirements (Michigan Compiled Laws). Thus, insurers were prevented from offering coverage in 1975 that did not comply with the law.

Conceptual framework

Health insurance markets are characterized by high market concentration (Harrington 2013; Dafny 2010). In a setting with few sellers, the decisions of one insurer can depend on

those of others in the market. All things being equal, profit-maximizing insurers should seek to minimize MLRs, but insurance plans with relatively low MLRs may lose market share to plans with higher MLRs if people select their insurance plans based on the MLR or related characteristics such as actuarial value. If insurers could collude to offer insurance with a cost-minimizing MLR, they could enjoy higher profits.

In the absence of a minimum MLR rule, insurers would be unlikely to collude on MLRs for two reasons. First, explicit collusion is illegal, and tacit collusion—choosing the same MLR without explicitly communicating—is difficult to coordinate. Second, even if insurers were able to collude, maintaining the colluding equilibrium would be difficult because it is costly: each insurer could temporarily capture market share and increase profits by deviating from the equilibrium.

Establishment of a minimum MLR rule should change health insurance markets in two ways that facilitate tacit collusion, which could therefore result in lower MLRs. First, a minimum MLR rule creates a mandated floor that can serve as a focal point (Schelling 1960), defined as a natural or obvious price such as a regulatory ceiling or floor. When firms can set prices but cannot explicitly collude, they may cluster around a focal point. Knittel and Stango (2003), as well as Ma (2007) show that this occurs with price ceilings. Focal points allow tacit collusion, which can be stable over time (Eckert 2004; Leufkens and Peeters 2011). Second, a minimum MLR rule reduces the costs of tacit collusion by removing asymmetric information between insurers. The rule requires insurers to report MLRs to insurance commissioners each year, making MLRs effectively public through official processes such as the Freedom of Information Act (Michigan Department of Insurance and Financial Services, personal

communication,). Lowering the cost of collusion helps make the colluding equilibrium more stable over time.

If firms tacitly collude on MLRs, the result is an MLR that is lower than it would in an unregulated equilibrium. If the unregulated MLR exceeds the new minimum mandated MLR, the MLRs of for-profit insurers should decrease toward the focal point. To test for tacit collusion, I need to observe the distribution of insurers' MLRs, which requires insurer-level data. Unfortunately, only state-level data are available. Therefore, I cannot test for tacit collusion directly. However, I can look for evidence *consistent* with tacit collusion using aggregate data. Aggregate data are state-level, enrollment-weighted averages of commercial (for-profit) and Blue Cross and Blue Shield (non-profit) insurance plans in the market. If most people are enrolled in plans with MLRs above the minimum, and insurers that sell these plans reduce their MLRs in response to the policy, then aggregate MLRs will also fall. However, if most people are enrolled in plans with MLRs below the minimum that must increase their MLRs in response to the policy, aggregate MLRs may not fall, even if insurers with MLRs above the minimum still reduce their MLRs. To summarize, I hypothesize that for-profit insurers' MLRs will fall as a result of the rule (hypothesis 1).

The conceptual framework outlined above describes the behavior of profit-maximizing firms. However, Blue Cross and Blue Shield (BCBS) insurers may not be affected by the rule in the same way because they might not have the same incentives to minimize the MLR. Additionally, the MLRs of non-profit insurers are quite high, averaging over 90 percent, so no BCBS insurers are likely to be forced to comply with the law. Therefore, I hypothesize that non-profit insurers' MLRs will not be affected by the rule (hypothesis 2).

Data

Dataset

The ideal dataset for evaluating Michigan's minimum MLR rule would have observations at the insurer-level from each state. Unfortunately, such data are unavailable. State-by-year data are less useful because they do not allow me to estimate the effect of the rule separately for insurers with low pre-rule MLRs versus those with high pre-rule MLRs. Separating these effects is important because insurers below the minimum must increase their MLRs in order to continue operating in Michigan, but insurers above the minimum could reduce their MLRs to the minimum (Abraham, Karaca-Mandic, and Simon 2014). Instead, I am only able to estimate an effect on the average MLR among insurers in that state. This average will be affected by the share of the health insurance market that is enrolled in low or high pre-rule MLR plans and the size of the effect for each type of insurer.

To estimate the effect of a minimum MLR rule on MLRs, I construct a dataset of state-level premium revenue and total benefit payments for commercial and BCBS insurers for each year between 1969 and 1980 using Health Insurance Association of America (HIAA) Sourcebook of Health Insurance Information reports (Health Insurance Association of 1959-1980). The reports contain state-by-year statistics from HIAA member surveys, which gather information on operations from all insurance companies offering group, or individual accident and health insurance policies. In 1966, HIAA's sample represented 99 percent of membership in commercial insurance plans in the group market, and 83 percent of membership in commercial insurance plans in the individual market (Reed 1967). The surveys did not sample BCBS insurers. Information on premiums and benefits for BCBS plans come from the National

Association of Blue Shield Plans and the Blue Cross Association (Reed 1967) and were republished by HIAA in the Sourcebook of Health Insurance reports. From these data, I calculate state-by-year MLRs for commercial and BCBS insurers, defined as total benefit payments divided by aggregate premium revenue.

I supplement my dataset with population estimates from the Surveillance, Epidemiology, and End Results Program. From these data, I obtain estimates of total state population, elderly population (65+), working age population (20-64), and the fraction of the population that is female (Surveillance Epidemiology and End Results Program 1969-2012). I also include estimates of state per capita income from the Bureau of Economic Analysis (Bureau of Economic Analysis 1969-1980) and measures of the size of the state's hospital and surgical insurance markets in thousands of covered lives from the HIAA member surveys. These covariates partially control for differences in health insurance and medical care demand between states, over time. Descriptive statistics for the sample can be found in the appendix (Table 3.7).

Descriptive Trends

Figure 3.1 illustrates the trend in MLRs and the timing of the Michigan rule. Because the rule requires insurers to obtain certification in the calendar year before the plan is sold, a change in forecasted MLRs that took place in 1974 did not affect actual MLRs until 1975. Similarly, if insurers did not anticipate the rule, any response to the rule would not be observable until 1976 or later. The left panel of the figure illustrates the trend in MLRs for commercial insurers. Although average MLRs are higher before 1976, the trend in Michigan looks similar to the rest of the United States, but the level is much higher. After 1976, however, the average MLR rises in the rest of the United States yet remains constant in Michigan. This suggests that Michigan's

commercial insurers diverged from prevailing trends. The right panel of the figure is noisy but shows no clear trend in the MLR for BCBS insurers.

Table 3.1 presents the data in Figure 3.1 in terms of simple, unadjusted means of the MLR and its components. The table divides the data into Michigan and the rest of the United States, before and after the rule took effect, providing a simple difference-in-differences estimate. Michigan's commercial MLR did not change between the pre- and post-periods (80%). Without a control group, it would appear that the rule had no effect. However, MLRs in the rest of the country rose faster, from 71 to 78 percent between the pre- and post-periods (6p.p.). If the rest of the United States can serve as a counterfactual for Michigan had the minimum MLR rule not been adopted, Table 3.1 implies that Michigan's commercial MLR actually fell (-6p.p.). BCBS loss ratios remained high (93-94%) in both Michigan and the United States, before and after the rule took effect, suggesting no change.

Aggregate premiums increased among commercial insurers in Michigan after the policy ($21,000), but they increased less in the rest of the United States ($9,000). Comparing these two differences suggests that premiums increased by $12,000 as a result of the rule. I observe a similar pattern for total benefit payments, although the implied effect is smaller; total benefit payments only increased by $6,000. For Michigan's BCBS insurer, aggregate premiums and benefits both increased relative to the rest of the United States after the policy took effect, but premiums and benefits increased by a similar amount ($55,000).

Estimation strategy

The purpose of this paper is to estimate the effect of a minimum MLR rule in Michigan on insurer MLRs and their components, comparing trends in Michigan to a plausible

counterfactual. I use a difference-in-differences strategy, comparing the change in the MLR in Michigan before and after 1976, to the change in the rest of the United States. Table 3.1presents difference-in-differences estimates in terms of unadjusted means, but static or time-varying differences between states, as well as secular trends in the MLR and its components may confound the simple estimates. To control for potential confounding factors, I estimate the following multivariate regression:

$$(2)\ Y_{st}^{m} = \beta_0 + \beta_1 MI_s + \beta_2 POST_t + \beta_3 (MI \times POST)_{st} + \gamma X_{st} + g_t + f_s + u_{st}$$

Y_{st}^{m} denotes the state-level MLR for insurers of type *m*—where *m* is either commercial or BCBS—in state *s* at time *t*. In alternate regressions, Y_{st}^{m} denotes the aggregate components of the MLR: state-level premium revenue or state-level total benefit payments. MI_s is an indicator set to one if the observation comes from the state of Michigan, and $POST_t$ indicates that the observation comes from 1976 or later—the first year the rule would have affected MLRs. The parameter β_3 gives the effect of the rule on MLRs in Michigan.

To control for differences in the characteristics of health insurance markets, I add time-varying state-level controls, X_{st}. These controls include factors that affect the demand for health insurance and health care, such as per capita income and the age and sex distribution of the population. In select specifications, I also include the size of the hospital and surgical insurance markets in thousands of covered lives. In all models, I include year and state fixed effects g_t, f_s to control for time-invariant state characteristics and common trends in the MLR over time. All

regressions are estimated by ordinary least squares with heteroskedasticity-robust standard errors, clustered at the state level.

Unobserved state-level factors that are not captured by state-specific linear time trends could threaten the internal validity of my difference-in-differences design. For example, changes in economic conditions specific to Michigan could affect the MLR and bias estimates of β_3. To control for this type of confounding factor, I also employ a triple-difference estimator with a within-state control group, according to equation (3):

$$(3) Y_{mst} = \alpha_0 + \alpha_1 MI_s + \alpha_2 POST_t + \alpha_3 COMM_m + \alpha_4 (MI \times POST)_{st} + \alpha_5 (MI \times COMM)_{ms} + \alpha_6 (POST \times COMM)_{tm} + \alpha_7 (MI \times POST \times COMM)_{stm} + \gamma X_{st} + g_t + f_s + e_{stm}$$

$COMM_m$ indicates that the observation is from commercial insurers rather than BCBS insurers. The parameter α_7 is the coefficient on the triple interaction and gives the change in commercial MLRs after the rule, between Michigan and the rest of the United States, using the change in BCBS insurer MLRs relative to the rest of the United States as a control group.

The estimation sample includes 11 years of data (1969-1980) from the contiguous United States, excluding two states: Pennsylvania and Tennessee. These states enacted their own minimum MLR rules in 1975 and 1974 respectively (America's Health Insurance Plans 2010). I omit Tennessee because the state had a policy similar to a minimum MLR rule dating as far back as 1945 (America's Health Insurance Plans 2010). I omit Pennsylvania because it yields relatively fewer post-period observations than the rest of the states. Regressions that include controls for the size of the insurance market do not include data from the years 1968, 1978, or 1980 because information on the size of the insurance market is unavailable.

One potential problem with the estimates is that they may also be influenced by outliers. Figure 2.2 shows MLRs for both the commercial and BCBS markets, with dashed lines indicating zero and one. Over the study period, 45 observations have an MLR that exceeds one. Because premium revenue must cover both benefit costs and administrative costs, in expectation the MLR should be bounded from above by one. Observations may exceed one if insurers under-predict the cost of reimbursing medical care. To address outliers, I use robust regression. Robust regression uses Cook's D statistic to remove influential outliers from the sample and then re-weights the remaining observations to calculate an unbiased ordinary least squares estimate (Berk 1990).

Another problem with the estimates is that there is only one treated group. When the number of treated groups is small, the standard asymptotics do not apply, and the standard errors will be incorrect (Donald and Lang 2007; Conley and Taber 2011). To address this problem, I follow Buchmueller, DiNardo, and Valletta (2011) and Abadie, Diamond, and Hainmueller (2010) in using a version of Fisher (1935) permutation test to generate more conservative confidence intervals for the estimates. I estimate equation (2) and equation (3) 47 times, replacing the treatment state with each state in the sample. This allows me to estimate 46 placebo effects and one estimate of the treatment effect for Michigan. I then use the estimates to approximate the sampling distribution of the treatment effect. If the treatment effect using Michigan is significant at the five percent (10%) level, it will rank in the top or bottom 2.5 percent (5%) of the distribution.

Results

Michigan's Commercial MLR fell relative to the rest of the United States

My research strategy compares the change in MLRs in Michigan before and after the rule to the change in the rest of the United States, as in equation (2). Table 3.2 shows that MLRs fell by seven percentage points (s.e.=0.0082) in a model with state and year fixed effects, as well as state-level demographic and income controls (column 1). Adding controls for the size of the health insurance market (column 2), the effect increases to eight percentage points (s.e.=0.0092). Relative to the average MLR among commercial insurers before the minimum MLR rule (80%), this point estimate represents an 8-10 percent decrease, in keeping with hypothesis one.

Few of the controls are significant. As expected, the coefficient on the Michigan dummy is both positive and statistically significant because the average MLR among Michigan's commercial insurers is higher than that of insurers in the rest of the nation, as shown in Figure 3.1 and Table 3.1. Additionally, the post-1975 dummy is positive and statistically significant because MLRs are rising over the period.

These results should be interpreted with caution. First, the presence of outliers could influence the results. In order to reduce this possibility, I re-estimate equation (2) using robust regression. The results are similar in magnitude to the ordinary least squares estimates, but the standard errors are larger. In a model with state and year fixed effects and state-level demographic and income controls (column 3), Michigan's commercial MLR fell by about six percentage points (s.e.=0.022). Adding controls for the size of the hospital and surgical insurance markets (column 4), the reduction increases to seven percentage points (s.e.=0.026). In terms of a proportional reduction, these point estimates represent about a nine percent decrease.

Secondly, the standard errors, even allowing arbitrary correlation in the error terms between observations from the same state, may be incorrect because there is only one treated unit. In order to provide more conservative estimates, I use the permutation test, as described above. Figure 3.3 shows the distribution of the treatment and placebo effects. Michigan's point estimate of -0.07925 percentage points ranks in the bottom 5th percentile of the distribution, making it statistically significant at the 10 percent level in a two-sided test.

Michigan's BCBS MLR was unaffected

Figure 3.1 suggests that Michigan's BCBS insurers were unaffected by the rule, and Table 3.3 provides support for this observation. Controlling for state and year fixed effects as well as state-level income, and demographic factors, Michigan's BCBS MLR increased by a statistically insignificant one percentage point (s.e.=0.024). Adding controls for the size of the health insurance market, the effect falls by an order of magnitude to 0.1 percentage points (s.e.=0.0174). The results are similar adjusting for outliers using robust regression (columns 3-4). Together, these results suggest that BCBS MLRs were unaffected, in keeping with hypothesis two.

Michigan's commercial MLR fell relative to the rest of the United States and to BCBS insurers

A potential threat to my difference-in-differences estimates is that there could be time-varying, state-level confounding factors that affect Michigan differently than the rest of the United States. One way to control for such threats is to use a within-state control group that would also be affected by such time-varying factors but not by the treatment. Because Michigan's BCBS insurers were likely subject to similar health insurance demand and regulatory

and economic conditions but were unaffected by the rule, they can serve as a within-state control group for commercial insurers. Table 3.4 presents the results from regressions specified in equation (3). The coefficient of interest is the triple interaction, which gives the difference in the difference-in-differences estimates presented in Table 3.2. The interaction coefficient is negative and statistically significant, meaning that commercial MLRs fell as a result of the rule. Controlling for state and year fixed effects and state-level income and demographic controls (column 1), Michigan's commercial MLR fell by eight percentage points (s.e.=0.0129). The results are robust to the inclusion of controls for the size of the health insurance market (column 2). In terms of the pre-rule MLR among Michigan's commercial insurers (80%), these point estimates imply a 10 percent decrease in the MLR after the rule took effect. Adjusting for potential outliers with robust regression, the size of the standard errors increases, but the magnitude of the effect remains unchanged. The results of the permutation test are displayed in Figure 3.4; Michigan's point estimate of -0.0819 ranks in the 6th percentile of the distribution of state effects and is therefore only marginally statistically insignificant at the 10 percent level.

Premiums increased disproportionately among Michigan's commercial and BCBS insurers

Because the MLR is the ratio of benefits to premiums, the estimated reduction in the MLR for commercial insurers could arise as a result of a disproportionate increase in premiums or a disproportionate decrease in total benefits. To investigate this issue, I examine the effect of the rule on premiums and benefits separately.

Figure 3.5 provides descriptive premium revenue and benefit payment trends for Michigan and the rest of the United States, for both commercial and BCBS insurers. Although the level is higher in Michigan, benefit payments and premium revenue before the rule took

effect appear to rise at the same rate as in the rest of the United States. After the rule, however, both commercial benefit payments and premium revenue appear to have increased slightly, but premium revenue growth slightly outpaced benefit payment growth. Among Michigan's BCBS insurers, both the level and the rate of premium revenue growth are greater than those in the rest of the United States. While benefit payments and premium revenue are rising non-monotonically, there appears to be no trend break at 1976 for benefit payments and a slight trend break at 1976 for premium revenue. This suggests that BCBS insurers might have also been affected by the rule, and therefore should not be used as a within-state control group for Michigan's commercial insurers.

To test whether premiums or benefits payments for Michigan's commercial and BCBS insurers were affected by the rule, I re-estimate equation (2) using state-level premium revenue and benefit payments for each type of insurer as the dependent variable. Table 3.5 presents the results for commercial and BCBS premium revenue for my preferred specification with the full set of controls. For differences-in-differences, I find that commercial premium revenue rose by $13,800 (s.e.=$1,800) using ordinary least squares (column 1), and nearly $10,000 (s.e.=$2,000) using robust regression (column 2). As a fraction of Michigan's pre-rule commercial premium revenue ($129,000), this effect represents a 7-11 percent increase.

As Figure 3.5 suggests, BCBS premiums increased in Michigan relative to the rest of the United States. The estimated increase is about $16,000 (s.e.=$2,100) using ordinary least squares (column 3) or robust regression (s.e.=$2,200, column 4). As a fraction of Michigan's BCBS MLR before the rule took effect ($180,000), the effect represents a statistically significant nine percent increase. This result casts doubt on the use of BCBS insurers as a control in triple-

difference estimates. Therefore, I do not re-estimate equation (3) using the components of the MLR as dependent variables.

Table 3.6 presents the results for commercial and BCBS aggregate benefit payments. Estimating the effect using differences-in-differences by ordinary least squares (column 1), I find that benefits increased by $8,300 (s.e.=$2,000), but the effect becomes small and statistically insignificant at the five-percent level using robust regression ($2,800, s.e.=$1,700, column 2). Scaled by aggregate benefit payments among Michigan's commercial insurers before the rule took effect ($103,000), the ordinary least squares estimate implies an eight percent increase in benefit payments, while the robust regression estimates imply no change. Among BCBS insurers, the point estimates suggest that benefits increased by $15,100 (s.e.=$1,500) using ordinary least squares (column 3) and did not increase at all using robust regression ($1,400, s.e.=$1,600, column 4).

Figure 3.6 applies the multiple permutation test to the difference-in-difference results for commercial premiums and benefits. Relative to the 46 placebo effects provided by other states in the sample, Michigan's estimated effect is not statistically significant for either premiums or benefits.

Taken together, Table 3.5 and Table 3.6 suggest that premium revenue increased among commercial and BCBS insurers, but aggregate benefit payments may not have changed. The estimated increases in premium revenue for both commercial and BCBS insurers are proportionally similar, suggesting that the law could have affected both types of insurers. Although the estimated increase in premium revenue survives robust regression, it does not survive the multiple permutation test.

Discussion

The findings of this paper suggest that the establishment of a minimum MLR rule can result in unintended consequences. The introduction of such a rule in Michigan resulted in at least a six percentage point decrease in MLRs among for-profit commercial insurers relative to the rest of the United States. My preferred specification suggests that the reduction in MLRs was driven by a disproportionate increase in premium revenue.

Although I cannot explicitly test for it, the results are consistent with tacit collusion, a potentially important implication of price regulation previously unstudied in the context of minimum MLR regulations. Establishment of a mandated minimum MLR creates a focal point in the market for health insurance. Such focal points could facilitate tacit collusion by providing a natural or obvious price that serves as a coordination target for firms in the absence of explicit collusion (Knittel and Stango 2003; Ma 2007).

In order to make generalizations from the effects in Michigan to the ACA's minimum MLR rule, it is important to compare the two rules. Michigan's rule differs from the ACA's minimum MLR rule in two ways. First, the ACA's rule is based on actual MLRs, while the Michigan rule is based on predicted MLRs. If actual and predicted MLRs differ systematically, Michigan's rule could be more or less binding than the ACA's. Second, the ACA modifies the definition of an MLR. The numerator of the ratio includes not only medical care reimbursement but also expenditures on quality improvement activities such as wellness or health promotion programs (Kirchhoff and Mulvey 2012). The denominator is also modified; it is defined as premium revenue less state taxes and regulatory fees.

Despite these definitional differences, Michigan's minimum MLR rule resembles the ACA's rule in several ways that should affect the actions of insurers. Both establish a minimum MLR that applies nearly universally, and the relative stringency of the minimum is similar; both rules have minimums that are 5-10 percentage points lower than the average prevailing loss ratio (America's Health Insurance Plans 2011; Harrington 2013). Both rules also have an enforcement mechanism; under the ACA, insurers must pay a rebate if their MLRs fall below the minimum, while, under Michigan's rule, insurers are forbidden from offering plans that do not meet the requirements. Most importantly for generalization of these results to the ACA, both rules contain features that facilitate tacit collusion. First, the ACA's mandated minimum can serve as a focal point. Second, the law's explicit public reporting requirements for MLRs should reduce information asymmetries between insurers more than Michigan's rule does, thus further lowering the cost of maintaining a colluding equilibrium over time.

This paper makes a number of contributions to the literature on minimum MLR rules. It is the first to use an individual state's minimum MLR rule to evaluate the effect of such a policy. Although over 30 states had such rules at the time of the passage of the ACA (America's Health Insurance Plans 2010), the effect of this type of rule on state insurance markets had never been estimated. Previous natural experiments provide a unique opportunity to estimate the effects of minimum MLR rules, given the lack of straightforward control groups under the ACA. I find that a control group, in this case, the rest of the United States, is important; without one, I would have concluded that Michigan's minimum MLR rule resulted in higher MLRs.

The paper has several limitations. First, because the data are aggregated to the state level, it is not possible to accurately measure individual insurers' response to the policy. In theory, commercial MLRs could also fall if people with higher health expenditures switch from

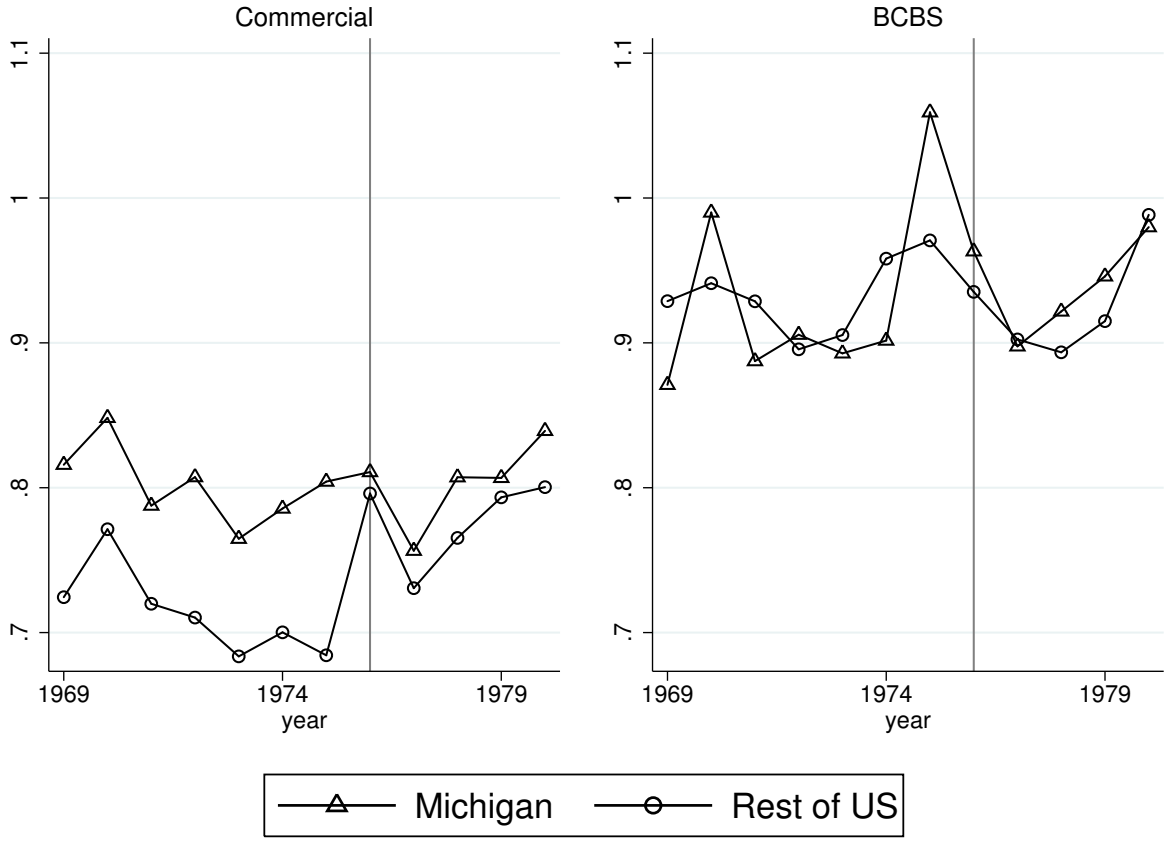
commercial insurance to BCBS plans because they have a higher MLR. The remaining individuals who purchase commercial insurance could be less costly, and therefore medical care reimbursements would temporarily fall as a result of selection rather than insurers' response to the minimum MLR rule. This is likely not a significant problem because I detect no change in the BCBS MLR. Second, I am unable to control for market concentration because these data are not readily available for this period. Karaca-Mandic et al. (2013)show that more concentrated markets have lower MLRs, so within-state changes in market structure that are not eliminated in the triple-difference specification could affect the results. It is unclear how omission would bias the estimated effects. Finally, I estimate triple-difference models using non-profit BCBS insurers as a control group for commercial insurers because I predict that they should not be affected by the policy. In fact, my results suggest that premiums were affected, and therefore BCBS insurers should not serve as a control group for commercial insurers. Fortunately, this does not change the conclusion of the paper, because the difference-in-difference and triple difference results tell the same story.

Conclusion

The ACA establishes a minimum MLR rule, yet very little is known about its likely effect on insurance markets. This paper uses the adoption of a minimum MLR rule in Michigan to show that such rules can result in lower MLRs, potentially as a result of tacit collusion. If the results from this paper can be generalized to today, this paper has an important policy-relevant conclusion: the establishment of a minimum MLR rule could result in a reduction in some insurers' MLRs. As a result, policymakers should be as worried about maintaining the MLRs of insurers above the minimum as they are with increasing loss ratios of those below it.

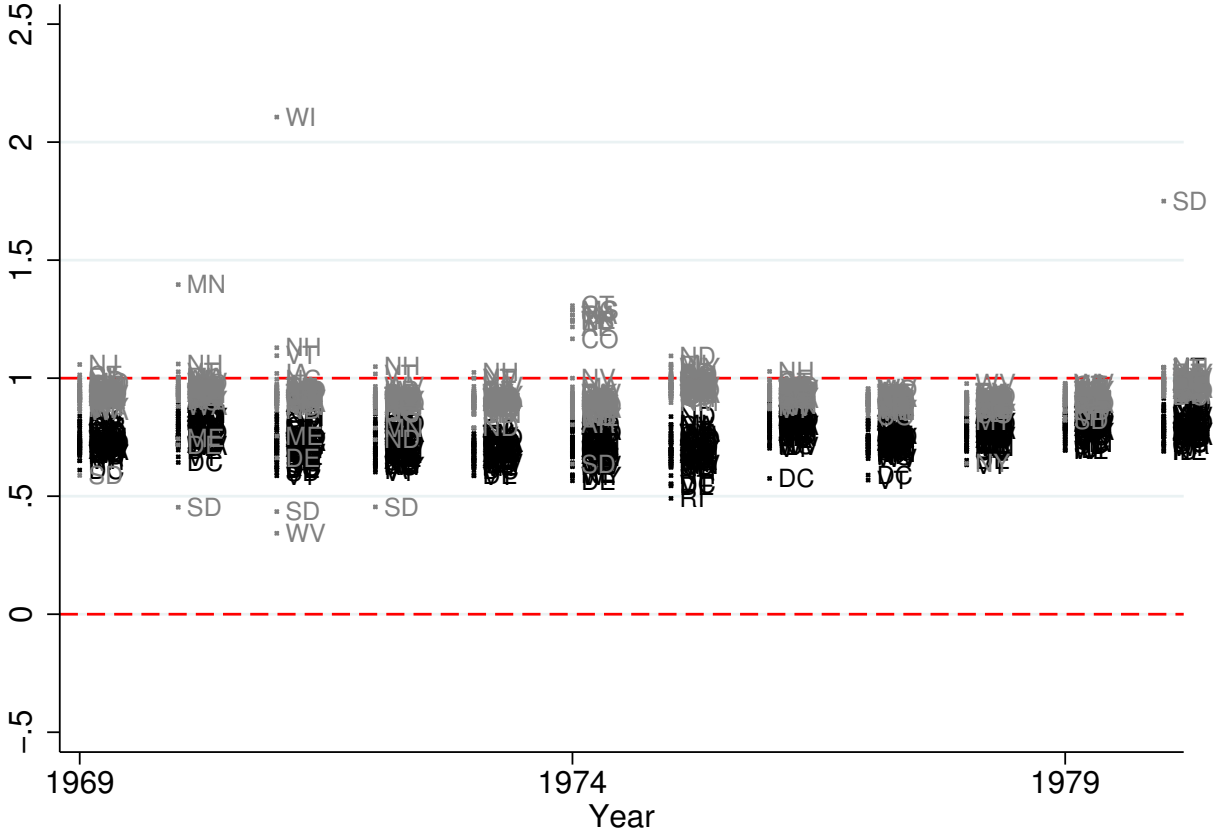
Figures and Tables

Figure 3.1 Trends in the medical loss ratio: commercial and Blue Cross and Blue Shield insurers



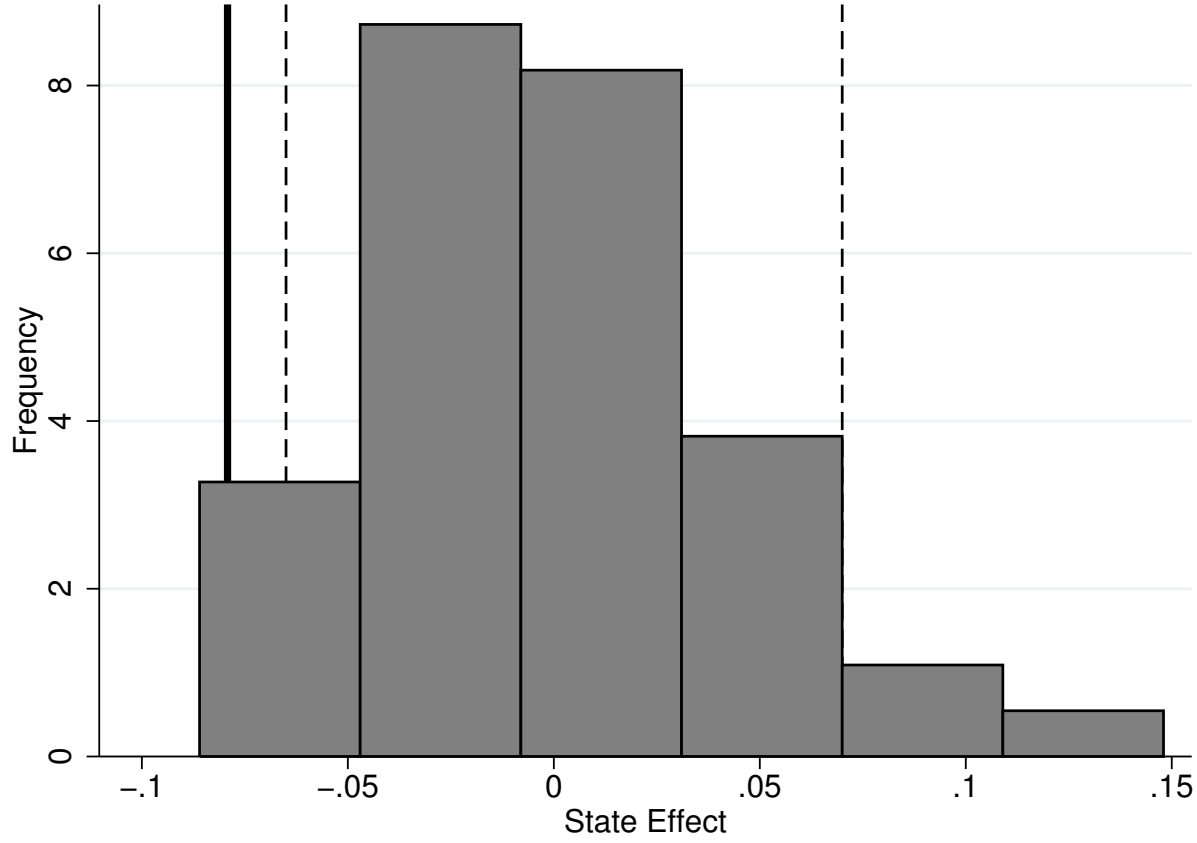
The figure plots the average state-level medical loss ratio for commercial (left panel) and Blue Cross and Blue Shield (right panel) insurers in Michigan (triangles) and the rest of the United States (circles). The data come from Health Insurance Association of America surveys and Blue Cross and Blue Shield Association surveys. The vertical line marks 1976, the first year in which health insurance plans would have been affected by insurers' response to the policy.

Figure 3.2 Medical loss ratio outliers



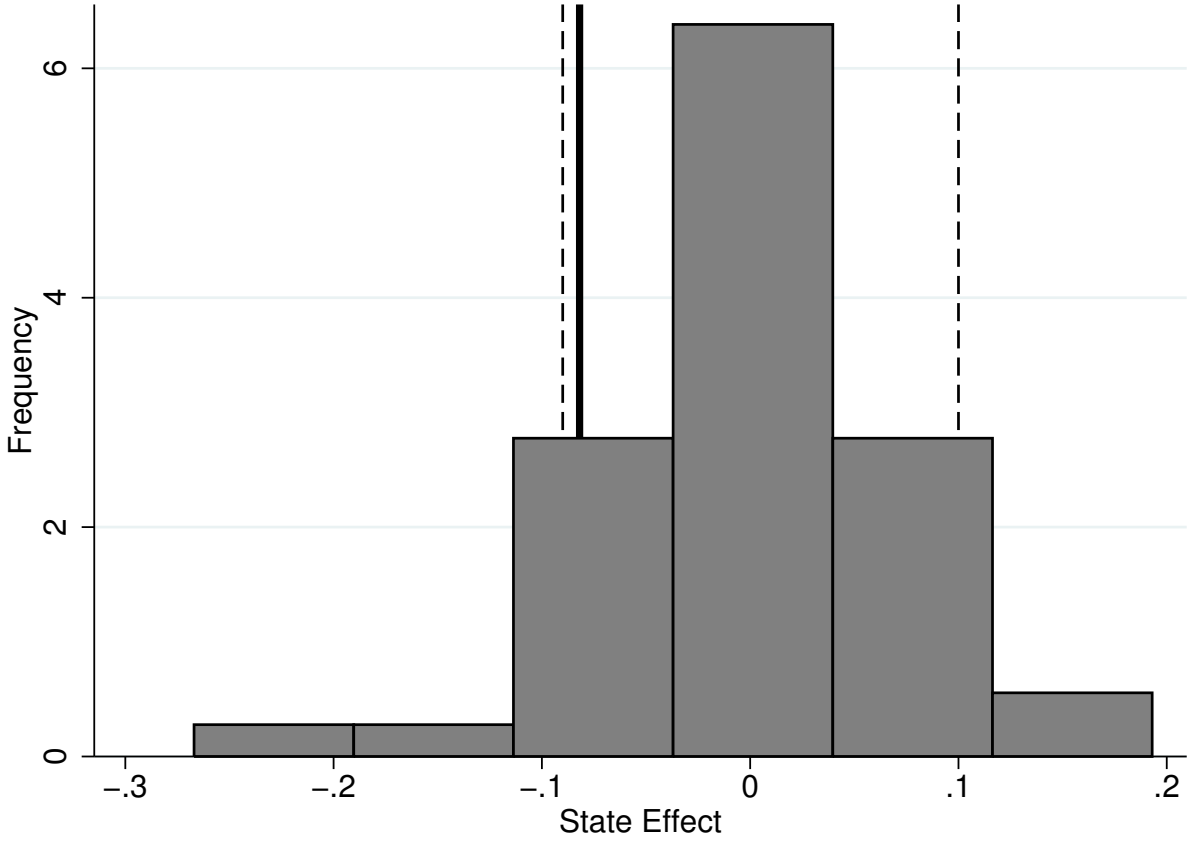
The figure plots the raw data for the estimation sample. Each point represents a state-level MLR for either commercial (black) or Blue Cross and Blue Shield (gray) insurers. The dashed horizontal lines mark 1 and 0, the bounds for the MLR.

Figure 3.3 State difference-in-difference effects: commercial medical loss ratios



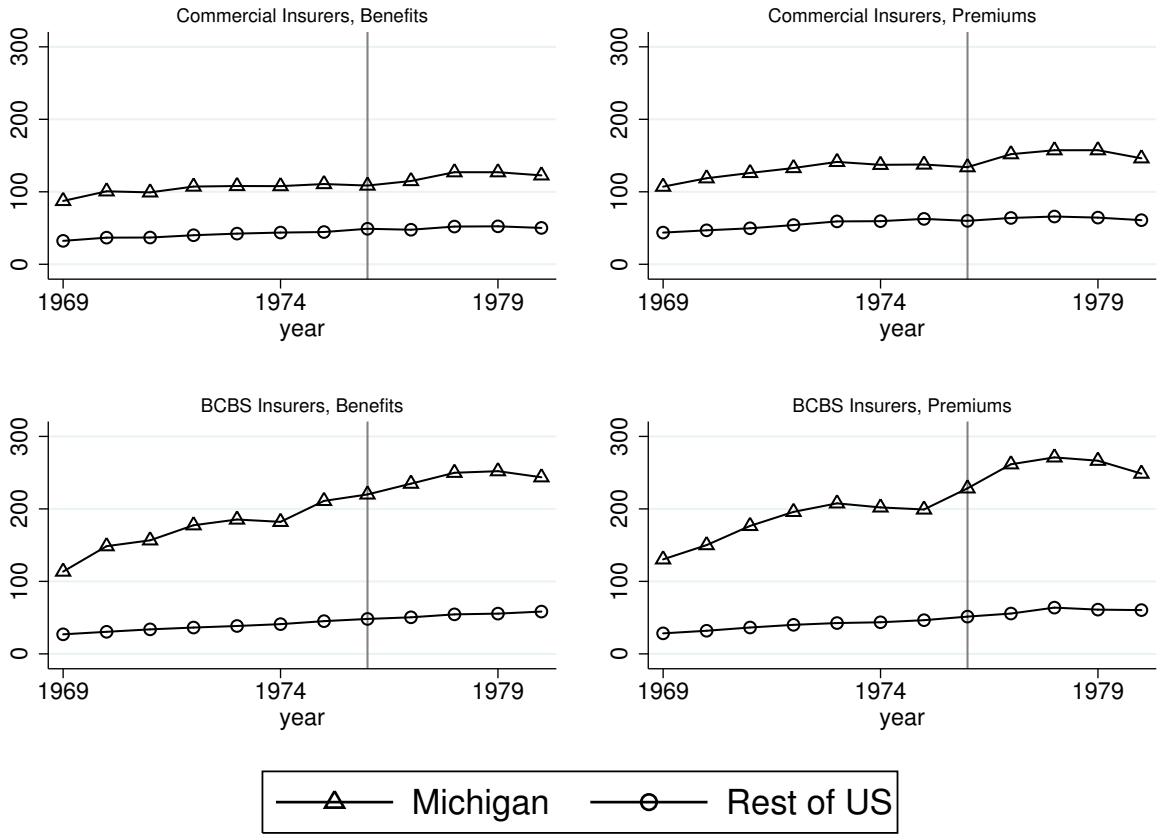
The figure plots the distribution of difference-in-differences estimates (eq(3)) using each state in the estimation sample (N=47) as the control group. All estimates are obtained using ordinary least squares and include state and year fixed effects, as well as controls for state population, the age and sex distribution of the population, and the size of the health insurance market. Standard errors are clustered at the state level. The dashed vertical lines mark the 5th and 95th percentiles. The black vertical line marks the effect for Michigan.

Figure 3.4 State triple difference effects: commercial medical loss ratios



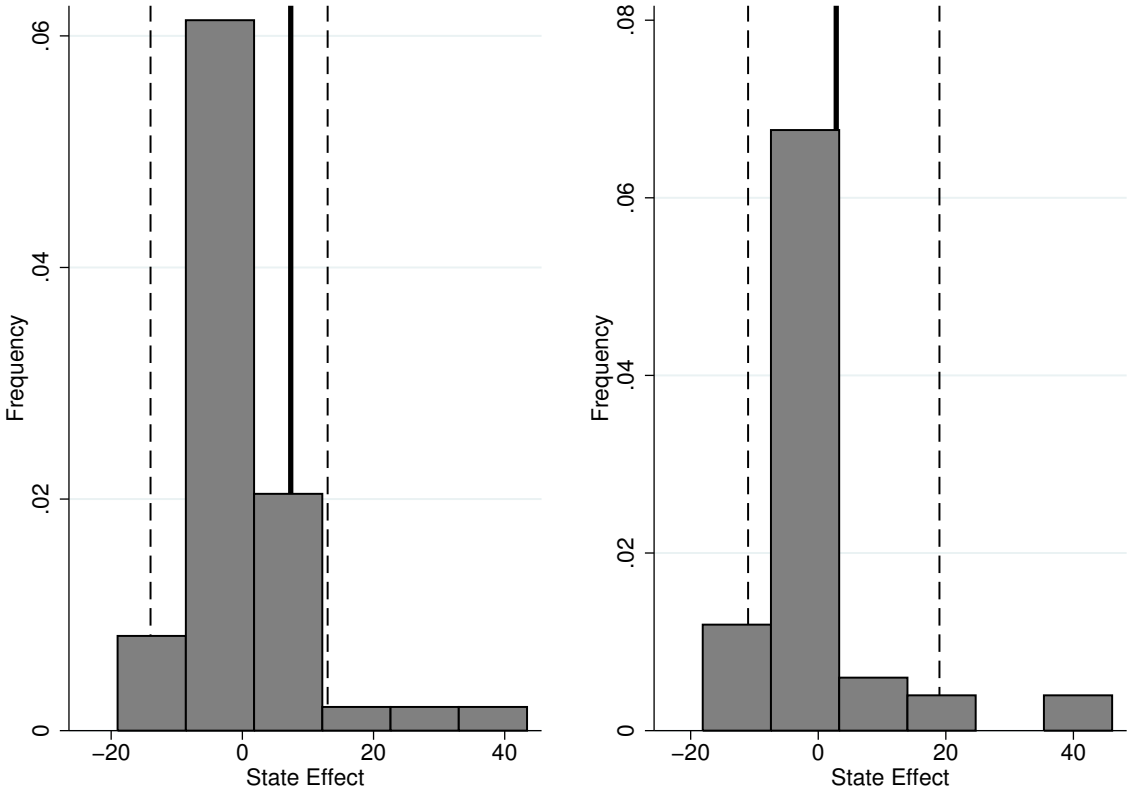
The figure plots the distribution of triple difference estimates (eq(4)) using each state in the estimation sample (N=47) as the control group. All estimates are obtained using ordinary least squares and include state and year fixed effects, as well as controls for state population, the age and sex distribution of the population, and the size of the health insurance market. Standard errors are clustered at the state level. The dashed vertical lines mark the 5th and 95th percentiles. The black vertical line marks the effect for Michigan.

Figure 3.5 Trends in premiums and benefits: commercial and Blue Cross and Blue Shield insurers



The figure plots the average state-level components of the medical loss ratio for commercial (top panel) and Blue Cross and Blue Shield (bottom panel) insurers in Michigan (triangles) and the rest of the United States (circles). Premiums and benefits represent total premium revenue and benefit payments and are in 1980 dollars and expressed in thousands of dollars. The data come from Health Insurance Association of America surveys and Blue Cross and Blue Shield Association surveys. The vertical line marks 1976, the first year in which health insurance plans would have been affected by insurers' response to the policy.

Figure 3.6 State difference-in-difference effects: commercial and Blue Cross and Blue Shield premiums and benefits



The figure plots the distribution of difference-in-difference estimates (eq(2)) using each state in the estimation sample (N=47) as the control group. The left panel shows the state effects for premiums and the right panel shows the results for benefits. All estimates are obtained using ordinary least squares and include state and year fixed effects, as well as controls for state population, the age and sex distribution of the population, and the size of the health insurance market. Standard errors are clustered at the state level. The dashed vertical lines mark the 5th and 95th percentiles. The black vertical line marks the effect for Michigan.

Table 3.1 Descriptive trends in the medical loss ratio and its components

	MLR		Premiums		Benefits	
	Commercial	BCBS	Commercial	BCBS	Commercial	BCBS
Michigan						
1969-1975	0.80	0.93	129	180	103	168
1976-1980	0.80	0.94	149	255	120	240
Difference	*0.00*	*0.01*	*21*	*75*	*17*	*72*
Rest of US						
1969-1975	0.71	0.93	54	38	40	36
1976-1980	0.78	0.93	63	58	50	53
Difference	*0.06*	*0.00*	*9*	*20*	*11*	*17*
Difference in Differences	*-0.06*	*0.01*	*11*	*55*	*6*	*55*

The table displays the average state-level MLR, premium revenue, and aggregate benefit payments for commercial and Blue Cross and Blue Shield insurers in Michigan, and the rest of the United States, in the period before the law took effect (1969-1975) and the years after the law took effect (1976-1980).

Table 3.2 The effect of Michigan's minimum medical loss ratio rule on commercial loss ratios

	(1)	(2)	(3)	(4)
Michigan	0.119**	0.095	0.124**	0.092
	[0.036]	[0.062]	[0.026]	[0.048]
Post-1975	0.073**	0.113**	0.062**	0.106**
	[0.018]	[0.016]	[0.013]	[0.010]
Michigan Post-1975	-0.068**	-0.079**	-0.061**	-0.074**
	[0.008]	[0.009]	[0.022]	[0.026]
Working Aged Population (20-64)	0.00003	0.00012	-0.00004	-0.00014
	[0.0003]	[0.0004]	[0.0002]	[0.0002]
Elderly Population (65+)	-0.00001	-0.00006	0.00011	0.00024
	[0.0003]	[0.0005]	[0.0002]	[0.0003]
Total Population	-0.0004	-0.0003	-0.0003	-0.0000
	[0.0006]	[0.0008]	[0.0004]	[0.0005]
Fraction Female	2.1	3.991	1.42	3.3*
	[2.7]	[3.64]	[1.25]	[1.7]
Per Capita Income	-0.0011	-0.0040	-0.0012	-0.0031
	[0.0023]	[0.0034]	[0.0017]	[0.0022]
Size of Hospital Insurance Market		-0.0001		-0.0001
		[0.0001]		[0.0001]
Size of Surgical Insurance Market		0.00001		0.00001
		[0.0001]		[0.0001]
Constant	-0.3	-1.351	0.021	-1.01
	[1.4]	[1.88]	[0.64]	[0.85]
Observations	564	423	564	423
R-squared	0.71	0.74	0.75	0.78

The table displays results from regressions as specified in equation (2). The dependent variable is the average state-level MLR among commercial insurers. Columns 1 and 2 are estimated by ordinary least squares with heteroskedasticity robust standard errors clustered at the state level. Columns 3 and 4 are estimated by robust regression. All models include state and year fixed effects. * indicates statistical significance at the 1% level, and * indicates significance at the 5% level.

Table 3.3 The effect of Michigan's minimum MLR rule on BCBS MLRs

	(1)	(2)	(3)	(4)
Michigan	-0.0412	-0.084	-0.018	-0.049
	[0.046]	[0.058]	[0.021]	[0.039]
Post-1975	0.046*	-0.068**	0.056**	-0.0470**
	[0.022]	[0.017]	[0.011]	[0.0083]
Michigan × 1975	0.0103	0.001	0.027	0.011
	[0.024]	[0.017]	[0.018]	[0.021]
Working Age Population (20-64)	0.00001	-0.00015	-0.00005	-0.00011
	[0.00026]	[0.00045]	[0.00013]	[0.00019]
Elderly Population (65+)	-0.00053	-0.00018	-0.00011	0.00000
	[0.00055]	[0.00065]	[0.00015]	[0.00023]
Total Population	0.00008	0.00010	0.00004	0.00003
	[0.00006]	[0.00009]	[0.00003]	[0.00004]
Fraction Female	6.4	6.5	-0.8	1.2
	[7.4]	[7.6]	[1.0]	[1.3]
Per Capita Income	0.0040	0.0041	-0.0023	-0.0029
	[0.0041]	[0.0055]	[0.0014]	[0.0018]
Hospital Insurance Market		-0.00002		-0.00000
		[0.00003]		[0.00001]
Surgical Insurance Market		0.00003		0.00001
		[0.00002]		[0.00001]
Constant	-2.3	-2. 4	1.37**	0.36
	[3.8]	[3.9]	[0.51]	[0.69]
Observations	564	423	564	423
R-squared	0.18	0.23	0.58	0.61

The table displays results from regressions as specified in equation (2). The dependent variable is the average state-level MLR among Blue Cross and Blue Shield insurers. Columns 1 and 2 are estimated by ordinary least squares with heteroskedasticity robust standard errors clustered at the state level. Columns 3 and 4 are estimated by robust regression. All models include state and year fixed effects. * indicates statistical significance at the 1% level, and * indicates significance at the 5% level.

Table 3.4 The effect of Michigan's minimum medical loss ratio rule on commercial medical loss ratios, triple-difference estimator

	(1)	(2)	(3)	(4)
Michigan	-0.007	-0.0354	-0.003	-0.032
	[0.031]	[0.044]	[0.025]	[0.045]
Commercial	-0.219**	-0.222**	-0.2102**	-0.2095**
	[0.011]	[0.012]	[0.0034]	[0.0039]
Post-1975	0.001	-0.0016	0.0098	0.0032
	[0.011]	[0.0077]	[0.0080]	[0.0075]
Michigan X Commercial	0.092**	0.082**	0.094**	0.079**
	[0.011]	[0.012]	[0.024]	[0.027]
Michigan X Post-1975	0.0113	0.002	0.023	0.004
	[0.018]	[0.014]	[0.026]	[0.033]
Post-1974 X Commercial	0.069**	0.077**	0.0630**	0.0663**
	[0.013]	[0.011]	[0.0053]	[0.0068]
Michigan X Post-1975 X Commercial	-0.079**	-0.082**	-0.084*	-0.080
	[0.013]	[0.011]	[0.037]	[0.046]
Working Age Population (20-64)	0.00002	-0.00002	-0.00007	-0.00014
	[0.00019]	[0.00032]	[0.00013]	[0.00021]
Elderly Population (65+)	-0.00027	-0.00012	0.00004	0.00020
	[0.00033]	[0.00042]	[0.00016]	[0.00025]
Total Population	0.00002	0.00004	0.00001	0.00002
	[0.00004]	[0.00006]	[0.00003]	[0.00005]
Fraction Female	4.2	5.3	0.1	1.4
	[4.2]	[4.7]	[1.0]	[1.5]
Per Capita Income	0.0015	0.0001	-0.0014	-0.0032
	[0.0022]	[0.0030]	[0.0014]	[0.0020]
Hospital Insurance Market		-0.00002		-0.00000
		[0.00001]		[0.00001]
Surgical Insurance Market		0.00002		0.00001
		[0.00001]		[0.00001]
Constant	-1.21	-1.8	0.88	0.20
	[2.17]	[2.4]	[0.54]	[0.76]
Observations	1,128	846	1,128	846
R-squared	0.64	0.65	0.85	0.84

The table displays results from regressions as specified in equation (3). The dependent variable is the average state-level commercial MLR. Columns 1 and 2 are estimated by ordinary least squares with heteroskedasticity robust standard errors clustered at the state level. Columns 3 and 4 are estimated by robust regression. All models include state and year fixed effects. * indicates statistical significance at the 1% level, and * indicates significance at the 5% level.

Table 3.5 The effect of Michigan's minimum medical loss ratio rule on premium revenue

	(1)	(2)	(3)	(4)
Michigan	49*	83*	71.9**	73.9**
	[18]	[31]	[2.4]	[3.9]
Post-1975	8.2	-2.9	4.2**	-1.48
	[6.3]	[2.8]	[1.3]	[0.82]
Michigan × 1975	13.8**	9.9**	16.6**	15.9**
	[1.8]	[2.0]	[2.1]	[2.1]
Working Age Population (20-64)	0.12	0.17	0.032*	0.023
	[0.19]	[0.20]	[0.015]	[0.019]
Elderly Population (65+)	0.09	0.08	0.052**	0.079**
	[0.17]	[0.17]	[0.018]	[0.022]
Total Population	-0.018	-0.022	-0.0016	0.0011
	[0.039]	[0.041]	[0.0034]	[0.0042]
Fraction Female	-574	-493	-197	-133
	[660]	[510]	[120]	[130]
Per-capita Income	-0.09	-0.25	-0.15	-0.19
	[0.20]	[0.35]	[0.16]	[0.18]
Hospital Insurance Market		-0.0156		-0.0055**
		[0.0093]		[0.0012]
Surgical Insurance Market		0.0076		0.0047**
		[0.0087]		[0.0010]
Constant	301	286	127*	102
	[330]	[260]	[60]	[68]
Observations	564	423	564	423
R-squared	0.99	0.99	0.99	0.99

The table displays results from regressions as specified in equation (2) and equation (3). The dependent variable in columns 1 and 2 is state-level premium revenue for commercial insurers. Columns 3 and 4 are state-level premium revenue for Blue Cross and Blue Shield insurers. Columns 1 and 3 are estimated by ordinary least squares with heteroskedasticity robust standard errors clustered at the state level. Columns 2 and 4 are estimated by robust regression. All models include state and year fixed effects. * indicates statistical significance at the 1% level, and * indicates significance at the 5% level.

Table 3.6 The effect of Michigan's minimum medical loss ratio rule on benefit payments

	(1)	(2)	(3)	(4)
Michigan	40*	58*	60.1**	60.1**
	[19]	[23]	[1.8]	[2.9]
Post-1975	8.5	3.2	5.00**	2.42**
	[6.3]	[2.2]	[0.93]	[0.61]
Michigan × 1975	8.3**	2.8	15.1**	1.4
	[2.0]	[1.7]	[1.5]	[1.6]
Working Age Population (20-64)	0.14	0.15	0.040**	0.028*
	[0.18]	[0.18]	[0.011]	[0.014]
Elderly Population (65+)	0.05	0.09	0.041**	0.063**
	[0.18]	[0.16]	[0.013]	[0.017]
Total Population	-0.026	-0.021	-0.0061*	-0.0027
	[0.038]	[0.036]	[0.0025]	[0.0031]
Fraction Female	-471	-429	-143	-146
	[630]	[440]	[87]	[99]
Per-capita Income	-0.00	-0.29	-0.18	-0.35**
	[0.22]	[0.31]	[0.12]	[0.13]
Hospital Insurance Market		-0.0176*		-0.0045**
		[0.0072]		[0.00087]
Surgical Insurance Market		0.0124		0.00422**
		[0.0067]		[0.00078]
Constant	243	233	92*	97
	[320]	[220]	[45]	[51]
Observations	564	423	564	423
R-squared	0.98	0.99	0.99	0.99

The table displays results from regressions as specified in equation (2) and equation (3). The dependent variable in columns 1 and 2 is state-level aggregate benefit payments for commercial insurers. Columns 3 and 4 are state-level aggregate benefit payments for Blue Cross and Blue Shield insurers. Columns 1 and 3 are estimated by ordinary least squares with heteroskedasticity robust standard errors clustered at the state level. Columns 2 and 4 are estimated by robust regression. All models include state and year fixed effects. * indicates statistical significance at the 1% level, and * indicates significance at the 5% level.

Appendix
Table 3.7 Estimation sample descriptive statistics

	Mean	SD	N
MLR	0.84	0.13	1128
Premiums	55	72	1128
Benefits	46	62	1128
Working Age Population (20-64) (1,000's)	536	584	1128
Elderly Population (65+) (1,000's)	250	248	1128
Total Population (1,000's)	1503	1552	1128
Fraction Female	0.51	0.0064	1128
Per capita Income ($1,000's)	4.4	1.9	1128
Hospital Insurance Market	3235	3605	846
Surgical Insurance Market (Covered Lives)	3054	3465	846

The table displays the means, standard deviations and observations for MLRs, premium revenue ($1,000), aggregate benefit payments ($1,000) from Health Insurance Association of America member surveys. It also displays counts of working age, elderly and total population in 1,000's as well as the fraction of the state population that is female from the Surveillance, Epidemiology and End Results program, and state –per capita income in $1,000 from the Bureau of Economic Analysis. Hospital and surgical insurance market size, in terms of 1,000's of covered lives, come from Health Insurance Association of America member surveys, but are not available in all years.

Chapter 4 Do Pap smears reduce cervical cancer mortality? Evidence from federal family planning funding

No form of clinical cancer better documents the remarkable effects of early diagnosis and curative therapy on the mortality rate than cancer of the cervix…the lowered mortality rate reflects the earlier discovery of curable lesions…Thanks for these impressive improvements are largely owed to the Papanicolaou cytologic test for the detection of cancer

-- Robbin, Cotran, and Kumar (1974)

The most widely cited evidence of the contribution of cytologic screening to the reduction in cervical cancer mortality is the long-term decline in deaths from cervical cancer in the United States coincident with the introduction of the Pap smear. In actuality, death rates had begun to decline prior to widespread use of Pap smears, perhaps due to […] other factors.

--Shingleton et al. (1995)

Introduction

Hailed as a cancer screening success story, the Papanicolaou ("Pap") smear is widely credited for dramatic reductions in cervical cancer over the 20th century (Casper and Clarke 1998; DeMay 2000; National Institutes of Health Consensus Development Panel 1996; Waxman 2005). Shortly after Dr. Papanicolaou presented his work on the cervical cancer screening test in 1928 (Papanicolaou 1928), deaths from cancers of the female reproductive organs began to precipitously fall. Cancers of the uterus, including both the uterine body, and the cervix, declined over 80 percent from 36 deaths per 100,000 women in 1930—the first year it was measured—to six deaths per 100,000 women in 2009 (Siegel et al. 2011). Since the 1950s, the

American Cancer Society has heavily promoted the Pap smear, and guidelines have recommend regular Pap smears for women of reproductive age since 2001 (Waxman 2005).

Although many studies (National Cancer Institute 2014) show that screening is correlated with reductions in cervical cancer mortality, whether the Pap smear has a causal effect on cervical cancer mortality remains an open question. In some countries, reductions in cervical cancer mortality actually preceded the introduction of mass screening campaigns (Raffle 1997; Gardner and Lyon 1977). In the United States, for example, uterine cancer mortality had already declined for nearly three decades before the first mass screening campaign began in 1957. This included over a decade of decline before the Pap smear became an accepted medical practice in the mid-1940s (Gardner and Lyon 1977; Löwy 2011). Furthermore, the test has never been evaluated in a randomized controlled trial because of the ethical challenges of withholding a cancer screening with presumed benefits. Experimental evidence is particularly important because the endogeneity of screening and cervical cancer mortality risk may confound estimates of the effect of Pap smears on cervical cancer mortality in observational studies.

This paper takes a new approach to estimating the causal effect of the Pap smear on cervical cancer mortality in the United States. I exploit a natural experiment in the availability of subsidized Pap smears provided through federal grants for family planning. Starting in 1965, these grants provided funding to family planning clinics to provide subsidized reproductive care, including Pap smears, to low-income women. As documented by Bailey (2012), the chaotic nature of the grant-making process for the earliest grants between 1965 and 1973 offers a natural experiment in the availability of subsidized reproductive health care. I find that women who lived in communities that received grant funding were 8-13 percent more likely to have had a Pap smear in the last year. I also find that increased screening did not result in statistically

significant changes in cervical cancer mortality up to 15 years later. However, due to imprecision of the estimates, I cannot rule out existing estimates of the Pap smear's effectiveness.

Although the results of this paper are inconclusive, the Pap smear's role in the prevention of cervical cancer mortality should be re-evaluated. First, public resources are frequently directed toward increasing Pap smear utilization among women who receive screenings less frequently than guidelines recommend. Public efforts to increase Pap smear screenings include national screening campaigns (Henson, Wyatt, and Lee 1996; Gardner 2006), mandates that require insurers to cover Pap smears in 20 states (Bitler and Carpenter 2012), Medicaid family planning program expansions (Wherry 2013), and the recent Affordable Care Act requirement that nearly all insurers not only cover, but also waive cost sharing for Pap smears (Kaiser Family Foundation 2013b). This most recent effort is expected to extend subsidized screening, including Pap smears, to an estimated 20 million women (Sommers and Wilson 2012).

Second, alternative screening methods have been introduced in recent years. The first genetic test for the virus that causes nearly all cases of cervical cancer, Human Papilloma Virus (HPV), was developed in 2000 (National Cancer Institute 2014). Genetic HPV tests are nearly twice as likely as Pap smears to detect pre-cancerous cells among women who are actually at-risk for cervical cancer (95% vs. 55%), but are only slightly more likely to produce a false-positive result (6% vs. 3%) (National Cancer Institute 2014). While HPV tests have been used alongside Pap smears over the last decade, the FDA approved another version this April, which may be used as an alternative to the Pap smear (Food and Drug Administration 2014). Still, despite evidence that it may be superior, the approval of this new test has been controversial due to the perceived superiority of the Pap smear (Coaltion Letter 2014).

Cervical cancer etiology, screening, and treatment

Cancer of the uterus has been one of the most recognizable diseases affecting women throughout history (Löwy 2011). In the beginning of the 20th century, it was the leading cause of cancer death among women of reproductive age (15-44), and the eighth leading cause of death among women of all ages (United States Census Bureau 1910). It is generally accepted that cervical cancer is caused almost exclusively by HPV, which can be transmitted through sexual activity (National Cancer Institute 2014). HPV infections may take 10-20 years to turn into cervical cancer, but only a fraction of the many women who are infected will develop invasive cervical cancer (National Cancer Institute 2014; Saslow et al. 2002).

Invasive cervical cancer is preceded by pre-cancerous lesions on the cervix. Depending on the severity, most pre-cancerous lesions that will progress to cervical cancer do so within 2-5 years (Holowaty et al. 1999). As a result of its long natural history, the risk of cervical cancer mortality increases with age, with the highest risk of mortality between ages 45 and 70 (Saslow et al. 2002).

The idea behind the Pap smear is that cervical cancer can be anticipated by routinely checking the cervix for pre-cancerous lesions. During a Pap smear, the doctor uses a device called a speculum to visualize the cervix and exfoliates cervical cells with a small brush. The cells are then examined under a microscope for the presence of pre-cancerous lesions. Depending on their size and shape, these lesions can be mild, moderate, or severe, and may include cancerous cells that have not yet spread to surrounding tissue. Mild lesions often disappear over time—70 percent disappear within six years, and only six percent become severe. It is estimated that between 10 and 20 percent of severe lesions turn into invasive cervical cancer

(National Cancer Institute 2014). Of the estimated 50 million Pap smears performed each year, 2-3 million detect mild lesions and about 600,000 detect moderate or severe lesions (Jones 1995).

Current guidelines by the American College of Obstetricians and Gynecologists (2008) recommend that women with mild lesions undergo further testing such as an HPV DNA test or a repeat Pap smear within the year. In some cases, the presence of mild lesions is followed by a cervical tissue biopsy, called a colposcopy. For most women with moderate or severe lesions, guidelines recommend colposcopy or excision of the abnormal tissue. Depending on the severity of the lesions, the method for tissue removal ranges from a minimally invasive procedure that can be done with local anesthetic in a doctor's office, to hysterectomy, a serious operation that requires general anesthesia (Löwy 2011).

Although the Pap smear has changed very little since its introduction, both the classification of lesions and guidelines for follow-up have evolved over the last century. In the past, confusion over classification sometimes led to ambiguous test results and a propensity to treat even mild lesions (Löwy 2011). Through the 1950s, women with cervical lesions—and sometimes even very minor abnormalities—were treated with radiation therapy, hysterectomy, or both. Both treatments could result in sterility (Löwy 2011). After it was discovered in the 1960s that many mild lesions spontaneously regress after biopsy, immediate hysterectomy and radiation therapy were gradually replaced with more conservative measures that removed only the minimum necessary amount of suspect tissue. These measures include burning or freezing lesions, zapping lesions with lasers, or using specialized equipment to cut out abnormal cells (Löwy 2011).

What do we know about the effect of pap smears on cervical cancer mortality?

Although it has been extensively studied—a MEDLINE search of the word "Pap smear" yields over 3,000 publications—and its effectiveness is taken as given (National Cancer Institute 2014; National Institutes of Health Consensus Development Panel 1996), there are several reasons to re-evaluate the causal role of the Pap smear in preventing cervical cancer. First, the time-series evidence from the United States and several other countries suggests that cervical cancer mortality was declining decades before the Pap smear was widely used. Second, the observational study designs to which researchers are limited often cannot rule out alternative hypotheses.

The observational evidence in support of the Pap smear's role in decreasing cervical cancer mortality can be categorized into several groups. The first group consists of cohort studies. This type of evidence compares cohorts of women who were screened to those who were not (e.g. Andrae et al. (2012)). But because women who seek screening are also likely to make better health investments than those who do not, advantageous selection into screening is likely. Assuming advantageous selection, comparing the mortality of women who are diagnosed with cervical cancer as a result of screening to those who are diagnosed subsequent to the emergence of symptoms—even controlling for the stage of the disease—overestimates the benefits of the Pap smear on mortality.

The second group consists of cross-country comparisons. These studies compare screening and mortality rates across countries and find a negative association between screening rates and cervical cancer mortality (e.g. Arbyn et al. (2009) or Lăără, Day, and Hakama (1987)). The chief problem with this approach is that the screening rates could be endogenous: countries

with higher screening rates have better medical resources, which could also directly affect cervical cancer mortality. In this example, both the screening rates and mortality are artifacts of a third, confounding factor.

The third group is within-country comparisons over time, which compare changes in screening to changes in cervical cancer mortality within a country (e.g.,Quinn et al. (1999); Mählck, Jonsson, and Lenner (1994); Miller, Lindsay, and Hill (1976); or Pettersson, Bjorkholm, and Naslund (1985)). One problem with studies of this type is that they often do not have control groups because screening is conducted as part of a national campaign in which all women are targets. Without a control, group, a reduction in cervical cancer mortality due to cervical cancer screening cannot be separated from cervical cancer mortality reductions resulting from other factors. For example, changes in treatment options for cervical cancer that occur at the same time as a screening campaign may also reduce cervical cancer mortality.

Furthermore, two features of the time series literature actually cast doubt on the effectiveness of the Pap smear: most plots of age-adjusted cervical cancer mortality rates show no trend break in mortality after the establishment of cervical cancer screening programs (Arbyn et al. 2009), and in some countries, the decline in cervical cancer mortality pre-dates the introduction of the screening program altogether (Raffle 1997). A notable exception is a study of screening and mortality in Iceland by Johannesson, Geirsson, and Day (1978). They show that the cervical cancer mortality rate was increasing before Iceland's national campaign and then fell after several years of screening.

Figure 4.1 presents time series evidence from the United States. Cancers of the female genitalia, including the uterine body and uterine cervix, rise beginning in 1910, the first year they

are measured, and peak in 1935 at 42 deaths per 100,000 women, falling thereafter. The earliest recorded uterine (1930), and cervical (1946) cancers follow a similar downward trend. Although the reduction in age-adjusted gynecological cancers appears to be precipitated by the introduction of the Pap smear in the late 1920s, the test would not become accepted medical practice for at least a decade. The original presentation of Dr. Papanicolaou's work (line A) was at a little-attended eugenics conference in Battle Creek, Michigan (Papanicolaou 1928). The work was met with skepticism because it was based on only a few cases, including Papanicolaou's wife (Löwy 2011). It was not until 1941 (line B), that the first-peer reviewed paper on the subject was published (Papanicolaou and Traunt 1941). By this point, uterine cancer mortality had already declined by 11 percent.

By the mid-1940s, Americans had begun to hear about the test through the popular press, and demand began to grow despite severe supply constraints. At that time, there were few pathologists with the training necessary to read the tests (Gardner 2006). These constraints led the American Cancer Society to convene the first Cytological Diagnostic Conference in 1948 (line C). At the conference, it was decided that neither physicians nor cancer control advocates would publicize the test until supply constraints had been alleviated (Gardner 2006). As a result of these constraints, relatively few Pap smears were administered in the 1950s; national surveys of cytologic facilities estimated that only one million tests were read in 1955 (Horn and Siegel 1961). Once capacity to read Pap smears caught up, the American Cancer Society launched the first nationwide Pap smear campaign in 1957 (line D). The number of Pap smears performed climbed to about 15 million in 1968 (Manos and Robins 1972)—still only a fifth of the 70 million yearly Pap smears that are performed today (Sirovich and Welch 2004).

Uncertainty over inconsistent time series evidence and the use of observational studies to estimate causal effects have previously incited debates over the Pap smear's role in cervical cancer mortality declines (Raffle 1997; Gardner and Lyon 1977). Yet these concerns remain unaddressed, in part because of the inability to conduct a randomized controlled trial. In the absence of true experimental evidence, a natural experiment—where Pap smear screening occurs in an essentially random way—may help to add evidence to this debate.

Federal family planning grant-making as a natural experiment

In the absence of a randomized controlled trial, generating causal evidence on the effect of Pap smears on cervical cancer mortality requires some type of exogenous variation in the treatment, such as an instrument. I use a natural experiment in Pap smear access provided by initial federal grants for family planning. Starting in 1965, the federal Office of Economic Opportunity government began awarding grants to community organizations to fund family planning clinics (for a detailed description of the program and funding scheme, see Bailey (2012)). The grants established clinics with the primary purpose of dispensing free or low-cost contraceptives to low-income women, but they were also encouraged to provide subsidized preventive health services (Gold 2001; Dryfoos 1976), such as Pap smears. The chaotic grant-making process between 1965 and 1973 resulted in nearly random variation in access to subsidized reproductive care (Bailey 2012).

A valid instrument for cervical cancer screening must be both relevant (it predicts Pap smear use) and excludable (it has no effect on cervical cancer mortality except through increased use of Pap smears). Free or heavily subsidized screenings through funded clinics should have increased the fraction of women who were screened because lowering the cost of the test

increases its net expected benefit (Picone, Sloan, and Taylor 2004). I can use microdata on Pap smear utilization to test whether this is true.

The second criterion, excludability, is more difficult to demonstrate because it cannot be tested directly. The family planning clinics affected more than just screening. In fact, the chief goal of the federal family planning grants was to fund clinics that would assist women in achieving their desired number of children by providing free or low-cost birth control (Bailey 2012). Evaluations of the program using expansions in the 1990s found that family planning clinics increased respondents' likelihood of having used birth control in the past month (Kearney and Levine 2009). The concern for excludability is that greater availability of some kinds of contraception could have an independent effect on cervical cancer incidence and mortality through HPV transmission rates. HPV is spread through sexual contact and can be prevented with barrier contraceptive methods such as condoms, but is not affected by non-barrier methods such as birth control pills or the intra-uterine device. If the introduction of funded family planning clinics resulted in increased use of condoms, transmission rates may have fallen, resulting in fewer cases of cervical cancer. Alternatively, if condom use decreased because women used other forms of birth control instead, then transmission rates may have risen. Increased use of contraceptives may have also had an effect on the frequency of sexual intercourse. As the price of pregnancy prevention falls, couples can have more sexual intercourse and still achieve desired fertility. All things being equal, more unprotected sexual activity should increase HPV transmission. I can use microdata on condom use and frequency of sexual intercourse to test whether the instrument is valid.

Estimation strategy and data

To estimate the effect of the Pap smear on cervical cancer mortality, I use variation in access to free or low-cost Pap smears through the effectively random grant-making process for family planning clinics between 1965 and 1973. I link data on the timing of Office of Economic Opportunity grants compiled by (Bailey 2012) with microdata on Pap smear use and county-level mortality statistics. The first goal of my analysis is to test whether federal family planning funding increased cervical cancer screening in the community in which the clinic was funded. The second goal is to test whether family planning funding reduced deaths from cervical cancer among women in the community in which the clinic was funded.

To test whether introduction of a family planning clinic increases the likelihood of Pap smear utilization, I use the only source of microdata on Pap smear use during this time period: the 1970 National Fertility Survey (Westoff and Ryder 1977). The National Fertility Survey is a nationally representative survey of ever-married women aged 14-45, designed to provide information on marital fertility and family planning. This sample is likely to be more representative of older than younger women. According the 1970 census, only 33 percent of women under age 25 were ever-married, but this fraction is over 90 percent for women aged 25 or older (Ruggles et al. 2010). The survey contains information on the primary sampling unit (PSU) in which each woman lived, which is about the size of a county. This information allows me to observe whether, in 1970, the woman lived in a PSU with a federally funded family planning clinic and the date the clinic was initially funded. I estimate the effect of receiving a grant on Pap smear utilization using equation (1):

(1) $Pr(PAP)_{ip} = \alpha_0 + \alpha_1 1(BEFORE70)_{ip} + \gamma X_{ip} + f_s + e_{ips}$

The variable PAP_{ip} indicates having received a Pap smear in the 12 months prior to the survey, and $BEFORE70_{ip}$ indicates that woman i lived in a population sampling unit, p, that received family planning center funding before 1970. X_{ip} is a vector of individual controls including 10-year age categories, a dummy for whether the population sampling unit is urban, as well as dummies for white race and Catholic religion, and dummies for completed years of education. I also include state fixed-effects, f_s to remove time-invariant state characteristics. This regression is run on a sample of 6,676 women from both ever- and never-funded counties and the standard errors are both robust to heteroskedasticity, and clustered at the PSU level. I also use a specification identical to equation (1) to test the effect of family planning funding on the channels of HPV transmission: frequency of sexual intercourse and use of condoms.

To estimate the effect of Pap smears on cervical cancer mortality, I use linked data on grant timing and county-level, age-adjusted mortality statistics from 1959 through 1988 constructed from Vital Statistics Multiple Cause of Death records (National Center for Health 2008). Following Bailey's (2012) study of the effect of family planning funding on birth rates, I use a flexible difference-in-differences approach, called an event study. This specification compares mortality in each period in event time to the year before the clinic was funded, and to average mortality across unfunded counties. Equation (2) illustrates the approach:

$$(2) Y_{ct} = \beta_0 + \sum_{k=-6}^{-2} \beta_k D_c 1(t - F_c = k) + \sum_{j=0}^{15} \beta_j D_c 1(t - F_c = j) + \delta X_{ct} + f_c + g_t + f_s * g_t$$

The dependent variable is Y_{ct}, the age-adjusted, county-level cervical cancer mortality rate per 100,000 women. Age adjustment re-weights the cancer statistics to control for differences in the age distribution across counties. I age-adjust using five-year age categories from the 1960 census. D_{cj} is a dummy variable for each treated county *c,* interacted with

indicators for each of the k periods before, and j periods after the community organization in the county received funding (F_c). The set of coefficients β_k gives the difference in the outcome between k periods before the clinic was funded, and the year before the clinic received the grant, F_c (the omitted category), and the average outcome in unfunded counties over the sample period. For example, if Y_{ct} represents cervical cancer mortality, β_{-6} gives the difference in age-adjusted cervical cancer mortality in the county between six periods before the clinic was funded, and the year before it was actually funded, compared to average cervical cancer mortality in the rest of the unfunded counties. The set of coefficients β_j are conceptually similar; they give the difference in age-adjusted cervical cancer mortality between periods, after the clinic was funded and the year before it received the grant, relative to unfunded counties. If family planning funding increases the use of Pap smears, and Pap smears reduce cervical cancer mortality, I expect the estimated β_j coefficients to be negative.

X_{ct} is a vector of county-level controls following (Bailey 2012). These include interactions of characteristics measured in the 1960 census with a linear time trend, such as the fraction of the county that is urban, the fraction that is non-white, and measures of age and income distributions in each county. I also include county-level fixed-effects to control for differences between counties, f_c, year fixed-effects to remove national trends in cervical cancer mortality, g_t, and state-by-year fixed-effects to control for non-linear time trends across states, $f_s \times g_t$. All regression are weighted by county-level population in the 1960 census.

Because changes, or a lack of changes, in mortality could reflect either the effectiveness of the Pap smear, or the effectiveness of treatments for invasive cervical cancer, I also investigate the effect of family planning clinics on the incidence of invasive cervical cancer. I

use historical data from the Connecticut Tumor Registry, the oldest statewide registry in the U.S. and the only source of such information before 1973 (Haenszel and Curnen 1986). I estimate the following event study:

$$(3)\ Y_{ct} = \theta_0 + \sum_{k=-6}^{-2} \theta_k D_c 1(t - F_c = k) + \sum_{j=0}^{15} \theta_j D_c 1(t - F_c = j) + \delta X_{ct} + f_c + g_t$$

Now Y_{ct} represents the age-adjusted cervical cancer incidence rate, equal to the 1960 census-weighted rate of invasive cervical cancer per 100,000 women. The set of coefficients θ_k gives the effect of federal family planning funding on cervical cancer incidence k years before the clinic was funded, and θ_j gives the effect j years after funding.

Results

Descriptive trends

Table 4.1 presents descriptive statistics from the 1970 National Fertility Survey. The fraction of women in unfunded counties who had received a pap smear within the previous year is similar to that of women who lived in counties that would eventually be funded but were not at the time of the survey (1970). Screening in the last year was six percentage points higher among women who lived in population sampling units that had already received funding by 1970. In terms of modes of HPV transmission, the three types of women in the sample do not differ substantially. Frequency of sexual intercourse was approximately eight times per week for all women. Condom use in never-funded PSUs was also similar to that in PSUs funded before 1970. Interestingly, condom use was three to five percentage points lower among women in eventually-funded PSUs. Age and educational distributions were similar across women in never-

funded, eventually-funded, and funded PSUs. The only important difference is that eventually-funded PSUs were about 10 percentage points more likely to be urban than funded or never-funded PSUs.

Figure 4.2 gives trends in age-adjusted cervical cancer incidence and mortality over the sample period. Between 1959 and 1988, the cervical cancer mortality rate declines smoothly from about nine deaths per 100,000 women, to three – a 66 percent reduction. The average age-adjusted cervical cancer mortality rate among unfunded counties is six. Age-adjusted cervical cancer incidence also falls over the period, although the raw data are more variable, most likely because they come from only one state (Connecticut). Incidence falls from 18 cases per 100,000 to eight per 100,000—a 55 percent reduction. The figure also gives the cumulative number of grants awarded over the sample period. Grants are awarded yearly beginning in 1965, rising to 654 by 1973. Three of Connecticut's eight counties were awarded grants over this period.

Family planning and use of the Pap smear

Table 4.2 presents results from a test of whether family planning clinics increased use of Pap smears in the year prior to the survey. These regressions are specified according to equation (1). The result from column one indicates that women who lived in a community with a family planning clinic were approximately five percentage points more likely to have received a Pap smear in the previous 12 months. This result is both statistically and clinically significant. In terms of the average level of screening, this effect represents an eight percent increase. Adding state fixed-effects to control for level differences in screening across states that may be correlated with whether or not the PSU had a clinic, the effect increases in magnitude. Women

who lived in a PSU that had a funded family planning clinic by 1970 were eight percentage points or 13 percent more likely to have received a recent Pap smear.

Cervical cancer risk increases with age (Saslow et al. 2002), and false positives are very common among women under 20 (National Cancer Institute 2014). If increases in screening were concentrated among the youngest women, then it may be difficult to detect a change in mortality because deaths to younger women are a relatively rare event. However, if screening changes were driven by older women, then small changes in mortality will be easier to detect. Table 4.3 presents the results interacted with 10-year age categories. Among women aged 20-29 (the omitted group), living in a community with a funded family planning clinic increased the likelihood of having received a Pap smear in the previous year by almost six percentage points, or 8.5 percent in a model without state fixed-effects. While the effect among women aged 30-39 and 40-49 differed by only half a percentage point from the effect for women in their twenties, the youngest women (aged 14-19) were two percentage points less likely to have received a recent Pap smear than were women aged 20-29. Although the interaction is not statistically significant, a Wald test of the joint significance of the main effect and the interaction cannot reject that the effect is different from zero ($p=0.55$). The addition of state fixed-effects in column two increases the magnitude of the effect for women aged 20-29 to nine percentage points, or nearly 14 percent. However, the effect among the youngest women is four percentage points smaller, and remains statistically insignificant. The results suggest that the 8-13 percent screening increase was driven by women aged 20 and older.

Table 4.4 provides evidence that funded family planning clinics had little effect on two behaviors that likely affect the transmission of HPV: sexual intercourse and condom use. Columns one and three present results from a regression specified as in equation (1), with

frequency of sexual intercourse in the previous month, and recent condom use as dependent variables. Neither channel of HPV transmission is significantly affected. Adding state fixed-effects in columns two and four, women who had access to a funded family planning clinic increased sexual intercourse by less than one time per month, and the effect is not statistically different from zero. In terms of condom use, women with access to a funded family planning clinic reduced condom use by a statistically insignificant 1.4 percentage points. Although they are not statistically significant, the point estimates are both imprecise. As a result, I cannot rule out a change of slightly less than one additional sexual encounter per month or a 20 percent change in condom use (0.03/0.16). Therefore I cannot conclude that my instrument does not also affect HPV transmission rates. If transmission rates fell, then the risk of cervical cancer should also fall, biasing my estimates in the direction of finding no effect.

Family planning funding resulted in no change in age-adjusted cervical cancer mortality

Figure 4.3 presents results from the mortality event study specified in equation (2). Each point on a black line represents the coefficient on an event-time dummy. These coefficients give the difference in the effect of funding in each period relative to the year before funding, and among unfunded counties, on age-adjusted cervical cancer deaths per 100,000 women. The grey lines give the 95 percent confidence interval for each point estimate. The figure shows that there are no pre-treatment effects, even controlling for state specific, non-linear time trends and linear trends in the 1960 county-level census characteristics.

In the first six periods after a grant is received, there appears to be no effect across all specifications. Although individual point estimates at periods one and four are almost statistically different than zero in some models, the variability of the effects over the six periods

suggests that these point estimates would not be different than zero if averaged together. The magnitude of the standard errors suggests that the point estimates are imprecisely estimated. The lower 95 percent confidence interval suggests that I cannot reject that a nearly 0.7 deaths per 100,000 women decrease in cervical cancer is different from zero. Relative to the average age-adjusted cervical cancer mortality in ever-funded counties in the year before they were funded (6.3 deaths per 100,000 women), this is about a 10 percent decrease in cervical cancer mortality. The longer-term effects of family planning funding are less variable over time. The point estimates are both constant and statistically insignificant after six periods in a regression with only county and year fixed-effects. Adding state-specific non-linear time trends and controls does not change the results. In order to increase the precision of the estimates, I pool the data into five-year periods. The results of this exercise are displayed in Table 4.2, and the standard errors are similar: I cannot rule out an approximate 10 percent decrease in cervical cancer mortality.

(Bailey 2012) shows that counties that received funding were more likely to be urban. I include interactions of a linear time trend with the fraction of a county that is urban in my main results. As an additional robustness check, I also re-estimate equation (2) using only funded counties. Figure 4.4 displays the results, which are similar. There appears to be no pre-trend. With county-level and year fixed-effects, cervical cancer mortality appears to fall after the introduction of a family planning clinic, although the reduction is not statistically significant. Adding controls for non-linear state-level trends eliminates the downward cervical cancer mortality trend. The standard errors are large. Although the pattern is similar to that in Figure 4.3, I cannot reject that an approximately 20 percent reduction in cervical cancer mortality is not different than zero.

Family planning funding did not change age-adjusted cervical cancer incidence

Figure 4.5 illustrates the effect of federal family planning grants on age-adjusted cervical cancer incidence, as in equation (3). The figure plots the coefficients on event study dummies and their 95 percent confidence intervals, for regressions with county and year fixed-effects, and also county-by-year fixed-effects. Not only is there no pre-treatment trend in invasive cervical cancer mortality, there is no change in the diagnosis of invasive cervical cancer after a family planning clinic is established in the county. Because the sample includes only Connecticut, the standard errors are large. As a share of average cervical cancer incidence over the year before funded counties received funding, the lower confidence interval implies that I cannot distinguish between zero and a forty percent reduction in cervical cancer incidence over the study period (mean age-adjusted cervical cancer incidence is 12 cases per 100,000 women). In order to increase the precision of the estimates, in Table 4.6 Pooled estimates of the effect of family planning center funding on age-adjusted cervical cancer incidence, I pool the data into five-year periods, similar to in Table 4.5 Pooled estimates of the effect of family planning funding on age-adjusted cervical cancer mortality. The standard errors are similar: column one of Table 4.6 Pooled estimates of the effect of family planning center funding on age-adjusted cervical cancer incidence suggests that I cannot rule out an approximately 40 percent decrease in cervical cancer mortality.

Discussion

This paper generates inconclusive, quasi-experimental evidence on the effectiveness of the Pap smear. Using exogenous variation in the timing and receipt of federal grants for family planning, I find that clinic funding led to at least an eight percent increase in Pap smear screenings in the 12 months prior to the survey, driven by older women. This increase in

screenings did not result in statistically significant reductions in cervical cancer mortality up to 15 years later, but the estimates are imprecise; I cannot rule out a 10 percent increase or decrease in cervical cancer mortality. I find similar results for cervical cancer incidence – there is no statistically significant change, but the standard errors are too imprecise to rule out even very large effects.

To put my estimates into perspective, I calculate a treatment-on-the-treated estimate using the lower 95 percent confidence interval of my pooled mortality estimates as a maximum bound for the change in cervical cancer mortality implied by my estimates. I then compare it to a similarly calculated estimate using results from a frequently cited paper on the relationship between a national cervical cancer screening in Iceland and cervical cancer mortality (Johannesson, Geirsson, and Day 1978; National Cancer Institute 2014). The method used to calculate these estimates is described in more detail in the appendix. The maximum treatment-on-the-treated effect implied by my estimates is a 72 percent reduction in cervical cancer mortality after about 10 years. The treatment-on-the-treated estimate I calculate for Johannesson, Geirsson, and Day (1978) is also 72 percent. This suggests that, although my estimates are imprecisely estimated, the maximum bound on the mortality effect is not implausible.

Even with better precision of the estimates, there are several reasons to interpret the results with caution. First, cervical cancer mortality depends not only on early detection, but also on follow-up and treatment choices, conditional on receiving an abnormal Pap smear result. The treatment of pre-cancerous lesions became more conservative in the 1960s (Löwy 2011), but access to newer, less invasive options may have been limited for the low-income population that used funded family planning clinics. Although many low-income women had access to

Medicaid (Goodman-Bacon 2013), these treatments were not necessarily covered. Even as late as 2009, only seven states' Medicaid programs included colposcopy as a covered family planning benefit (Ranji 2009). If women were not aware these newer treatments were available or were unable to pay for them, they may have been reluctant to act upon positive Pap smear results because they did not want to undergo hysterectomy. The non-pecuniary costs of hysterectomy were substantial; not only did women face non-trivial mortality risk during this major surgery, they also lost the ability to have children (Löwy 2011). Second, my first-stage sample is skewed towards older women, who may have also been more likely to seek Pap smears. If this is true, my first-stage estimates of family planning funding effects on Pap smears may be biased, overstating the effect of funding on screening. Third, my event study estimates describe changes in county-level cervical cancer mortality up to 15 years after grant funding is received, but migration across counties introduces measurement error, which attenuates the estimated coefficients. This would bias my estimates toward finding no effect.

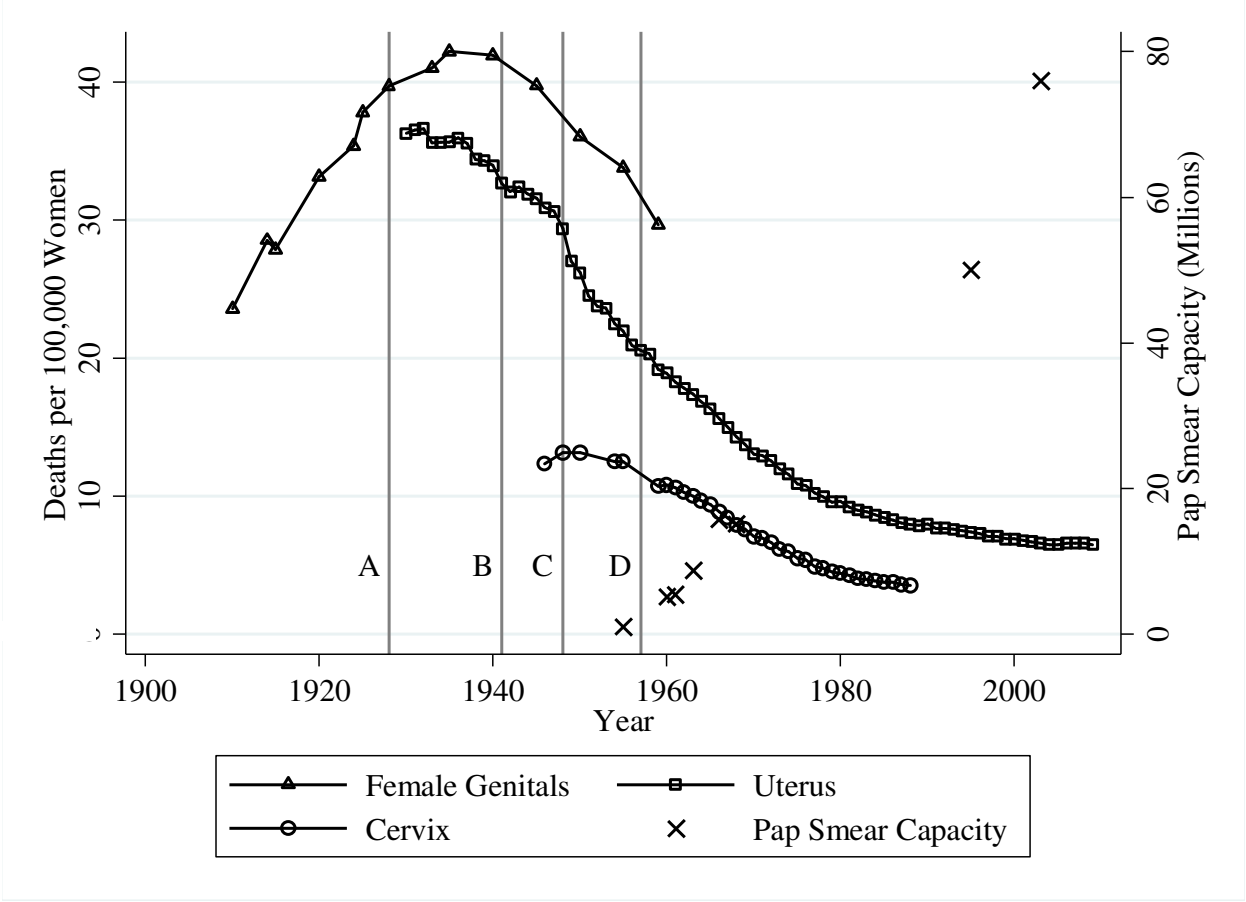
Conclusion

This paper uses a natural experiment to investigate the effect of Pap smears on cervical cancer mortality. I find that federal family planning grants increased screening, but had no clear effect on cervical cancer mortality. Although the point estimates suggest little relationship between cervical cancer mortality and Pap smear screening, the standard errors are large. I cannot distinguish my estimated zero from similar estimates in the literature. Despite inconclusive results using a natural experiment, the time-series evidence provided here and elsewhere (Raffle 1997; Gardner and Lyon 1977) suggests that the effect of Pap smears on cervical cancer mortality remains an open question. Given public policy efforts to increase

screening, as well as recent controversy over the approval of an alternative—and possibly superior—test, researchers should continue to investigate this issue.

Figures and tables

Figure 4.1 Age-adjusted gynecological cancer mortality, Pap smear capacity and adoption of the Pap smear



The figure plots age-adjusted gynecological cancers and Pap smears performed between 1910 and 2009. All mortality series are adjusted using the 1960 population distribution from the 1960 census. Female genital cancers come from 1910-1956 annual mortality statistics reports. Uterine cancers from the American Cancer Society, and cervical cancers come from 1945-1968 annual vital statistics reports and 1959-1988 multiple cause mortality files. The number of Pap smears comes from results of national surveys of cytologic facilities reported in (Horn and Siegel 1961; Horn and Watanabe 1965; Manos and Robins 1972), and estimates based on nationally representative population surveys (Jones 1995; Sirovich and Welch 2004). Line A indicates the first presentation of work on the Pap smear, line B indicates the first peer-reviewed publication on the Pap smear, line C indicates the first conference of exfolliative cytology, and line D indicates the start of the first national screening campaign.

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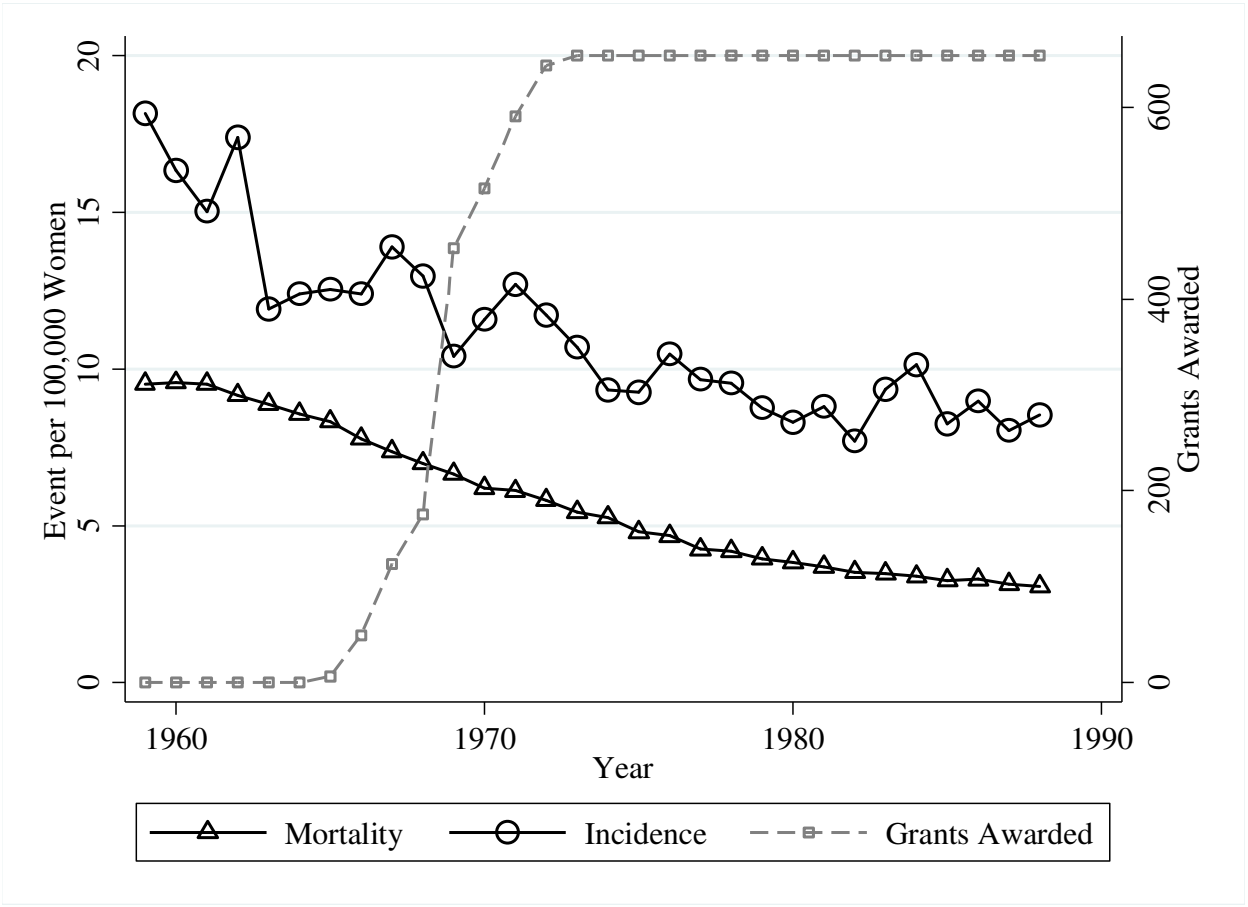
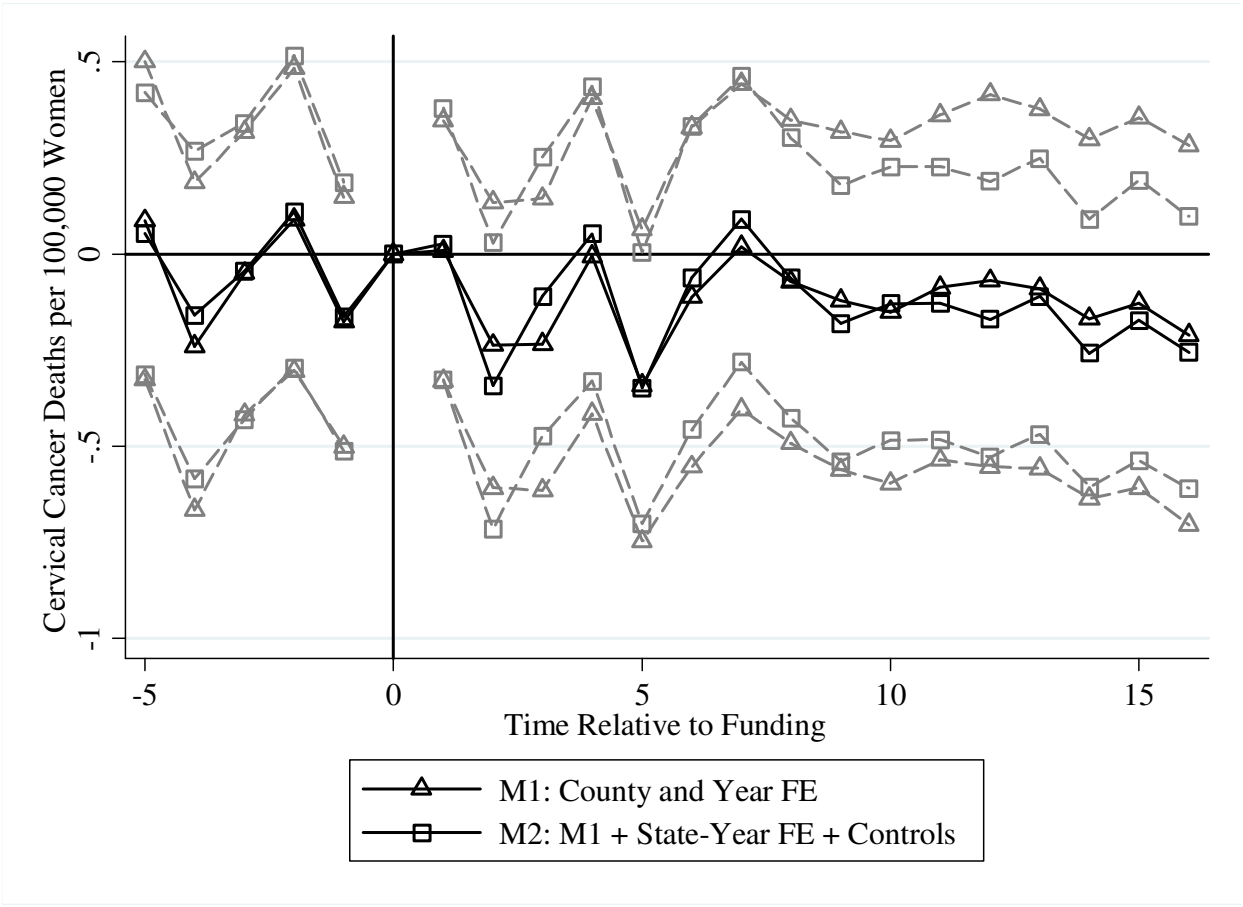
Figure 4.2 Age-adjusted cervical cancer mortality and incidence, and federal family planning grants



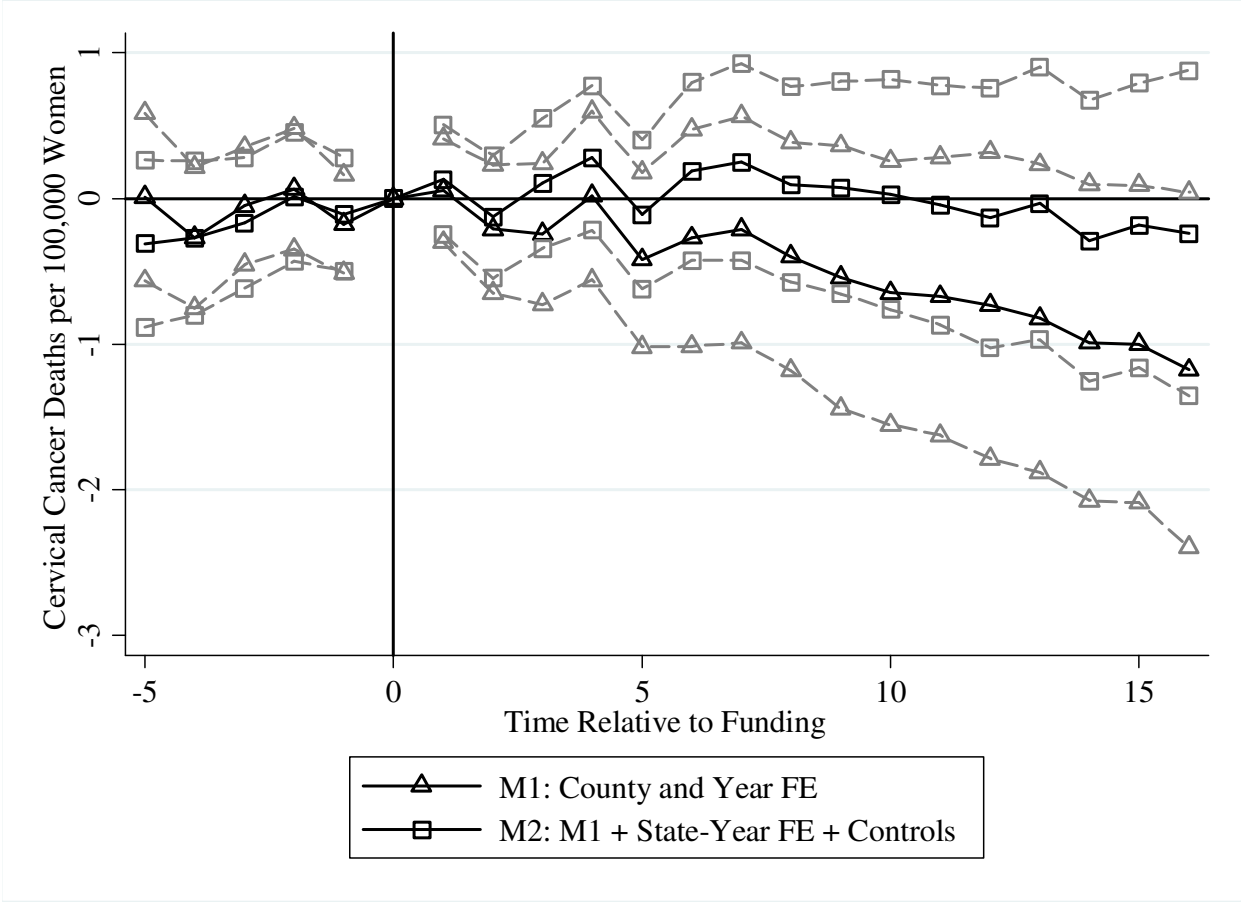
The figure plots age-adjusted cervical cancer mortality and incidence, as well as the cumulative total of federal grants for family planning. Age-adjusted cervical cancer mortality rates come from the 1959-1988 multiple cause mortality files and are adjusted to the 1960 age distribution using the 1960 census. Age-adjusted cervical cancer incidence comes from the historical Connecticut Tumor Registry and Connecticut tumor registry data from the Surveillence Epidemiology and End Results program. Data on federal family planning program grants were provided by Martha Bailey and are described in (Bailey 2012).

Figure 4.3 Event study estimates of the effect of family planning center funding on age-adjusted cervical cancer mortality



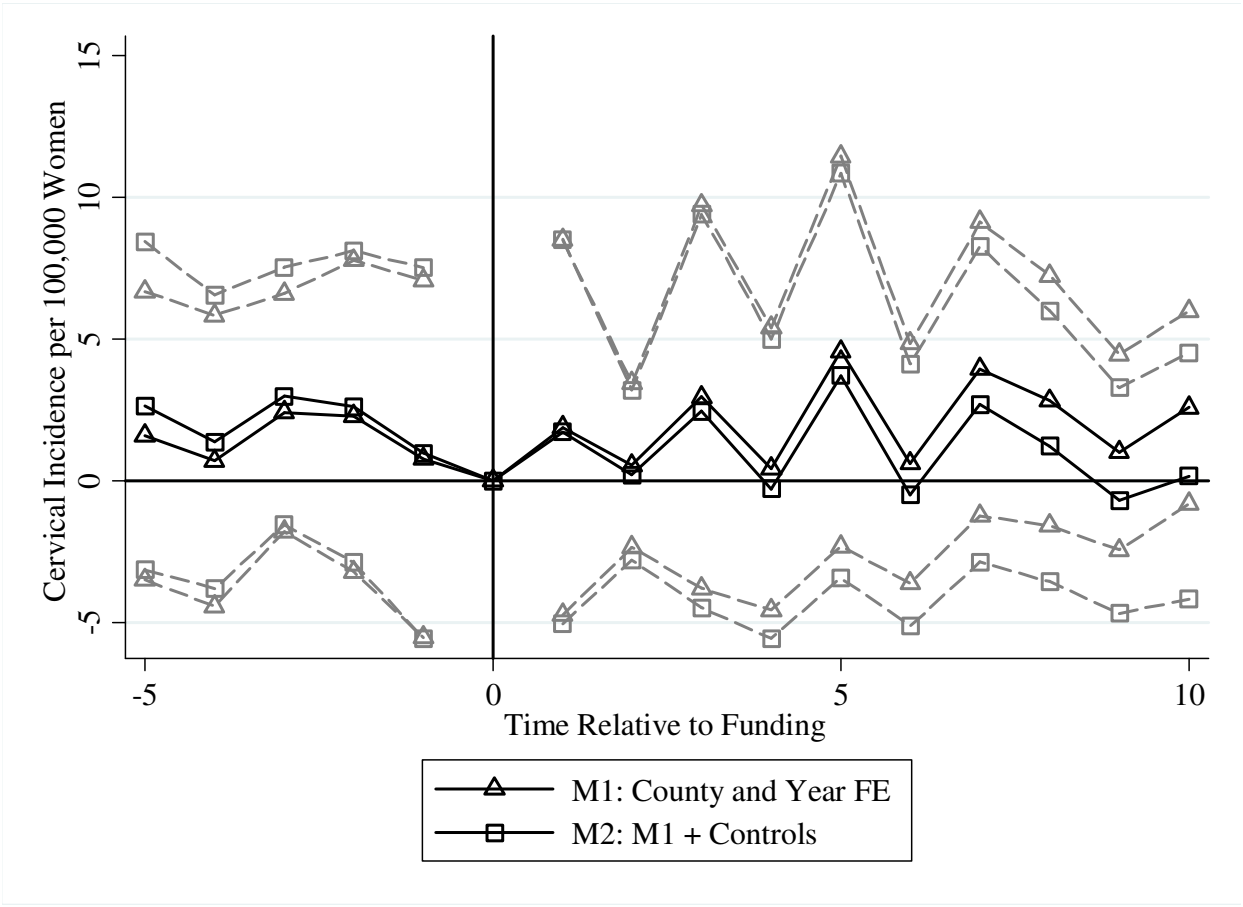
The figure plots event study point estimates and the 95% confidence intervals for the effect of living in a community with a federally funded family planning center on age-adjusted cervical cancer mortality relative to the year before funding was granted. Model one includes county and year fixed effects and model two adds state-by-year fixed effects and controls. Both models are weighted by county population. The sample includes 19,620 county-year observations from all counties. Age-adjusted cervical cancer mortality rates come from the 1959-1988 multiple cause mortality files and are adjusted to the 1960 age distribution using the 1960 census. All standard errors are heteroskedasticity robust and clustered at the county level

Figure 4.4 Event study estimates of effect of family planning center funding on age-adjusted cervical cancer mortality, funded counties only



The figure plots event study point estimates and the 95% confidence intervals for the effect of living in a community with a federally funded family planning center on age-adjusted cervical cancer mortality relative to the year before funding was granted. Model one includes county and year fixed effects and model two adds state-by-year fixed effects and controls. Both models are weighted by county population. The sample includes 19,620 county-year observations from all counties. Age adjusted cervical cancer mortality rates come from the 1959-1988 multiple cause mortality files and are adjusted to the 1960 age distribution using the 1960 census. All standard errors are heteroskedasticity robust and clustered at the county level.

Figure 4.5 Event study estimates of the effect of family planning center funding on age-adjusted cervical cancer incidence



The figure plots event study point estimates and the 95% confidence intervals for the effect of living in a community with a federally funded family planning center on age-adjusted cervical cancer incidence relative to the year before funding was granted. All models are estimated on data from the Connecticut Tumor Registry and include county and year fixed effects and use nationally representative probability weights. Model two introduces controls. The sample includes 248 county-year observations from all 8 counties in Connecticut. Incidence is adjusted to the 1960 age distribution using the 1960 census. All standard errors are heteroskedasticity robust and clustered at the county level

Table 4.1 Sample statistics, 1970 National Fertility Survey

			Funded			
	Never Funded (N=3203)		Eventually Funded (N=504)		Funded 1970 (N=3018)	
	mean	sd	mean	sd	mean	sd
Pap Smear Last Year	0.66	0.48	0.65	0.48	0.72	0.45
Sexual Last 4 Weeks	8.3	7.1	7.9	6.0	8. 6	8.4
Recent Condom Use	0.16	0.36	0.11	0.32	0.14	0.35
Age Categories						
14-19	0.04	0.18	0.04	0.21	0.03	0.18
20-29	0.41	0.40	0.37	0.41	0.42	0.40
30-39	0.37	0.48	0.35	0.48	0.36	0.48
40-45	0.18	0.38	0.24	0.43	0.18	0.38
Urban	0.35	0.48	0.45	0.50	0.31	0.46
White	0.86	0.35	0.94	0.23	0.88	0.32
Catholic	0.22	0.41	0.29	0.45	0.26	0.44
Education						
Elementary school	0.09	0.30	0.10	0.31	0.07	0.30
Less than high school	0.21	0.41	0.21	0.41	0.19	0.39
High school	0.47	0.50	0.47	0.50	0.45	0.50
Less than college	0.14	0.35	0.13	0.34	0.16	0.37
College or more	0.09	0.29	0.09	0.29	0.13	0.33

The table presents sample statistics for never-funded, funded, and eventually-funded PSUs from the National Fertility Study (1970) estimation sample. Never-funded observations come from PSUs that never received federal family planning grants, eventually-funded observations come from PSUs that had not yet received funding by 1970, but would by 1973, and funded observations come from PSUs that had received funding by 1970. The sample statistics are weighted using the study's sampling weights.

Table 4.2 The effect of family planning funding on Pap smear use in the previous year

	(1)	(2)
Funded Before 1970	0.056***	0.083***
	(0.012)	(0.017)
Urban	0.022*	0.049***
	(0.013)	(0.015)
White	0.056***	0.078***
	(0.018)	(0.019)
Catholic	0.008	0.006
	(0.014)	(0.015)
Aged 15-19	-0.012	-0.004
	(0.031)	(0.031)
Aged 30-39	-0.085***	-0.077***
	(0.012)	(0.013)
Aged 40-55	-0.168***	-0.163***
	(0.017)	(0.017)
Elementary school	0.090***	0.094***
	(0.024)	(0.025)
High school	0.177***	0.172***
	(0.023)	(0.024)
Some college	0.220***	0.219***
	(0.026)	(0.027)
College or more	0.250***	0.235***
	(0.027)	(0.029)
Constant	0.503***	0.462***
	(0.028)	(0.031)
Observations	6,676	6,476
R-squared	0.052	0.066
State Fixed Effects	N	Y
Percent Change	0.083	0.122

The table displays estimates of the effect of living in a county that received funding before 1970 on the likelihood of receiving a Pap smear in the previous 12 months using the 1970 National Fertility Survey. All regressions include an indicator for whether the woman lived in a county (primary sampling unit) that received funding before 1970 and an indicator for whether the primary sampling unit was urban as well as controls for race, age, catholic religion, education, and children ever born at the time of the survey. All estimates are weighted using nationally-representative population weights and clustered at the primary sampling unit level. *** indicates statistical significance at the 1% level, ** at the 5% level, and * at the 10% level. Percentage change displays the effect as a percentage of the average for each dependent variable in 1970

Table 4.3 The age-specific effects of family planning funding on Pap smear use in the previous year

	(1)	(2)
Funded Before 1970	0.058***	0.092***
	[0.017]	[0.021]
Funded Before 1970 X 10-19	-0.023	-0.043
	[0.060]	[0.061]
Funded Before 1970 X 30-39	-0.006	-0.019
	[0.024]	[0.025]
Funded Before 1970 X 40-55	0.006	-0.004
	[0.034]	[0.034]
Constant	0.503***	0.457***
	[0.030]	[0.032]
Observations	6676	6476
R-squared	0.050	0.066
State Fixed Effects	N	Y
Percentage Change		
Age 10-19	0.052	0.073
Age 20-29	0.085	0.14
Age 30-39	0.077	0.11
Age 40-55	0.094	0.13
Test Funded Before 1970 | Age Category = 0		
Age 10-19	0.55	0.42
Age 30-39	0.032	0.0060
Age 40-55	0.041	0.0042

The table displays age-specific estimates of the effect of living in a county that received funding before 1970 on the likelihood of receiving a Pap smear in the previous 12 months using the 1970 National Fertility Survey. The omitted age category in women aged 20-29. All regressions include an indicator for whether the woman lived in a primary sampling unit that received funding before 1970 and an indicator for whether the primary sampling unit was urban as well as controls for race, age, catholic religion, education, and children ever born at the time of the survey. All estimates are weighted using nationally-representative population weights and clustered at the primary sampling unit level. *** indicates statistical significance at the 1% level, ** at the 5% level, and * at the 10% level. Percentage change displays the effect as a percentage of the average for each dependent variable in 1970.

Table 4.4 The effect of family planning funding on channels of Human Papilloma Virus transmission

	Frequency of Sex		Recent Condom Use	
	(1)	(2)	(3)	(4)
Funded Before 1970	0.26	0.20	-0.0126	-0.014
	[0.22]	[0.31]	[0.010]	[0.013]
Urban	0.425*	0.282	-0.024**	-0.026**
	(0.234)	(0.283)	(0.010)	(0.011)
White	0.769**	0.828**	0.051***	0.048***
	(0.318)	(0.335)	(0.010)	(0.011)
Catholic	-0.59***	-0.63***	-0.03***	-0.04***
	(0.225)	(0.243)	(0.010)	(0.011)
Aged 15-19	1.56*	1.44*	-0.05***	-0.04***
	(0.855)	(0.868)	(0.015)	(0.016)
Aged 30-39	-1.90***	-1.87***	0.045***	0.038***
	(0.245)	(0.252)	(0.011)	(0.010)
Aged 40-55	-3.72***	-3.68***	0.067***	0.063***
	(0.249)	(0.253)	(0.014)	(0.013)
Elementary school	0.243	0.275	0.027*	0.021
	(0.473)	(0.485)	(0.014)	(0.015)
High school	0.963**	0.894*	0.097***	0.084***
	(0.451)	(0.458)	(0.013)	(0.014)
Some college	1.010**	0.901*	0.120***	0.114***
	(0.483)	(0.493)	(0.018)	(0.017)
College or more	0.955*	0.902*	0.145***	0.132***
	(0.516)	(0.526)	(0.020)	(0.019)
Constant	8.13***	8.21***	0.014	0.0277
	[0.49]	[0.55]	[0.016]	[0.018]
Observations	5516	5354	6720	6519
R-squared	0.044	0.050	0.030	0.048
State Fixed Effects	N	Y	N	Y

The table displays estimates of the effect of living in a primary sampling unit that received funding before 1970 on the number of times the woman reported having had sexual intercourse in the previous four weeks (columns 1 and 2) and having used a condom since her last pregnancy (or first intercourse if she had no children) (columns 3 and 4) using the 1970 National Fertility Survey. All regressions include an indicator for whether the woman lived in a primary sampling unit that received funding before 1970 and an indicator for whether the primary sampling unit was urban. All regressions include controls for race, age, catholic religion, education and children ever born at the time of the survey. All estimates are weighted using nationally-representative population weights and clustered at the primary sampling unit level. *** indicates statistical significance at the 1% level, ** at the 5% level, and * at the 10% level. Percentage change displays the effect as a percentage of the average for each dependent variable in 1970.

Table 4.5 Pooled estimates of the effect of family planning funding on age-adjusted cervical cancer mortality

	(1)	(2)
Periods -5 to -1	0.29	0.08
	[0.34]	[0.35]
Periods 1 to 5	0.16	0.152
	[0.36]	[0.37]
Periods 6 to 10	0.02	0.179
	[0.36]	[0.37]
Periods 11 to 15	-0.23	0.0847
	[0.34]	[0.35]
Constant	9.09***	2.527
	[0.24]	[5.2]
Observations	91,950	91,080
R-squared	0.048	0.068
County and Year Fixed Effects	Y	Y
State-by-year Fixed Effects	N	Y
County Trends	N	Y

The table displays the point estimates from an event study with dummies equal to five-year periods. The omitted category is the year before funding was granted. Estimates in column 1 include county and year fixed effects, and column 2 adds state-by-year fixed effects and controls. Both models are weighted by county population. The sample includes 19,620 county-year observations from all counties. Age-adjusted cervical cancer mortality rates come from the 1959-1988 multiple cause mortality files and are adjusted to the 1960 age distribution using the 1960 census. *** indicates statistical significance at the 1% level, ** at the 5% level, and * at the 10% level. All standard errors are heteroskedasticity robust and clustered at the county level

Table 4.6 Pooled estimates of the effect of family planning center funding on age-adjusted cervical cancer incidence

	(1)	(2)
Periods -5 to -1	-0.3	-0.2
	[2.0]	[1.9]
Periods 1 to 5	-1.1	-1.3
	[2.2]	[2.3]
Periods 6 to 10	0.7	0
	[2.0]	[2.6]
Constant	19.3***	23
	[1.5]	[45]
Observations	240	240
R-squared	0.44	0.47
County and Year Fixed Effects	Y	Y
County Trends	N	Y

The table displays the point estimates from an event study with dummies equal to five-year periods. The omitted category is the year before funding was granted. Estimates in column 1 include county and year fixed effects and column 2 adds controls. The sample includes 248 county-year observations from all 8 counties in Connecticut. Incidence is adjusted to the 1960 age distribution using the 1960 census. All standard errors are heteroskedasticity robust and clustered at the county level. *** indicates statistical significance at the 1% level, ** at the 5% level and * at the 10% level.

Appendix

Calculation of treatment on the treated

The parameters estimated in this paper provide intent-to-treat effects of federal family planning funding on Pap smear use and cervical cancer mortality. I convert these estimates to treatment-on-the-treated effects in order to compare them to other studies. Using the estimated change in screening as a result of funding in column two of Table 4.2 (0.083) and the average Pap smear screening rate in never-funded PSUs (0.66), I calculate that funding led to a 13 percent increase in screening. Using the lower 95 percent confidence interval on the estimate of the effect of family planning funding on cervical cancer mortality 5-10 years after funding from column two of table 3.5 (0.179, s.e.=0.37) and the average age-adjusted cervical cancer mortality rate over the estimation sample (6 deaths per 100,000 women), I find that family planning funding led to a nine percent decrease in age-adjusted cervical cancer mortality after 10 years. Dividing these two numbers gives the change in mortality given a one percent change in screening and an estimate of the treatment-on-the-treated. It is -72 percent.

I compare my treatment-on-the-treated effect to similarly calculated estimates using the results from (Johannesson, Geirsson, and Day 1978). They present rates of ever having received screening for each 10-year age cohort for each year between 1964 and 1974 and information on the age distribution of Icelandic women for each observation in the study period. Using these data, I calculate an age-adjusted screening rate for each time period and then calculate the percentage difference in Pap smear ever-use between 1964 (44%) and 1974 (77%). Using the five-year average mortality statistics provided in Johannesson, Geirsson, and Day (1978), I calculate the percentage change in age-adjusted cervical cancer mortality between the 1965-1970 period (26.5 deaths per 100,000 women) and the 1971-1975 period (12.2 deaths per 100,000 women). Dividing these two numbers gives the change in age-adjusted mortality given a one

percent change in the ever-screening rate and an estimate of the treatment-on-the-treated. It is also -72 percent.

Chapter 5 Conclusion

This dissertation presents evaluations of three pre-ACA policies resembling ACA reforms.

In the first chapter, I use variation across states and over time in state health insurance regulations and health insurance tax subsidies to show that the effect of a tax subsidy similar to the ACA's tax credits depends on the type on regulations that affect underwriting. I show that people with pre-existing conditions do not respond to tax subsidies in places where they cannot buy health insurance, but that they respond disproportionately in places where they can buy risk-rated health insurance. Most importantly for the ACA, in states in which people with pre-existing conditions face the same premium as healthy people, tax subsidies have no effect on either group.

The first chapter contains four policy-relevant findings. First, I show that job-lock affects self-employment for people with pre-existing conditions. Policymakers have consistently referred to people with pre-existing conditions as the quintessential case for job lock, yet little work explores the effect of high-cost medical conditions on employment outcomes. Second, I provide evidence that the benefits of two decades of increased generosity in the tax code towards health insurance for the self-employed accrued unevenly across health status and geography. While people with high-risk health conditions benefited disproportionately in the majority of states, they did not benefit at all in states in which they were mostly likely to be denied health insurance. Third, I show that tax subsidies and non-group market regulations interact. This

provides support for the ACA's two-pronged approach (tax subsidies and non-group market reforms) and casts doubt on recent proposals to repeal parts of the ACA or replace the ACA with tax-based subsidies alone. Additionally, it illustrates the importance of accounting for both types of policies in future evaluations of the ACA on self-employment. Fourth, the results of this paper suggest that the ACA's tax subsidies might not have an effect on self-employment, contrary to predictions by researchers and policymakers. The ACA's non-group market reforms establish community rating and guaranteed issue, and prohibit elimination riders and therefore, look most similar to community-rated states. Note that, while these results show that tax subsidies do not have an effect on self-employment in community rated states, my results do not imply that community rating and guaranteed issue will not have a direct effect on self-employment.

In the second chapter, I use the introduction of a minimum medical loss ratio rule in Michigan to show that a minimum MLR rule results in at least a six percentage point decrease in MLRs among for-profit commercial insurers. The reduction in commercial medical loss ratios was driven by a disproportionate reduction in benefit payments. The findings of this paper suggest that the establishment of a minimum MLR rule can result in unintended consequences. This chapter makes a number of contributions to the literature on the minimum MLR rule. It is the first to use an individual state's minimum MLR rule to evaluate the effect of such a policy. Although over 30 states had such rules at the time of the passage of the ACA, the effect of this type of rule on state insurance markets had never been estimated. I am also the first to use regulatory differences between states as a control group. This paper demonstrates the importance of a control group. Without a control group, I would have concluded that Michigan's

minimum MLR rule resulted in higher MLRs. Finally, the paper is the first to present a conceptual framework illustrating possible perverse incentives of minimum MLR rules.

In the third chapter I use quasi-experimental variation in the availability of subsidized pap smears to estimate the effect of this screening test on cervical cancer mortality. Although I find a large (8-13%) increase in the likelihood of receiving a Pap smear in the last year, event study point estimates suggest little correlation between screening and mortality. However, my estimates are imprecise so I cannot rule-out existing estimates of the Pap smear's effect on cervical cancer mortality. Even though the results using a natural experiment in Pap smear availability are inconclusive, the time-series evidence suggests that much of the 80 percent reduction in cervical cancer over the 20^{th} century had already occurred by the time Pap smears were used with sufficient frequency to substantially reduce cervical cancer.

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