ESSAYS IN HEALTH-RELATED PUBLIC POLICY

By

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To my parents, Michael and Dianne, and to my husband, Joseph

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My research examined three separate studies of health-related public policy. In the first study, I analyzed the effect of non-economic damage caps on the frequency of medical malpractice claims, recognizing that such laws are likely endogenous. I constructed a unique instrument using past and current values of state political composition and other factors. I also exploited exogenous Supreme Court findings of unconstitutionality. In both cases, I found that caps on non-economic damages are not associated with a reduction in medical malpractice claim frequency. This result is robust to alternative specifications and comparison groups.

Next, I considered risky behavior. The FDA recently approved a proposal to allow emergency contraception, or Plan B, to be available through pharmacies without a prescription. While this change is only now occurring nationally, several states had previously allowed pharmacy access to emergency contraception. In particular, Washington State was the first state to implement such a program in 1998. Proponents of pharmacy access argued that improved access could decrease the number of abortions. Opponents cited concern that pharmacy access could lead to an increase in risk-taking, especially among teens or young adults, and hence lead to increased rates of sexually-transmitted diseases. In my paper, I used county-level data as well as specific timing of pharmacy participation to consider the intended and unintended

consequences of pharmacy access to emergency contraception in Washington. My findings support both claims. Pharmacy access is associated with a small decrease in abortions for some age groups. In addition, pharmacy access is associated with an increase in Chlamydia rates for young women. These results are robust to an alternative comparison group as well as alternative definitions of treatment.

In the final study, I analyzed any-willing provider (AWP) legislation. In recent years, many states have implemented AWP legislation, which requires a managed care organization (MCO) to accept any provider, who agrees to the managed care organization's reimbursement rates, terms, and conditions, into its network. Proponents argue that AWP laws provide for larger networks, more patient choice, greater competition among providers, and arguably increased quality of care. Opponents cite AWP legislation as prohibiting managed care organizations from selective contracting and obtaining discounts by offering providers a larger volume of patients. Such legislation is therefore argued to prevent MCOs from effectively reducing health care costs. A small literature exists on the effect of these laws on hospital expenditures, physician expenditures, and total health care expenditures. Most studies, however, fail to recognize that the vast majority of the existing laws target pharmacies exclusively, as opposed to more comprehensive laws that also apply to physicians and hospitals. If AWP legislation prevents cost reduction available through selective contracting, then states with such legislation may incur higher health care expenditures. My paper is the first to analyze the impact of pharmacy-specific AWP legislation on state-level prescription drug expenditures.

CHAPTER 1
INTRODUCTION

My analysis of public policy issues is comprised of three studies: (1) Non-economic damage caps and medical malpractice claim frequency: is it time for a second opinion?, (2) The effects of increased access to the morning-after pill on abortion and sexually-transmitted disease rates, and (3) The impact of pharmacy-specific any-willing provider legislation on prescription drug expenditures.

The first study considered the effect of non-economic damage caps, one particular type of tort reform, on the frequency of medical malpractice claims, while recognizing that such laws are endogenous. I constructed a unique instrument by calculating the predicted probability that a law is in place in each of the prior years given state political composition and other factors. I then used the cumulative probability, based on current and past influences, as an instrument for the enactment of a cap. This procedure is preferable to using instruments of contemporaneous political control, an approach typically exploited in the literature. My approach produces strong first stage statistics. I found that caps on non-economic damages are not associated with a reduction in claim frequency. This result is robust to alternative specifications and comparison groups. In addition, I exploited exogenous Supreme Court findings of unconstitutionality. If non-economic damage caps are effective in reducing claim frequency, then the removal of such caps will increase claim frequency. Using this alternative approach, I again found no relationship between non-economic damage caps and claim frequency.

I also considered the effects of increased access to the morning-after pill. A recent FDA decision allowed emergency contraception, or Plan B, to be available through pharmacies without a prescription. While this change is only now occurring nationally, several states had previously allowed pharmacy access to emergency contraception. In particular, Washington State

was the first state to implement such a program in 1998. Proponents of pharmacy access argue that improved access could decrease the number of abortions. Opponents cite concern that pharmacy access could lead to an increase in risk taking, especially among teens or young adults, and hence lead to increased rates of sexually-transmitted diseases. In my paper, I used county-level data as well as specific timing of pharmacy participation to consider the intended and unintended consequences of pharmacy access to emergency contraception in Washington. My findings support both claims. Pharmacy access is associated with a small decrease in abortions for some age groups. In addition, pharmacy access is associated with an increase in Chlamydia rates for young women. These results are robust to an alternative comparison group as well as alternative definitions of treatment.

Finally, my analysis focused on another type of health policy, any-willing provider (AWP) legislation. In recent years, many states have implemented AWP legislation, which requires a managed care organization (MCO) to accept any provider, who agrees to the managed care organization's reimbursement rates, terms, and conditions, into its network. Proponents argue that AWP laws provide for larger networks, more patient choice, greater competition among providers, and arguably increased quality of care. Opponents cite AWP legislation as prohibiting managed care organizations from selective contracting and obtaining discounts by offering providers a larger volume of patients. Such legislation is therefore argued to prevent MCOs from effectively reducing health care costs. A small literature exists on the effect of these laws on hospital expenditures, physician expenditures, and total health care expenditures. Most studies, however, fail to recognize that the vast majority of the existing laws target pharmacies exclusively, as opposed to more comprehensive laws that also apply to physicians and hospitals. If AWP legislation prevents cost reduction available through selective contracting, then states

with such legislation may incur higher health care expenditures. My research is the first to analyze the impact of pharmacy-specific AWP legislation on state-level prescription drug expenditures.

CHAPTER 2
NON-ECONOMIC DAMAGE CAPS AND MEDICAL MALPRACTICE CLAIM FREQUENCY: IS IT TIME FOR A SECOND OPINION

Introduction

Medical malpractice litigation is a source of much concern. Claim frequency is on the rise. Between 1993 and 2002, claims increased by 18% (National Center for State Courts, 2003). In addition, median awards jumped from $253,000 in 1992 to $431,000 in 2001, a real increase of 70% (Bureau of Justice Statistics, 2001). As a result, physicians are faced with rising insurance premiums. In spite of soaring premiums, major medical malpractice insurers are exiting the industry (Freudenheim, 2001). Anecdotal evidence suggests that there may be additional effects of medical malpractice litigation. For example, physicians are said to be transferring states in search of lower insurance premiums (Wagner, 2004). Specialists in obstetrics/gynecology are reportedly refusing to perform difficult deliveries and changing their procedure usage due to the fear of malpractice suits (American College of Obstetricians and Gynecologists, 2004). Some claim that physicians are changing their fields of specialty (American College of Obstetricians and Gynecologists, 2004)[1] or are practicing without insurance (Silverman, 2004).

The public policy response has been tort reform, with particular interest in imposing caps on non-economic damages.[2] Non-economic damages are typically those awarded for pain and

[1] Fourteen percent of respondents to an ACOG Survey stopped practicing obstetrics because of the risk of medical liability claims.

[2] Tort reform, with a focus on medical malpractice litigation, has also been proposed at the national level. The Comprehensive Medical Malpractice Reform Act of 2005 (HR 321) calls for a cap on non-economic damages of $250,000, adjusted for inflation from 1975.

suffering, loss of consortium or companionship, or emotional distress.[3] Proponents of tort reform argue that non-economic damage caps will decrease claim severity through a reduction in non-economic damage awards. Additionally, non-economic damage caps alter the expected benefits of filing a claim by reducing the available non-economic damages. If the expected benefits fail to exceed the costs of pursuing a medical malpractice action, then some claims may be deterred. Is this behavioral response to caps on non-economic damages common enough to lead to a discernable reduction in claim frequency?

An extensive literature exists regarding the effects of tort reform on a number of different outcomes: claim frequency (Danzon, 1984; Danzon, 1986),[4] claim severity (Danzon, 1984; Danzon, 1986; Yoon, 1991; Browne & Puelz (1999), insurance profitability (Viscusi & Born, 1995; Born, Viscusi, & Carlton, 1998), and premiums (Thorpe, 2004), and physician supply (Matsa, 2005; Klick & Strattman, 2003). Only one main study specifically targets medical malpractice claim frequency, as opposed to the frequency of general tort claims. Danzon (1984) focuses on the determinants of claim frequency as a result of tort reforms enacted during the 1970s.[5] Danzon reports that while limits on awards do have an effect on claim severity, such caps (as well as other tort reforms) do not have an effect on claim frequency.

[3] Although most commonly referred to as non-economic damages, these damages are "non-pecuniary" in the sense that they do not compensate the plaintiff for lost earnings or medical expenses, but rather for a loss that is difficult to quantify. The "non-economic" characterization is clearly wrong. These damages may be non-pecuniary, but they are hardly non-economic.

[4] Danzon (1984) and Danzon (1986) focus on medical malpractice claim frequency specifically, Lee, Browne, & Schmidt (1994) and Browne & Puelz (1999) focus on general tort claims and automobile accident claims, respectively.

[5] Danzon (1986) presents similar evidence to Danzon (1984), but does not specifically address the relationship between damage caps and claim frequency.

My study focuses on the effect of non-economic damage cap legislation on the frequency of medical malpractice claim frequency, as measured by successful medical malpractice lawsuits. My paper furthers the existing literature in several ways. As noted earlier, this is a subject that has received little attention in the literature. This is the first study to examine the impact of recent non-economic damage caps legislation on claim frequency and to use appropriate instrumental variables estimation.

The literature that estimates the effects of various policies, including tort reforms, is plagued by the fact that economic and political forces partly determine whether a policy is enacted. The estimated impact of the policy may reflect the factors that played a role in enacting the policy rather than the true effect of the policy. Others working in the policy area have recognized this problem and have employed instrumental variables procedures to deal with the endogeneity bias due to spurious correlation between the policy and the factors that determine whether the policy is enacted. My study is the first to employ instrumental variable techniques to study the effect of non-economic damage caps on the frequency of medical malpractice claims.

My study uses a novel strategy to remedy policy endogeneity.[6] The instrumental variable policy literature assumes that only current factors determine whether a law is in place. But legislators deal with a small number of issues each year and as a result laws are changed infrequently. Therefore, the probability that a law is in place not only reflects the current factors

[6] Other studies have treated tort reforms as endogenous. Klick & Stratmann (2003), who consider the effect of tort reform on physician supply, use indicators for whether the state legislature is controlled by the Democratic party, if corporations can make political contributions, and other unspecified instruments. Rubin & Shepherd (2005), who study the effect of tort reform on accidental death rates, use the state population voting Republican in each presidential election as well as per capita employment in the legal profession. Sharkey (2005) uses contemporaneous state political control as an instrument for damage caps, but finds that it is a weak instrument and proceeds with OLS estimation.

affecting the enactment of the law but also incorporates past influences. The cumulative probability that a law is in place, based on current and past values of these factors, should best explain whether the law is currently in place. I compare the fits under various formulations and demonstrate that it is important to consider prior values in addition to current values of factors affecting the enactment of a law. A better fit is obtained using prior influences of law enactment than with contemporaneous influences. The best fit is obtained when the predicted probability that a law is in place reflects the probability that the law was enacted in each of the prior years (i.e., the cumulative probability). This unique strategy for instrumentation yields strong first stage results, making the estimated effects of damage caps more plausible. The novel strategy used here for taking account of the stickiness of state laws when dealing with policy endogeneity should be of value in other studies that consider the impact of public policies.

The effects of non-economic damage caps on claim frequency are also estimated using an alternative methodology. This approach exploits exogenous changes in the law (state court findings of unconstitutionality) to consider the effect of removing a cap on claim frequency. This approach thus avoids having to deal with the endogeneity associated with the enactment of a law.

Since caps on non-economic damages should decrease the expected return of filing a suit, one would expect to find a negative relationship between claim frequency and the imposition of damage caps. While ordinary least squares (OLS) results indicate a negative and statistically significant relationship between non-economic damage caps and claim frequency, two-stage least squares (2SLS) results yield different findings. When using the instrumental variables approach, caps do not have a statistically significant effect on claim frequency. This result is robust to a variety of specifications including different instrument definitions. Similarly,

estimates based on exogenous state Supreme Court rulings indicate that the removal of a damage cap does not have an identifiable effect on claim frequency.

Empirical Model

Proponents of tort reform argue that caps on non-economic damages will decrease claim frequency and claim severity, reduce malpractice premiums, and thereby improve access to health care. It may, therefore, be difficult to argue that imposing a cap is an exogenous policy change. To assume exogeneity, we must believe that caps on non-economic damages were not implemented with the underlying factors of the health care industry or medical malpractice in mind. If we suspect that caps are enacted as a result of underlying factors, then we should be concerned that non-economic damage caps and the number of suits are simultaneously determined, and therefore endogenous.

Model and Dependent Variable

A two-stage least squares approach is utilized to account for the potential endogeneity of non-economic damage caps. A model representing claim frequency is specified in the following equation:

$$M_{it} = \alpha_{it} + \delta S_{it} + \beta R_{it} + \varphi_i + \theta_t + \varepsilon_{it} \qquad (2\text{-}1)$$

where $i = 1,...I$ for each state and $t = 1,...T$ for each year. M_{it} represents the natural logarithm of the number of successful medical malpractice suits per 100,000 population in state *i* in year *t*. [7] Since claim frequency reflects the size of the state, the number of successful suits is

[7] This logarithmic transformation was made for ease of interpretation. All models were initially run with the number of successful medical malpractice suits per 100,000 of the population as the dependent variable rather than the natural logarithm. Estimates obtained from the initial models are very similar to the estimates presented here.

scaled by the state population.[8] S_{it} is a vector indicating the state-level demographic variables and R_{it} indicates a vector of tort reform variables. φ_i denotes state fixed effects and θ_t indicates year fixed effects.

Independent Variables

The independent variables used to control for variations in state demographic and health characteristics are described in this section.[9] Dummy variables indicating the existence of non-economic damage caps are also included. Additionally, state and year fixed effects were utilized to capture unobserved state and time differences. Summary statistics are included in Table 2-1 and Table 2-2 contains the source information for each of the variables included.

Disposable Income per Capita. In general, higher levels of per capita income are associated with better health levels.[10] Higher incomes are associated with a greater propensity for healthy activity, less demand for medical care, and therefore less exposure to medical malpractice. It is also possible that individuals with higher incomes are less likely to file suit given the higher opportunity cost of their time. The predicted sign of this variable, IncomePerCapita, is negative.

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[8] Total suits scaled by the state population provided a better fit than total suits scaled by the number of physicians.

[9] The literature has suggested that lawyers per capita may be a determinant of claim frequency. This variable was not included for three main reasons. First, the number of lawyers per capita is likely to be endogenous since lawyers respond to the cost of suing. Second, lawyers per capita may be multicollinear with the percent of the population living in a metropolitan area. Finally, this variable was never significant in any model, and the results are therefore omitted.

[10] Nominal disposable income per capita was converted to 1991 dollars using the Consumer Price Index-Urban (CPI).

Percent of Population Living in Metropolitan Areas. The inclusion of an urbanization variable captures the level of medical complexity and legal specialization. Urban areas are centers of medical treatment, and are likely to offer more complex medical treatments which could lead to more occurrences of medical malpractice.[11] Additionally, metropolitan areas may exhibit less personal doctor-patient relationships than less urban areas. This anonymity may contribute to a higher rate of litigation. Moreover, metropolitan areas provide greater access to litigation. Hence, the percent of the population living in metropolitan areas, Metro, is predicted to have a positive impact on claim frequency.

Personal Health Care Expenditures. Medical care – physician visits, prescription drugs, nursing homes, and the like – are a growing portion of our economy. To capture the importance and size of the medical sector, the level of personal health care expenditures as a percentage of Gross State Product (GSP) has been included in this model. One would expect that as the size of the health care industry increases, the greater the interaction between individuals and the medical community. This increased exposure to many different components of the medical sector could potentially lead to more medical malpractice exposure and could therefore result in more suits.[12]

Unemployment Rate. The state unemployment rate was included as a general measure of the economic conditions during the relevant time period. When unemployment rates are high,

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[11] Danzon (1984) and Lee, Browne, and Schmidt (1994) also used an urbanization variable in their studies. Each found urbanization to be statistically significant and a strong positive determinant of claim frequency.

[12] Following other studies, specific medical treatment variables had been included in my study prior to including personal health care expenditures as a percentage of GSP. Initially, percent of the population over age 65, surgeries per capita, outpatient visits per capita, and births per capita were included in the model. While some of these variables had an effect on the number of suits per capita, personal health care expenditures is a more comprehensive measure of health care utilization and ultimately provided a better fit for the model.

opportunity cost for lawyers and those injured is low and therefore may lead to more suits. An increase in the unemployment rate, however, could also be associated with a decrease in claim frequency. If unemployment is high, we may see more individuals without insurance. If this is the case, individuals without health insurance benefits may be less likely to seek medical care, and we may see a decrease in claim frequency during economic downturns.

Non-Economic Damage Caps. Caps on non-economic damages are designed to decrease the non-economic damages available to a plaintiff, which reduces the value of the total award. At the margin, the expected benefits may no longer exceed the expected costs, and therefore some medical malpractice cases will not be filed. Thus, it is possible that claim frequency will decline following the imposition of a cap on non-economic damages. If, however, the driving force of most suits is due to economic losses, then non-economic damage caps may have little effect on claim frequency.

States that enacted a damage cap during the relevant data period receive a value of 1 for the year in which their damage cap was implemented and every year thereafter. For example, Alaska, which established a cap in 1997, receives a value of 1 for the years 1997 through 2001, and 0 otherwise. Potential lags in policy effectiveness will be considered in Section IV.

Table 2-3 lists the states and associated years in which non-economic damage caps were implemented as well as the dollar amount of the cap (in the year in which it was enacted).[13] Only reforms that were implemented during the 1991-2001 period were used in this analysis. Any state that enacted a non-economic damage cap before 1991 or that had a cap on total damages was omitted from this analysis. Any state with a specific medical malpractice non-economic

[13] Tables 2-4 and 2-5 contain more detailed documentation of state laws on non-economic damage caps.

damage cap is included as well as any state with a non-economic damage cap on general tort claims.

The American Tort Reform Association (ATRA) outlines its "ideal package" regarding medical malpractice reform. Included in the suggested reforms is a severe non-economic damage cap of $250,000, collateral source reform, and a sliding scale for attorneys' fees. The collateral source rule prevents evidence involving payments to the plaintiff from a third party, such as insurance payments, workers' compensation, or social security benefits, from being admitted at trial. Collateral source reform typically involves the admissibility of third party payments at trial. Almost all collateral source reforms took place prior to the time period considered in my study. The other proposed reform, a sliding scale for attorneys' fees, limits the amount that attorneys can collect in contingency fees. All states which enacted limits on attorneys' fees did so before the time period considered in my study. Therefore, any potential effect of these reforms should be contained in the state fixed effects. Variables indicating the presence of these two reforms are therefore not included in the model.

While other reforms such as joint and several liability reform and limits on punitive damages may have an impact on tort filings in general, they are not necessarily relevant when considering medical malpractice specifically. The ATRA does not consider these reforms as an essential remedy to the current health care crisis. Joint and several liability affects tort filings *indirectly* because the reform does not directly target damages.[14] Punitive damages are rarely awarded in medical malpractice cases.[15] Damages of this type are used solely to punish the

[14] See Kessler & McClellan (1996) who categorize reforms as direct and indirect.

[15] Eisenberg et al (1997, page 623) comment that "juries rarely award punitive damages and appear to be reluctant to do so in areas of law that have captured most attention, products liability and medical malpractice. Punitive damages are most frequently awarded in business/contract cases and intentional tort cases."

defendant, not to compensate the plaintiff.[16] As a result, these two reforms are not included as independent variables in the analysis.

Instrumentation

If indeed non-economic damage caps are endogenous, an instrumental variable is required in order to proceed. In this case, a proper instrument for the enactment of a non-economic damage cap must be unrelated to the underlying conditions of the health care industry. The political composition of state or federal government is often utilized as an instrumental variable in studies of public policies.[17] In my paper, a unique instrument using state political composition is devised in order to account for the enactment of non-economic damage caps in a particular year.[18]

Tort reform, and caps on non-economic damages in particular, are policies typically supported by Republicans. As such, we would expect to find caps in states with more conservative representation. Control in this context is defined as one party controlling the State Senate, the State House, and the Governor's Office. If in a particular year, Republicans gain

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[16] It is also possible that juries use non-economic damage awards to punish physicians for malpractice infractions beyond any compensation for pain and suffering.

[17] See, for example, Klick & Stratmann (2003), Rubin & Shepherd (2005), and Sharkey (2005).

[18] Several other instruments, *TermLimits* and *MedSchool*, were initially attempted in conjunction with *RepEver*, but lacked sufficient power as measured by first stage statistics. Term limits lower the value of holding office and in turn can generate a lower level of contributions from special interest groups. It is possible that a state with term limits may be less likely to adopt such laws if special interest groups are less likely to try to influence legislators who have limited tenure. Additionally, states with reputable medical schools may be more likely to support damage caps to protect their prosperous physicians from being sued. To capture this idea, the union of the top 50 medical schools by research and the top 50 medical schools by primary care were used. Both of these potential variables were determined to be weak instruments and were therefore omitted.

control of all three branches, we might expect a non-economic damage cap to be passed in that state. Contemporaneous control, *RepCurrent*, equals 1 if Republicans controlled the Senate, House, and Governor's Office in state *i* in year *t*. If Republicans did not hold control, that state receives a value of 0 in year *t*. An alternative form of the instrument involves considering prior values of control, or when a change in political composition occurred, rather than simply the state of current control. To capture this idea, the instrumental variable *RepEver* was created. This instrument equals 1 in the first year in which the Republicans controlled all three branches of state government between 1991 and 2001, and every year thereafter. The idea is that once Republicans take control, a cap will be enacted and will persist every year thereafter. A non-economic damage cap has never been rescinded, so this particular definition is credible.[19]

In order for the first stage to be successful, we must have strong correlation between the measure of political composition used and the enactment of the cap. Figure 2-1 illustrates the relationship among the instrument *RepCurrent*, the instrument *RepEver*, and the enactment of the damage cap by state. As shown in Figure 2-1, *RepEver* performs well for seven of the eight states that enacted caps between 1991 and 2001: Alaska, Illinois, Montana, North Dakota, Ohio, South Dakota, and Wisconsin. Illinois, for example, experienced a political shift in 1994 when Republicans took control of the State House, State Senate, and Governor's Office. A non-economic damage cap was enacted in Illinois in 1995. The law adoption almost perfectly coincides with the change in political composition. Republican control existed in Illinois during 1994 and 1995, as illustrated by *RepCurrent.*

[19] Several non-economic damage caps have been found unconstitutional. This, however, is not a result of a change in political composition, but rather an inherent problem with the construction of the law.

Given that the endogenous variable is binary, it may be more appropriate to use an alternative technique to standard instrumental variables estimation. *RepEver* assumes that the law was passed when Republicans gained control of state government. But the probability that a law is in place in a particular year reflects prior probabilities that the law was passed. In other words, a state that has had Republican control for four years would be more likely to have a non-economic damage cap than a state with Republican control for only one year. To take account of this consideration, a binary choice model is utilized.[20] This procedure produces valid standard errors[21] and more precise estimates. First, a logit model is estimated for observations in which a cap has not yet been passed. The dependent variable equals 1 if the cap was enacted in that year and 0 if no cap was enacted in that year. Explanatory variables in the logit model include *RepCurrent* as well as the four explanatory variables defined previously. Predicted probabilities obtained from the logit procedure estimate the probability that a cap was adopted in each year given contemporaneous characteristics. For each state, these predicted probabilities can vary over time and are higher when the Republicans control of state government. These probabilities are then used to construct the cumulative probability that a cap was passed in year *t*, beginning with 1991. For example, the probability of passing a law in year 0 is p_0, where p_0 is the predicted probability. The probability of not passing a law in year 0, therefore, is $1-p_0$. Similarly, the probability of not passing a law in year 1 is $1-p_1$. To find the probability of a law at the end of year 1, we must consider both year 0 and year 1. The probability, therefore, of a law at the end of year 1 is $1-(1-p_0)(1-p_1)$. This expression can be restated as $p_0+(1-p_0)p_1$,

[20] See Wooldridge (2002) pages 623-625 and Cameron & Trivedi (2005) pages 192-193.

[21] Valid standard errors would not be available in the event that the first stage is a binary choice model and the predicted values were then substituted in the second stage.

meaning that the probability of a law being enacted in year 1 is the probability that it was enacted in year 0 plus the probability that it was enacted in year 1 times the probability that it was not enacted in year 0.

If the predicted probabilities of adoption for this state are 0.05 in 1991 (year 0), 0.08 in 1992, and 0.11 in 1993, then the probability of having a cap in 1991 is simply 0.05, while the probability of having a cap in 1992 is 0.13. Using the same method, the probability of having a law in 1993 is estimated to be 0.22. The probability of having a cap rises over time, reflecting more years with some prospect of adoption. The probability of having a cap is also greater in states in which Republican's have controlled state government for multiple years.

This calculation of cumulative probabilities from the predicted probabilities is conducted for the remaining years. In this way, the cumulative probabilities account for prior influences, including past political composition. These cumulative probabilities are then used as an alternative instrument to *RepCurrent* and *RepEver*. By construction, the cumulative probabilities rise over time by state and are constrained between 0 and 1.

Data

Under the requirements of the *Health Care Quality Improvement Act*,[22] all medical malpractice payments must be reported.[23] These individual reports are contained in the *National Practitioner Data Bank (NPDB) Public Use File*.[24] The malpractice portion of this data set is

[22] Title IV of Public Law 99-660, *Health Care Quality Improvement Act of 1986*.

[23] The Health Care Quality Improvement Act of 1986 imposes civil penalties of up to $11,000 for a failure to report each medical malpractice payment, per Section 421(c).

[24] *National Practitioner Data Bank Public Use File*, (August 30, 2005), U.S. Department of Health and Human Services, Health Resources and Services Administration, Bureau of Health Professionals, Division of Practitioner Data Banks.

relatively complete in the sense that it contains all reported medical malpractice payments.[25] These data do not contain information on cases filed that resulted in no payment, which would include dismissals, directed verdicts, summary judgments, and verdicts for the defendant. In my analysis, *cases* and *payments* refer to judgments for the plaintiff or settlements. No data set is able to completely represent the total number of malpractice occurrences; however, this data set provides a useful subset.

Each record contains information about the practitioner, such as the work state, home state, license state, license field, age group, and year of graduation.[26] Specific information regarding the alleged malpractice incident includes the cause of the malpractice action, year, payment amount, and whether the case was settled or fully litigated.

While the dataset is not suitable for some research projects, it is acceptable for my purposes.[27] I do not need information on physician field of specialization. Rather, I require a measure of aggregate claims by state and by year for my dependent variable, which this dataset

[25] Recent controversy exists as to the completeness of the data bank. If the physician named in the original suit is removed from the final settlement papers, the payment is not required to be reported. Only payments associated with physicians, not hospitals or insurance companies, are subject to mandatory reporting. This loop-hole in the system has been referred to as the corporate shield, and therefore might indicate undercounting of settlements in the data bank (Hallinan 2004).

[26] The state variable required some necessary assumptions. The database reports three state variables: work state, home state, and license state. However, only the work state or the home state is required, not both. Additionally, a practitioner can be licensed in more than one state. In this data set, only the first state of license listed is recorded. Work state has been used as the primary measure of a practitioner's state; if no work state was reported, then home state was used. If neither home state nor work state was provided, then license state was used.

[27] Other researchers, e.g., Baicker & Chandra (2004) and Matsa (2005), have used the NPDB.

provides. Although the NPDB was not designed as a research tool, it is perhaps the best nationally collected data set on medical malpractice suits.[28]

Ultimately, the units of observation in this analysis are not at the individual claim level, but rather are a measure of claim frequency per capita. Individual-level data were aggregated to the number of successful suits in a given state, in a given year. The NPDB began collecting data at the end of 1990 and, therefore, 1991 is the first complete year of data. Observations from 30 states for a period of 11 years, 1991-2001, were constructed. Eighteen states that had previously enacted damage caps or total damage caps were removed from the analysis. Ultimately, the sample was reduced further by removing several years for two specific states. Ohio and Illinois passed non-economic damage caps between 1991 and 2001 and their courts subsequently found them unconstitutional during the same time period. The years following the finding of unconstitutionality for Illinois (1998 and after) and for Ohio (1999 and after) are omitted from the analysis. This creates an unbalanced panel dataset with a total of 324 observations.

Identification

This analysis uses a specific set of states over an eleven-year period. In order to identify the effect of caps on claim frequency, the most appropriate comparison is between states that enacted caps on non-economic damages between 1991 and 2001 and states that did not have damage caps during the relevant time period. There are several ways to categorize the states and therefore, several sets of potential comparison groups. The analysis was conducted with multiple comparison groups in order to determine whether the results are robust and not conditional on the specific set of states used.

[28] Very few states collect detailed data on closed claims. Those that do include Florida, Illinois, Missouri, Minnesota, Massachusetts, Nevada, and Texas, of which only Texas and Florida make their data available for research purposes. See Black (2005).

Table 2-6 describes some specific criteria for considering the remaining potential control states. As previously listed in Table 2-3, the states with recent non-economic damage caps are those in *Sets 1* and *2* of Table 2-6. The original analysis is estimated using only those states described in *Set 3* of Table 2-6 as comparison states, that is, states that never had non-economic damage caps. This specification excludes states that had non-economic damage caps that were later found unconstitutional prior to 1991. A second specification excludes only states that had caps previously in place, but includes states that may have had caps in the past, but do not have caps now due to laws being found unconstitutional (includes *Set 4).*

Since changes in the law occurred in different years for different states, it is difficult to illustrate pre and post characteristics. In 1991, however, none of the states in the reduced sample (*Sets 1, 2, and 3)* had yet adopted a non-economic damage cap. Table 2-7 presents baseline statistics for capped states and non-capped states. As shown in the table, both sets of states look very similar in terms of the observed characteristics. We can, therefore, be fairly confident that the capped and non-capped states are similar prior to any policy change.

Policy Endogeneity

Many studies that consider the effect of tort reforms, and in particular non-economic damages caps, fail to account for the fact that such policies were put into effect in a nonrandom fashion. In other words, such policies may have underlying causes that could be correlated with the policy change. This leads to an endogeneity problem that would bias any coefficient estimates of the policy. In order to properly identify the effect of the policy, we must correct for the endogeneity, but in addition, consider first the potential direction of the bias. If policies are enacted in response to a surge in claim frequency due to omitted factors, then we might expect the estimate of the policy change to be biased upwards. In other words, if the true effect of a non-

economic damage cap is negative (or zero), such a bias would favor a finding of no effect (or a positive effect).

At first blush, this explanation may sound reasonable. This does not appear to be the case, however, based on the available data. For each of the states which enacted a cap between 1991 and 2001, Table 2-8 displays the percentage change in the number of suits per 100,000 capita between the year in which the cap was enacted ($t = 0$) and one year prior ($t = -1$) as well as between $t = -1$ and $t = -2$. Each state is then compared to the average percentage change of the set of comparison states (see *Set 3* of Table 2-6) for the corresponding years. The comparison group for each state with a cap is comprised of the same states, but differs according to which years are used, since each capped state has its own specific year zero. The experience of the comparison states is a baseline, so consider the sign of the difference column. The difference illustrates that, relative to the comparison set, the level of litigation in most capped states was declining somewhat, not spiking, in the two years prior to the policy change.

Between $t = -2$ and $t = -1$, only three states experienced positive differences. Only Alaska experienced a large change in suits, which is mainly due to the low level of litigation in that state. Between $t = -1$ and $t = 0$, only two states experienced a positive difference, where one of the two values is close to zero. Overall, it does not appear that an abnormally high number of suits occurred in the period before the cap for any of the capped states relative to the comparison set.

It appears that states which enacted non-economic damage cap legislation did not do so in response to surges in claims in the years preceding the policy change. An alternative explanation for the endogeneity of the caps is the presence of factors which are associated with the enactment of a cap that discourage litigation and therefore would bias claim frequency in the negative

direction. For example, there may be a common perception that medical malpractice cases are filed too often, receive excessive publicized awards, or are often frivolous. There may be more of a general backlash due to this perception in states enacting caps. This would bias the coefficient down (i.e., more negative).

Empirical Results

First Stage

The estimates obtained from the first stage are displayed in Table 2-9. This table reports several different instrumental variables including (1) *RepCurrent*, (2) *RepEver*, and (3) cumulative probabilities derived from predicted probabilities from a logit model using *RepCurrent.* In all three cases, the instrument is a positive and significant predictor of *Cap.* The important difference among these three specifications is seen in the first stage statistics: partial R-squared[29] and F- statistic.[30] Low F-statistic or partial R-squared statistics indicate the presence of a weak instrument. *RepCurrent* alone does not appear to be a very strong instrument, with a low F-statistic and partial R-squared. This contemporaneous version of control is what has previously appeared in the literature. *RepEver* provides an improvement over *RepCurrent*, with an F-statistic well above 10 and partial R-squared of approximately 13 percent. The statistics associated with *RepEver* are consistent with the information provided in Figures 1 -8; *RepEver* is a strong predictor of *Cap.*[31] Using the predicted cumulative probabilities from the logit model

[29] A partial R-squared is defined as the variation in the endogenous variable that is explained by the instrument. In this case, it is the variation in the adoption of non-economic damage caps that is explained by the instrument choice. This statistic is obtained from the first-stage regression.

[30] The standard threshold level for a valid instrument is an F-statistic of greater than 10 (Cameron & Trivedi, 2005).

[31] The strength of the instrumental variable *RepEver* is not conditional on its particular definition of control. Variations of *RepEver* were utilized to test the particular definition of control: *RepEver60* and *RepEver2*. *RepEver60* requires a 60 percent majority of Republicans in the

with *RepCurrent* provides even stronger first stage statistics, with an F-statistic over 75 and a partial R-squared of about 22 percent. Given the strength of the first stage statistics, the predicted cumulative probabilities will be used as the preferred instrument choice in what follows.

Second Stage

The 2SLS results from estimating Equation (1) are displayed in Table 2-10. OLS results are presented in column (1) of this table for comparison purposes. Without accounting for endogeneity, damage caps have a negative and statistically significant effect on claim frequency. If non-economic damage caps reduce the incentives to file by reducing the expected benefits of filing a claim, then we might expect to find this result. Using the OLS estimates, one would conclude that a cap reduces the amount of litigation by about 21 percent. But when the instrument variables procedure is used, the coefficient is no longer statistically significant. Columns 2 through 5 in Table 2-10 employ the predicted cumulative probabilities obtained from the logit model as the instrument of choice.[32] Table 2-10 also displays the confidence interval around *Cap.* As seen in column (2), this coefficient is imprecise. The interval ranges from a 25 percent decrease to a 18 percent increase in suits per 100,000 of the population. It is important to note that the lack of statistical significance on the variable *Cap* is not a result of substantially larger 2SLS standard errors.

The remaining coefficients are also presented in column (2). The percent of the population living in a metropolitan area and the unemployment rate are not statistically significant. Disposable income per capita also has a statistically significant impact on the amount of

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House, in the Senate, and a Republican Governor. *RepEver2* defines control according to only the State House and Senate, but not the Governor. Each of these alternatives provided similar results to using *RepEver*.

[32] Similar results to those presented in Table 7 are obtained when using the other instrument variations, *RepCurrent* and *RepEver*.

litigation. This negative coefficient fits our hypothesis. The demand for medical care was controlled for using *PersonalHealthExpend*. This variable is highly significant and positive. The coefficient indicates that a one unit increase in *PersonalHealthExpend* leads to a five percent increase in claim frequency.

Several other variants of capped and non-capped states were used to estimate equation (1). The results of these alternatives are presented in columns (3) - (5) of Table 2-10. Column (3) uses the same comparison states as Column (2), however, this specification does not omit states that had caps prior to 1991 that were found unconstitutional before or during 1991. Originally omitted from the analysis, observations for Alabama, New Hampshire, and Washington are included in column (3). The results using this alternative set of comparison states are very similar to the results presented in column (2). Column (4) employs the same set of comparison states as column (2) while column (5) uses the comparison group as column (3), but the years following the findings of unconstitutionality in Ohio and Illinois are not omitted. Again, these results are consistent with those presented in the original model shown in column (2). In all cases, caps were not statistically significant. Caps do not reduce claim frequency.

Lags in Suit Duration

The dataset used in my study records claims at their date of completion, whether that date signifies the time of settlement or the time of judgment. If caps affect the decision to file, it may be necessary to consider a lag in suits based on several variants in duration. Not only it is possible that there is a lag in the policy's effectiveness, but there may also be time between the opening and closing of a claim. The previous analysis is re-estimated according to Equation (2):

$$M_{it+s} = \alpha_{it} + \delta S_{it} + \beta R_{it} + \varphi_i + \theta_t + \varepsilon_{it} \qquad (2\text{-}2)$$

where *s* is the lag time, and is defined as either 1, 2, or 3 years. In other words, given a one year lag, a suit that is closed in year *t*+1, will be explained by year *t* characteristics, including the status of the state's non-economic damage cap policy. Similarly, given a two year lag, a suit that is closed in year *t*+2 will be explained by year t characteristics. In this way, this specification accounts for both a lag in policy effectiveness as well as a lag in suit duration. Table 2-11 shows the results from each of these three additional specifications.[33] These results use the same set of comparison states and instrument choice as column (2) of Table 2-10.

Compare the results in Table 2-11 to those in column (2) of Table 2-10. In all three cases, damage caps do not have a statistically significant effect on claim frequency. The coefficients on the remaining variables, however, lose precision when we introduce additional lags in suits.

Alternative Methodology

If caps are effective in reducing claim frequency, then the removal of a cap should increase claim frequency. Thus, an alternative method is to examine claim frequency following the removal of a cap due to unconstitutionality. In several states, previously enacted damage caps were held unconstitutional by the court system.[34] This approach exploits a purely exogenous change in the law. A change in the law, implemented by the court system, is not due to the

[33] When estimating models with lagged variables, the number of observations is typically declines. However, in this case, data from the National Practitioner Data Bank is also available for 2002-2004. Although these years were not used in the original analysis, due to data availability of other covariates, the number of suits per 100,000 of the population for 2002-2004 are used when considering lags in suit duration. As a result, the number of years of data is not decreased when estimating Equation (2). In addition, data for the covariates exists for 1990. When introducing lags in suits, I am able to add one year of data (1990), or 30 observations, to the original number of observations.

[34] See Table 2-5. Those states which are relevant for this portion of the analysis include Illinois, Ohio, and Oregon. The caps in both Illinois and Ohio were found unconstitutional at the end of 1997 and 1999, and are therefore coded as changing in 1998 and 2000, respectively.

underlying factors of the healthcare industry, but rather is based on some problem with the law's construction. It is safe to assume, therefore, that rulings of unconstitutionality are not endogenous in the same way that the implementation of caps may be endogenous.

As before, we compare two groups of states: states with non-economic damage caps and states whose caps were found unconstitutional during the relevant time period.[35] In this case, states that had caps in place throughout the time period comprise the comparison group. States with caps which were found unconstitutional during the relevant time period define the treatment group.[36] Since each state adopted non-economic damage caps in different years, the number of observations for each state differs. The total number of states in this section of the analysis is 23, and the number of observations is 218.

If caps are effective in reducing claim frequency, then removing a cap will increase claim frequency, all else equal. To test this hypothesis, the following equation was estimated:

$$M_{it} = \alpha_{it} + \delta\, S_{it} + \beta\, unconstitutional_{it} + \varphi_i + \theta_t + \varepsilon_{it} \qquad (3)$$

The coefficient on *unconstitutional* is the parameter of interest and is hypothesized to be positive. All other variables are as defined in Equation (1).

The results presented in column (1) of Table 2-12 suggest that caps are not effective at reducing suits. The coefficient is not precisely estimated, and we therefore can not identify an

[35] States whose caps were found unconstitutional during the relevant time period include Illinois, Ohio, and Oregon. The relevant comparison states include Alaska, California, Colorado, Hawaii, Idaho, Kansas, Louisiana, Maryland, Massachusetts, Michigan, Missouri, Montana, New Hampshire, New Mexico, North Dakota, South Dakota, Texas, Utah, and Wisconsin.

[36] Some of these caps were only in effect for several years, therefore, there is some question as to how many cases would actually have been affected by the caps during the time period. In Illinois, for example, it is more likely that the cap (effective from 1995-1997) had a larger impact on settlements than on suits that received judgments. This is because the cap applied to malpractice acts committed *after* the law's passage. Such suits would need to be resolved (by a judgment or a settlement) before the law was later relaxed.

effect. As tested in the previous section, there may be some time between when a suit is filed and when a suit is closed. To account for this, the analysis was conducted with the same three variations of lag times in suits. The results found in columns (2) – (4) of Table 2-12 are robust to these variations. We are unable to identify an effect of removing such a cap in any of the models. Using exogenous changes in the law does not provide any evidence that caps are effective at reducing claim frequency.

Robustness Checks and Additional Considerations

The lack of statistical significance with respect to caps on claim frequency is of crucial policy significance. Thus, I conducted several robustness checks using the original models estimated with Equation (1) in this section. All robustness checks provide similar evidence; no effect can be identified between non-economic damage caps and claim frequency.

Severe Damage Caps

For those states that have non-economic damage caps, the amount of the cap ranges from approximately $250,000 to $600,000 in nominal terms. Additionally, several states have overall damage cap reforms in place. Overall damage caps are limits on the total amount a plaintiff can recover at trial – economic and non-economic damages. Caps on overall (total) damages are *severe* in the sense that they limit economic and non-economic components. Initially, states which have caps on overall damages were removed from the dataset. These states, which altered their total damage caps during the relevant time period, include Indiana, Nebraska, and Virginia.[37] In this section, let *SevereCap* equal 1 if state *i* in year *t* has either a non-economic damage cap or an overall damage cap in place, and 0 otherwise. Additionally, consider overall damage caps in conjunction with *severe* non-economic damage caps only (less than $500,000).

[37] Indiana changed their cap in 1993, Nebraska changed their cap in 1992, and Virginia changed their cap in 1999.

Let this variable be *SevereCap2*. Using the same instrumental variable procedure described previously, Equation (1) is re-estimated using these two alternatives of the reform variable. The results are consistent with those presented in Table 2-10. We are unable to identify an effect of damage caps on claim frequency. These results are contained in Table 2-13.

Additional Considerations

One may be concerned that these results are affected by serial correlation. Each observation does not contain entirely new information about each state-level observation. Bertrand, Duflo, and Mullainathan (2004) discuss the problems of using differences-in-differences (DD) in the presence of serial correlation. Serial correlation can produce standards errors that are *too small*, meaning that we would be more likely to find an effect of a treatment than not. If indeed serial correlation is a problem in this analysis, it would imply that the standard errors are even larger than in the previous analysis. It is, therefore, even more likely that there is no effect of non-economic damage caps in this context.

Conclusions

It is undeniable that the health care industry is currently facing problems that deserve careful consideration and thoughtful solutions. Among the myriad concerns is the frequency of medical malpractice claims. A commonly-offered solution is the use of non-economic damage caps in medical malpractice litigation as part of a tort reform effort. Over the past 30 years, proponents of tort reform have had some success in persuading state legislatures. Thirty states have enacted and/or modified tort reforms by incorporating damage caps in an attempt to address health care concerns. Due to the fact that these reforms are widespread at the state level and that non-economic damage caps are being proposed at the national level, it is essential that policy makers understand the true effects of such reforms.

To analyze this issue, I utilize two different approaches. First, I treat caps as endogenous using a unique instrument of political control. This novel approach should be of value to studies that consider the impact of public policies. The literature has used contemporaneous political measures as instrumental variables for the enactment of public policies. My paper, however, recognizes that laws are sticky and that the probability that a law is in place hinges upon probabilities that the law was enacted in prior years. I calculate cumulative probabilities using predicted probabilities of enacting a cap obtained from a logit model. Using the cumulative probabilities as an instrument for the enactment of a damage cap yields strong first stage results. In the 2SLS estimates, I find no evidence to suggest that non-economic damage caps are effective in reducing medical malpractice claim frequency. This finding is robust to a variety of additional specifications, including different instruments and alternative sets of comparison groups. Second, I exploit exogenous changes in the law, when state courts find damage cap legislation to be unconstitutional. Again, I can identify no effect between caps and claim frequency. Since caps are ostensibly intended to reduce claim frequency, this particular tort reform strategy may be misguided.

Table 2-1. Summary statistics of variables*

Variable	Mean	Standard Deviation	Median	Minimum	Maximum
Ln(Suits)	1.70	0.42	1.69	0.53	2.69
Suitsper100Capita	6.02	2.67	5.40	1.70	14.8
IncomePerCapita	17,857	2,572	17,610	12,525	26,375
Metro	64.05	22.89	67.83	23.50	100.00
PersonalHealthExpend	12.13	2.00	12.31	6.16	16.64
Unempl	5.17	1.44	5.08	2.24	9.23
Cap	0.142	0.350	0	0	1

*This table contains observations for all states which enacted non-economic damage caps during the relevant time period and states which never had non-economic damage caps. These statistics include 30 states over 11 years (330 observations).

Table 2-2: Data sources

Data	Source Information
Disposable Income per Capita (real)	Bureau of Economic Analysis (BEA), www.bea.gov
Consumer Price Index	The Economic Report of the President, 2004
Percent of Population Living in Metropolitan Areas	Statistical Abstract of the United States, National Data Book (Note: These data are reported every other year and required interpolation for the remaining years)
Personal Health Care Expenditures (as a percent of GSP)	Centers for Medicare and Medicaid Services, www.cms.hhs.gov
Unemployment Rate	Bureau of Labor Statistics (BLS), www.bls.gov
Population	Census Bureau, www.census.gov
Non-economic Damage Caps	No one source provides accurate data on the status, date of enactment, date of change, or amount of caps. A number of sources were used. These include (but are not limited to) the following: (1) American Tort Reform Association (ATRA), Medical Liability Reform, http://www.atra.org/show/7338 (2) ATRA Tort Reform Record, December 31, 2003. www.atra.org (3) Center for Justice and Democracy, www.centerjd.org (4) National Conference of State Legislatures, State Medical Liability Laws Table, http://www.ncsl.org/programs/insur/medliability.pdf (5) McCullough, Campbell & Lane, Summary of Medical Malpractice Law, www.mcandl.com
Political Control Data	(1) Book of the States (2) Statistical Abstract of the United States, National Data Book
Term Limits	Book of the States, 2002
Medical School Rankings	U.S. News & World Reports, *America's Best Graduate Schools 2006*, "Top Medical Schools – Primary Care" and "Top Medical Schools – Research

Table 2-3. States enacting non-economic damage reforms (1991-2001)

State	Year	Nominal Amount ($)
Alaska	1997	$400,000
Illinois	1995	$500,000
Montana	1995	$250,000
New Mexico	1992	$600,000
North Dakota	1995	$500,000
Ohio	1997	$250,000
South Dakota	1997	$500,000
Wisconsin	1995	$350,000

Some state courts found non-economic damage caps unconstitutional. Illinois held the cap unconstitutional in 1997 while Ohio held the cap unconstitutional in 1999. See *Best v. Taylor Mach. Works*, 689 NE 2d. 1057 (1997) and *State ex rel. Ohio Academy of Trial Lawyers v. Sheward*, 86 Ohio St. 3d 451 (1999), respectively.

Table 2-4. State limits on damages

State	Year	Amount	Description
Alabama	1987	$400,000	Non-economic damage cap; Held unconstitutional in 1991.
Alaska	1997	$400,000	Non-economic damage cap.
Arizona	--	--	--
Arkansas	--	--	--
California	1975	$250,000	Non-economic damage cap.
Colorado	1988	$250,000	Non-economic damage cap.
Connecticut	--	--	--
Delaware	--	--	--
Florida	2003	$500,000	Non-economic damage cap for single practitioner (($1 million for multiple practitioners).
Georgia	--	--	--
Hawaii	1986	$375,000	Non-economic damage cap.
Idaho	1987	$400,000	Non-economic damage cap.
	2003	$250,000	Non-economic damage cap.
Indiana	1993	$1.25 mil	Overall (total) damage cap.
Illinois	1995	$500,000	Non-economic damage cap; Held unconstitutional in 1997.
Iowa	--	--	--
Kansas	1988	$250,000	Non-economic damage cap.
Kentucky	--	--	--
Louisiana	1975	$500,000	Cap on all damages, exclusive of future medical expenses and related benefits.
Maine	--	--	--
Maryland	1986	$350,000	Non-economic damage cap.
	1994	$500,000	Increased by $15,000 every year thereafter.
Massachusetts	1986	$500,000	Non-economic damage cap.
Michigan*	1986	$225,000	Non-economic damage cap.
	1993	$280,000	Non-economic damage cap (or $500,000 under extreme circumstances).
Minnesota	1986	$400,000	Non-economic damage cap, but does not apply to pain & suffering damages.
Mississippi	2003	$500,000	Non-economic damage cap.
Missouri*	1986	$350,000	Non-economic damage cap.
Montana	1995	$250,000	Non-economic damage cap.
Nebraska	1992	$1.75 mil	Overall (total) damage cap.
Nevada	2002	$350,000	Non-economic damage cap.
New Hampshire	1986	$875,000	Non-economic damage cap; held unconstitutional in 1991.
New Jersey	--	--	--

Table 2-4. State limits on damages (continued)

State	Year	Amount	Description
New Mexico	1992	$600,000	Total damage cap, but does not include punitive or future medical expenses and related benefits.
New York	--	--	--
North Carolina	--	--	--
North Dakota	1995	$500,000	Non-economic damage cap.
	2003	$350,000	Non-economic damage cap.
Ohio	1997	$250,000	Non-economic damage cap (or $500,000 under extreme circumstances); held unconstitutional in 1999.
Oklahoma	2003	$350,000	Non-economic damage cap (applies to pregnancy and emergency care only).
Oregon	1987	$500,000	Non-economic damage cap; held unconstitutional in 1999.
Pennsylvania	--	--	--
Rhode Island	--	--	--
South Carolina	--	--	--
South Dakota	1997	$500,000	Non-economic damage cap.
Tennessee	--	--	--
Texas	1977	$500,000	Non-economic damage cap; applies only to wrongful death actions.
Utah*	1986	$250,000	Non-economic damage cap.
	2001	$400,000	Non-economic damage cap.
Vermont	--	--	--
Virginia	1999	$1.5 mil	Overall (total) damage cap.
Washington	--	--	--
West Virginia	1986	$1 mil	Non-economic damage cap.
Wisconsin*	1995	$350,000	Non-economic damage cap.
Wyoming	--	--	--

*Limits on damages are adjusted for inflation.

Table 2-5. Non-economic damage caps held unconstitutional

State	Year Enacted	Year Unconstitutional	Amount
Alabama	1987	1991	$400,000
Illinois	1995	1997	$500,000
Ohio*	1997	1999	$500,000 ($250,000 if less severe)
New Hampshire	1976, 1986	1980,1991	$875,000
Minnesota	1986	1990	$400,000
Oregon	1987	1999	$500,000
Washington	1986	1989	43% wage *life expectancy

*Ohio re-enacted a cap in 2003, however, this does not apply to the data used in this analysis.

Table 2-6. Description of categories of states

Set	No.	Criteria	States
1	6	States which enacted non-economic damage caps and they were not found unconstitutional.	AK, MT, NM, ND,SD, WI
2	2	States which enacted non-economic damage caps between 1991-2001, but whose caps were found unconstitutional between 1991-2001.	IL,OH
3	22	State which never had damage caps during the relevant time period.	AR, AK, CT, DE, FL, GA, IA, KY, ME, MN, MS, NV, NJ, NY, NC, OK, PA, RI, SC, TN, VT, WY
4	3	States which had non-economic damage caps in place prior to 1991, where these caps were found unconstitutional before/during 1991.	AL, NH, WA
5	13	States which had non-economic damage caps in place prior to 1991, where these caps are still in effect.	CA, CO, HI, ID, KS, LA, MD, MA, MI, MO, TX, UT, WV
6	1	States which had non-economic damage caps in place prior to 1991, where the cap was found unconstitutional between 1991-2001.	OR
7	3	States with caps on total damages.	IN, NE, VA

Table 2-7. Baseline statistics (1991)

	States with Caps (8 states)		States without Caps (22 states)	
Variable	Average	Standard Deviation	Average	Standard Deviation
Ln(Suits)	1.84	0.42	1.68	0.43
Suitsper100	6.8	2.54	5.90	2.70
Metro	54.08	221.6	67.13	23.5
IncomePerCapita	16,408	2,252	16,870	2,620
Unempl	6.25	1.71	6.58	1.10
PersonalHealthExpend	11.63	2.64	11.66	1.96

Table 2-8. Change in suits between year t-2 and t-1; t-1 and t=0

State	States with Caps Percent Change from *t*-2 to *t*-1	States without Caps Percent Change from *t*-2 to *t*-1	Difference
Alaska	70.26%	4.76%	65.49%
Illinois	-14.05%	3.58%	-17.63%
Montana	14.68%	3.58%	11.10%
New Mexico			
North Dakota	11.87%	2.08%	9.80%
Ohio	1.98%	4.76%	-2.79%
South Dakota	-3.19%	4.76%	-7.96%
Wisconsin	-11.63%	3.58%	-15.20%

State	States with Caps Percent Change from *t*-1 to *t*=0	States without Caps Percent Change from *t*-1 to *t*=0	Difference
Alaska	-44.84%	-1.49%	-43.35%
Illinois	-18.36%	-11.72%	-6.64%
Montana	-23.72%	-11.72%	-12.00%
New Mexico	-5.02%	6.16%	-11.18%
North Dakota	-28.12%	-11.72%	-16.39%
Ohio	-8.70%	-1.49%	-7.21%
South Dakota	-0.27%	-1.49%	1.22%
Wisconsin	12.30%	-11.72%	24.03%

Table 2-9. First stage results

	(1) IV=*RepCurrent*	(2) IV=*RepEver*	(3) IV=*Cumulative Probabilities*
Variable Name	Cap	Cap	Cap
IncomePerCapita	-0.00006	-0.00002	-0.000089
	[0.00004]	[0.00004]	(0.000036)**
Metro	0.0192	0.019	-0.0254
	[0.0136]	[0.0128]	(0.0132)*
Unempl	0.0586	0.0596	0.0286
	[0.0236]**	[0.0221]***	-0.0214
PersonalHealthExpend	0.0356	0.0506	0.0301
	[0.0242]	[0.0229]**	-0.0217
Instrument	0.1133	0.3341	1.4359
	[0.0485]**	[0.0507]***	(0.1637)***
Observations	324	324	324
Number of States	30	30	30
R-squared	0.67	0.71	0.74
State Fixed Effects	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
First Stage Statistics			
F-statistic	5.46	43.50	76.98
Partial R-squared	0.0192	0.1349	0.2162

Standard errors in parentheses
significant at 10%; ** significant at 5%; *** significant at 1%

First stage results were obtained using ordinary least squares (OLS) estimation.

Table 2-10. Ordinary least squares (OLS) and two-stage least squares (2SLS) results

	(1) OLS	(2) 2SLS IV=*Cumulative Probabilities*	(3) 2SLS IV=*Cumulative Probabilities*	(4) 2SLS IV=*Cumulative Probabilities*	(5) 2SLS IV=*Cumulative Probabilities*
Variable Name	Ln(Suits)	Ln(Suits)	Ln(Suits)	Ln(Suits)	Ln(Suits)
IncomePerCapita	-0.000081	-0.00007	-0.00007	-0.00007	-0.00007
	[0.000034]**	[0.00004]*	[0.00003]**	[0.00004]*	[0.000033]**
Metro	0.0166	0.0104	0.0112	0.0102	0.0111
	[0.0117]	[0.0115]	[0.0113]	[0.0115]	[0.0112]
Unempl	-0.0281	-0.0315	-0.0314	-0.0329	-0.0325
	[0.0203]	[0.0236]	[0.0212]	[0.0236]	[0.0212]
PersonalHealthExpend	0.0586	0.052	0.0446	0.0545	0.0474
	[0.0208]***	[0.0200]***	[0.0196]**	[0.0202]***	[0.0197]**
Cap	-0.2106	-0.0366	-0.0538	-0.0248	-0.0453
	[0.0506]***	[0.1096]	[0.1064]	[0.1134]	[0.1104]
Observations	324	324	357	330	363
Number of States	30	30	33	30	33
R-squared	0.83	0.85	0.86	0.84	0.86
State Fixed Effects	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
First Stage Statistics					
F-statistic		76.98	85.45	65.27	71.82
Partial R-squared		0.2162	0.2166	0.1863	0.1857
Confidence Intervals					
Cap	(-0.2777, -0.0890)	(-0.2523, 0.1792)	(-0.2631, 0.1555)	(-0.2479, 0.1984)	(-0.2625, 0.1719)

Robust standard errors in parentheses
*significant at 10%; ** significant at 5%; *** significant at 1%

Columns (1) and (2) present the results of Equation (1) omitting states (20) which had caps before 1991 (and still have caps today), states which had caps before 1991 which were found unconstitutional, and states which have caps on total damages. The years after the Ohio and Illinois caps were found unconstitutional are also omitted.
Column (3) omits the same states as in column (2), but includes observations for Alabama, New Hampshire, and Washington. These are states whose caps were found unconstitutional before or during 1991, and therefore do not have caps during the relevant time period.
Column (4) contains the same set of states as columns (1) and (2), but does not omit the years following the findings of unconstitutionality for Ohio and Illinois.
Column (5) contains the same set of states as column (3), but does not omit the years following the findings of unconstitutionality for Ohio and Illinois.

Table 2-11. 2SLS results - variants of duration*

Variable Name	(1) One Year Lag Ln(Suits)	(2) Two Year Lag Ln(Suits)	(3) Three Year Lag Ln(Suits)
IncomePerCapita	-0.00004	-0.00002	-0.00004
	[0.00004]	[0.00003]	[0.00003]
Metro	0.0144	0.0035	-0.0094
	[0.0097]	[0.0083]	[0.0102]
Unempl	-0.0063	0.0149	-0.0009
	[0.0205]	[0.0198]	[0.0179]
PersonalHealthExpend	0.035	0.0027	-0.0154
	[0.0197]*	[0.0185]	[0.0181]
Cap	-0.0279	0.0276	-0.0375
	[0.0990]	[0.0991]	[0.0927]
Observations	354	354	354
Number of States	30	30	30
R-squared	0.83	0.83	0.83
State Fixed Effects	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
First Stage Statistics			
F-statistic	76.44	76.44	76.44
Partial R-squared	0.1988	0.1988	0.1988

Robust standard errors in parentheses
significant at 10%; ** significant at 5%; *** significant at 1%

*The control states in this table correspond to the initial specification of control states used in column (2) of Table 6.

Table 2-12. OLS results using unconstitutionality of caps*

Variable Name	(1) No Lag Ln(Suits)	(2) One Year Lag Ln(Suits)	(3) Two Year Lag Ln(Suits)	(4) Three Year Lag Ln(Suits)
IncomePerCapita	-0.00014	-0.00006	0.00002	0.00005
	(0.000029)***			
	*	(0.000035)*	-0.000038	-0.000033
Metro	0.0147	0.0119	0.0043	-0.0096
	(0.0089)	(0.0080)	(0.0069)	(0.0107)
Unempl	-0.0187	0.0155	0.0118	0.0338
	(0.0218)	(0.0181)	(0.0145)	(0.0154)**
PersonalHealthExpend	0.0662	0.0292	0.0883	0.0789
	(0.0446)	(0.0328)	(0.0340)**	(0.0323)**
Unconstitutional	-0.0319	-0.0511	-0.0641	0.0131
	(0.0649)	(0.0646)	(0.0709)	(0.0660)
Observations	218	221	232	232
R-squared	0.82	0.83	0.83	0.83
State Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes

Robust standard errors in parentheses
significant at 10%; ** significant at 5%; *** significant at 1%

Table 2-13. Results for severe cap.

Variable Name	(1) 2SLS Ln(Suits) SevereCap	(2) 2SLS Ln(Suits) SevereCap2
RealPCdispos	-0.00006	-0.00007
	(0.00004)*	(0.00004)*
Metro	0.0148	0.0144
	(0.0114)	(0.0110)
Unempl	-0.038	-0.0398
	(0.0214)*	(0.0210)*
PersonalHealthExpend	0.063	0.0631
	(0.0201)***	(0.0201)***
Cap	-0.0482	-0.0919
	(0.1072)	(0.2013)
Observations	346	346
Number of States	32	32
R-squared	0.85	0.85
State Fixed Effects	Yes	Yes
Year Fixed Effects	Yes	Yes
First Stage Statistics		
F statistic	76.75	27.84
Partial R-squared	0.2043	0.0852

Robust standard errors in parentheses
significant at 10%; ** significant at 5%; *** significant at 1%

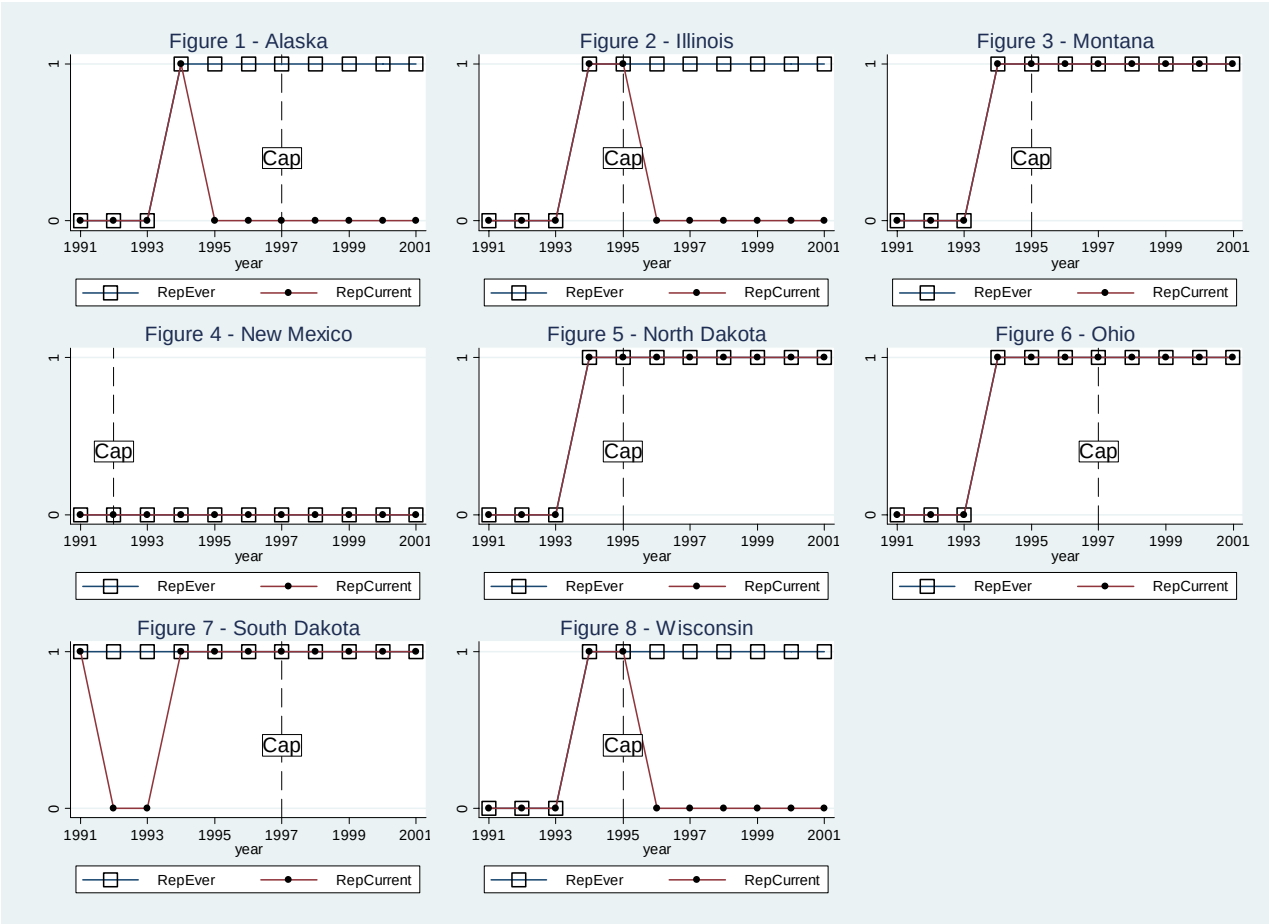

Figure 2-1. Description of enactment of cap and change in political composition

CHAPTER 3
THE EFFECTS OF INCREASED ACCESS TO THE MORNNG-AFTER PILL ON ABORTION AND STD RATES

Introduction

Over six million pregnancies occur each year in the United States: three million are unintended and over one million are terminated by abortion.[38] Some of these unintended pregnancies could be prevented and the corresponding abortions avoided with easier access to emergency contraception. Emergency contraception, also known as the morning-after pill or Plan B, is a type of birth control that can be taken up to 72 hours after sexual activity that can prevent a pregnancy from occurring. On August 24, 2006, the FDA approved sales of emergency contraception through pharmacists without a prescription for individuals age 18 and older.[39] This approval follows a previous rejection by the FDA of a proposal that would have allowed emergency contraception to be available over-the-counter without an age restriction.[40] In its earlier rejection, the FDA pointed to potential misuse by teenage girls, who would be able to purchase the product without a doctor's supervision. The recent decision restricts access to women over the age of 18, thereby alleviating this particular concern. Those who oppose easier access raised concerns that over-the-counter access could lead to increased sexual activity.[41]

Pharmacy access to emergency contraception was an approach first adopted in the U.S. by the State of Washington. In 1997, Washington began a pilot program to expand access to emergency contraception through pharmacies. Enabling pharmacy provision dramatically changes the accessibility of emergency contraception. Pharmacy access facilitates faster

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[38] *Contraception Counts*, March 2006, Alan Guttmacher Institute, www.guttmacher.org.

[39] Harris (August 25, 2006).

[40] CBS News, May 6, 2004.

[41] CBS News, June 11, 2006.

provision of the medication because there are no appointment delays with a doctor. Moreover, it provides evening and weekend access. Since 1997, eight states have followed Washington's lead, adopting similar initiatives allowing pharmacists to dispense emergency contraception without a prescription.[42] Eight additional state legislatures have introduced similar legislation, but failed to pass it, while two other bills are still under consideration.[43] By studying the effects of the program in Washington, we can gain an understanding of how pharmacy access may affect the rest of the country in the future. Focusing on Washington, the first state to change access, utilizes the most post-implementation years. Additionally, although eight other states have since implemented similar initiatives, many of these laws were passed quite recently.

Increased accessibility to emergency contraception reduces the expected costs of engaging in sexual activity. If a pregnancy is possible, whether due to contraceptive failure or unsafe sexual activity, use of emergency contraception can prevent an unwanted pregnancy. Additionally, emergency contraception may be more ethically appealing in that it works like oral contraceptives to prevent a pregnancy from occurring rather than terminating an existing pregnancy. If emergency contraception is used as a substitute for a subsequent abortion, then abortion rates could decline. If individuals recognize that the costs associated with engaging in risky sexual behavior are lower, however, then these individuals may enjoy greater risk taking. If indeed increased access to emergency contraception increases the amount of sexual behavior, it is possible that sexually transmitted disease (STD) rates will also increase as a result.

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[42] These states include Alaska, California, Hawaii, Maine, Massachusetts, New Mexico, New Hampshire, and Vermont.

[43] Proposals failed in Colorado, Kentucky, Illinois, Maryland, Oregon, Texas, Virginia, and West Virginia. Proposals are still in progress in New Jersey and New York, although similar proposals in these states have previously failed to pass (http://www.go2ec.org/Legislation.htm).

In my paper, I consider the intended and unintended consequences of increased access to emergency contraception. Using county-level data on Chlamydia rates and abortion rates as well as dates of pharmacy participation, I estimate the treatment effect, if any, of pharmacy access to emergency contraception on several outcomes. The results indicate that pharmacy access is associated with an increase in Chlamydia rates, both overall and for females, and is associated with a decrease in abortion rates for some age groups. This result is robust to the use of an alternative comparison group as well as alternative definitions of treatment.

My paper contributes to the existing literature by exploiting a difference-in-difference methodology to consider the impact of pharmacy access to emergency contraception. Although difference-in-difference estimates could suffer from selection bias due to individual pharmacist or pharmacy participation decisions, I show that the treatment and control groups are statistically indistinguishable in terms of Chlamydia rates. Although the treatment and control groups are statistically distinguishable in terms of abortion rates, selection would bias the estimates upwards, i.e., in favor of finding no effect. My estimates, therefore, are conservative estimates of the relationship between pharmacy access and abortion rates.

Previous Literature

Economic models generally assume that individuals respond to economic and policy-related factors. Economic models related to risky behavior are no exception. Empirical evidence suggests, however, that this is often, but not always the case. In contrast, non-economic models of risky behavior often assert that individuals, and teens especially, make decisions in a more spontaneous or random fashion. This section provides an overview of the economic literature on risky sexual behavior and its potential consequences, as well as reviews some related medical studies.

An extensive literature exists on the effects of various public policies on risky behavior and the potential consequences. There is much less literature, however, on risky sexual behavior. For present purposes, the most relevant literature involves those papers which focus on the increased accessibility and availability of family planning services or emergency contraception on outcomes such as pregnancy, abortion, and STDs. Paton (2002) considers the impact of the increased provision of family planning services in England on underage conceptions and abortions. He finds no evidence that increased attendance at family planning clinics reduces teen pregnancy or abortion. Additionally, he considers the impact of a court ruling, which for approximately one year barred family planning services from being offered to women under the age of 16 without parental consent.[44] A reduction in the accessibility to family planning services should affect this age group differently than those aged 16-19 (who were unaffected by the ruling). Paton, however, found no evidence that these two groups experienced different conception or abortion rates. In another study, Paton (2006) finds no effect of the provision of emergency contraception at family planning clinics on abortion rates. The author, however, identifies a positive effect on STD rates. Finally, Girma and Paton (2006) find no effect of free access to over-the-counter emergency contraception on teen pregnancy rates using a matching estimator approach. All three of these papers use regional data from England.

Several medical papers exploit randomized control trials to examine the impact of emergency contraception or family planning services on a variety of outcome measures. The initial sample selection of women, however, may be problematic in these studies. In Raine et al, (2005), for example, a randomized control trial was administered in California between July 2001 and June 2003. Their initial sample, however, included women aged 15-24, who had been

[44] The ruling was the Gillick ruling in December 1984; it was later overturned in 1985.

sexually active in the last 6 months, who could participate in a follow-up visit 6 months later, and who were already attending the family planning clinic. Women were randomized into three groups of emergency contraception access: (1) advanced provision, (2) pharmacy access, and (3) clinic access. The administrators of the trial, however, eliminated Group (3) in December 2001 because the California legislature passed pharmacy access legislation. The majority of the study, therefore, compared Group (1) to Group (2). Given the high level of access in both groups, we may not expect to find a difference in abortion or STD incidence between Groups (1) and (2). Utilizing group (3) gives a baseline of traditional access for comparison purposes. The study found no evidence that Groups (1) and (2) were different in terms of pregnancy or STD rates. Several other studies have used similar randomization procedures, but fail to find any difference in abortion, pregnancy, or STD rates. The evidence, however, is consistent in showing that advanced provision of emergency contraception does increase its use. [45]

A somewhat separate literature examines the effects of various policies on state-level STD rates. Such policies affect the costs and benefits of engaging in risky behavior and may in turn affect sexual outcomes. Sen (2003a) and similarly Sen (2003b) consider the effect of restrictions on Medicaid funding for abortions on risky sexual behavior, measured by state-level gonorrhea rates.[46] Increased restrictions on Medicaid funding for abortions increase the price of abortions to individuals who would otherwise rely on Medicaid payment. If the price of an abortion increases, we would expect individuals to engage in less risky sexual behavior because an unintended pregnancy would be more costly. This should lead to a reduction in sexual activity, which could

[45] Glasier and Baird (1998), Falk et al (2001), and Glasier et al (2004).

[46] Several studies have considered state-level Medicaid funding restrictions, but have focused on abortions, pregnancies, and births as the outcome measures. The results are these papers are consistent; increases in the price of abortion decreases the demand for abortion. See Blank, George, and London (1996), Hass-Wilson (1996), and Levine, Trainor, and Zimmerman (1996).

be reflected in lower STD rates. In both papers, using slightly different panel data techniques, Sen is unable to identify an effect of Medicaid funding restrictions on gonorrhea rates.

In contrast to Sen's restriction in abortion access, Klick and Stratmann (2003) analyze the exogenous change of abortion legalization on risky behavior measured by state-level gonorrhea and syphilis rates. If abortion lowers the cost of engaging in sexual activity by providing insurance in the event of a pregnancy, then the legalization of abortion could increase risky behavior and therefore STD rates. Klick and Stratmann find that STD rates increased as a result of abortion legalization, confirming this hypothesis.

Many researchers have studied the relationship between alcohol or substance use and sexual behavior and its potential consequences.[47] Although previous literature has positively linked alcohol consumption to sexual behavior,[48] the identification strategies used are questionable because substance abuse and sexual decisions are dependent upon a common set of unobservable personal factors.[49] Other studies consider public policies targeted at alcohol or drugs. Restrictive alcohol policies, such as higher taxes on alcohol or stricter drunk driving laws, have the potential to reduce alcohol consumption which could in turn reduce risky sexual

[47] See, for example, Sen (2003) who studies the effect of beer taxes on teen abortion rates, finding a small but statistically significant negative effect on abortion rates.

[48] See, for example, Graves & Leigh (1995).

[49] Two specific studies attempt to correct for this omitted variable problem by employing instrumental variables approaches. Rees et al (2001) find that the link between substance use and sexual activity is weaker than previously suggested in the literature. In contrast, Sen (2002) finds that substance use increases the probability of engaging in sexual activity. Rashad and Kaestner (2004), however, discuss the pitfalls of these identification strategies, suggesting that the relationship between substance abuse and sexual behavior is still uncertain. As with most instrumental variables approaches, the success of the identification strategy lies heavily in the exogeneity and correlation of the instruments; these two criteria are questioned in both papers.

behavior.[50] Chesson et al (2000) use state-level panel data to consider the impact of liquor and beer taxes on STD rates. The authors find that an increase in either tax is associated with a reduction in gonorrhea and syphilis rates. Grossman et al (2004) consider the impact of alcohol taxes as well as drunk driving laws on the incidence of gonorrhea.[51] Similar to Chesson et al, the authors find that more restrictive alcohol policies have a negative effect on gonorrhea rates, but this result is only statistically significant for males. In a similar paper, Carpenter (2005) examines the impact of Zero Tolerance Laws on state-level gonorrhea rates. He finds that the adoption of a Zero Tolerance policy has a negative effect on gonorrhea rates, but this effect is only significant for males between the ages of 15 and 19.

My paper joins a small literature focusing on the effects of emergency contraception and other family planning services on abortion rates and STD rates. The main papers in the economic literature discussed previously (Paton (2002), Paton (2006), and Girma and Paton (2006)) present findings from England. However, England experiences much lower rates of teen pregnancy, abortion, and sexually-transmitted disease than the United States.[52] In the economic literature, this paper is the first to consider the American experience of pharmacy access to emergency contraception. I utilize a difference-in-difference approach by exploiting the similarities between the treatment and control groups before the program began. Using the differences across

[50] More restrictive alcohol policies could also reduce abortions through a decrease in risky behavior. Sen (2003) finds evidence that higher beer taxes are associated with small but statistically significant reductions in teen abortion rates.

[51] Chesson et al use a panel data framework with a lagged dependent variable and fixed effects. Estimation in this way is inconsistent because of the endogeneity of the lagged dependent variable. Grossman et al improve on this specification by accounting for the endogeneity and implementing FD2SLS with subsequent lagged dependent variables as instruments.

[52] Darroch, Singh, & Frost (2001).

participating and nonparticipating counties as well as timing of participation, I estimate a treatment effect of pharmacy access on several outcome measures.

The Relative Costs of Sexual Activity

We generally assume that individuals behave rationally, taking account of the costs and benefits of engaging in a particular behavior. There is some debate as to whether individuals, especially teens, make sexual decisions rationally or make such decisions in a more random fashion.[53] Levine (2000) finds evidence suggesting that individuals behave rationally with respect to sexual decisions, that is, they respond to incentives or to changes in costs and benefits. His results indicate that individuals respond to specific changes in costs and benefits, such as changes in labor market conditions, abortion access, welfare benefits, and AIDS prevalence.

In particular, my paper focuses on individual responses to changes in costs. If the cost of an activity (sexual behavior, for example) increases, we would expect the associated behavior to decrease simply because the costs are higher and may not outweigh the benefits for some individuals. If, however, individuals make sexual decisions in a random fashion, i.e., fail to weigh the costs and benefits of a particular decision, then changes in the costs of sexual activity or its potential consequences may have no effect on sexual behavior.

Increased access and awareness of emergency contraception represents a decrease in the cost of engaging in sexual behavior. Emergency contraception decreases the potential costs of engaging in sexual activity, because it can eliminate a potential pregnancy before it actually occurs. This may forgo possible moral dilemmas that arise with abortion decisions. We would expect, therefore, that changes in the costs associated with engaging in risky sexual behavior affect the amount of sexual activity. If the costs associated with engaging in sexual activity, or

[53] Paton (2006).

the costs associated with a potential pregnancy, decrease, then we would expect the amount of this behavior to increase. This could be reflected in an increase in the rate of sexually transmitted diseases.

Pharmacy Access to Emergency Contraception

History of Emergency Contraception

In 1997, the FDA approved the use of certain oral contraceptive pills for use as emergency contraception, although they had been used on an off-label basis for years. As demonstrated by Albert Yuzpe in 1974, use of oral contraceptives in specific dosages after a sexual encounter can be used as a safe and effective method of preventing pregnancy. This method, known as the Yuzpe method, is the basis for today's morning-after pill. Preven, the first emergency contraception on the market, was approved by the FDA in 1998 and is modeled after the Yuzpe regimen. Two tablets are taken initially and then followed by two additional tablets 12 hours later. Plan B, a progestin-only product, was approved in 1999 and is now the only emergency contraception on the market. [54]

The Washington State Pilot Project

Pharmacists in Washington State have been able to form collaborative agreements with physicians since 1979.[55] A collaborative agreement grants a pharmacist the ability to dispense a prescription medication, within a specified protocol, without a physician's prescription. Outlined in the agreement is the particular medication, the criteria for who is eligible to receive the medication, and the process of review regarding pharmacist decisions by the prescriber.

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[54] Preven was discontinued by its manufacturer, Barr Laboratories, in 2004.

[55] RCW 18.64.011. Originally, collaborative agreements were filed according to individual pharmacies, with a primary responsible pharmacist named on the agreement. After the fall of 2003, the Board of Pharmacy began recording collaborative agreements by individual pharmacist.

Collaborative agreements have been used successfully in Washington with other medications, and are also common in other states.[56] Currently, eight other states have similar initiatives.

A Washington State report estimated that 53 percent of pregnancies in Washington in 1997 were unintended.[57] At that time, emergency contraception was available only through a physician. Not only were many women unfamiliar with emergency contraception, but health care professionals rarely discussed emergency contraception with their patients. As a result of these concerns, the Emergency Contraception Collaborative Agreement Pilot Project was initiated in July of 1997. The program was the first of its kind to enable pharmacists to directly dispense emergency contraception without a prescription. This is, however, distinctly different from an over-the-counter designation. The main goal of the program was to reduce unintended pregnancies in Washington through increased access to and awareness of emergency contraception.[58] Participants included the Washington Board of Pharmacy, Washington State Pharmacy Association, University of Washington Department of Pharmacy, and an organization called PATH (Program for Appropriate Technology in Health). Funding was provided by the David and Lucile Packard Foundation, while media coverage was handled by Eglin DDB Seattle.

The planning phase of the program began in July of 1997, while the majority of the program activities occurred during the 16-month period between February 1998 and June 1999. The pilot program encouraged Washington pharmacists to form collaborative agreements with respect to emergency contraception. Pharmacies continued to file for access well after the official

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[56] Collaborative agreements have been used with respect to other medications such as immunizations, asthma therapy, diabetes screening, cholesterol screening, and chronic disease management (Gardner et al).

[57] County Profiles, Birth and Unintended Pregnancy Statistics: February 2001, Washington State Department of Social and Health Services.

[58] Gardner et al. (2001).

program ended. Once a pharmacist forms such an agreement with a physician, the agreement is submitted to and approved by the State Board of Pharmacy.[59] Agreements were initially valid until 2001 and then required a renewal every two years thereafter.[60] Information about emergency contraception was sent to Washington state pharmacists, including a list of willing physician and nurse practitioner prescribers and a template collaborative agreement.[61] In order to file a collaborative agreement with the Board of Pharmacy, each pharmacist must first participate in a training session. These sessions included training in not only patient care and appropriate provision of the medication, but also providing referral information, talking with parents if necessary, and counseling on future contraceptive decisions.

To be effective, emergency contraception must be taken within 72 hours of sexual activity, and even then, is most effective if taken within the first 24 hours.[62] If taken within 72 hours, emergency contraception can reduce the chance of pregnancy by 89 percent.[63] Prior to dispensing emergency contraception, the pharmacist performs a brief consultation with the patient to rule out potential existing pregnancy.[64] If necessary, the patient is referred to a primary care physician or other health care professional.

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[59] The agreements were initially made between pharmacy and physician, rather than between pharmacist and physician. This recently changed and now the agreements are between pharmacist and physician.

[60] Gardner et al (2001).

[61] Downing (2004).

[62] CBS News. November 24, 2003.

[63] CBS News, November 24, 2003.

[64] Pharmacists were reimbursed for consultation time, approximately $13.50 per counseling session. http://www.go2ec.org/ProfileWashington.htm.

Pharmacies are convenient – they are open evenings, weekends, and holidays. No appointment is required and a patient does not have to see her primary physician. This means no delay due to scheduling an appointment or fear of discussing such matters with your primary physician. Washington State law does not prohibit the provision of contraceptive or family planning services to minors. No parental consent is required. The program, therefore, improved emergency contraception access to women of all ages. The patient cost of emergency contraception is between $30 and $40.

Additionally, the pilot program involved a mass consumer awareness campaign using extensive media coverage. Public service announcements, mainly in the form of TV and radio advertisements, occurred between July 1997 and March 1998. Additional paid TV, radio, and newspaper advertisements were publicized between July and August of 1998.[65] Both the existence of emergency contraception and the recent accessibility changes were heavily publicized in these advertisements. While the campaign targeted females aged 18-34, participants believed that they were reaching younger females as well. News of Washington's program was also recognized by local and national print and TV news stories; some 120 stories appeared.[66] This campaign promoted the use of a new national hotline that allows women to call and locate their nearest provider of emergency contraception. 1-888-NOT-2-LATE provides information regarding both pharmacies and clinics where emergency contraception is available.[67]

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[65] Trussell (2001).

[66] Gardner (2001).

[67] A website maintained by Princeton was also established containing the same information: www.Not-2-Late.com.

Data

Data on Chlamydia rates and abortion rates were obtained from the State of Washington. These data are described in this section and summary statistics are provided in Table 3-1.

Chlamydia

The State of Washington collects detailed data on sexually transmitted diseases by date of diagnosis. As such, the reports capture new incidents of the particular disease. These statistics are reported at the county level by age group and gender.[68] Among the most reliable statistics are disease rates for Chlamydia.[69] These data are available for the years 1992 through 2005. Using occurrences by date of diagnosis, rates are calculated per 100,000 of the relevant population.

In the United States, Chlamydia is the most commonly reported STD. The disease does not always present with symptoms, but if symptoms arise, they are typically discovered within three weeks.[70] Women experience a greater risk of contracting Chlamydia (and other STDs) and are more likely to have symptoms, as well as serious complications, simply because of their physical design.[71] Because most women have yearly physicals or gynecological visits, however, the disease is also more likely to be diagnosed in women.[72] The disease is easily diagnosed, treated, and cured with antibiotics, but can lead to serious health problems if not treated promptly.

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[68] Disease & Reproductive Health Assessment Unit, Community & Family Health Division, Washington State Department of Health. These data were graciously made available by Mark Stenger at the Washington Department of Health.

[69] Data are also collected for Gonorrhea, Herpes, and Syphilis. Gonorrhea incidence is less common in Washington than Chlamydia. Herpes statistics are largely underreported and Syphilis is a rare disease in the State of Washington (per Mark Stenger).

[70] WebMD, http://www.webmd.com/hw/std/aa29303.asp.

[71] Reproductive Health Technologies Project, http://www.rhtp.org/std/types.asp.

[72] Reproductive Health Technologies Project, http://www.rhtp.org/std/types.asp.

The CDC reports that in 2004 over 900,000 incidences were reported in the United States.[73] Figure 3-1 shows Chlamydia rates over time for the United States from 1992 through 2003.[74] In the US, Chlamydia rates have been on the rise since the beginning of the data period in 1992, reaching a high rate of 300 in 2003. While Washington State has experienced increases as well, the pattern for Chlamydia is not the same as the national pattern. Figure 3-2 shows the rate of Chlamydia diagnosis in the State of Washington between 1992 and 2005 for all diagnoses and female diagnoses. Chlamydia rates were relatively stable between 1995 and 1997. In the years following 1997, Chlamydia rates rose for both groups and have reached all time high levels relative to the past 14 years. Between 1998 and 2005, overall Chlamydia rates increased by approximately 47 percent, while female Chlamydia rates increased by approximately 39 percent. Because of the predominance of the disease in women, overall and female Chlamydia rates are studied. Age-specific rates are available for females aged 15-19 and 20-24.[75]

Abortion Data

Washington gathers and reports detailed statistics on induced abortions by year, by county, and by age group.[76] One of the main goals of the pilot program was to reduce the number of unintended pregnancies in the Washington area. Unintended pregnancies are difficult to measure,

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[73] Chlamydia - CDC Fact Sheet, Centers for Disease Control and Prevention, http://www.cdc.gov/std/Chlamydia/STDFact-Chlamydia.htm.

[74] Centers for Disease and Control, http://wonder.cdc.gov/std.html.

[75] Race and ethnicity information is also collected, but is often missing. STD counts by race may be incomplete and I have therefore not utilized the data by race.

[76] Data are available through the Washington Department of Health, Center for Health Statistics, http://www.doh.wa.gov/ehsphl/chs/chs-data/abortion/viewdown.htm. The Center for Health Statistics does not calculate rates when the number of cases is less than or equal to five. To avoid large jumps in rates, I have utilized the actual number of abortions by county and calculated rates for all values of occurrences. Caution should be taken, however, in interpreting rates associated with a small number of occurrences.

but the effects of the program can be captured by abortion rates. Figure 3-3 shows the overall abortion rate for females (aged 15-44) in Washington State. Between 1992 and 2004, abortion rates in Washington decreased by approximately 20 percent. Between 1998 and 2004, overall abortion rates decreased by approximately 5 percent. Figure 3-4 displays the changes in abortion rates for the US between 1992 and 2003 for comparison purposes. In the United States, the abortion rate has fallen from 1992 until 1998, and then remained fairly stable from 1998 to 2003.

Of particular concern are abortion rates for young women, mainly women aged 15-19 and 20-24. Females aged 15-19 are of particular interest because most potential pregnancies in this age group are unintended and most of these young women are unmarried. At least some of the women in the 20-24 age band will be unmarried and have unplanned pregnancies. Figure 3-5 illustrates the trend in abortion rates for Washington women aged 15-19 and 20-24. These two age bands experience the highest abortion rates of all age groups. Similar to the overall rate, abortion rates for both age bands are somewhat static between 1995 and 1997. Abortion rates for women aged 15-19 decreased by approximately 19 percent between 1998 and 2004. For women aged 20-24, abortion rates decreased by 9 percent between 1998 and 2004.

Program Participation

Information on the filing of collaborative agreements was provided by the Board of Pharmacy, Washington Department of Health.[77] The Board of Pharmacy approves all collaborative agreement filings and therefore was able to provide a list of pharmacies by location

[77] I also received similar, but less complete, information from the Office of Population Research at Princeton University, the organization which manages the Not-Too-Late website and hotline. They maintain a current list of participating pharmacies in states with pharmacy access legislation. Their database is designed to provide women seeking emergency contraception with current provider locations. Although the database was not designed to keep historical participation records, I have utilized this information for some purposes.

and date of filing.[78] I subsequently used this information to determine when certain areas gained eligibility to dispense emergency contraception without a prescription. While participation in the program and the formation of collaborative agreements are at the pharmacist level, I aggregate access to the county-level. If a pharmacist had an agreement on file in year t in county i, I designate that county as having pharmacy access for that year and the remaining years in the dataset.[79] In later specifications, I substitute this definition of treatment with the percent of total pharmacies with pharmacy access in county *i* in year *t*.

Initially, the pilot was intended to focus on counties in the Puget Sound area, mainly King, Pierce, and Snohomish counties. These counties were chosen because they are located in the Seattle area and monitoring would be more feasible. Since the law allows any pharmacist across the state to form a collaborative agreement, when news of the pilot spread, pharmacists in other areas of the state began to participate. The majority of the pilot program occurred between February 1998 and June 1999. During this time, 11,976 prescriptions for emergency contraception were dispensed by pharmacists.[80] Pharmacists in 18 counties were involved during

[78] In almost all cases, I relied on the dates of collaborative agreement filing provided by the Department of Health. There were a few circumstances, however, where there was a discrepancy in dates and I relied on dates compiled by Princeton. These circumstances involved Washington counties that had little participation over the time period. The Princeton information showed the same pharmacy as the Washington Department of Health information, but with a much earlier date of initial participation. In these instances, I adjusted the definition of treatment to reflect the Princeton information.

[79] Some specific pharmacies may have lost approval to dispense emergency contraception, but it does not appear from the data that any county lost access to emergency contraception. In other words, there may have been a change in the number of pharmacies dispensing emergency contraception in each county over time.

[80] Gardner et al (2001).

the pilot program.[81] After the official pilot program activities concluded, additional pharmacies across the state continued to file collaborative agreements and access to emergency contraception continued to spread. As of 2006, 294 pharmacies in 31 (of 39) counties are eligible to provide emergency contraception without a prescription.[82]

Figure 3-6 displays a map of Washington State, shading the counties which had pharmacy access in 1998, the first full year of the program. In Figure 3-7, the same map is displayed for a 2002, and Figure 3-8 shows pharmacy access for 2005. As shown, pharmacy access has grown over time between 1998 and 2005. Today, almost all counties in Washington State have some pharmacies which provide nonprescription access to emergency contraception.

Identification

In this section, I compare the participating and nonparticipating counties before the program began. A county is defined to be participating or treated if any pharmacy access to emergency contraception is available in that county. If no pharmacy access is available in a county, then it is considered a nonparticipating or control area. The program was officially initiated in 1997, but most of the program activities began in 1998. The pretreatment years, or years before any pharmacy access was available in Washington, are defined as 1995-1997. Baseline statistics for these years are presented in Table 3-2.

Chlamydia

To properly identify the effect of pharmacy access on Chlamydia rates, we require the treatment and control groups to be similar but for the treatment. More specifically, the groups

[81] These counties include Benton, Clallam, Clark, Cowlitz, Island, King, Kitsap, Pierce, Skagit, Skamania, Snohomish, Spokane, Thurston, Wahkiakum, Walla Walla, Whatcom, Whitman, and Yakima.

[82] A national website, www.not-2-late.com, provides a current listing of EC providers in Washington State.

must be on the same trajectory prior to the program's implementation. With difference-in-difference estimation, we do not require the groups to be at the same level necessarily but we do require the groups to exhibit similar trends. County fixed effects, which are employed in what follows, control for any time-invariant changes in unobservable characteristics. But Chlamydia is a communicable disease, and therefore, the level at time t may be determined in part by the level at time t-1.[83] In other words, the greater the number of individuals infected with the disease, the more quickly it can spread and cause new infections.

In my paper, however, I use county-level observations and illustrate that pre-treatment, areas with and without pharmacy access are not only on the same trajectory, but are statistically indistinguishable in terms of their levels. Consider Figure 3-9, which plots the overall Chlamydia rate for the treatment and control groups between 1995 and 2005. As shown, both groups follow the same trajectory between 1995 and 1997, while the treatment group is at a slightly higher level. After the start of the pilot program, indicated by the dotted vertical line at 1998, the two groups diverge in terms of their overall Chlamydia rates. Similarly, Figure 3-10 illustrates that female Chlamydia rates exhibit a similar pattern to overall Chlamydia rates.

At first glance, it may appear troublesome that Chlamydia rates are higher in treatment counties than in control counties. After conducting a difference in means t-test, however, I fail to reject the null hypothesis that the difference in the two groups is zero and conclude that the two

[83] Some of the previous empirical literature has indicated the importance of including a lagged dependent variable as a covariate. Panel OLS estimates with a lagged dependent variable are inconsistent in the presence of fixed effects. This occurs because the lagged dependent variable is correlated with the state fixed effects. Alternative methods have been developed to allow estimation. One option is to take first differences and estimate a 2SLS model using lagged levels or lagged differences as instruments ($Y_{i,t-2}$ or $Y_{i,t-2} - Y_{i,t-3}$). Such instruments are typically highly correlated with the first lagged difference, but uncorrelated with the transformed error. Arellano and Bond (1991) and more recently Blundell and Bond (1998) developed a GMM style approach to exploit a similar FD2SLS idea, where lagged levels and/or lagged differences serve as instrumental variables.

groups are statistically indistinguishable at the five percent level. The means test was conducted for 1995, 1996, and 1997, and these results are presented in Table 3-3. For females in 1997, we would reject the null at the 10 percent level. It is possible, however, that some effects of the program began occurring as early as 1997. The actual program was initiated in 1997, and therefore, any effects of the initial activities, prior to start of the official program, could be observed here. But for the treatment, we conclude that the counties are alike. In 1998, the first year of the pilot program, Figures 3-9 and 3-10 show the rates begin to slightly diverge between the treatment and control group. Without expanded access to emergency contraception, and given no other shocks, we would expect the groups to continue to look and trend similarly.

Given that our treatment and control groups are similar prior to 1998, we assume that without treatment, the groups would continue to be on the same trajectory. Other studies which use the lagged STD rate as a right-hand side regressor may not be able to exploit such an environment. Many other studies which estimate STD rates utilize state or regional level data, which may not satisfy these conditions. Since both groups experience the same trend in the years before the pilot program, and their levels are not statistically different, we do not need to account for a one-period lag in the Chlamydia rate.

Abortions

The initial treatment and control groups also look similar in terms of their abortion rates. Figure 3-11 displays the overall abortion rate for the years 1995-2004 by treatment status. Although the initially treated counties exhibit a slightly higher level, both groups trend similarly during the pretreatment years. The two groups trend somewhat similarly before 1998, with the control group experiencing somewhat larger declines in abortion rates. Figures 3-12 and 3-13 illustrate the abortion rate for age 15-19 and 20-24 by treatment status for the years 1995 through

2004.[84] Abortion rates for ages 15-19 trend similarly in the treatment and control group. Both groups experience slight declines during the pretreatment years, with the control group experiencing a slightly more dramatic decline between 1997 and 1998. For 20-24 abortion rates, both groups experience slight increases in abortion rates during the pretreatment years, with the control group experiencing a slightly more dramatic increase between 1996 and 1997.

Table 3-3 shows the results of the difference in means tests with respect to abortion rates. For these measures, we conclude that abortion rates in the treatment group are statistically different from the control group. The areas with pharmacy access exhibit slightly higher levels of abortion rates. To the extent that this is a time-invariant characteristic, the county fixed effects used in the analysis deal with the difference in levels. If the true effect of pharmacy access is to reduce abortions, however, then higher abortion rates in the treatment group would lead coefficient estimates to be upward biased. Such a bias could lead to a finding of no effect. If an effect is identified, however, then it is a conservative estimate.

Pharmacy Participation

The decision to form a collaborative agreement and ultimately dispense emergency contraception is made by the individual pharmacist or pharmacy. Pharmacy participation is based on a variety of factors. Possible factors include the demand for emergency contraception in the area, the attitudes and beliefs of the pharmacist or pharmacy management, or a desire to change the pharmacist-patient relationship. If a given pharmacist forms a collaborative agreement because of high demand for emergency contraception in the area, which could be correlated with a high degree of sexual activity in the area, then the coefficient estimates on pharmacy access

[84] For ages 15-19 and 20-24, there are two control counties which have virtually zero abortions in these two age bands. I have dropped these two counties from the control group.

with respect to Chlamydia rates could be biased upwards. Figures 3-9 and 3-10 illustrated the similarities in the trends of the treatment and control groups before the pilot program began. Not only did the two groups trend similarly, but Table 3-3 indicated that pretreatment the two groups are statistically indistinguishable. It does not appear that pharmacies in treatment counties are different from the control group. Moreover, we should not expect the coefficient estimate on pharmacy access to be biased upwards.

As discussed above, pharmacy participation could be related to the overall level of sexual activity or risky behavior in the pharmacy's area. This could be correlated with the level of teen pregnancy or the level of abortions in the area. Pharmacy access to emergency contraception may lead to fewer abortions and/or fewer teen pregnancies because some pregnancies may be prevented and some abortions may not be necessary. If pharmacies that formed collaborative agreements did so because of these factors, then it is possible that the coefficient estimate on abortion rates or teen pregnancy rates could be biased upward. If the true effect of treatment is negative, however, this would upward bias my results in terms of finding no effect or a positive effect. If I find a negative effect, then the coefficient estimates would be a conservative estimate.

Given that pharmacies choose to file a collaborative agreement at different times throughout the data period, we must also consider the timing of participation. Because not all areas of Washington gained pharmacy access to emergency contraception at the same time, the timing of participation could affect the coefficient estimates. Many counties experienced pharmacy participation between 1998 and 1999, during the pilot program. Any county with a participating pharmacy during this time is considered an "early" participant. Any county which did not have a pharmacy participate until 2000 or after is considered a "late" participant.

To properly identify the effect of pharmacy access, we must ensure that the early participants look similar to the late participants. Difference in means tests were conducted for early and late participants and are contained in Table 3-4. These tests confirm that the overall and female Chlamydia rates between the early and late participants are not statistically distinguishable. The same conclusion is reached for abortion rates for women aged 15-19. Early and late participants are not statistically different. When considering the overall abortion rate, however, the early and late participants are statistically different in 1995 and 1996, but are statistically indistinguishable in 1997.

Other Characteristics

Difference in means tests were also conducted for other county-level characteristics such as the county-level unemployment rate, per capita income (real), and divorce rate. We would expect the counties which had pharmacy access to look similar to counties without pharmacy access. The unemployment rate and per capita income may suggest something about the economic conditions of the county, while the divorce rate may suggest something about the family environment for young people in the area. Table 3-5 reports differences in means tests for 1995, 1996, and 1997 for the three county-level measures. In each and every case, we fail to reject the null hypothesis that there is no difference in these measures between the treatment and control counties at the five percent level.

Empirical Methodology

My study exploits county-level variation in both Chlamydia and abortion rates to examine the intended and unintended effects of pharmacy access to and awareness of emergency contraception. The equations below will be estimated using a fixed effects model:

$$Chlamydia_{it} = \beta_0 + \beta_1 PharmacyAccess_{it} + \phi_i + \theta_t + \varepsilon_{it} \qquad (3\text{-}1)$$

$$Abortion_{it} = \beta_0 + \beta_1 PharmacyAccess_{it} + \phi_i + \theta_t + \varepsilon_{it} \qquad (3\text{-}2)$$

where $Chlamydia_{it}$ indicates the Chlamydia rate in county *i* in year *t*. The rate is defined as the number of occurrences divided by the relevant population, multiplied by 100,000. $Abortion_{it}$ is defined as the number of abortions divided by the relevant population, multiplied by 1,000. $PharmacyAccess_{it}$ is a dummy variable for county-level pharmacy access to emergency contraception. In the initial specifications, a county is defined as having pharmacy access in year *t* if at least one pharmacy has a collaborative agreement on file with the Board of Pharmacy. In later specifications, pharmacy access is defined as the percentage of total county pharmacies with pharmacy access. Both equations also contain county and year fixed effects. The fixed effects will capture any time-invariant county characteristics which could bias the estimated effects of pharmacy access. Equations (3-1) and (3-2) are estimated without other county-level characteristics. Any covariate must be identified from within county variation over fourteen years.[85] Available county-level measures, however, do not vary considerably over time.. For comparison purposes, however, the results with several covariates are presented in Section VIII. Unless otherwise specified, data are available for 39 counties in Washington State for 14 years, 1992 through 2005, yielding a total of 546 observations when estimating Equation (3-1). For estimation of equation (3-2), data are available from 1992 to 2004, yielding 507 observations.

[85] Levine (2001), who explains the amount of sexual behavior using various costs of engaging in sexual behavior, uses labor market conditions, generosity of the welfare system, and abortion restrictions as independent variables. While these measures vary at the state-level, they do not vary substantially at the county-level.

Results

Chlamydia Rates

Table 3-6 contains the coefficient estimates for Overall, Female, Female Age 15-19, and Female Age 20-24 Chlamydia rates.[86] The coefficient of pharmacy access on total Chlamydia rates is statistically significant and positive. Counties with pharmacy access experienced an increase in the total Chlamydia rate, with a coefficient of 24. Relative to a baseline average of pre-treatment years (three-year average of 1995-1997), pharmacy access increases the Chlamydia rate by approximately 18 percent. [87] For females, pharmacy access is also associated with an increase of 18 percent, relative to the three-year pre-treatment average. Equation (3-1) was also estimated for males but is not reported here. Pharmacy access does not have a statistically significant effect on Chlamydia rates for men.

Columns (3) and (4) of Table 3-6 contain regression results by female age group. Statistically significant results are obtained for those females aged 20-24. Pharmacy access is associated with a 331 unit (or a 28 percent) increase in the 20-24 Female Chlamydia rate. The coefficient estimate for *PharmacyAccess* for females aged 15-19 is not statistically significant.

The results using several covariates are presented in Table 3-8 for comparison purposes. The additional independent variables employed are county-level per capita income and the county unemployment rate. In almost all circumstances, the covariates included are statistically insignificant. In all cases, inclusion of the covariates does not sufficiently change the statistical

[86] To determine pharmacy access, I use the Washington Department of Health information primarily, but in some situation I supplement the dates with information from the Office of Population Research at Princeton University. The results are fairly consistent with the results presented in this section if I utilize Washington Department of Health data exclusively.

[87] Three-year pretreatment averages are contained in Table 3-7.

significance of the treatment effect, although the magnitude of the treatment effect is slightly different.

Abortion Rates

Table 3-9 reports the results obtained from estimating Equation (3-2) for all women (15-44), women aged 15-19, and women aged 20-24. Abortion rates also appear to be affected by pharmacy access to emergency contraception. If women use emergency contraception as a substitute for abortion, then the number of abortions may decrease. Treatment is associated with a one unit decrease in the overall abortion rate. Relative to a three-year pre-treatment average, this decrease corresponds to a 6 percent decrease in abortions. For teens aged 15-19, treatment is associated with over a two unit (or an 11 percent) decrease in abortion rates. Pharmacy access is also statistically significant with respect to abortion rates for women aged 20-24. Again relative to the three-year pre-treatment average, abortion rates for women 20-24 were reduced by 15 percent.

Although the results are not reported here, I also conducted the analysis for women aged 25-29, 30-34, 35-39, and 40-44. Pharmacy access was not statistically significant in the regressions of any of these four age bands. Pharmacy access to emergency contraception appears to mainly impact the abortion rate for younger women, particularly those aged 15-24.

Lag in Treatment

The results presented in the previous section use the actual dates of participation based on the initial collaborative agreement filing when considering the timing of treatment. These results are fundamentally unchanged if we lag the treatment time by 6 months or by 1 year. The effect of pharmacy access to emergency contraception is robust to these two alternative treatment definitions.

Alternative Treatment Definitions

In the previous section, counties were considered treated if any pharmacies in county *i* had pharmacy access in year *t*. In this section, treatment is defined as the number of pharmacies with pharmacy access divided by the total number of pharmacies in that county in that year. In other words, treatment is the fraction of county pharmacies with pharmacy access.

I count pharmacies using Washington Department of Health collaborative agreement information filed with the Board of Pharmacy. I have tried to eliminate some duplicative records which appear in this information in order to count *new* collaborative agreements. After counting the number of collaborative agreements, I divide the number of pharmacies with access by the total number of pharmacies in that county in that year. [88] The results of this estimation are contained in what follows.[89]

Chlamydia Rates

Table 3-10 reproduces the results of Equation (3-1) using this alternative definition of treatment. The results in Table 3-10 are similar to those in Table 3-7. Pharmacy access is associated with an increase in both overall and female Chlamydia rates. In this specification, however, pharmacy access is associated with an increase in the female Chlamydia rates for

[88] The total number of pharmacies by county by year was obtained from County Business Patterns, U.S. Census Bureau, http://www.census.gov/epcd/cbp/view/cbpview.html, relevant years. These data are only available from 1993 through 2002, so I used the counts for 2002 as counts for the years 2003, 2004, and 2005. Because pharmacy totals are not available for 1992, I am unable to use this year of data in what follows.

[89] I alternatively use all records provided to me by the Department of Health by county by year. In this counting mechanism, I do not try to eliminate any potential duplications, but rather use all the records provided. The results using these counts of pharmacies are consistent with my corrected counts.

women age 15-19 as well as for women age 20-24. For the female 20-24 Chlamydia rate, a one percent increase in pharmacy access is associated with a 4.5 percent increase in the disease rate.

Abortion Rates

Table 3-11 shows the results for county abortion rates when estimating Equation (3-2). Using the percent of county pharmacies with pharmacy access yields much weaker results in this model. In Table 3-9, pharmacy access was associated with decreases in overall abortion rates, female abortion rates for women aged 15-19, and female abortion rates for women aged 20-24. Using this alternative treatment definition, however, pharmacy access is only associated with a decrease in abortion rates for women aged 15-19.

Other Considerations

My empirical strategy relies on variation in pharmacy access by county as well as outcome variables that are measured at the county-level. Although to my knowledge these are the best data available at this time, there are some drawbacks of using county-level data to identify this treatment effect. First, I define treatment at the county-level first as a binary indicator and then as the percentage of pharmacies in a county with pharmacy access. Restricting treatment to the county-level could cause problems if some areas of certain counties are in fact “more” treated than others. First, some counties in Washington State are very large while some are much smaller. Other counties in Washington are more densely populated while other counties are more rural. Furthermore, the population in some counties is concentrated in specific areas of a larger geographic region. It is possible, therefore, that I could be misclassifying treated and nontreated counties. In other words, if a county has some pharmacy access but this access is all concentrated around a border with a nontreated county, then it is possible that the nontreated county could be just as affected or more affected by pharmacy access. To the extent that any misclassification

means that I classify areas without pharmacy access nontreated when in fact they are treated, this would bias my estimates in favor of finding no effect.

Falsification Tests

To test that the identification of the treatment effect is not capturing a general trend in increased disease or in overall risky behavior, a falsification exercise is performed and reported in Table 3-12 and 3-13.

I use Washington State Cancer Registry data on county cancer rates.[90] Washington cancer data are available by county in three-year averages beginning with 1992-1994 and ending with 2002-2004. In order to utilize this data, I use the midpoint of the three-year ranges as observations for that year. In this way, I am able to use county-level cancer rates for 1993 through 2003. There are some observations which are not reported for some counties in some years. These missing observations account for number of observations used in each regression.

If the identified effects of pharmacy access on sexually transmitted diseases are capturing an upward trend in general disease rates in Washington State, then we should find a positive coefficient on pharmacy access with respect to cancer rates. Table 3-12, where each column is a separate regression, shows the results when regressing pharmacy access, along with county and year fixed effects, on several cancer rates including total cancer rates, female cancer rates, total lung cancer rates, female lung cancer rates, and female breast cancer rates. In all cases, I find no evidence that pharmacy access is associated with an increase in cancer rates.

Additionally, I test if the effect of pharmacy access is capturing a general upward trend in risky behavior. I use measures of risky behavior including alcohol or substance use as well as

[90] Washington State Cancer Registry, http://www3.doh.wa.gov/WSCR/default.htm, relevant years.

various criminal behaviors.[91] To ensure that the participation in emergency contraception by pharmacies is unrelated to other measures of risky behavior, I regress *PharmacyAccess* on the rates contained in Table 3-13. Each column is a separate regression. Alcohol/Drug Related Death Rate is calculated as the number of alcohol/drug related deaths per 100 total deaths. The remaining rates are calculated as the number of arrests of the particular crime divided by 1,000 of the respective population. For example, alcohol-related arrests (18+) is defined as the total number of alcohol related arrests for individuals aged 18 and over divided by 1,000 of the 18 and over population.

Pharmacy access has no effect on any other measure of risky behavior. Pharmacy access is not statistically significant in any of the regressions presented in columns (1) through (9). Pharmacy access to emergency contraception does not appear to have an effect on these alternative measures of risky behavior.

Additional Control Group: Oregon

Chlamydia

This section utilizes an additional source of data to increase the size of the control group. This section is used as a supplement to original methodology because the available Washington data are richer than the available Oregon data. We can, therefore, compare some of the results in this section with Section VIII, but due to data availability, we cannot compare all measures. Oregon, which does not have pharmacy access to emergency contraception, looks similar to Washington in the pre-treatment years and is therefore an appropriate comparison group. As

[91] 2005 Risk and Protection Profile for Substance Abuse Prevention, Research and Analysis Division, Washington State Department of Social and Health Services, http://www1.dshs.wa.gov/rda/research/4/47/updated/default.shtm. Most measures are available for 1993 through 2004, except Alcohol and Drug Related Deaths which is available for 1992 through 2003. Some counties did not report certain measures for certain years due to small sample sizes or missing information. I have coded these observations as missing observations.

illustrated in Figure 3-14, Washington and Oregon exhibit similar trends between 1994 and 1998; the rates are almost identical. After the start of the pilot program and subsequent pharmacy access, rates for both states increase, but Washington experienced greater increases in Chlamydia rates. Figure 3-14 confirms that Oregon is suitable as a comparison group in terms of Chlamydia rates. Additionally, Figure 3-15 presents overall Chlamydia rates for the treatment and control groups. In this figure, untreated areas of Washington are combined with Oregon counties (also untreated) to comprise a larger control group. As shown, both groups trend similarly during the pre-program period. Differences in means tests, shown in Table 3-14, confirm that the treatment and control groups are statistically indistinguishable. Equation (3-1) is reestimated using the additional Oregon county-level data. Oregon, however, only publishes overall Chlamydia rates; county-level rates are not available by gender or by age.[92] Chlamydia rates for Oregon are available from 1994 through 2005. As a result, Equation (3-1) is now estimated for 1994-2005. Summary statistics for these data, and the abortion data, are contained in Table 3-15. The results from the estimation are contained in Table 3-16.

The coefficient estimate on *PharmacyAccess* when including the Oregon data is larger and more precise than the coefficient presented in Section VIII. Without including the Oregon data, the treatment effect for overall Chlamydia rates was 28.9. Upon using Oregon county data, the treatment effect is 38.8. Relative to the three-year pretreatment average, the latter coefficient represents a 29 percent increase in overall Chlamydia rates. Three-year pretreatment averages are contained in Table 3-17.

Using the combined Washington and Oregon data, I also re-estimate the model using the percent of pharmacies with pharmacy access as the treatment. Table 3-18 contains the results of

[92] Oregon Department of Human Services, http://oregon.gov/DHS/ph/std/annrep.shtml.

this estimation. Again, increased pharmacy access is associated with an increase in the overall Chlamydia rate.

Abortion

A comparable alternative approach can also be conducted for abortion rates. In order to use Oregon counties as additional control counties, we must confirm that Oregon and Washington look and trend similarly before introduction of pharmacy access in Washington. Figures 3-16, 3-17, and 3-18 present graphical evidence that support this requirement. As shown, Washington and Oregon trend similarly between 1995 and 1997 in terms of the overall abortion rate, 15-19 abortion rate, and 20-24 abortion rate.

We can further compare the counties which were treated in Washington with the untreated counties from both Washington and Oregon. Figures 3-19, 3-20, and 3-21 illustrate these trends graphically. Overall abortion rates declined somewhat for both treatment and control groups during the time period. Abortion rates for age 15-19 show sharper declines after the introduction of pharmacy access, while abortion rates for age 20-24 show small declines.

The results of the reestimation of Equation (3-2) are presented in Table 3-19. Using this alternative approach, we are unable to identify an effect of pharmacy access on the overall abortion rate for women aged 15-44 or for teens age 15-19. We are, however, able to identify a negative effect of pharmacy access on abortions by females age 20-24. The coefficient on pharmacy access accounts for approximately 11 percent of the decrease in abortion rates for females aged 20-24.

This model was reestimated using the percent of pharmacies with pharmacy access as the treatment in place of the binary treatment indicator. The results of this estimation are contained in Table 3-20. In this model, the results for overall abortion rates are much weaker. Using this

definition of treatment, I am unable to identify an effect of pharmacy access on overall abortion rates, abortion rates for age 15-19, or abortion rates for age 20-24.

Conclusions

The FDA recently approved a proposal to allow emergency contraception to be available nationwide without a prescription for women over the age of 18. The State of Washington, however, was the first state to implement a program to increase access to emergency contraception through pharmacies. In my paper, I employ county-level data from Washington to consider the impact of such a program. Using a difference-in-difference methodology, and taking care to ensure that the treatment and control groups are similar pre-treatment, I find evidence of effects with respect to both STD rates and abortion rates. The results suggest that increased access is associated with a reduction in the abortion rate, particularly for young women. This result is stronger when using the binary treatment definition than when using the percentage of pharmacies with access. A tradeoff, perhaps, is that increased access is also associated with an increase in the overall and female Chlamydia rates. In particular, the results for suggest increases in the Chlamydia rate for females aged 20-24. When using the percentage of pharmacies with access, the results suggest that increased pharmacy access is associated with not only increases in the female 20-24 Chlamydia rate, but also the female age 15-19 Chlamydia rate.

Table 3-1. Summary statistics

Summary Statistics*					
Variable	Mean	Median	Min	Max	Std. Dev.
Chlamydia Rate	169.3	165.9	0	440.0	82.7
Female Chlamydia Rate	266.4	260.4	0	683.7	127.9
Female Chlamydia Rate, Age 15-19	1,603.1	1,646.7	0	3,605.8	750.9
Female Chlamydia Rate, Age 20-24	1,545.0	1,477.3	0	5,199.3	875.4
Abortion Rate	14.5	14.8	0	29.1	5.1
Abortion Rate, Age 15-19	19.4	19.4	0	42.9	8.4
Abortion Rate, Age 20-24	33.4	34.0	0	70.2	13.9
Unemployment Rate	7.47	7.2	1.6	17.6	2.5
Real income per capita	$26,749	$25,470	$18,650	$53,583	$5,085.4
Pharmacy Access (binary)	0.37	0	0	1	0.48
Percent of Pharmacies with Access	12.3	0	0	100	23.2

*STD summary statistics and explanatory variables are calculated for the years 1992 through 2005. Summary statistics for the abortion variables are calculated for the years 1992 through 2004.

Table 3-2. Baseline statistics

	1995		1996		1997	
Variable	Treat	Control	Treat	Control	Treat	Control
Chlamydia Rate	141.3	106.8	142.4	108.4	140.2	101.7
Female Chlamydia Rate	228.7	189.8	231.5	169.8	224.3	164.2
Abortion Rate	15.5	10.3	15.7	9.1	15.7	8.6
Abortion Rate, Age 15-19	23.2	12.1	21.9	11.4	21.9	11.7
Abortion Rate, Age 20-24	34.1	18.7	34.8	18.2	34.8	25.1

Table 3-3. Difference in means t-tests between treatment and control

[Null Hypothesis: difference in means is zero]

Overall Chlamydia Rate			
Year	Mean Treatment	Mean Control	P-value
1995	141.3	106.8	0.1163
1996	142.4	108.4	0.1693
1997	140.2	101.7	0.0655
Female Chlamydia Rate			
Year	Mean Treatment	Mean Control	P-value
1995	228.7	189.8	0.2773
1996	231.5	169.8	0.1280
1997	224.3	164.2	0.0828
Abortion Rate 15-44			
Year	Mean Treatment	Mean Control	P-value
1995	15.5	10.3	0.0025
1996	15.7	9.1	0.0003
1997	15.7	8.6	0.0004
Abortion Rate 15-19			
Year	Mean Treatment	Mean Control	P-value
1995	23.2	12.1	0.0002
1996	21.9	11.4	0.0002
1997	21.9	11.7	0.0007
Abortion Rate 20-24			
Year	Mean Treatment	Mean Control	P-value
1995	34.1	18.7	0.0013
1996	34.8	18.2	0.0015
1997	34.8	25.1	0.0753

Table 3-4. Difference in means t-tests between early and late adopters

[Null Hypothesis: difference in means is zero]			
Overall Chlamydia Rate			
Year	Mean Early	Mean Late	P-value
1995	149.0	131.1	0.3423
1996	142.7	142.2	0.9802
1997	145.5	133.1	0.4874
Female Chlamydia Rate			
Year	Mean Early	Mean Late	P-value
1995	243.4	209.3	0.2713
1996	229.1	234.6	0.8714
1997	230.3	216.6	0.6583
Abortion Rate 15-44			
Year	Mean Early	Mean Late	P-value
1995	16.4	13.3	0.0457
1996	16.7	13.4	0.0546
1997	16.5	13.8	0.1621
Abortion Rate 15-19			
Year	Mean Early	Mean Late	P-value
1995	24.1	21.1	0.2452
1996	22.7	19.9	0.2511
1997	23.3	18.8	0.1144
Abortion Rate 20-24			
Year	Mean Early	Mean Late	P-value
1995	36.5	28.4	0.0720
1996	38.7	25.7	0.0079
1997	35.7	32.6	0.5684

Table 3-5: Difference in means t-tests for county characteristics

Unemployment Rate			
Year	Mean Treatment	Mean Control	P-value
1995	7.5	8.8	0.2018
1996	7.3	8.9	0.1409
1997	6.2	7.5	0.1380
Real Per Capita Income			
Year	Mean Treatment	Mean Control	P-value
1995	$25,642	$23,401	0.1513
1996	$25,072	$26,453	0.3985
1997	$26,826	$24,025	0.1020
Divorce Rate			
Year	Mean Treatment	Mean Control	P-value
1995	6.42	5.74	0.1894
1996	6.41	6.06	0.4418
1997	6.27	5.74	0.2049

Table 3-6. Chlamydia rates overall, by gender, and by gender/age

Variable Name	(1) All	(2) Female	(3) Females 15-19	(4) Females 20-24
PharmacyAccess	23.88	38.06	147.64	330.60
	[12.36]*	[17.62]**	[102.95]	[155.85]**
R-squared	0.75	0.75	0.55	0.52
Number of Observations	546	546	546	546
County Fixed Effects	X	X	X	X
Year Fixed Effects	X	X	X	X

Clustered standards errors (at the county-level) in brackets.
Chlamydia Rate = (Number of Cases / Relevant Population) * 100,000
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 3-7. Three-year pretreatment average, Washington

Variable	Average (1995-1997)
Chlamydia Rate	133.04
Female Chlamydia Rate	215.80
Female Chlamydia Rate 15-19	1,362.4
Female Chlamydia Rate 20-24	1,163.4
Abortion Rate	14.18
Abortion Rate, Age 15-19	19.90
Abortion Rate, Age 20-24	31.34

Table 3-8. Chlamydia rates overall, by gender, and by gender/age with covariates

Variable Name	(1) All	(2) Female	(3) Females 15-19	(4) Females 20-24
PharmacyAccess	19.75	33.86	92.12	298.2
	[10.89]*	[16.30]**	[95.56]	[153.33]*
Per Capita Income	0.0041	0.0045	0.0471	0.0405
	[0.0024]*	[0.0039]	[0.0267]*	[0.0277]
Unemployment Rate	-1.41	-1.06	-22.11	5.08
	[3.96]	[5.41]	[35.34]	[45.27]
R-squared	0.75	0.76	0.55	0.52
Number of Observations	546	546	546	546
County Fixed Effects	X	X	X	X
Year Fixed Effects	X	X	X	X

Clustered standards errors (at the county-level) in brackets.
Chlamydia Rate = (Number of Cases / Relevant Population) * 100,000
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 3-9. Abortion rates overall and by age

Variable Name	(1) Age 15-44	(2) Age 15-19	(3) Age 20-24
PharmacyAccess	-0.9	-2.14	-5.25
	[0.37]**	[1.15]*	[1.70]***
R-squared	0.85	0.72	0.66
Number of Observations	507	481	481
County Fixed Effects	X	X	X
Year Fixed Effects	X	X	X

Clustered standards errors (at the county-level) in brackets.
Abortion rate = (Number of Abortions / Relevant Population) * 1,000
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 3-10. Chlamydia rates overall, by gender, and by gender/age

Variable Name	(1) All	(2) Female	(3) Females 15-19	(4) Females 20-24
Percent of Total Pharmacies Participating	0.496	0.651	3.311	4.485
	[0.175]***	[0.263]**	[1.785]*	[2.075]**
Number of Observations	507	507	507	507
R-squared	0.77	0.77	0.56	0.55
County Fixed Effects	X	X	X	X
Year Fixed Effects	X	X	X	X

Clustered standards errors (at the county-level) in brackets.
Chlamydia Rate = (Number of Cases / Relevant Population) * 100,000
* significant at 10%; ** significant at 5%; *** significant at 1%
By using the percent of total pharmacies participating, I am only able to utilize data from 1993 through 2005. The data for total number of pharmacies for 1992 is incomplete, so I drop 1992 observations in these regressions. Because of using this method, in five cases, the percent of participating pharmacies out of total county pharmacies exceeds 100. In these five cases, I recode these percentages to 100 until updated data is available.

Table 3-11. Abortion rates overall and by age

Variable Name	(1) Age 15-44	(2) Age 15-19	(3) Age 20-24
Percent of Total			
Pharmacies Participating	-0.004547	-0.03404	0.002877
	[0.005630]	[0.019525]*	[0.044819]
R-squared	490	490	490
Number of Observations	0.86	0.76	0.62
County Fixed Effects	X	X	X
Year Fixed Effects	X	X	X

Clustered standards errors (at the county-level) in brackets.
Abortion rate = (Number of Abortions / Relevant Population) * 1,000
* significant at 10%; ** significant at 5%; *** significant at 1%
By using the percent of total pharmacies participating, I am only able to utilize data from 1993 through 2004. The data for total number of pharmacies for 1992 is incomplete, so I drop 1992 observations in these regressions. Because of using this method, in one case, the percent of participating pharmacies out of total county pharmacies exceeds 100. In this case, I recode the percentage to 100 until updated data is available.

Table 3-12. Falsification tests using county cancer rates

Variable Name	(1) Total Cancer Rate	(2) Female Cancer Rate	(3) Total Lung Caner Rate	(4) Female Lung Cancer Rate	(5) Female Breast Cancer Rate
PharmacyAccess	3.63	4.742	1.343	0.52	-7.859
	[9.610]	[13.629]	[2.999]	[2.658]	[8.858]
No. of Observations	429	429	424	395	428
R-squared	0.63	0.66	0.67	0.69	0.47
County Fixed Effects	X	X	X	X	X
Year Fixed Effects	X	X	X	X	X

Table 3-13. Falsification exercise

Variable Name	**(1)** Alcohol & Drug Related Deaths	**(2)** Alcohol Related Arrests (Age 18+)	**(3)** Drug Related Arrests (Age 18+)	**(4)** Violence Related Arrests (Age 18+)	**(5)** Property Crime Arrests (Age 18+)
PharmacyAccess	0.10	0.84	-0.12	0.14	-0.48
	[0.28]	[1.026]	[0.35]	[0.22]	[0.36]
R-squared	0.73	0.74	0.64	0.58	0.78
No. of Observations	410	453	453	453	453
County Fixed Effects	X	X	X	X	X
Year Fixed Effects	X	X	X	X	X

Variable Name	(6) Property Crime Arrests (Age 10-17)	(7) Violence Related Arrests (Age 10-17)	(8) Alcohol Related Arrests (Age 10-17)	(9) Drug Related Arrests (Age 10-17)
Treatment	-1.32	0.27	-1.722	0.32
	[2.59]	[0.47]	[1.46]	[0.52]
R-squared	0.69	0.44	0.72	0.67
No. of Observations	456	456	456	456
State Fixed Effects	X	X	X	X
Year Fixed Effects	X	X	X	X

Clustered standards errors (at the county-level) in brackets.
Rates = (Number of Occurrences / Relevant Population) * 1,000
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 3-14. Difference in means t-test, Chlamydia rates

Overall Chlamydia Rates [Null Hypothesis: difference in means is zero]			
Year	Mean Washington	Mean Oregon	P-value
1995	133.3	134.5	0.9402
1996	134.5	132.6	0.9091
1997	131.3	135.2	0.8090
Overall Chlamydia Rates			
Year	Mean Washington Treated	Mean Washington or Oregon Untreated	P-value
1995	141.3	129.0	0.4455
1996	142.4	127.7	0.4090
1997	140.2	128.5	0.4801

Table 3-15. Summary statistics, Washington and Oregon

STD Summary Statistics, 1994 – 2005				
Variable	Mean	Median	Minimum	Maximum
Chlamydia Rate	157.5	147.0	0	642.9
Abortion/Birth Summary Statistics, 1992 – 2004				
Abortion Rate	12.7	12.9	0	30.6
Abortion Rate, Age 15-19	16.6	16.5	0	49.1
Abortion Rate, Age 20-24	28.0	27.6	0	83.5

Table 3-16. Chlamydia rates including Oregon

Variable Name	(1) Chlamydia Rate
PharmacyAccess	38.79
	[9.11]***
R-squared	0.77
Number of Observations	900
County Fixed Effects	X
Year Fixed Effects	X

Clustered standards errors (at the county-level) in brackets.
Chlamydia Rate = (Number of Cases / Relevant Population) * 100,000
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 3-17. Three-year pre-treatment average, Washington and Oregon

Variable	Average (1995-1997)
Chlamydia Rate	133.54
Abortion Rate	12.92
Abortion Rate, Age 15-19	17.77
Abortion Rate, Age 20-24	26.19

Table 3-18. Chlamydia rates including Oregon

Variable Name	(1) Chlamydia Rate
Percent of Total	
Pharmacies Participating	0.81
	[0.179]***
R-squared	900
Number of Observations	0.77
County Fixed Effects	X
Year Fixed Effects	X

Clustered standards errors (at the county-level) in brackets.
Chlamydia Rate = (Number of Cases / Relevant Population) * 100,000
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 3-19. Abortion rates including Oregon

Variable Name	(1) Abortion Rate 15-44	(2) Abortion Rate 15-19	(3) Abortion Rate 20-24
PharmacyAccess	0.20	-0.52	-2.76
	[0.37]	[0.81]	[1.44]*
R-squared	0.87	0.75	0.73
No. of Observations	900	900	900
County Fixed Effects	X	X	X
Year Fixed Effects	X	X	X

Clustered standards errors (at the county-level) in brackets.
Abortion Rate = (Number of Cases / Relevant Population) * 1,000
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 3-20. Abortion rates including Oregon

Variable Name	(1) Abortion Rate 15-44	(2) Abortion Rate 15-19	(3) Abortion Rate 20-24
Percent of Total			
Pharmacies Participating	0.007	-0.001	0.034
	[0.005]	[0.010]	[0.027]
R-squared	900	900	900
No. of Observations	0.87	0.75	0.73
County Fixed Effects	X	X	X
Year Fixed Effects	X	X	X

Clustered standards errors (at the county-level) in brackets.
Abortion Rate = (Number of Cases / Relevant Population) * 1,000
* significant at 10%; ** significant at 5%; *** significant at 1%

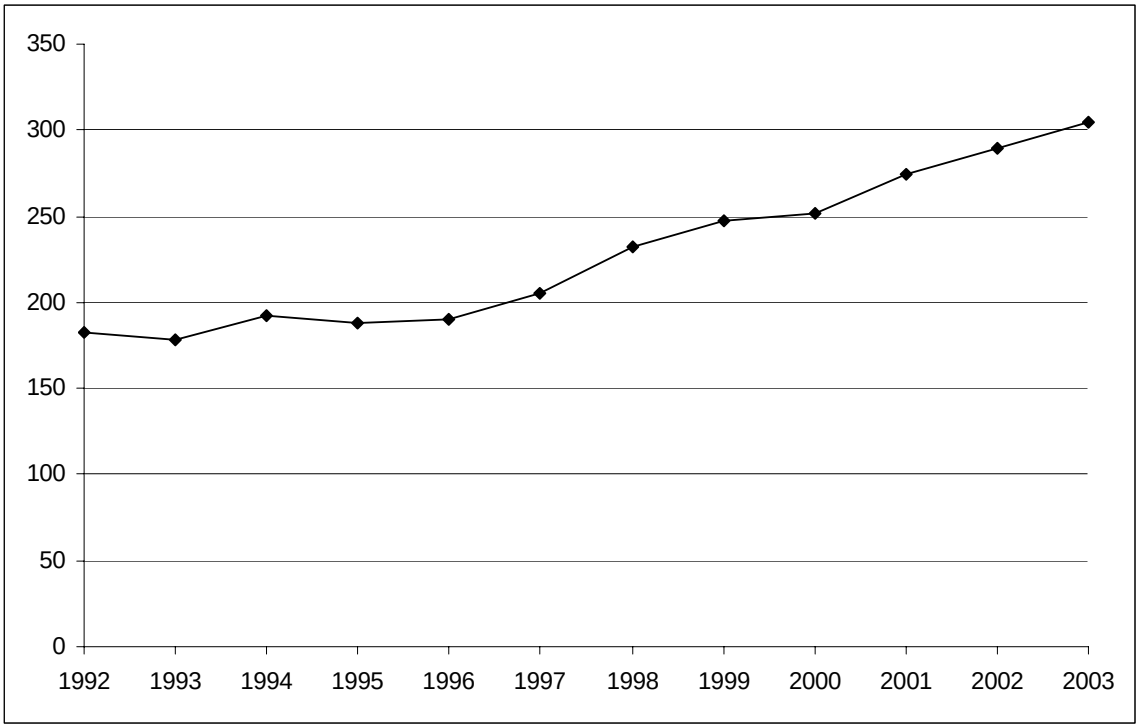

Figure 3-1.Chlamydia rates in the United States, 1992 – 2003.

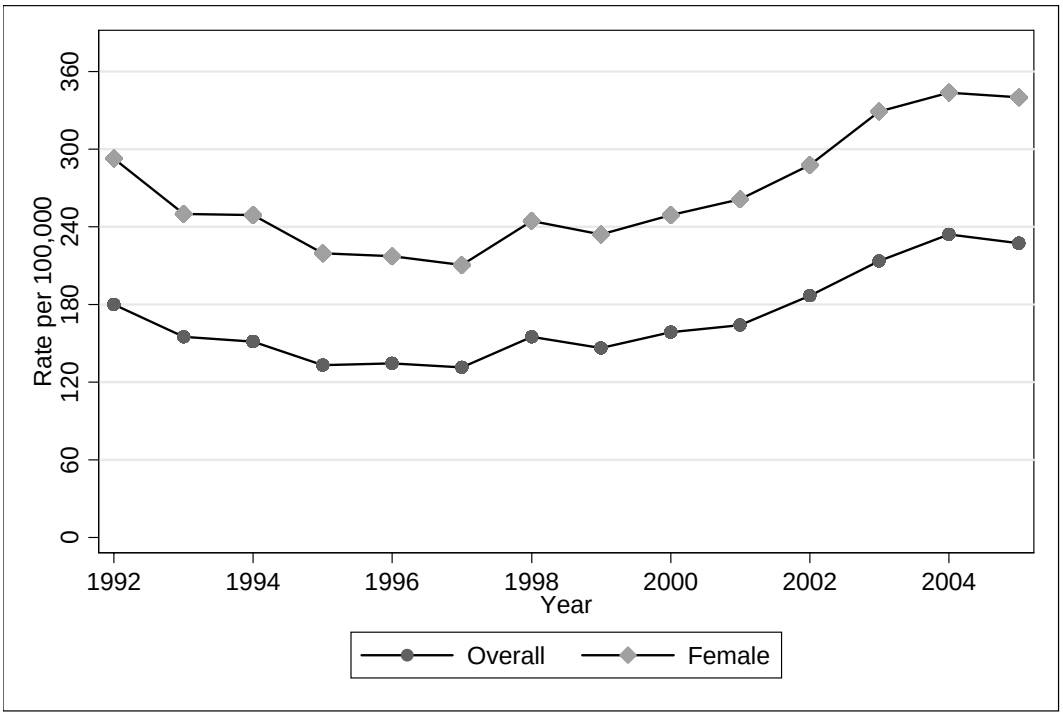

Figure 3-2. Overall and female chlamydia rates in Washington state

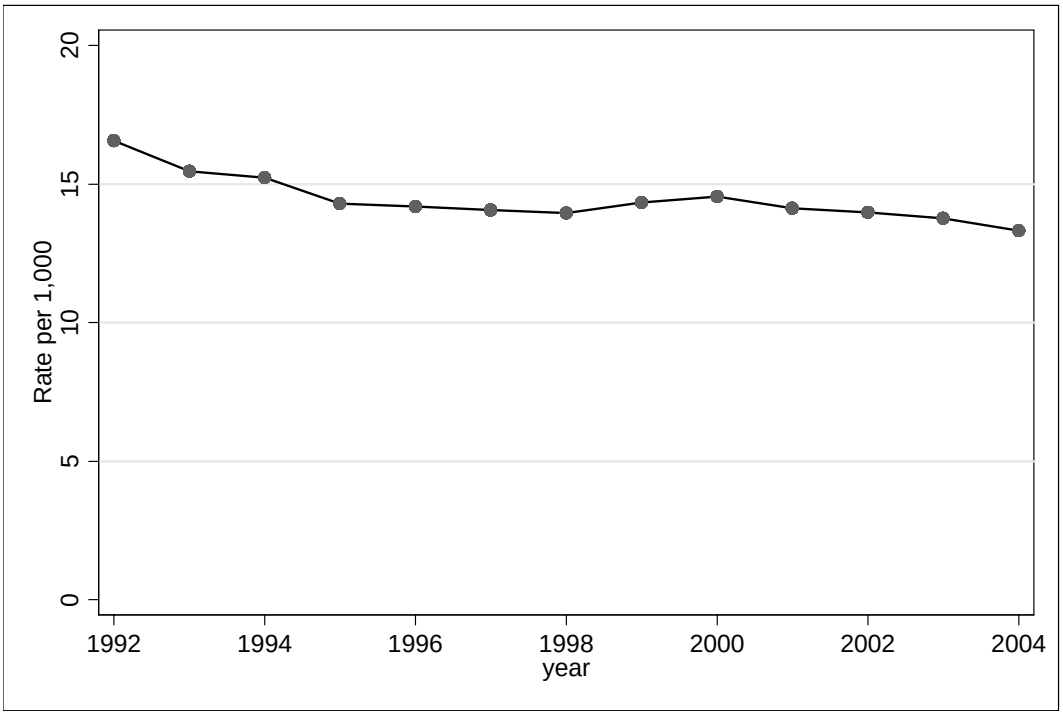

Figure 3-3. Overall abortion rate (age 15-44) in Washington state

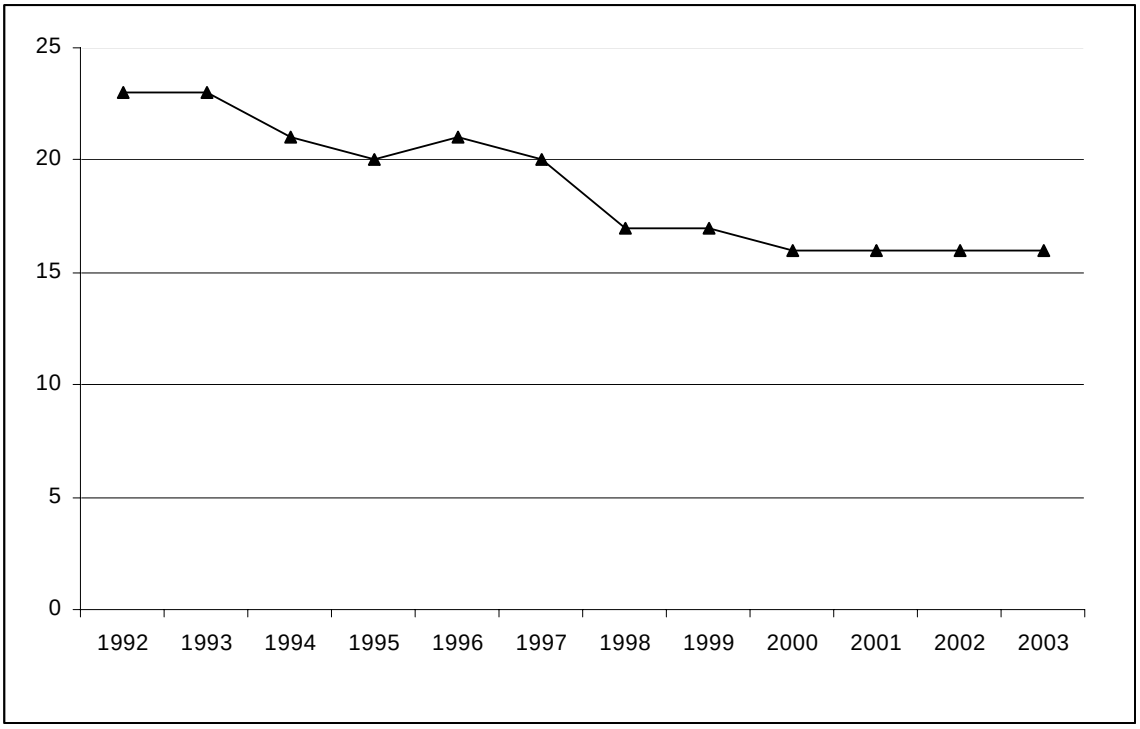

Figure 3-4. Abortion rates in the United States, 1992 – 2003.

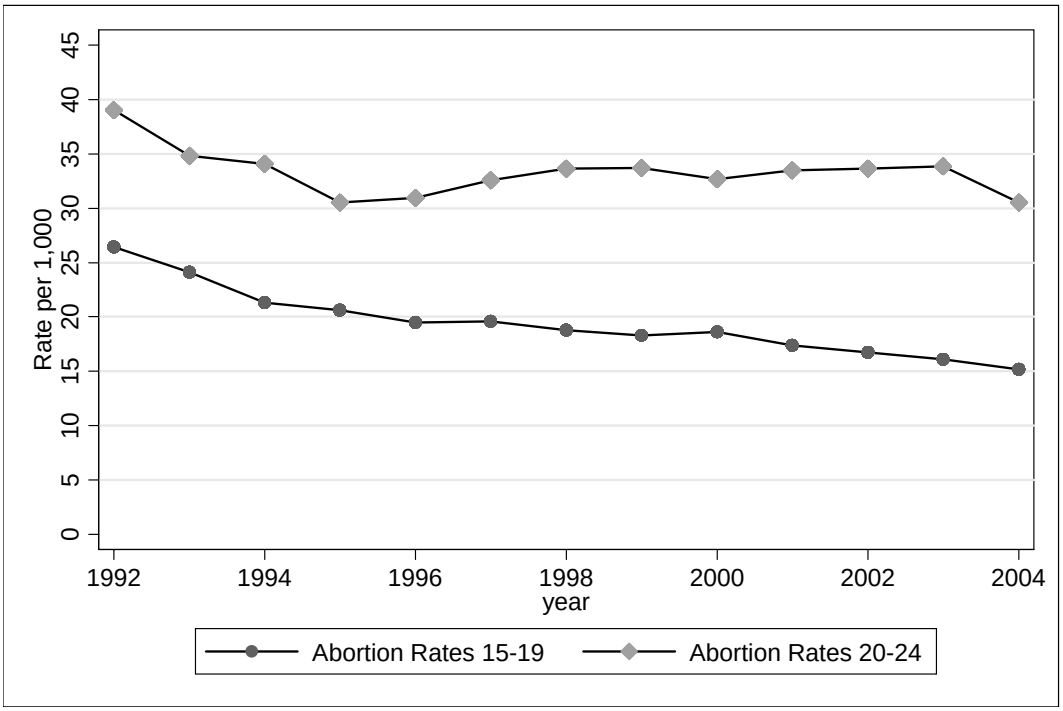

Figure 3-5. Abortion rates in Washington state, ages 15-19 and ages 20-24

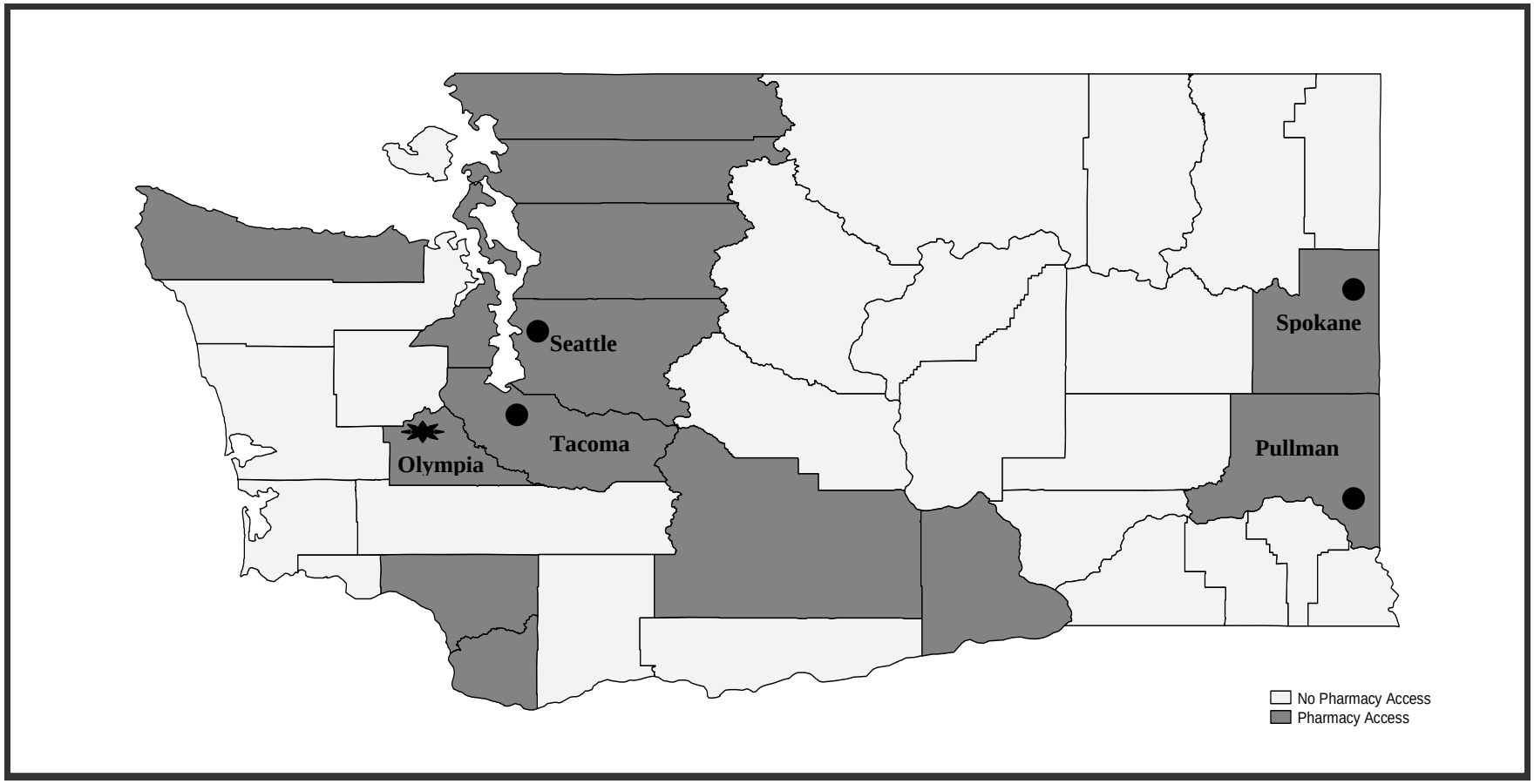

Figure 3-6. Washington state pharmacy access in 1998

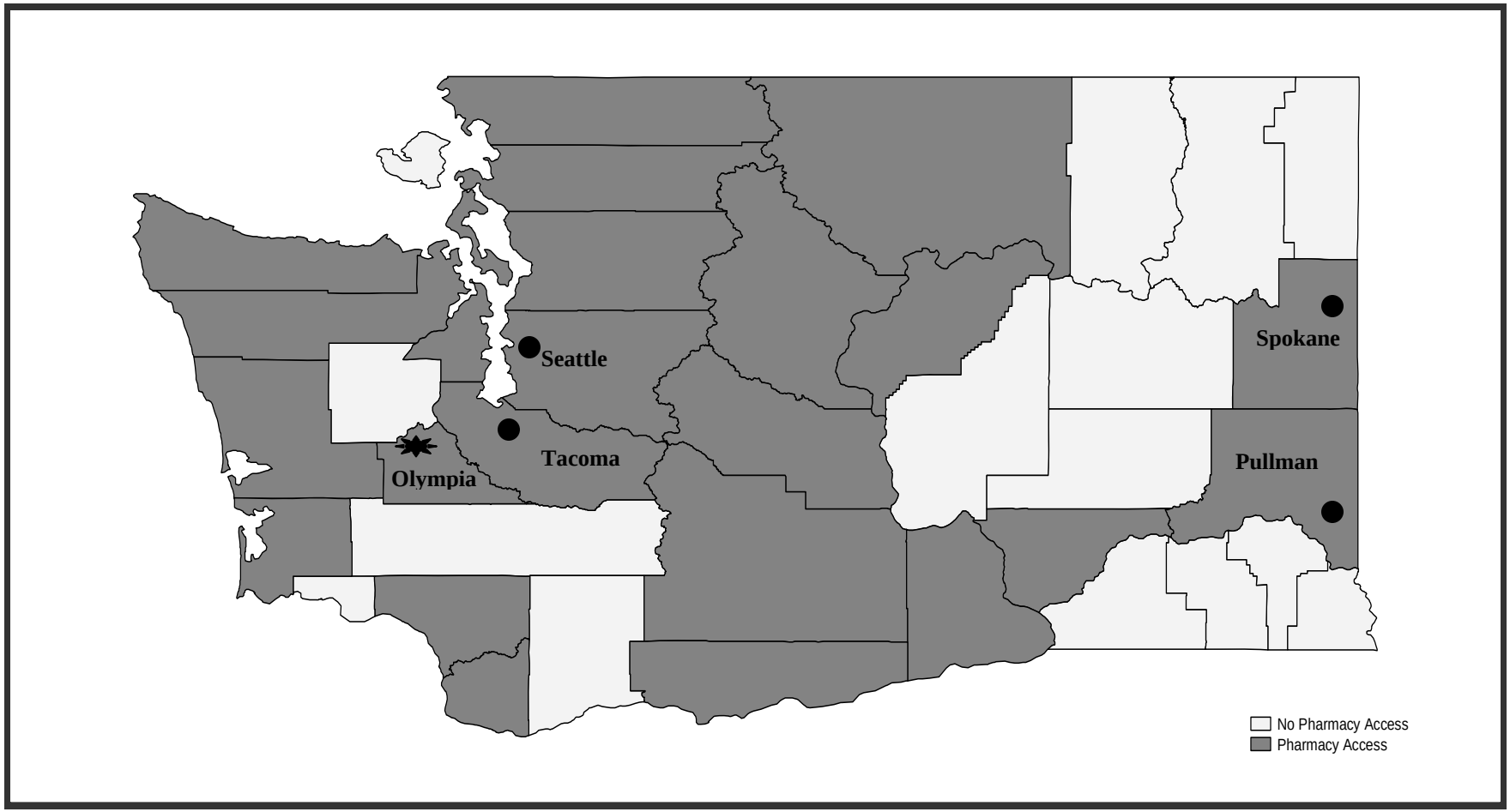

Figure 3-7. Washington state pharmacy access in 2002

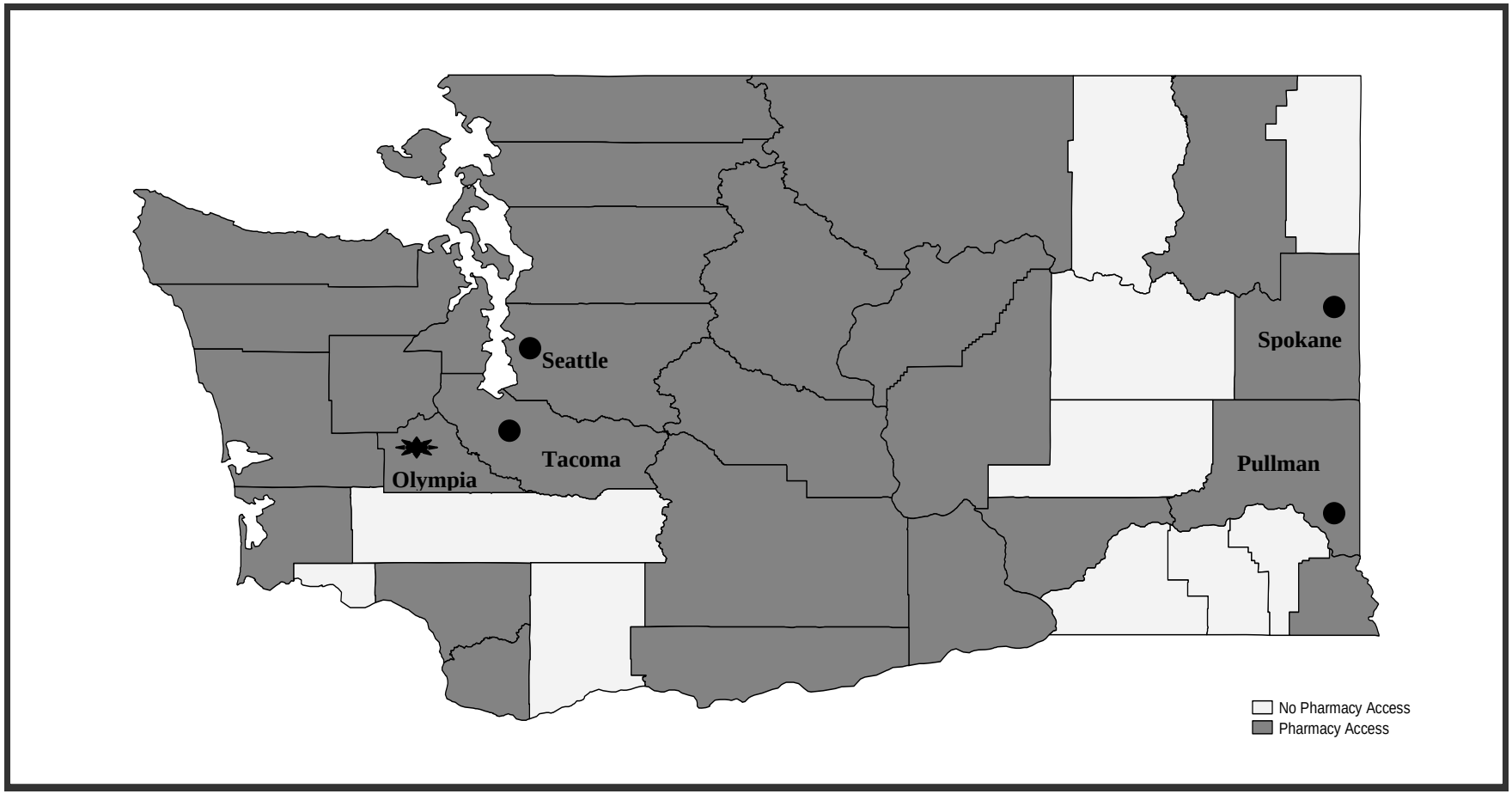

Figure 3-8. Washington state pharmacy access in 2005

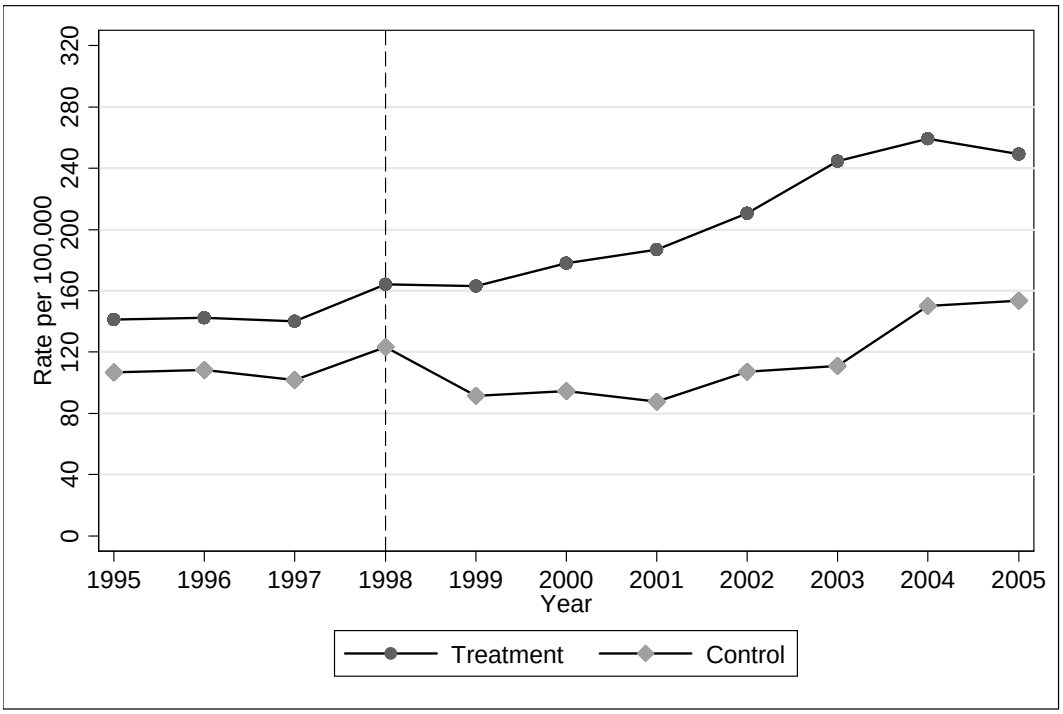

Figure 3-9. Overall chlamydia rates by treatment and control group

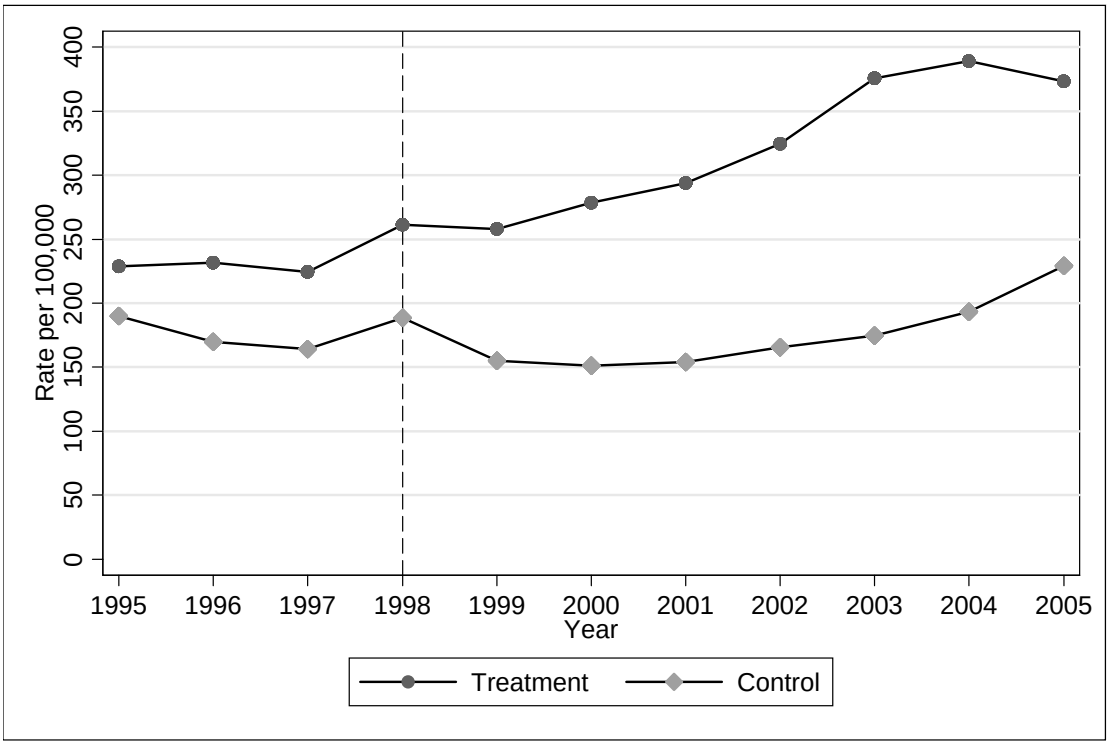

Figure 3-10. Female chlamydia rates by treatment and control group

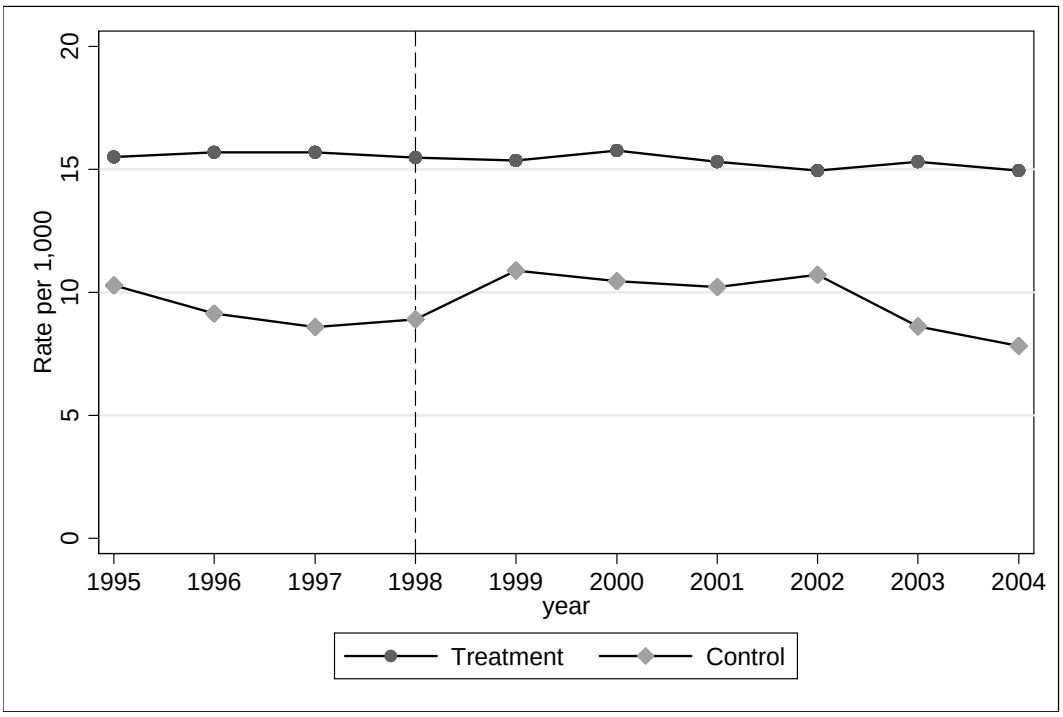

Figure 3-11. Overall abortion rates (age 15-44) by treatment status

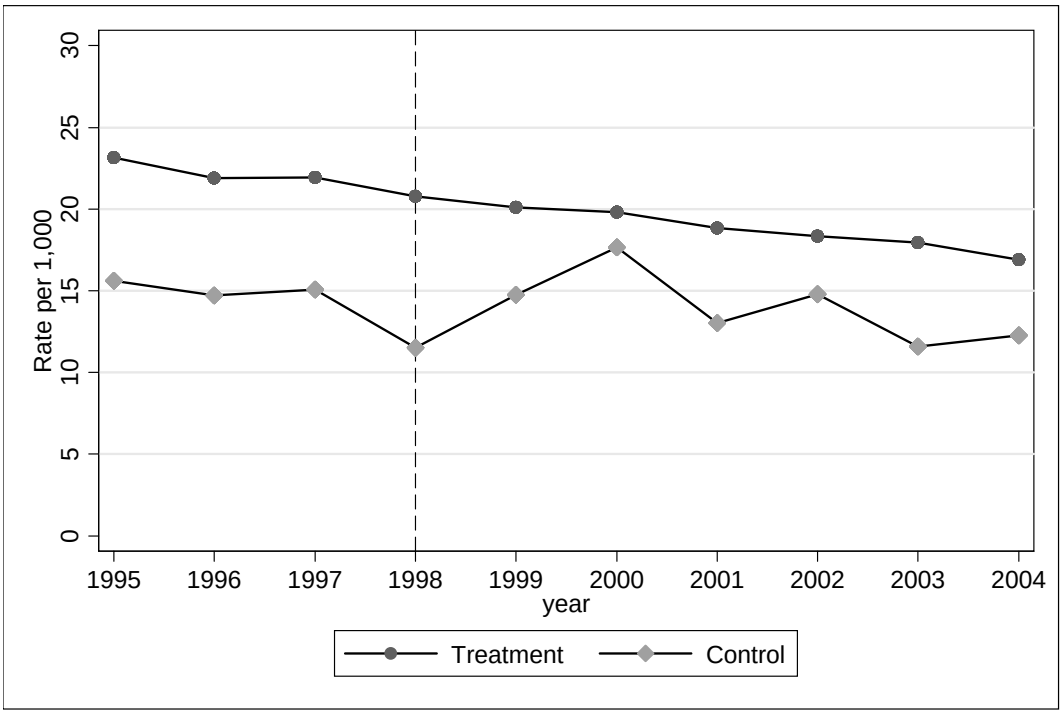

Figure 3-12. Abortion rates (age 15-19) by treatment status

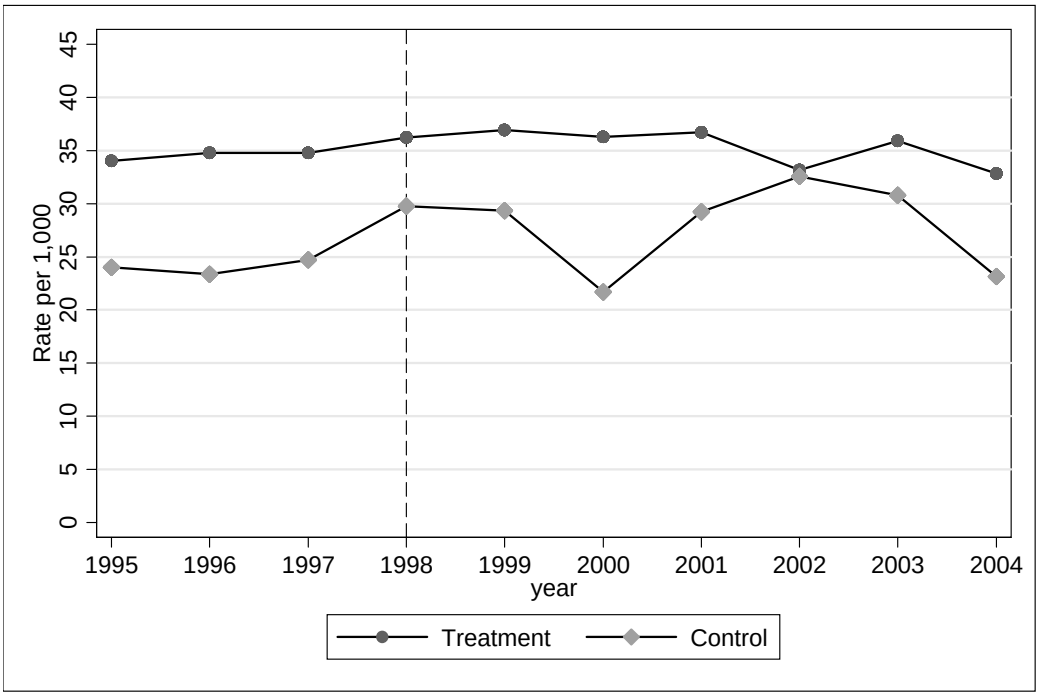

Figure 3-13. Abortion rates (age 20-24) by treatment status

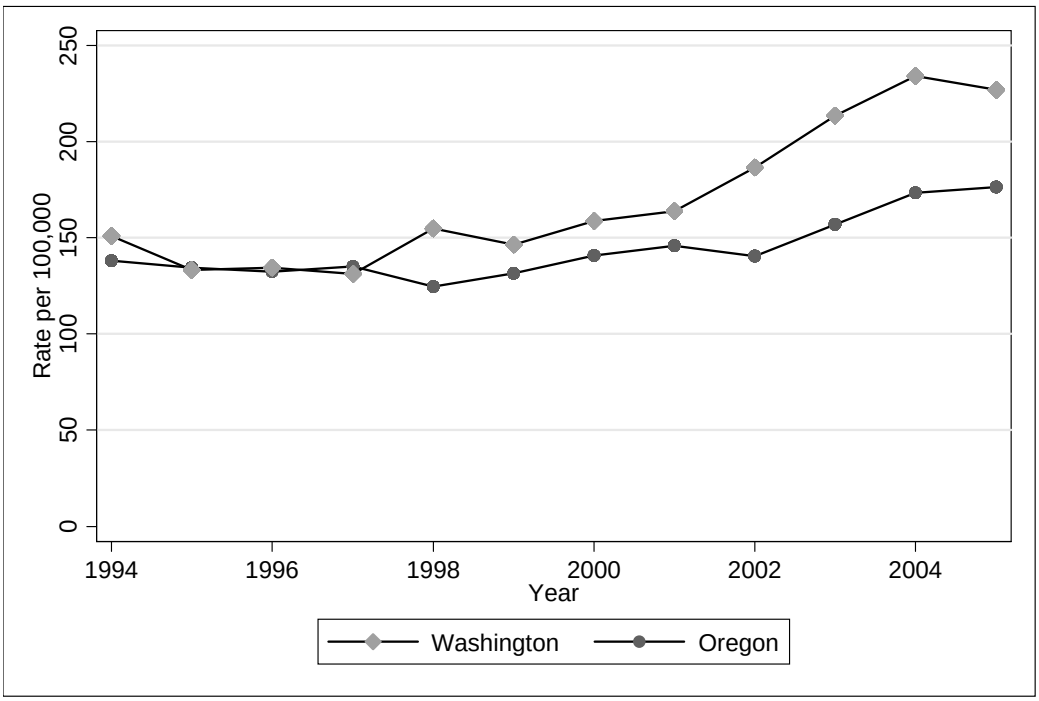

Figure 3-14. Overall chlamydia rates, Washington and Oregon

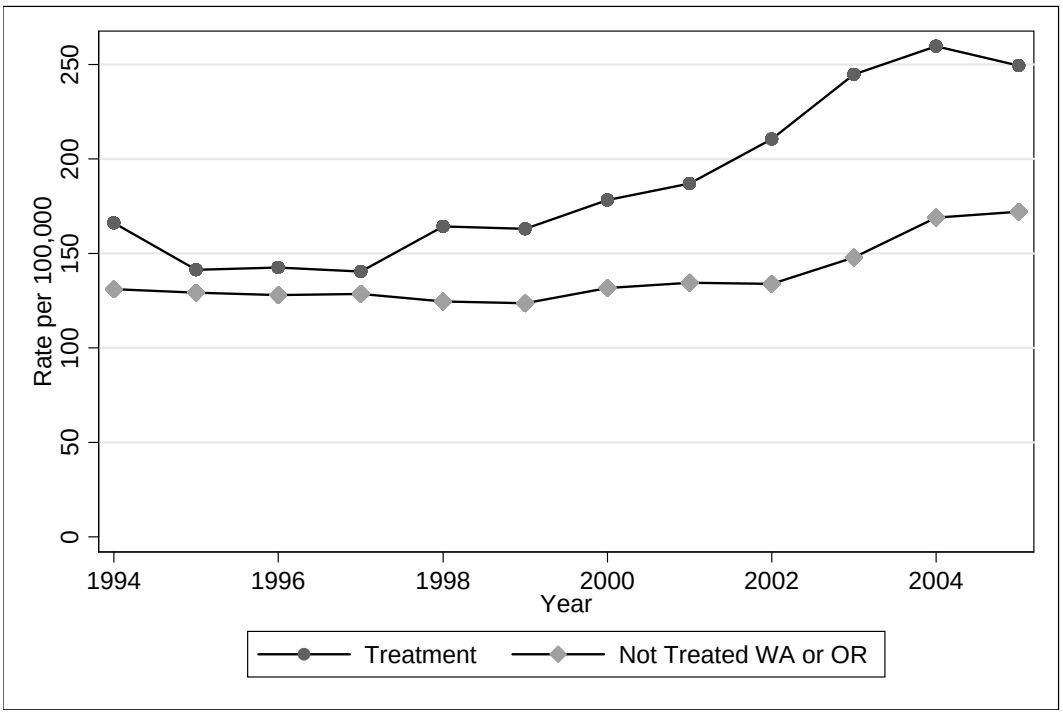

Figure 3-15. Overall chlamydia rates by treatment status

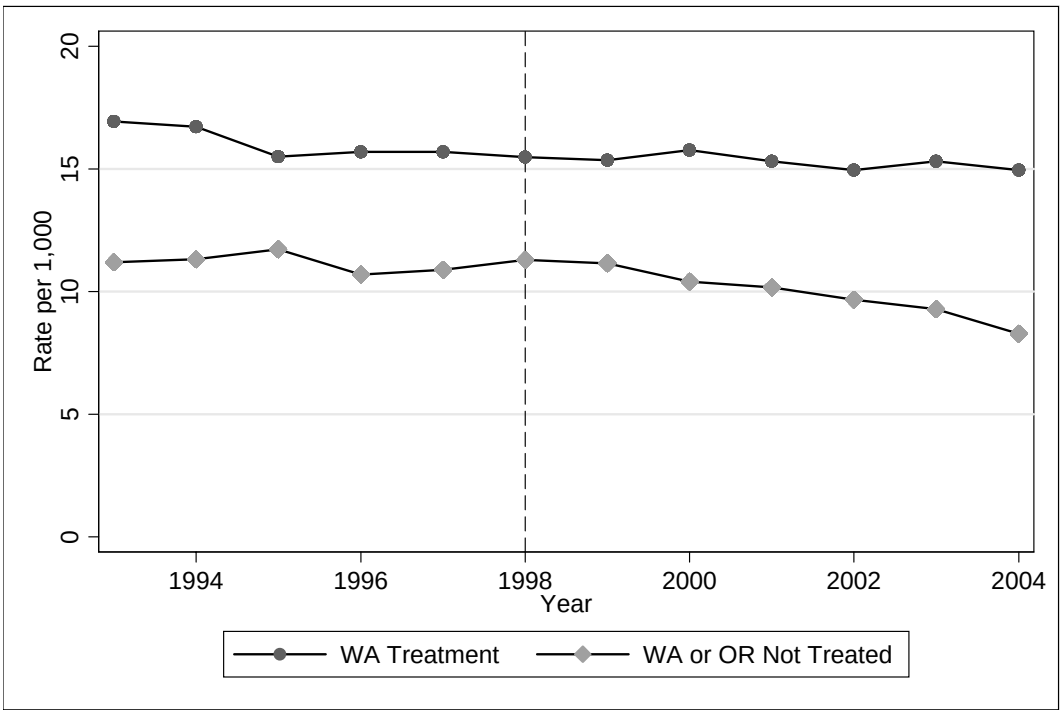

Figure 3-16. Abortion rates, Washington and Oregon

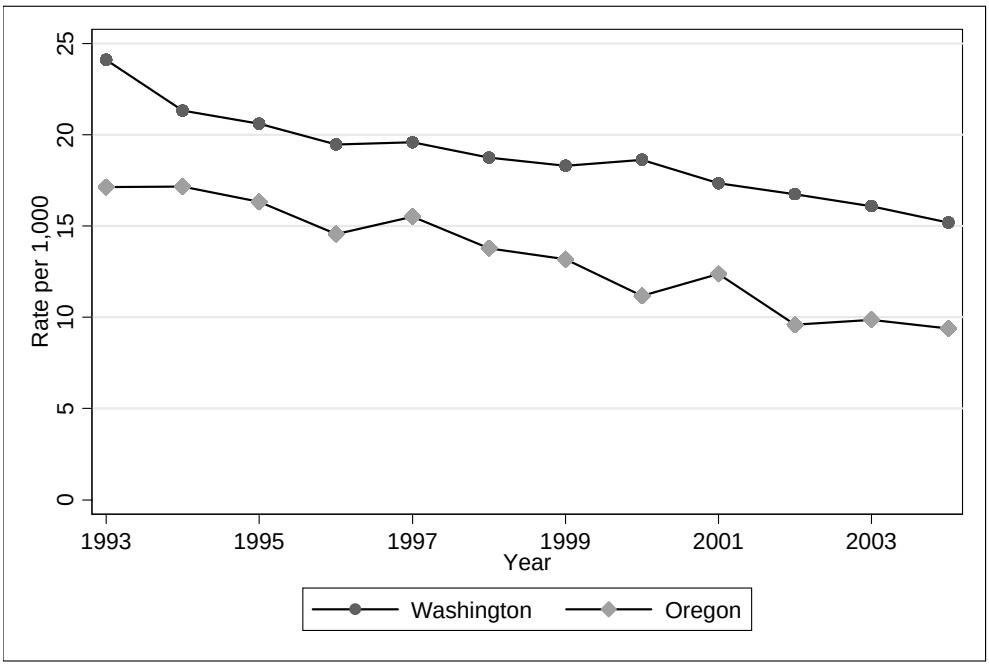

Figure 3-17. Abortion rates 15-19, Washington and Oregon

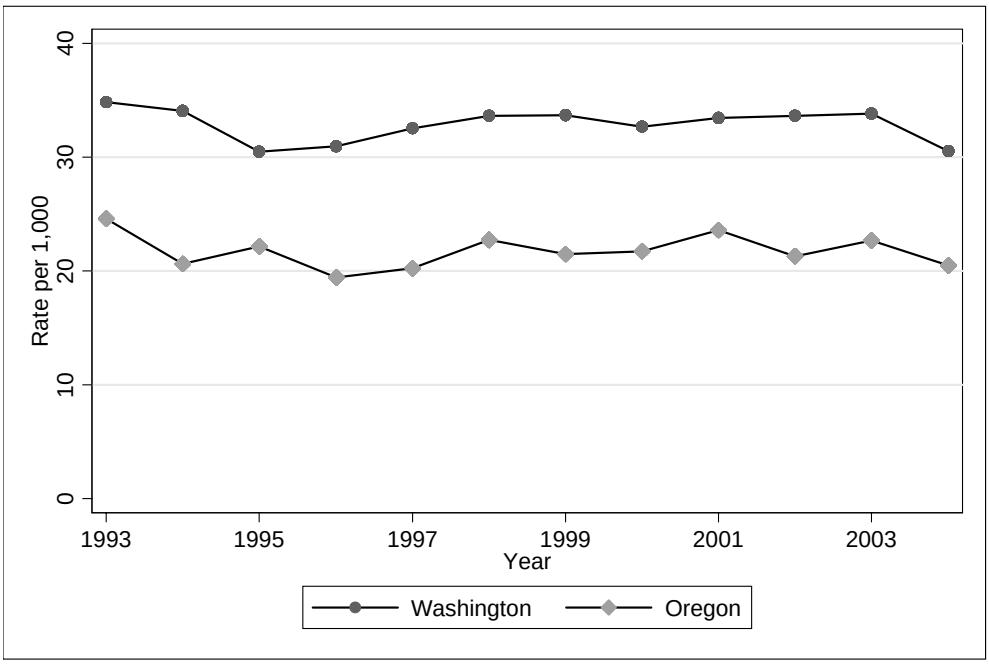

Figure 3-18. Abortion rates 20-24, Washington and Oregon

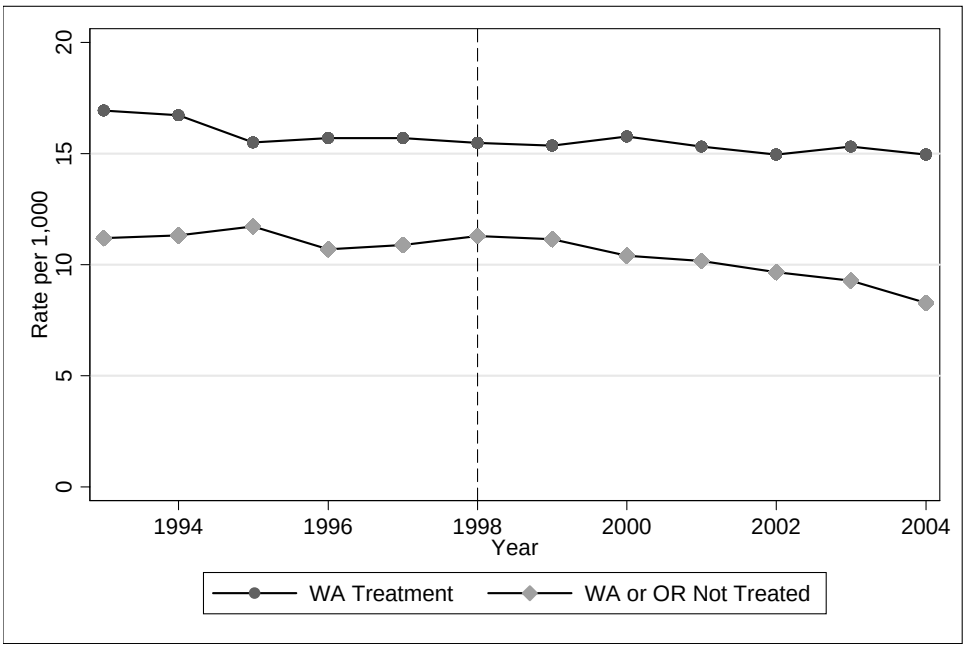

Figure 3-19. Abortion rates by treatment status

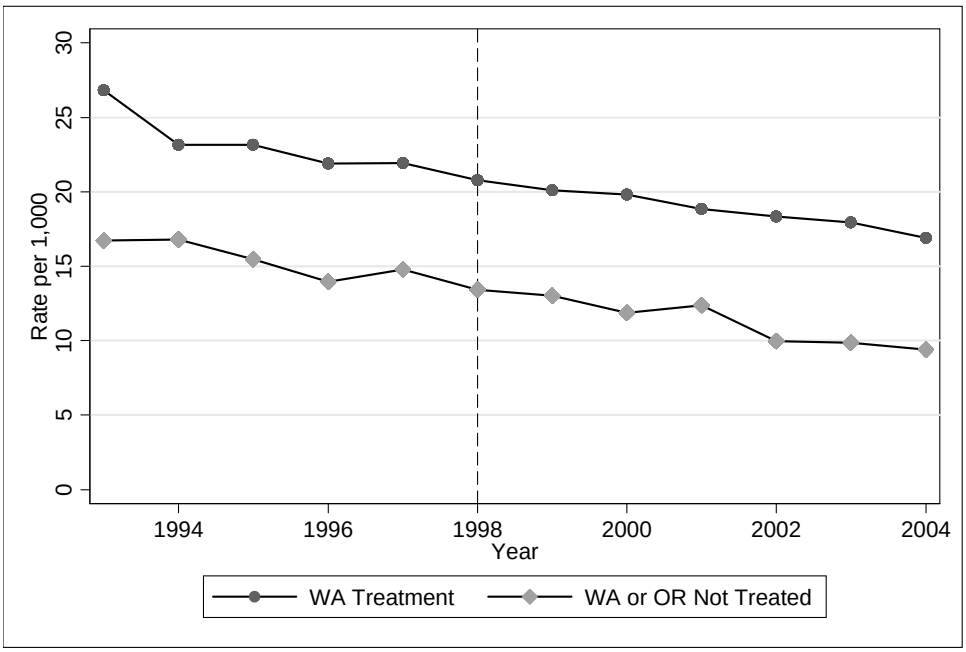

Figure 3-20. Abortion rates 15-19 by treatment status

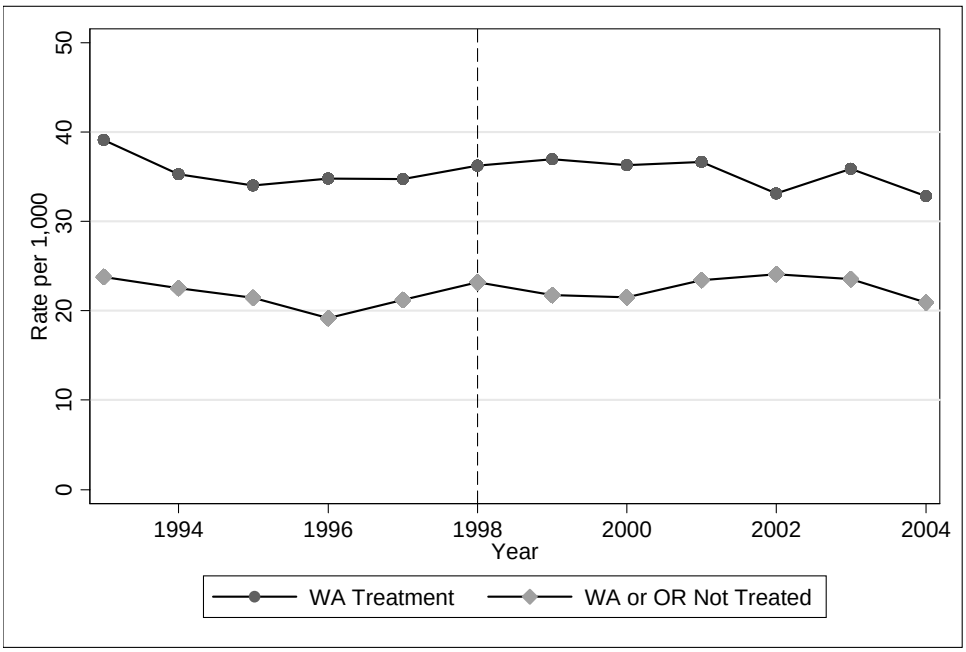

Figure 3-21. Abortion rates 20-24 by treatment status

CHAPTER 4
THE IMPACT OF PHARMACY-SPECIFIC ANY-WILLING-PROVIDER LEGISLATION ON PRESCRIPTION DRUG EXPENDITURES

Introduction

In recent years, many states have implemented Any-Willing-Provider (AWP) legislation. This form of health care legislation requires a managed care organization (MCO) to accept any provider who agrees to the MCO's reimbursement rates, terms, and conditions. A provider in this context could be a physician, hospital, or pharmacy. Proponents argue that AWP laws increase network size, expand patient choice, and increase competition among providers. Opponents believe that AWP legislation prevents managed care organizations from selectively contracting and obtaining discounts by offering providers a larger volume of patients. Additionally, administrative costs could be affected simply due to the difficulties of dealing with a larger number of pharmacies, physicians, or hospitals in the network. Such legislation, therefore, may prevent MCOs from effectively reducing health care costs as much as they might otherwise.

A small literature exists on the effect of AWP laws on several categories of health care expenditures including total health care expenditures, hospital expenditures, and physician expenditures. Most studies, however, fail to recognize that the vast majority of the existing AWP laws target pharmacies exclusively, as opposed to more comprehensive laws that apply to some combination of physicians, hospitals, and pharmacies. If AWP legislation limits cost reductions available through selective contracting, then states with such legislation may incur higher health care expenditures. There are additional potential consequences of AWP legislation, which related mainly to physicians and hospitals. For example, some opponents argue that AWP forces HMOs to contract with physicians who might be "higher-cost" in that they provide relatively inferior care or use excessive resources. If MCOs must now contract with physicians who might not comply with the MCO's treatment philosophy, then health care costs could increase. Arguments

of this type obviously cannot apply to pharmacies since pharmacists do not have prescribing authority.

Given that most AWP laws focus exclusively on pharmacies, it is relevant to consider the impact of these specific types of AWP laws. For pharmacies specifically, AWP laws not only increase patient choice but also allow independent pharmacies to compete with large chain pharmacies. If AWP laws prevent the MCO from selectively contracting with specific pharmacies or if AWP laws raise administrative costs in dealing with pharmacies for the MCO, then it is possible that expenditures on prescription drugs could increase. The effect of pharmacy AWPs is distinct from the effect of more comprehensive laws. My study is the first to analyze the impact of pharmacy-specific AWP legislation on state-level prescription drug expenditures per capita. I find that AWP legislation is associated with increases in pharmaceutical drug expenditures per capita. This result is robust to several alternative specifications. Additionally, I find evidence consistent with other studies in terms of the relationship between AWP and health care expenditures as well as the relationship between HMO market share and health care expenditures.

Managed Care and Any-Willing-Provider Legislation

Managed Care and Health Maintenance Organizations

In the early 1980s, managed care was a new concept on the health care front. Enrollments in managed care increased dramatically during the 1980s, and continued to gain popularity through the 1990s. Today, managed care is the predominant form of health care in the United States. Managed care is a broad term that encompasses several types of health care plans such as Health Maintenance Organizations (HMOs) and Preferred Provider Organizations (PPOs). HMOs, the more restrictive type of plan, usually require that all services be authorized by an in-network primary care physician, otherwise known as a gatekeeper. In contrast, PPOs offer

enrollees lower cost sharing when using services within some preferred provider list and higher cost sharing when using services outside the preferred provider network. PPOs do not have formal gatekeepers. These forms of managed care plans are in contrast to what was traditionally known as an indemnity plan or a Fee-For-Service (FFS) plan. A FFS plan would permit its enrollees to purchase medical services from any provider of their choice. The providers submit a claim to the insurance company and all covered claims would be paid. A FFS plan has no gatekeepers and no restrictions on medical services or on choice of provider, although FFS subscribers are subject to policy limits.

The conventional belief is that managed care lowers health care spending through various cost containment strategies. This decrease in spending is dependent mainly upon selective contracting which allows MCOs to obtain volume discounts. By promising providers a certain volume of patients, managed care organizations are able to negotiate volume discounts. Additionally, because MCOs limit their provider networks, they reduce the number of providers with whom they contract. This can reduce administrative expenses. In these ways, states with higher HMO enrollments are believed to have lower health care spending.

But HMO presence does not necessarily reduce health care spending in all areas. Managed care stresses the importance of well patient visits as well as preventive care. One goal of managed care is to reduce hospital expenditures by substituting less expensive physician visits and other preventive services for more costly hospital stays. Increased HMO presence is associated with a shift in expenditures from hospitals to physicians. This substitution is also found between hospitals and prescription drugs. In a 1998 study, Cutler and Sheiner report that HMO enrollment is associated with a decrease in hospital spending growth, but increases spending growth for physicians' services and prescription drug expenditures.

Any-Willing-Provider Legislation

In the 1980s and again in the 1990s, a new form of legislation appeared in the United States. Many states passed any-willing-provider laws that require managed care organizations to accept any provider into their network if the provider agrees to the conditions, terms, and reimbursement rates. Managed care organizations have come under criticism for a variety of reasons. One consequence of a managed care organization's cost reducing strategies is the restriction in provider choice. Accordingly, proponents of AWP argue that such legislation will increase the number of available providers in a network and thereby increase competition among providers.

The main mechanism through which managed care organizations are able to constrain costs is through limited provider networks, selective contracting, and volume discounting. As such, in order for managed care organizations to be effective at cost containment, they must be able to negotiate volume discounts by committing to a larger volume of patients to each provider. AWP prevents a managed care organization from being able to offer providers a much larger patient base and therefore diminishes its ability to selectively contract. Additionally, if managed care organizations are required to accept any provider who agrees to its terms into its network, then it will have less control over the quality of care and the types of providers with whom it collaborates. This could force a managed care organization to contract with providers that use relatively excessive medical resources or provide relatively inferior care. A final concern involves administrative or transactions costs, which increase with the number of providers with whom the managed care organization contracts.

Any-willing-provider legislation varies across states and over time. AWP laws are heterogeneous in their application. For example, some AWP laws target hospitals, physicians, pharmacies, or some combination of the three. The focus of my paper centers on AWP

legislation that targets pharmacies specifically. For the remainder of what follows, we will focus on the AWP laws enacted in states that focus on pharmacy providers specifically, while controlling for effects of other types of AWP laws.

Many states have enacted some form of any-willing-provider legislation. Most of these laws were passed in the 1990s, but some were passed earlier. Table 4-1 describes the states that passed pharmacy-specific any-willing-provider laws as well as those which have other variants of the legislation. Twenty-six states have AWP laws in place, with 23 states having laws which apply to pharmacies, and 15 of which apply *only* to pharmacies.

Previous Literature

A small literature examines the effects of any-willing-provider legislation. In particular, a study by Vita (2001) considers the impact of AWP legislation on general health care expenditures. Using state-level per capita expenditures for total health care spending, hospital care, and physician care, Vita considers the relationship between AWP laws and personal health care expenditures. He categorizes the laws as ranging from weak to moderate to strong, depending on their application. He finds that states with AWP legislation have higher per capita total health care expenditures controlling for demographic factors and state trends. He also finds some evidence that AWP laws are associated with increases in hospital care spending.

Most AWP laws are applicable to only pharmacies, meaning that HMOs in states with pharmacy AWPs must admit any pharmacy that agrees to its terms, conditions, and reimbursement rates into the network. Other states have physician or hospital AWP laws, which require contracting with those types of providers. Given the overwhelming presence of pharmacy-specific laws, it is important for us to understand the effects of pharmacy-specific legislation on health care spending. Prior studies have failed to consider these types of laws specifically or to consider the effect of AWP laws on pharmaceutical expenditures. To fill in this

gap in the literature, my paper considers the impact of pharmacy-specific AWP legislation on pharmaceutical health care expenditures.

Data

Health Care Spending

The Centers for Medicare and Medicaid (CMS) publish state-level health expenditures for specific health accounts and medical products, such as hospital care, physician services, prescription drug expenditures, nursing home care, dental care, and the like. These data are available by state for a panel of years and are based on the state of the provider.[93] In other words, these data are based on the state where the services were received rather than the state where the health care consumer resides. I use state-level observations on medical expenditures per capita for the years 1987 through 1998.[94] While there are many categories of health care spending, the main focus in my paper will be on pharmaceutical expenditures. Additionally, I show results for total health care expenditures, physician services, and hospital care in order to compare my results with Vita (2001). For the purposes of this analysis, state of provider data are preferred to the state of residence data.[95] State of provider data distinguishes among prescription drug expenditures, nonprescription drug expenditures, and other nondurable medical expenditures. State of residence data, however, group these three into one category. We might be concerned, however, about using state of provider data when analyzing hospital care or physician services

[93] Data are also available based on State of Residence, but the panel of data available is shorter, 1991-1998.

[94] State of provider data are available as early as 1980, but due to the availability of some other variables, including HMO enrollment and market share, Medicare enrollment, Medicaid enrollment, and the number of insured individuals , I am able to utilize state of provider data only as early as 1987.

[95] State of residence data exist for 1991 through 1998, but do not distinguish between prescription drugs and nonprescription drugs and medical sundries.

expenditures. For example, some individuals may cross state lines to receive some forms of medical treatment like a surgery at a renowned hospital or an office visit with a specialist. But it is less likely, however, that many individuals cross state lines in order to obtain prescription drugs. Pharmaceutical expenditures, therefore, are unlikely to be affected significantly by the state of provider characterization. Additionally, using data based on state of provider makes it possible to utilize five additional years of data for every state.

Health care expenditures have increased dramatically since 1987. Real total health care spending per capita[96] increased 49 percent between 1987 and 1998. Similarly, hospital care and physicians' services expenditures per capita have increased 33 percent and 50 percent, respectively, between 1987 and 1998. Expenditures on prescription drugs per capita more than doubled between 1987 and 1998, an increase of 119 percent. Figure 4-1 illustrates the pattern of expenditures per capita over time for three categories: hospital care, physicians' services, and prescription drugs.

Health Maintenance Organization Presence

One of the essential factors in this analysis is a measure of HMO presence. Relevant data on HMO enrollments and HMO penetration rates were obtained through Forte Information Resources.[97] Aventis Pharmaceuticals sponsors a yearly survey of HMOs whose results are summarized in an annual publication called the *Managed Care Digest Series*, managed and published by Forte.[98] These publications are available for the years 1986 through 2004. I utilize

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[96] All dollar figures are adjusted to 1998 dollars.

[97] Data published in each digest were collected by SMG Marketing – Verzipan LLC, a health care consulting firm which also conducts market research. Data were gathered mainly by mail and telephone surveys.

[98] *Managed Care Digest Series, HMO-PPO/Medicare-Medicaid Digest*, Aventis Pharmaceuticals, relevant years.

HMO data for 1987 through 1998. These publications contain state-level information on the number of HMOs serving each state, the total HMO enrollment, and HMO penetration. In the analysis, I use HMO enrollments to calculate HMO market shares as well as HMO penetration rates. HMO market share is calculated as the total HMO enrollment divided by the number of covered lives.[99] HMO penetration is defined as the total HMO enrollment divided by the state population.

HMO enrollment has changed dramatically between 1987 and 1998. In 1987, average HMO market share in the United States was 11.6 percent. By 1994, HMO market share had increased to 20.2 percent. In 1998, HMO market share reached 33 percent.

Sample

Data for HMO enrollment and market share are only available as early as 1986. Medicare enrollment, Medicaid enrollment, and the number of covered lives are only available for as early as 1987. The data used in this analysis, therefore, spans 1987 through 1998. The District of Columbia is omitted from this analysis. The sample is composed of 50 states over 12 years for a total of 600 observations.

Empirical Methodology

To consider the effect of any-willing-provider legislation on health care expenditures, I implement a fixed effects model using ordinary least squares. The estimated model is defined by equation 4-1:

$$Exp_{it} = \alpha_0 + \beta_1 AWP_{it} + \lambda X_{it} + \delta_i + \theta_t + \mu_{it} \qquad (4\text{-}1)$$

where *Exp* indicates the expenditures of the particular category of health care, *AWP* indicates whether the state has an AWP law in place in that year, *X* is a vector of state-level demographic

[99] Historical Health Insurance Tables, Table HI-4, U.S. Census Bureau.

and health characteristics, δ is a vector of state indicators, and θ is a vector of year indicators. Included in the vector of X covariates are the following variables: HMO market share, state-level unemployment rate,[100] real per capita income,[101] percentage of the population over the age of 65,[102] percentage of the population of African American race,[103] and population density.[104] In some specifications, as noted, I include the percentage of the population insured by Medicare and the percentage of the population insured by Medicaid. In these specifications, the percentage of the population over the age of 65 is omitted. I cluster my standard errors at the state-level and weight each observation according to the state population. Table 4-2 contains summary statistics for the relevant variables used in this analysis.

To begin, I estimate similar models to Vita (2001) to determine if the estimated effects of any AWP laws on total health care expenditures, physician services, and hospital care are comparable. To supplement that analysis and because most AWP laws apply only to pharmacies, I test the effect of pharmacy-specific AWP legislation on prescription drugs expenditures, while controlling for the laws affecting other providers. Since the majority of these types of legislation target pharmacies as opposed to doctors and hospitals, it is important to understand the consequences of these specific types of laws on health care expenditures.

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[100] Bureau of Labor Statistics, relevant years.

[101] Personal income per capita, Bureau of Economic Analysis, relevant years. Nominal dollars were inflated to 1998 dollars using changes in CPI-U from the Economic Report of the President.

[102] U.S. Census Bureau, relevant years.

[103] U.S. Census Bureau, relevant years.

[104] Statistical Abstract of the United States, relevant years.

Results

General Any-Willing-Provider Legislation

HMOs are said to be effective in reducing health care expenditures. One way this occurs is through selective contracting. AWP laws prevent an MCO from successfully negotiating volume discounts and therefore from reducing health care spending effectively. This section provides empirical evidence to support this claim.

Table 4-3 displays the results of regressions that consider any-willing-provider legislation in general. In these specifications, AWP identifies states that have either a law applying to pharmacies, hospitals, physicians, or some combination of the three providers. In this specification, AWP laws are associated with an increase in total health care expenditures. The magnitude of this effect is approximately $100 per capita. Relative to an average value of total health care expenditures over the time period of $3127, this accounts for a 3 percent increase in expenditures per capita. AWP laws are also associated with increases in expenditures on pharmaceutical drugs. The magnitude of this coefficient is approximately $15 per capita. Relative to average pharmaceutical spending of $220 per capita, this increase accounts for a 7 percent increase in pharmaceutical spending per capita. There is no evidence that physician services or hospital care expenditures are higher as a result of AWP legislation.

The results with respect to HMO market share are consistent with those of Cutler and Sheiner (1998). Increased HMO presence is associated with a reduction in hospital expenditures per capita. Although HMO market share is not statistically significant in the other three models, the signs of the coefficients are consistent with previous empirical evidence. All models were also estimated using HMO penetration rates in place of HMO enrollment rates. The results are consistent across both specifications.

Table 4-4 illustrates the results of a similar specification that includes the percentage of the population insured by Medicare and the percentage of the population insured by Medicaid, omitting the percentage of the population over the age of 65. These results are not fundamentally different from the results presented in Table 4-3. AWP legislation is associated with an increase in total expenditures and prescription drug expenditures. The magnitudes in Table 4-4 are quite similar to those in Table 4-3.

Some of the other covariates used in this analysis proved significant in some models. The state-level unemployment rate and real income per capita are associated with higher levels of health care spending. The percent of the population of African American race is positive and significant in some models, as is the Fraction of the population with Medicare. Population density, however, is not significant in any of the models.

Heterogeneous Application of Any-Willing-Provider Legislation

Any-willing-provider laws may apply to one or more of the following providers: pharmacies, hospitals, and physicians. Most of the current laws, however, apply only to pharmacy providers, while very few apply to hospitals or physicians or both. As a result, I estimate several specifications which take into account whether the state law applies to pharmacies or has other applications. Since the majority of these types of laws impact only pharmacies, the most relevant category to consider is expenditures on pharmaceutical drugs. Any effect on expenditures for hospital care or physician services should be less prominent. Table 4-5 utilizes Pharmacy AWP, which indicates if a particular state has an AWP law which applies to pharmacies *only*. Pharmacy Plus indicates states which have an AWP targeting pharmacies as well as another provider type. Hospital/Physician AWP indicate states which have AWP applying to hospitals or physicians, but not to pharmacies. These results are consistent with the results presented in Tables 4-3 and 4-4. Pharmacy-specific AWP laws are associated with an

increase in pharmaceutical drug expenditures per capita. Similarly, laws which target pharmacies as well as other providers are also associated with an increase in pharmaceutical drug expenditures per capita. Additionally, laws which apply to hospitals or physicians but not to pharmacies are associated with an increase in hospital care expenditures per capita, but not with a rise in physician services expenditures per capita. Reassuringly, these laws have no impact on pharmacy expenditures.

Policy Endogeneity & Robustness

Policy Endogeneity

I attempted several forms of instrumental variables which had been suggested in the literature, such as the percentage of firms defined as large, i.e., with more than 500 employees. Ohsfeldt et al (1998) examines the likelihood of a state to pass an AWP law based on the winners (providers, hospitals, pharmacies), the losers (MCOs, employers, and employees), and the political environment. [105] When considering all AWP laws, the only variable that predicts the enactment of the law is the number of hospital beds per capita. Other variables such as number of physicians or pharmacists per capita and measures of the political climate were not significant with respect to any-willing-provider law presence. Because AWP laws vary in their applicability across states, the authors categorize the laws by what entities they target. Their model works well for laws that are focused on hospitals, but does not perform well for laws which target pharmacies and physicians. Only one variable, the percentage of employers defined as "large," is related to the enactment of a pharmacy-specific any-willing provider law. Additionally, I utilized a measure of political control, since more conservative states tend to support and enact AWP legislation. Neither of these potential instrumental variables had any predictive power in the first

[105] This variable was also explored in an earlier study, McLaughlin (1987).

stage where AWP or pharmacy AWP was the dependent variable. This suggests a lack of valid instruments for the policy change.

Other variables used in the public choice studies to predict the enactment of AWP legislation, such as the number of physicians[106] or hospital beds per capita,[107] would certainly not be exogenous with respect to health care expenditures per capita. All of these instruments, therefore, are not suitable as instruments in this analysis.[108]

It is still possible that AWP laws themselves are endogenous with respect to health care expenditures per capita. If states enact these laws as a reaction to changes in health care spending, then any estimated effect of AWP legislation will capture not only any change in spending, but also any trend that was already occurring. If this is true, then any estimated effect of AWP legislation would be biased upwards. If the likelihood that a state would enact an AWP law is constant over time, then some of this potential bias is captured within the state fixed effects.

To consider the possibility that estimated relationships in the previous section are spurious, I examine state-specific trends before and after the change in legislation. I create a piece-wise linear function or spline. For each state, I define *year of adoption* as the year in which the state adopted a pharmacy-specific AWP law. The first segment of the piece-wise linear function captures the beginning of the data period to the year of law's adoption. The second segment represents the year of adoption through the end of the data period. If AWP legislation is associated with a change or shock with respect to health care expenditures, then we would see a

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[106] Marsteller et al (1997).

[107] Ohsfeldt el al (1998).

[108] Bound, Jaeger, and Baker (1995); Staiger and Stock (1997).

change in the slope or trend line. To consider this potential change, I estimate separate regressions for each state enacting pharmacy-specific AWP laws by regressing the two slope coefficients on pharmaceutical expenditures per capita. I then test the difference between these two slopes (slope1 = slope2) to determine if there is a statistically significant difference.

Table 4-6 reports the results of this approach. Each row is a separate regression and columns (1) and (2) report the estimated slope coefficients. Column (3) reports the F-statistic for the restriction that the two coefficients are equal. This trend analysis works well for twelve of the fourteen states with pharmacy-specific AWP legislation. In only two cases are the two slope coefficients not statistically different. It appears from this component of the analysis that the relationship between pharmacy-specific AWP legislation and pharmaceutical expenditures per capita is not spurious. There does appear to be a change in the trend of expenditures with respect to prescription drugs when AWP laws are adopted.

Robustness & Sensitivity Analysis

Another form of legislation targeted at managed care organizations is freedom of choice (FOC) legislation. FOC legislation allows managed care subscribers to access providers outside the managed care network, for a different fee but without having to pay the full price of care. FOC laws could increase health care costs in a similar manner to AWP laws.

While my paper focuses exclusively on the effect of AWP legislation on health care expenditures, in what follows I add an indicator for FOC as an additional explanatory variable. The results presented in Table 4-7 and 4-8 suggest that FOC are not associated with a change in health care expenditures per capita, for any of the four categories, and the signs and significance on the AWP coefficients are similar to the results without an FOC indicator.

Conclusion

While the growth in the number and enrollment of health maintenance organizations has reduced health care spending in some areas, it has done so through cost containment strategies such as limiting patient choice. One political movement aimed at giving consumers of health care more choice has resulted in the enactment of any-willing-provider legislation. Because such state laws require a managed care organization to contract with any provider who is willing to submit to the plan's terms, conditions, and reimbursement policies, patients by definition will experience more choice within their managed care network. A potential side effect of such legislation is an increase in health care spending due to two main factors: (1) inability of managed care organization to selectively contract and (2) increases in administrative costs associated with contracting with more providers. Most any-willing-provider laws target pharmacies specifically, meaning that a managed care organization in a state with such a law would only be required to contract with any willing pharmacy, not any willing physician or hospital. As demonstrated through this analysis, pharmacy-specific AWP legislation is associated with an increase in pharmaceutical drug expenditures per capita. Additionally, any-willing-provider laws in general are associated with increases in total personal health care expenditures (as well as pharmaceutical drug expenditures), a result which is consistent with prior findings.

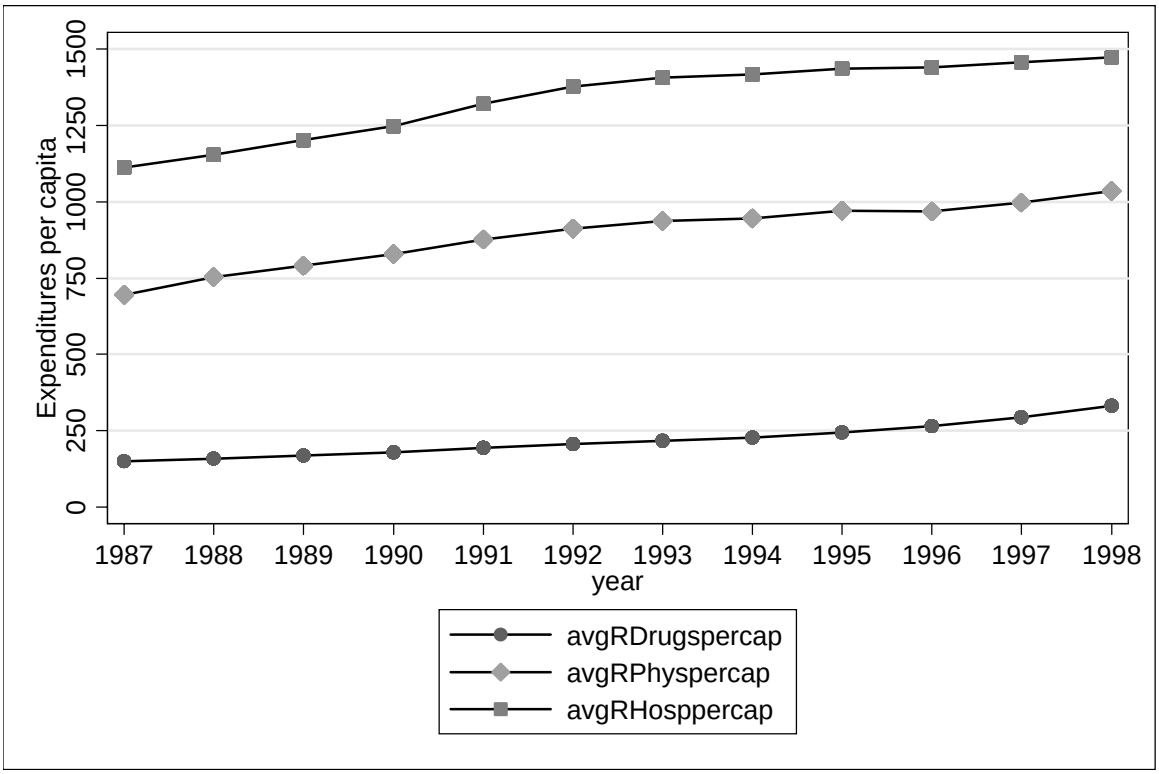

Figure 4-1. Expenditures per capita, 1987-1998.

Table 4-1. Description of state any-willing-provider (AWP) legislation

State	Year of Law	Applicability of Law
Alabama	1988	Pharmacy
Arkansas	1991; 1995	Pharmacy; Physician/Hospital
Connecticut	1982	Pharmacy
Delaware	1994	Pharmacy
Florida	1993	Pharmacy
Georgia	1983	Pharmacy, Physician, Hospital
Idaho	1994	Physicians, Hospital
Illinois	1985	Physician
Indiana	1994	Pharmacy, Physician, Hospital
Kansas	1994	Pharmacy
Kentucky	1994	Physicians, Hospital
Massachusetts	1995	Pharmacy
Minnesota	1994	Pharmacy
Mississippi	1994	Pharmacy
Montana	1991	Pharmacy, Hospital, Physician
New Hampshire	1992	Pharmacy
New Jersey	1994	Pharmacy
New Mexico	1987	Pharmacy, Physician
North Carolina	1993	Pharmacy
North Dakota	1989	Pharmacy
Oklahoma	1994	Pharmacy
South Carolina	1994	Pharmacy
South Dakota	1990	Pharmacy
Texas	1991	Pharmacy, Hospital, Physician
Virginia	1983	Pharmacy, Hospital, Physician
Wyoming	1990	Pharmacy, Hospital, Physician

Table 4-2. Summary statistics

Variable	Mean	Std. Dev.	Min	Max
Real Total Health Care per Capita	3127.51	559.35	1800.01	4912.64
Real Hospital Care per Capita	1271.50	215.30	708.48	1992.16
Real Physicians Services per Capita	878.09	188.03	459.11	1520.21
Real Prescription Drugs per Capita	219.87	63.02	105.26	436.85
HMO Market Share	19.06	14.62	0	74.75
HMO penetration	16.50	12.66	0	60.8
Any AWP	0.31	0.46	0	1
Pharmacy AWP	0.16	0.37	0	1
Pharmacy Plus AWP	0.11	0.31	0	1
Hospital/Physician AWP	0.04	0.18	0	1
Population Density	168.54	233.18	0.9	1093.8
Percent Over Age 65	12.54	2.09	3.34	18.59
Percent Black	9.83	9.30	0.28	36.41
Real per Capita Income	23,269	3,571	15,322	36,822
Fraction Medicare	12.99	2.22	4.33	19.55
Fraction Medicaid	9.85	3.42	1.90	22.16

Table 4-3. Expenditures per capita results

Variable Name	(1) Total	(2) Hospital	(3) Physician	(4) Drugs
AWP	106.44	36.31	23.51	14.90
	[34.98]***	[20.74]	[16.28]	[6.75]**
HMO Market Share	0.05	-2.89	0.72	0.44
	[1.81]	[0.89]***	[0.83]	[0.38]
Population Density	-1.85	-0.93	0.05	-0.01
	[1.92]	[0.95]	[0.28]	[0.23]
Percent Black	124.51	46.62	4.34	27.72
	[70.99]*	[31.96]	[14.54]	[16.17]*
Real income per capita	0.11	0.05	0.02	0.01
	[0.03]***	[0.02]***	[0.01]**	[0.01]*
Unemployment Rate	42.85	11.29	22.17	-0.24
	[9.97]***	[5.64]*	[4.90]***	[2.06]
Population Over 65	63.36	42.84	0.07	6.72
	[43.91]	[24.33]*	[22.64]	[9.61]
Observations	600	600	600	600
R-squared	0.98	0.96	0.97	0.96
State Fixed Effects	X	X	X	X
Year Fixed Effects	X	X	X	X

Clustered standards errors (at the state-level) in brackets; weighed by the state population.
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 4-4. Expenditures per capita results with fractions of government insurance

Variable Name	(1) Total	(2) Hospital	(3) Physician	(4) Drugs
AWP	103.51	34.11	23.78	14.67
	[35.88]***	[20.72]	[16.62]	[6.87]**
HMO Market Share	0.22	-2.63	0.62	0.45
	[1.73]	[0.83]***	[0.74]	[0.40]
Population Density	-1.67	-0.8	0.03	-0.079
	[1.93]	[0.96]	[0.29]	[0.23]
Percent Black	126.17	42.74	8.15	28.45
	[71.61]*	[33.09]	[14.69]	[15.93]*
Real income per capita	0.11	0.05	0.02	0.01
	[0.03]***	[0.02]***	[0.01]**	[0.01]*
Unemployment Rate	41.33	12.11	20.77	-0.59
	[10.36]***	[5.62]**	[4.77]***	[1.96]
Fraction Medicare	8.98	10.12	-4.49	-0.26
	[7.09]	[3.88]**	[3.22]	[1.25]
Fraction Medicaid	3.84	-0.99	1.96	0.39
	[4.79]	[1.92]	[2.37]	[0.73]
Observations	600	600	600	600
R-squared	0.98	0.96	0.98	0.96
State Fixed Effects	X	X	X	X
Year Fixed Effects	X	X	X	X

Clustered standards errors (at the state-level) in brackets; weighed by the state population.
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 4-5. Expenditures per capita results with heterogeneous applicability of AWP law

Variable Name	(1) Total	(2) Hospital	(3) Physician	(4) Drugs
Pharmacy AWP	108.5	28.89	27.05	14.65
	[40.04]***	[24.22]	[20.94]	[7.86]*
Pharmacy Plus AWP	10.53	13.67	4.89	12.03
	[28.10]	[18.11]	[7.27]	[6.55]*
Hospital/Physician AWP	179.03	80.82	41.65	21.12
	[91.33]*	[35.85]**	[29.70]	[11.51]*
HMO Market Share	0.04	-2.66	0.58	0.43
	[1.76]	[0.85]***	[0.74]	[0.40]
Population Density	-1.69	-0.404	-0.77	-0.079
	[1.95]	[0.355]	[0.94]	[0.23]
Percent Black	124.94	42.342	43.69	28.35
	[71.83]*	[32.856]	[33.07]	[16.17]*
Real income per capita	0.11	0.052	0.05	0.01
	[0.03]***	[0.017]***	[0.02]***	[0.01]*
Unemployment Rate	41.06	11.254	12.03	-0.64
	[10.34]***	[5.503]**	[5.55]**	[1.97]
Fraction Medicare	9.68	10.642	10.4	-0.2
	[7.06]	[3.831]***	[3.90]**	[1.26]
Fraction Medicaid	3.56	-0.839	-1.03	0.41
	[4.73]	[1.936]	[1.92]	[0.72]
Observations	600	600	600	600
R-squared	0.98	0.96	0.98	0.96
State Fixed Effects	X	X	X	X
Year Fixed Effects	X	X	X	X

Clustered standards errors (at the state-level) in brackets; weighted by the state population.
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 4-6. Spline regression results*

State	(1) Slope 1	(2) Slope 2	(3) F-statistic Slope 1 = Slope 2
Alabama	-9.01	15.78	1.69
Delaware	12.05	39.05	46.51
Florida	13.02	33.11	54.90
Kansas	10.84	21.95	36.42
Massachusetts	12.60	30.67	40.71
Minnesota	9.99	25.57	49.73
Mississippi	10.96	24.91	23.76
New Hampshire	10.19	19.03	5.57
New Jersey	16.88	33.81	42.32
North Carolina	8.31	24.18	42.59
North Dakota	7.71	14.36	2.69
Oklahoma	11.07	22.22	51.36
South Carolina	10.85	30.66	172.56
South Dakota	10.98	12.41	0.15

*Each row is a separate regression.

Table 4-7. Expenditures per capita results with freedom of choice (FOC) indicator

Variable Name	(1) Total	(2) Hospital	(3) Physician	(4) Drugs
AWP	97.5	26.1	26.03	16.6
	[34.49]***	[19.85]	[18.67]	[6.69]**
FOC	25.43	33.84	-9.54	-8.17
	[27.88]	[21.24]	[12.42]	[5.56]
HMOMarket	0.27	-2.57	0.61	0.44
	[1.72]	[0.82]***	[0.73]	[0.40]
Population Density	-1.73	-0.88	0.05	0.02
	[1.98]	[1.00]	[0.28]	[0.23]
Percent Black	128.07	45.27	7.43	27.84
	[71.64]*	[32.18]	[14.47]	[15.94]*
Real income per capita	0.11	0.05	0.02	0.01
	[0.03]***	[0.02]***	[0.01]**	[0.00]**
Unemployment Rate	40.91	11.55	20.93	-0.46
	[10.12]***	[5.24]**	[4.82]***	[1.86]
Fraction Medicare	8.15	9.02	-4.18	0.01
	[6.94]	[3.84]**	[3.01]	[1.26]
Fraction Medicaid	3.94	-0.86	1.93	0.36
	[4.74]	[1.80]	[2.35]	[0.75]
Observations	600	600	600	600
R-squared	0.98	0.98	0.98	0.96
State Fixed Effects	X	X	X	X
Year Fixed Effects	X	X	X	X

Clustered standards errors (at the state-level) in brackets; weighed by the state population.
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 4-8. Expenditures per capita results with heterogeneous applicability of AWP law and FOC indicator.

Variable Name	(1) Total	(2) Hospital	(3) Physician	(4) Drugs
Pharmacy AWP	100.84	18.19	29.93	16.84
	[38.08]**	[22.09]	[23.98]	[7.86]**
Pharmacy Plus AWP	14.33	18.99	3.45	10.95
	[29.46]	[19.21]	[7.95]	[6.24]*
Hospital/Physician AWP	179.81	81.91	41.36	20.89
	[94.91]*	[40.47]**	[28.42]	[10.73]*
FOC	26.8	37.45	-10.1	-7.65
	[28.06]	[22.64]	[13.65]	[5.99]
HMOMarket	0.11	-2.57	0.55	0.41
	[1.75]	[0.84]***	[0.73]	[0.40]
Population Density	-1.75	-0.85	0.04	0.01
	[2.00]	[0.98]	[0.29]	[0.23]
Percent Black	127.47	47.22	6.73	27.63
	[71.96]*	[32.01]	[13.86]	[16.21]*
Real income per capita	0.11	0.05	0.02	0.01
	[0.03]***	[0.02]***	[0.01]**	[0.00]*
Unemployment Rate	40.66	11.47	20.89	-0.53
	[10.07]***	[5.17]**	[4.92]***	[1.87]
Fraction Medicare	8.79	9.14	-4.00	0.06
	[6.86]	[3.89]**	[2.96]	[1.26]
Fraction Medicaid	3.7	-0.83	1.85	0.37
	[4.69]	[1.80]	[2.36]	[0.73]
Observations	600	600	600	600
R-squared	0.98	0.96	0.98	0.96
State Fixed Effects	X	X	X	X
Year Fixed Effects	X	X	X	X

Clustered standards errors (at the state-level) in brackets; weighted by the state population.
* significant at 10%; ** significant at 5%; *** significant at 1%

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BIOGRAPHICAL SKETCH

Christine Ann Piette graduated from Emory University with a Bachelor of Arts in economics. She earned her degree with highest honors after completing and defending a thesis on the labor market effects of earning a GED versus a high school diploma. After completing her degree at Emory, Christine worked as a research assistant for a management consulting firm in Tallahassee, Florida for one year.

In 2003, Christine began the graduate program at the University of Florida in the Department of Economics. After two years of coursework and successful completion of field examinations, Christine earned her Master of Arts in economics. Both during her coursework and in the two years following, Christine was employed by the department as both a research assistant and a teaching assistant. Additionally, Christine served as a teaching assistant for an executive level MBA course during two consecutive years. In the fall of 2006, Christine was an instructor for an upper-level elective, government regulation of business, within the economics department. After successful completion of all requirements for her degree, Christine will earn the Doctor of Philosophy degree in August 2007. She has accepted an assistant professor position at the University of North Carolina at Chapel Hill where she will begin in the fall of 2007.