

## More Evidence on the Quality-Quantity Trade-off in Medical Care \*

Anca M. Cotet†

Ball State University

March 2010

### **Abstract**

This paper investigates the impact of the regulation prohibiting physicians from prescribing drugs without a prior physical examination on health outcomes. This requirement should improve health by reducing illegal access to prescription drugs. On the other hand, it hampers the practice of physician-patient telemedicine, a service evaluated by most previous studies to be of somewhat lower quality but effective in improving access to care. The empirical results suggest that this regulation leads to an increase of approximately 0.2 in the expected monthly number of days lost to illness and 0.4 percent increase in mortality rates the equivalent of 33 more deaths per 1 million people. The magnitude of the impact is larger in rural areas, and in areas with low physician density.

JEL Classification: I18, K32

Keywords: telemedicine, medical care quality

---

\* Special thanks Cotton M. Lindsay, Michael Maloney, Andryi Protsyk, Gary Santoni, Neeraj Sood, Lee Spector, Kevin K. Tsui, and Robert D. Tollison for valuable criticism. I also gratefully acknowledge the helpful comments I received from the participants to the presentations of this paper.

† Department of Economics, Ball State University, WB 201, 2000 W. University Ave., Muncie, IN, 47304. E-mail: amcotet@bsu.edu, Phone: 1-765-285-3724, Fax: 1-765-285-4313

## Introduction

The health-care industry has experienced dramatic technological improvements over the past several decades. As medical technologies advance and new services are offered, policies are proposed to reinforce previous regulations, a process that draws attention to the question of whether old regulations are still optimal. This paper investigates the impact of one type of regulation affecting the practice of physician-patient telemedicine: that prohibiting physicians from prescribing drugs without a prior physical examination (i.e., physical examination requirement or *PER* hereafter). These regulations restrict the provision of physician-patient telemedicine.<sup>1</sup>

Technological progress stimulated the development of telemedicine, which is defined as physician-patient or physician-physician communication using telephones, videophones, fax machines, computers, or any other device that enables the transmission of information between parties located at a distance. Such services improve access to care but are suspected to be of lower quality, because physicians have less information about patients before making decisions. As will be shown in the following pages, previous studies indicate that there is a small increase in the probability of a wrong diagnostic when physicians use only information obtained electronically, as compared to using additional information obtained in face-to-face encounters. Thus, it is postulated that the quality of physician-patient telemedicine is lower than that of traditional face-to-face medical services.<sup>2</sup> Moreover there is a concern that doctors' inability to check the identity of their patients allows for illegitimate access to prescription drugs.

---

<sup>1</sup> The Office of the Advancement of Telehealth identifies this regulation as a barrier to the development of telemedicine.

<sup>2</sup> Any reference to medical services refers to advice and procedures offered on the occasion of a single visit and not to an entire course of treatment for a longer-term medical condition. Telemedicine and traditional medicine could be complements if we consider the case of patients who, having obtained advice through teleconsults, decide to continue with traditional consults while pursuing treatment. However, telemedicine and traditional medicine are substitutes with respect to each instance of a request for medical advice. The fact that the buyers of medical care, the

Since 1998, more than 30 states have adopted regulations prohibiting physicians from prescribing drugs to patients without a prior physical examination. These regulations are expected to reduce illegal access to prescription drugs, which should improve overall health of the population. At the same time, they restrict access to some telecare services, thus making care more costly. Some people will experience an improvement in health as they switch to face-to-face consults, which are associated with a lower probability of misdiagnosis. However, if access to face-to-face consults is too costly and people delay seeking care too long, they may experience worsening health. Moreover, some people may forgo medical care altogether and just resort to self-treatment. These people likely experience worsening health as they have an even higher probability of misdiagnosis than a physician through an electronic encounter. The net effect on health is thus an empirical question and a thorough econometric analysis is required before one could make any definitive statements about the impact of such regulations.<sup>3</sup>

If such restrictions improve health, then the availability of this type of service is not desirable even if it eases access to care. However, if these restrictions affect health negatively, then easier access to medical care through the services targeted by the *PER* is desirable despite their lower quality. Ultimately, this analysis provides some indications about whether the availability of certain medical services, which are believed to be of lower quality but also to ease

---

patients, sometimes choose not to pursue the suggested treatment or any recommended follow-up visits is a good example as to why a medical service should be defined with respect to a consultation and not an entire treatment. It is also consistent with previous literature suggesting that given the particularities of health care, it is likely that physicians/suppliers determine the procedures to be performed on the occasion of each visit, while consumers determine only whether and when to initiate an episode of care (McCombs, 1984).

<sup>3</sup> The effect on traditional care is also ambiguous. Electronic access to physicians may change patients' demand for face-to-face consults (Berman and Fanaughty, 2005). On one hand some *PER* will induce some people to replace tele-consults with face-to-face consults, which will increase the number of visits to the doctor. On the other hand, to the extent there is complementarity between telecare and traditional care, tele-visits induce an increase in face-to-face visits as people that otherwise would have chosen self-treatment now pursue a course of treatment suggested by their tele-encounters with a doctor. This complementarity suggests *PER* could lead to a reduction in traditional visits to the doctor. Using county level data from Area Resource File, I find no evidence of a significant change in the number of outpatient visits associated with ARF adoption. (results not reported but available on request)

access to medical care, affects health negatively, as many would expect if current quality standards are optimally chosen.

While other types of regulations also generate quality-quantity trade-offs, the analysis of *PER* impact is especially susceptible to be interpreted as a test if current quality standards are too high for several reasons. First, according to previous literature to be reviewed below, the difference in diagnostic accuracy between regular and telemedicine consultation is small. As a result, there is little concern that the results are caused by large departures from current quality standards.

Second, the introduction of telemedicine does not affect physicians' incentives to continue providing quality care, as is the case with other types of regulation (such as tort reform, as Kessler and McClellan (1996) and Currie and Macleod (2008) point out). As a result, the distribution of medical-care quality potentially available is preserved, while only the cut-off that determines the quality of care actually available changes. In other words, the distribution is preserved, and only the point of truncation changes.

Third the policy-makers' intent was to prevent the sale of prescription drugs that people might use for purposes other than treatment.<sup>4</sup> The concern raised was the relative ease with which individuals can obtain prescriptions through teleconsults when physicians are not perfectly able to distinguish whether the persons requesting drugs are really who they claim to be or if they are really experiencing the stated symptoms. Since the expressed intent of this regulation was to prevent illegitimate access to prescription drugs, it is unlikely that regulation adoption is driven by differences in health outcomes caused by the use of telemedicine. Figure 1 presents the 2003 geographical distribution of non-radiology consults through telemedicine networks

---

<sup>4</sup> 2001 Telemedicine Report to Congress on Telemedicine; U.S. Department of Health and Human Services, Health Resources and Services Administration, Office for the Advancement of Telehealth p.29, available at <ftp://ftp.hrsa.gov/telehealth/report2001.pdf>

(Grigsby, 2004), a proxy for total teleconsults. There is no indication of a significant correlation between telemedicine use and *PER* adoption, which is mapped in Figure 2. It is plausible that the timing of adoption of telemedicine regulation is the result of vagaries of the political process within each state, and therefore the variation in this type of regulations is presumably exogenous.

This paper uses data between 1994 and 2006 to calculate changes in the health outcomes of the group exposed to this regulation and compares them to changes in the health outcomes of the group that was not exposed. The sample in this study is representative of the country as a whole, because no *PER* regulation in any state was adopted before this period. The model includes county and year fixed effects, and state-specific trends in all specifications; therefore, omitted variable bias is unlikely to be a source of concern. In addition, to improve identification, the model also includes state health and hospital expenditures. This variable improves identification by acting as a proxy for state unobservable characteristics that may be correlated with mortality and the likelihood of adopting health regulations. Numerous specification tests are run to check for signs of selection and to address potential confounds.

The information about health outcomes comes from two main sources of data. First, I use mortality data from the Compressed Mortality Files compiled by National Center for Health Statistics. While the magnitude of the effect on mortality is small, overall, the analysis suggests that regulations restricting telemedicine lead to health deterioration. The results indicate that *PER* adoption leads to an increase in mortality of about 33 deaths in a population of 1 million people<sup>5</sup>, which represents less than half of a percent (0.4%) increase in the mortality rate. As expected, statistically significant increases in mortality are more likely in the sub-populations prone to use telemedicine: people living in rural counties or in counties with low physician

---

<sup>5</sup> The population weighted mean mortality rate per 1,000,000 is 8258.798

density. In addition the decomposition of the effect on mortality by cause of death indicates *PER* leads to an increase in non-injury mortality but to a decrease in mortality from injury.

The estimated effects on mortality likely understate the total impact on health, because it ignores the impact on morbidity. To address this concern, I use a second source of data: the Behavioral Risk Factor Surveillance System (BRFSS). These data have two advantages. First, they offer a measure of morbidity, and second, these are individual-level data such that endogeneity is not a source of concern. The measure of morbidity investigated is number of days lost to illness, the answer to the question: “During the past 30 days, for about how many days did poor physical or mental health keep you from doing your usual activities, such as self-care, work, or recreation?” We find that *PER* adoption leads to an increase in the expected monthly days lost to illness of about 0.2.

A second caveat to this analysis is that the estimates may overstate the policy’s health impact because they consider only the instantaneous impact and does not account for adaptations likely to occur when the price of medical care changes. Specifically, it is likely that when medical care becomes more expensive because of telemedicine unavailability, people will buy less medical care and substitute toward other inputs to improve health.

Ultimately the results of this analysis provide information about the effectiveness of the provision of services regulated by *PER*. Most studies investigating the effectiveness of electronically delivered health advice are controlled experiments, which may not be easily generalizable because scientists who run experiments and physicians who choose to offer telemedical care may face different constraints. Moreover, the controlled experiment literature does not account for general equilibrium effects such as selection into users of telecare and improved access to care that reduces the time from the onset of symptoms to diagnosis. As such this study

complements the controlled-experiment literature investigating the effectiveness of various electronically delivered medical services.

The paper proceeds as follows. Section I reviews the statistical evidence on the impact of using quality standards in medical care and on the quality of telemedicine services. Section II describes the data sources and presents the empirical approach. Section III describes and assesses the results and the limitations of this analysis. Section IV concludes the paper.

### **I. Background: Medical Care Market and Technological Innovation**

The health-care market is characterized by asymmetric information: the seller has more information about the quality of the service than the buyer. To protect themselves, buyers in such a market are willing to pay only for average quality, inducing sellers to withdraw the now underpriced high-quality services from the market (Akerlof, 1970). This characteristic of the market, as Arrow (1963) notes, necessitates the development of trust relations between physicians and patients and highlights the social value of a reliable source of information about the quality of services. One solution is to create social institutions that provide reliable information, reducing the uncertainty regarding quality and increasing demand for medical services. An example of such an endeavor is the minimum quality guarantee offered by regulations such as the professional licensing of physicians.

Every US state has adopted such regulations. As a result, there are fewer doctors and, thus, less competition. This raises the question: Was licensing in the public interest, or was it driven by the desire to restrict competition? The answer is not clear-cut; rather, as Moore (1961) finds after surveying licensing regulations, licensing is often not strictly in the public interest and extends into the realm of competition restrictions. This should be expected in a world where

policy-makers respond to pressures from different interest groups and choose not to grant perfect cartels with their regulations (Peltzman, 1976). In fact, there is evidence to support the view that medical licensing tends to create a monopoly (Friedman and Kuznet, 1945) (Kessel, 1958), but also that licensing is consumer-demand driven (Leffler, 1978). However, if professional groups design the tests for granting licenses, we should expect that in the process of maximizing net gains, these groups will choose standards higher than optimal (Leland, 1979). Standards that are too high in medical field make access to care too difficult.

All else held constant, there is a way to ease access to medical care: the practice of medicine at a distance, through what we now call telemedicine. Such practices have existed since the 1960s, but telemedicine really took off in the 1990s when improvements in technology made it more useful and reliable (Emery, 1998; Darkins and Cary, 2000). The notion of telemedicine today covers both physician-patient and physician-physician communications using telephones, videophones, fax machines, computers, or other devices that enable the transmission of information between parties located at a distance. In the case of physician-patient telemedicine, the patient obtains medical advice in the absence of any face-to-face encounter with a physician. Physician-physician telemedicine differs from physician-patient telemedicine in that patients obtain specialized advice from a physician they never meet, through the intermediary of another physician that meets with them in person.

While physician-physician communication is generally viewed in a positive light, because patients benefit from both face-to-face consultations and the specialized advice that otherwise would be inaccessible to them, physician-patient telemedicine remains controversial. The controversy stems from the fact that even though telemedicine consultations have lower transportation costs and lower time costs, as expressed in time spent for both transportation

(Smith et al., 2003) and consultation (Guilfoyle et al., 2003), and significantly improve access to medical care (Martinez et al., 2004), they also provide less information to the physician, creating a potential for mistakes. Comparisons between tele-consultations and face-to-face consultations, of which I will mention just a few for brevity, indicate that this may indeed be the case. Smith et al. (2003) find that of 58 ear, nose, and throat assessments, 81% of the diagnoses were the same for the tele-consultation and the face-to-face consultation. In the case of trauma, the percent of incorrect tele-diagnoses was even smaller: Only about 2% or less of the original tele-diagnoses was considered incorrect after a face-to-face review (Tachakra et al., 02/2000) (Tachakra et al., 12/2000). There is also evidence of a higher incidence of mistakes in tele-dermatology compared to face-to-face encounters (Loane et al., 1998) (Chao et al., 2003) (Oztas et al., 2004) (Oakley et al., 2006), even when a general practitioner was present with the patient in the videoconference room (Nordal et al., 2001). There is also some evidence that in some cases telemedicine may not be significantly less effective than in-person consults. Some examples are genetic services (Stalker et al., 2006) or high blood pressure (Bradford et al. 2001). More information about the outcomes of such comparisons is readily available in reviews of the literature (Currell et al., 2000) (Miller, 2001) (Hersh et al., 2002) (Hersh et al., 2006), which, similar to the small sample of studies mentioned above, indicate that telemedicine offers services of close but somewhat lower quality than face-to-face consultations, and that the relative quality varies with the type of health problem targeted.

Physician-patient telemedicine's ultimate effect on health is ambiguous and depends on the stringency of licensing requirements, because this determines the number of practicing physicians and thus, the ease of access to care (Kleiner and Kurdle, 2000). If requirements are

too strict, relaxing them will have a positive effect on health; otherwise, the impact will be negative.

Several states require physicians to meet their patients in person before prescribing drugs (see Table 1). These regulations hamper certain physician-patient telemedicine practices. If the quality standard is too high, as predicted by theory when professional groups set the standard, as in the case of medicine, telemedicine probably improves outcomes, and the *PER* adoption has a negative effect. One caveat is that the effect also depends on the relative quality of telemedicine and traditional medical services. However, the literature indicates only a small difference in quality, so this should not be a major concern for the purpose of this analysis. A second caveat is that the *PER*'s impact also depends on the pervasiveness of physician-patient telecare. While surveys of telemedicine use are sparse and do not have continuity in time, some general estimates are available. MedMarket Diligence estimates that in 2003, there were 169 million telemedicine care visits, or "telemedicine information exchanges between a practitioner and patient."<sup>6</sup> As early as 1995, an estimated 4,000 teleconsults per month were performed in rural hospitals nationwide (Hassol et al. 1996), which is convincing evidence that the *PER* is indeed binding. Not only is *PER* binding but it also does not seem to be systematically related to the pervasiveness of telemedicine as shown in Figure 1 and 2.

To bolster further the argument that *PER* is indeed generating the observed effect on health outcomes, I provide additional evidence that people who gain most from using telemedicine, those located in rural areas or in areas with low physician density, experience the largest changes in health.

---

<sup>6</sup> Technologies, Products & U.S. Markets in Telemedicine, 2003, (December 2003) report E101, MedMarket Diligence, LLC quoted by Glenn Wachter, "How High Will Telemedicine Soar?" For the Record, Vol. 16 No. 5 p. 28, March 8, 2004.

## II. Data and Empirical Approach

This paper uses the 1994 to 2006 data to investigate the impact of regulations requiring physicians to perform a physical examination prior to prescribing drugs on mortality and morbidity. The source of mortality data is the Compressed Mortality Files compiled by National Center for Health Statistics (NCHS). In addition to being comprehensive--it contains information from all death certificates filed in the 50 states and the District of Columbia--this dataset has the advantage of reporting the population for each demographic group as defined by sex, race, and age. We use this county-level panel data to estimate the following specification:

$$(1) \quad \text{Mortality rate}_{ct} = \beta \text{PER}_{st} + \theta X_{ct} + \gamma_c + \lambda_t + \omega_{st} + \text{HHEXP}_{st} + \varepsilon_{ct}$$

where  $c$  indexes counties,  $s$  states, and  $t$  time. The dependent variable is the log of mortality rate per 100,000 individuals, so that coefficient estimates can be interpreted as percentage changes.<sup>7</sup> The mortality rate excludes war related mortality. There is some correlation between the timing of *PER* adoption and the war in Afghanistan so including war related mortality could bias the results. The murder and suicide mortality are also excluded. On the right-hand side, *PER* stands for Physical Examination Regulation, and it is a dummy variable indicating whether in a particular state and year there was any regulation, rule or policy requiring physicians to perform physical examinations on their patients before prescribing drugs.  $X$  is a vector of time-varying determinants of mortality measured at the county level, such as percent of population that is female, African-American, dummies for county age composition: 15-24, 25-44, 45-64, and above 65, log wages, and the number of physicians per 1,000 people. By including county fixed effects,  $\gamma_c$ , this specification controls for differences in mortality rates that are common to people in the same county (for instance, differences in the overall level of health due to climactic

---

<sup>7</sup> Using log of mortality rates as dependent variable has the advantage that it counts equivalent relative changes in mortality rates equally. In addition, to improve the readability of the tables, this variable is multiplied by 10.

conditions). Year fixed effects,  $\lambda_t$ , absorb any time-varying differences in the dependent variable common to all counties such as changes in federal level health care policies. In addition, state-specific trends,  $\omega_{st}$ , controls for differences in the general trends in mortality in a state that would affect the likelihood that a state would adopt the *PER*. An example is trends in mortality generated by a state's institutional particularities. Controlling for these trends reduces the burden of exogeneity of the *PER* because now the *PER* must be exogenous only after accounting for state-specific trends in mortality rates. The model specification also controls for state health and hospital expenditures (*HHEXP*). This variable is introduced to improve identification by acting as a proxy for state unobservable characteristics correlated with both mortality rates and *PER* adoption such as higher state interest in health policy that would affect the likelihood of adoption of health regulations.  $\varepsilon_{ct}$  is the error term.

Since telemedicine generates the most significant savings in the time cost and not the monetary cost of telemedicine, we also investigate the effect of telemedicine on the categories that experience the largest savings: people located in predominantly rural areas, and people located in areas with a low density of physicians.

The key identifying assumption in this paper is that *PER* adoptions are not driven by differences in health outcomes. One way to assess the validity of this identification assumption is to test whether the distribution of the observable covariates is balanced across treatment and control groups. Table 2 Panel A presents the mean values of a number of variables for year 1997, the year prior to the date of the first *PER* adoption, as well as the results from two-sided t-tests for the equality of these means. The table shows that the samples of *PER* adopting and non-adopting states are balanced across a wide variety of variables: population, age distribution, race distribution, log wages, physicians' density in population, and mortality rates.

Table 2 Panel B presents the marginal effects from Probit specifications that consider all of these variable simultaneously. Mortality is still not a significant predictor of *PER* adoption, and in fact it enters with a negative sign. To the extent to which *PER* was adopted by states with lower mortality rates, the results provide a lower bound for how large is the increase in mortality associated with *PER*. Two variables are statistically significant: percent African-American and log wages, however the correlation is driven by the high proportion of Southern states that adopted *PER*. After controlling for Southern state by using a dummy both the estimated coefficient on percent African-American and the one for log wages decrease sharply, and the statistically significant correlation between African-American and *PER*, or log wages and *PER* disappears. Even if many Southern states adopted *PER*, in the robustness check I show that in fact, after controlling for state fixed effects and state specific time trends, the *PER* effect does not vary by region. Moreover, the results are also robust to the inclusion of higher order state specific time trends. Combined, these results suggest that the identification assumption is plausible. Numerous other validity tests are reported in the robustness analysis.

Some issues regarding the estimation strategy should be mentioned. First, the estimates obtained from counties with large populations are more precise than those from smaller counties. To control for this source of heteroskedasticity, this paper reports regressions weighted by population in each county-year. Second, the unit of observation is more detailed than the level of variation of the independent variable of interest, the state level. Third, there are no instances of repeats in the data; thus, it is highly likely that the error terms are correlated within the state over time. In the presence of autocorrelation, estimated standard errors tend to be biased downward, making coefficient estimates spuriously statistically significant. Moreover, misspecification of the autocorrelation process, which is likely to occur with short time series like the ones used in

this paper, can also lead to downward bias in the standard error estimates. One viable solution is to allow for an arbitrary autocorrelation process (Bertrand et al., 2004). To correct for all these potential problems, this paper reports robust standard errors clustered at the state level. In the robustness check I show the results hold under clustering at county level.

For the analysis of BRFSS individual-level data<sup>8</sup>, I use a Negative Binomial<sup>9</sup> model with a similar specification:

$$(2) \quad \text{Days lost to illness}_{it} = \beta \text{PER}_{st} + \theta X_{it} + \text{Physicians}_{st} + \gamma_s + \lambda_t + \omega_{st} + \varepsilon_{it}$$

where  $X$  is a vector of individual characteristics, such as gender, race, age, education, marital status, health insurance, and income. The variable physicians is measured at state level and represents the number of physicians per 1000 people. The model specification includes state fixed effects,  $\gamma_s$ , time fixed effects,  $\lambda_t$ , and state specific time trends,  $\omega_{st}$ . All regressions were estimated using BRFSS weighting variables. Robust standard errors clustered at state level are calculated and reported throughout the analysis.

### III. Results

#### 3.1. Main Specification

Table 3 presents the main results obtained from the estimation of equations (1) and (2). The results indicate that *PER* adoption leads to an increase in the monthly number of days lost to illness and in mortality. The magnitude of the *PER* effect is quantitatively small: Mortality increases by approximately 33 deaths per 1 million people, a 0.4% increase measured at the

---

<sup>8</sup> BRFSS does not have data for Rhode Island in 1994 and for District of Columbia for 1995.

<sup>9</sup> A Poisson specification yields similar results, but tests indicate that overdispersion is a problem – results not reported but available on request.

mean of the data.<sup>10</sup> The expected number of days lost to illness is approximately 0.2 higher for people in states that adopted *PER* than for people in states that did not adopt *PER*. The timing of the effect depends on the severity of the investigated health problem. For the measure of general health wellbeing used in this paper (monthly days of illness), there is evidence of an instantaneous effect, with no such evidence in the case of mortality but the strongest effect on both mortality and morbidity shows with a one-year lag.

*PER* is expected to affect health through three channels. First, reduced illegitimate access to prescription drugs should reduce the incidence of accidental poisoning. Second, by making the access to medical care harder for some people it will increase the time to diagnosis and treatment and for some people and will even make self-treatment the preferred health care option for others. Third, by eliminating a lower quality service it should reduce the incidence of medical care adverse effects. Thus the prediction is that if *PER* is to improve health outcomes it is more likely it will do so in the case of accidental injuries. In support of this predictions, the results reported in Table 3 suggest that the *PER* is negatively correlated to injury-related mortality. The caveat is that the negative effect on injury caused mortality is not very precisely estimated, the standard errors are quite wide possibly due to a noise.<sup>11</sup> The increase in mortality associated with the adoption of *PER* is mainly driven by the increase in mortality from non-injury-related causes.

---

<sup>10</sup> As shown in Figure 3, there is a sharp drop in mortality rates in 2004 followed by an increase in 2005. The results are not driven by noise in this period. In fact the estimates are more precise when excluding years 2005 and 2006 (results not reported but available on request).

<sup>11</sup> Figure 5 using the raw data is consistent with an increase in non-injury mortality in adopting states relative to non-adopting states. However, when investigating injury mortality rates (Figure 6), states that adopted *PER* in 2000 have lower mortality rates than the states that did not adopt by 2000, while the opposite is true for states that adopted *PER* in 2001. While raw data is not enough to draw a conclusion, the graphs are suggestive of significant variation in injury mortality trends by state. The R-squared from the regressions also indicate that even the large number of fixed effects and state trends is not able to explain nearly as much variation in injury mortality as in non-injury mortality. The standard errors are tighter in a specification controlling for quadratic state time trends as shown in robustness check, but even that specification has caveats (as discussed in the robustness check section) and should be interpreted with caution.

To bolster the case that the observed effect of *PER* on health is mediated by *PER* impact on telecare I present several falsification tests. The *PER* cannot negatively affect individuals that do not use telecare services. In practice, the people more likely to use telemedicine services are those who do not have easy access to regular face-to-face consultations. These tend to be people located in rural areas or in areas with low physician density who incur high transportation costs to get to a physician's office for a face-to-face consultation.

### 3.2. Rural versus Urban

The prediction is that this policy has larger effects on health in predominantly rural counties. To identify whether the regulation affects these counties differently than predominantly urban counties, I interact the *PER* variable with the percent of the county population living in urban areas as measured in year 2000 (sources of data are detailed in Appendix B). For easier interpretation of the main effect, the variable percent urban population is centered such that the main effect is calculated for a person living in a county that is 50% urban.<sup>12</sup> As shown in Table 4, the *PER* leads to an increase in mortality, but the effect is significantly smaller in predominantly urban areas. As predicted the availability of medical services covered by the *PER* has a larger beneficial effect in rural areas. One explanation is that this sub-population is more likely to try to obtain electronically delivered medical advice. In addition, because the transportation cost of face-to-face consultations is lower in urban areas than in rural areas, urban users of telecare services are more likely than rural users to switch toward in-person consults and less likely to delay seeking diagnosis and treatment.

---

<sup>12</sup> The average individual lives in a county with a 79% urban population. The main effect of *PER* on log mortality rate is for such individual is 0.041 with a standard error of 0.024 and thus significant at 10% significance level.

### 3.3. Physician Density

A second category of the population likely to use telemedicine consists of people located in counties with a low physician density. As the transportation costs are higher, the *PER* should have a larger negative effect on the health of these individuals. To test this prediction, I interact the *PER* dummy with the county number of physicians per 1,000 individuals. The physicians variable is centered such that the reported (Table 5) coefficients of the *PER*'s impact are calculated at the average level of physician density in the population.<sup>13</sup> The estimates indicate that the effect of *PER* adoption is lower the more physicians there are in the county. These results suggest that when certain electronically delivered medical advice regulated by the *PER* is not available, the fewer physicians there are, the more likely people will delay seeking medical help or even forgo medical care altogether and experience worsening health.

These estimates provide support for the idea that *PER* adoption has an overall negative impact on the population, and mostly affects people located in areas with limited access to medical care.

### 3.4. Falsification Test

Since the *PER* hampers physician-patient telemedicine but not physician-physician telemedicine, we can construct a falsification test by investigating the effect on mortality from neoplasm. It is extremely unlikely that physicians would recommend drugs for such a condition without ever meeting their patients in person and as such *PER* should have no effect on neoplasm mortality. Table 6 reports the estimated effect of the *PER* separately for mortality from neoplasm and other non-injury mortality. The coefficient in the case of neoplasm mortality is very small and not statistically significant, even though neoplasm accounts for 25% of total non-injury mortality. One standard error bands of the estimated effect on other non-injury mortality rates

---

<sup>13</sup> The average individual lives in a county with 2.49 doctors per 1000 people.

excludes the estimated effect on neoplasm mortality rate. I find that the statistically significant effect on non-injury mortality is driven by the impact on mortality from other causes.

### 3.5. Addressing Confounds

A series of sensitivity checks is performed and some of the results are reported in Table 7. The preferred specification, using lag *PER*, is reported in the top row for comparison.

The identifying assumption is that *PER* adoption is not caused by local trends in health outcomes. To support the validity of this assumption I show that *PER* is statistically unrelated to past mortality rates. For this purpose I ran regression models of log mortality rates on future *PER* status; if health outcomes cause *PER* adoption, we should find a relationship between current mortality rates and future *PER* adoption. As reported in rows [2]-[4] of Table 7A, 1-year lead, 2-years lead and 3-years lead of *PER* are not statistically significantly correlated to current mortality rates. Also note that previously I show that current year regressions failed to find any significant relationship between mortality and *PER* and even in the case of morbidity the relationship is weak while the 1-year lag of *PER* is a significant predictor of health outcomes. These temporal validity tests suggest that causality runs from *PER* to mortality and not in opposite direction.

As shown in Table 1 and 2 *PER* regulation seems to have a geographical pattern raising the concern that there are some trends in mortality to trigger the adoption. Previously reported results show *PER* has a significant effect after controlling for time trends. However, *PER* could have been adopted as a response to accelerations in the rate of growth of mortality rates. Such a possibility would not be entirely captured by linear trends. Specifications enhanced with quadratic time trends were tested with similar results. In the presence of higher order time trends the coefficient on injury mortality becomes significant (row [5]). Some caution should be used in

putting too much emphasis on this result given that overcontrolling for unmeasured factors sometimes leads to unstable parameter estimates (Schneider, Klein, and Murphy, 1981).<sup>14</sup>

In addition since southern states are more likely to have adopted *PER*, I test whether the impact varies by geographical area. Row [6] of Table 7A presents the estimated coefficient for the interaction term between *PER* and southern states.<sup>15</sup> These coefficients are not significant, suggesting the identified effect is not driven by differences in geography of the adopting states.

Moreover, specification [7] presents the results of another test for signs of selection. This test addresses the question of whether the early-adopting states are the ones that would benefit most from such regulation, which would signal reverse causality, because adoption would likely be triggered by health outcomes. In the sample used, the first states to adopt the *PER* are Maryland, North Carolina, Ohio, and Texas in 1999<sup>16</sup>, while the first large wave of adoption took place in years 2000<sup>17</sup>. A dummy equal to 1 if the state adopted the *PER* before 2000, and zero otherwise, is interacted with the *PER* variable and the coefficient on this variable is reported in row [7] of Table 7A.<sup>18</sup> This coefficient is not statistically significant at the standard significance levels, providing support for the idea that the early-adopting states are in fact similar to the later-adopting ones, and thus, the timing of *PER* adoption is exogenous.

Similarly, I show that the estimated effect on late adopters is not significantly different from the effect on earlier adopting states. Row [8] reports the estimated coefficient on the interaction term of *PER* and a dummy equal to 1 if the state adopted *PER* on or after year 2003.<sup>19</sup>

---

<sup>14</sup> Other specifications using higher order state specific time trends were tested with substantially similar results.

<sup>15</sup> Southern states: Alabama, Arkansas, Delaware, Florida, Georgia, Kentucky, Louisiana, Maryland, Mississippi, North Carolina, Oklahoma, South Carolina, Tennessee, Texas, Virginia, and West Virginia.

<sup>16</sup> The first state to adopt the *PER* is District of Columbia in 1998, but DC is not in the sample used because data for state health and hospital expenditures are not available. The results are robust to a sample including DC and excluding the controls for state health and hospital expenditures (results not reported but available on request).

<sup>17</sup> Ten states adopted *PER* in 2000.

<sup>18</sup> Using other cut-offs, such as before 2001, delivers similar results.

<sup>19</sup> Using other cut-offs such as after 2002 or after 2004 delivers similar results.

The results also hold under the exclusion of the counties of the state capitals (row [9]). This result provides reassurance against endogeneity concerns such as those generated by the possibility that some counties can create pressure to obtain the desired regulation. Arguably, counties of the state capitals have a more significant weight in the decisions of the policy makers. Their exclusion does not change the obtained estimates, providing support for the identifying assumption.

In addition, I report results that control for potentially relevant time-varying state characteristics: no health insurance, and education (data sources detailed in Appendix B). A higher proportion of population with no health insurance makes telecare a more attractive option in such states but also affect mortality rates. Education may influence the likelihood that the regulation is adopted and it is known to affect health (Grossman, 2000). The results are robust to the inclusion of these additional controls, further evidence that the identifying assumption is plausible.

All these results add up to support the assumption of exogeneity. If the identifying assumption is valid, the most damaging possible interpretation of the results left is that they are driven by noise in the data. For instance, if the population is very small, the data could indicate large changes in the mortality rate from one year to another. This is especially a source of concern because the positive impact of the *PER* is more likely to lead to increases in mortality in predominantly rural areas, which are also more likely to have small populations. One way to reduce the impact of noise in the data is to exclude counties with very small populations where there is extremely high variance in mortality rates. Thus, to investigate the possibility that the results are driven by noise, specification [11] runs the same regression on the sample of county-year observations with populations of at least 10,000 individuals. The results obtained from this

specification are similar to those obtained from the entire sample, providing reassurance against a noise-driven explanation of the estimates.

In addition excluding Alaska, a state with very low population density and thus potentially highest gains from telecare, does not alter the significance of the coefficients.

Some alternative specifications are also presented. For instance, the regulation may trigger changes in the time trends of mortality. To investigate this hypothesis, I estimate the equation below and report the results in row [13] of Table 7A.

$$(3) \quad Mortality\ rate_{ct} = \beta PER_{st} + \mu PER_{st} * t + \theta X_{ct} + \gamma_c + \lambda_t + \omega_{st} + HHEXP_{st} + \varepsilon_{ct}$$

I find no evidence of a change in slope for non-injury mortality, likely because the mean shift captures most of the change in mortality. The coefficient is significant in the case of injury-related mortality but it is not robust to the inclusion of quadratic time trends.

Throughout the paper I report standard errors corrected for clustering at state level. Clustering at county level may be more appropriate if the concern is that autocorrelation within county over time is a more significant problem than the errors correlation within state over time. The results hold under clustering at county level and in fact they show that clustering at state level is a stronger restriction.

When restricting the sample to the states adopting the *PER* during this period (row [15]), we obtain similar results. The coefficients, however, are not statistically significant, consistent with a model specification that cannot fully account for the decreasing trend in mortality expected in the absence of *PER* adoption.

Since the regulation of interest is at the state level, it is useful to test the robustness of the results at state level. The main specification accounts for the existence of common random effects at the state level by computing standard errors corrected for clustering at the state level.

Another solution to this problem is to aggregate the data at the state level. However, using state-level data also may have significant disadvantages. First, it aggregates over significantly different populations. And second, the physical examination requirement regulation is more likely to be endogenous at the state level. Specification [16] in Table 7A presents results obtained on state-level data for the 1994-2006 period. Controls included are state and year fixed effects, state-specific trends, and state-level, time-varying controls, such as: age, gender, and race composition, log wage, physicians, no health insurance, education, and state health and hospital expenditures. The obtained coefficient estimates are somewhat smaller and not statistically significant, understandable given how demanding the large number of fixed effects and state time trends are on the data.<sup>20</sup> However, these results are generally similar to the previous results, providing support for the idea that the timing of adoption is purely the result of a political process, rather than the variance in the perceived differential advantage in imposing the requirement of a physical examination prior to prescribing drugs.

Endogeneity is less of a concern for the analysis of the effect on the number of days lost to illness because that analysis uses individual-level data. However, I also perform a series of robustness checks for this specification. The results reported in Panel B of Table 7 are consistent with those obtained in the case of mortality. The first row reports the main results where the *PER* variable is measured at time  $t-1$ . The second specification indicates the results are robust to using an OLS specification with log dependent variable. There is no evidence of a change in trend. The results are robust to the exclusion of Alaska. Using only data from adopting states the estimated effect is smaller in magnitude but qualitatively similar. Consistent with the results obtained using mortality rates, I find no evidence of selection: The estimated effect is not significantly different in early adopting states, or in late adopting states.

---

<sup>20</sup> There regressions run on 650 observations.

Individual level data allows a more in depth analysis of the effect of PER on various demographic groups. The effect does not seem to vary significantly by gender but there is significant variation by race. It appears that the effect is very small on blacks. One explanation is that there are racial differences in use of technology.<sup>21</sup> The result is also consistent with some previous studies indicating blacks are less likely to access health related electronic resources.<sup>22</sup>

While BRFSS does not have very detailed information on income, an analysis of the effect by income brackets is possible. The effect decreases with income though the differences are relatively small and there may be non-linearities. It appears that the effect first decreases with income and then slightly increases. One potential explanation that may prove to be a fruitful area for future research is offered. Intuition suggests that another category of people likely to use telemedicine services is defined by the relative price of telecare that different people face even when they are neighbors. Telemedicine may reduce the monetary price but it certainly reduces the time cost of medical services. As a result, people with high cost of time as proxied by high wages may be more likely to use telemedicine. Higher wages, of course, also produce an income effect, translating into more medical care being bought and an increased likelihood of face-to-face consultations. However, a backward-bending labor supply suggests that at very high wage levels, people tend to work more and as a result tend to consume less time-intensive goods. High-wage individuals also face a very high cost of traditional medical care because of its high time cost, and as such, they may be more likely to postpone medical visits. On the other hand,

---

<sup>21</sup> The literature suggests that the racial gap in computer ownership persists after controlling for socioeconomic characteristics (Goolsbee and Klenow, 2000) so there may be differences in the rate of technology adoption by race.

<sup>22</sup> Some studies found significant racial divide in probability of looking for health information on-line (Rimer et. al., 2005; MedlinePlus Survey Results 2005) although other studies suggest the difference is relatively small (Rutten, 2007)

low-wage individuals are more likely to switch to self-care when the cheaper option is no longer available.

Overall, these results show the estimates are robust to variations in the sample, the choice of controls, or functional form, providing support for the validity of the empirical strategy.

#### **IV. Conclusions**

The rapid development of electronically delivered medical care raised awareness about the nature of patients' relationship with their physicians. While word to mouth evidence indicates the practice of prescribing drugs through telephone to people they have known for some time was not foreign to some physicians, extending this practice to people physicians never met could generate problems if the physicians are not able to verify identity. In order to limit illicit access to prescription drugs, many states adopted regulations requiring physicians to perform a physical examination before prescribing drugs. Under the threat of losing their license physicians tie patients' ability of obtaining a prescription to a face-to-face consult. In other words, these regulations restrict certain physician-patient telemedicine practices. This affects health through several channels.

By restricting illegal access to prescription drugs, these regulations are expected to improve public health. At the same time these regulations also restricts physicians' ability of prescribing drugs electronically to real patients. Such services are expected to be of lower quality, because physicians cannot collect as much information during tele-encounters as they do during face-to-face consultations, but have the advantage of improving access to medical care. As such there is a quality-quantity trade-off: If these services are eliminated, the quality of the marginal service delivered increases, however access to medical services becomes more difficult.

In principle, consumers in need of treatment have three choices: traditional consults, teleconsults, and self-treatment. However, if a face-to-face physical examination is required, the only alternative to traditional consults is self-treatment. Once it is understood that the presence of this regulation does not imply that every person with health problems will actually meet with a doctor and that there is a real possibility some will give up physician consultations altogether or at least delay seeking medical care, the implications of such regulation for consumer health are less clear than might be imagined.

The ambiguity arises from the effect of the law on various groups of population. The marginal effect of this hands-on policy is to raise the cost of access to medical advice for some individuals. Among the individuals that would have otherwise used telemedicine services, after *PER* adoption some will choose to meet a physician in person and others will give up on obtaining professional advice altogether because it would be too expensive. Those that switch from electronically obtained advice to advice based on face-to-face encounters will obtain a higher quality service and thus may experience an improvement in their health. However, they may experience worsening health if access to care becomes difficult enough to induce them to delay seeking medical care too long. In addition, those that exchange electronically obtained professional advice for self-treatment will likely suffer a decline in health, because they have a higher probability of making a mistake than a physician.

This study finds that adoption of the physical examination requirement leads to an increase in the number of days lost to illness and an increase in non-injury mortality. There is also some evidence that injury mortality decreases. The increase in mortality is mostly driven by increased mortality in rural areas, and in areas with low physician density. Numerous falsification checks are performed and some results are reported, including: testing the temporal

validity of the identifying assumption, restricting the sample to the most populous counties, excluding counties of state capitals, testing whether early adopters differ from late adopters, etc. The results are robust to these alternative specifications.

Overall, the results suggest that allowing physicians to prescribe drugs following teleconsults can improve health. Moreover, these results provide just the lower bound of the gain, because it does not measure the gain from the reduction in medical-care costs. From a policy point of view, these results suggest that deterring illegal access to prescription drugs should take forms other than regulations that hamper telecare practice.

## **Appendix: Data Sources**

### INDIVIDUAL LEVEL DATA

1. *Days Lost to Illness, Gender, Age, Race, Education, Income, and Marital Status* – Behavioral Risk Factor Surveillance System (BRFSS) from Center of Disease control (CDC)

### COUNTY LEVEL DATA

1. *Mortality Rates* - Compressed Mortality Files compiled by National Center for Health Statistics (NCHS)
2. *Gender, Age, and Race Composition* - Compressed Mortality Files compiled by National Center for Health Statistics (NCHS)
3. *Wage* as defined by average annual pay – U.S. Department of Labor, Bureau of Labor Statistics, Quarterly Census of Employment and Wages (QCEW) data
4. *Physicians* – U.S. Department of Health and Human Services - Area Resource Files

### STATE LEVEL DATA

1. *PER (the regulation prohibiting physicians from prescribing drugs without a prior physical examination)* - Federation of State Medical Boards; Office for the Advancement of Telehealth; States Legislatures
2. *Education* as defined by the percent of population with a high-school degree– U.S. Census Bureau;
3. *No Health Insurance* as defined by the percent people not covered by health insurance – U.S. Census Bureau;
4. *State Health and Hospital Expenditures* (per capita amounts deflated using CPI) – U.S. Census Bureau; State Government Finances;
5. *CPI price index* – Statistical Abstract of the United States, 2008
6. All other state level data – race composition, age composition and wages are obtained from the county level data.

## REFERENCES

- [1] Akerlof, George "The Market for "Lemons": Quality Uncertainty and the Market Mechanism" *The Quarterly Journal of Economics*, Vol. 84, No. 3. (Aug., 1970), pp. 488-500.
- [2] Arrow, Kenneth J. "Uncertainty and the Welfare Economics of Medical Care" *The American Economic Review*, Vol. 54, No. 5 (Dec., 1963), pp 941-973.
- [3] Avraham, Ronen. "Database of State Tort Law Reforms (DSTLR 2nd)," Northwestern Law & Econ Research Paper No. 06-08, November 2006. Available at SSRN: <http://ssrn.com/abstract=902711>.
- [4] Berman Matthew, and Andrea Fenaughty. "Technology and Managed Care: Patients Benefits of Telemedicine in a Rural Health Care Network," *Health Economics*, 14(6), 2005: 559-573.
- [5] Bertrand, Marianne, Esther Duflo and Sedhil Mullainathan. "How much Should We Trust Differences-in-Differences Estimates?" *The Quarterly Journal of Economics*, 119(1), 2004, pp 249-275.
- [6] Bradford, David W., Andrew N. Klein, M.A. Krousel-Wood, and Richard N. Re. "Testing Efficacy with Detection Controlled Estimation: An Application to Telemedicine" *Health Economics*, 10(6), 2001: 553-564.
- [7] Chao, L.W., Cestari T.F., Bakos L. Oliveira M.R., Miot H.A., Zampese M., Andrade C.B., Bohm G.M. "Evaluation of and Internet-based Teledermatology System" *Journal of Telemedicine and Telecare*, Vol. 9, Supplement 1, 1 June 2003, pp 9-12(4).
- [8] Currie, Janet and W. Bentley Macleod. "First Do No Harm? Tort Reform and Birth Outcomes," *The Quarterly Journal of Economics*, Vol 123(2), 2008, pp 795-830.
- [9] Currell R, Urquhart C, Wainwright P, Lewis R. "Telemedicine versus face to face patient care: effects on professional practice and health care outcomes" *Cochrane Database Syst Rev*. 2000; (2):CD002098.
- [10] Darkins, Adam W. and Margaret A. Cary "Telemedicine and Telehealth: Principles, Policies, Performance and Pitfalls," Free Association Books, London, 2000.
- [11] Emery, Sherry. "Telemedicine in Hospitals: Issues in Implementation" Garland Publishing Inc., New York & London, 1998.
- [12] Friedman Milton and Simon Kuznets. "Income from Independent Professional Practice" New-York: *National Bureau of Economic Research*, 1945.
- [13] Goolsbee, Austan and Peter J. Klenow. "Evidence on Learning and Network Externalities in the Diffusion of Home Computers," *Journal of Law and Economics*, 45(2) Part 1, October 2002: 317-343.

- [14] Grigsby, Bill “2004 TRC Report on US Telemedicine Activity With an Overview of Non-US Activity”, New-Jersey: *Civic Research Institute Inc*, 2004.
- [15] Grossman, Michael “The Human Capital Model.” In *Handbook of Health Economics: Volume 1A*, edited by Anthony J. Culyer and Joseph P. Newhouse. Amsterdam: North-Holland, 2000.
- [16] Guilfoyle C., Wootton R., Hassall S., Offer J., Warren M., Smith D., Eddie M., “User Satisfaction with Allied Health Services Delivered to Residential Facilities via Videoconferencing” *Journal of Telemedicine and Telecare*, Vol. 9., Supplement 1, 1 June 2003, pp. 52-54(3).
- [17] Hassol A., Gaumer G., Grigsby J., Mintzer C.L., Puskin D.S. and Brunswick M., “Rural telemedicine: a national snapshot”, *Telemed J 2* (1996), pp. 43–48.
- [18] Hersh W., Helfand M. Wallace J., Kraemer D., Patterson P., Shapiro S., Greenlick M. “A Systematic Review of the Efficacy of Telemedicine for Making Diagnostic and Management Decisions” *Journal of Telemedicine and Telecare*, Vol. 8, No. 4, 1 August 2002, pp 197-209(13).
- [19] Hersh, William R., David H. Hickam, Susan M. Severance, Tracy L. Dana, Kathryn Pyle Krages, Mark Helfand “Diagnosis, Access and Outcomes: Update of a Systematic Review of Telemedicine Services” *Journal of Telemedicine and Telecare*, Vol. 12, Supplement 2, September 2006, pp 3-31(29).
- [20] Kessel, Reuben A. “Price Discrimination in Medicine” *Journal of Law and Economics*, Vol. 1 (Oct., 1958), pp. 20-53.
- [21] Kessler, Daniel and Mark McClellan. “Do Doctors Practice Defensive Medicine?” *The Quarterly Journal of Economics*, 111(2), May 1996, pp 353-390.
- [22] Kleiner, Morris M., Robert T. Kurdle “Does Regulation Affect Economic Outcomes? The Case of Dentistry” *Journal of Law and Economics*, Vol 43, No. 2. (Oct., 2000), pp 547-582.
- [23] Leffler, Keith B. “Physician Licensure: Competition and Monopoly in American Medicine” *Journal of Law and Economics*, Vol. 21, No. 1. (Apr., 1978), pp 165-186.
- [24] Leland, Hayne E. “Quacks, Lemons, and Licensing: A Theory of Minimum Quality Standards” *The Journal of Political Economy*, Vol. 87, No. 6. (Dec., 1979), pp. 1328-1346.
- [25] Loane M.A., Corbett R., Bloomer S.E., Eedy D.J., Gore H.E., Mathews C.; Steele K., Wootton R. “Diagnostic Accuracy and Clinical Management by Realtime Teledermatology. Results from the Northern Ireland Arms of the UK Multicentre Teledermatology Trial” *Journal of Telemedicine and Telecare*, Vol. 4, No.2, 1 June 1998, pp 95-100(6).

- [26] Martinez, Andres, Valentin Villarroel, Joaquin Seoane, Francisco del Pozo “A Study of a Rural Telemedicine System in the Amazon Region of Peru” *Journal of Telemedicine and Telecare*, Vol. 10. No. 4, 1 August 2004, pp. 219-225(7).
- [27] McCombs, Jeffrey S. “Physician Treatment Decisions in a Multiple Treatment Model: The Effect of Physician Supply,” *Journal of Health Economics*, 3(2), August 1984: 155-171.
- [28] Miller E. A. “Telemedicine and doctor-patient communication: an analytical survey of the literature” *Journal of Telemedicine and Telecare*, Vol. 7, No. 1, 1 February 2001, pp. 1-17(17).
- [29] Moore, Thomas G. “The Purpose of Licensing” *Journal of Law and Economics*, Vol. 4. (Oct., 1961), pp 93-117.
- [30] National Center for Health Statistics (2007). Compressed Mortality File, 1989-1998 (machine readable data file and documentation, CD-ROM Series 20, No.2E), National Center for Health Statistics, Hyattsville, Maryland.
- [31] National Center for Health Statistics. Compressed Mortality File, 1999-2004 (machine readable data file and documentation, CD-ROM Series 20, No. 2J). Hyattsville, Maryland. 2006.
- [32] Nordal, E.J., Moseng D., Kvammen B., Lochen M-L. “A Comparative Study of Teleconsultations versus Face-to-Face Consultations” *Journal of Telemedicine and Telecare*, Vol. 7, No. 5, 1 October 2001, pp 257-265(9).
- [33] Oakley, Amanda M.M., Felicity Reeves, Jane Bennett, Stephen H. Holmes, Hadley Wickham “Diagnostic Value of Written Referral and/or Images for Skin Lesions” *Journal of Telemedicine and Telecare*, Vol. 12, No. 3, April 2006, pp 151-158(8).
- [34] Oztas M.O., Calikoglu E., Baz K., Birol A., Onder M., Calikoglu T., Kitapci M.T., Reliability of Web-based Teledermatology Consultations” *Journal of Telemedicine and Telecare*, Vol. 10, No. 1, 1 February 2004, pp 25-28(4).
- [35] Peltzman, Sam “Toward a More General Theory of Regulation” *Journal of Law and Economics*, Vol. 19, No. 2, Conference on the Economics of Politics and Regulation (Aug., 1976), pp. 211-240.
- [36] Rimer, Barbara K.; Elizabeth J Lyons; Kurt M Ribisl; J Michael Bowling; Carol E Golin; Michael J Forlenza; Andrea Meier. “How New Subscribers Use Cancer-Related Online Mailing Lists,” *Journal of Medical Internet Research*, 2005, 7(3):e32
- [37] Rutten L.F., Moser R.P., Beckjord E.B., Hesse B.W., Croyle R.T.. (2007) Cancer Communication: Health Information National Trends Survey. Washington, D.C.: National Cancer Institute. NIH Pub. No. 07-6214 Available at: [http://hints.cancer.gov/hints/docs/hints\\_report.pdf](http://hints.cancer.gov/hints/docs/hints_report.pdf).

[38] Schneider, Lynne, Benjamin Klein, and Kevin M. Murphy. "Government Regulation of Cigarette Health Information." *Journal of Law and Economics*, 24(3), December 1981, 575–612.

[39] Smith A.C., Youngberry K., Christie F., Isles A., McCrossin R., Williams M., Van der Westhuyzen J., Wootton R. "The Family Cost of Attending Hospital Outpatient Appointments via Videoconference and in Person" *Journal of Telemedicine and Telecare*, Vol. 9., Supplement 2, 2 December 2003, pp. 58-61(4).

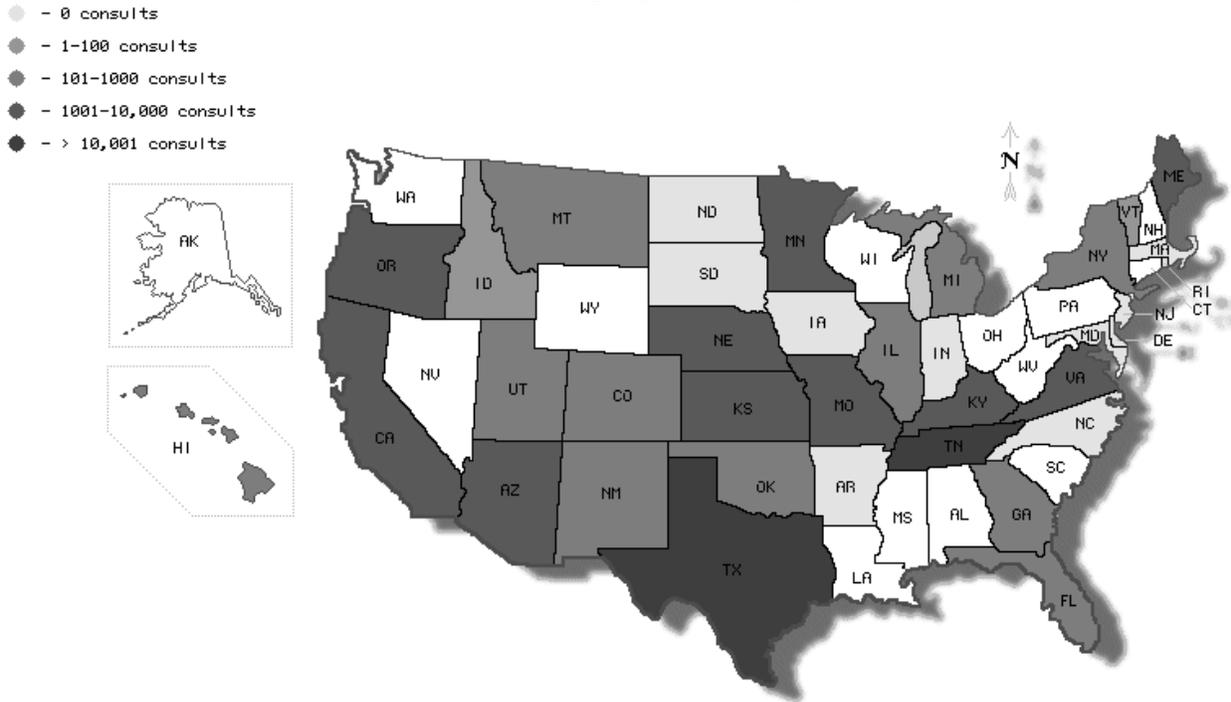
[40] Stalker H.J., Wilson R., McCune H., Gonzalez J., Moffett M., Zori R.T. "Telegenetic Medicine: Improved Access to Services in an Underserved Area" *Journal of Telemedicine and Telecare*, Vol. 12, No. 4, June 2006, pp 182-185(4).

[41] Tachakra S., Lynch M., Newson R., Stinson A., Sivakumar A., Hayes J., Bak J. "A Comparison of Telemedicine with Face-to-Face Consultations for Trauma Management" *Journal of Telemedicine and Telecare*, Vol. 6, Supplement 1, 10 February 2000, pp 178-181(4).

[42] Tachakra S., Loena M., Uche C.U. "A Follow-up Study of Remote Trauma Teleconsultations" *Journal of Telemedicine and Telecare*, Vol. 6, No.6, 1 December 2000, pp 330-334(5).

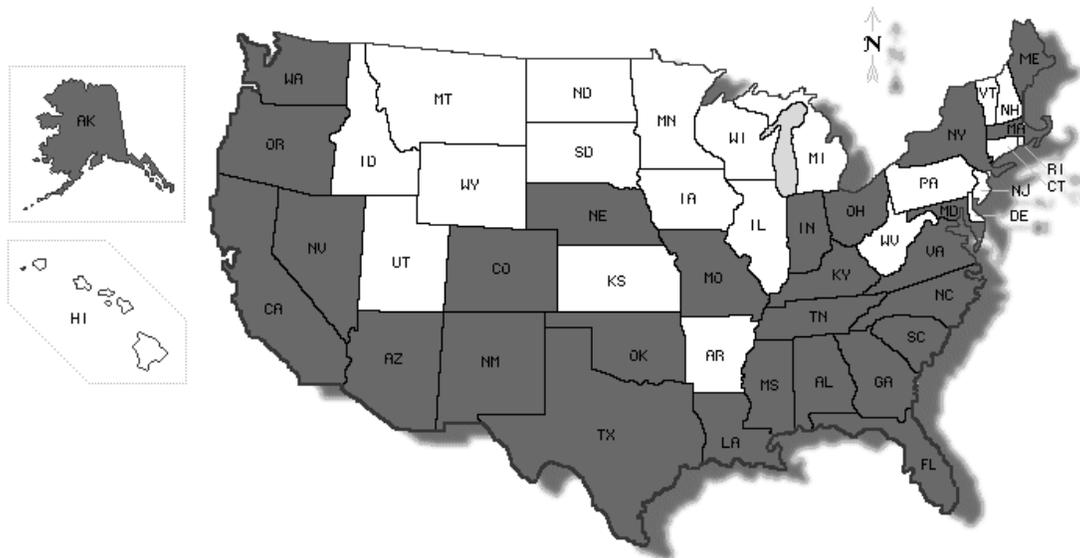
[43] U.S. National Library of Medicine, "MedlinePlus Survey Results: 2005" webpage. <http://www.nlm.nih.gov/medlineplus/survey2005/index.html>

Figure 1. Number of Non-Radiology Teleconsults per State (based on 88 telehealth programs surveyed in 2003)



Note: Areas not shaded did not respond to the survey  
 Source: Grigsby, Bill “2004 TRC Report on US Telemedicine Activity With an Overview of Non-US Activity”, New-Jersey: Civic Research Institute Inc, 2004: 88

Figure 2: *PER* Coverage in 2003



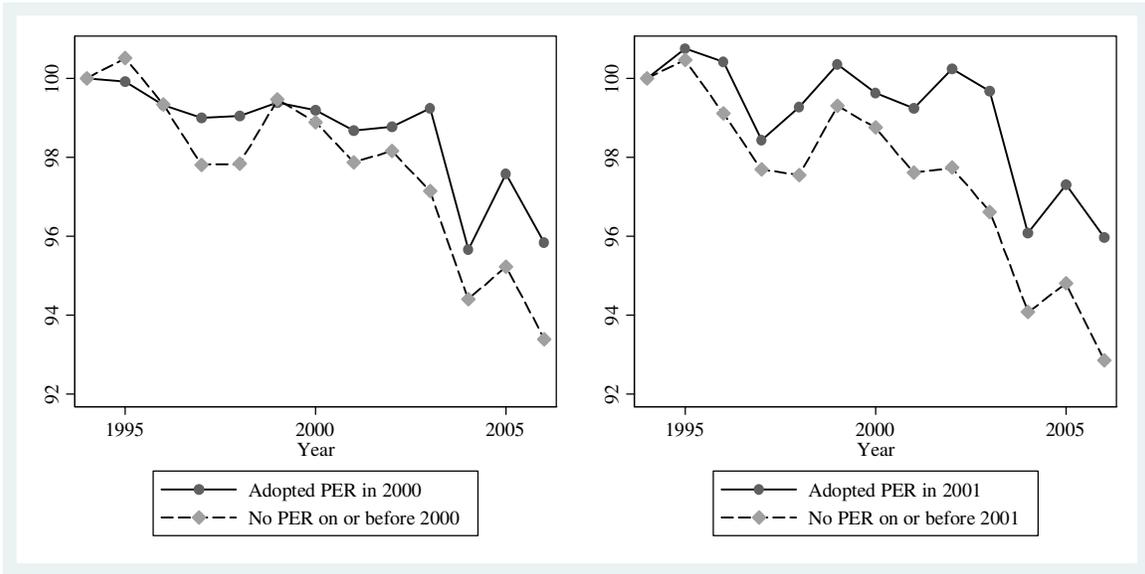
Note: Shaded areas represent states that adopted *PER* by 2003  
 Source: Federation of State Medical Boards; Office for the Advancement of Telehealth; States Legislatures

Figure 3



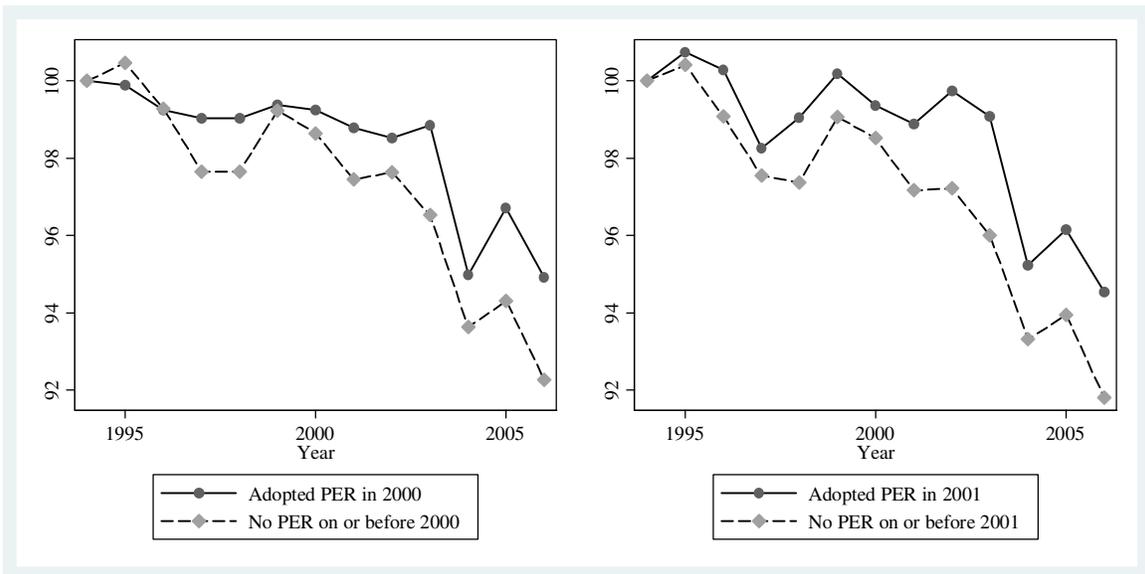
Data are national aggregate mortality per 100,000 individuals, indexed to equal 100 in the year 1994

Figure 4  
Mortality Rates comparison of *PER* Adopting and Non-adopting States - Raw Data



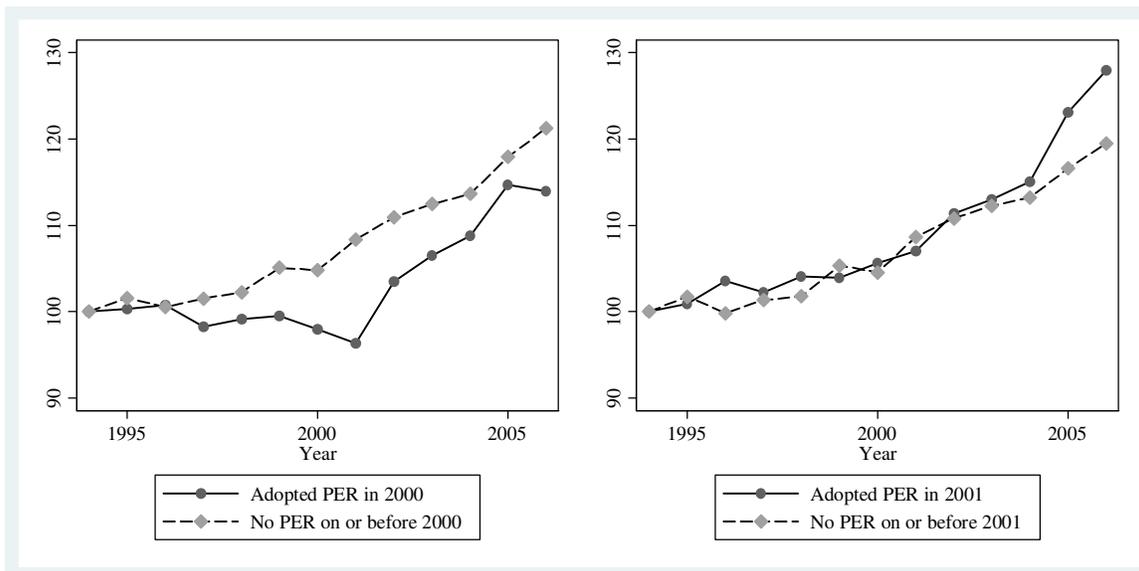
Data are national aggregate mortality per 100,000 individuals, indexed to equal 100 in the year 1994

Figure 5  
Non-Injury Mortality Rates comparison of *PER* Adopting and Non-adopting States - Raw Data



Data are national aggregate mortality per 100,000 individuals, indexed to equal 100 in the year 1994

Figure 6  
 Injury Mortality Rates comparison of *PER* Adopting and Non-adopting States - Raw Data



Data are national aggregate mortality per 100,000 individuals, indexed to equal 100 in the year 1994

Table 1: Summary of State Policies Prohibiting Physicians from Prescribing Drugs without a Prior Physical Examination of the Patient

State	Year	
Alabama	2000	AL Admin. Code Rules Chapter 540-X-9-11ER
Alaska	2000	AK Admin. Code Title 12, Part 1, Chapter 40, Article 6 Section 967
Arizona	2000	AZ Rev. Stat. § 32-1831
California	2000	CA Bus. & Prof. Code §§ 4067, 2242.1
Colorado	2000	Board Policy
District of Columbia	1998	Board Policy
Florida	2003	64B8-9.014 Standards for Telemedicine Prescribing Practice.
Georgia	2002	Rules 360-3-.02
Idaho	2006	ID Statutes Section 54-1733.
Indiana	2003	844 IAC 5-3-1 Rule 3 & 844 IAC 5-4-1 Rule 4
Kentucky	2002	KRS 311.597(1)(e)
Louisiana	2000	Board Policy
Maine	2002	Board Policy
Maryland	1999	Board Policy
Massachusetts	2001	Board Policy
Mississippi	2000	Board Policy
Missouri	2001	MO Statute 334.100.2(4)(h)
Nebraska	2001	Board Policy
Nevada	2001	NV Revised Statutes 453.3611-453-3648
New Hampshire	2004	Board Policy
New Mexico	2001	NM Admin Code, Title 16, Chapter 10, Part 8, Section 8
New York	2003	Board Policy
North Carolina	1999	Board Policy
Ohio	1999	OH Board Administrative Rules 4731-11-09
Oklahoma	2000	Board Policy
Oregon	2001	Board Policy
South Carolina	2001	Board Rule
Tennessee	2000	Board Policy
Texas	1999	Board Policy
Utah	2004	Code 58-1-501
Virginia	2000	Code 54.1-3303
Washington	2001	Board Policy
West Virginia	2004	Title 11, Legislative Rule, WV Board of Medicine

Source: Federation of State Medical Boards; Office for the Advancement of Telehealth; States Legislatures

Table 2: Are *PER* Adopting and Non-Adopting States Similar?  
 Panel A: Pairwise t-Tests of Sample Balance

	No PER	PER	t tests
Population	817.578 (1371.472)	1073.939 (1939.334)	0.50
Age 15-24	13.414 (2.829)	13.777 (2.755)	1.09
Age 25-44	30.843 (2.477)	31.575 (3.202)	1.62
Age 45-64	20.993 (1.631)	20.518 (2.154)	-1.24
Age > 65	13.549 (3.082)	12.294 (3.765)	-1.64
Female	51.207 (1.047)	50.989 (1.309)	-0.95
Black	10.575 (12.106)	13.627 (13.804)	1.42
Log wages	2.913 (0.235)	2.854 (0.215)	-1.25
Physicians	2.456 (1.645)	2.390 (1.847)	-0.37
Log( Mortality Rate)*10	67.526 (2.372)	66.645 (2.703)	-1.67

Notes: All entries represent weighted averages, where the weight is county population. All averages are calculated on all available county data for year 1997 (similar results are obtained using year 1994). \* significant at 5% significance level; \*\* significant at 1% significance level.

Table 2 Panel B: Multivariate Regression

	[1]	[2]
Population	0.312*10 <sup>-4</sup> (0.300*10 <sup>-4</sup> )	0.357*10 <sup>-4</sup> (0.300*10 <sup>-4</sup> )
Age 15-24	-0.006 (0.012)	-0.017 (0.012)
Age 25-44	0.037 (0.023)	0.011 (0.016)
Age 45-64	0.013 (0.023)	-0.009 (0.021)
Age > 65	0.013 (0.027)	-0.010 (0.016)
Female	0.002 (0.028)	-0.002 (0.029)
Black	0.008** (0.004)	-0.001 (0.003)
Log wages	-0.993** (0.393)	-0.502* (0.266)
Physicians	0.021 (0.018)	0.028* (0.015)
Mortality Rate	-0.049* (0.029)	-0.024 (0.023)
South		0.343 (0.117)

Notes: These are Probit regressions using 1997 county level data. The dependent variable is a dummy equal to 1 if the county had the regulation by 2006, and zero otherwise. Robust standard errors clustered at state level are reported in parentheses. \* significant at 5% significance level; \*\* significant at 1% significance level.

Table 3: Estimates of the Impact of *PER* Adoption on Health Outcomes

<i>Panel A</i>				
	Days Lost to Illness	Mortality Rate	Non-Injury Mortality Rate	Injury Mortality Rate
	[1]	[2]	[3]	[4]
<i>PER</i> , t	0.129* (0.067) [0.206]	0.008 (0.023)	0.020 (0.023)	-0.236 (0.168)
Observations	2,121,563	40,776	40,776	40,776
R squared		0.9741	0.9736	0.7145
<i>Panel B</i>				
	[5]	[6]	[7]	[8]
<i>PER</i> , t	0.081* (0.048) [0.127]	-0.010 (0.023)	0.001 (0.023)	-0.220 (0.169)
<i>PER</i> , t-1	0.106** (0.046) [0.168]	0.044** (0.021)	0.045** (0.021)	-0.038 (0.103)
Observations	2,121,563	40,776	40,776	40,776
R squared		0.9741	0.9736	0.7145

Notes: Period investigated is 1994–2006. The dependent variable in specification [1] and [5] measures the number of days lost to illness in the past 30 days. The dependent variable in all other specifications is the log of annual mortality rate per 100,000 people. To improve readability the log was multiplied by 10. Specifications [1] and [5] report the estimates obtained using Negative Binomial models that control for state and year fixed-effects, and state specific time trends and for gender, race, age, education, marital status, income, health insurance, and state level physicians per capita. Robust standard errors clustered at state level are reported in parentheses. Marginal effects are reported in brackets. The estimates reported in columns [2]–[4] and [6]–[8] are from fixed effects regressions based on a sample of 3136 counties. These regressions include county and year fixed-effects, and state specific time trends. Other controls included in these regressions are county gender, age, and race composition, log wages, physicians, and state health and hospital expenditures. These regressions are weighted by the county population. Robust standard errors clustered at state level are reported in parentheses. \* significant at 10% significance level; \*\* significant at 5% significance level; \*\*\* significant at 1% significance level.

Table 4: Estimates of the Impact of *PER* Adoption on Mortality in Rural versus Urban Counties

	Mortality Rate	Non-Injury Mortality Rate	Injury Mortality Rate
<i>PER</i>	0.113** (0.043)	0.121** (0.045)	-0.210 (0.153)
<i>PER</i> *% Urban	-0.002** (0.001)	-0.003** (0.001)	-0.001 (0.003)
Observations	40776	40776	40776
R squared	0.9742	0.9736	0.7145

Notes: *PER* is measured in period t-1 in the first 2 columns and in period t in the last column. The dependent variable is the log of annual mortality rate per 100,000 people. To improve readability the log was multiplied by 10. The variable “% Urban” represents the percent of the county population living in urban areas in 2000. This variable is centered around 50 such that the main effect is calculated for a person living in a county classified as 50% urban. The main effect for the “% Urban” variable is also included. The estimates are from fixed effects regressions based on a sample of 3136 counties for the period 1994-2006. Each model includes county and year fixed-effects, and state specific time trends. Other controls are county gender, age, and race composition, log wages, physicians, and state health and hospital expenditures. All regressions are weighted by the county population. Robust standard errors clustered at state level are reported in parentheses. \* significant at 10% significance level; \*\* significant at 5% significance level; \*\*\* significant at 1% significance level.

Table 5: Estimates of the Impact of *PER* Adoption on Mortality by Physician Density

	Mortality Rate	Non-Injury Mortality Rate	Injury Mortality Rate
<i>PER</i>	0.043* (0.026)	0.049* (0.028)	-0.236 (0.168)
<i>PER</i> *Physicians	-0.053*** (0.014)	-0.053*** (0.014)	-0.031 (0.030)
Observations	40776	40776	40776
R squared	0.9743	0.9737	0.7145

Notes: *PER* is measured in period t-1 in the first 2 columns and in period t in the last column. The dependent variable is the log of annual mortality rate per 100,000 people. To improve readability the log was multiplied by 10. The variable “Physicians” is defined by the number of non-federal physicians for every 1,000 individuals and is mean centered. The main effect for the “Physicians” variable is also included. The estimates are from fixed effects regressions based on a sample of 3136 counties for the period 1994-2006. Each model includes county and year fixed-effects, and state specific time trends. Other controls are county gender, age, and race composition, log wages, physicians, and state health and hospital expenditures. All regressions are weighted by county population. Robust standard errors clustered at state level are reported in parentheses. \* significant at 10% significance level; \*\* significant at 5% significance level; \*\*\* significant at 1% significance level.

Table 6: Falsification Test - The impact of *PER* Adoption on Mortality Caused by Neoplasm

	All Non-Injury Mortality Rate	Neoplasm Mortality Rate	Other Non-Injury Mortality Rate
<i>PER</i> , t-1	0.045** (0.022)	0.010 (0.030)	0.057** (0.027)

Notes: The dependent variable is the log of annual mortality rate per 100,000 people. To improve readability the log was multiplied by 10. The estimates are from fixed effects regressions based on a sample of 3136 counties for the period 1994-2006. Each model includes county and year fixed-effects, and state specific trends. Other controls are county gender, age, and race composition, log wages, physicians, and state health and hospital expenditures. All regressions are weighted by county population. Robust standard errors clustered at state level are reported in parentheses. \* significant at 5% significance level; \*\* significant at 1% significance level.

Table 7: Alternative Specifications  
 Panel A: The Impact of *PER* Adoption on Mortality

	Mortality Rate	Non-Injury Mortality Rate	Injury Mortality Rate
[1] Main	0.039* (0.020)	0.045** (0.022)	-0.236 (0.168)
[2] 1-Year lead of PER	-0.019 (0.034)	-0.011 (0.039)	-0.195 (0.170)
[3] 2-Year lead of PER	0.021 (0.028)	0.024 (0.027)	-0.037 (0.146)
[4] 3-Year lead of PER	0.031 (0.019)	0.032 (0.020)	-0.017 (0.162)
[5] Quadratic time trends	0.034 (0.021)	0.045** (0.022)	-0.302** (0.143)
[6] PER*South	-0.039 (0.043)	-0.041 (0.047)	0.244 (0.230)
[7] Early adoption	0.007 (0.059)	-0.004 (0.060)	0.276 (0.327)
[8] Late adoption	0.054 (0.071)	0.069 (0.079)	0.246 (0.259)
[9] Exclude county of state capital	0.046** (0.020)	0.052** (0.021)	-0.233 (0.162)
[10] Add state level covariates	0.038* (0.020)	0.043** (0.021)	-0.252 (0.176)
[11] County pop>10,000	0.039* (0.020)	0.045** (0.022)	-0.237 (0.173)
[12] Alaska excluded	0.039* (0.020)	0.045** (0.022)	-0.244 (0.168)
[13] Trend break	0.001 (0.009)	-0.006 (0.009)	0.087** (0.042)
[14] Cluster by county	0.039** (0.016)	0.045*** (0.016)	-0.236*** (0.074)
[15] Only adopting states	0.021 (0.018)	0.030 (0.020)	-0.225 (0.185)
[16] State level	0.028 (0.021)	0.033 (0.021)	-0.119 (0.117)

Notes: The independent variable, *PER* is measured in period *t-1* in the first 2 columns and in period *t* in the last column. The dependent variable is the log of annual mortality rate per 100,000 people. To improve readability the log was multiplied by 10. Estimates reported are from fixed effects regressions based on a sample of 3136 counties for the period 1994-2006. Unless otherwise specified, the model specifications include county and year fixed-effects, and state specific trends. All specifications control for county gender, age and race composition, log wages, physicians, and state health and hospital expenditures. The regressions are weighted by the county population. Robust standard errors clustered at state level are reported in parentheses. Row [1] gives the coefficient estimate and standard errors from the primary specification. Row [6] reports the coefficient on the interaction term between the *PER* variable and a dummy equal to 1 for southern states. Row [7] reports the coefficient on the interaction term between the *PER* variable and a dummy equal to 1 if the state adopted *PER* before 2000 and zero otherwise (excluding 2000). Row [8] reports the coefficient on the interaction term between the *PER* variable and a dummy equal to 1 if the state adopted *PER* after 2002 and zero otherwise (excluding 2002). Row [10] adds to the primary specification state level controls: percent without health insurance, and percent with at least high-school education. Row [13] reports the coefficient for the trend break.

Table 7 Panel B: The Impact of *PER* Adoption on the Number of Days Lost to Illness

	Days Lost to Illness
[1] Main	0.145** (0.067) [0.234]
[2] OLS (Log dependent variable)	0.055*** (0.019)
[3] Trend break	0.025 (0.023) [0.038]
[4] Omit Alaska	0.145** (0.067) [0.233]
[5] Only adopting states	0.064*** (0.023) [0.102]
[6] Early adoption	0.201 (0.182) [0.334]
[7] Late adoption	-0.067 (0.108) [-0.097]

Table 7B continues

---

[8] By gender	
PER, t-1	0.139** (0.069) [0.223]
Female*PER, t-1	0.015 (0.019) [0.023]
[9] By race	
PER, t-1	0.163** (0.066) [0.265]
Black*PER, t-1	-0.150*** (0.040) [-0.210]
[10] By Income	
PER, t-1	0.193*** (0.068) [0.311]
Inc 25k-50k * PER, t-1	-0.038 (0.035) [-0.054]
Inc 50k-75k * PER, t-1	-0.095 (0.060) [-0.133]
Inc >75k * PER, t-1	-0.077 (0.095) [-0.108]

---

Notes: The dependent variable measures the number of days lost to illness in the past 30 days. Panel B reports the estimates obtained using Negative Binomial models that control for state and year fixed-effects, and state specific time trends and for gender, race, age, education, marital status, income, health insurance, and state level physicians per capita. Robust standard errors clustered at state level are reported in parentheses. Marginal effects are reported in brackets. Row [3] reports the coefficient for the trend break. Row [6] reports the coefficient on the interaction term between the *PER* variable and a dummy equal to 1 if the state adopted *PER* before 2000 and zero otherwise (excluding 2000). Row [7] reports the coefficient on the interaction term between the *PER* variable and a dummy equal to 1 if the state adopted *PER* after 2002 and zero otherwise (excluding 2002).