Medical Regulation and Health Outcomes:

The Impact of the Physician Examination Requirement

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September 2010

Abstract

This paper investigates the impact on health outcomes of the regulation prohibiting physicians from prescribing drugs without a prior physical examination. This requirement could improve health by reducing illegal access to prescription drugs. But it reduces access to health care by making it more difficult for patient and physician to use many forms of telemedicine. Thus, this regulation generates a trade-off between access and safety. Our empirical results suggest that the physician examination requirement leads to an increase of approximately 14 percent in the number of days lost each month to illness and an increase in mortality rates of 0.4 percent, 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.

Key words: health outcomes, the physical examination requirement, safety-access trade-off,

telemedicine

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Introduction

The internet became a leading source of information and commerce during the 1990s. Two developments were directly relevant for health outcomes: the spread of telemedicine and the rise of the internet pharmacy. Both offered the promise of improved health outcomes, particularly for under-served rural communities. Telemedicine, which spans email, videoconferencing, and high speed transmission of diagnostic images, reduced the cost of long distance access to specialist care. Internet pharmacies reduced distribution costs, improved access for homebound patients, and increased price competition, especially in thin markets.

It quickly became apparent, however, that in the case of internet pharmacies, the new technology posed potential threats to patient care. Many internet pharmacies permitted patients to complete online health status questionnaires; based on these, prescriptions were written by doctors under contract with the pharmacies—physicians who had no physical contact with the patients. This process eliminated direct observation of patient by physician, traditionally a central element of diagnostic evaluation. It also became evident that the process was relatively easy to exploit by patients who wished to obtain potent pain-killers (e.g. Oxycontin, Vicodin) and other pharmaceuticals for use in non-therapeutic applications.

Across the country, a combination of medical boards and legislatures responded to these concerns by prohibiting physicians from prescribing medicine without conducting a prior physical examination. Since 1998 more than 30 states have instituted this type of rule, which has come to be known as the physical examination requirement (or PER hereafter). The avowed intent of this requirement is to improve the quality of physician diagnoses, by promoting direct observation of patient by physician, and to reduce patient abuse of the prescription process. But the requirement also has had the effect of reducing access to health care. Not merely did the PER raise the implicit cost of using internet pharmacies; perhaps more importantly, it made it more difficult for patient and physician to use many forms of telemedicine, including email, fax, and even telephone consultations.

The imposition of the physical examination requirement thus created a tradeoff between access and safety (or quality) in the provision of health care services. By raising the implicit cost of non-therapeutic use of potentially harmful drugs (such as Oxycontin), and by improving diagnoses, PER offered the promise of higher quality care. But the PER also made it more difficult and more costly for patients to receive care. Hence it is possible that the physical examination requirement has induced some patients to choose no medical care or less care, rather than the now higher-cost care. Whatever the original intentions of medical boards and legislatures, it is thus conceivable that health care outcomes have been worsened by the institution of PER. This paper seeks to determine what the net effect of the physician examination on health outcomes has in fact been.[1]

We examine the impact of PER rules on health outcomes over the period 1994 to 2006, utilizing measures of both mortality and morbidity. We find that adoption of the physical examination requirement is associated with a subsequent increase in mortality of about 33 deaths

[1] Note that the effect of PER on traditional care is also ambiguous. Electronic access to physicians may change patients' demand for face-to-face consults (Berman and Fenaughty, 2005). PER may induce some people to replace teleconsults with face-to-face consults, which will increase the number of visits to the doctor. But there may be complementarity between telecare and traditional care. In this case, televisits 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 teleencounters 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, we find no evidence of a significant change in the number of outpatient visits associated with PER adoption. (results not reported but available on request) We leave the further investigation of this topic for future research.

per one million people. Although this represents only about a 0.4 percent rise in mortality, the estimated effects are remarkably robust. Moreover, we find that the adoption of PER had its greatest impact in precisely the locales that would be expected: rural counties with low physician densities. In decomposing this impact on mortality, we find that the overall effect of PER includes both an increase in disease mortality and a *decline* in injury mortality rates, a result that we shall argue is also consistent with our view of this regulation.

The estimated effect on mortality likely understates the full impact of PER adoption on health outcomes, because it does not account for morbidity. Thus, we use the individual-level data contained in the Behavioral Risk Factor Surveillance System (BRFSS) to explore this issue. The relevant question in the survey asks: "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 the adoption of PER is associated with an increase in monthly days lost to illness of about 0.2. This outcome represents a nine percent increase in days lost, and provides additional independent evidence of the adverse health effects associated with PER adoption.

Our study has two important limitations. First, we retrospectively use observational data, rather than implementing a prospective, controlled trial. Hence we must attempt to control by statistical methods for factors that otherwise might be directly controlled for by trial design. Second, the physical examination requirement was not imposed on states; instead, it has been chosen by them. This raises the possibility that some other observed or unobserved confounding factors are correlated with both PER adoption and health care outcomes in ways that bias our results. Nevertheless, our overall statistical approach, complemented by exhaustive robustness checks of the results, suggests to us that it is unlikely that any residual biases are empirically important.

The remainder of the paper proceeds as follows. Section I reviews the statistical evidence 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.

I. Background: Medical Care Market and Technological Innovation

As commercial use of the internet expanded in the late 1990s, internet pharmacies quickly rose to prominence: between 1998 and 1999, for example, the number of websites devoted to retail delivery of pharmaceuticals rose to 400 from fewer than 30 (Rost, 2000). There were (and remain) three core models of the internet pharmacy (Oliver, 2000). The first type was either part of or partnered with existing brick and mortar pharmacies or mimicked their operations: They followed all procedures normally followed by traditional pharmacies, including accepting prescriptions only from patients' physicians. The second type of online pharmacy permitted patients to complete online health status questionnaires that were then evaluated by physicians under contract with the pharmacy. After a favorable review, the physician wrote a prescription, which was then filled and sent to the patient. The third type of online pharmacy—typically headquartered in a foreign nation—dispensed with much, if not all, of the foregoing formalities. Patients requested drugs online, and paid for and received them, often with no physician oversight.

The first type of internet pharmacy generally followed local pharmacy board regulations. The physician examination requirement was directed not at them, but at the practices of second and third types of internet pharmacies. The practices of these pharmacies quickly produced a series of well-publicized stories of grave illness or death resulting from inappropriate prescriptions, or from drugs obtained with no prescription at all. As early as 1998 the medical board of the District of Columbia implemented the first physical examination requirement, and it was followed in 1999 by similar medical board actions in four states. Over the next five years, medical boards in 13 states and legislatures in 15 more states followed suit with physical examination requirements (see Table 1). The motivating factor in all instances that we have been able to document was the provision of prescription drugs without appropriate physician oversight.[2]

While not specifically directed at telemedicine, the imposition of physical examination requirements certainly had the potential to adversely affect key aspects of it. The practice of medicine at a distance, what we now call telemedicine, had its roots in 1960s. But telemedicine expanded dramatically in the 1990s, as 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. With physician-physician telemedicine, patients obtain specialized advice from a physician they never meet, through the intermediary of another physician that meets with the patient in person. In the case of physician-patient telemedicine, the patient obtains medical advice in the absence of any face-to-face encounter with a physician. It is here that PER

[2] Some examples: Prepared Statement of the Federal Trade Commission on "Drugstores on the Net: The Benefits and Risks of Online Pharmacies" Before the Subcommittee on Oversight and Investigations of the Committee on Commerce United States House of Representatives, Washington, D.C. July 30, 1999; Virginia Study of the Sale of Prescription Drugs via the Internet; House Document NO. 13 - response of the Board of Medicine to the request contained in House Joint Resolution 759 of the 1999 Session of the General Assembly; White House Press Release, The Clinton Administration Unveils New Initiative to Protect Consumers Buying Prescription Drug Products Over the Internet, December 28, 1999 at http://clinton4.nara.gov/WH/New/html/19991229.html

impinges, because it constrains the ways that physicians can tailor their decisions to fit the needs of their patients.

Even though PER constrains physicians to meet their patients in person before prescribing drugs, and thus limits certain forms of telemedicine, the likely impact of the regulations on health outcomes is ambiguous. Physician-physician telecommunication 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; yet physician-patient telemedicine has its detractors. The controversy stems from the fact that although telemedicine consultations have lower transportation costs (Smith et al., 2003) and lower time costs (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. 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) (Reynolds et al., 2009), which 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.[3]

The physician examination requirement thus presents a tradeoff between access and safety. By raising the implicit and explicit cost of care, it effectively reduces patient access to care. But by ensuring that a non-inferior method of production is used, it enhances the quality of the outcome when care is chosen. Whether a net change in health outcomes is detectable depends

[3] To consider but a few examples: 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). But Smith et al. (2003) found that of 58 ear, nose, and throat assessments, only 81% of the diagnoses were the same for the tele-consultation and the face-to-face consultation. 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). Another study found the agreement between face-to face and videoconferencing assessment of cognitive function of older adults to be 0.63 (Martin-Khan et al. 2007). In some cases, it appears that telemedicine may not be significantly less effective than in-person consults. Some examples are genetic services (Stalker et al., 2006) and high blood pressure (Bradford et.al. 2001).

in part on telemedicine's actual or potential importance in the market. Surveys of telemedicine use are sparse and are conducted erratically over time, but 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." [4] As early as 1995, an estimated 4,000 teleconsults per month were performed in rural hospitals nationwide (Hassol et al. 1996). This suggests that the PER is indeed binding, and that rural areas and those with low physician densities are likely to feel its impact most strongly.

II. Data and Empirical Approach

2.1. Data

This paper uses data covering 1994 to 2006 to investigate the impact on mortality and morbidity of regulations requiring physicians to perform a physical examination prior to prescribing drugs. Our sample begins in 1994 to allow us to include the earliest adopting states and to control for possible pre-existing trends in mortality and morbidity; it extends as far as practicable, given the lag times in the availability of data.

The source of mortality data is the Compressed Mortality Files compiled by National Center for Health Statistics (NCHS). These data are comprehensive, for they contain information from all death certificates filed in the 50 states and the District of Columbia. They also have the advantage of including detailed demographic information, including sex, race, and age.[5] To assess the potential impact of PER on morbidity, we use data from the Behavioral Risk Factor

[4] 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.

[5] We exclude murder and suicide and war-related mortality, because it seems implausible to attribute changes in them due to the physical examination requirement. Nevertheless, including them in the data has no substantive impact on any of our results.

Surveillance System (BRFSS). The measure of morbidity we use is number of days lost to illness, which is 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?" It is worth noting that by using county-level mortality data and individual-level morbidity data, we can think of the physical examination requirement as having been imposed by the state on the county and the individual. Because of the great heterogeneity that exists at the county and individual levels, the use of data at these levels reduces the chances that county-level or individual-level health status (the dependent variable) is exerting any important influence on the statewide choice of policy (the independent variable).

2.2. Empirical Strategy

The key identifying assumption in our analysis is that after controlling for county specific factors and state specific trends, PER adoptions are not driven by differences in health outcomes. Several pieces of evidence suggest that this assumption is valid. First, we conducted a test of the type suggested by Heckman and Hotz (1989), where the focus is comparing differences in pre-program outcomes between the treatment and comparison groups to look for signs of selection.[6] 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

[6] The idea behind these tests is that the mean outcome of the control group provides an acceptable estimate of the counterfactual mean if selection bias is balanced between the two samples (participants and nonparticipants) so that it cancels out when computing the mean impact (Heckman and Smith, 1995).

marginal effects from Probit specifications that consider all of these variable simultaneously.[7] The results are robust to model specification: columns [1] and [2] present a probit model using 1997 data, while columns [3] and [4] present the coefficients obtained from a discrete-time hazard model implemented via probit. Two variables are statistically significant in both models: percent black and log wages. This correlation is probably driven by the high proportion of Southern states among those adopting PER; black populations in the South are higher than the national average, and wages are lower. After controlling for Census region fixed effects we see in columns [2] and [4] that for all practical purposes, the association of both variables with PER is completely or substantially eliminated, a finding that emphasizes the importance of controlling for regional heterogeneity, which we do in our analysis of the effects of PER, reported later.[8]

Numerous other validity tests including specifications that control for selection through matching are reported in the robustness analysis. We note here, however, that Figure 1 presents the 2003 geographical distribution of non-radiology consults through telemedicine networks (Grigsby, 2004), a proxy for total teleconsults. There is no indication of any appreciable correlation between telemedicine use and *PER* adoption, which is mapped in Figure 2.[9] It is thus unlikely that regulation adoption is driven by differences in health outcomes caused by the practice of telemedicine. We also note that, as shown in Figure 4 the trends in mortality rates in the early years of our sample appear to be much the same among both adopters of PER and non-adopters. Combined, these results suggest that the identification assumption is plausible, an inference that is supplemented later with additional evidence.

[7] The multivariate regressions exclude percent female because it is highly correlated with the combination of over 65, income, and regional dummies, so much so that its inclusion induces poor convergence properties for probit.
[8] Although many Southern states adopted *PER*, in the robustness checks reported later, we show that after controlling for state fixed effects and state specific time trends, the *PER* effect does not vary by region.
[9] Reports of the use of telemedicine are in terms of broad estimated ranges—data that are suitable for constructing suggestive maps such as ours, but not for compelling statistical tests. Nevertheless, if we use the mid points of the ranges reported, we find that the coefficient of correlation between this and PER adoption is approximately 0.25. This measure of telemedicine use is not a statistically significant predictor of PER adoption (p-value 0.146).

Our main specification for mortality estimates the following equation using county-level panel data:

$$(1) \qquad \textit{Mortality rate}_{ct} = \beta\, PER_{sz} + \theta\, X_{ct} + \gamma_c + \lambda_t + \omega_s t + HHEXP_{st} + \varepsilon_{ct},$$

where *c* indexes counties, *s* states, and *t* time. Because the health effects of PER may lag behind its adoption, we are agnostic ex-ante as to whether a change in *PER* status should be recorded with a lag or not; hence we let the data indicate the relevant time period, denoted by *z* in equation 1. The dependent variable is the log of mortality rate per 100,000 individuals, implying that that coefficient estimates can be interpreted as percentage changes.[10] On the right-hand side, *PER* stands for Physical Examination Requirement, and it is a (0, 1) 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, log wages, the number of physicians in the county per 1,000 residents, and dummies for county age composition: 15-24, 25-44, 45-64, and 65 and above. By including county fixed effects, γ_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 climatic conditions or unmeasured cultural factors). Year fixed effects, λ_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, ω_{st}, controls for differences in the general trends in mortality in a state that that might affect the likelihood of state adoption of

[10] 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.

PER.[11] 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, which would affect the likelihood of adoption of health regulations. Finally, ε_{ct} is the error term.

Telemedicine generates the most significant savings in the time cost rather than the monetary cost of telemedicine. Hence, we shall separately investigate the effect of telemedicine on those groups that experience the largest savings: people located in predominantly rural areas, and people located in areas with a low density of physicians.

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, we report weighted regressions with the weights being 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 repeals in the data. It is thus likely that the error terms are correlated within each 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. To correct for all these potential problems, this paper reports robust standard errors clustered at

[11] 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* variable because now the *PER* must be exogenous only after accounting for state-specific trends in mortality rates.

the state level, a method that allows for an arbitrary autocorrelation process (Bertrand et al., 2004).[12]

For the analysis of morbidity, employing the BRFSS individual-level data[13], we use a negative binomial[14] model with a similar specification:

$$(2) \qquad \textit{Days lost to illness}_{it} = \beta\, PER_{sz} + \theta\, X_{it} + Physicians_{st} + HHEXP_{st} + \gamma_s + \lambda_t + \omega_s t + \varepsilon_{it},$$

where X is a vector of individual characteristics, such as gender, race, age, and income. The variable *physicians* is measured at the state level and represents the number of physicians per 1000 people. The model specification includes state fixed effects, γ_s , time fixed effects, λ_t , and state specific time trends, $\omega_s t$. All regressions were estimated using BRFSS weighting variables, to account for its survey nature. Robust standard errors clustered at state level are calculated and reported throughout the analysis.

III. Results

3.1. Main Specification

PER is expected to affect health through three channels. First, by reducing non-therapeutic access to pain-killers and other psychoactives, PER should reduce the incidence of accidental poisonings and other accidents (e.g., motor vehicles) that might occur while under the influence of such drugs. Second, by enhancing physician oversight of access to prescription drugs, it should reduce the incidence of adverse drug interactions and other adverse effects associated with self-diagnosis and treatment. Third, by raising the implicit and explicit costs

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[12] As shown among the robustness checks in the Supplemental Results Appendix, our results continue to hold even under clustering at county rather than the state level. In addition, we performed Levin-Lin-Chu stationarity tests for mortality rates and found we cannot reject the null hypothesis of stationarity.
[13] BRFSS does not have data for Rhode Island in 1994 and for District of Columbia for 1995.
[14] A Poisson specification yields similar results, but tests indicate that over-dispersion is present; as with all results reported but not presented, these are available on request.

medical care, *PER* should increase the time to diagnosis and treatment and might even make self-diagnosis and treatment the preferred health care option for some people. Thus, we predict that if *PER* is to improve health outcomes it is more likely to do so in the case of accidental injuries. We also expect that the effects of *PER* on injury-related mortalities will be more rapid than for disease mortality, because the impacts of treatment delays and greater use of self-diagnosis and self-treatment will manifest themselves only over time through the working of the disease processes.

Table 3 presents the main results obtained from the estimation of equations (1) and (2) for the overall mortality and morbidity rates, and also distinguishes between the causes of death: injury versus disease. *PER* adoption leads to an increase in overall mortality and in the monthly number of days lost to illness. The timing of the effect depends on the nature of the health measure. For both overall mortality and disease mortality, the effects of PER show up with a one year lag. In the case of injury mortality, the effects are largest with no lag in effect. For morbidity (monthly days of illness) the impacts are roughly the same whether PER is treated as having an instantaneous effect or a lagged effect but the lag effect is more precisely estimated.[15]

We find some evidence that PER is associated with a decrease in injury mortality, with the caveat that the negative effect on injury mortality is not very precisely estimated.[16] Thus, the increase in overall mortality associated with the adoption of PER is driven by the increase in

[15] For symmetry we report results obtained using the same set of control variables. If our identifying assumption is correct, adding extra controls should not affect the magnitude of the estimates. The estimated effect of PER on morbidity (days lost to illness) is robust to adding controls for: education, health insurance, and marital status. Similarly, we find that the estimated effect of PER on mortality is robust to adding controls for medical care cost, or state level controls for education, health insurance, or Medicaid enrollment. These results are contained in the Supplemental Results Appendix.

[16] We obtain smaller standard errors and a statistically significant coefficient for injury mortality if we control for quadratic time trends. The caveat is that this specification may suffer from overcontrolling for unmeasured factors, known to sometimes lead to unstable parameter estimates (Schneider, Klein, and Murphy, 1981). No other estimates are sensitive to the inclusion of higher order time trends. Results reported in Supplemental Results Appendix.

mortality from disease-related causes, which is dampened by the modest beneficial impact of PER in lowering injury mortalities.

The magnitude of the PER effect is quantitatively small: Overall mortality increases by approximately 33 deaths per 1 million people, a 0.4% increase measured at the mean of the data.[17] The expected number of days lost to illness each month is approximately 0.25 higher for people in states that adopted *PER* than for people in states that did not adopt *PER*; this is roughly a fourteen percent increase in morbidity.

3.2 Falsification Tests

A useful check on our results is to look for an association between PER and mortality in samples where there is no reason for such a relationship. This we do in the following three sections of the paper, by focusing on population density, physician density, and cause of death.

3.2.1 Rural versus Urban

The physician examination requirement cannot adversely 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 areas where they must incur high transportation costs to get to a physician's office for a face-to-face consultation.

We thus predict that PER will have larger effects on health in predominantly rural counties. To identify whether the regulation affects these counties differently than predominantly urban counties, we interact the *PER* variable with the proportion of the county population living

[17] 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).

in urban areas as measured in year 2000[18] (sources of data are detailed in Data Appendix). As shown in Table 4, the adoption of PER is associated with an increase in mortality, but the effect is appreciably larger in predominantly rural areas.[19,20] Our explanation is that residents of rural communities are more likely to try to obtain electronically delivered medical advice than will urban residents. Hence, PER will impinge on a larger fraction of rural residents. In addition, because the transportation cost of face-to-face consultations is higher in rural areas than in urban areas, face-to-face consultations are a worse substitute for telecare in rural areas. Thus, in the face of PER, rural residents are less likely than urban residents to switch from telecare toward in-person consults and more likely to delay seeking diagnosis and treatment than are urban residents.

3.3.2 Physician Density

A second category of the population likely to use telemedicine consists of people located in counties with a low physician density. Because transportation costs are higher, the *PER* should have a larger adverse effect on the health of these individuals. To test this prediction, we interact the *PER* dummy with the number of physicians per 1,000 individuals in the county.[21,22] The estimates indicate that the effect of *PER* falls as physician density rises. These results are consistent with the interpretation that the fewer physicians there are, the more likely it is that

[18] The coefficients and standard errors of the interaction terms between *PER* and *% Urban* are: -0.002 (0.001) for total mortality rate, -0.003 (0.001) for disease mortality rate; and -0.001 (0.003) for injury mortality rate. These coefficients are significant at the 1% significance level in the case of total and disease mortality.
[19] The average individual lives in a county with approximately 79% urban population.
[20] While it is true that the variability of mortality rates is higher in less populated areas, these results are not driven by noise. We find that the results are not sensitive to excluding sparsely populated areas (county pop <10000); these results are reported in the Supplemental Results Appendix.
[21] The coefficients and standard errors of the interaction terms between *PER* and *Physicians* are: -0.053 (0.014) for total mortality, -0.053 (0.014) for disease mortality, and -0.031 (0.030) for injury mortality. These coefficients are significant at the 1% significance level in the case of total and disease mortality.
[22] The average individual lives in a county with 2.49 doctors per 1000 people.

PER will induce people to delay seeking medical help or even forgo medical care altogether and thus experience worsening health.

3.2.3 Cause of Death

Because the physician examination requirement 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 highly unlikely that physicians would recommend drugs for such a condition without ever meeting their patients in person, so as a practical matter the physical examination requirement should not be a binding constraint. Therefore, PER should have little if any effect on neoplasm mortality. Table 6 reports the estimated effect of the PER separately for neoplasm mortality and other disease mortality. The coefficient in the case of neoplasm mortality is very small and not statistically significant, even though neoplasm accounts for 25% of total disease mortalities. One standard error bands around the estimated effect on other disease mortality rates exclude the estimated effect on neoplasm mortality rate. We infer that the statistically significant effect of PER on disease mortality is driven by the impact on causes of death other than neoplasms.

3.3. Specification checks – addressing selection

Individual counties cannot choose whether or not to obey a state-wide regulation;[23] however, it could still be true that common pre-treatment characteristics of all counties in a state led to PER adoption. One way to check for signs of selection is to investigate how sensitive the estimates are to the specific periods over which "before" and "after" are defined (Heckman, 1999). Our results are robust to the exclusion of the four years 1994-1997 (the years prior to any

[23] Of course the residents of some counties might be better able to create pressure to obtain the desired regulation. Counties in which state capitals are located likely have a more significant weight in the decisions of the policy makers. The exclusion of such counties does not change the estimates we obtain, providing support for the identifying assumption. Again, these results are in the Supplemental Results Appendix.

adoption). They are also robust to the exclusion of 2003-2006, the last four years of our sample.[24]

We argue that PER adoption was triggered by concerns over unauthorized use of prescription drugs. Because such usage might be correlated with accidental injuries and injury mortality rates, this suggests the possibility of selection bias: high injury mortality states might be more likely to adopt PER. To control for this possible source of selection, we have also estimated our models using propensity score matching. We calculate propensity scores based on counties' pre-period characteristics[25]: oxycodone consumption per capita,[26] hydrocodone consumption per capita,[27] percent living in poverty, crime, high-school education, and percent black.[28] To achieve balancing of covariates we added several interaction terms (Dehejia and Whaba, 2002).[29] In the spirit of Rubin (1973) and Rubin (1979) we run the original specification on this smaller sample of matched counties. In the case on overall and disease mortality, the

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[24] Other tests of selection were tried with similar results. For example, we find no significant difference between early adopters and all other states. Under the hypothesis of selection, the early adopters should be the states to benefit most from PER, so these results provide additional support for the assumed exogenous nature of PER. We also find no significant difference between late adopters and all other states. All of the above mentioned results are reported in Supplemental Results Appendix.

[25] Institutional background suggests that PER was adopted to prevent misuse of prescription drugs. We chose to include in the calculations of the propensity score those variables expected to be correlated with misuse of prescription drugs. Because oxycodone and hydrocodone data is available starting 1997, *PER* status by 2006 was predicted based on the levels of all variables in 1997. We use nearest neighbor matching with replacement, a 0.0001 caliper.

[26] Oxycodone consumption spiked with the introduction of OxyContin. United States General Accounting Office; December 2003, "PRESCRIPTION DRUGS OxyContin Abuse and Diversion and Efforts to Address the Problem" mentions that media reports of OxyContin abuse and diversion began to surface in 2000 but people addicted to OxyContin reported to treatment centers as early as 1999.

[27] The 1999 Drug Abuse Warning Network, which collects data on drug-related episodes in hospital emergency departments, reported that mentions of hydrocodone as a cause for visiting an emergency room increased by 37 percent among all age groups from 1997 to 1999.

[28] Note that because oxycodone and hydrocodone data is available starting 1997, and PER is measured with a lag our matching results use only mortality data on and after 1999. Nevertheless, the estimates obtained on 1999-2006 sub-sample are substantially the same as those obtained on the entire sample, so the change in sample is not responsible for any differences in estimates. The estimates obtained using the main specification on 1999-2006 data are: 0.046 (0.022) significant at 5% for total mortality; 0.059 (0.021) significant at 5% for disease mortality, and -0.351* (0.202) significant at 10% for injury mortality.

[29] To achieve the balancing of covariates propensity score calculations include several interaction terms: oxycodone*crime; high-school*crime, high-school*poverty squared. Propensity score calculations are reported in the Supplemental Results Appendix.

matching estimates are substantively identical to those obtained without matching. For injury mortality the estimated coefficient is substantially the same, but is estimated more precisely estimated and in fact statistically significant.[30,31] Overall, it appears that selection bias is unlikely to be important in our analysis.

IV. Conclusions

Beginning in 1998 many states across America began requiring physicians to conduct physical examinations of patients prior to issuing any prescription for them. This physical examination requirement (PER) was initiated in response to the emergence and rapid spread of internet pharmacies. The intent of the requirement was to prevent or reduce non-therapeutic or inappropriate patient access to a variety of drugs, ranging from powerful pain-killers to treatments for erectile dysfunction. But the regulation also had the effect of raising the implicit cost of telemedicine, thereby creating a tradeoff: access to medical care was impaired, but for some patients the quality of the care provided was increased. The overall impact of PER rules is thus ambiguous, leading to our empirical investigation extent of the impact of the physician examination requirement on health outcomes.

Using county level data on mortality and individual level data on morbidity, we establish several key results. First, the adoption of PER is associated with an increase in overall mortality rates, by about 33 deaths per year per million persons, which is roughly 0.4 percent. This overall effect includes differential effects that depend on the cause of death. The adoption of PER is

[30] Similar results were obtained through matching without replacement, though a larger caliper was used (0.001) because otherwise the sample would have been too small. Note the sample drops substantially in matching with replacement, because out of 3125 counties 2185 had PER by the end of the period, which leaves just a small control group.

[31] The statistically significant (negative) effect for injury morality is not confined to the matching specification. It can also be obtained by using quadratic rather than linear time trends in our main specification (-0.302 statistically significant at the .05 level).

associated with a rise in disease-related mortality rates, presumably because it raised the implicit cost of, and thus reduced access to, medical care. To a smaller degree, the adoption of the physical examination requirement is associated with reduced injury-related mortalities, although this effect is less certain than, and smaller than, the observed elevation of disease-related mortality rates, thus yielding the rise in overall mortality.

The second documented impact of the physical examination requirement is on morbidity. Using individual-level data, we find that the adoption of PER is associated with an increase in monthly days lost to illness of about 0.25. This outcome represents a fourteen percent increase in days lost, and provides additional independent evidence of the adverse, albeit modest, impact of PER adoption on health outcomes.

As would be expected, given its posited impact on access to care, PER adoption has had its greatest effect by elevating mortality in rural areas, and in areas with low physician density. Further, we find that the effects of PER are more important for those classes of diseases where such an effect would be expected. We have conducted numerous sensitivity tests on the results, including: tests of the temporal validity of the identifying assumption, using matching to address selection, restricting the sample to the most populous counties, excluding counties of state capitals[32], and changing sample size with regard to years included and states included. Our results are robust to these alternative specifications.

In addition to offering insight into the observed health outcomes of the physical examination requirement as implemented at the state level, our paper offers guidance in other matters. For example, in 2008 the federal government implemented a nationwide physical examination requirement, prompted by concerns over non-therapeutic access to drugs from foreign-based internet pharmacies. Although some of the circumstances leading up to this

[32] Not all these results were reported in paper but are available in the Supplemental Results Appendix.

legislation differ from those observed earlier at the state level, the methods we use may be of value in subsequent examination of the effects of this federal law. Our findings are also relevant to policy discussions of the appropriate regulation of telemedicine. It appears that even if (as suggested by others) telemedicine offers somewhat lower quality care, its impact on access to care in rural and physician-deprived locales may be important in improving overall health outcomes. And finally, our results help illuminate some of the key margins to consider in the broader discussion over the regulation and provision of medical care, emphasizing the importance of identifying the relevant tradeoffs between access and quality of care.

Data Appendix

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. *Percent living in poverty* - U.S. Census Bureau's Small Area Income and Poverty Estimates (SAIPE)
5. *Physicians* – U.S. Department of Health and Human Services - Area Resource Files
6. *Medical care cost* – calculated as Medicare hospital cost divided by Medicare enrollment. Medicare hospital costs data source is Centers for Medicare and Medicaid Services Medicare, Cost Report Data files. Medicare enrollment data source is U.S. Census Bureau, USA counties data file.
7. *Crime Rate* - Uniform Crime Reporting Program Data [United States]: County Level Detailed Arrest and Offense Data

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. *Medicaid Enrollment* – Current Population Survey, March Supplement;
6. *CPI* price index – Statistical Abstract of the United States, 2008;
7. *Oxycodone, Hydrocodone Consumption* – US Department of Justice, Drug Enforcement Administration;
8. All other state level data – race composition, age composition and wages are obtained from the county level data.

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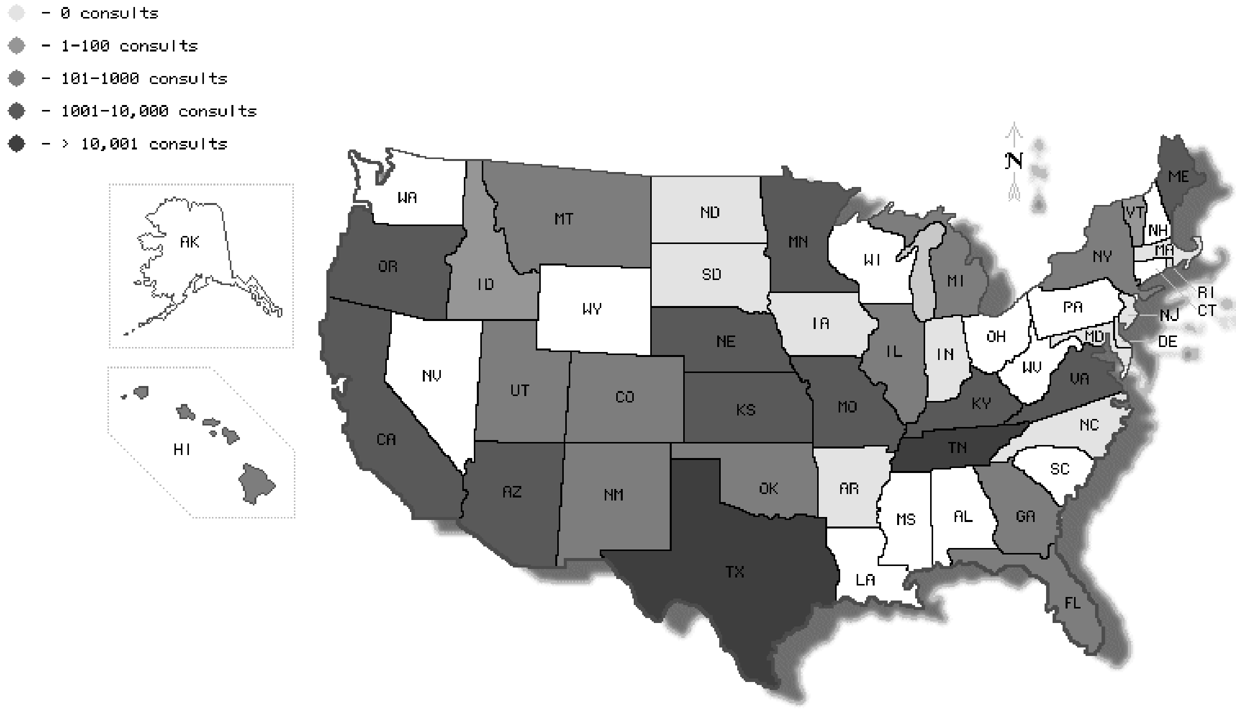
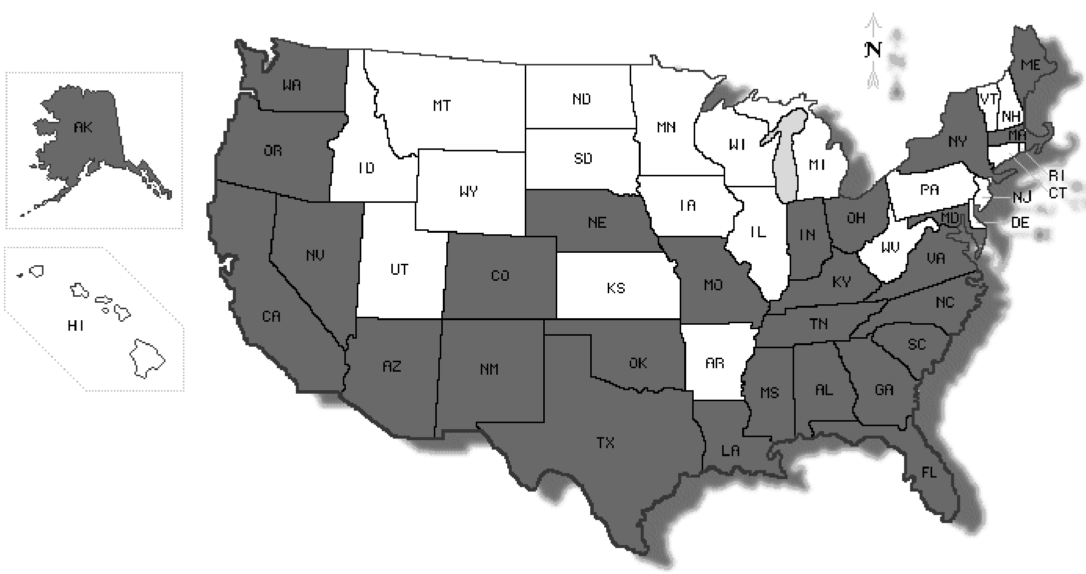
Figure 1. Number of Non-Radiology Teleconsults per State
(based on 88 telehealth programs surveyed in 2003)



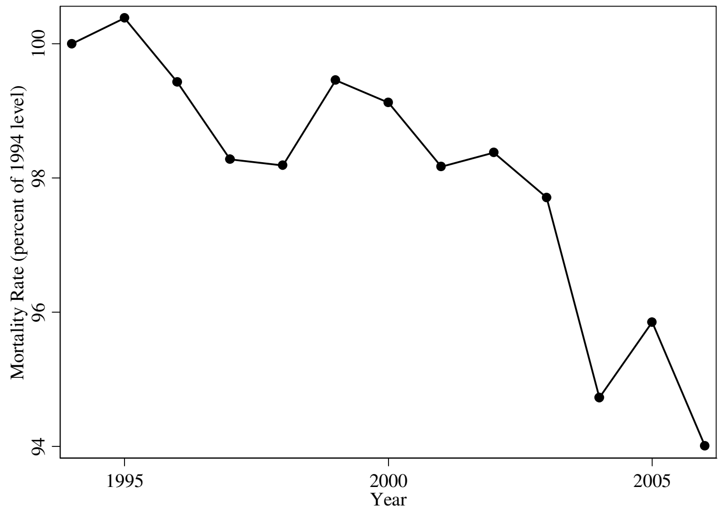
Note: Areas not shaded did not respond to the survey
Source: Grigsby, Bill "2004 TRC Report on US Telemedicine Activity With an Overview of Non-US Activity", New-Jersey: Civic Research Institute Inc, 2004: 88

Figure 2: *PER* Coverage in 2003



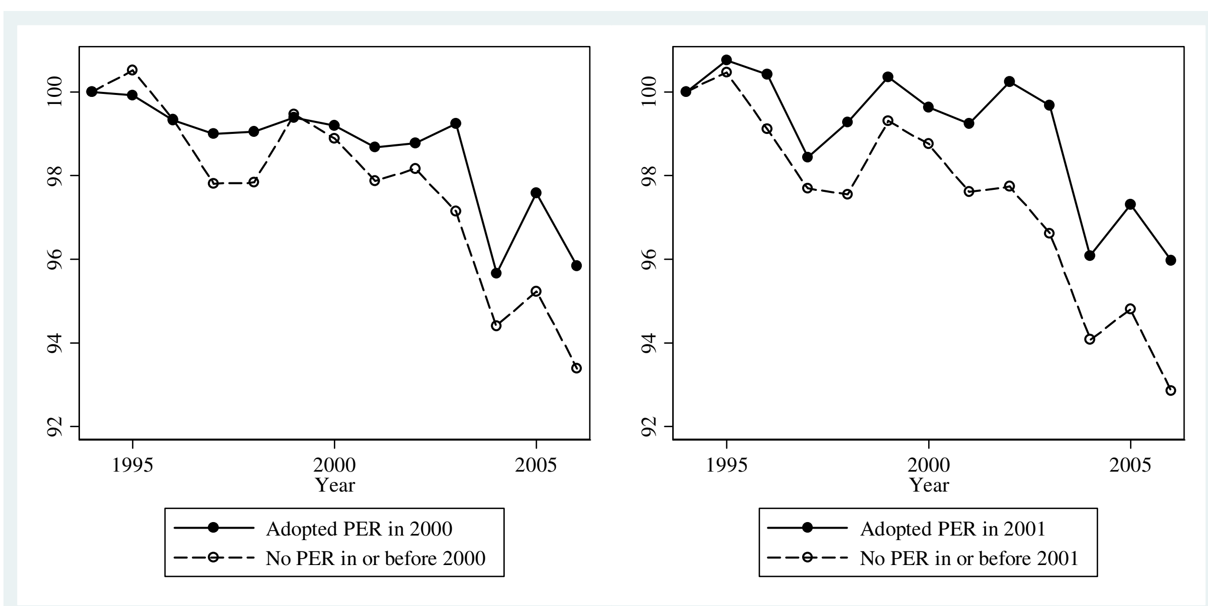
Note: Shaded areas represent states that adopted *PER* by 2003
Source: Federation of State Medical Boards; Office for the Advancement of Telehealth; States Legislatures

Figure 3
Mortality Rates 1994-2006



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

Figure 3
Mortality Rates comparison of PER Adopting and Non-adopting States



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

Table 1: State policies prohibiting physicians from prescribing drugs without a prior physical examination of the patient

State	Year	Implementation (Medical board or legislation)
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; State Legislatures

Table 2: Sample balance in *PER* adopting vs. non-adopting states

Panel A: Pairwise t-tests of variable means

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

Notes: All entries are weighted means (and standard deviations), where weights are county populations, for all available county data for 1997, the last year prior to any PER adoptions.
* significant at 10%; ** significant at 5% level; *** significant at 1% level.

Table 2: Sample balance in PER adopting vs. non-adopting states

Panel B: Multivariate regression tests of pre-period sample balance

Dependent variable is (0, 1) indicating PER status

	Probit		Hazard Model	
	[1]	[2]	[3]	[4]
Population	$0.318 \bullet 10^{-4}$	$0.088 \bullet 10^{-4}$	$0.145 \bullet 10^{-4}$	$0.145 \bullet 10^{-4}$
	$(0.300 \bullet 10^{-4})$	$(0.200 \bullet 10^{-4})$	$(0.100 \bullet 10^{-4})$	$(0.200 \bullet 10^{-4})$
Age 15-24	-0.006	-0.002	-0.003	-0.006
	(0.011)	(0.007)	(0.005)	(0.005)
Age 25-44	0.036	0.023	0.012	-0.003
	(0.020)	(0.015)	(0.008)	(0.009)
Age 45-64	0.013	0.006	0.003	-0.003
	(0.023)	(0.017)	(0.009)	(0.008)
Age > 65	0.013	-0.011	-0.002	-0.005
	(0.027)	(0.018)	(0.008)	(0.005)
Black	0.008**	0.002	0.002*	0.000
	(0.004)	(0.002)	(0.001)	(0.001)
Log wages	-0.994***	-0.425*	-0.362***	0.037
	(0.380)	(0.218)	(0.111)	(0.070)
Physicians	0.022	0.021	0.006	-0.000
	(0.019)	(0.015)	(0.006)	(0.004)
Mortality Rate	-0.049*	0.011	-0.018*	-0.003
	(0.029)	(0.025)	(0.010)	(0.005)
Region FE	No	yes	no	yes

Notes: Columns [1] and [2] show the marginal effects from a probit specification for 1997, the last year prior to any PER adoptions. Columns [2] and [3] show marginal effects from a discrete time hazard model, implemented via probit. Robust standard errors in parentheses, adjusted for clustering on state. The regions follow Census region definition: Northeast, Midwest, West, and South.
* significant at 10%; ** significant at 5% level; *** significant at 1% level.

Table 3: Estimates of the Impact of *PER* Adoption on Health Outcomes, 1994-2006

	Alternative lag structures		
	PER, t-1	*PER, t*	*PER, t+1*
Dependent Variable			
Mortality			
- all causes	0.039*	0.008	-0.019
	(0.020)	(0.023)	(0.034)
- disease	0.046**	0.019	-0.011
	(0.022)	(0.023)	(0.039)
- injury	-0.132	-0.236	-0.195
	(0.115)	(0.168)	(0.170)
Morbidity	0.262**	0.225*	0.133
	(0.120)	(0.116)	(0.124)
- days lost to illness			

Notes: Each cell represents the PER coefficient and standard error from a different regression.
Rows 1-3: The dependent variable in first 3 rows is the log of annual mortality rate per 100,000 people. To improve readability the log of mortality rates was multiplied by 10. The estimates are from weighted regressions for 3137 counties; they include 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. Weights in regressions are county populations.
Row 4: Using individual-level data, the dependent variable is the number of days lost to illness in the past 30 days. These estimates are marginal effects after negative binomial models that control for state and year fixed-effects, and state specific time trends and for gender, race, age, income, state level physicians per capita, and state health and hospital expenditures. Robust standard errors clustered at state level are reported in parentheses.
* significant at 10% level; ** significant at 5% level; *** significant at 1% level.

Table 4: Comparing the impact of PER adoption on mortality in rural versus urban counties

	Mortality Rate	Disease Mortality Rate	Injury Mortality Rate
75% Rural	0.175**	0.193***	-0.190
	(0.070)	(0.071)	(0.171)
50% Rural	0.113**	0.126***	-0.211
	(0.043)	(0.046)	(0.153)
25% Rural	0.051**	0.058**	-0.232
	(0.024)	(0.027)	(0.163)
20% Rural	0.039	0.045*	-0.236
	(0.024)	(0.026)	(0.168)
15% Rural	0.026	0.032	-0.241
	(0.024)	(0.026)	(0.173)

Notes: The estimates were obtained from a model including an interaction term between PER and a variable *% Urban* measuring the share of the county population living in urban areas in 2000. *PER* is measured in period t-1 in the first 2 columns and in period t in the last column. See text for explanation. The dependent variable is the log of annual mortality rate per 100,000 people. To improve readability the log of mortality rates was multiplied by 10. The average individual in our sample lives in a county that is approximately 21% rural. The estimates are based on 3137 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, with county populations as the weights. Robust standard errors clustered at state level are reported in parentheses.
* significant at 10% level; ** significant at 5% level; *** significant at 1% level.

Table 5: Comparing the impact of PER adoption on mortality by physician density

	Mortality Rate	Disease Mortality Rate	Injury Mortality Rate
1 physician/1000 pop	0.123***	0.130***	-0.068
	(0.023)	(0.025)	(0.093)
2 physician/1000 pop	0.069***	0.076***	-0.109
	(0.023)	(0.025)	(0.103)
2.5 physician/1000 pop	0.042	0.049*	-0.129
	(0.026)	(0.028)	(0.112)
3 physician/1000 pop	0.016	0.022	-0.150
	(0.030)	(0.032)	(0.122)

Notes: The estimates were obtained from a model including an interaction term between PER and the variable *Physicians,* defined as the number of non-federal physicians for every 1,000 individuals. The level of this variable is also included in the regressions. *PER* is measured in period t-1 in the first 2 columns and in period t in the last column. See text for explanation. The dependent variable is the log of annual mortality rate per 100,000 people. To improve readability the log of mortality rate was multiplied by 10. The estimates are based on 3137 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, with county populations as the weights. Robust standard errors clustered at state level are reported in parentheses.
* significant at 10%; ** significant at 5% level; *** significant at 1% level.

Table 6: The differential impact of *PER* adoption on neoplasm-caused mortality

	All disease mortality rate	Neoplasm mortality rate	Other disease mortality rate
PER, t-1	0.045**	0.010	0.057**
	(0.022)	(0.030)	(0.027)

Notes: The dependent variable is the log of annual mortality rate per 100,000 people. To improve readability the log of mortality rate was multiplied by 10. The regressions are for 3136 counties over 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, with county populations as the weights. Robust standard errors clustered at state level are reported in parentheses.
* significant at 10%; ** significant at 5% level; *** significant at 1% level.

Table 7: PER and Mortality - Specification checks of selection

	Mortality Rate	Disease Mortality Rate	Injury Mortality Rate
[1] 1998-2006	0.032	0.044**	-0.347*
	(0.021)	(0.020)	(0.174)
	[-0.24]	[-0.07]	[-0.46]
[2] 1994-2002	0.055*	0.063**	-0.268*
	(0.032)	(0.032)	(0.153)
	[0.42]	[0.44]	[-0.14]
[2] 1996-2004	0.065***	0.073***	-0.273
	(0.021)	(0.024)	(0.192)
	[0.90]	[0.83]	[-0.15]
[3] Matching	0.025	0.069**	-0.451***
	(0.032)	(0.033)	(0.139)
	[-0.37]	[0.58]	[-0.99]

Notes: The dependent variable is the log of annual mortality rate per 100,000 people. To improve readability the log of mortality rates was multiplied by 10. 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, with county populations as weights. Robust standard errors clustered at state level are reported in parentheses. t-statistics for the test of a difference between these results and the main results are reported in brackets.
* significant at 10%; ** significant at 5% level; *** significant at 1% level.

Supplemental Results Appendix (to be made available on a web-site)

SECTION 1: Tests of Model Specification – Mortality Regressions

The identifying assumption of our regression model is that PER is determined independently after accounting for county fixed effects and state specific trends. We run a series of tests of model specification to assess the validity of this assumption. The entries in Table A1 are the estimated impact (with standard error in parentheses) of PER on the outcome of interest. Each row applies to a separate specification.

Row [1] reports the main specification discussed in the text.

Under our identifying assumption, our estimates should be insensitive to changes in the set of control variables. This first section of the appendix examines this issue empirically.

Row [2] includes the cost of medical care as a regressor, showing that our estimates of the effects of PER are robust to this inclusion. Our measure of cost is county level hospital cost from Medical Audit data normalized by Medicare enrollment. Data was not available for some counties; in these cases we imputed the cost as being equal to that in the nearest county in the same year. (Specifically we searched up to the fifth closest county based on the distance between county population centroids.[33])

Row [3] adds a variable for Medicaid enrollment that controls for changes in access to health care due to changes in Medicaid eligibility. The estimates are substantively the same as the estimates obtained in the main specification.

Row [4] includes two potentially relevant time-varying state characteristics: proportion of the population without health insurance, and proportion with at least high-school education level, again showing that the estimates effects of PER are robust.

Rows [5] – [7] present additional checks of the sensitivity of the results to the choice of sample period. The results are robust to the exclusion of the early 2 years and late 2 years of our main sample. When the sample is further restricted to 1997-2002, the estimated effects of PER rise, but these estimates are not statistically different from the estimates obtained using the full sample. We think the results for 1997-2002 are influenced by the brevity of the sample period, which does not allow us to estimate the state specific trends with precision. Indeed, the results

[33] The results obtained without imputing the cost for counties with no data are substantively the same: 0.031 (0.022 for total mortality; 0.042 (0.021) significant at 5% significance level for disease mortality; and 0.364 (0.182) significant at 10% significance level for non-injury mortality.

obtained on 1997-2002 sample are in fact almost identical with the estimates obtained from a specification that does not control for state specific trends.[34] It is important to fully control for mortality trends because these could be important source of confound for PER. Mortality trends differ by geographical area due to differences in main cause of death and differential advances in medical knowledge about various diseases generate differential changes in trends. The estimates obtained on the 1994-2000 sample are of similar numerical magnitude as our main specification estimates, but not statistically significant. This is hardly surprising, given the lack of variation in PER over that period (the effect of PER is registered with a lag so just 4 states adopted *PER* during the sample period 1994-2000).

Row [8] shows that limiting the sample to the 48 continental states has no substantive impact on the results.

Row [9]: Our model yields consistent estimates under the hypothesis that counties cannot choose to receive or reject treatment. Nevertheless, some counties might be able to create pressure to obtain desired regulation. If so, then it seems most likely that the county of the state capital is most likely to have a more significant weight in the decisions of the policy makers. The results in row [9] reveal, however, that our results are robust to the exclusion of the counties of the state capitals.

Row [10]: Another potential concern may be that the geographical pattern of PER reflects mortality trends that trigger *PER* adoption. Previously reported results show *PER* has a significant effect after controlling for time trends. However, *PER* could have been adopted in response to accelerations in the rate of change in mortality. Such a possibility would not be entirely captured by linear trends. In row [10] we see that the inclusion of quadratic time trends leaves the effect of PER on overall and disease mortality substantively unaffected. But now the (negative) coefficient on injury mortality becomes significant. Although this finding is consistent with our expectations, as discussed in the text, we acknowledge that the result could be driven by over-controlling for unmeasured factors, known sometimes to lead to unstable parameter estimates (Schneider, Klein, and Murphy, 1981).

[34] The estimates obtained on the 1997-2002 sample when we do not control for state specific trends are: 0.093 (0.033) significant at 1% significance level for total mortality; 0.104 (0.032) significant at 1% significance level for disease mortality; and -0.223 (0.191) for injury mortality. The estimates obtained on the entire sample when we do not control for state specific trends are: 0.093 (0.040) significant at 5% significance level for total mortality; 0.094 (0.039) significant at 1% significance level for disease mortality; and 0.051 (0.132) for injury mortality.

Row [11] restricts the sample to only those states that adopted PER during our sample. Not surprisingly, because this restriction eliminates the control states, it also eliminates much of the information available to estimate the effects of PER. 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.

The results thus far all 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 populations are very small, the data could indicate large changes in the mortality rate from one year to another. Such random changes in mortality rates from one year to another might be spuriously associated with the implementation of PER. 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.

Row [12] thus seeks to reduce the impact of noise in the data by excluding counties with very small populations, where there may be extremely high variance in mortality rates. Here we thus estimate the main regression specification, but restricted to those county-year observations involving 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.

Another way to reduce the effect of noise is to aggregate data at the state level. State level aggregation also offers an alternative way to account for the existence of common random effects at the state level. In the main specification we allowed for such random effects by computing standard errors corrected for clustering at the state level. Using state-level data also may have significant disadvantages, however. First, such data aggregates over significantly different populations. And second, the danger of reverse causality is higher at the state level. There is significant variance in mortality rates across counties in a state and any single county is unlikely to lead to statewide regulation (cf. the discussion above regarding row [7]); however, changes in state level mortality trends could influence state policy makers.

Row [13] presents results obtained on state-level data for the 1994-2006 period. Controls include state and year fixed effects, state-specific trends, and state-level, time-varying controls, such as: age, gender, and race composition, log wage, physicians, proportion of population without health insurance, education, and state health and hospital expenditures. The results we obtain are smaller and less precisely estimated coefficients than observed in other specifications. This finding is unsurprising, given how demanding the large number of fixed effects and state time trends are on the data.[35] Nevertheless, even these are consistent to those observed with much larger sample sizes, providing support for the idea that the timing of adoption is not determined by pre-period mortality rates.

Row [14]: Throughout the paper we 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 important problem than error correlation by state over time. Here we see that the results not only hold under clustering at county level; they are even more precisely estimated.

Rows [15] and [16] show that neither the second lag of PER nor the second lead of PER are good predictors of adoption. Even if not statistically significant, the coefficients on the second lag of PER are consistent with the notion that individuals and physicians are adapting to PER in ways that mitigate its adverse effects.

[35] There regressions are run on 650 observations, which must identify 109 coefficients.

Table A1: The Impact of *PER* Adoption on Mortality - Tests of Model Specification

	Mortality Rate	Disease Mortality Rate	Injury Mortality Rate
[1] Main	0.039*	0.045**	-0.236
	(0.020)	(0.022)	(0.168)
[2] Control for cost of medical care	0.034	0.046**	-0.346*
	(0.022)	(0.022)	(0.176)
[3] Control Medicaid enrollment	0.039*	0.045**	-0.234
	(0.020)	(0.022)	(0.168)
[4] Add other state level covariates	0.038*	0.043**	-0.252
	(0.020)	(0.021)	(0.176)
[5] 1996-2004	0.065***	0.073***	-0.273
	(0.021)	(0.024)	(0.192)
[6] 1997-2002	0.108***	0.119***	-0.299
	(0.038)	(0.039)	(0.204)
[7] 1994-2000	0.056	0.069	-0.219
	(0.057)	(0.057)	(0.196)
[8] 48 contiguous states	0.038*	0.044**	-0.233
	(0.020)	(0.022)	(0.164)
[9] Exclude county of state capital	0.046**	0.052**	-0.233
	(0.020)	(0.021)	(0.162)
[10] Quadratic time	0.034	0.045**	-0.302**
	(0.021)	(0.022)	(0.143)
[11] Only adopting states	0.021	0.030	-0.225
	(0.018)	(0.020)	(0.185)
[12] County pop>10,000	0.039*	0.045**	-0.237
	(0.020)	(0.022)	(0.173)
[13] State level	0.028	0.033	-0.119
	(0.021)	(0.021)	(0.117)
[14] Cluster by county	0.039**	0.045***	-0.236***
	(0.016)	(0.016)	(0.074)
[15] 2-Year lag of PER	-0.009	-0.023	0.314
	(0.025)	(0.027)	(0.215)
[16] 2-Year lead of PER	0.021	0.024	-0.036
	(0.028)	(0.027)	(0.146)

Notes: See text and notes to text tables for complete description of methods. Row [1] gives the coefficient estimate and standard errors from the primary specification. Due to data limitation on the availability of our measure of cost of health, specification estimates in row [2] on based on 1998-2006 data. For all other rows, see text of Supplemental Results Appendix. * significant at 10% level; ** significant at 5% level; *** significant at 1% level.

SECTION 2: Alternative mortality model specifications

Here we report on findings relating to alternative model specifications. The entries in Table A2 refer not to the estimated effects of PER; instead they refer to the estimated coefficient and standard errors of variables added to the main model.

Row [1] of Table A2 shows the effect of an examination of whether PER triggered changes in the time trends of mortality (as opposed to simply causing a shift in the intercept, which is the specification implicit in the main model). For each of the three mortality measures, we estimate the following equation and report the coefficient (and robust standard error) of the interaction term between *PER* and the time trend in row [1] of Table A2:

$$(1)\ Mortality\ rate_{ct}=\beta\ PER_{sz} + \mu\ PER_{sz} * t + \theta X_{ct} + \gamma_c + \lambda_t + \omega_s t + HHEXP_{st} + \varepsilon_{ct},$$

The results imply no evidence of a change in slope for disease mortality, likely because the mean shift (β) captures most of the change in mortality. The estimated value of (μ) is significant in the case of injury-related mortality but in results not shown here we find that this significance is not robust to the inclusion of quadratic time trends.

Row [2] tests for geographical heterogeneity of the effect of PER. The appearance of Figure 1 in the main body of the paper suggest that Southern states are more likely to adopt PER, but in fact, the estimated coefficients for the interaction term between *PER* and southern states[36] are not statistically significant for any of the mortality measures, rejecting the hypothesis of regional geographic heterogeneity of the *PER* effect. This result is also supports our identification strategy, for it is consistent with the idea that county fixed effects and state specific trends are able to account for all geographical heterogeneity that may be correlated at the same time with both *PER* adoption and mortality.

Row [3] reports a test for heterogeneity of the *PER* effect by timing of adoption. If selection is an issue we expect that states that benefit most from such regulation would be the first adopters. The first states to adopt the *PER* are Maryland, North Carolina, Ohio, and Texas in 1999[37], while the first large wave of adoptions took place in year 2000[38]. A dummy equal to 1 if

[36] Southern states: Alabama, Arkansas, Delaware, Florida, Georgia, Kentucky, Louisiana, Maryland, Mississippi, North Carolina, Oklahoma, South Carolina, Tennessee, Texas, Virginia, and West Virginia.

[37] The first jurisdiction to adopt the PER is the District of Columbia in 1998. DC is not in the sample used in our analysis, because data for state health and hospital expenditures are not available for DC. The results are robust to a sample including DC and excluding the controls for state health and hospital expenditures. Estimates obtained from this alternative specification are: 0.039 (0.021) significant at 10% significance level for mortality; 0.045 (0.022) significant at 5% for disease mortality; -0.225 (0.161) for injury mortality.

the state adopted the *PER* before 2000, and zero otherwise, is interacted with the *PER* variable and the coefficient on this variable is reported here.[39] For all three mortality measures the estimated coefficient is not statistically significant, providing support for the idea that the early-adopting states are in fact similar to the later-adopting ones, and thus that the timing of *PER* adoption is exogenous.

Table A2: Additional Specifications
Estimated coefficients and standard error for interaction terms

	Mortality Rate	Disease Mortality Rate	Injury Mortality Rate
[1] PER • Year	0.001	-0.006	0.087**
	(0.009)	(0.009)	(0.042)
[2] PER • South	-0.039	-0.041	0.244
	(0.043)	(0.047)	(0.230)
[3] PER • Early adopter	0.007	-0.004	0.276
	(0.059)	(0.060)	(0.327)
[4] PER • Late adopter	0.054	0.069	0.246
	(0.071)	(0.079)	(0.259)
[5] PER • Black	-0.010***	-0.009***	-0.019***
	(0.002)	(0.002)	(0.007)

Notes: See text and notes to text tables for complete description of methods. Row [1] reports the coefficient on the interaction term between the *PER* variable and a time trend. Row [2] reports the coefficient on the interaction term between the *PER* variable and a regional dummy equaling 1 for the South and 0 elsewhere. Row [3] 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 [4] 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).
* significant at 10% level; ** significant at 5% level; *** significant at 1% level.

Row [4] of Table A2 reports the estimated coefficient on an interaction term of *PER* and a dummy equal to 1 if the state adopted *PER* on or after year 2003.[40] Here we see that the estimated effect of PER among late adopters is not significantly different from the effect among early adopting states.

[38] Ten states adopted PER in 2000.
[39] Using other cut-offs, such as before 2001, delivers similar results.
[40] Using other cut-offs such as after 2002 or after 2004 delivers similar results.

Row [5] of Table A2 reports tests of heterogeneity of the effect by race. We find that PER has a lower effect on disease mortality for blacks, but a larger effect for injury mortality for blacks. The former may be explained by differential adoption of technology by race.

SECTION 3: Robustness checks of morbidity specification.

Endogeneity is even less of a concern for the analysis of the effect of *PER* on the number of days lost to illness, because morbidity is measured at the individual level. Nevertheless, results from a series of robustness checks of the morbidity results are reported in Table A3.

Table A3: The Impact of *PER* Adoption on the Number of Days Lost to Illness - Robustness Check

	Days Lost to Illness
[1] Main	0.262**
	(0.120)
[2] OLS (Log dependent variable)	0.049**
	(0.018)
[3] Added controls: education, health insurance, and marital status	0.264**
	(0.105)
[4] Only adopting states	0.125***
	(0.040)
[5] 48 contiguous states	0.273**
	(0.121)

Notes: Using individual-level data, the dependent variable is the number of days lost to illness in the past 30 days. Rows [1] and [3]-[5] these estimates are marginal effects after negative binomial models that control for state and year fixed-effects, and state specific time trends and for gender, race, age, income, state level physicians per capita, and state health and hospital expenditures. Row [2] uses the same controls, but using OLS. Robust standard errors clustered at state level are reported in parentheses. * significant at 10% level; ** significant at 5% level; *** significant at 1% level.

Row [1] shows the main result on morbidity, discussed in the txt.

Row [2] shows the effects of estimating an OLS with log dependent variable rather than negative binomial model. We see that the results are robust to this change in specification.

Row [3] reveals that the results are robust to the addition of controls such as education, health insurance, and marital status.

Row [4] of Table A3 reveals that only data from adopting states produces a smaller estimated effect of PER, but one that is statistically significant despite the exclusion of the control states from the sample.

Row [5] of Table A3 shows that the results are substantially the same when we exclude the non-continental states of Alaska and Hawaii.

SECTION 4: Alternative morbidity model specifications

Some alternative specifications are presented in Table A4. As revealed by the statistically insignificant interaction terms in Rows [1]-[3], we find no evidence of a change in trend mortality due to PER, and no difference of its effects for either early or late adopters.

Rows [4]-[6] explore a more in depth analysis of the effect of PER on various demographic groups, made possible by the fact that the morbidity data are collected at the individual level. Row (4) shows that there is no significant difference between the outcomes by gender. In row [5] we see that the impact of PER on blacks is much smaller than on whites. Indeed, the net effect of PER on black morbidity is negligible. One explanation for this finding is that there are racial differences in use of technology.[41] The result is also consistent with some previous studies indicating blacks are less likely to access health related electronic resources.[42]

Although BRFSS does not have detailed information on income, we can differentiate among board income brackets. Row [6] suggests that the adverse impact of PER diminishes slightly as income rises, although these effects too imprecisely estimated to place much reliance upon.

[41] 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.

[42] 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)

Table A4: Additional specification tests
Estimates of interaction terms

	Days Lost to Illness
[1] PER • Year	0.031
	(0.040)
[2] PER • Early adopter	0.557
	(0.535)
[3] PER • Late adopter	-0.254
	(0.173)
[4] By gender	
PER	0.254**
	(0.122)
PER • Female	0.015
	(0.033)
[5] By race	
PER	0.295**
	(0.121)
PER • Black	-0.246***
	(0.053)
[6] By Income	
PER	0.326***
	(0.115)
PER • Inc 25k-50k	0.023
	(0.039)
PER • Inc 50k-75k	-0.100
	(0.072)
PER • Inc >75k	-0.071
	(0.119)

Notes: Using individual-level data, the dependent variable is the number of days lost to illness in the past 30 days. These estimates are marginal effects after negative binomial models that control for state and year fixed-effects, and state specific time trends and for gender, race, age, income, state level physicians per capita, and state health and hospital expenditures. Early adopters are states that adopted PER before 2000 (excluding 2000). Late adopters are states that adopted PER after 2002 (excluding 2002). Robust standard errors clustered at state level are reported in parentheses. * significant at 10% level; ** significant at 5% level; *** significant at 1% level.

SECTION III: Propensity Score

We matched counties in the period before any PER adoptions and used the sample of matched counties to estimate the effect of PER. We attempted several matching procedures. First we performed nearest neighbor matching with replacement using a caliper of 0.0001 (Leuven and Sianesi 2003). We checked the robustness of the results using nearest neighbor matching without replacement. For this case we used a larger caliper of 0.001 to be able to retain a reasonable-sized sample. In both cases we imposed common support condition by dropping treatment observations whose propensity score is higher than the maximum or less than the minimum propensity score of the controls. The results from these two procedures are reported in Table A5. In both situations the matching was performed based on a propensity score calculated using a logit model with the following variables: oxycodone consumption per capita, hydrocodone consumption per capita, percent living in poverty, crime, high-school education, and percent black. Specifically we predict the adoption of PER by 2006 using the level of the above-mentioned county characteristics in 1997. To achieve balancing of covariates we added several interaction terms (Dehejia and Wahba, 2002).

The logit regression used to predict propensity score is reported in Table A6. The covariates are statistically significant in most cases, but this is because we have followed the convention of the propensity matching literature in reporting regular standard errors. In results not reported here, we have re-estimated the equations in Table A6 with clustering by state; only hydrocodone remains as a significant predictor of PER. To further examine the possibility that hydrocodone usage might have influenced PER adoption we also perform matching based on hydrocodone only. Specifically, we stratified counties based on their level of consumption of hydrocodone in 1997. We imposed a common support condition by dropping treatment observations whose propensity score was higher than the maximum or less than the minimum propensity score of the controls. We obtained 12 strata. We run the original regression on the sample of counties in common support with strata fixed effects. These results are reported in Table A5 last row. It is evident that they are substantively identical to our main results.

Table A5. The effect of PER on mortality - balanced sample 1999-2006			
	Mortality Rate	Disease Mortality Rate	Injury Mortality Rate
[1] Main Specification	0.041*	0.053**	-0.270*
	(0.022)	(0.021)	(0.155)
[2] Nearest neighbor matching with replacement	0.025	0.069**	-0.451***
	(0.032)	(0.033)	(0.139)
[3] Nearest neighbor matching without replacement	0.047	0.077*	-0.377
	(0.037)	(0.042)	(0.243)
[4] Stratification	0.039*	0.052**	-0.345*
	(0.022)	(0.021)	(0.203)

For comparability, the first row reports the estimates obtained from the main specification on the smaller sample used for matching. The sample used in matching is smaller due to data constraints on variables used to predict PER adoption. * significant at 10% level; ** significant at 5% level; *** significant at 1% level.

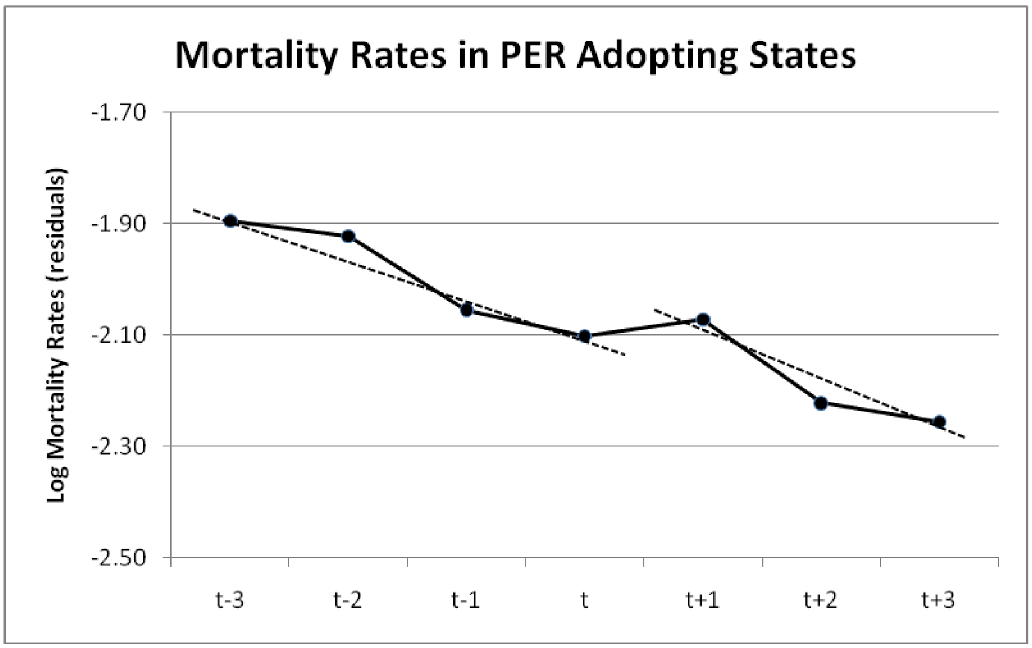
Table A6. Propensity Score Calculations		
	PS - matching with replacement	PS - matching without replacement
Oxycodone	$0.105 \bullet 10^{-2}$ ***	$0.099 \bullet 10^{-2}$***
	(0.010×10^{-2})	($0.009 \bullet 10^{-2}$)
Hydrocodone	$0.113 \bullet 10^{-2}$***	$0.115 \bullet 10^{-2}$
	($0.006 \bullet 10^{-2}$)	($0.006 \bullet 10^{-2}$)
Crime	-0.028***	0.001***
	(0.004)	(0.000)
Education	-0.363***	-0.219***
	(0.031)	(0.019)
Poverty	-0.013	-0.010
	(0.037)	(0.011)
Black	0.012**	-0.473***
	(0.006)	(0.135)
Oxycodone x Crime	$-0.549 \bullet 10^{-6}$***	$-0.275 \bullet 10^{-6}$***
	($0.093 \bullet 10^{-6}$)	($0.075 \bullet 10^{-6}$)
Education x Crime	$0.036 \bullet 10^{-2}$***	
	($0.005 \bullet 10^{-2}$)	
Education x Poverty^2	$0.182 \bullet 10^{-6}$	
	($0.125 \bullet 10^{-4}$)	
Education x Black		0.006***
		(0.002)

Notes: The dependent variables are dummies equal to 1 of a county adopted PER by 2006 and zero otherwise. * significant at 10% level; ** significant at 5% level; *** significant at 1% level.

SECTION IV: Trends in Mortality Rate

A graph of the trend in mortality in PER-adopting states in the years preceding and following PER is presented below.

The solid line shows the behavior of mortality over the full period shown. The two dashed lines show the estimated trends for the two sub-periods, up to and after PER. The average rate of decline is the same in both sub-periods, but the intercept is higher for the post-PER period, implying that PER raised mortality rates. The graph has significant caveats. It is difficult to aggregate across time periods in a meaningful manner. We attempted controlling for time fixed effects, but it is not at all obvious that time FE can fully address this issue. In the paper we included some versions of this picture that retain only states that adopted PER in 2000 or 2001 as compared, which eliminates the issue of aggregation across time periods.



Notes: Period t represents the year of adoption of PER. The solid line shows the trend in mortality rates (log). Because different states adopted PER at different points in time we retain only the residual variation in mortality rates after removing the effect of time. The dashed lines represent the fitted lines for the periods t-3 to t, and t+1 to t+3.

Additional References (not cited in main body of the paper):

E. Leuven and B. Sianesi. (2003). "PSMATCH2: Stata module to perform full Mahalanobis and propensity score matching, common support graphing, and covariate imbalance testing". http://ideas.repec.org/c/boc/bocode/s432001.html. This version: version 3.1.5 2may2009 E. Leuven, B. Sianesi