Supplemental Results Appendix – NOT FOR PUBLICATION

Section I The Estimated Effect of Mortality Using All Data

Matching has the advantage of controlling for the factors that led to the adoption of regulation and allows us to obtain unbiased estimates of the PER's effect. The disadvantage is that the resulting sample is small, and thus some tests may not be very powerful. The resulting matching estimates are larger than the estimates obtained on the entire sample; one standard error bands of the matching estimate exclude the OLS estimate. They are not statistically significantly different at conventional significance levels, however. This appendix reports the results obtained on the entire sample.

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 coefficients obtained on the entire sample of 3,125 counties using main specification discussed in the text: a regression that controls for county gender, age, and race composition; log wages; physicians; poverty rate; unemployment rate; education level; Medicaid enrollment; state health and hospital expenditures; county and year fixed effects; and quadratic state specific time trends.

Rows [2] – [5] specifications test the sensitivity of the results to the choice of sample period. The results are robust to either the exclusion of the earliest 4 years of sample or to the latest 4 years of sample. They are also 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 on disease mortality 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 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.[1] 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

[1] The estimates obtained on the 1997-2002 sample when we do not control for state specific trends are: 0.088 (0.030) significant at 1% significance level for total mortality; 0.098 (0.030) significant at 1% significance level for disease mortality. The estimates obtained on the entire 1994-2006 sample when we do not control for state specific trends are: 0.100 (0.041) significant at 5% significance level for total mortality; 0.099 (0.041) significant at 1% significance level for disease mortality.

of death and differential advances in medical knowledge about various diseases generating differential changes in trends.

To assess whether reverse causality is an issue, we test whether the identified trend in utilization rates happened before the PER's adoption and thus cannot be attributed to this regulation. For this purpose, we add to the main specification a variable defined as 1 if the PER was effective in the following year. Coefficients for the one-year lead PER variable are not statistically significant, as shown in row [6] of Table A1. We also find that the two-year lead of PER is not significantly correlated with the current level of utilization rates. Overall, these results suggest that the observed trend did not begin in the years prior to PER adoption, and thus causality runs from PER to mortality rates and not the other way around.

There may be concerns that 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 [8] reveal, however, that our results are robust to the exclusion of the counties of the state capitals.

Row [9] 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.

Because the independent variable of interest is measured at state level while the dependent variable is measured a county level, 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 hold under clustering at county level.

One potential concern is that the results 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 [11] 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 [8]); however, changes in state level mortality trends could influence state policy makers.

Row [12] presents results obtained on state-level data for the 1994-2006 period. Controls include state and year fixed effects, quadratic state-specific trends, and state-level, time-varying controls, such as: age, gender, and race composition, log wage, physicians, education, state unemployment rate, state poverty rate, Medicaid enrollment, and state health and hospital expenditures. The results we obtain are smaller and less precisely estimated 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.[2] Nevertheless, even these are consistent to those observed with much larger sample sizes.

[2] There regressions are run on 650 observations, which must identify over 170 coefficients.

Table A1. The impact of PER on mortality - all sample (not for publication)

	Mortality rate	Disease mortality rate	Injury mortality rate
[1] Main	0.035	0.045*	-0.309*
	(0.020)	(0.021)	(0.146)
[2] 1998-2006	0.077**	0.087**	-0.275
	(0.028)	(0.030)	(0.201)
[3] 1994-2002	0.078	0.089*	-0.391
	(0.039)	(0.040)	(0.226)
[4] 1996-2004	0.068*	0.076**	-0.201
	(0.026)	(0.027)	(0.166)
[5] 1997-2002	0.092*	0.102*	-0.279
	(0.045)	(0.045)	(0.215)
[6] 1-year Lead of PER	-0.045	-0.039	-0.191
	(0.034)	(0.036)	(0.117)
[7] 2-year Lead of PER	-0.000	-0.002	0.088
	(0.023)	(0.023)	(0.138)
[8] Drop county of state capital	0.039	0.050*	-0.313*
	(0.021)	(0.022)	(0.144)
[9] Only adopting states	0.026	0.036	-0.270
	(0.021)	(0.022)	(0.152)
[10] Cluster by county	0.035*	0.045**	-0.309**
	(0.016)	(0.016)	(0.080)
[11] County Pop>10000	0.034	0.045*	-0.312*
	(0.020)	(0.021)	(0.150)
[12] State level	0.029	0.033	-0.303
	(0.032)	(0.034)	(0.264)

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. Models in rows 1 to 11 include county and year fixed-effects, and quadratic state specific time trends. Other controls are county gender, age, and race composition, log wages, physicians, poverty rate, unemployment rate, education level, Medicaid enrollment, and state health and hospital expenditures. Regressions in rows 1 to 11 are weighted, with county populations as the weights. Row [12] reports coefficients from a model using state level data. This model includes state and year fixed effects and quadratic state specific time trends. It also controls for the same covariates as county level specifications. This regression is weighted by state population.
Robust standard errors clustered at state level are reported in parentheses.
* significant at 5% level; ** significant at 1% level.

In addition 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 interaction variables added to the main model.

Row [1] 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[3], while the first large wave of adoptions took place in year 2000[4]. 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 here.[5] 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.

Row [2] 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.[6] Here we see that the estimated effect of PER among late adopters is not significantly different from the effect among earlier-adopting states.

Row [3] tests the heterogeneity of the PER effect by rurality. The interaction term between percent rural and *PER* is positive and significant, an indication that the PER leads to a larger increase in disease mortality in predominantly rural areas than in urban areas.

Row [4] tests the heterogeneity of the PER effect by physician density. Consistent with previous results we find that the PER has a smaller impact on disease mortality in areas with higher physician density.

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.

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[3] 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: 0.035 (0.021) significant at 10% significance level for mortality; 0.046 (0.022) significant at 5% for disease mortality; -0.256 (0.116) significant at 5% for injury mortality.

[4] Ten states adopted PER in 2000.

[5] Using other cut-offs, such as before 2001, delivers similar results.

[6] Using other cut-offs such as after 2002 or after 2004 delivers similar results.

Row [6] tests for geographical heterogeneity of the effect of PER. The appearance of Figure 2 in the main body of the paper suggests that Southern states are more likely to adopt PER, but in fact, the estimated coefficients for the interaction term between PER and southern states[7] are not statistically significant for any of the mortality measures, rejecting the hypothesis of regional geographic heterogeneity of the *PER* effect. This result also supports our identification strategy, for it is consistent with the idea that county fixed effects and quadratic 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.

Table A2. Tests of heterogeneity of PER impact on mortality- all sample (not for publication)

	Mortality rate	Disease mortality rate	Injury mortality rate
[1] PER*Early adopter	0.001	-0.009	-0.116
	(0.050)	(0.051)	(0.257)
[2] PER*Late adopter	0.033	0.037	0.401
	(0.075)	(0.081)	(0.386)
[3] PER*Rural	0.003*	0.003*	0.002
	(0.001)	(0.001)	(0.003)
[4] PER*Physicians	-0.056**	-0.055**	-0.041
	(0.013)	(0.013)	(0.031)
[5] PER*Black	-0.010**	-0.009**	-0.019*
	(0.002)	(0.002)	(0.007)
[6] PER*South	0.005	0.004	0.143
	(0.037)	(0.039)	(0.229)

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. All regressions include county and year fixed-effects, and quadratic state specific time trends. Other controls are county gender, age, and race composition, log wages, physicians, poverty rate, unemployment rate, education level, Medicaid enrollment, 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% level; ** significant at 1% level.

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

Section II The Estimated Effect of PER on Morbidity – Robustness Checks

In addition, we present several alternative specifications for the model estimating the impact of PER on morbidity, as measured by number of days lost to illness, using the full sample of data.

Row [1] of Table A3 shows the result using the main specification, which was discussed in the text, using the full sample of data.

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] 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 [4] indicates that there is no significant difference between early adopters and the rest of the states. There is also no significant difference between late adopters and the rest of the states, as shown in row [5].

Rows [6]-[8] 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 [6] shows that there is no significant difference between the outcomes by gender. In row [7] 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.[8] The result is also consistent with some previous studies indicating blacks are less likely to access health related electronic resources.[9]

Although BRFSS does not have detailed information on income, we can differentiate among broad income brackets. Row [8] suggests that the adverse impact of PER diminishes slightly as income rises, although these effects too imprecisely estimated to place much reliance upon.

[8] 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.
[9] 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 A3: The impact of PER adoption on the number of days lost to illness - Robustness check using all sample (not for publication)

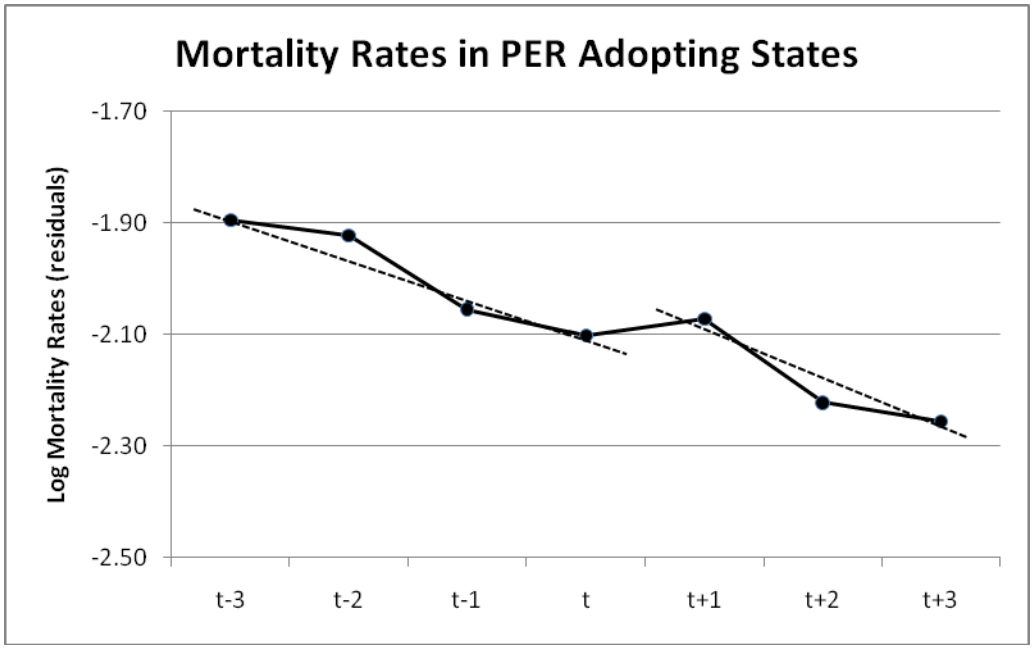
	Panel A: Alternative specifications
[1] Main	0.229*
	(0.101)
[2] OLS (Log dependent variable)	0.054**
	(0.018)
[3] Only adopting states	0.103**
	(0.037)
	Panel B: Interaction terms
[4] PER*Early adopter	0.378
	(0.295)
[5] PER* Late adopter	-0.230
	(0.155)
[6] PER*Female	0.015
	(0.030)
[7] PER*Black	-0.220**
	(0.051)
[8] PER* Inc 25k-50k	-0.053
	(0.047)
PER*Inc 50k-75k	-0.133
	(0.084)
PER*Inc>75k	-0.117
	(0.127)

Notes: Using individual-level data, the dependent variable is the number of days lost to illness in the past 30 days. The estimates reported in rows [1] and [3] - [8] 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, education, state level physicians per capita, state poverty rate, unemployment rate, Medicaid enrollment, and health and hospital expenditures. Row [2] controls for the same variables, but using OLS. Robust standard errors clustered at state level are reported in parentheses.
* significant at 5% level; ** significant at 1% level.

Section III: 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.

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