Laws Requiring Health Plans to Provide Direct Access to Obstetricians and Gynecologists, and Use of Cancer Screening by Women

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Background. Many states have passed legislation mandating that health plans provide direct access to obstetricians/gynecologists (hereinafter “ob/gyns”) for women, limiting the ability of plans to require referrals or otherwise restrict access. One benefit of these laws may be improved preventive screening rates, but no literature has examined the relationship between ob/gyn direct access laws and use of breast cancer and cervical cancer screening.

Data and Methods. We use repeated cross-sections of privately insured women age 18–64 (Pap test) and 40–64 (mammography) from the Behavioral Risk Factor Surveillance System for 1996–2000, linked to data on the presence of ob/gyn direct access laws by state. Outcome measures are receipt of mammography and receipt of a Pap test within the past 2 years. Regression analyses are used to assess the relationship between the presence of ob/gyn direct access laws and screening, adjusting for a range of individual characteristics, fixed state characteristics, and time trends.

Results. We find no statistically significant relationships between the presence of an ob/gyn direct access law and receipt of either mammography or Pap test screening. We explore a range of alternate specifications and find none that yield clear evidence of a relationship.

Conclusions. Laws requiring direct access to ob/gyns are not associated with large or consistent measurable impacts on use of cancer screening.

Key Words. Cancer screening, managed care, state regulation

Reacting to patient and provider dissatisfaction with the restrictive practices of managed care plans during the 1990s, many states adopted managed care patient protection regulations designed to restrain these health plan activities (Blendon et al. 1998; Noble and Brennan 1999; Sloan and Hall 2002). Limited access to specialists was a common concern, leading many states to enact “direct access” laws, which limit the ability of health plans to require referrals

before the plan will cover specialist care. One common form of direct access law provides for women to obtain direct access to care delivered by obstetricians and gynecologists (hereinafter "ob/gyns").

Maryland was the first state to pass an ob/gyn direct access law in 1994 (Henry J. Kaiser Family Foundation 2000). By 2001, 42 states plus the District of Columbia had passed some form of ob/gyn direct access law, all except Alaska, Arizona, Hawaii, Iowa, North Dakota, Oklahoma, South Dakota, and Wyoming (Stauffer and Morgan 2000; Cauchi 2003). All of these laws have in common a requirement that some health plans provide women with direct access to ob/gyns for at least some services, though the specific provisions can vary from state to state. For example, some states require plans to treat ob/gyns as specialists to which women may go without a referral from their primary care provider, while others require plans to allow women to designate their ob/gyn to be their primary care provider, and some states require both (Henry J. Kaiser Family Foundation 2000; Stauffer and Morgan 2000).

Although often broadly written, these laws would tend to affect care for some women more strongly than others. The laws do not apply to women covered by public-sector health plans such as FEHBP, Medicaid, and Medicare (direct access to ob/gyns for enrollees in these programs was provided for under a 1998 executive order). The Employee Retirement Income Security Act of 1974 exempts plans offered by firms that self-insure. Although the laws vary in whether or not they explicitly apply only to HMOs or are written to cover a broader range of health plans, HMOs seem more likely than other types of plans to have attempted to restrict access to ob/gyns. Thus, these laws might be expected to have their strongest effects on women with private HMO coverage, particularly those in non-self-insured HMOs.

Little is known about the impacts of these laws on health care. Some literature has debated conceptual issues around patient protection laws (e.g., Miller 1997; Sloan and Hall 2002), and has pointed out the need for more information about managed care regulations in general and ob/gyn direct access laws in particular (e.g., Hellinger 1996; Henderson et al. 2002). One study included ob/gyn direct access laws among many others in analyses of managed care patient protection laws, reporting limited effects of patient

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protection laws as a group on generalized measures of utilization and patient satisfaction (Sloan et al. 2005), but beyond this, we are aware of no empirical evidence on the impacts of ob/gyn direct access laws on care utilization.

Despite the lack of data, proponents view these laws as a beneficial step for women's health care (e.g., Henderson et al. 2002), often arguing that they will facilitate the delivery of high quality care for women. Among the potential benefits cited are improvements in screening for breast and cervical cancer. A variety of reports suggest that women who see an ob/gyn are noticeably more likely to get recommended screening exams (e.g., Horton et al. 1994; Weisman et al. 1995; Finison et al. 1999; Weisman and Henderson 2001; Henderson et al. 2002; Haggstrom et al. 2004). While it is not clear that these reports effectively distinguish causal effects of seeing an ob/gyn from variations in screening due to an increased propensity of women interested in screening to see ob/gyns, they have frequently been interpreted as implying a causal effect and thus as suggesting that, if direct access laws were successful in increasing utilization of ob/gyns, there would be increases in screening rates.

This analysis empirically investigates the relationship between the passage of ob/gyn direct access laws and the receipt of screening for breast and cervical cancer. We use the fact that different states adopted direct access laws at different times to identify the effects of the laws, comparing changes in health care utilization before and after passage, using contemporaneous changes in nonadopting states to control for time trends.

In principle, one might like to begin an investigation of these laws with an evaluation of direct evidence on whether they influence visits with ob/gyns or other measures of the most proximate potential impacts of the laws. We lack data on these kinds of measures from sources with enough cross-sectional and time series breadth to permit analyses of the type we undertake here. We thus focus on receipt of screening exams which will provide evidence about one important aspect of health care delivery and may also provide indirect evidence about the overall effects of the laws on ob/gyn utilization.

DATA AND METHODS

BRFSS Data

We use individual-level data from the Behavioral Risk Factor Surveillance System (BRFSS) for the years 1996–2000. The BRFSS is the primary tool of the Centers for Disease Control and Prevention (CDC) for assessing changes in health behaviors and risk factors in the U.S. population. It is primarily a

telephone survey, supported by the CDC and administered by states using a standardized core questionnaire and survey procedures. The data can be used to produce nationally representative estimates of the prevalence of a number of health behaviors, including rates of receipt of Papanicolaou (Pap) tests and mammography screening (Centers for Disease Control and Prevention 1996–2000). In the time period we study, the median response rate across states varied from a low of 48 percent in 2000 to a high of 63 percent in 1996. Evaluation of the reliability and validity of the BRFSS data, including studies of cancer screening and many of the related demographic variables we use here (e.g., Bowlin et al. 1993; Stein et al. 1993; Stein et al. 1996; Martin et al. 2000; Nelson et al. 2001; Andreson et al. 2003; Caplan et al. 2003), are generally supportive of its validity.

We extracted repeated cross-sections of women living in the 50 United States and the District of Columbia, ages 18–64 for analysis of Pap test receipt and ages 40–64 for analysis of mammogram receipt. These services are generally recommended for women in these age groups (U.S. Preventive Services Task Force 1996, 2002, 2003).

We restricted the sample to women with private insurance, as the laws do not apply to women with public coverage. We identified women with private coverage as those who indicated having health care coverage either through their employer, someone else's employer, or a plan that they or someone else bought on their own. The BRFSS data do not provide sufficient detail to specifically identify private HMO enrollees in all of the years we study.

For each individual, we constructed two measures of screening receipt for analysis. Respondents were asked if they had ever had a mammogram and, if so, how long it had been since their last mammogram. We used these data to construct an indicator for having had a mammogram within the past 2 years. Similarly, respondents were asked whether they had ever had a Pap smear and, if so, how long it had been since their last Pap smear. We used responses to these questions to construct an indicator for having received a Pap smear in the past 2 years. (While guidelines generally call for the receipt of mammography at least every two years, evidence seems to support Pap test screening at least every 3 years. If we repeat our analysis of Pap test receipt using a 3-year time window we obtain very similar results.)

We also constructed a number of other variables to use as controls in the analysis. These include measures of age (18–29, 30–34, 35–39, 40–44, 45–49, 50–54, 55–59, and 60–64), race (white non-Hispanic, white Hispanic, black, and other), marital status (married or member of an unmarried couple;

divorced, widowed, or separated; and never married), health status (excellent, very good, good, fair, poor), education (< 12 years, 12 years, 13–15 years, 16 years or greater), and income (< \$10,000, \$10,000–19,999, \$20,000–34,999, \$35,000–49,999, \$50,000–74,999, and \$75,000 or greater). We excluded women with missing data for any of the analysis variables.

Direct Access Law Data

For each woman, we linked data on whether she resided in a state that had an ob/gyn direct access law in place in the year in which she was surveyed. We developed our data on the passage of laws from a number of secondary source compilations of information about laws, examining original state statutes and regulations to resolve discrepancies between sources (Johnson et al. 1996; Families USA Foundation 1998; Laudicina and Pardo 1999; Stauffer and Morgan 2000; Moore 2001; Cauchi 2003; NETSCAN iPublishing Inc. 2004; National Conference of State Legislatures 2005). Where applicable, we used the effective dates rather than enactment dates of the policies. In cases where laws appeared to be amended over time, we identified the earliest point at which the law appeared to be in effect, even if it was strengthened or weakened later. We are not aware of any states that had passed a mandate but then repealed it entirely during the study period.

In some analyses, we attempted to stratify laws based on the strength of their provisions. This is inherently a subjective task. For analysis here, we placed laws into groups based on their strength and comprehensiveness according to data provided by the American College of Obstetricians and Gynecologists (Moore 2001). We define as “strong” those laws that (a) explicitly allow for self-referral to an ob/gyn, or allow the ob/gyn to be designated as the primary care physician, or both, and (b) explicitly cover all ob/gyn or acute ob/gyn procedures. Those that do not meet these criteria, typically because they circumscribe the set of services covered by the law but sometimes because they do not provide a concrete means of obtaining direct access, are designated “weak” laws.

We classify BRFSS respondents from a given state and year as being subject to a mandate if their state had a mandate in place before the beginning of the year in which their survey took place. Since we are frequently unable to tell exactly when within a year a given mandate went into force, we exclude observations from a given state in the year in which the state implemented the mandate. For example, for a state that adopted a mandate in 1998, we would include data on women from that state surveyed in 1996 and 1997, who clearly

were not affected by the mandate, and women surveyed in 1999 and 2000, who were affected, but we exclude those surveyed in 1998, some of whom would have been subject to the mandate and some of whom would not.

HMO Market Share and Self-Insurance Data

We placed respondents into groups based on HMO market share in their state. Data used for this classification were computed based on Interstudy data (Baker 2001; Baker, Phillips et al. 2004). Based on the average 1996–2000 market share, we identified the bottom tercile (0–13.4 percent), middle tercile (13.5–24.9 percent), and top tercile (25.0–46.3 percent) of states, and constructed a variable indicating the tercile of each respondent's state of residence.

We also classified respondents based on state-level rates of enrollment in non-self-insured HMOs. We obtained from AHRQ estimates of the share of private sector employees with health insurance who were enrolled in a non-self-insured HMO over the period 1996–2000, by state by year by firm size, based on Medical Expenditure Panel Survey—Institutional Component (MEPS-IC) data. We assumed that all HMO enrollees in firms with fewer than 50 employees were not self-insured, and computed estimates of the average 1996–2000 share of private sector employees in non-self-insured HMOs by state. We then identified the bottom tercile (3.4–15.5 percent), middle tercile (15.6–25.2 percent), and top tercile (25.3–44.4 percent) of states based on this information, and constructed a variable indicating the tercile of each respondent's state of residence.

Methods

We investigate the relationship between passage of mandates and utilization using individual-level regressions of the form

$$Y_{i,s,t} = b_0 + b_1 \text{DIRECT ACCESS}_{s,t} + b_2 X_{i,s,t} + b_3 \text{STATE}_s + b_4 \text{YEAR}_t + e_{i,s,t} \quad (1)$$

where $Y_{i,s,t}$ is a dummy variable indicating receipt of a screening test by individual i in state s at time t , *DIRECT ACCESS* is a dummy variable indicating the presence of an ob/gyn direct access law, X is a vector of control variables related to preventive care utilization, *STATE* is a vector of state dummy variables to control for fixed characteristics of states and their populations not captured by X , *YEAR* is a set of dummy variables for year to capture trends over time, and e is an error term.

The state dummy variables will capture all characteristics of states that remain fixed over time. The year dummies will capture time trends in utilization rates common to all states. With these variables included, the regression results can be interpreted as capturing the change from the time before a direct access law was passed to the time after, implicitly using other states not passing laws in the same year to control for contemporaneous time trends.

As our dependent variables are dichotomous, we use logistic regression to estimate equation (1). We weighted the regression to account for the sampling design, nonresponse, and poststratification using weights provided with the BRFSS data. We computed standard errors accounting for the stratified sampling design used in the BRFSS.

RESULTS

Descriptive Statistics and Cross-Sectional Comparisons

Our analysis samples contain 100,140 women aged 40–64 for analysis of mammography receipt and 189,840 women aged 18–64 for analysis of Pap test receipt. Table 1 shows the demographic and related characteristics of our samples. Nearly 77 percent reported having had a mammogram in the last 2 years, and nearly 86 percent reported having a Pap test in the last 2 years.

Six states had passed mandates by the end of 1995. Nine states passed mandates during 1996, followed by 12 states in 1997, seven in 1998, six in 1999, and two in 2000. Nine states had not passed mandates by the end of 2000. Table 2 shows the share of women in our samples covered by mandates by year. In both cases, the share covered by mandates rises from about 11 percent in 1996 to about 94 percent in 2000. Table 2 also reports screening receipt rates by survey year. The share of women reporting receiving a mammogram within the past 2 years rises by about 6 percentage points between 1996 and 2000. There is also a small upward trend in reported Pap test rates.

Main Logistic Regression Models

The fact that the prevalence of mandates trends up at the same time that screening rates trend up might suggest a relationship between the two. Other simple comparisons might also suggest a relationship. For example, in an unadjusted cross-sectional comparison, the share of women reporting receiving a mammogram within the past 2 years is 1.6 percentage points higher among women living in states subject to mandates than among women living in states with no mandates (77.5 versus 75.9 percent, $p < .01$), and the reported

Table 1: Sample Characteristics

<i>Characteristic</i>	<i>Mammography Sample (Women Age 40–64)</i>		<i>Pap Test Sample (Women Age 18–64)</i>	
	<i>Mean</i>	<i>Standard Deviation</i>	<i>Mean</i>	<i>Standard Deviation</i>
<i>N</i>	100,140		189,840	
Mammogram in last 2 years	0.769	0.422	—	—
Pap test in last 2 years	—	—	0.858	0.349
Age				
18–29	—	—	0.218	0.413
30–34	—	—	0.131	0.338
35–39	—	—	0.145	0.352
40–44	0.278	0.448	0.141	0.348
45–49	0.249	0.432	0.126	0.332
50–54	0.205	0.404	0.104	0.305
55–59	0.155	0.362	0.078	0.269
60–64	0.113	0.316	0.057	0.232
Race				
White, non-Hispanic	0.815	0.388	0.785	0.411
White, Hispanic	0.043	0.202	0.057	0.232
Black	0.092	0.290	0.097	0.295
Other	0.050	0.218	0.061	0.240
Health status				
Excellent	0.260	0.439	0.279	0.449
Very good	0.361	0.480	0.386	0.487
Good	0.276	0.447	0.257	0.437
Fair	0.083	0.276	0.065	0.246
Poor	0.020	0.138	0.013	0.113
Marital status				
Married/member of couple	0.740	0.438	0.695	0.460
Divorced/widowed/separated	0.211	0.408	0.151	0.358
Never married	0.049	0.216	0.154	0.361
Highest education level (years)				
< 12	0.060	0.237	0.054	0.225
12	0.327	0.469	0.299	0.458
13–15	0.293	0.455	0.313	0.464
16 or more	0.320	0.467	0.334	0.472
Income				
< \$10,000	0.011	0.105	0.019	0.136
\$10,000–19,999	0.071	0.257	0.078	0.268
\$20,000–34,999	0.243	0.429	0.263	0.440
\$35,000–49,999	0.234	0.423	0.237	0.425
\$50,000–74,999	0.226	0.419	0.218	0.413
\$75,000 or more	0.214	0.410	0.186	0.389
BRFSS year				
1996	0.185	0.388	0.189	0.392
1997	0.184	0.387	0.185	0.388
1998	0.207	0.405	0.209	0.407
1999	0.181	0.385	0.180	0.385
2000	0.244	0.429	0.237	0.425

Table 2: Share of Sample Women Receiving Screening and Covered by an Ob/Gyn Direct Access Mandate, by Year

	<i>All Years</i>	<i>Year</i>					<i>p-value</i>
		<i>1996</i>	<i>1997</i>	<i>1998</i>	<i>1999</i>	<i>2000</i>	
Mammography sample (women age 40–64)							
<i>N</i>	100,140	14,749	16,046	20,017	21,836	27,492	
Share in state with mandate	0.600	0.111	0.375	0.596	0.875	0.938	<.001
Share with mammogram in last 2 years	0.769	0.744	0.754	0.759	0.778	0.799	<.001
Pap test sample (women age 18–64)							
<i>N</i>	189,840	28,816	30,876	38,195	41,446	50,507	
Share in state with mandate	0.596	0.111	0.376	0.601	0.874	0.937	<.001
Share with Pap test in last 2 years	0.858	0.850	0.860	0.852	0.865	0.862	<.001

Note: *p*-values from χ^2 tests of the hypothesis that the percentages are equal across years.

Pap test receipt rate is 0.8 percentage points higher (86.1 versus 85.3 percent, $p < .01$).

However, these kinds of comparisons do not control for time trends, underlying characteristics of states with and without mandates, and other sample characteristics. To address these issues, we estimated logistic regression models in the form of equation (1). Regression coefficients are shown in Table 3. For both mammography and Pap tests, the odds ratios associated with passage of a mandate are positive, but do not even approach statistical significance at conventional levels. The *p*-value from the test of the hypothesis that the direct access law odds ratio in the mammography regression is equal to 1 is .99. In the Pap test regression, the *p*-value is .82.

Leaving aside the lack of statistical significance for a moment, if one were to evaluate the implied change in the screening rates around the sample mean rates, holding all covariates fixed at their sample means, these coefficients suggest that passage of the average direct access law was associated with an increase in mammography screening rate of 0.009 percentage points, and an increase in the Pap test rate of 0.12 percentage points.

These odds ratios reflect the average effects over all of the women in our sample. As this population includes some people who were not likely to have been subject to the law, the population average effects should be interpreted as composites of the effect in the subpopulation that was affected by the law, and

Table 3: Results from Logistic Regression Analysis

	<i>Mammogram in Last 2 Years (Women Age 40–64)</i>		<i>Pap Test in Last 2 Years (Women Age 18–64)</i>	
	<i>Odds Ratio</i>	<i>SE</i>	<i>Odds Ratio</i>	<i>SE</i>
Direct access law	1.001	(0.052)	1.011	(0.049)
Age				
30–34	—	—	1.021	(0.045)
35–39	—	—	0.724**	(0.030)
40–44	—	—	0.581**	(0.023)
45–49	1.757**	(0.056)	0.501**	(0.021)
50–54	2.710**	(0.102)	0.506**	(0.022)
55–59	2.953**	(0.128)	0.438**	(0.020)
60–64	3.238**	(0.152)	0.364**	(0.017)
Race				
White, Hispanic	1.417**	(0.118)	1.133*	(0.069)
Black	1.336**	(0.068)	1.943**	(0.096)
Other	0.816**	(0.063)	0.648**	(0.034)
Health status				
Very good	1.015	(0.031)	1.000	(0.030)
Good	1.040	(0.037)	0.845**	(0.027)
Fair	0.957	(0.053)	0.826**	(0.040)
Poor	1.064	(0.092)	0.795**	(0.068)
Marital status				
Divorced/widowed/separated	1.035	(0.033)	0.952	(0.027)
Never married	0.895	(0.051)	0.406**	(0.014)
Highest education level (years)				
12	1.335**	(0.074)	1.371**	(0.065)
13–15	1.458**	(0.084)	1.566**	(0.077)
16 or more	1.649**	(0.102)	2.213**	(0.117)
Income				
\$10,000–19,999	0.945	(0.112)	1.384**	(0.106)
\$20,000–34,999	1.303*	(0.146)	1.559**	(0.114)
\$35,000–49,999	1.604**	(0.183)	1.699**	(0.129)
\$50,000–74,999	1.921**	(0.224)	1.809**	(0.144)
\$75,000 or more	2.393**	(0.289)	2.215**	(0.182)
BRFSS year				
1997	1.022	(0.047)	1.063	(0.041)
1998	1.087	(0.051)	1.016	(0.042)
1999	1.203**	(0.060)	1.116*	(0.049)
2000	1.304**	(0.067)	1.082	(0.048)
N	100,140		189,840	

Note: Survey-design-adjusted standard errors in parentheses. Models also contain 50 state indicator variables.

*.01 < p < .05;

** p < .01.

a much smaller or even zero effect in the subpopulation that was not affected. Even though the results are not significant, it could be useful to see how large the effects in the most affected subpopulation could be if one were to take the estimated odds ratios at face value. It turns out that even then the impact of a law on the most affected subpopulation is likely to be, at best, modest in size. To illustrate, we considered an example in which some of the women in our sample were affected by the law but others were not, and explored how large the change in the share of affected women who visit an ob/gyn would have to be to account for the population average odds ratios we observed. We build this example based on assumptions about the share of women in our sample affected by the laws, the share of women who see an ob/gyn regularly, and screening rates among women who do and do not see an ob/gyn regularly.

We assume that 30 percent of women were affected by these laws. This is based loosely on Interstudy and other nationwide estimates of HMO enrollment; estimates from responses to questions about health plan characteristics in the 1996 and 1997 BRFSS data indicating that about 45 percent of women in those BRFSS samples self-reported being in a plan that had a list of providers and required beneficiaries to choose a certain doctor or clinic from whom to receive all of their routine care, two characteristics generally associated with HMOs; estimates from the MEPS-IC that about 80 percent of HMO enrollees are in non-self-insured plans; and estimates from the 1999 and 2000 Kaiser/HRET employer surveys that a little over 20 percent of employer-offered HMOs are self-insured.

We assume that, at baseline, 50 percent of women see an ob/gyn regularly. Data on use of ob/gyns that precisely fits the structure of this study is scarce, but data from Commonwealth Fund surveys suggest that about 49 percent of women listed an ob/gyn among their regular physicians in 1993 (Weisman et al. 1995) and about 46 percent did so in 1998 (Weisman and Henderson 2001). 1999 data from a survey done in Baltimore, MD puts the figure at about 63 percent (Henderson, Weisman et al. 2002).

Based generally on literature suggesting that women who see ob/gyns have higher screening rates than women who do not (Horton et al. 1994; Weisman et al. 1995; Finison et al. 1999; Weisman and Henderson 2001; Henderson et al. 2002; Haggstrom et al. 2004), and assuming this effect is caused by seeing an ob/gyn and not by selection, we use the estimate that 90 percent of women who see an ob/gyn will receive appropriate screening, compared with 70 percent of women who see other types of physicians.

In this scenario, at baseline, the population screening rate would be 80 percent, generally similar to what we observe in our data for both mammography and Pap tests. We can use this framework to find the increase in the rate of seeing an ob/gyn among women affected by the law that would be required to generate the population-level odds ratio we observe for mammography. We find that if the rate of seeing an ob/gyn in the affected subpopulation rose 0.25 percentage points from the assumed baseline after passage of a law, from 50 to 50.25 percent, the odds ratio for receipt of screening would be 1.001, about what we observe in our analysis of mammography screening rates. If we apply the framework to examine the Pap test results, we find that an increase in the rate of seeing an ob/gyn by 2.9 percentage points, from 50 to 52.9 percent, would generate an odds ratio for screening of 1.011, about what we see in our Pap test analysis.

We conclude that the population average effects we observe in the regression analysis would, even if they were statistically significant, be unlikely to imply large shifts in ob/gyn use in just the subset of the population most likely to have been affected.

HMO Market Share, Self-Insurance, and Other Alternate Specifications

We expect that the effects of direct access laws would primarily be observed among subsets of the privately insured women we study, particularly women in HMOs, and even more particularly those in HMOs that are not self-insured. We would thus expect that the effects of laws to be stronger in states with higher shares of the population enrolled in HMOs, particularly those that are not self-insured. To investigate, we estimated two modified versions of equation (1). In the first, we interact the direct access law indicator with indicators for whether the respondent lived in a state with low, medium, or high HMO market share. Results are shown in the first two columns of Table 4. For mammography, results do not show a pattern consistent with stronger effects of laws as HMO market share increases. For Pap tests, there is a small progression toward larger effects of laws as market share increases, but the progression is very small and none of the results indicate a significant relationship between laws and screening.

In a second specification, we interacted the direct access law indicator with indicators for whether the respondent lived in a state with low, medium, or high prevalence of non-self-insured HMOs, based on data from the MEPS-IC. Results are shown in columns 3 and 4 of Table 4. None of the odds ratios are significantly different from one in either the mammography model or the

Table 4: Results from Logistic Regression Models, Incorporating HMO Market Share and Self-Insurance

	<i>Interaction with Mean 1996–2000 State HMO Market Share</i>		<i>Interaction with Mean 1996–2000 Prevalence of Non–Self-Insured HMOs</i>	
	<i>Mammography</i>	<i>Pap Test</i>	<i>Mammography</i>	<i>Pap Test</i>
Had D.A. law, low HMO market share state	1.054 (0.092)	0.936 (0.075)	—	—
Had D.A. law, medium HMO market share state	0.906 (0.065)	1.012 (0.065)	—	—
Had D.A. law, high HMO market share state	1.056 (0.068)	1.017 (0.061)	—	—
Had D.A. law, low non–self-insured HMO state	—	—	1.056 (0.092)	0.937 (0.075)
Had D.A. law, medium non–self-insured HMO state	—	—	0.974 (0.058)	1.041 (0.057)
Had D.A. law, high non–self-insured HMO state	—	—	1.030 (0.083)	0.981 (0.071)
N	100,140	189,840	100,140	189,840

Note : Samples for mammography models include women age 40–64; samples for Pap test models include women 18–64. Survey-adjusted standard errors in parentheses. Models also contain co-variates shown in Table 3 and 50 state indicator variables. None of the coefficients are statistically significant at the .05 level.

Pap test model, and there is no evidence in either of a progression toward stronger law effects with higher non–self-insured-HMO prevalence.

To explore the impact of varying strength of laws, we estimated separate effects for laws that appear stronger and weaker based on their language and characteristics. We designated 25 of the laws to be strong laws and 17 to be weak laws. Of women covered by any law, 65 percent are covered by strong laws (in both the mammography and Pap test samples). Regression results (shown in the appendix) indicate that the stronger laws are associated with less of an increase in mammography screening than the weak laws, and vice versa for Pap tests. None of the odds ratios are statistically significant.

Beyond the models described here, we estimated a number of additional specifications that allow for variations in the sample definition (e.g., dropping California, where some BRFSS questions vary), additional interaction terms (e.g., interacting the law indicator with education, or with the baseline rate of screening in each state), and other changes to the model (e.g., adding variables to explore the extent of bias from unobserved preferences for prevention

across states). Results are presented in the appendix to this paper. None produced evidence consistent with an effect of direct access laws on screening.

DISCUSSION

We examined the relationship between passage of ob/gyn direct access laws and use of screening for breast and cervical cancer among women. We find no evidence for a strong or consistent relationship. None of the regression results associating passage of laws with screening rates are statistically significant. Interpretation of the point estimates from our main model suggests that, even if they were statistically significant, the implied changes in screening rates or ob/gyn use would be quite limited. Further, when we estimated models that allowed the effects of the laws to vary with HMO market share and the prevalence of non-self-insured HMOs, two mediating factors that would be expected to enhance the effect of a law, we found no evidence suggesting such a pattern. Models that allowed for separate effects of stronger and weaker laws also produced results inconsistent with a true effect—weaker laws were more strongly associated with mammography screening than stronger laws. We interpret this as evidence that direct access laws are unlikely to have had a meaningful effect on use of cancer screening.

This study was not designed to identify the reasons these laws would have lacked significant effects. It may be that knowledge and application of the laws were limited, so that they did not systematically influence care patterns. It may be that seeing an ob/gyn does not have a causal effect on screening rates. It is also not clear how widespread effective restrictions on access to ob/gyns actually were before law passage. Another possibility is that the laws were passed during a time when many health plans were responding to a backlash against restrictive managed care policies and may have loosened access to ob/gyns independent of the laws. Perhaps the passage of laws in some states and the threat of passage in others contributed to a more generalized move to lesser restrictions. If this is true, then the laws may still have had some impact by helping to change the climate within which health plans operate, though it would be diffuse and not precisely related to the specific changes in utilization and screening seen in any one state at any one time. Finally, it is possible that women most interested in direct access to ob/gyns and screening were least likely to select HMOs in the first place, so the group of women most likely to be affected by the law were relatively less likely to be interested in making use of it.

This analysis focused on women in private plans, who were the target of state direct access laws. We do not study women in public plans such as Medicare or Medicaid, who are not covered by these state laws, but instead are provided direct access to ob/gyns under a federal executive order. While we expect that our results can provide generally instructive information for those with an interest in the effects of direct access regulations in public programs, differences in the populations and specific settings may lead to variations in the effects.

As this study reports a lack of association, a natural question concerns the amount of statistical power available to detect effects. We believe our results are not due simply to a lack of statistical power. Leaving statistical significance aside, our point estimates indicate that laws are associated with only small changes in utilization. Even with much larger samples, it is not clear that these estimates would be statistically significant simply because the estimated effects are themselves so small. Further, even if they were statistically significant, the practical implication that there is, at best, only a weak relationship between passage of laws and screening utilization would be unchanged.

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SUPPLEMENTARY MATERIAL

The following supplementary material for this article is available online:

Table A1: Summary Statistics for the Distribution of CASRO Response Rates for BRFSS Data, across 50 States and DC.

Table A2: Complete Results from Logistic Regression Models Examining the Impact of BRFSS Response Rate Variation on Results.

Table A3: Missing Data Rates for Key Analysis Variables.

Table A4: Complete Results from Logistic Regression Models Examining the Impact of Item Nonresponse for Income Variables on Results.

Table A5: State Law Data Used in the Analysis.

Table A6: Complete Results from Logistic Regression Models Interacting D.A. Law Variable with HMO Market Share Measures (Paper T5).

Table A7: Complete Results from Logistic Regression Models (Paper T6).

Table A8: Complete Results from Logistic Regression Models Including Women Surveyed in the Same Year as Law Passage.

Table A9: Complete Results from Logistic Regression Models, Interacting Law Variables with 1990 HMO Market Share.

Table A10: Complete Logistic Regression Results from Models Examining the Effects of Adding Controls for Smoke Detector Battery Checking.

Table A11: Complete Results from Logistic Regression Models for Additional Specification Tests (1).

Table A12: Complete Results from Logistic Regression Models for Additional Specification Tests (2).