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Insurance Mandates and Mammography

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ABSTRACT

The 1990s witnessed historic reductions in breast cancer mortality. Striking increases in screening mammography—rates more than doubled from 1987 to 2000 among prime age women—are widely seen as responsible for a substantial share of these improvements, though we know very little about what caused mammography rates to increase. In this paper we show that state mandates requiring private insurers to cover mammography significantly contributed to the large increase in screening rates. We use data on over half a million 25–64 year old women from the CDC’s Behavioral Risk Factor Surveillance System. Our empirical strategy exploits variation in the timing of mandate adoption across states as well as in the ages of women targeted by each law, resulting in triple difference estimates of the effects of mammography mandates. We find robust evidence that state insurance mandates requiring coverage of an annual mammogram significantly increased past year mammography screenings by about 8 percent, and these effects are plausibly concentrated among insured women. Moreover, we find that the mammography mandates had no effects on the probability a woman obtains cervical cancer screenings or clinical breast exams (which were not explicitly targeted by mandates). Our results confirm that regulating private insurance markets to require coverage for relatively low-cost services such as mammograms can have meaningful effects on population preventive health behaviors.

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1. Introduction

Breast cancer is the most commonly diagnosed cancer and the second leading cause of cancer death among women in the United States; 40,000 women die of breast cancer each year. Early detection of breast cancer through regular screening mammograms is commonly understood to be the most important determinant of survival. Cutler (2008), for example, documents that increases in routine cancer screenings such as mammography represent the most important factor behind the reversal in age-adjusted cancer mortality rates that occurred in the 1990s. Indeed, the increase in population mammography rates was particularly broad-based from 1987 to 2000: screening rates among 25-64 year old women approximately doubled for women of different race/ethnicity, marital status, education, and even household income groups.¹ As such, the increase in mammography over the 1990s is arguably one of the more striking improvements in women's preventive public health behaviors.²

What factors contributed to this substantial increase in mammography among prime age women? Surprisingly, there is extremely little evidence on the policy determinants of these broad-based improvements. In this paper we provide the first comprehensive assessment of the utilization effects of state laws requiring private insurers to cover screening mammograms. These "mammography mandates" were adopted by nearly every state in the US from 1987 to 2000, making mammograms one of the most commonly conducted medical procedures whose coverage is specifically

¹ Figure 2 shows these trends for all women age 25–64 and for subgroups of women of different ages. We focus here on prime age women to abstract from public policy effects on mammography use among Medicare-eligible women, which has already received a great deal of attention (see Card, Dobkin, and Maestas, 2008a and 2008b for discussions of health care utilization effects of Medicare, and see Kelaher and Stellman 2000 for a policy analysis of the effects of changes in reimbursement of mammography among the Medicare population.

² Over this same time period, rates of Pap test rates for cervical cancer among 25–64 year old women were essentially flat.

mandated by state insurance laws. Such mandates require private insurers to cover (or, in a few cases offer) mammography benefits in the plans they sell. Firms which purchase insurance are directly affected by these mandates; self-insured firms are not required to comply due to the well-known exemption provisions of the Employee Retirement Income Security Act (ERISA).³ Butler (2000) estimates that about a third of women have private insurance that would potentially be affected by mandates such as those we study here.⁴

Could state-mandated changes in private insurance coverage of mammograms possibly explain part of the increase in mammography over this time period? Prior studies on the reasons women do not obtain mammograms despite their relatively modest expense (about \$100 per screening) suggest that cost is a potential barrier. Moreover, private insurance coverage of screening mammography did not become widespread until the mid 1990s despite the fact that the lifesaving benefits of mammograms were established in the mid 1970s.⁵ By 2000, however, coverage of mammography in private health insurance plans was nearly universal (Kaiser/HRET 1999). As such, it is plausible that state mammography mandates could have contributed to the striking increase in the population mammography rate over the 1990s by increasing coverage of the benefit in insurance plans.

³ Whether and to what extent self-insured firms respond to state insurance mandates (perhaps due to competitive labor market concerns) is an empirical question that has not been settled in the literature.

⁴ The 2000 Medical Expenditure Panel Survey (MEPS) Household Component for 2000, for example, finds that 72.4% of adults 18–64 had private coverage in 2000, while 7.6% had public coverage and the rest were uninsured. The Insurance Component of the MEPS allows for tabulations of the share of workers who are enrolled in private insurance by various characteristics. For 2000, the MEPS IC suggested that of private employees, 89.4% were at firms which offered health insurance. At firms which offered insurance, 64.1% of employees were enrolled in health insurance, and of those enrolled, 48.3% were in self-insured plans (plans which are exempt from these type of mandates). This means that around 30 percent of workers were enrolled in non-self insured plans $((1-.483) * .641 * .894 = .296)$. If we assume that the same share of women with private insurance as workers with private insurance are enrolled in these type of plans, this would suggest about 21% of women would have private insurance subject to these type of regulations $(.296 * .724 = .214)$.

⁵ A 1986 article in *The New York Times* lamented that “health insurance plans rarely, if ever, cover screening mammograms, which can detect problems at the earliest and most curable stage” (Brozan 1986).

Of course, there are several reasons why mammography mandates may have had no effects or even *negative* effects on mammography utilization. In addition to the somewhat limited potential coverage of mandates (mostly due to ERISA exemptions), some researchers and policymakers have also argued that benefits mandates such as those we study here cause some employers—particularly small firms—to reduce offers of health insurance. Finally, it is possible that mandates largely codified the benefits that were already being offered by insurance plans. As such, the relationship between mammography insurance mandates and mammography use is ultimately an empirical question.

To evaluate the utilization effects of state mammography mandates, we draw on data with information about mammography use for over a half million women from the 1987–2000 Behavioral Risk Factor Surveillance System (BRFSS), a publicly available dataset from the Centers for Disease Control that is designed to be representative at the state level in each year. The BRFSS questionnaires have asked women about mammography screenings since 1987, and the data also include standard demographic characteristics for the survey respondents. A standard empirical approach in this setting would take advantage of the staggered timing of adoption of the state insurance mandates across states in a difference in differences (DD) framework with state and year fixed effects, as well as controls for individual-level characteristics (e.g., age, race, education, and marital status), annual state economic and demographic characteristics, and other relevant public policies that may affect health insurance coverage, access, and screening outcomes. Our approach for identifying the effects of the mammography mandates on outcomes makes use of additional variation induced by the age-specific nature of

mammography mandates. Specifically, almost all state mammography mandates define different covered benefits for women of various ages (most commonly, age 35 to 39, 40 to 49, and 50 and older, thus providing no treatment to women age 34 and younger). Thus, we can estimate fully interacted triple difference models that control for state by age group effects, year by age group effects, and a full set of state by year dummies. In these augmented models, the effects of the mandates are identified from differences in mammography screenings for women whose age makes them treated compared to the associated outcomes for women whose age makes them untreated coincident with the timing of policy adoption within each state.

To preview, we find strong evidence that state mandates for screening mammography—particularly mandates requiring insurance plans to cover annual mammograms—have statistically significant and economically meaningful effects on recent and lifetime mammography rates. Specifically, we estimate that adoption of an annual cover mandate increases past year mammography rates by about 8 percent relative to the baseline. This result is highly robust to a variety of specification checks. State mammography mandates also changed the stated reasons for having obtained mammograms (reducing the probability a woman was screened and reports her most recent mammogram was due to a problem and increasing the probability a woman was screened and reports her most recent mammogram was part of a routine screening). We also confirm that the mandate effects are plausibly driven by effects among insured women. We find no evidence that the mandates affected Pap tests or clinical breast exams, suggesting that the mandate effects are unique to mammography and are not reflecting other unobserved determinants of women’s health more generally. We

estimate that mandates for annual coverage of mammograms can account for about 7 percent of the total increase in the annual mammography rate in the population from 1987 to 2000. Our results have important implications for debates about the most effective ways to increase other types of preventive behaviors that are at levels far below well-accepted medical recommendations, such as screenings for colon and prostate cancer. Specifically, our results suggest that private insurance market regulations have the potential to play an important role at improving preventive public health behaviors.

The paper proceeds as follows: Section 2 outlines institutional details regarding mammography and the insurance mandates under study. Section 3 briefly describes the previous literature, and we describe the research design in Section 4. Section 5 describes the data and empirical approach. Section 6 presents the main results, and Section 7 concludes.

2. Institutional Details

Mammography is the standard approach for screening women for early detection of breast cancer. In mammography, a woman's breasts are placed on a machine that takes low-dose X-ray pictures to check for abnormalities. Screening mammography is different from diagnostic mammography in that the latter is typically done in the presence of a physician with on-site interpretation of the results, while the former can be done in a variety of settings and is not generally read on-site. Diagnostic mammography usually occurs when a woman has had a previous abnormal screening mammogram (approximately 10% of those screened in the early 1990s), as well as among women with a family history of breast cancer (Dans and Wright 1996). In addition to diagnostic

mammography, abnormal screening results can also lead to more invasive procedures such as biopsy.

Nearly all states adopted mammography benefits mandates for qualified private health insurance plans from 1987 to 2000. The modal state mammography mandate adopted in the late 1980s and early 1990s calls for private insurance plans within the state to either cover or, less commonly, offer baseline screening mammograms for 35 to 39 year olds, biennial mammograms for 40 to 49 year olds, and annual mammograms for women age 50 and older. These mandates apply to the insurance companies who sell insurance to private employers (or, in some cases to individuals). Women who have their own employer-related private insurance coverage or who have insurance through employed husbands would be affected by these mandates if the firm were not self insured.⁶

These age-based benefits reflect the age-specific mammography frequency recommendations supported by the American Cancer Society (ACS) from 1983 until 1991. In 1992 the ACS eliminated the recommendation that 35 to 39 year olds obtain a baseline screening mammogram, and in March 1997 the ACS further revised its recommendations to state that annual screening mammography should begin at age 40.⁷

⁶ More precisely, these mandates cover private plans where the risk is not taken on by the purchaser. So, employers who self-insure and take on the risk of the insurance themselves are exempt from such mandates via ERISA. Despite this large ERISA exemption, many employees are still likely to be covered by these mandates. Bitler and Schmidt (2009) find that 14–19% of private employees were enrolled in a plan affected by mandates involving infertility treatment (which have only been passed in 15 states).

⁷ There has not, however, been uniform agreement across major medical organizations with respect to these recommendations. The US Preventive Services Task Force (USPSTF), for example, did not recommend annual screening mammography for women over age 40 until 2002—prior to this, the USPSTF only recommended annual mammograms for women age 50 and older (i.e., the pre-1997 ACS guidelines). Despite the fact that different organizations have used different age cutoffs for screening mammography recommendations, a study by Rathore et al. (2000) shows that the ACS guidelines are the ones that are most commonly applied in state mammography mandates. For our preferred triple difference models described below, it is important to note that our estimates of the effects of mandates will rely only on variation at the state by age group by year level coincident with the timing of mandate adoption; any

In recognition of these changes, some of the mammography mandates adopted in the latter part of our sample period revised pre-existing guidelines to require plans to cover (or less commonly offer) annual mammography screenings for women age 40 and older. Moreover, a handful of states have used different age-based cutoffs in their laws, and these laws provide us additional age by state by year variation in our triple difference models below. For example, Wisconsin's 1990 law requires coverage for two mammograms for women age 45 to 49, provided they have not had one within two years (i.e., this law mandated coverage of nearly biennial mammography beginning at age 45). The District of Columbia's 1990 law did not set any explicit age limits, which we interpret as requiring coverage for annual screening mammography for all women. Texas' 1987 mandate requires coverage for annual mammograms for all women age 35 and older. As such, there is substantial age by state by year variation in the types of treatments espoused in state laws that forms the basis of our identification in the DDD empirical models below.

In addition to the age groups targeted by the laws, the other important distinction that we focus on here for the mammography mandates is whether the law is a cover mandate or an offer mandate. Cover mandates require privately sold plans to include coverage of mammography while offer mandates only require that insurers offer at least one such plan to an employer. We would typically expect offer mandates to be weaker than cover mandates (i.e., have smaller effects on utilization) since the latter should more strongly reduce barriers to screening mammography for those privately insured women

recommendations from major medical organizations will be absorbed by the age group times year interactions since, although the recommendations themselves are age-based, they are nationwide guidelines (i.e., not state-specific).

who did not have coverage previously.⁸ In practice, the bulk of our results pertain to cover mandates because they are far more common in our setting than are offer mandates. For example, only eight states ever had any type of offer mandate for screening mammography over our sample period, and five of these eventually adopted a cover mandate.⁹

3. Previous Literature

We do not review here an enormous literature in public health that documents associations between demographic characteristics and mammography rates for various segments of the female population.¹⁰ Surprisingly, there is extremely little research that estimates the effects of state insurance benefit mandates requiring coverage of mammography.¹¹ We were able to find just two unpublished public health abstracts that have examined the effects of multiple state mammography mandates. Both of these multi-state mammography mandate studies used cross-sectional designs. Mor and Shackelton (2005) use county level mammography screening rates from the 2002 wave of

⁸ If there were no incentives to adjust coverage decisions besides the text of the laws, employers in offer states who did not wish to add the coverage could simply choose plans which did not include the “offered” coverage of mammograms.

⁹ Conley and Taber (2005) show that over rejection can be severe in difference in differences models with a small number of policy changes.

¹⁰ This literature includes several studies on the relationship between health insurance coverage and mammography (see, for example, Trivedi, Rakowski, and Ayanian 2008), though these studies are largely descriptive and do not directly address what drives variation across individuals in the presence or type of coverage.

¹¹ A handful of studies have evaluated changes in Medicare reimbursement policy for screening mammography. Given the existing studies, the nearly universal coverage of Medicare for women 65 and older (and large effects of eligibility for Medicare on various utilization measures documented in Card, Dobkin, and Maestas, 2008a, 2008b), and the fact that the laws we evaluated refer to private insurance, we focus on women under age 65 in our analysis. These studies, however, are clearly related to the questions we study here since they relate to the utilization effects of changes in public policy related to insurance coverage of mammography. Kelaher and Stellman (2000) find that when Medicare Part B began covering biennial mammography in 1991, past two year mammography rates for Medicare eligible women significantly increased relative to younger women who were not eligible for Medicare.

the BRFSS and find that past year screening rates are 8% higher in states with specific mammography mandates. Pettibone (2003) used a single cross section of the MEPS and found that women age 40 to 49 were more likely to receive an annual mammogram if they lived in a state with a mammography mandate, though no effects were found for women of other age groups. Of course, unobserved fixed differences across states could contribute both to the presence of a mammography screening mandate and to mammography screening behaviors, and cross-sectional designs cannot rule out these factors. Finally, we note that one published study has evaluated the effects of a single state's mammography mandate on utilization. Dans and Wright (1996) examined claims data for outpatient mammograms for women in Maryland's Blue Cross Blue Shield plan before and after the state's 1991 mammography mandate was implemented. They found evidence of a modest increase in overall screening rates. We could find no quasi-experimental work (published or otherwise) that used the timing of mandate adoption for all 50 states to control for fixed differences across states.

This absence of a substantial literature on the utilization effects of mammography benefits mandates is striking for several reasons. As noted previously, mammography is one of the most commonly mandated benefits at the state level (CAHI 2008), and over this time period when most states were adopting mammography mandates, mammography rates saw unprecedented increases for older women. Indeed, public health studies that have documented the increasing trend in mammography over the 1980s and 1990s discuss the role of mammography mandates as a seemingly well-documented determinant of the improvement in women's preventive health. Nelson et al. (2002), for example, write in *JAMA* that “[e]ducational campaigns directed toward health

care practitioners and the general public, state mandates for insurance coverage of mammograms, and programs for providing mammography services to low-income women have all played a role in increasing breast cancer screening in nearly all states.”

Finally, the lack of research on mammography benefits mandates also contrasts markedly with other types of state level insurance benefit mandates, some of which have received a great deal of attention. Pregnancy benefits, (Gruber 1994a), infertility treatment (Bitler 2008, Schmidt 2007, Bundorf, Henne, and Baker 2007, Buckles 2008, and others), mental health parity (Pacula and Sturm 2000, Harris, Carpenter, and Bao 2007, Busch and Barry 2008, and others), and overnight hospital stays for newborn deliveries (Liu, Dow, and Norton 2004) are just some of the examples of mandated insurance benefits that have generated substantial literatures.

Importantly, researchers have identified a number of considerations for understanding the extent to which any mandated benefits laws should be expected to affect utilization. First, it is commonly argued that mandated benefits laws could cause employers—particularly small firms—to reduce offers of health insurance in response to the rising costs when mandated benefits laws are adopted. While the empirical evidence on this is very mixed (Gruber 1994b, Jensen and Gabel 1989, Jensen and Morrissey 1999), any such effects would reduce the potential for benefits mandates to affect utilization. Second, as we noted above, certain insurance plans are exempted from compliance requirements with any state health insurance mandates. The largest of these is the exemption because of ERISA for self-funded insurance plans which generally affects large employers. Buchmueller et. al. (2007) use the MEPS-IC and find that this is the most important factor that reduces the potential population covered by mental health

parity mandates. Liu, Dow, and Norton (2004) also find evidence that the ERISA exemption diluted the effects of minimum length of stay legislation at the state level using variation induced by a federal mandate—which cannot be avoided even by self-insured plans.

Third, it is possible that benefits mandates do not have much “bite” to the extent that pre-existing private health insurance plans were already covering or offering the services addressed in the mandates. However, available evidence indicates that benefits coverage for these services was far from complete over our sample period, implying that there was substantial latitude for mammography benefits mandates to affect benefits coverage and, subsequently, utilization. Sullivan and Rice (1991), for example, report that the Health Insurance Association of America (HIAA) employer benefits survey fielded in 1990 showed that about 68 (67) percent of private plans were covering mammography (Pap tests) in 1990.¹² McKinney and Marconi (1992) similarly report that 63 to 72 percent of non-self-insured plans (i.e., those potentially subject to the benefits mandates) covered screening mammography in the 1990 HIAA survey. By 1999 the Kaiser/HRET Survey of Employer-Sponsored Health Benefits found that 94 percent of conventional plans and 98 percent of HMO plans were covering mammography screening, suggesting a large increase in mammography coverage over a period of significant mandate adoption. These patterns indicate that: 1) private insurance coverage of these services was far from universal at the time the first mandates were adopted; and

¹² We do not know, however, whether these figures refer to screening mammography or diagnostic mammography, though popular press suggests that private plan coverage of diagnostic mammograms was far greater than those for screening mammograms (Brozan 1986). This would suggest even greater latitude for benefits mandates to have changed actual benefits coverage over our time period.

2) this rate increased substantially over the 1990s, such that private insurance coverage of mammography was nearly universal by 2000.¹³

Finally, it is natural to ask—given the fairly low cost of low-dose screening mammography (\$50—\$150 per screening according to Breen and Brown 1994)¹⁴—why weren't all employers and health plans covering these screenings even in the absence of a mandate? Note that although the cost of an individual screening is relatively low, the population at risk of using a mandate is very large: currently, all women age 40 and older are recommended to get regular screening mammograms annually. In contrast, most benefits mandates that have been studied previously (e.g., infertility treatment, substance use/alcoholism treatment) have the potential to affect a much smaller portion of the population and are for services that are used far less frequently than are screening mammography. Finally, even though the direct costs of the actual screening are fairly low, the subsequent costs associated with a positive screening—biopsy, mastectomy, and other cancer treatments—can be much larger. Like many screening tests, mammography tests have a high false positive rate: given that upwards of 10 percent of screening mammograms can produce abnormal results, these costs are potentially very large. Poplack et al. (2005), for example, used New Hampshire mammography registry data to find that 13 percent of women had diagnostic imaging after a screening. Total direct costs per capita (using Medicare reimbursement rates) were \$99 per woman if the women

¹³ There are, of course, other reasons why mandates could have limited effects on utilization other than those we list here. For example, individuals may not be aware that they are covered by or eligible for such benefits and/or individuals may not be able to afford the copayments and deductibles associated with obtaining the benefits (since these are generally not regulated by state insurance mandates).

¹⁴ We are not aware of good estimates of how the costs of mammography have changed over time. Mammography technology, however, seems not to have changed substantially over the period we study. More recently, however, computer-aided detection (CAD), designed to assist radiologists in reviewing suspicious areas of the breast, has increased. The Food and Drug Administration approved the first use of CAD in June 1998, though their use was very rare through 2001. The clinical efficacy of CAD has not been fully documented (Fenton et al. 2007).

only had screening mammograms but rose to \$286 per woman with diagnostic imaging and \$993 per woman if there was a biopsy. The overall total cost of \$124 was therefore substantially higher than the costs of screening mammography alone.¹⁵

4. Research Design

We are interested in identifying the casual effects of state laws requiring private insurers within a state to cover or offer screening mammography on population mammography rates. An obvious concern with the raw associations between mandates and mammography use is that unobserved characteristics about women living in states with mandates may contribute both to screening behaviors and to policy adoption. Alternatively, there were other changes to the health care delivery system over our time period that could introduce bias: HMO penetration increased over this time period in a way that was plausibly correlated with policy adoption, for example, and it is generally believed that HMOs are particularly good at increasing use of preventive services. A third way in which simple correlations might be misleading is if states engaged in public outreach efforts that corresponded with the timing of and age groups affected by the mandates. In all three of these cases, the association between the mammography mandates and screening outcomes is likely to be overstated.

The standard approach in economics and policy evaluation to deal with these potential omitted variables is to use variation in the timing of adoption of the policies in

¹⁵ Note that ideally we would observe the marginal premium cost of adding mammograms to the policy. Following Gruber (1994b) and others, we searched for information about the share of premium costs due to mammography mandates. Evidence from a 2000 Texas Department of Insurance report on the cost of mandates suggests that the Texas mandate for mammography screening was responsible for 0.6% of total premium costs. This figure is smaller than the analogous premium shares for the 5 mandates identified as “expensive” in Gruber 1994b but is still substantial and similar in magnitude to benefits related to alcohol treatment, chiropractor services, and continuation of health insurance coverage (Gruber 1994b).

state- and year-level fixed effects models of mammography use. To the extent that the unobserved factors contributing both to outcomes and to policy adoption are time invariant within a state or within a year, the two-way fixed effects models will remove this bias. Moreover, direct controls for adoption of other relevant programs, policies, and state characteristics (such as managed care and HMO penetration) can further reduce the omitted variables bias problem. In these difference-in-differences models the key identifying assumption is that there were no other unobserved shocks to outcomes coincident with policy adoption that affected screening outcomes.

Of course, it is certainly not the case that all women should have been treated by the policies. Indeed, previous research has argued that one reason benefits mandates (and direct access laws) may have limited effects is that many people with private health insurance are actually not subject to the mandates because they work for large firms with self-funded health plans that are exempted from state mandates through ERISA provisions (and additionally often may already cover the services in question). One could imagine incorporating these other behavioral dimensions directly into the estimation to estimate triple difference models using some variant of health insurance, employment status, firm size, being offered private health insurance coverage, or HMO participation as a way to identify the treatment group of interest. This approach has at least two direct limitations in our context. First, over the sample period of interest, the BRFSS does not consistently include variables that could proxy for women's having private health insurance that would not be subject to the ERISA exemption (e.g., own/spouse's employment status, firm size, having a private plan offered, or being covered by an

employed spouse's health insurance).¹⁶ Second, even if we did observe such information we might be concerned that many of these characteristics (e.g., firm size, whether the individual takes up coverage, whether the firm offers coverage) are themselves choices and decisions that could be correlated in important ways with the unobserved determinants of outcomes or could directly respond to the presence of the mandates.

Fortunately, our setting for the mammography insurance mandates allows another source of variation in addition to the staggered timing of mandate adoption and on a margin that is clearly exogenous: age. As described above, most state mammography mandates have age-based rules regarding the frequency with which mammography is required to be included as part of the cover or offer mandates. The age-based variation means that we can relax the identification assumption in the difference in differences model by including age group by state, age group by year, and state by year fixed effects in a triple difference setting similar to that proposed by Gruber (1994a). In this augmented model we identify the effects of the mammography mandates on outcomes only using the variation in outcomes for “treated” women at or above the age-based eligibility threshold relative to outcomes for “control” women under the age-based eligibility threshold coincident with timing of mandate adoption. Note that any nationwide age-specific confounders such as age-based cancer screening guidelines adopted by major medical organizations are subsumed by the age group by year interactions. State by age group fixed effects further control for time invariant differences across women of different ages within each state.

¹⁶ In fact, over our time period BRFSS only consistently reports whether the respondent is covered by any health plan and whether the respondent is employed (working for wages and salaries or self-employed). From 1996–2000 there are more detailed questions about the source of health insurance coverage, though even in these years there is still no information on firm size or whether the plan is self-funded.

The key advantage of this fully interacted DDD specification is that most of the other important likely confounders such as HMO penetration, the extent of self-insurance within the state, and/or other state laws relating to health insurance and women's health do not plausibly vary by age. For example, it is extremely unlikely that 35 year old women (who are generally treated by the mandates we study) are differentially likely to be enrolled in HMOs or to work for firms that self-insure compared to 34 year old women. It is even less plausible that any such age differences are correlated with the mandates. In any case, these other factors that do not vary by age are completely accounted for when we include a full set of state by year indicators.¹⁷ As such, the only remaining threats to identification in the fully interacted model are those omitted variables that are themselves age-specific in the same way as the mandates and that are correlated with the timing of mandate adoption. Such biases are likely to be very small.

5. Data Description and Empirical Approach

Our main data come from the Center for Disease Control's Behavioral Risk Factor Surveillance System (BRFSS). Fielded annually since 1984, the BRFSS has included questions about mammograms in every year since 1987 and was designed to be representative at the state level. Surveys are fielded by the individual states and then sent to CDC to be compiled into a public-use dataset. Our analysis focuses on the period 1987 to 2000, a period during which over 45 states adopted mandates. We stop our sample in 2000 because major federal laws changed in 2000 regarding funding for breast cancer treatments for low-income women through the Breast and Cervical Cancer

¹⁷ The triple difference approach also alleviates concerns about policy endogeneity: while one may worry that unobserved shocks to mammography rates drive mandate adoption, those same shocks would have to differ by age in a systematic way to bias the estimated mandate effect in the augmented model.

Prevention and Treatment Act of 2000.¹⁸ State participation in the BRFSS increased over the late 1980s, and the last state joined the BRFSS in the mid 1990s. In practice, this means that we have an unbalanced panel; because many states adopted laws prior to 1990 we use all available data (i.e., any state/year combination with BRFSS data), though in robustness analyses we focus on the subset of states for which we have a balanced panel.¹⁹

The BRFSS breast health questions allow us to create consistent measures of mammography use along several dimensions for women age 18 and older (as discussed below, we restrict our eventual samples to women 25–64). Specifically, in 1995, women were asked: “A mammogram is an X-ray of each breast to look for breast cancer. Have you ever had a mammogram?” Women who report ever having had a mammogram are then asked about the timing of their most recent mammogram, as well as the reason for their most recent mammogram.²⁰ We create three key outcome variables related to mammography use: first, we identify Ever Had Mammogram as equal to one if the

¹⁸ Specifically, the BCCPTA gives states the option to use their Medicaid programs to cover breast cancer treatments for women who were screened through the National Breast and Cervical Cancer Early Detection Program (NBCCEDP). The National Council of State Legislatures reports that 49 states have adopted these programs. We do not examine this program because we lack information on breast cancer treatments; moreover, the total number of women served by the BCCPTA is very small relative to the number of women screened through the NBCCEDP (we do control for whether the state has implemented a pilot or full NBCCEDP program in all specifications).

¹⁹ The number of states in the balanced panel changes depending on the first year of the panel. This is because the mammography questions were only asked as part of a women’s health module in 1988 (questions in modules of the BRFSS are not administered by all states). As such, the 15 states observed in all years from 1987 to 2000 includes: California, Illinois, Indiana, Kentucky, Maine, Maryland, Montana, Nebraska, New Hampshire, New Mexico, New York, North Carolina, South Carolina, Washington, and Wisconsin. If we create a panel starting 1989, however, several more states are included. The same is true if we simply eliminate 1988 data to create a 1987–2000 (less 1988) balanced panel. Results are robust to restricting the sample to these balanced panels (as is discussed below in footnote 33).

²⁰ Beginning in 1989, the survey eliminated an introductory screener question about whether the respondent heard of a mammogram (this screener was preceded by text informing women that a mammogram was an X-ray of the breast to detect cancer). After this, the introduction to the question about lifetime mammography use included a sentence defining a mammogram. We code women in the early waves who report that they had not ever heard of a mammogram as also not ever having had a mammogram (this is a very small share of women).

woman reports ever having had a mammogram and zero otherwise. Second, we create Mammogram in the Past Year as equal to one if the woman reports that she had a mammogram within the past year and zero otherwise.²¹ Third, we create Mammogram in the Past Two Years as equal to one if the woman reports that she had a mammogram within the past two years.²² Recall of the timing of a woman's most recent mammogram beyond one year is likely to be problematic (Warnecke et. al. 1997); as such, we focus on Mammogram in the Past Year as our main outcome of interest.

Women are also asked about the reason for their most recent mammogram, and we identify three outcomes of interest using this information. First, we create Routine Mammogram that equals one if a woman reports her most recent mammogram was part of a routine checkup and zero otherwise. Second, we create Problem Mammogram that equals one if a woman reports her most recent mammogram was due to a specific breast problem. Third, we create Cancer Mammogram that equals one if a woman reports her most recent mammogram was due to cancer. These three measures are mutually exclusive. The analysis sample for these outcomes includes all women—including those who have not ever had a mammogram—since we are interested in effects on population mammography use.

We also observe standard demographic characteristics in the BRFSS, including age, race, education, marital status, family income (in ranges), and employment status.²³

The BRFSS also includes a very basic measure of health insurance coverage: we are able

²¹ Item non-response is fairly low for these questions. We omit observations with a “don't know” or “refused” response to the mammogram questions.

²² Note that we lack information on the exact timing of the most recent mammogram (beyond first year, second year, or later).

²³ We choose not to control directly for employment or household income in the regression models below due to their likely endogeneity with our outcomes and key variables of interest. Additionally, household income is missing for a large share of the sample. Finally, the cutoffs for the income ranges do not vary over time and are thus not defining constant percentiles of the income distribution.

to identify whether the woman is covered by “any health plan”, though for our main sample period we cannot distinguish who pays for the plan, what the plan covers, whether the plan is in her own name, whether it is a public plan, and other important related questions.²⁴

To estimate the effect of the various public policies on outcomes we use straightforward difference-in-difference and augmented triple difference models that identify the effects of the mandates using variation across states in the timing of adoption and in the ages of women treated by the various policies. We begin with the fully saturated triple difference model, which embeds the difference in differences specification. Specifically, we formulate the triple difference model as:

$$(1) Y_{iast} = \beta_0 + \beta_1 X_{iast} + \beta_2 (\text{Share of Relevant Reference Window Treated by a Mammography Mandate for Baseline Screening})_{ast} + \beta_3 (\text{Share of Relevant Reference Window Treated by a Mammography Mandate for Biennial Screening})_{ast} + \beta_4 (\text{Share of Relevant Reference Window Treated by a Mammography Mandate for Annual Screening})_{ast} + \beta_5 Z_{st} + \beta_6 S_s * A_a + \beta_7 T_t * A_a + \beta_8 S_s * T_t + \epsilon_{iast}$$

where Y_{iast} are the various dichotomous screening outcomes for woman i in age group a in state s at time t . X_{iast} is a vector of individual level demographic controls that includes: 5-year age group dummies, race/Hispanic ethnicity, education, and marital status.²⁵ The

²⁴ For a subset of our analysis period (1996–2000) the BRFSS did include questions about the type of health insurance coverage held by individuals, though there is much less policy variation that we can use for identification over this time period.

²⁵ Specifically, we control for age group dummies (30–34, 35–39, 40–44, 45–49, 50–54, 55–59, 60–64), race/ethnicity (non-Hispanic black, other non-Hispanic, Hispanic), education (less than high school, high school degree, some college, and don’t know/refused), and marital status (widowed/divorced/separated, never married, cohabitating, and don’t know/refused); thus the omitted categories are: age group is 25–29, race/ethnicity is white non-Hispanic, education is college degree or more, and marital status is currently married.

first three policy variables reflect the mammography mandates which vary at the age, state, and year level.²⁶ Recall that the modal mandate adopted in the late 1980s requires coverage for a baseline screening mammogram for women age 35–39, a biennial mammogram for women age 40–49, and an annual mammogram for women age 50 and older. Thus for a state with the modal mandate, the baseline screening mammogram law dummy would be on for women 35–39, the biennial screening mammogram law would be on for women 40–49, and the annual screening mammogram law would be on for women 50–64. In the latter part of our sample period, several states made their laws more generous by requiring coverage of an annual mammogram for women age 40 and older.²⁷ Though we do not show this in the equation, we enter each of these variables separately for cover and offer mandates.²⁸

²⁶ There is a great deal of variation across states in the language regarding when the laws are supposed to take effect. Some states set a date after which “all policies sold or renewed after that date” must comply with the mandate, while others state that benefits must be changed effective immediately. We have coded plans as taking effect January 1 of the year after the year in which they are passed, with the logic that most policies are negotiated in the fall to take effect at the beginning of the following calendar year.

²⁷ Our policy data come from the National Cancer Institute’s State Cancer Legislative Database (SCLD). SCLD tracks every piece of legislation pertaining to different types of cancers, including breast cancer. We used a SCLD-produced table showing every state’s mammography mandate activity that included information on substantive revisions to the state laws, the year and quarter of law adoption, the age groups and mammography frequency described in the law, whether the law is an offer or a cover mandate, whether the law applies to the state’s Medicaid program, and whether the law applies to public employees within the state (usually through the state’s medical plan for public employees). To verify the information in the SCLD table we next consulted actual text of each state’s laws by calling up individual records in SCLD. Discrepancies were discussed between the two authors. Our information on state participation in the NBCCEDP program comes from personal correspondence with Janet Royalty at the CDC. Our information on direct access laws comes from Baker and Chan (2007).

²⁸ Note that the BRFSS questions introduce a “reference window” problem due to the fact that the questions typically ask about screening behavior over some recent period. Given this, it is important to account for the systematic BRFSS interview structure when defining someone as treated by the policy in question. Specifically, we can make use of the fact that BRFSS interviews are distributed almost uniformly across the calendar year. This information, coupled with our decision rule regarding when individuals are first treated, means that we can create a more precise treatment variable that captures the share of the recent period that the individual was treated by the mammography mandate. The intuition here is straightforward: since we define a policy to turn “on” in January 1 of the year following adoption, it is true that people interviewed in, say, February of what we define as the first treatment year will have only been exposed to two months of treatment while people interviewed in, say, November of that same year in that same state will have been exposed to 11 months of treatment. Similarly, for the past two year outcomes we code individuals interviewed in January after the adoption year as being treated 1/24, February of the adoption

Dummy variables for each state are captured by S_s , and in the DD models, control for time-invariant state-specific factors. Dummy variables for each survey year are captured by T_t , and in the DD specifications, control for period-specific shocks common to all states in any given year.²⁹ $S_s * A_a$ is a full set of state by age group dummies, $T_t * A_a$ is a full set of year by age group dummies, and $S_s * T_t$ is a full set of state by year dummies.³⁰ The $T_t * A_a$ indicators remove biases common to all women of a particular age in a given year; for example, the introduction of age-specific screening guidelines on a national level. The $S_s * A_a$ indicators account for other age-specific state effects which would arise, for example, if a certain state targeted women of a certain age through education campaigns. Finally, the full set of state by year interactions $S_s * T_t$ account for any other efforts to increase mammography rates in a particular state and year that would be expected to affect women of different ages equally (e.g., general education campaigns, other laws that are not age-specific). In this augmented triple difference model, the coefficients of interest, β_2 – β_4 , use variation at the age by state by year levels to identify the effects of screening mammography mandates from differences in screening rates for

year as being treated 2/24, and so forth, until December of the following year (i.e., December in the second year after adoption) as being fully treated (i.e., 24/24). Note that even if our assumptions about when insurance policies reset are incorrect, it remains the case that people interviewed toward the beginning of the calendar year after implementation will, by construction, have less potential treatment than individuals interviewed toward the end of the calendar year no matter when the policy was implemented. Finally, note that since we do not observe birth date information in the BRFSS we are incorrectly coding some fraction of the reference window for people who “aged into” eligibility within the reference window (generally people who turned age 35, 40, or 50 within the reference window). Given a uniform distribution of birthdays and correct self-reports of age, this measurement error will result in attenuation of our coefficients of interest.

²⁹ We also include month of interview dummies throughout (though not shown in the equation) to account for idiosyncratic month effects (e.g., October is Breast Cancer Awareness Month).

³⁰ Note that throughout our reported DDD specifications we are controlling for age using 5-year age group dummies and their associated interactions with state and year fixed effects. We also experimented with estimating a fully flexible specification that included single year of age dummies and their associated interactions with each state and year fixed effect, and results were nearly identical to those reported in all cases, suggesting that we are not losing much by failing to account for the sharp single-year-of-age differences in mammography among 35 to 45 year-old women evident in Figure 1. For computational ease we report the specifications that control for age-group dummies instead of single-year-of-age dummies.

women at or above the affected minimum age threshold compared to the rates for women below the affected minimum age threshold coincident with timing of policy adoption within each state. Throughout, we cluster the standard errors at the state level (Bertrand, Duflo, and Mullainathan 2004). Regressions are weighted to be population representative, and the main sample is all women aged 25–64 interviewed by the BRFSS in survey years 1987–2000.

In practice, we also estimate more standard DD models which would be appropriate and the best we could do if we did not have additional age-based variation. For the models without the state by year (and state by age and age by year) fixed effects, we also include covariates that vary at the state and year level and that are standard in two-way fixed effects models such as ours. These variables are captured in Z_{st} (which falls out of the fully interacted DDD model), a vector of state economic and demographic characteristics, including: the unemployment rate, the HMO penetration rate, the number of obstetric beds in the state per 1000 women age 15–44, the share of women age 15–44 with private health insurance, the share of women age 15–44 who work (or whose spouses work) at private firms of various sizes (<25, 25–99, 100+), fraction black, fraction Hispanic, and fraction urban. The Z_{st} vector also includes controls for other relevant public policies that may be expected to affect outcomes, including: the presence of a law requiring women to be able to see an OB/GYN without first obtaining a referral from her primary care provider; the presence of a state low-income screening program through the National Breast and Cervical Cancer Early Detection program, Medicaid

expansions for pregnant women (a proxy for generosity of the states' public health insurance programs), and welfare reform.³¹

6. Results

In Figure 1 we show the age distribution of the probability that women received a mammogram in the past two years for women age 25–64. As has been well established, mammography rates are strongly increasing in age, particularly starting around age 35. There are noticeable sharp increases in mammography rates at age 35 and again at age 40 that are likely due to the mammography screening recommendations from the American Cancer Society and other organizations. There is not, however, a visible increase at age 50 despite that the ACS and USPSTF both had an age-50 recommendation regarding annual mammograms for much of the early part of the sample.

In Figure 2 we show the trend from 1987 to 2000 for past year mammography use. We present trends for four age groups: 25 to 34 year olds (who were almost never treated by mammography mandates), 35 to 39 year olds (who were usually targeted in provisions calling for baseline mammograms), 40 to 49 year olds (who were usually targeted in provisions calling for biennial mammograms), and 50 to 64 year olds (who were usually targeted in provisions calling for annual mammograms). Several features are notable in Figure 2. First, there was almost no improvement in recent mammography for women age 25 to 34 years old; this fact is rarely reported in the public health

³¹ Baker and Chan (2007) do not find any relationship between direct access laws and mammography use among women age 40–64 using data from the 1996–2000 BRFSS. The NBCCEDP was created by the 1990 Breast and Cervical Cancer Mortality Prevention Act. This program provides federal funds for cancer screening of low-income uninsured women, and states began participating at various times from 1991–1996. Adams et al. (2003, 2006) find a positive and significant relationship between the age of a state's NBCCEDP program and rates of past two year mammography among women age 40–64.

literature since women over 40 are typically the subject of these studies. Second, there was noticeable improvement in recent mammography for 35 to 39 year old women from 1987 to until about 1993, after which the rates fell substantially; this is likely attributable to the removal of the “baseline” screening mammogram recommendation from the American Cancer Society Guidelines in 1992. Third, there were steady, long-lasting, and remarkably large increases in mammography use for the two older groups of women: 40 to 49 year olds and 50 to 64 year olds. These facts have been largely documented in previous research: past year mammography rates among both groups of older women roughly doubled over this period 1987 to 2000. These patterns in Figures 2 are visually consistent with a role for mammography mandates in increased mammography use: note that the majority of the legislative action regarding mammography occurred in the 1987–1992 period only for women age 35 and older; indeed, these age groups all saw increases in mammography over this time period.

Table 1 presents descriptive statistics of the key demographic variables as well as for the other screening outcomes used in this analysis for adult women in the BRFSS. Column 1 presents results for all women, while the remaining columns present associated descriptive statistics for age-specific samples of interest: 25 to 34 year old women, 35 to 39 year old women, 40 to 49 year old women, and 50 to 64 year old women. (As in Figure 2, these age groups reflect the modal laws.) We present basic demographic characteristics (e.g., age, race/ethnicity, education, marital status) as well as the fraction of women in each group who had a Pap test or a clinical breast exam (CBE) in the past year. We continue with mammography screening outcomes and means of key mammography screening policy variables in Table 2.

The patterns of demographic characteristics across groups indicates that most of the sample for each age group is white non-Hispanic, while about ten percent of the sample is black non-Hispanic, and nine percent of the sample is Hispanic. Educational attainment is predictably higher for younger women compared to the women age 50–64. Over two-thirds of the sample is married and over 60 percent is employed. Over 85 percent of women report that they have a health care plan. Nearly one quarter of women reports household incomes greater than \$50,000. Finally, note that other non-mammography screening levels (past year Pap tests and clinical breast exams) are fairly regularly high across age groups—much higher than the associated mammography rates below in Table 2—and show the opposite age patterns (i.e., younger women are more likely to obtain these screenings).

Regarding health outcomes and the policy variables in Table 2, we find that, as seen in Figure 1, mammography rates are strongly increasing with age, and the same is true when we consider whether the woman reports her most recent mammogram was routine. Table 2 also shows that there is a much weaker age gradient for screenings due to problems. We also show in Table 2 the means of our policy variables of interest, and we show two variants of the policy coding. The top variables show means of the contemporaneously coded policies while the bottom variables show means of the “share of the previous year” policies that take into account the reference windows for past year outcomes. These variables illustrate the substantial age-specific variation in the key policy variables of interest. Specifically, note that 25 to 34 year olds are never treated by cover or offer mandates except for a handful of state laws that require annual coverage for all women in the state. The age-specific targeting of the mammography mandates is

particularly evident in Table 2: note that 35 to 39 year olds are the only group (compared to women in other age groups) to be treated by cover and offer mandates regarding baseline screening mammograms, 40 to 49 year olds are the only group (compared to women in other age groups) to be treated by cover and offer mandates regarding biennial screening mammograms, and 50 to 64 year olds are the most likely to be treated by cover and offer mandates regarding annual screening mammograms. Note also that there is some additional age-specific variation with respect to the mandates induced by the fact that a handful of mandates differ from the modal age-based laws. This can be seen in the fact that some small but meaningful fraction of 35 to 39 year olds are also treated by annual cover and offer mandates. This is particularly true for 40 to 49 year olds, many of whom are treated by annual cover and offer mandates in the later years of the sample period when several states moved their age of eligibility for annual mammograms from 50 to 40 in response to changes in the cancer screening guidelines of some major medical organizations.

We present the first set of results in Table 3 for the Mammogram in the Past Year outcome. We present coefficient estimates on the key mandate variables of interest, and in each column we add successively more controls. Column 1 shows the raw association net of age group dummies, Pap screening mandates, NBCCEDP programs, and direct access laws. Column 2 adds individual demographic characteristics. Column 3 adds the state economic and demographic variables, as well as the remaining policies in the Z vector (e.g., welfare reform). Column 4 adds state, year, and month fixed effects and is akin to the standard difference in differences approach that relies on the staggered timing

of policy adoption. Column 5 adds state by age group, year by age group, and state by year fixed effects and is the fully saturated DDD model.

The first column of Table 3 shows results for the model that includes only the policy variables directly related to mammography screening, Pap screening, NBCCEDP programs, direct access laws, and age group indicators; we report coefficients only for the mandate variables. The results in column 1 of Table 3 indicate that there is a strong raw association between most of the mandates and the probability that a woman age 25–64 reports having had a mammogram in the past year. For example, the presence of cover mandate for annual mammography is associated with a 4.9 percentage point increase in the probability of past year mammography screening. In the second and third columns we find that these relationships are largely unchanged when we control for individual and state demographic characteristics and other state policies. In column 4 we control for unrestricted state and year fixed effects, and the magnitudes of the coefficient estimates fall substantially for every policy variable, though nearly all remain statistical significant at conventional levels.³²

Turning to our preferred augmented DDD model in column 5 with a full set of two-way interactions for age, state, and year, we continue to find that cover mandates for annual mammography significantly increase the probability of having had a mammogram in the past year by 1.5 percentage points, or about 8 percent of the baseline annual

³² Note that the point estimates on the offer mandates are generally larger than the point estimates on the associated cover mandates in Columns 1 through 4 of Table 3. Because of the much smaller number of states with offer mandates, however, the standard errors on the offer mandate coefficients are also much larger than those on the cover mandates, and none of the differences between offer and cover mandates is statistically significant. As we discuss below, the offer mandate estimates are also more sensitive to specification and choice of outcome variable than our main result for annual cover mandates. Finally, because such a small number of states implement offer mandates, we suspect some of these significant findings are due to the type of over-rejection possible in state-year panel estimation with a small number of policy changes (e.g., Conley and Taber, 2008).

mammography rate.³³ To get a sense of the true effect size of the annual cover mandate, one should weight up the estimate to account for the fact that only about a third of women in the BRFSS could have been directly treated by the mandate (i.e., privately insured women whose insurance is not subject to ERISA exemptions) (Butler 2000). The true effect size of an annual cover mandate on past year mammography rates, then, is closer to 4.5 percentage points. Given that past year mammography rates increased by about 22.4 percentage points over our time period (see Figure 2), we estimate that annual cover mandates account for about seven percent of the overall increase ($1.5/22.4=6.7$). The other coefficients on the mammography mandates are also substantially smaller and generally insignificant in the DDD specification.³⁴

In all subsequent models for mammograms we only report results from our preferred triple difference specification that includes the full set of age group, state, and year fixed effects and their two-way interactions.³⁵ These results are shown in Table 4 and confirm that mammography mandates are significantly related to past two-year and lifetime mammography rates, as well as the stated reasons for obtaining a mammogram. Specifically, we estimate that cover mandates for annual mammography screening increase past two-year mammography rates by approximately 2.1 percentage points, and this effect is statistically significant. As in the past year mammography model in Table 3,

³³ Also, the main results are essentially unchanged when we control for single year of age dummies (instead of 5-year age group dummies) and their associated interactions with each state and year fixed effect. For the past year mammogram outcome, for example, the coefficient estimate on the annual cover mandate variable is .015 with a standard error of .006 (versus an estimate of .015 with a standard error of .005 in the main specification).

³⁴ Although we do find a positive and marginally significant relationship between biennial offer mandates and past year mammography rates, this estimate—and all the estimates on the offer mandate variables—are based on a very small number of state policy changes (see the bottom rows of Table 2) and are therefore prone to over-rejection problems (Conley and Taber 2005). That the point estimates in column 4 are uniformly smaller than those in column 3 suggests that previous cross-sectional studies of mammography mandates have likely overstated their effects.

³⁵ Results from the less saturated models generally produced larger associations and are available upon request.

the other mandate coefficients are generally positive but not individually statistically distinguishable from zero. In column 2 we estimate that mammography cover mandates for biennial and annual screening are estimated to significantly increase lifetime mammography use by 1.6 and 1.3 percentage points, respectively. Finally, columns 3 and 4 of Table 4 suggest that baseline mammography mandates increased the likelihood that a woman reports she received her most recent mammogram as part of a routine checkup and decreased the likelihood she reports the most recent mammogram was due to a specific problem.³⁶

In Table 5 we provide more direct evidence on the most likely mechanism through which mandates affect utilization: an insurance coverage channel. Specifically, we begin by estimating a triple difference model where the outcome variable is an indicator for whether the woman currently has any health plan. This is the closest variable we have to representing health insurance coverage. Recall that one possible employer response to rising costs of state mandates is to reduce offers of health insurance to employees; as such, it is possible that mandates such as those we study here could reduce health insurance coverage (though we have argued that this is unlikely given the age-specific nature of the benefits and our empirical models). In column 1 of Table 5 we show that biennial and annual breast cancer insurance mandates are not meaningfully associated with changes in health insurance coverage of women.³⁷ In column 2 we show

³⁶ We also investigated the robustness of the main results for mammography to address concerns about the unbalanced panel. Results from a balanced panel for all states observed continuously from 1987–2000; an alternative balanced panel for all states observed in 1987 and then continuously from 1989–2000 (taking into account that the questions were only asked of a small number of women in 1988); and a balanced panel of all states observed continuously from 1989–2000 produced similar estimates to those reported in Tables 3 and 4 and confirmed that mammography mandates increased past year mammography rates. These results are available upon request.

³⁷ The one exception is the findings that cover mandates for baseline mammograms are estimated to significantly increase the likelihood of reporting a health plan. Note first that this estimate is not negative,

that among insured women, there are statistically significant utilization effects of cover mandates for annual mammograms on past year mammography rates. As expected, we do not find that mandates significantly increased utilization rates among uninsured women in column 3.

In Table 6 we examine whether mandates affected other related screening behaviors by women that are also related to preventive health. Specifically, we consider Pap tests (the standard cervical cancer screening tests) and clinical breast exams (manual examinations of the breast performed by a physician that do not involve X-rays and presumably could be done during an office visit). If mandates were significantly related to women's health more generally (particularly in an age-specific way), we might be less convinced that the effects we have identified are really due to the effects of the insurance mandates and may instead be proxying for other types of outreach efforts or information campaigns regarding women's preventive health behaviors other than mammography screening for breast cancer.

We first show that the relationship between annual cover mandates and past year mammography is robust to restricting attention to the sampled years in which we observe the other outcomes: in column 1, for example, we show that 1988–2000 (when questions about Pap tests were asked), all types of cover mandates significantly increased past year mammography rates, and in column 3 we show that the main finding for annual coverage mandates is similarly robust for the period 1990–2000 (when clinical breast exam

which is what one might be concerned about if mandates induce firms to reduce offers of health insurance. More importantly, the estimated relationship between annual cover mandates and health plans is very small and statistically indistinguishable from zero. Since our mammography findings in the DDD models of Tables 3 and 4 are driven by the annual cover mandates, this null finding between annual cover mandates and health plans is particularly important as it suggests that the most robust utilization effects we identify are unlikely to be caused by changes in health plan coverage per se.

questions were asked).³⁸ We then directly show that the relationship between annual cover mammography mandates and past year screenings is unique to mammography. Specifically, in columns 2 and 4 we show that neither past-year Pap tests nor past-year clinical breast exams were significantly related to mammography mandates requiring insurers to cover baseline, biennial, or annual mammograms; i.e., the breast cancer mandate effects are unique to mammography, which further supports the hypothesis that mandates affected insurance coverage for mammography only and subsequently affected utilization.

Finally, in Table 7 we provide descriptive evidence on the effects of mandates for various subgroups of women in the fully saturated DDD models. Column 1 shows that among white women there were large and statistically significant increases in past year mammography rates associated with annual cover mandates. Coefficient estimates also indicate meaningful increases in past year mammography associated with annual cover mandates for non-Hispanic black and Hispanic women, though these estimates are not statistically precise given the smaller sample sizes. In columns 4 through 7 we find that the annual cover mandates are estimated to increase past year mammography rates substantially for the least and most educated groups, though again the estimates are not individually statistically significant at conventional levels.³⁹

7. Conclusion

³⁸ All of the models in Table 6 include the full set of controls in the triple difference specification.

³⁹ In Tables 5 and 7 we find a handful of statistically significant (and often wrong-signed) coefficients for offer mandates, though the very small number of states driving identification of these coefficients (as illustrated in the means of the policy variables in the bottom rows of Table 2) makes these findings subject to over-rejection concerns. Moreover, the magnitude of the significant estimates is implausibly large.

The results above suggest that state laws requiring private insurers to cover annual screening mammograms played an important role at increasing the rates of past year mammography over an unprecedented period of improved preventive health behaviors among women from 1987 to 2000. Specifically, we estimate that mammography mandates account for about 7 percent of the overall doubling of the annual mammography rate among 25–64 year old women over this time period. What factors account for the remainder of the increase? Several possibilities are likely. First, previous research has identified direct provision of mammograms to low-income women through the National Breast and Cervical Cancer Early Detection Program (which expanded greatly over our time period) as significant determinant of mammography use (Adams et al. 2006, Adams et al. 2003).⁴⁰ Second, the adoption of screening guidelines from the American Cancer Society and the United States Preventive Services Task Force were likely responsible for some of the secular age-specific increases in use from both a patient and provider perspective. Finally, educational outreach about the lifesaving effects of mammography is likely important. To the extent these efforts were correlated with mammography mandate adoption (as is plausible), these effects are likely reflected in part by the much larger associations between mammography mandates and mammography use we identify in our state and year fixed effects models in Column 4 of Table 3 (i.e., without the age by state, age by year, and state by year fixed effects). For example, annual cover mandates in the DD models were estimated to increase past year mammography rates by 3.6 percentage points, or an effect three times as large as our

⁴⁰ These programs were also targeted to women over age 40, and throughout we have controlled for the differential timing of statewide implementation of pilot and full NBCCEDP efforts. We did not find consistent evidence that these programs increased mammography rates, though we do not have good measures of the intensive margin of service provision across states.

preferred estimate. Part of this additional association likely reflects state specific mammography outreach correlated with mandate adoption. As such, while we are able to isolate a meaningful effect of annual cover mandates on past year mammography use, there are also clearly other factors responsible for the overall increase observed over the 1990s.

Given that nearly all states have already adopted these public policies, what are the public policy implications of our study? One important feature of mammography mandates is that there is still wide variation in the ages of women who are targeted by these laws. Moreover, most states' existing recommendations are *not* in accordance with current recommendations from the American Cancer Society or the United States Preventive Services Task Force. Specifically, the majority of state mandates still cover annual screening mammograms for women age 50 and older, despite that most major medical organizations now recommend annual mammograms for women beginning at age 40. As such, our results suggest that there remains substantial latitude for public policy to increase screening rates. As states are now considering benefits mandates for other cancer screenings such as prostate and colon cancer, our findings also have implications for effective policy design.

Overall these results significantly advance our understanding of one of the more remarkable public health improvements of the past several decades. An important remaining question is whether increased rates of mammography screening are welfare enhancing for all women. Recent research has raised potentially important concerns about the appropriateness of routine mammography, particularly for younger women. In

future work we will use other data sources to more comprehensively evaluate the effects of these public policies on other health and labor market outcomes.

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Figure 1
BRFSS 1987-2000

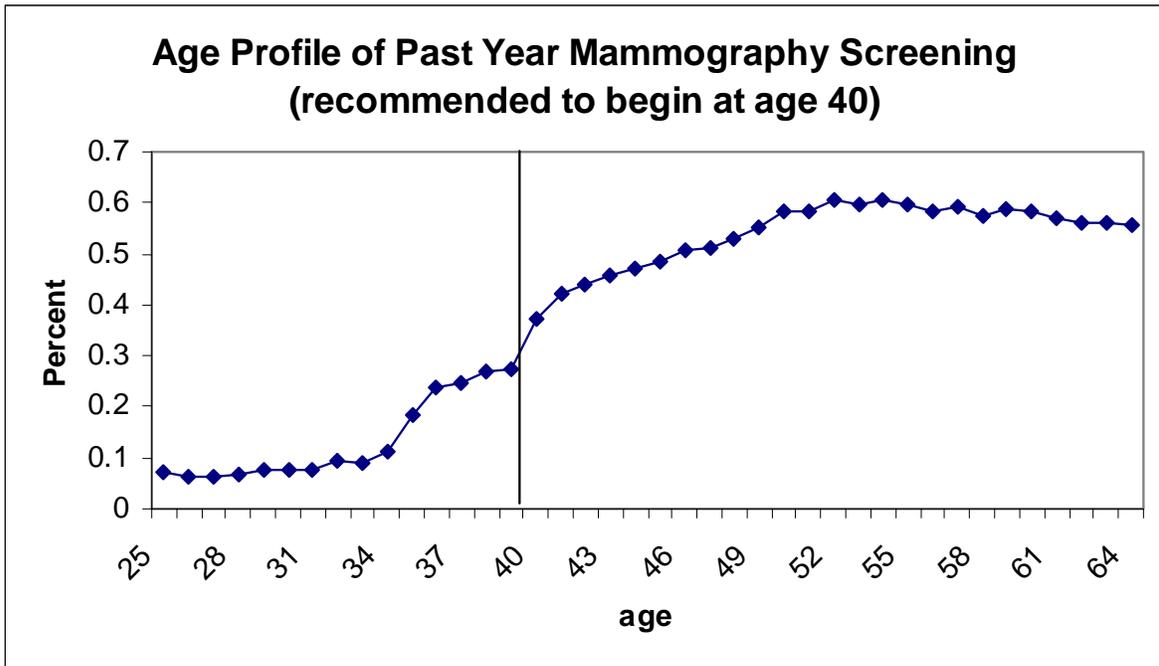


Figure 2
BRFSS 1987-2000

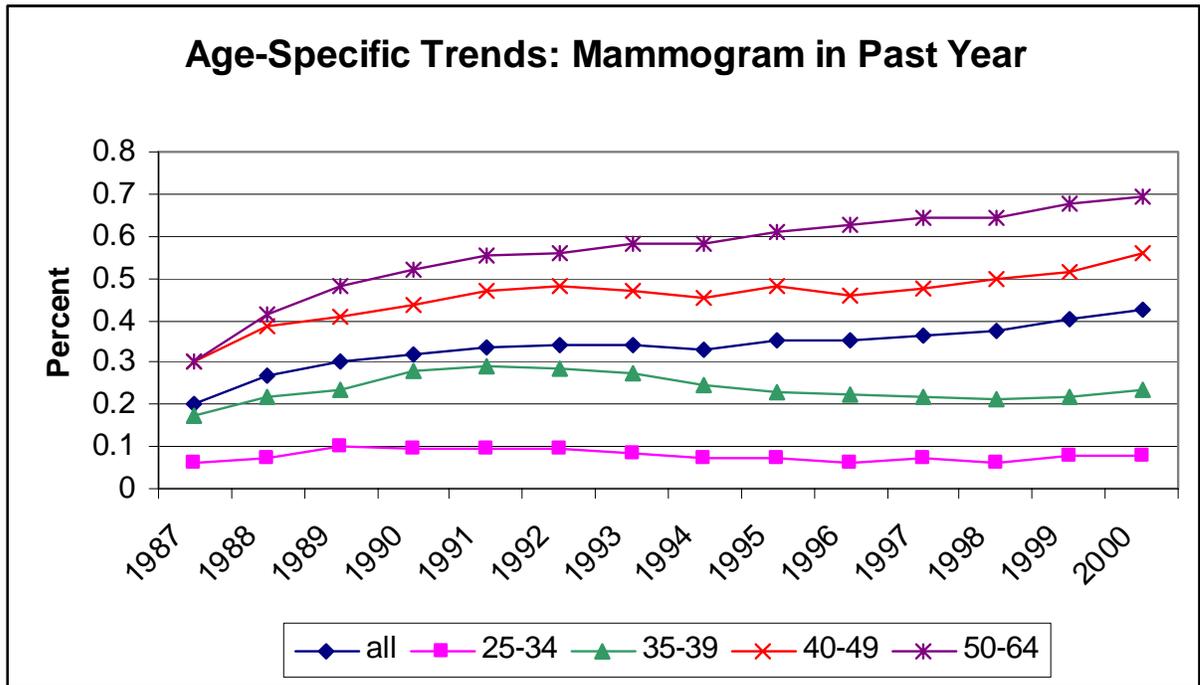


Table 1
Descriptive Statistics, BRFSS Females

Variable	All 25–64	25–34	35–39	40–49	50–64
White non-Hispanic	.762	.722	.749	.769	.806
Black non-Hispanic	.105	.114	1.06	.104	.095
Other race non-Hispanic	.035	.040	.037	.036	.027
Hispanic	.094	.121	.105	.086	.068
Less than high school degree	.118	.093	.093	.099	.177
HS degree	.339	.315	.319	.328	.386
Some college	.272	.290	.289	.281	.235
Bachelors degree or more	.270	.301	.298	.290	.199
Married	.679	.636	.710	.709	.681
Widowed/Divorced/Separated	.195	.121	.169	.212	.275
Never married	.103	.204	.095	.061	.037
Living with a partner	.021	.038	.023	.015	.005
Employed	.602	.641	.647	.669	.467
Unemployed	.051	.059	.051	.050	.044
Not in labor force	.274	.243	.222	.195	.414
Has any health care plan (1990-00)	.859	.824	.856	.874	.882
HH income <35K	.463	.523	.431	.394	.482
HH income 35-50K	.180	.189	.198	.192	.150
HH income > 50K	.240	.202	.277	.304	.201
HH income missing	.116	.086	.094	.110	.167
Had Pap test last year (from 1988)	.695	.774	.703	.674	.624
Had clinical breast exam last year (from 1990)	.697	.723	.680	.683	.691
N	593737	170352	97610	162580	163195

Notes: Author calculations from 1987–2000 BRFSS adult females 25–64 who completed interviews by December 2000. Some of the variables are not defined in some of the years (e.g., health insurance is not asked until 1990). Statistics are weighted. Between 0.1% and 0.3% of observations are missing values for education, marital status, employment status, or health insurance. A larger share is missing household income. Questions about Pap tests and clinical breast exams not asked for all years and all states, and thus are reported for a smaller number of observations than the reported N. The NBCCEDP program for mammograms is targeted to women 40 and older, so the NBCCEDP variables are set to zero for women 25-39 for mammogram outcomes.

Table 2
Mammogram Outcomes and Mandate Variables, BRFSS Females

Variable	All 25–64	25–34	35–39	40–49	50–64
Ever had a mammogram	.550	.174	.459	.760	.817
Had a mammogram w/in 1 year	.346	.080	.241	.469	.583
Had a mammogram w/in 2 years	.451	.115	.344	.636	.709
Reason last mammogram: routine	.467	.111	.357	.666	.733
Reason last mammogram: problem	.073	.060	.098	.084	.063
Reason last mammogram: cancer	.008	.001	.003	.008	.019
Means of policy variables for contemporaneous outcomes:					
Treated by cover mandate (baseline mammogram)	.088	0	.563	0	0
Treated by offer mandate (baseline mammogram)	.013	0	.084	0	0
Treated by cover mandate (biennial mammogram)	.130	0	0	.492	0
Treated by offer mandate (biennial mammogram)	.014	0	0	.053	0
Treated by cover mandate (annual mammogram)	.259	.008	.077	.195	.704
Treated by offer mandate (annual mammogram)	.033	0	.001	.034	.086
Means of policy variables for past year outcomes:					
Share treated by cover mandate (baseline mammogram)	.084	0	.541	0	0
Share treated by offer mandate (baseline mammogram)	.014	0	.088	0	0
Share treated by cover mandate (biennial mammogram)	.126	0	0	.476	0
Share treated by offer mandate (biennial mammogram)	.015	0	0	.057	0
Share treated by cover mandate (annual mammogram)	.249	.008	.076	.187	.647
Share treated by offer mandate (annual mammogram)	.033	0	.001	.033	.084
N	593737	170352	97610	162580	163195

Notes: Author calculations from 1987–2000 BRFSS adult females 25–64. Statistics are weighted. N is maximum possible N; a small number of observations are missing for various measures (e.g., individuals who did not answer questions about the timing of their last mammogram are not asked why they had it). Past year outcomes are the share of the prior calendar year (relative to the respondent’s interview date) that a law has been in effect, assuming it first impacted health insurance policies as of January 1 of the year after it was passed.

Table 3:
Insurance Mandates Requiring Coverage of an Annual Mammogram Significantly Increased Past Year Mammography
BRFSS 1987-2000, Adult Women 25-64, Incremental Controls

	(1) No controls (except for age group dummies and policies related to screening/access to OB/GYNs)	(2) + Individual X's	(3) + State X's and relevant public policies in Z vector	(4) + State, year, and month fixed effects	(5) + State x age, year x age, and state x year fixed effects (Fully Saturated DDD Model)
Treated by cover mandate (baseline mamm.)	.023** (.010)	.022** (.009)	.015** (.007)	.006 (.007)	-.008 (.009)
Treated by offer mandate (baseline mamm.)	.040** (.012)	.039*** (.011)	.031*** (.008)	.021** (.010)	.006 (.013)
Treated by cover mandate (biennial mamm.)	.041*** (.010)	.040*** (.010)	.039*** (.009)	.020** (.009)	.013 (.011)
Treated by offer mandate (biennial mamm.)	.044* (.023)	.045** (.021)	.050** (.019)	.037** (.016)	.019* (.011)
Treated by cover mandate (annual mamm.)	.049*** (.017)	.048*** (.016)	.048*** (.012)	.036*** (.012)	.015*** (.005)
Treated by offer mandate (annual mamm.)	.063*** (.016)	.063*** (.015)	.057*** (.011)	.038*** (.012)	.009 (.015)
Adjusted R squared	.20	.21	.21	.21	.22
N	591170	591170	591170	591170	591170

Notes: Each column shows selected coefficients from one regression. Mandate variables control for share of last calendar year law was in effect. In addition to controls for which coefficients are reported, additional controls are included as indicated in the column label. Age groups dummies for being 30–34, 35–39, 40–44, 45–49, 50–54, 55–59, and 60–64 included in all regressions as are controls for Pap test mandates, NBCEDPP pilot and full programs, and laws mandating access to OB/GYNs. Individual Xs added in column 2 include controls for race/ethnicity, education, and marital status. Column 3 adds controls for the following variables for each state and year: share of women 15–44 with private health insurance; share of women who work or who have a husband who works at a firm with 24 or fewer employees, 25–99 employees or 100 or more employees; the unemployment rate; welfare reform; the level of HMO penetration (as a share of the population); the number of obstetric beds per 100 women 15–44, the eligibility threshold for Medicaid eligibility for a pregnant woman in the state as a share of the FPL; and the share urban, share black, and share Hispanic in the state. Column 4 adds state, year, and month of interview fixed effects. Column 5 adds state by age group, year by age group, and state by year fixed effects. * significant at 10%; ** significant at 5%; *** significant at 1%. Standard errors throughout are clustered at the state level and estimates are weighted.

Table 4:
Mammography Insurance Mandates and Other Mammography Screening Outcomes
BRFSS 1987-2000, Adult Women 25-64, DDD Models

	(1) Mammogram in past two years	(2) Ever had a mammogram	(3) Last mammogram: routine	(4) Last mammogram: problem
<i>Treatment over relevant exposure window for the outcomes in columns 1-4:</i>				
Treated by cover mandate (baseline mamm.)	.008 (.010)	.003 (.008)	.020** (.009)	-.017*** (.005)
Treated by offer mandate (baseline mamm.)	.025 (.019)	.028** (.011)	.032*** (.008)	-.006 (.006)
Treated by cover mandate (biennial mamm.)	.019 (.013)	.016** (.007)	.012* (.007)	.002 (.005)
Treated by offer mandate (biennial mamm.)	.006 (.023)	-.001 (.012)	-.000 (.018)	-.002 (.008)
Treated by cover mandate (annual mamm.)	.021*** (.007)	.013** (.006)	.010 (.007)	.002 (.006)
Treated by offer mandate (annual mamm.)	.009 (.014)	-.002 (.014)	-.006 (.014)	.003 (.005)
Adjusted R squared	.29	.34	.33	.01
N	591170	592468	590834	590834

Notes: Each column shows the results from a separate DDD regression model with the specification in column 5 of Table 3 but a different dependent variable. Relevant mandate variables for specifications for column 1 control for share of last two calendar years the law was in effect. Relevant mandates variables for specifications for columns 2, 3, and 4 control for whether a mandate has been implemented as of January of this year. See notes to Table 3 for description of additional control variables. * significant at 10%; ** significant at 5%; *** significant at 1%. Standard errors throughout are clustered at the state level and estimates are weighted.

Table 5:
Mandate Effects on Past Year Mammography Driven by Insured Women
BRFSS 1987–2000, Adult Women 25–64, DDD

Outcome is →	(1) Insured	(2) Mammogram in past year, among insured	(3) Mammogram in past year, among uninsured
Treated by cover mandate (baseline mamm.)	.013** (.006)	.003 (.011)	-.011 (.029)
Treated by offer mandate (baseline mamm.)	-.017** (.008)	-.007 (.015)	.109** (.049)
Treated by cover mandate (biennial mamm.)	.002 (.007)	.008 (.011)	-.034* (.018)
Treated by offer mandate (biennial mamm.)	-.006 (.006)	.018*** (.006)	.062* (.035)
Treated by cover mandate (annual mamm.)	.004 (.009)	.028*** (.010)	-.017 (.022)
Treated by offer mandate (annual mamm.)	-.006 (.013)	.004 (.015)	.003 (.023)
Adjusted R squared	.11	.25	.13
N	507647	436086	68101

Notes: Each column shows the results from a separate DDD regression model with the specification in column 5 of Table 3 but a different dependent variable. Mandate controls are for share of last year mandate was in effect. Sample sizes in columns 2 and 3 do not exactly equal the sample size in column 1 because we do not exclude the small number of women with missing data on past year mammography in the model in column 1. See notes to Table 3 for list of additional control variables. * significant at 10%; ** significant at 5%; *** significant at 1%. Standard errors throughout are clustered at the state level and estimates are weighted.

Table 6:
Mammography Mandates Did Not Affect Pap Tests or Clinical Breast Exams (CBE)
BRFSS 1987–2000, Adult Women 25–64, DDD Models

	(1)	(2)	(3)	(4)
Outcome is →	Mammogram in past year	Pap test in past year	Mammogram in past year	Clinical breast exam in past year
Sample is →	1988–2000 (when Pap test questions asked)	1988–2000 (when Pap test questions asked)	1990–2000 (when CBE questions asked)	1990–2000 (when CBE questions asked)
Treated by cover mandate (baseline mamm.)	-.001 (.008)	-.010 (.011)	-.005 (.012)	-.013 (.012)
Treated by offer mandate (baseline mamm.)	.003 (.011)	.016 (.016)	.004 (.011)	.023*** (.008)
Treated by cover mandate (biennial mamm.)	.012 (.010)	-.014 (.008)	.006 (.010)	-.006 (.007)
Treated by offer mandate (biennial mamm.)	.019* (.011)	.039*** (.009)	.017 (.011)	.024 (.018)
Treated by cover mandate (annual mamm.)	.018*** (.006)	.001 (.012)	.018** (.007)	.003 (.008)
Treated by offer mandate (annual mamm.)	.006 (.014)	.024 (.016)	.004 (.008)	.025* (.014)
Adjusted R squared	.22	.05	.23	.05
N	571817	539200	535890	534242

Notes: Each column shows the results from a separate DDD regression model with the specification in column 5 of Table 3 but estimated for a different sample. The sample in Columns 1 and 2 includes the set of states and years in which questions about Pap tests were asked. The sample in Columns 3 and 4 includes the set of states and years in which questions about clinical breast exam were asked. See notes to Table 3 for list of additional control variables. * significant at 10%; ** significant at 5%; *** significant at 1%. Standard errors throughout are clustered at the state level and estimates are weighted.

**Table 7:
Results by Group: Mammography in Past Year
BRFSS 1987-2000, Adult Women 25-64, DDD Models**

	(1) White, non- Hispanic	(2) Black, non- Hispanic	(3) Hispanic	(4) Less than high school degree	(5) High school degree	(6) Some college	(7) College degree or more
Treated by cover mandate (baseline mamm.)	-.002 (.009)	-.030 (.032)	-.034 (.037)	-.026 (.023)	-.023 (.020)	.016 (.013)	-.014 (.015)
Treated by offer mandate (baseline mamm.)	.006 (.017)	.012 (.031)	.149*** (.039)	.077** (.030)	.009 (.019)	.015 (.025)	-.033 (.029)
Treated by cover mandate (biennial mamm.)	.016 (.009)	.050* (.029)	.008 (.047)	.012 (.026)	.029 (.018)	-.002 (.017)	-.006 (.016)
Treated by offer mandate (biennial mamm.)	.012 (.009)	.106*** (.024)	.111 (.170)	.099* (.051)	.009 (.035)	.025 (.019)	-.018 (.013)
Treated by cover mandate (annual mamm.)	.018*** (.006)	.028 (.018)	.047 (.051)	.035 (.022)	.001 (.012)	.005 (.016)	.023 (.019)
Treated by offer mandate (annual mamm.)	.015 (.017)	-.003 (.017)	.025 (.064)	.050* (.028)	.013 (.016)	.009 (.032)	-.020 (.029)
Adjusted R squared	.23	.18	.18	.13	.20	.24	.29
N	473842	58011	34891	59541	197322	168298	165303

Notes: Each column shows the results from a separate DDD regression model with the specification in column 5 of Table 3 but estimated for a different sample. Column 1 sample is non-Hispanic white women; column 2 sample is non-Hispanic black women; and column 3 sample is Hispanic women. Column 4 sample is women with less than a high school degree; column 5 sample is women with exactly a high school degree; column 6 sample is women with some college education; and column 7 sample is women with at least a bachelor's degree. See notes to Table 3 for list of additional control variables. * significant at 10%; ** significant at 5%; *** significant at 1%. Standard errors throughout are clustered at the state level and estimates are weighted.