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PUBLIC HEALTH INSURANCE EXPANSIONS AND HOSPITAL TECHNOLOGY ADOPTION

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ABSTRACT

This paper explores the effects of public health insurance expansions on hospitals' decisions to adopt medical technology. Specifically, we test whether the expansion of Medicaid eligibility for pregnant women during the 1980s and 1990s affects hospitals' decisions to adopt neonatal intensive care units (NICUs). While the Medicaid expansion provided new insurance to a substantial number of pregnant women, prior literature also finds that some newly insured women would otherwise have been covered by more generously reimbursed private sources. This leads to a theoretically ambiguous net effect of Medicaid expansion on a hospital's incentive to invest in technology. Using American Hospital Association data, we find that on average, Medicaid expansion has no statistically significant effect on NICU adoption. However, we find that in geographic areas where more of the newly Medicaid-insured may have come from the privately insured population, Medicaid expansion slows NICU adoption. This holds true particularly when Medicaid payment rates are very low relative to private payment rates. This finding is consistent with prior evidence on reduced NICU adoption from increased managed-care penetration. We conclude by providing suggestive evidence on the health impacts of this deceleration of NICU diffusion, and by discussing the policy implications of our work for insurance expansions associated with the Affordable Care Act.

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1. Introduction

The development and diffusion of medical technology have been primary drivers of health care cost growth during recent decades (Newhouse 1992; Fuchs 1996; Cutler 2005). In addition, dramatic changes have occurred in the U.S. health insurance landscape since the 1960s, including growth in private insurance, the introduction of Medicare and Medicaid, and subsequent expansions of the Medicaid program. Weisbrod (1991) argues that technology advancement and insurance expansion are intimately linked: the availability of expensive, lifesaving technology increases consumer demand for health insurance, and the ability of patients to pay for expensive medical care with insurance increases the incentives to produce and adopt new medical technology. Given this theoretical endogeneity, empirically identifying the effect of one on the other is difficult. Finkelstein (2007) finds convincing evidence that the introduction of Medicare drastically increased hospital spending above and beyond what would be predicted using the patient-demand elasticities implied by the RAND Health Insurance Experiment, suggesting additional supply-side responses on the part of hospitals. She also presents evidence that some of this additional supply-side response may be due to technology adoption by hospitals, but this finding is only suggestive due to data limitations during the period she studies.[1]

In this paper we exploit exogenous changes in Medicaid eligibility to directly estimate the causal effect of the availability of public health insurance on technology adoption by hospitals. We provide evidence from a period of substantial Medicaid eligibility expansion among low-income pregnant women by examining hospitals' decisions to adopt a form of technology that appeals directly to patients undergoing childbirth: neonatal intensive care units (NICUs).

In addition to providing the first direct empirical evidence of hospital technology adoption reactions to public health insurance expansions in the U.S., studying supply-side responses to Medicaid expansions fills an important gap in the literature. Medicaid covered about 20 percent of all Americans in 2010 and accounted for about 15 percent of national health care spending (DeNavas, Proctor, and Smith

[1] In another setting, Chou, Liu, and Hammitt (2004) find that the introduction of national health insurance in Taiwan also increased hospitals' adoption of medical technology.

2012; Kaiser 2012). Expansions of the Medicaid program under the Affordable Care Act (ACA) are expected to result in an additional 13 million enrollees by 2024 (Congressional Budget Office 2014). These reasons make it important to understand how the supply-side responds to Medicaid expansions, especially as Medicaid pays providers much less than Medicare does (Norton and Zuckerman 2000). Supply-side reactions to Medicaid expansions may differ substantially from responses to Medicare. In addition, while the Medicaid expansions we study insured a substantial number of otherwise-uninsured pregnant women, some newly covered women came from the rolls of private sources that are more generous in their reimbursements.[2] Dave at al. (2011) and Dubay and Kenney (1997) both find private insurance crowd-out estimates close to 50 percent for groups of pregnant women affected by Medicaid expansions. Due to crowd-out, the net effect of Medicaid expansions on hospitals' incentives to invest in technology is ambiguous.

We proceed in this paper by first developing an analytical model to illustrate how incumbent hospitals' NICU adoption decisions might change following Medicaid eligibility expansions, considering both the insurance-expansion effect and the Medicaid crowd-out effect. We then draw guidance from this model to help us empirically distinguish between these two effects based on how the eligibility expansions interact with local market characteristics, such as the pre-expansion insurance rate and reimbursement ratios between private and Medicaid patients.

We use data for 1985 to 1996 from the American Hospital Association (AHA), a source covering virtually all hospitals in the U.S., and consider changes in Medicaid eligibility over time and across states for pregnant women. We find that on average, state-level Medicaid expansions have no effect on the likelihood that a hospital offers a NICU. However, we find that Medicaid expansions decelerate NICU diffusion in areas where we expect crowd-out to be strongest. We also find that this result of slowed NICU diffusion is driven by states in which Medicaid reimbursement generosity is lowest as compared to private-pay rates. These findings are consistent with our prior that the crowd-out of public coverage in

[2] It is important to note that while Medicaid is less generous than private insurers from the provider perspective (Currie, Gruber, and Fischer 1995), Medicaid imposes lower cost sharing for pregnant patients than private insurance does. As Gruber (2008) puts it, Medicaid "provides the best insurance money can't buy."

these states has the largest negative impact on incentives to profitably invest in technology designed to attract maternal deliveries. Although it is reasonable to expect increased NICU adoption in areas where the insurance-expansion effect is likely to dominate the Medicaid crowd-out effect, we find little evidence of acceleration. One possible explanation for this finding is that Medicaid payment rates are still too low compared to private rates to incentivize adoption in these markets.

We conclude our paper by discussing the policy implications of our findings. Because current Medicaid expansions are not identical in nature to these past expansions, our findings also underline the need to continue studying the impact of insurance expansions on technology adoption under the ACA.

2. Background

A. Previous Literature

The literature on the supply-side responses to public insurance expansions is relatively sparse compared to the literature that estimates the demand-side responses to expansions. Except for Finkelstein's (2007) examination of hospital expenditures, the supply-side literature has focused on innovation rather than provider-level decisions (Acemoglu et al. 2006; Blume-Kohout and Sood 2008; Finkelstein 2004; Clemens 2012). While these studies typically find that insurance expansions spur innovation overall, Hult and Philipson (2012) point out that returns to innovation should be non-monotonic as public insurance eligibility expands through the income distribution. Eligibility expansions at higher levels of income are less likely to impact demand and are likely to result in lower payment rates to providers than previous sources of payment held by higher-income individuals. While the Medicaid expansions we study still affect relatively low-income populations, if Medicaid replaces higher-reimbursing private coverage for some individuals, the expansion may have a limited or even negative net impact on hospitals' incentives to invest in new technologies, as with innovation.

In terms of provider behavior, a set of papers has examined the impact of Medicaid *fees* on the amount of time physicians spend with patients and on the setting in which care occurs (Decker 2007, 2009), but we are aware of only one paper that studies supply-side responses to *eligibility expansions*:

Garthwaite (2012) studies physicians' labor-supply responses to Children's Health Insurance Program (CHIP) expansions. Our paper is the first to examine the effect of eligibility expansions and fees on provider decisions to invest in technology adoption.

In the area of private-sector insurance, existing studies find the proliferation of managed care to decelerate technology adoption (Baker 2001; Baker and Phibbs 2002). These previous findings are consistent with Weisbrod's (1991) hypothesis that the effect of insurance on technology adoption will depend on how that insurer pays; while Medicare caused a large shift out in the demand curve for medical care, managed care reduced patient and physician incentives for utilizing technology, and health care providers responded by changing their technology adoption decisions. These findings are also consistent with Chandra and Skinner's (2011) model of productivity growth in health care, in which insurance impacts the extent and type of technology diffusion through both patient and provider incentives.

B. The Medicaid Expansions

When Medicaid commenced in 1965, coverage for pregnancy-related costs initially applied only to single mothers receiving welfare benefits. The program then expanded rapidly over the 1980s and 1990s as eligibility for Medicaid was decoupled from welfare eligibility. States were given the option to provide Medicaid for pregnant women under 100 percent of the federal poverty line in 1987 and then up to 185 percent of the federal poverty line in 1988, while still receiving federal matching funds. In 1990 the federal government mandated all states to provide Medicaid to pregnant women in families earning less than 133 percent of the federal poverty line. The percentage of deliveries covered by Medicaid rose from 19.5 percent in 1985 to 35.2 percent in 1996, while uninsured deliveries fell from 13.7 percent to 4.1 percent (Dave et al. 2011).

While these Medicaid expansions extended coverage to many previously uninsured individuals, about a quarter to a half of the increased Medicaid coverage has been attributed to crowd-out of private coverage (Cutler and Gruber 1996; Congressional Budget Office 2007). Dave et al. (2011) and Dubay and Kenney (1997) find crowd-out rates of 45 to 55 percent for marginal pregnant women made eligible by these expansions.

C. Neonatal Intensive Care

Neonatal intensive care is an important policy context for examining technology adoption during the 1980s and 1990s. It is also a particularly relevant technology to the population newly covered by the Medicaid expansions studied in this paper. Like many medical technologies, NICUs have significantly improved health outcomes on average (Cutler and Meara 2000); however, it is unclear if the recent NICU diffusion has been optimal from the standpoint of maximizing health benefits and minimizing costs. Early development of NICUs was concentrated in large, regional, and typically academic hospitals. In the 1980s and 1990s diffusion greatly accelerated; between 1980 and 1996, the number of NICUs almost tripled, with most of the new units located in smaller, community hospitals (Baker and Phibbs 2002).[3] There is also evidence that NICU diffusion exceeded medical need (Howell et al. 2002), that it has not been correlated with geographic variation in infant health (Goodman et al. 2001), and that it may lead to excessive utilization (Freedman 2012a). Organizations such as the March of Dimes and the American Academy of Pediatrics have repeatedly advocated for a system in which NICU resources are concentrated in larger, regional centers (Committee on Perinatal Health 1976, 1993; Committee on Fetus and Newborn 2004).

In order to understand the potential impact of insurance on providers' NICU adoption decisions, we briefly discuss the two main reasons why hospitals generally choose to open NICUs. First, the existence of a NICU allows hospitals the opportunity to continue treating, rather than transferring, sick infants who would have been born in a hospital regardless of whether the hospital offered a NICU. The profitability of treating infants in a NICU may vary by insurer, but in general NICU reimbursement rates are relatively high (Horwitz 2005; online appendix).

Second, hospitals may adopt medical technologies or services like NICUs as a signal of quality to

[3] Acemoglu and Finkelstein (2008) provide evidence that Medicare incentives—due to the Medicare policy of reimbursing hospitals for capital expenditures retrospectively at cost—may have been one contributing factor in this diffusion, as it left hospitals with wide leeway regarding which costs they attributed to Medicare.

attract patients (Gaynor 2006). While only about 10 percent of babies receive NICU services in any given year (Schwartz, Kellogg, and Muri 2000), the existence of a NICU is an important determinant of patients' hospital choice (Phibbs et al. 1993) because most preterm labor is spontaneous and unpredictable.[4] Furthermore, it is thought that mothers are likely to return to the same hospital for subsequent care of themselves and their families if they have a positive birth experience (Friedman et al. 2002). As a result, providing a NICU may also generate downstream profits in the future.

3. A Simple Model of Insurance Expansion and NICU Adoption

We motivate our empirical analysis with a simple model of technology adoption in response to a Medicaid expansion; from this model, we generate testable empirical predictions. Because we are most interested in the incentives faced by existing hospitals making technology adoption decisions and for the purpose of maintaining the tractability of our model, we focus on incumbent hospitals and abstract away from hospital entry, exit, and merger behaviors. We expect these compositional behaviors to not be directly affected by the rollout of Medicaid expansions due to its limited scope of expanding coverage only to pregnant women; we present empirical evidence to support this assumption in Section 5.

We assume that the number of deliveries of each insurance type performed by a hospital is characterized by $(\alpha_{\mathrm{P}}^{\mathrm{t}}, \alpha_{\mathrm{M}}^{\mathrm{t}}, \alpha_{\mathrm{U}}^{\mathrm{t}})$, where t equals 0 before Medicaid eligibility expansion and 1 after expansion, and the subscripts denote private (P), Medicaid (M), and uninsured (U) patients. Medicaid changes the overall market patient mix by allowing some of the previously uninsured to gain access to Medicaid coverage (coverage expansion) and inducing some of the privately insured to switch to Medicaid (crowd-out); we expect $\alpha_{\mathrm{P}}^{1} \leq \alpha_{\mathrm{P}}^{0}$, $\alpha_{\mathrm{U}}^{1} \leq \alpha_{\mathrm{U}}^{0}$ and $\alpha_{\mathrm{M}}^{1} \geq \alpha_{\mathrm{M}}^{0}$. We assume that privately insured patients are the most profitable to treat and uninsured patients are the least profitable, with the profitability of Medicaid patients falling in between and depending on a state's Medicaid reimbursement

[4] There are a variety of documented correlates of preterm delivery such as tobacco use, nutrition, stress, and demographics, but there is in fact little understanding of what conditions and events can be used to predict and diagnose preterm labor before it occurs (Behrman and Butler 2007).

generosity. Therefore, $\theta_P \geq \theta_M > \theta_U$, where θ denotes the profitability of treating a patient of each insurance type. We simplify our model by assuming that these profitability levels are not altered by Medicaid eligibility expansion itself.

An incumbent hospital decides whether to adopt a NICU if it does not already operate one, knowing that following adoption it would attract $\alpha_P^t (1 + \varepsilon_P)$ privately insured, $\alpha_M^t (1 + \varepsilon_M)$ Medicaid, and $\alpha_U^t (1 + \varepsilon_U)$ uninsured patients, where ε denotes how demand responds to NICU availability and is arguably positive.[5] Based on evidence that demand elasticity with respect to NICU availability is positive but differs by payer (e.g., Phibbs et al. 1993), it is reasonable to assume that $\varepsilon_P \geq \varepsilon_M \geq \varepsilon_U$. NICU adoption in period *t* occurs if the increase in profitability[6] from attracting more patients outweighs the cost of adoption, i.e.

$$\Delta\pi^t = \theta_P * \left(\alpha_P^t \varepsilon_P\right) + \theta_M * \left(\alpha_M^t \varepsilon_M\right) + \theta_U * (\alpha_U^t \varepsilon_U) \geq FC \quad (1)$$

We also assume that the total number of patients giving birth at a particular hospital does not change after Medicaid expansion, so the increase in the number of new Medicaid patients is equal to the total change in the private patient population and the uninsured patient population, $\alpha_M^1 - \alpha_M^0 = \alpha_P^0 - \alpha_P^1 + \alpha_U^0 - \alpha_U^1$.

To understand how Medicaid eligibility expansions alter hospital incentives, we compare the difference in the net gain associated with NICU adoption before (*t*=0) and after Medicaid expansion (*t*=1):

$$\begin{aligned}\Delta\pi^1 - \Delta\pi^0 &= \theta_P \varepsilon_P (\alpha_P^1 - \alpha_P^0) + \theta_M \varepsilon_M (\alpha_M^1 - \alpha_M^0) + \theta_U \varepsilon_U (\alpha_U^1 - \alpha_U^0) \\ &= -(\theta_P \varepsilon_P - \theta_M \varepsilon_M)\left(\alpha_P^0 - \alpha_P^1\right) + \left(\theta_M \varepsilon_M - \theta_U \varepsilon_U\right)(\alpha_U^0 - \alpha_U^1)\end{aligned} \quad (2)$$

The first expression on the right-hand side measures the decrease in profitability after Medicaid expansions due to the crowd-out effect, and the second expression measures the increase in profits

[5] One implicit assumption that we make here is that hospitals can adjust their nursery bed capacity to meet the increasing demand of pregnant women. Our results also hold if we allow ε_U to be negative, which allows hospitals to avoid uninsured patients and attract more profitable patients (privately insured and Medicaid) after NICU adoption.

[6] We assume that hospitals are purely profit driven, but all model predictions hold if we instead assume an alternative objective function determined by a weighted summation of profits and patient welfare, such as the number of patients that a hospital treats (see Sloan 2000; Horwitz and Nichols 2007).

through the coverage expansion effect. If there is some level of crowd-out, the sign of this expression is ambiguous, suggesting that Medicaid expansion could either slow down or speed up NICU adoption. Assuming for simplicity that the profitability of treating uninsured patients is zero (i.e. $\theta_U = 0$), Medicaid is likely to decelerate NICU adoption (with the above expression being negative) if the following inequality holds:

$$\frac{\theta_M}{\theta_P} < \frac{\alpha_P^0 - \alpha_P^1}{\alpha_U^0 - \alpha_U^1 + \alpha_P^0 - \alpha_P^1} * \frac{\varepsilon_P}{\varepsilon_M} \tag{3}$$

This inequality (3) is more likely to hold under the following conditions:

a. The market initial uninsurance rate, α_U^0, is relatively small.

b. The level of crowd-out, $(\alpha_P^0 - \alpha_P^1)$, is high relative to the full increase in Medicaid eligibility, $(\alpha_U^0 - \alpha_U^1 + \alpha_P^0 - \alpha_P^1)$.

c. The profitability ratio of Medicaid to private patients, $\frac{\theta_M}{\theta_P}$, is relatively small.

As suggested by this model, Medicaid expansion is likely to decelerate NICU adoption when the pre-expansion uninsurance rate is low, or conversely when the rate of insurance is high. The baseline insurance rate could also proxy for the likely level of overall crowd-out in order to incorporate the insight from condition (b). In particular, we hypothesize that in areas with lower pre-expansion insurance rates, Medicaid expansion is more likely to lead to a greater increase in overall insurance coverage, whereas markets with higher insurance coverage would likely see relatively greater numbers of privately insured individuals shift toward Medicaid coverage. The model also predicts that the relative profitability of treating patients of each payer type will play an important role in determining hospital responses to Medicaid expansion; thus we also examine how the impact of expansions and potential crowd-out differ by relative Medicaid fee generosity across states.

4. Data

A. Hospital Data

We test our hypotheses regarding technology adoption using data from the American Hospital Association (AHA) Annual Survey of Hospitals from 1985 through 1996. The AHA surveys all hospitals in the U.S., collecting information on service provisions and location. Our sample includes non-Federal, acute-care, and children's hospitals.[7] As in Baker and Phibbs (2002), in most of our analysis we restrict our sample to hospitals with an obstetrics unit and at least 50 births in the first year of the sample, which could be considered candidates for adopting a NICU; we provide several tests on sample selection.

Within the AHA data, we consider a hospital as having a NICU if it reports having beds in a neonatal intermediate or neonatal intensive care unit.[8] Table 1 lists the number and fraction of hospitals with NICUs in our sample. While the total number of hospitals fell in this time period due to hospital closures and consolidation, the number (percentage) of hospitals with a NICU increased from 589 (14.8%) to 857 (24.8%) between 1985 and 1996.

B. Measuring Medicaid Eligibility

Our empirical approach exploits variation in Medicaid expansions across states and over time to estimate its impact on the likelihood that a hospital adopts a NICU. We cannot use a hospital's actual Medicaid patient load to identify this effect because it is likely correlated with other unmeasured local economic factors that directly affect NICU adoption. In addition, areas with greater Medicaid take-up may also have more need for health care. To overcome these endogeneity concerns, we use a measure of simulated Medicaid eligibility that only varies due to legislative changes and not demographic or health trends. This methodology follows Currie and Gruber (1996), Cutler and Gruber (1996), and many subsequent studies of Medicaid expansions.

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[7] We exclude federally owned hospitals (such as Veterans' hospitals), which are associated with very different payment structures and incentives. Additionally, we exclude long-term care facilities and specialty hospitals, such as cardiac hospitals, which would not be in the set of hospitals treating infants. As an exception to the specialty-care exclusion, we include children's hospitals, since they typically provide neonatal intensive care.

[8] The AHA data are self-reported and therefore have some missing values and inconsistencies from year to year. Specifically, about 6 percent of hospital-year observations have missing values for the number of either neonatal intensive or neonatal intermediate care beds. In the Appendix, we provide details on how we address this problem by imputing missing values based on data from surrounding years.

To construct a simulated eligibility measure for pregnancy-based Medicaid at the state-by-year level, we pool all women between the ages of 18 and 39 from the March Current Population Surveys covering 1985 through 1996 for each state. We then calculate what fraction of these pooled samples would be eligible for Medicaid if they were to become pregnant based on the eligibility rules for each state and year. Because we calculate this simulated measure using the same sample of women for each year, variation over time is only driven by state eligibility rules and not by changes in demographic composition. As shown in Table 1, the fraction of individuals eligible for Medicaid increased from 9.7 percent to 31.0 percent over our sample period.

C. *Local Market-Level Controls*

We also control for time-varying, county-level data on characteristics of a hospital's market using data from the Health Resources and Services Administration's Area Resource File (ARF). These controls include the number of births and low-weight births, the total population, female population by age categories, and per-capita income. We use linear interpolation to fill in values when a measure is not reported for all years. We summarize the mean and standard deviation of these variables in Table 2.

We calculate state-level insurance rates among the population under 65 years of age as our proxy for crowd-out from the March Supplement of the Current Population Survey (CPS).[9] Ideally, we would also exploit variation in insurance rates across sub-state markets; however, no available data measures insurance rates in large enough samples below the state level for this time period. Instead, we use county-level unemployment[10] and county-level fraction of individuals below the federal poverty line as alternative proxies for crowd-out.

D. Medicaid and Private-Fee Data

[9] In results available upon request, our findings are robust to using the insurance rate only among 18- to 39-year-old women.

[10] Baughman (2007) finds that the effect of Medicaid and CHIP expansions on children's health insurance coverage differs by local labor-market conditions; for example, there are larger increases in overall insurance rates in markets with higher unemployment rates.

Unfortunately, comprehensive data on Medicaid hospital reimbursements are not available for the time period of our study.[11] However, we have obtained data used in Currie, Gruber, and Fischer (1995) on state-level ratios of Medicaid-to-private physician fees for total obstetric care of a vaginal birth, including both prenatal care and delivery, over the period 1979-1992. We use these ratios as a proxy for the relative generosity of Medicaid versus private payers for childbirth-related services by state.

In most of our analysis below we utilize the 1985 fee ratio to stratify our sample based on relative Medicaid-to-private reimbursement generosity at the baseline of our sample.[12] On average, the low-ratio states (22 states with fee ratios less than the sample median) have a Medicaid-to-private fee ratio of 0.27, whereas high-Medicaid-generosity states (23 states with fee ratios greater than the median) have a ratio of 0.63.[13]

5. Graphical Evidence and Aggregate-Level Analyses

Our conceptual model considers how Medicaid eligibility impacts the technology adoption behavior of incumbent hospitals, abstracting away from any effect on NICU availability through the composition (entry, exit, and merger) of hospitals in the market. While our research question centers on the technology adoption decisions of existing firms, we first provide evidence of the overall effect of Medicaid eligibility on NICU availability at the state-level, including all non-Federal, acute-care, and children's hospitals. This aggregate state-level analysis can show very transparently the connection between Medicaid expansion and general NICU diffusion. After presenting these aggregate-level results, we provide evidence that compositional effects are likely to be minimal and then proceed in the next section to directly estimate the effect of eligibility expansions on incumbent hospital behavior.

Figure 1 plots the change in the number of hospitals with a NICU per 100,000 women of

[11] While there is work documenting trends in state Medicaid physician reimbursements (see Norton and Zuckerman 2000; Zuckerman, Williams, Stockley 2009), there are no such sources for hospital reimbursements (personal communication with Stephen Zuckerman, Urban Institute). Quinn (2008) documents Medicaid hospital-payment methods by state as of 2004 but does not provide any information on rates or trends over time.

[12] In Appendix Table 3, we also control for yearly fee data to ensure that our results are not biased by fee changes. For this analysis, we extend the Currie, Gruber, and Fischer (1995) data through 1996, as detailed in Appendix 2.

[13] Fee data are unavailable for Alaska, Arizona, Kentucky, Rhode Island, Texas, and Wyoming.

childbearing age in each state against the change in the fraction of women from the fixed CPS sample that would be eligible for Medicaid under each state's eligibility rules between 1985 and 1996, along with a regression line. While NICU availability and Medicaid eligibility both trend upward during the study period, Figure 1 provides evidence from the raw data that increases in NICU availability are only slightly more concentrated in states with larger Medicaid expansions.

Figure 2 shows the same scatter plot for four groups of states based on whether their 1985 baseline Medicaid-to-private obstetric-fee ratio and their 1985 baseline insurance rate are above or below the median. This figure suggests a slightly positive relationship between eligibility and NICU provision in states that are expected to have low crowd-out due to low insurance rates (Panels A and C). However, in states with high insurance rates and therefore high crowd-out, the relationship is negative (Panels B and D). This negative relationship is particularly evident in states that also have low fee ratios (Panel B).

We verify the aggregate statistical relationship revealed by these figures with the following state-year-level regression equation:

$$NICU_{st} = \beta_0 + \beta_1 PctElig_{st} + \beta_2 PctElig_{st} * Ins85_s + \beta_3 X_{st} + \beta_4 Ins85_s * t + \alpha_t + \gamma_s + \omega_s t + \varepsilon_{st} \tag{4}$$

$NICU_{st}$ is the number of hospitals operating a NICU per 100,000 women of child-bearing age, and $PctElig_{st}$ is the simulated Medicaid eligibility rate in the hospital's state and year.[14] X_{st} controls for time-varying state population characteristics. α_t and γ_s represent year and state fixed effects to control for general adoption trends and fixed state-specific characteristics, respectively. In some specifications we also include an interaction of eligibility and $Ins85_s$, which measures the insurance rate in the hospital's state at its baseline in 1985 as our proxy of potential crowd-out. In these specifications we also add an interaction between baseline insurance and a linear time trend to account for the fact that areas with higher insurance may have differential NICU adoption trends even in the absence of changes to Medicaid.

[14] We follow papers such as Currie and Gruber (2001), DeLeire, Lopoo, and Simon (2011), and Garthwaite (2012) in estimating the reduced-form effect of simulated Medicaid eligibility. In the spirit of Currie and Gruber (1996), we have also estimated instrumental variable regressions in which we instrument for the actual fraction of CPS respondents eligible for Medicaid with the simulated fraction eligible for Medicaid. These instrumental variable results are very similar to the reduced-form results presented in the paper, and are available upon request.

For ease of interpretation, we standardize the insurance rate so that β_2 reflects changes in the eligibility gradient with a one-standard-deviation change in the insurance rate from its sample mean.

The identifying assumption of this specification is that the timing of state-level legislative changes to Medicaid eligibility rules is uncorrelated with unobserved determinants of NICU availability. To account for any potential systematic differences in NICU provision trends that are correlated with legislative choices, we also include state-specific linear time trends, $\omega_s t$, in some specifications. These time trends have been found to be important in previous studies of Medicaid eligibility and insurance choices (e.g. Card and Shore-Sheppard 2004; Shore-Sheppard 2008).[15]

Results are presented in Table 3, with Panel A considering all states and Panels B and C focusing on states with below- and above-median Medicaid-to-private fee ratios, respectively. Each panel presents results with and without an interaction between simulated eligibility and the 1985 state insurance rate; we also add controls progressively across columns. Consistent with the relatively flat linear fit line in Figure 1, there is no statistically significant effect of Medicaid eligibility on NICU provision when all states are considered together, regardless of the inclusion of control variables. However, our results verify the patterns in Figure 2 that suggest clear heterogeneity by insurance rate and fee ratio. For states with low fee ratios (Panel B of Table 3), the relationship between eligibility and NICU provision largely depends on the baseline insurance rate. For states with high fee ratios (Panel C of Table 3), there is no statistically significant impact of eligibility on NICU provision, regardless of the extent of crowd-out.

Our aggregate results could be partially driven by changes in NICU availability associated with hospital entry, exit, and merger decisions. However, summary statistics suggest little scope for meaningful effects through these margins. Over our twelve-year period, only 139 new hospitals enter, of

[15] Card and Shore-Sheppard (2004) and Shore-Sheppard (2008) find that Cutler and Gruber's (1996) findings about the effects of Medicaid eligibility on insurance coverage, which cover a time period similar to our own, decrease substantially in magnitude when they include state-by-year and state-by-age dummies. Their results suggest that the model should be fully saturated in the context of insurance coverage outcomes. Our identification is driven only by state-year variation, but in the spirit of these other papers, we add state-specific linear trends to address similar concerns.

which 9 operate a NICU upon opening and 24 eventually open a NICU by 1996.[16] Exit is more common, with 605 hospital closures during this time period, though only 21 (3.5%) of these hospitals operate a NICU at the time of closure. This time period also witnessed increasing hospital merger activities. Among a total of 179 merger events between two or more general acute-care hospitals,[17] 74 involve at least one hospital operating a NICU, and almost all of these (64) involve exactly one hospital operating a NICU at the time of the merger. Additionally, almost all of the hospitals resulting from these mergers involving NICUs also operate a NICU after the merger (66). To summarize, entry, exit, and merger change the set of hospitals in the market, but have little effect on the number of NICUs in operation. In analysis presented in Appendix 3, we also find that Medicaid expansions are not correlated with market participation activities themselves. In light of this discussion, we expect that Medicaid expansions are likely to impact NICU availability primarily through the adoption decisions made by incumbent hospitals in the market, which we investigate in the following section.

6. Incumbent Hospitals' Response to Medicaid Expansions

We now focus on how incumbent hospitals' technology adoption decisions respond to Medicaid expansions through a hospital-level analysis that controls for hospital fixed effects. This analysis is directly connected to our analytical model of individual, incumbent hospital behavior and has the advantage of identifying these behavioral effects. In addition, this hospital-level analysis allows us to better explore heterogeneity of effects by hospital characteristics (such as ownership and market competition) and better control for the hospital's local market environment. Following Baker and Phibbs (2002), we focus on the set of hospitals that are most relevant to NICU adoption (i.e., those non-Federal,

[16] Using data obtained from the NBER and created by Sujoy Chakravarty of Rutgers University, we identify entry and exit by combining information from the annual AHA appendices on reasons for the addition and deletion of hospitals and additional information from web searches.

[17] We also identify mergers by means of the annual AHA appendices on ID additions and deletions, which catalog cases in which IDs are changed due to mergers. Typically, a merger results in the deletion of the IDs associated with the merging hospitals, and in the addition of a new ID for the resulting merged hospital. We consider here only mergers in which at least two of the merging hospitals were general acute-care hospitals. If a general acute-care hospital merges with a specialty hospital, such as a psychiatric hospital, the result is almost always a general acute-care hospital. We therefore do not consider these types of mergers, since they do not impact the number of general acute-care hospitals that might operate NICUs.

acute-care, and children's hospitals with an obstetrics unit and at least 50 births in 1985). In robustness checks discussed below, we show that our results hold with several alternative sample selection criteria.

A. Main Results

We estimate the following regression equation at the hospital level:

$$NICU_{hcst} = \beta_0 + \beta_1 PctElig_{st} + \beta_2 PctElig_{st} * Ins85_s + \beta_3 X_{ct} + \beta_4 Ins85_s * t + \alpha_t + \gamma_h + \omega_s t + \varepsilon_{hcst} \quad (5)$$

Here, $NICU_{hcst}$ is an indicator variable that equals one if a hospital offers a NICU in a given year. All other variables are defined as above in Equation 4 except X_{ct}, which controls for time-varying characteristics of a hospital's county (the number of births, the number of low-birth-weight births, female population by age, and per-capita income), and γ_h, which represents hospital fixed effects. Standard errors are clustered at the state level.

Panel A of Table 4 displays results for all the hospitals in our sample. Columns 1 through 3 report the coefficient estimates of the eligibility variable from Equation 5 without the baseline insurance interaction as we progressively add controls. Column 1 includes only year dummies and hospital fixed effects. As we add controls for county-level characteristics in Column 2 and then state-specific linear time trends in Column 3, the coefficient estimates are always small and statistically insignificant. The remaining columns allow the effect of eligibility to vary by the baseline state-level insurance rate. In Columns 4-7 we again progressively add controls. Regardless of the controls included, the eligibility main effect is not statistically different from zero, and the point estimates of the interaction terms are negative and do not vary by much. These patterns of results as controls are added suggest that Medicaid eligibility expansions are not correlated with observed determinants of NICU adoption, and are therefore less likely to be correlated with unobserved determinates. However, including controls and state-specific trends in Column 7 does lead to smaller standard errors, and the interaction terms become statistically significant at the 5-percent level.[18]

The estimates in Column 7 imply that at the mean level of state insurance, eligibility does not affect NICU provision, but that this effect does vary with the state insurance rate. To facilitate the interpretation, Figure 3 plots the effect of eligibility on NICU provision as the insurance rate varies, based on the estimates from Column 7. Below the mean insurance rate, the effect is not statistically significant at any values within two standard deviations of the mean insurance rate. In contrast, for many values above the mean level of insurance, we find the effect of eligibility on the likelihood that a hospital has a NICU to be negative and statistically significant at the 5-percent level.

Panels B and C of Table 4 present results separately for states with low and high Medicaid-to-private fee ratios. We find that the main result that eligibility effects differ by expected crowd-out is being fully driven by low-fee-ratio states. In the low-fee-ratio sample, the interaction effect is statistically significant at the 5-percent level when all controls are included, mostly due to greatly increased precision once we include state-specific trends.[19] These estimates suggest that the overall effect of Medicaid eligibility on NICU provision is statistically significant and negative for states with insurance rates more than 0.56 standard deviations above the mean. For all insurance rates below this threshold, there is no statistically significant effect. In states with high fee ratios, we find no statistically significant effect of eligibility regardless of the state insurance rate.[20] These results suggest that when Medicaid payment rates are low and crowd-out is expected to be most prevalent, increased eligibility has substantial negative

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[18] The fact that our point estimates are not largely affected by the inclusion of state-specific time trends, while our standard errors decrease substantially, suggests that allowing for these trends only reduces the overall variability of the error term. Wolfers (2006) cautions against relying on difference-in-difference models in which state linear trends drastically change the conclusions; however, the fact that these trends do not greatly impact our point estimates suggests that this argument does not present a concern in our context. We also conduct placebo tests that suggest that eligibility changes are not correlated with unobserved determinants of NICU provision in Section 6C.

[19] The interaction effect estimate is virtually unchanged by adding the insurance times year term, and the point estimate becomes slightly smaller as we include population controls. However, the standard error falls greatly once we include state-specific trends.

[20] In addition to state insurance rate, we also use two alternative measures for crowd-out (county-level unemployment and county-level fraction of individuals below the federal poverty line). We find that these results, as reported in Appendix Table 1, are consistent with those using state insurance rate. We also allow the interaction between eligibility and crowd-out, conditional on reimbursement rates, to be nonlinear in Appendix Table 2. As in Figure 2, we split states into four categories, and coefficient estimates confirm the raw data plotted in Figure 2. We find a large, negative, and statistically significant effect of eligibility in states with low-Medicaid-to-private-fee ratios and high baseline insurance rates. For all three other groups we find no statistically significant impact.

effects on NICU provision. In states with higher Medicaid generosity relative to private insurers, eligibility has no effect on NICU provision, regardless of the expected crowd-out. This implies that even in states in which Medicaid reimbursements are most generous and eligibility expansions are most likely to substantially increase the fraction of pregnant women covered by insurance, Medicaid payments might still be too low for hospitals to experience a change in their financial incentives for operating a NICU.[21]

B. Assessing Magnitudes of Results

In order to gauge the overall magnitude of these estimated effects, we conduct a counterfactual simulation of NICU adoption in the absence of the Medicaid expansion. We first use our regression results to predict the counterfactual probability that each hospital would have operated a NICU had Medicaid eligibility remained at its 1985 level in each state. We then sum these counterfactual predictions to obtain the counterfactual number of hospitals operating a NICU by year. Finally, we compare the number of NICUs present in the data in 1996 to the counterfactual number that would have been in operation in 1996 without a Medicaid eligibility expansion.

Because we only find statistically significant effects of eligibility on NICU provision in low-fee-ratio and high-insurance-rate states (insurance rates greater than 0.56 standard deviations above the mean), we conduct our counterfactual simulation in these areas alone based on the estimate in Panel B, Column 7. We find that in the absence of the Medicaid expansion, there would have been 9.6 additional NICUs in these low-paying, high-insurance-rate states in 1996.[22] This magnitude of effect is equivalent to 7.0% of actual growth between 1985 and 1996 in low-fee-ratio states and to 24% of actual growth in the 9

[21] We have explored whether a lack of variation in NICU diffusion might drive the null results in low-crowd-out, high-fee states, where we would most expect to find positive effects. These states had lower baseline NICU provision in 1985 (9%) as compared to the overall sample (14.8%), but a similar rate of diffusion over the study period (9 percentage points as compared to 10 percentage points in the full sample), suggesting that lack of variation does not drive this finding.
[22] Not surprisingly, we find very similar results in an alternative simulation based on the full sample regression estimate in Panel A, Column 7 of Table 4.

states with low-fee ratios and high-insurance rates.[23]

C. Placebo Tests

Our identification strategy relies on state-level changes in Medicaid eligibility being uncorrelated with other unobserved state-level changes that might affect the likelihood of NICU adoption. Previous studies have found instances of state-level factors that could lead states to adopt new technologies at different speeds. For example, Skinner and Staiger (2005) find persistent differences in states' likelihood to adopt technologies, both medical and non-medical. If these differences are correlated with Medicaid expansion decisions and not captured by our state-specific linear time trends, our estimates would be biased. In addition to general tastes for technology, there may be other policy changes that affect technology adoption, and our results would be confounded if such changes coincided with Medicaid eligibility policy.[24]

We conduct several additional tests of our identification assumption beyond the above discussion of successively adding controls. First, we test whether future changes in Medicaid eligibility impact current NICU adoption. If we were to find that future changes to Medicaid eligibility are correlated with current NICU provision, it would shed doubt on the assumption that eligibility expansions are independent of pre-existing NICU adoption trends. We test this in Columns 1, 3, and 5 of Table 5 by adding a two-year lead of eligibility and its interaction with baseline insurance.[25] In the full sample and

[23] Between 1985 and 1996, the number of NICUs in our analysis sample increased by 138 in low-fee-ratio states and 40 in low-fee-ratio states with insurance rates greater than 0.56 standard deviations above the mean.

[24] The period that we study, particularly the early 1990s (Ku and Coughlin 1995; Coughlin, Ku, and Kim 2000), also coincides with expansions of the Disproportionate Share Program (DSH), which allowed states to receive matching federal funds in order to distribute additional payments to hospitals treating a disproportionate number of indigent patients. Hospital-level information on DSH payments were not made available until after 1998, and the only reliable data available for our study period are state-level aggregated DSH hospital payments for 1993 from Ku and Coughlin (1995) and Coughlin, Ku, and Kim (2000). We find that our results are consistent if we focus on just the pre-1990 period, when states were reluctant to implement DSH programs. We also conduct additional analysis by incorporating DSH payments to adjust relative fee-ratio across states; the results are similar and available upon request.

[25] Many states changed eligibility multiple times during our study period, often in consecutive years. For example, if we consider policies that changed simulated eligibility by one percentage point or more in a given year, one quarter of these changes were followed by another change of one percentage point or more in the next year, often because Medicaid policy changes are designed as phased expansions. In order to avoid estimating lead effects that are

the low-fee-ratio sample, we find no statistically significant lead effects. In the high-fee-ratio states, we do find a negative and statistically significant main effect of future Medicaid eligibility, suggesting that these states may have seen slowing NICU adoption concurrent with eligibility increases; overall, however, this placebo test supports our identifying assumptions. In unreported results, we also find that these results are not sensitive to which controls are included; lead effects are small and statistically insignificant in all cases.

In Columns 2, 4, and 6 of Table 5, we also include two-year lags of eligibility and its interaction with 1985 insurance rates in order to better trace out the timing of responses to eligibility changes. These results suggest that in the full sample and in low-fee-ratio states, there are additional lagged effects of eligibility when crowd-out is prominent. In high-fee-ratio states, where we may have expected some positive effects to take place by allowing for lagged effects, given the likely planning and construction time involved in opening a NICU, we still find no statistically significant effects.

To further ensure that tastes for technology and other concurrent technology-related policies do not confound our results, we present an additional placebo test by examining hospitals' adoption of two cardiac-care-related technologies, catheterization labs and open-heart surgery, which we would not expect to be affected by Medicaid eligibility expansions for pregnant women.[26] These two technologies diffused rapidly during our study period, with the percent of sample hospitals providing catheterization labs increasing from 19.7 to 34.2 and the percent providing open-heart surgery increasing from 12.7 to 19.4. The estimation results, reported in Table 6, support that the main findings of this paper are not driven by unobserved heterogeneity at the state-year level that is common to the adoption of NICUs and cardiac-care-related treatments.[27]

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simply picking up contemporaneous effects of consecutive changes, we have chosen to include only two-year leads in these regressions. Similarly, we include two-year lagged effects in subsequent analysis.

[26] If hospitals face capital constraints and respond to Medicaid expansions for pregnant women by investing in NICUs, they might delay their adoption of other technologies, such as those related to cardiac care. Nevertheless, even in such cases, we expect the impact on cardiac care to be opposite to that of NICUs and to be much less pronounced.

[27] All the other coefficients are small and not statistically significant, with one exception of a positive main effect of Medicaid eligibility on catheterization labs in high-fee-ratio states. The fact that this positive effect only occurs to

D. Additional Robustness Tests

As discussed in Section 5 above, Medicaid expansions are unlikely to impact NICU provision through market participation decisions. Appendix 3 provides additional evidence that any attrition from our main analysis sample due to hospital exit or merger is not correlated with these expansions. To further ensure that different treatments of entry, exit, and merger do not lead to different results, we present estimates from various versions of our hospital-level sample in Table 7. Column 1 repeats the results from our main analysis sample, which does not allow for entry but allows for exit. Column 2 shows that coefficient estimates are very similar if we exclude exiting hospitals by only considering the subsample of these hospitals that remain in the data for all 12 years. Column 3 replicates our hospital analysis for the sample of all general acute-care and children's hospitals, and Column 4 considers the subset of these hospitals in the sample for all 12 years, thus excluding entry and exit. All of these different samples yield consistent results.

Table 7 presents some additional robustness tests for the main results. In Column 5 we present results of our main model weighted by the number of births delivered by each hospital in 1985. This allows us to interpret the results as the effect of Medicaid eligibility on the fraction of infants born in hospitals with NICUs, rather than the effect for the average hospital. If anything, we find stronger results when weighting by births. Column 6 considers conventional wisdom that hospitals are not likely to close NICUs once they have been adopted. We therefore redefine our NICU indicator by assuming cases in the data in which hospitals reported having a NICU one year but not a following year may be due to reporting errors. The results are very similar to our main findings.

E. Heterogeneity

Hospitals with different characteristics or in different market environments might respond

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labs in high-fee-ratio states, where we do not find any effect of Medicaid eligibility, does not raise significant concern about the identification strategy given our other tests.

differently to Medicaid expansions. Table 8 explores such heterogeneity. Columns 1 through 3 study ownership type; here, we estimate our main specifications for government-owned, nonprofit, and for-profit hospitals. We might expect for-profit hospitals to be more responsive to financial incentives in making technology adoption decisions. However, the results suggest that our main findings are driven by nonprofit hospitals. A caveat of these results is that the for-profit sample size is much smaller, leading to much larger standard errors and potentially to insignificant results due to a lack of precision. We also find smaller and not statistically significant coefficients for government-owned hospitals, which is not surprising, since non-Federal but government-owned hospitals tend to be smaller and less financially capable of operating NICUs.

We also explore heterogeneity by two different measures of a hospital market's competitive environment. First, because hospitals often invest in NICUs in order to compete for births, we might expect adoption incentives to interact with local market competition. Second, the marginal benefit of adoption is likely to vary with the number of existing NICUs.

In Columns 4 through 6 of Table 8, we separately estimate our main specification for subsamples stratified by level of competition as measured by the Herfindahl-Hirschman index (HHI) of births in a county.[28] In Columns 7 through 9, we stratify the sample by the predicted fraction of other hospitals in the county already operating a NICU.[29] We find the results are consistent with our main findings in monopoly markets (Columns 4 and 7)[30] and in competitive markets, either measured by a low HHI (Column 6) or by the fraction of competing hospitals already operating a NICU (Column 9). However, we find no evidence that Medicaid expansions slow NICU adoptions in markets with

[28] We use the number of births reported by each hospital in each year from the AHA data to construct county-level market shares and then use these market shares to construct a county-level HHI in the market for births. In unreported results we also construct a measure of HHI based on all hospital admissions and find similar results.

[29] We do not use the actual fraction of other hospitals operating a NICU to avoid the potential bias caused by splitting the sample by realized values of the dependent variable. Instead we use a regression of NICU provision on all of the variables in our interaction effect model to predict the probability that each hospital operates a NICU in a given year. For each hospital we then sum the probabilities associated with all other hospitals in the focal hospital's county. This represents the predicted number of other hospitals in the county operating a NICU. We then divide by the number of other hospitals to obtain the predicted fraction of other hospitals operating a NICU.

[30] The samples in Columns 4 and 7 are slightly different because a hospital that performs all of a county's births can have a birth HHI of 10,000, even if it is not the only hospital present in the county.

intermediate HHIs or markets with a smaller fraction of competing hospitals operating NICUs. This finding is in line with hospitals in these markets facing a higher marginal benefit of adopting a NICU due to strategic motives, such as preemption incentives. Negative financial incentives associated with Medicaid expansions and crowd-out might not be strong enough to change adoption behavior in these markets.

7. Discussion and Policy Implications

Our understanding of how large-scale insurance expansion affects technology adoption is informed mainly by empirical examples from Medicare and managed care. We contend that Medicaid could have an impact that is theoretically different and may fall somewhere between these prior two cases. This effect is important to investigate, especially as Medicaid coverage of adults is expanding substantially under the ACA. We use the case of hospital NICU technology during the last period of large Medicaid expansions involving an adult population (pregnancy-related eligibility during the 1980s and 1990s) to conduct the first analysis of Medicaid expansions and technology adoption. We draw our testable hypotheses from an analytical model in which we discuss the opposing incentives created for incumbent hospitals by Medicaid expansions, and we pay close attention to how the predicted behavioral responses rely on reimbursement levels and the degree of expected private insurance crowd-out.

Despite the fact that NICU technology diffused rapidly during the time of these Medicaid expansions, we find that on average Medicaid expansions had a negative but not statistically significant impact on hospitals' decisions to adopt NICUs. We also find important heterogeneity of effect due to Medicaid crowd-out. In high-insurance states where more new Medicaid patients are likely to have been privately insured at baseline, Medicaid eligibility expansions decrease the likelihood of adoption. This finding is particularly notable in states where Medicaid reimbursement rates are low relative to private rates. In low-insurance states where we might expect Medicaid expansion to most significantly increase the level of insurance coverage, we do not find any significant impacts on NICU provision, even in the subset of states with relatively high Medicaid payment rates.

The literature on technology adoption and health care costs recognizes the difficult tradeoff between medical advances and health outcomes (e.g. Cutler 2005). In the context of neonatal intensive care, the fact that Medicaid expansions slowed diffusion in high-insurance areas may imply that these eligibility changes actually curtailed expensive investments that are likely not to be health improving. This is partly because most diffusion of NICUs during this study period was driven by mid-level NICUs, which diverted high risk patients from more established, high-level facilities. Given previous findings that high risk infants born in these mid-level NICUs experience similar (Freedman 2012b) or worse (Baker and Phibbs 2002) health outcomes to those born in higher-level facilities, it appears that decreasing the number of hospitals offering NICUs would likely be cost saving and at least not harmful to health. Others have found that insurance expansions and coverage generosity likely increase access to high quality hospitals (Aizer, Lleras-Muney, and Stabile 2005) and NICUs (Currie and Gruber 1997, 2001) for low-income groups; however, our findings suggest that this increased access is likely not working through the mechanism of hospital investments in technology.

Taken together, our results portray the effect of Medicaid expansions as having slowed the adoption of technology, which contrasts with the evidence on the Medicare program. One factor our results point to as possibly responsible for this difference is that Medicaid payment rates are less generous than Medicare payments. Medicaid rates amounted to only 63 percent of private rates on average in higher-Medicaid-payment states during our study period. Therefore hospital responses to Medicaid expansions appear to have more in common with responses to the rollout of managed-care insurance than to previous large increases in the number of individuals covered by Medicare insurance. We note that these particular Medicaid expansions in our study were characterized by a high rate of crowd-out (Cutler and Gruber 1996, Dave et al. 2011, Dubay and Kenney 1997). Other expansions that do not exhibit as much crowd-out, such as parental Medicaid expansions (Hamersma and Kim 2013), may not lead to a deceleration of technology adoption. However, our results suggest that Medicaid's low payment rates to hospitals make it unlikely that such expansions would accelerate technology adoption, even if crowd-out were less prevalent.

Our findings also have important implications for the insurance expansions through the ACA. While it may be difficult to determine which well-defined technologies specifically appeal to the newly insured populations, such as neonatal intensive care in the case of pregnant women, our results are informative about the potential heterogeneity of supply-side responses to different types of insurance expansions.

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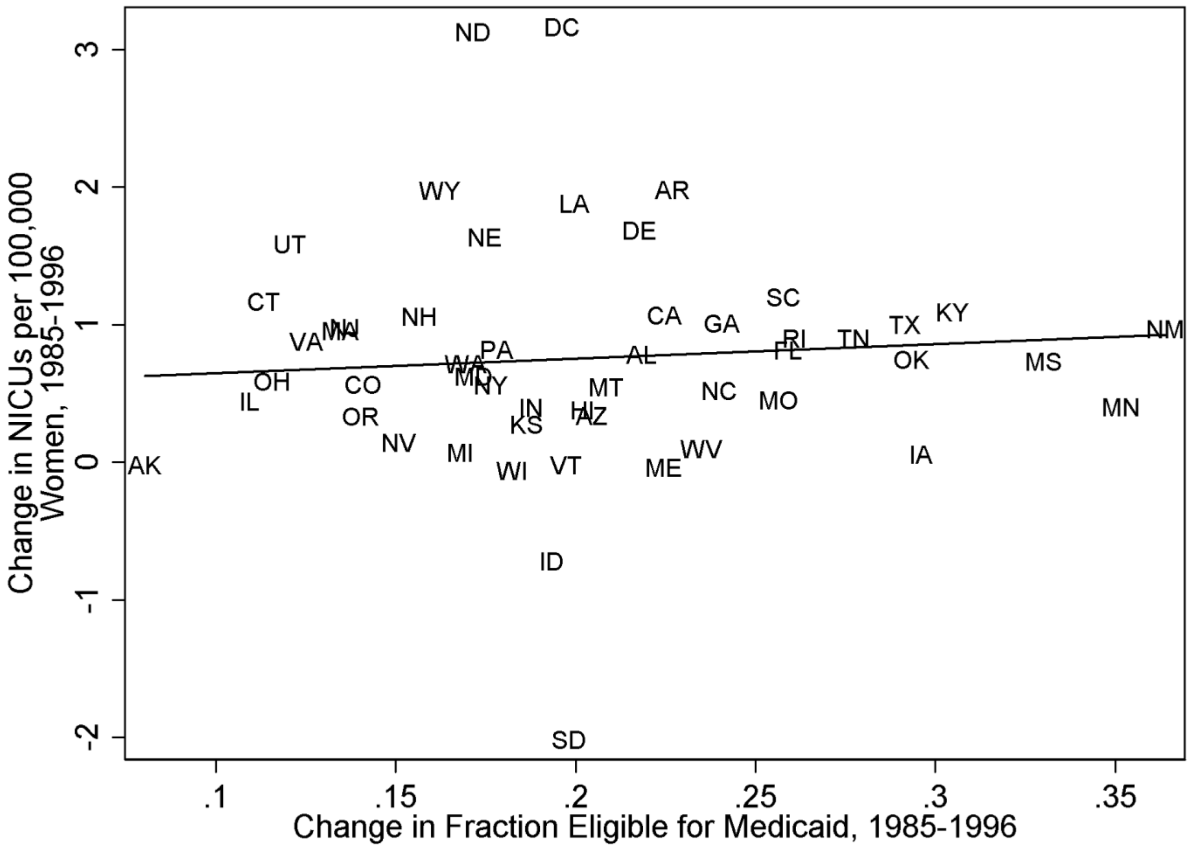
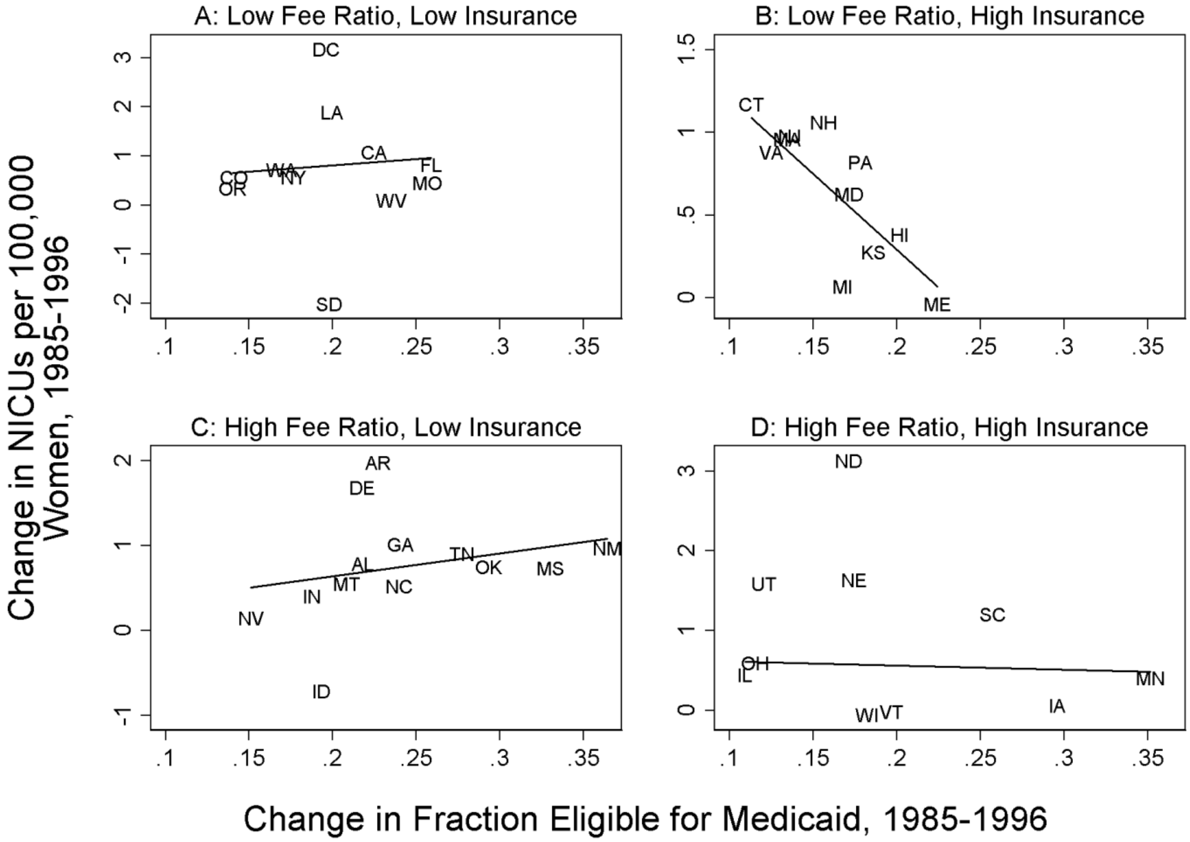
Figure 1: State-Level Changes in NICU Provision by Medicaid Eligibility Changes, 1985 - 1996



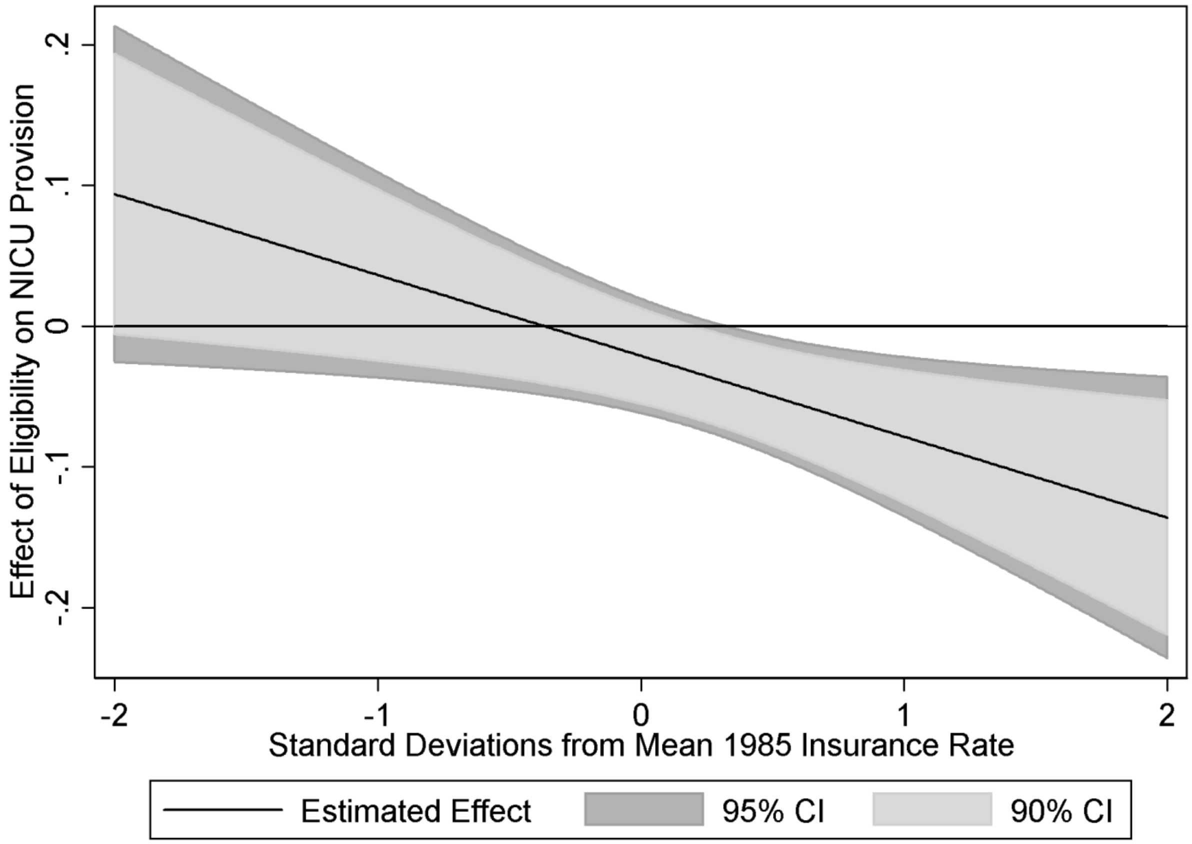
Notes: This figure plots changes in the number of hospitals that offer NICUs per 100,000 women of childbearing age against changes in the fraction of women in a fixed sample from the CPS that would be eligible for Medicaid conditional on pregnancy, under that state's eligibility rules. Changes are calculated between 1985 and 1996 for each state. The line represents the best linear fit, weighted by the number of women of childbearing age in 1985.

Figure 2: State-Level Changes in NICU Provision by Medicaid Eligibility Changes – Groups of States Stratified by Fee Ratio and Insurance Rate



Notes: This figure plots changes in number of hospitals that offer NICUs per 100,000 women of childbearing age against changes in the fraction of women in a fixed sample from the CPS that would be eligible for Medicaid conditional on pregnancy, under that state's eligibility rules. Changes are calculated between 1985 and 1996. The line represents the best linear fit weighted by the number of women of childbearing age in 1985. States are divided based on whether their 1985 insurance rate and 1985 Medicaid-to-private obstetric-fee ratio are above or below the median.

Figure 3: Effects of State Medicaid Eligibility Level on Hospital NICU Provision by 1985 State Insurance Rate



Notes: This figure plots the estimated effect of state Medicaid eligibility on the probability that a hospital offers a NICU for different values of the baseline state-level insurance rate, along with 95-percent and 90-percent confidence intervals. Baseline insurance is normalized to reflect the number of standard deviations from the sample mean. Estimates are from the regression reported in Panel A and Column 7 of Table 4 and control for hospital fixed effects; year dummies; state-specific time trends; county-level controls for number of births, number of low-birth-weight births, infant mortality rate, population, female population by age, and per-capita income; and an interaction between baseline insurance rate and a linear-year trend. Standard errors are clustered at the state level.

Table 1: Summary Statistics by Year

Year	Number of Hospitals	Number of Hospitals with a NICU	Fraction of Hospitals with a NICU	Mean State Medicaid Eligibility
1985	3,993	589	0.148	0.097
1986	3,957	627	0.158	0.111
1987	3,893	655	0.168	0.128
1988	3,830	707	0.185	0.190
1989	3,784	735	0.194	0.232
1990	3,728	742	0.199	0.254
1991	3,679	760	0.207	0.257
1992	3,635	777	0.214	0.268
1993	3,597	794	0.221	0.279
1994	3,557	847	0.238	0.280
1995	3,510	854	0.243	0.310
1996	3,450	857	0.248	0.310

Notes: Number and fraction of hospitals with a NICU are calculated from the AHA data. Sample includes hospitals with an active obstetric unit at the beginning of the time period. Mean state Medicaid eligibility is the mean at the year level for our hospital sample.

Table 2: Summary Statistics of County-Level Characteristics

	Mean	Standard Deviation
County-Level Infant Health Controls		
Number of Births	8,860	26,247
Number of Low-Birth-Weight Births	636	1,803
County-Level Population Controls		
Population	500,842	1,341,186
Female Population by Age		
15 to 19	17,495	46,964
20 to 24	20,413	56,747
25 to 29	22,771	63,297
30 to 34	22,225	60,653
35 to 44	37,663	100,042
45 to 54	26,773	70,159
55 to 59	11,282	28,916
60 to 64	10,823	27,326
65 to 74	18,515	45,861
Per-Capita Income ($)	16,910	5,264
Baseline Socioeconomic Characteristics		
State-Level Insurance (%)	82.545	4.284
County-Level Employment Rate (%)	91.880	3.446
County-Level Above Federal Poverty Line (%)	85.821	6.283
N	44,613	

Notes: All variables are calculated from the Area Resource File, except for State-Level Insurance, which is drawn from the March Supplement of the Current Population Survey. Baseline characteristics reflect 1985 levels.

Table 3: Aggregate State-Level Effect of State Medicaid Eligibility on NICU Provision

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Full Sample (N=612)							
Eligibility	0.167	0.176	-0.072	-0.165	-0.179	-0.034	-0.129
	(0.300)	(0.243)	(0.167)	(0.259)	(0.232)	(0.216)	(0.141)
Eligibility X				-0.560**	-0.354	-0.454**	-0.354**
1985 Insurance Rate				(0.187)	(0.231)	(0.186)	(0.168)
Panel B: Low-Medicaid/Private-Fee-Ratio States (N=264)							
Eligibility	-0.021	0.002	-0.093	-0.167	-0.181	-0.237	-0.036
	(0.461)	(0.423)	(0.189)	(0.431)	(0.395)	(0.279)	(0.258)
Eligibility X				-0.820*	-0.760	-0.892**	-0.685**
1985 Insurance Rate				(0.424)	(0.488)	(0.406)	(0.318)
Panel C: High-Medicaid/Private-Fee-Ratio States (N=276)							
Eligibility	0.220	0.092	-0.124	0.113	0.027	0.094	-0.118
	(0.455)	(0.279)	(0.244)	(0.386)	(0.396)	(0.273)	(0.219)
Eligibility X				-0.331	0.201	-0.013	0.042
1985 Insurance Rate				(0.259)	(0.359)	(0.361)	(0.314)
Year & State Dummies	X	X	X	X	X	X	X
1985 Insurance X Year					X	X	X
Other Controls		X	X			X	X
State Trends			X				X

Notes: Each column presents coefficient estimates from separate regressions of the number of NICUs per 100,000 women of childbearing age in a given state and year on the fraction of women eligible for Medicaid in that state and year. The number of NICUs per 100,000 women of childbearing age is calculated from all non-federal, general acute-care hospitals in the AHA data. Regressions are weighted by the number of women of childbearing age in 1985. Panel A includes all states. Panel B includes states with Medicaid-to-private obstetric-fee ratios below 0.41, and Panel C includes states with fee ratios above 0.41. In Columns 4-7 eligibility is interacted with the state-level baseline (1985) insurance rate. All regressions include state and year fixed effects, and each column progressively adds additional controls. Controls include an interaction between baseline insurance and a linear-year trend (in the interaction models); state-level controls for number of births, number of low-birth-weight births, population, female population by age and per-capita income; and state-specific time trends. Standard errors are clustered at the state level.

** - p < .05, * - p < .10

Table 4: Hospital-Level Effect of State Medicaid Eligibility on NICU Provision

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Full Sample (N=44,613)							
Eligibility	-0.044	-0.017	-0.017	-0.059	-0.059	-0.026	-0.021
	(0.044)	(0.036)	(0.021)	(0.042)	(0.043)	(0.034)	(0.020)
Eligibility X				-0.042	-0.044	-0.045	-0.057**
1985 Insurance Rate				(0.033)	(0.044)	(0.035)	(0.025)
Panel B: Low-Medicaid/Private-Fee-Ratio States (N=19,610)							
Eligibility	-0.054	-0.059	-0.033	-0.055	-0.057	-0.050	-0.018
	(0.073)	(0.065)	(0.027)	(0.080)	(0.065)	(0.053)	(0.030)
Eligibility X				-0.116	-0.110	-0.084	-0.078**
1985 Insurance Rate				(0.079)	(0.070)	(0.072)	(0.040)
Panel C: High-Medicaid/Private-Fee-Ratio States (N=20,380)							
Eligibility	-0.015	0.019	-0.032	-0.016	-0.018	0.014	-0.031
	(0.044)	(0.042)	(0.026)	(0.042)	(0.040)	(0.041)	(0.026)
Eligibility X				-0.008	0.003	0.020	-0.001
1985 Insurance Rate				(0.033)	(0.039)	(0.039)	(0.027)
Year & Hospital Dummies	X	X	X	X	X	X	X
1985 Insurance X Year					X	X	X
Other Controls		X	X			X	X
State Trends			X				X

Notes: Each column presents coefficient estimates from separate regressions of whether a hospital offers a NICU in a given year on the fraction of women eligible for Medicaid in the hospital's state and year. The sample includes general acute-care hospitals operating an active obstetric unit at baseline. Panel A includes the full sample of hospitals. Panel B includes hospitals in states with Medicaid to private obstetric fee ratios below 0.41, and Panel C includes hospitals in states with fee ratios above 0.41. In Columns 4-7 eligibility is interacted with the hospital's state-level baseline (1985) insurance rate. All regressions include hospital and year fixed effects, and each column progressively adds additional controls. Controls include an interaction between baseline insurance and a linear-year trend (in the interaction models); county-level controls for number of births, number of low birth weight births, population, female population by age and per-capita income; and state-specific time trends. Standard errors are clustered at the state level.

** - p < .05, * - p < .10

Table 5: Timing of Medicaid Eligibility Effect on NICU Provision

	All States		Low Medicaid/Private Fee-Ratio States		High Medicaid/Private Fee-Ratio States	
	(1)	(2)	(3)	(4)	(5)	(6)
This Year:						
Eligibility	0.017	-0.019	0.048	-0.007	-0.019	-0.033
	(0.021)	(0.019)	(0.032)	(0.026)	(0.020)	(0.028)
Eligibility X	-0.081**	-0.046*	-0.102**	-0.073**	-0.032**	0.004
1985 Insurance Rate	(0.021)	(0.025)	(0.038)	(0.029)	(0.016)	(0.030)
2 Years Later:						
Eligibility	-0.019		-0.005		-0.035**	
	(0.022)		(0.031)		(0.017)	
Eligibility X	-0.022		-0.005		-0.010	
1985 Insurance Rate	(0.024)		(0.022)		(0.022)	
2 Years Prior:						
Eligibility		-0.005		0.002		0.003
		(0.021)		(0.045)		(0.032)
Eligibility X		-0.034*		-0.086**		-0.001
1985 Insurance Rate		(0.020)		(0.043)		(0.028)
N	36,663	36,663	16,115	16,115	16,775	16,775

Notes: Each column presents coefficient estimates from separate regressions. All regressions include hospital fixed effects; year dummies; county-level controls for number of births, number of low birth weight births, population, female population by age and per-capita income; state-specific time trends; and an interaction between baseline insurance and a linear-year trend. Standard errors are clustered at the state level.
** - p < .05, * - p < .10

Table 6: Effect of Medicaid Eligibility on Cardiac Care Provision

	Cath Lab	Open Heart Surgery
	(1)	(2)
Panel A: Full Sample (N=46,197)		
Eligibility	0.029	-0.003
	(0.043)	(0.014)
Eligibility X	-0.007	-0.002
1985 Insurance Rate	(0.027)	(0.011)
Panel B: Low-Medicaid/Private-Fee Ratio States (N=18,880)		
Eligibility	-0.018	-0.015
	(0.085)	(0.026)
Eligibility X	-0.040	0.029
1985 Insurance Rate	(0.046)	(0.019)
Panel C: High-Medicaid/Private-Fee Ratio States (N=19,793)		
Eligibility	0.065**	-0.002
	(0.033)	(0.015)
Eligibility X	0.030	-0.016
1985 Insurance Rate	(0.031)	(0.016)

Notes: Each column presents coefficient estimates from separate regressions of whether a hospital offers a certain type of Cardiac care related technologies in a given year on the fraction of women eligible for Medicaid in the hospital's state and year and eligibility interacted with baseline insurance rates. Panel A includes the full sample of hospitals. Panel B includes hospitals in states with Medicaid to private obstetric fee ratios below 0.41, and Panel C includes hospitals in states with fee ratios above 0.41. All regressions include hospital fixed effects; year dummies; county-level controls for number of births, number of low birth weight births, population, female population by age and per-capita income; state specific linear-year trends; and an interaction between baseline insurance and a linear-year trend. Standard errors are clustered at the state level.

** - p < .05, * - p < .10

Table 7: Robustness Tests

	Baseline	Balanced Panel	All Hospitals	Balanced All Hospital Panel	Weighted Results	Assume NICUs Do Not Close
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Full Sample						
Eligibility	-0.021	-0.015	-0.017	-0.010	-0.033	-0.019
	(0.020)	(0.022)	(0.016)	(0.018)	(0.041)	(0.020)
Eligibility X	-0.057**	-0.066**	-0.040*	-0.053**	-0.144**	-0.055**
1985 Insurance Rate	(0.025)	(0.027)	(0.022)	(0.021)	(0.037)	(0.025)
N	44,613	41,400	62,505	54,538	43,515	44,613
Panel B: Low-Medicaid/Private-Fee-Ratio States						
Eligibility	-0.018	-0.007	-0.006	-0.005	-0.046	-0.016
	(0.030)	(0.034)	(0.028)	(0.031)	(0.043)	(0.027)
Eligibility X	-0.078**	-0.082*	-0.067*	-0.068*	-0.157**	-0.079**
1985 Insurance Rate	(0.040)	(0.044)	(0.036)	(0.036)	(0.064)	(0.036)
N	19,610	18,120	28,981	24,782	19,204	19,610
Panel C: High-Medicaid/Private-Fee-Ratio States						
Eligibility	-0.031	-0.033	-0.030	-0.024	-0.018	-0.030
	(0.026)	(0.027)	(0.025)	(0.023)	(0.041)	(0.025)
Eligibility X	-0.001	-0.011	0.013	-0.007	-0.038	0.003
1985 Insurance Rate	(0.027)	(0.028)	(0.024)	(0.024)	(0.043)	(0.026)
N	20,380	19,068	26,049	23,397	19,801	20,380

Notes: Each column presents coefficient estimates from separate regressions of whether a hospital offers a NICU in a given year on the fraction of women eligible for Medicaid in the hospital's state and year. Column 1 repeats our baseline results. Column 2 uses a balanced sample of hospitals from our baseline sample present in the data for all 12 years. Column 3 uses the full sample of non-federal, general acute-care hospitals in the AHA data, and Column 4 uses the balanced subset of these hospitals present in the data for all 12 years. Column 5 presents results weighted by the number of births delivered by each hospital in 1985. Column 6 constructs the dependent variable by assuming that NICUs do not close. All regressions include hospital fixed effects; year dummies; county-level controls for number of births, number of low-birth-weight births, population, female population by age and per-capita income; state-specific time trends; and an interaction between baseline insurance and a linear-year trend. Standard errors are clustered at the state level.
** - p < .05, * - p < .10

Table 8: Heterogeneity

	Hospital Ownership			Births HHI in County			Predicted Fraction of Other Hospitals in County with a NICU		
	Government	Non-Profit	For Profit	10,000	Above Median	Below Median	No other Hospitals	Below Median	Above Median
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Full Sample									
Eligibility	-0.005	-0.005	-0.125	-0.019	-0.021	-0.027	-0.025	-0.019	-0.039
	(0.029)	(0.027)	(0.103)	(0.015)	(0.037)	(0.053)	(0.017)	(0.032)	(0.043)
Eligibility X	-0.038	-0.088**	-0.041	-0.045*	-0.020	-0.131**	-0.045	-0.013	-0.096*
1985 Insurance Rate	(0.023)	(0.032)	(0.114)	(0.024)	(0.028)	(0.058)	(0.027)	(0.025)	(0.052)
N	13,058	27,753	3,622	19,522	12,546	12,545	17,944	13,335	13,334
Panel B: Low-Medicaid/Private-Fee-Ratio States									
Eligibility	-0.003	-0.010	-0.112	0.024	-0.056	-0.048	0.027	-0.075	-0.049
	(0.039)	(0.037)	(0.093)	(0.039)	(0.073)	(0.042)	(0.049)	(0.062)	(0.043)
Eligibility X	-0.031	-0.107**	-0.046	-0.095**	-0.036	-0.098*	-0.112*	0.019	-0.085*
1985 Insurance Rate	(0.066)	(0.054)	(0.114)	(0.047)	(0.053)	(0.053)	(0.062)	(0.041)	(0.048)
N	4,403	13,785	1,355	6,100	5,650	7,860	5,576	6,134	7,900
Panel C: High-Medicaid/Private-Fee-Ratio States									
Eligibility	-0.011	-0.040	-0.204	-0.039**	-0.047	-0.085	-0.045**	0.030	-0.131
	(0.040)	(0.030)	(0.138)	(0.017)	(0.050)	(0.122)	(0.020)	(0.039)	(0.083)
Eligibility X	-0.046	0.013	0.014	-0.016	0.016	0.016	-0.007	-0.027	-0.008
1985 Insurance Rate	(0.037)	(0.029)	(0.114)	(0.030)	(0.043)	(0.113)	(0.034)	(0.034)	(0.085)
N	7,110	11,719	1,463	11,071	5,663	3,646	10,184	6,018	4,178

Notes: Each column presents coefficient estimates from separate regressions of whether a hospital offers a NICU in a given year on the fraction of women eligible for Medicaid in the hospital's state and year. Columns 1-3 stratify by hospital ownership, Columns 4-6 by the county HHI calculated from market shares of births, and Columns 7-9 by the predicted fraction of other hospitals operating a NICU. All regressions include hospital fixed effects; year dummies; county-level controls for number of births, number of low-birth-weight births, population, female population by age and per-capita income; state-specific time trends; and an interaction between baseline insurance and a linear-year trend. Standard errors are clustered at the state level.
** - p < .05, * - p < .10

Appendix 1: AHA Data-Cleaning Steps

In our full sample of AHA data spanning 1980 to 2000, 6 percent of hospital-year observations have missing values for the number of neonatal intensive- or intermediate-care beds. However, it is often the case that the same hospital will have reported a value for the number of beds for the year before and after the missing value. If a cell is missing intensive- or intermediate-care beds but the values for that hospital in the year before and after are the same, we use that value to fill in the missing cell. This step fills in about half of the missing values in those variables; only about 3 percent of hospital-year observations are missing values for either of the two bed variables in our final data set, which spans 1985 to 1996.

Second, we define the NICU indicator variable as one if the number of total neonatal intermediate- and neonatal intensive-care beds is greater than zero. The NICU indicator is thus initially missing for 3 percent of observations. However, since we only need to know whether the number of beds is positive for this indicator, we can apply the "before and after" rule by looking at the number of beds variable to fill in additional missing values for this indicator variable; this step fills in about one third of the remaining 3 percent of cells with missing values for the NICU indicator. Third, we fill in any additional missing values of the indicator variable if its previous two years have the same value. The above procedures fill in most of the missing values in the data. About 100 to 150 observations (0.13 percent of the total observation) remain missing after this step, and we fill in those remaining missing values with the previous year's value.

In addition to missing values, we also observe a small number of instances of inconsistent reporting of whether or not a hospital has NICU beds from year to year. It is unlikely that a hospital will intermittently have a NICU on a yearly basis, so we use two rules to address this inconsistency in reporting in the created NICU dummy variable. Rule A is to convert zeros to ones if a hospital's reported beds indicate no NICU in three or fewer consecutive years but indicate a NICU in the year (s) right after or before. We then similarly convert ones to zeros if a hospital indicates a NICU in three or fewer consecutive years but no NICU in the surrounding years. Rule B creates an alternative series by reversing these two steps, first converting intervals of ones to zeros and then intervals of zeros to ones. To decide which rule is used for each hospital, we use the count of zeros and ones after we see the first transition (either from one to zero or zero to one) in the raw data. If the number of ones is equal to or bigger than the number of zeros, we apply rule A; otherwise we apply rule B.[31] This correction for intermittent NICU reporting changes less than 1 percent (892/78,824) of observations in the data at the hospital-year level, and about 12 percent (497/4,125) of the hospitals are affected. Also note that we perform this data cleaning on the full panel of data from 1980 to 2000, even though our analysis sample only includes 1985 through 1996. Therefore, even if a short interval of zeros or ones occurs at the beginning or end of our analysis sample, we are able to use additional data from outside the analysis period to verify and correct inconsistent reporting.

Appendix 2: Extending Medicaid-to-physician-fee-ratio series

In order to control for time-varying differences in fee ratios in our sample, we extend the Currie, Gruber, and Fischer (1995) data initially collected for 1985-1993 to include 1994-1996.[32] For 1993 through 1996, we collect Medicaid fees from a survey of states conducted by Bradley Gray and Kosali Simon. The fee we use covers the total OB/GYN reimbursement for a vaginal delivery. Unfortunately, the AHA financial data used to adjust the private fees according to hospital-cost growth are now restricted and not easily obtainable. Instead, we obtain data from the American Medical Association Socioeconomic Monitoring System on physician-office-visit reimbursements. These data report the mean physician-office-visit fee from private payers annually by census division. They also report the annual nationwide average OB/GYN office visit reimbursement. We therefore calculate multiple versions of the Medicaid-

[31] We use a simple example to illustrate how we correct the data using the rules. For example, we observe in the data that a hospital's NICU indicator variable has values across time of 001000100. Applying Rule A leads to 001111100, and applying Rule B leads to 000000000. In the example, the first transition happens at year 3, the count of zeros after year 3 is 5, and the count of ones is 2. Since the hospital has more zeros than ones, we use Rule B to adjust the data.

[32] The numerator of the Currie, Gruber, and Fischer (1995) index captures state-level Medicaid fees paid to OB/GYNs gathered from various sources. The denominator is based on state-level private fees for vaginal deliveries in 1989 as collected by Schwartz, Colby, and Reisinger (1991). Currie, Gruber, and Fischer (1995) then use financial data from the American Hospital Association to inflate and deflate this fee for previous and subsequent years based on hospital-cost growth.

to-private fee ratio for 1993-1996 using three different inflators to project the denominator of Currie, Gruber, and Fischer (1995)'s 1992 value forward. *Fee ratio 1* inflates the denominator utilizing the census-division-level growth rate of physician-office-visit reimbursements. *Fee ratio 2* utilizes the nationwide growth rate of OB/GYN office visit fees. *Fee ratio 3* calculates the average growth rate of the denominator by state from 1985-1992 from Currie, Gruber, and Fischer (1995)'s data and applies this average growth rate to subsequent years by state. Fee data is missing for Alaska, Arizona, Kentucky, Rhode Island, Texas, and Wyoming, so we exclude these states from analysis incorporating these fee indices.

Appendix Figure 1 shows mean Medicaid Obstetric Fees and the three Medicaid-to-private fee ratios over time. Medicaid fees trend upward over the time period, and the ratio trends upward as well until 1993, when it begins to fall, as Medicaid fees grow more slowly than private fees.

Appendix Table 3 presents results controlling for the ratio of Medicaid-to-private obstetric reimbursement rates. Regardless of which version of the fee ratio we use, the results in low- and high-fee ratio states are almost identical to our main estimates. The full sample results do not have a statistically significant interaction effect, mostly driven by the slightly different sample, since fee data are unavailable for six states. The fee ratio itself has a positive but not statistically significant effect on NICU provision. The fact that the coefficient estimates of eligibility and the interaction terms are unchanged suggests that our estimates do not appear to confound any changes to Medicaid payment rates with the changes in Medicaid eligibility associated with the expansions.

Appendix 3: Market Participation and Sample Attrition

This appendix provides more details about the impact of Medicaid expansions on the composition of hospitals. As discussed in the text, entry, exit, and merger are unlikely to impact the number of NICUs in the market. Here we provide additional evidence that Medicaid expansions are also uncorrelated with market participation decisions. We first present estimates from regressions of the number of exits per 100,000 women of childbearing age on eligibility and its interaction with baseline insurance following the specification from Column 7 of Table 3. These results suggest that Medicaid expansions do not have a direct impact on exit for the full sample or for the two subsamples based on relative fee ratios. Because of the significant number of zeroes in the entry and merger data, we focus only on discussing Medicaid eligibility's effect on exit. We have also estimated state-fixed-effect conditional logit models for entry, exit, and merger, and have found consistent results.[33]

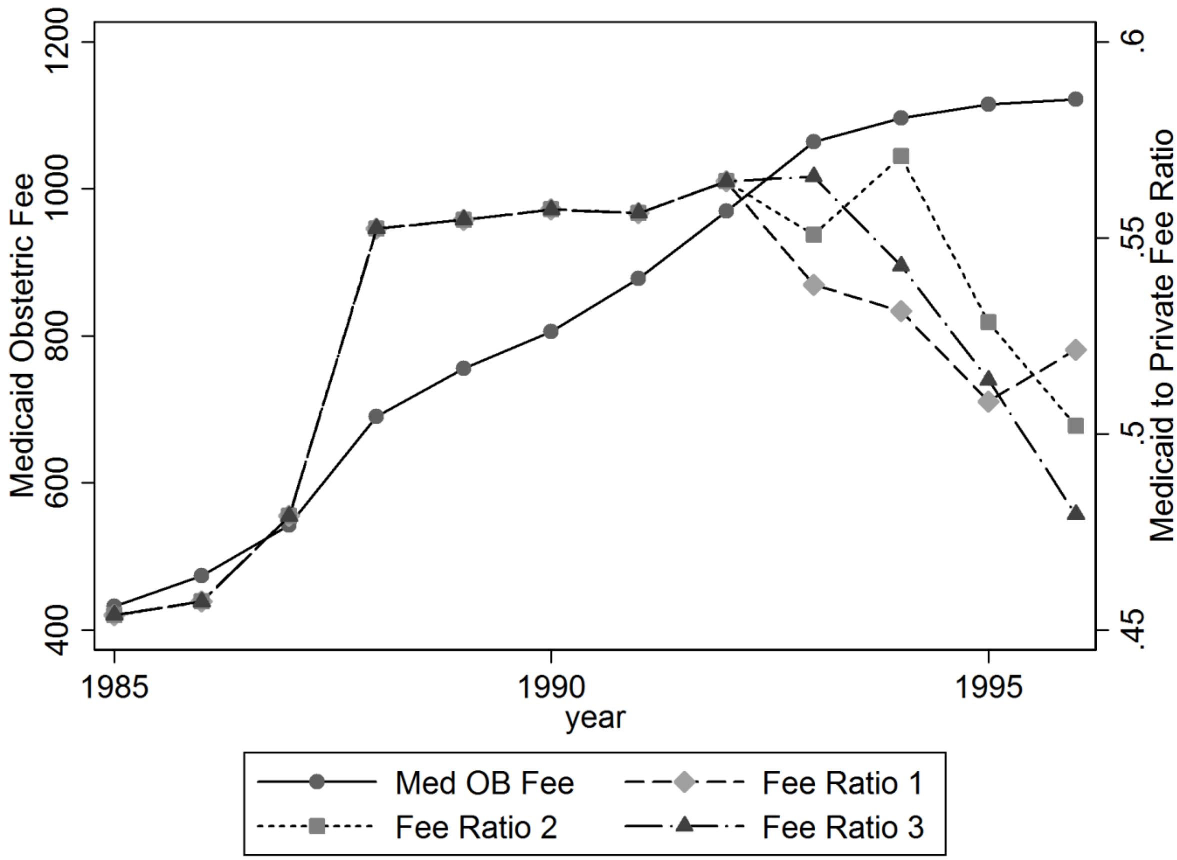
While these results suggest that Medicaid eligibility does not impact general hospital market participation, our analysis sample at the hospital level includes those non-Federal, acute-care, and children's hospitals with an active obstetric unit in 1985. We also ensure that the composition of this specific analysis sample is not correlated with Medicaid eligibility expansions. By definition, this sample does not allow for entry but is impacted by attrition due to exit and merger. Our main hospital-level analysis sample starts with 3,993 hospitals in 1985. By the end of our study period, 543 hospitals have left our sample: 254 are attributed to pure exit and 232 to merger. Most mergers in the data appear as if two hospitals exit from the sample. The remaining sample attrition is due to other events, such as ID or name changes and de-mergers. 105 of these 543 hospitals are operating NICUs when they leave our analysis sample, most of which are being operated by merging hospitals that we no longer track after merger.

To test whether or not attrition of our hospital-level sample is correlated with Medicaid expansions, we also regress the number of "exits" per 100,000 women of childbearing age on our policy variables and controls at the state-year level. Unlike the above analysis, in which we considered only pure exits (closures) from the full sample of hospitals (Column 1 of Appendix Table 4), here exit is simply defined as hospitals leaving our main analysis sample. These results are presented in Column 2 of Appendix Table 4. We find that Medicaid expansions have no statistically significant effect on sample attrition, and the point estimates are small as well. In Column 3 of Appendix Table 4 we also find that the

[33] At the state-year level, 87% of our 612 cells have zero entry, 80% have zero merger, and 56% have zero exits. To estimate our state-fixed-effect conditional logit models for entry, exit, and merger, we define a dummy variable to represent whether a state-year observation has experienced any entrances in the case of entry. We construct similar dummies for exit and merger. We find consistent results for the full sample and the two subsamples (based on fee ratios) that Medicaid expansions have no statistically significant effect on the likelihood of a state-year experiencing entry, exit, or merger.

Medicaid expansions have no effect on the number of hospitals with a NICU in operation leaving the sample each year. These results collectively suggest that sample attrition is unlikely to bias our main estimates.

Appendix Figure 1: Medicaid Obstetric Fee and Medicaid-to Private Fee Ratio, 1985-1996



Notes: Fee Ratio 1 inflates private fees after 1992 using census-division-level physician-office-visit fees. Fee Ratio 2 inflates private fees using nationwide obstetric-office-visit fees. Fee Ratio 3 inflates private fees using the average growth rate of private fees prior to 1992.

Appendix Table 1: Additional Crowd-Out Proxies

	Insurance Interaction	Employment Interaction	% > FPL Interaction
	(1)	(2)	(3)
Panel A: Full Sample (N=44,613)			
Eligibility	-0.021	-0.021	-0.025
	(0.020)	(0.020)	(0.019)
Eligibility X	-0.057**	-0.036	-0.033
Crowd-Out Proxy	(0.025)	(0.028)	(0.023)
Panel B: Low-Medicaid/Private-Fee-Ratio States (N=19,610)			
Eligibility	-0.018	-0.036	-0.024
	(0.030)	(0.031)	(0.028)
Eligibility X	-0.078**	-0.053*	-0.065*
Crowd-Out Proxy	(0.040)	(0.032)	(0.035)
Panel C: High-Medicaid/Private-Fee-Ratio States (N=20,380)			
Eligibility	-0.031	-0.026	-0.028
	(0.026)	(0.028)	(0.028)
Eligibility X	-0.001	0.027	0.006
Crowd-Out Proxy	(0.027)	(0.030)	(0.021)

Notes: Each column presents coefficient estimates from separate regressions of whether a hospital offers a NICU in a given year on the fraction of women eligible for Medicaid in the hospital's state and year. Panel A includes the full sample of hospitals. Panel B includes hospitals in states with Medicaid-to-private obstetric-fee ratios below 0.41, and Panel C includes hospitals in states with fee ratios above 0.41. In Column 1 eligibility is interacted with the hospital's state-level baseline (1985) insurance rate. In Column 2 eligibility is interacted with the county-level baseline (1985) employment rate (1 minus the unemployment rate). In Column 3 eligibility is interacted with the hospital's county-level baseline (average of 1979 and 1989) fraction above the federal poverty line. All regressions include hospital fixed effects; year dummies; county-level controls for number of births, number of low-birth-weight births, infant mortality rate, population, the female population by age, and per-capita income; state-specific linear-year trends; and an interaction between baseline insurance, employment, or fraction above poverty line and a linear-year trend. Standard errors are clustered at the state level.

** - $p < .05$, * - $p < .10$

Appendix Table 2: Effects of State Medicaid Eligibility on NICU Provision by 1985 Medicaid/Private-Fee-Ratio Categories and 1985 Insurance Rate Categories

	(1)
Eligibility X Low Ratio X	0.008
Low Insurance	(0.047)
Eligibility X Low Ratio X	-0.126**
High Insurance	(0.045)
Eligibility X High Ratio X	-0.011
Low Insurance	(0.038)
Eligibility X High Ratio X	-0.040
High Insurance	(0.026)
N	39,990

Notes: This table separates states into four groups based on whether their 1985 Medicaid-to-private obstetric-fee ratio is above or below the median and whether their 1985 insurance rate is above or below the median. Estimates are from a regression of whether a hospital offers a NICU in a given year on the fraction of women eligible for Medicaid in the hospital's state and year interacted with dummy variables for these four categories. Standard errors are clustered at the state level.

** - p < .05, * - p < .10

Appendix Table 3: Controlling for Medicaid/Private Fee Ratios

	Fee Sample	Census Div. Physician Fee Growth	National OB Fee Growth	Projected AHA Fee Growth
	(1)	(2)	(3)	(4)
Panel A: Full Sample (N=39,990)				
Eligibility	-0.033*	-0.037*	-0.038*	-0.038*
	(0.020)	(0.020)	(0.020)	(0.020)
Eligibility X	-0.029	-0.028	-0.027	-0.028
1985 Insurance Rate	(0.022)	(0.022)	(0.022)	(0.022)
Fee Ratio		0.013	0.017	0.017
		(0.014)	(0.014)	(0.014)
Panel B: Low-Medicaid/Private-Fee-Ratio States (N=19,610)				
Eligibility	-0.018	-0.022	-0.021	-0.021
	(0.030)	(0.029)	(0.029)	(0.029)
Eligibility X	-0.078**	-0.077**	-0.077**	-0.077**
1985 Insurance Rate	(0.040)	(0.038)	(0.039)	(0.039)
Fee Ratio		0.013	0.011	0.009
		(0.021)	(0.021)	(0.021)
Panel C: High-Medicaid/Private-Fee-Ratio States (N=20,380)				
Eligibility	-0.031	-0.037	-0.040	-0.041
	(0.026)	(0.028)	(0.027)	(0.028)
Eligibility X	-0.001	0.002	0.003	0.003
1985 Insurance Rate	(0.027)	(0.027)	(0.027)	(0.027)
Fee Ratio		0.019	0.029	0.028
		(0.020)	(0.020)	(0.020)

Notes: Each column presents coefficient estimates from separate regressions of whether a hospital offers a NICU in a given year on the fraction of women eligible for Medicaid in the hospital's state and year. Each column includes a control of alternative versions of Medicaid-to-private obstetric fees described in Appendix 2. All regressions include hospital fixed effects; year dummies; county-level controls for number of births, number of low-birth-weight births, population, female population by age, and per-capita income; state-specific time trends; and an interaction between baseline insurance and a linear-year trend. Standard errors are clustered at the state level.
** - p < .05, * - p < .10

Appendix Table 4: Medicaid Eligibility and Hospital Exit

	Hospital Exit from Sample of All General, Acute-Care Hospitals	Analysis Sample Attrition	Analysis Sample Attrition w/ NICUs
	(1)	(2)	(3)
Panel A: Full Sample (N=612)			
Eligibility	-0.178	-0.059	-0.035
	(0.130)	(0.145)	(0.056)
Eligibility X	0.062	-0.029	-0.014
1985 Insurance Rate	(0.136)	(0.138)	(0.032)
Panel B: Low-Medicaid/Private-Fee-Ratio States (N=264)			
Eligibility	-0.055	-0.133	-0.070
	(0.116)	(0.173)	(0.074)
Eligibility X	-0.036	-0.074	-0.025
1985 Insurance Rate	(0.104)	(0.150)	(0.054)
Panel C: High-Medicaid/Private-Fee-Ratio States (N=276)			
Eligibility	-0.262	0.043	-0.043
	(0.260)	(0.313)	(0.108)
Eligibility X	0.077	-0.074	0.015
1985 Insurance Rate	(0.273)	(0.299)	(0.077)

Notes: Each column presents coefficient estimates from separate regressions. The dependent variables are calculated at the state-year level and scaled by the number of women of childbearing age (in 100,000s). Column 1 includes exits from the sample of all non-federal, general acute-care hospitals in the AHA data. Columns 2 and 3 include the raw number of hospitals leaving the analysis sample and the number of exiting hospitals operating a NICU, respectively. Panel A includes all states. Panel B includes states with Medicaid-to-private obstetric-fee ratios below 0.41, and Panel C includes states with fee ratios above 0.41. All regressions include state and year fixed effects; an interaction between baseline insurance and a linear-year trend; state-level controls for number of births, number of low-birth-weight births, population, and female population by age and per-capita income; and state-specific time trends. Standard errors are clustered at the state level.

** - p < .05, * - p < .10