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# THE AGGREGATE EFFECTS OF HEALTH INSURANCE: EVIDENCE FROM THE INTRODUCTION OF MEDICARE*

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This paper investigates the effects of market-wide changes in health insurance by examining the single largest change in health insurance coverage in American history: the introduction of Medicare in 1965. I estimate that the impact of Medicare on hospital spending is over six times larger than what the evidence from individual-level changes in health insurance would have predicted. This disproportionately larger effect may arise if market-wide changes in demand alter the incentives of hospitals to incur the fixed costs of entering the market or of adopting new practice styles. I present some evidence of these types of effects. A back of the envelope calculation based on the estimated impact of Medicare suggests that the overall spread of health insurance between 1950 and 1990 may be able to explain about half of the increase in real per capita health spending over this time period.

## I. INTRODUCTION

Over the last half-century, the dramatic rise in medical expenditures has been one of the most salient features of the U.S. health care sector. Total health care expenditures in the United States as a share of GDP have more than tripled, from about 5

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percent in 1960 to about 16 percent in 2004 [CMS 2004]. Early work by Feldstein [1971, 1977] suggested that the spread of health insurance was a primary cause of the rapid rise in health spending. Such arguments prompted the undertaking of the Rand Health Insurance Experiment, one of the largest randomized, individual-level social experiments ever conducted in the United States, to investigate the impact of health insurance on health care utilization and spending [Manning et al. 1987]. Its findings suggested that the responsiveness of health spending to health insurance was substantially smaller than what Feldstein [1971, 1977] had estimated, and consequently, that the spread of health insurance was not an important cause of the rise in health spending [Manning et al. 1987; Newhouse 1992; Newhouse et al. 1993]. Today, the results of the Rand Experiment are generally accepted as the gold standard, and are widely used in both academic and applied contexts [Cutler and Zeckhauser 2000; Zweifel and Manning 2000].

This paper revisits this debate and suggests that the spread of health insurance may have played a much larger role in the growth of health spending than the Rand Experiment would suggest. The basic insight is that market-wide changes in health insurance may have fundamentally different effects on the health care sector than what partial equilibrium analyses such as the Rand Experiment would suggest.

To study the impact of market-wide changes in health insurance, I examine the impact of the introduction of Medicare in 1965. Medicare's introduction constituted the single largest change in health insurance coverage in American history. Medicare is currently one of the largest health insurance programs in the world, providing health insurance to forty million people and comprising one-eighth of the federal budget and 2 percent of GDP [US Congress 2000; National Center for Health Statistics 2002; Newhouse 2002]. Yet we know surprisingly little about the impact of its introduction. Indeed, to my knowledge, the only existing evidence comes from a comparison of time series patterns of health expenditures before and after its introduction [Feldstein and Taylor 1977].

I use the fact that the elderly in different regions of the country had very different rates of private health insurance coverage prior to Medicare to identify its effect. I study the impact of Medicare on the hospital sector. This was the single largest component of health spending at the time of Medicare's introduction and of the subsequent growth in health spending. My estimates suggest that, in its first five years, the introduction of Medicare was associated with an increase in spending that was over six times larger than what the estimates from the Rand Health Insurance Experiment would have predicted. They also suggest that the long-term impact of Medicare may have been even larger than its five-year impact.

One reason why the general-equilibrium impact of a market-wide change in health insurance may be much larger than what partial-equilibrium analysis would suggest is that market-wide changes in health insurance can fundamentally alter the nature and character of medical practice in ways that small-scale changes will not. Consistent with this, I find that the introduction of Medicare is associated with substantial new hospital entry. I also find some suggestive evidence that Medicare's introduction is associated with increased adoption of cardiac technologies and increased spending on non-Medicare patients; however, because of data limitations discussed below, these results are necessarily more speculative in nature than the other findings of the paper.

Extrapolation from the Rand estimates of the impact of health insurance on health spending suggests that the overall spread of health insurance between 1950 and 1990 can explain only a very small part of the six fold rise in real per capita health spending over this period [Manning et al. 1987; Newhouse 1992]. The results of the same exercise using my estimated impact of Medicare suggest that the overall spread of health insurance may be able to explain half of the increase in health spending over this period. Of course, important concerns about external validity suggest that the findings of each of these back of the envelope calculations should be viewed with considerable caution. Nonetheless, at a broad level, my findings raise the possibility that the spread of health insurance—and the public policies that encouraged it—may have played a much larger role in the substantial growth in the health care sector over the last half century than the current conventional wisdom suggests. At the same, however, my findings are not inconsistent with the conventional wisdom that technological change is the primary cause of the rapid rise in health expenditures (e.g., Newhouse [1992]; Fuchs [1996]; Cutler [2003]). The large impact of market-wide changes in health insurance on health spending may stem in part from their impact on decisions to adopt new medical technologies, as conjectured by Weisbrod [1991].

A complete picture of the impact of an aggregate change in health insurance requires an understanding not only of its impact on the health care sector—the subject of this paper—but also of its benefits to consumers. In related work, Finkelstein and McKnight [2005] explore these potential benefits. We find that while the introduction of Medicare appears to have had no impact on elderly mortality in its first ten years, it did substantially reduce the right tail of out of pocket medical spending by the elderly.

The rest of the paper proceeds as follows. Section II describes the data and empirical strategy. Section III presents estimates of the effect of Medicare on the hospital sector. Section IV shows that these estimates are substantially larger than what existing partial equilibrium estimates would have predicted; it also presents some evidence in support of the likely explanations. Section V provides a back of the envelope calculation for what the estimates imply for the contribution of the spread of health insurance to the growth of the health care sector over the last half century. The last section concludes.

## II. Studying the Impact of Medicare: Approach and Data

### *II.A. Identifying the Impact of Medicare: Geographic Variation in Pre-Medicare Insurance Coverage*

Medicare, enacted July 1, 1965 and implemented July 1, 1966, provided universal public health insurance coverage for the elderly. It covered hospital and physician expenses; the services covered and the reimbursement rates were very generous for the time [Somers and Somers 1967; Newhouse 2002].

Prior to Medicare, public health insurance coverage was practically nonexistent, and meaningful private health insurance for the elderly was also relatively rare [United States Senate 1963; Anderson and Anderson 1967; Epstein and Murray 1967; Stevens and Stevens 1974]. On the basis of data from the 1963 National Health Survey (NHS), I estimate that in 1963, only 25 percent of the elderly had meaningful (i.e., Blue Cross) private hospital insurance.[1] Upon the implementation of Medicare, hospital insurance coverage for the elderly rose virtually instantaneously to almost 100 percent [US HEW 1969].

The impact of Medicare on elderly insurance coverage varied considerably across the country. Through a special request, I obtained a version of the 1963 NHS that identifies in which of the eleven subregions the individual is located. Broadly speaking, insurance coverage for the elderly is higher in the North East and North, and lower in the South and West. Table I indicates that the proportion of the elderly without Blue Cross hospital insurance ranged from a low of 49 percent in New England to a high of 88 percent in the East South Central United States. The available data suggest that this geographic pattern was quite stable in the years prior to Medicare (see National Center for Health Statistics [1960]).

A key criterion for using geographic variation in private insurance coverage for the elderly to identify the impact of Medicare is that this insurance was redundant of what Medicare subsequently covered. Consistent with this, Lichtenberg [2002] and Finkelstein and McKnight [2005] present evidence of a substantial crowd-out effect of Medicare's introduction on private health insurance spending. The ability to identify Blue Cross insurance is also particularly important in this regard, as Medicare's benefit and reimbursement structure was explicitly modeled on the Blues [Anderson et al. 1963; Epstein and Murray 1967; Stevens and Stevens 1974; Ball 1995; Stevens 1999; Newhouse 2002].

### *II.B. Data: The American Hospital Association Annual Survey*

I use twenty-six years of hospital-level data from the annual surveys of the American Hospital Association (AHA) for every AHA-registered hospital in the U.S. These data, which are available in hard-copy in the annual August issues of *Hospitals: The Journal of the American Hospital Association*, cover the years from 1948 to 1975 (with the exception of 1954). The AHA data from the 1980s and later have been widely used to study the hospital sector (e.g., Cutler and Sheiner [1998];

1. Most private insurance at this time was extremely minimalist in nature, but Blue Cross plans had relatively comprehensive coverage (e.g., Anderson et al. [1963]; Stevens and Stevens [1974]). For more information on the 1963 NHS, see NCHS [1964]. I am extremely grateful to Will Dow for his work unearthing these data.

TABLE I
SHARE OF ELDERLY WITHOUT HOSPITAL INSURANCE, 1963

| | Blue Cross | Any insurance |
|---|---|---|
| New England (CT, ME, MA, NH, RI, VT) | 0.49 | 0.37 |
| Middle Atlantic (NJ, NY, PA) | 0.60 | 0.41 |
| East North Central, Eastern Part (MI, OH) | 0.55 | 0.32 |
| East North Central, Western Part (IL, IN, WI) | 0.75 | 0.42 |
| West North Central (IA, KS, MN, MO, NE, ND, SD) | 0.81 | 0.47 |
| South Atlantic, Upper Part (DE, DC, MD, VA, WV) | 0.75 | 0.45 |
| South Atlantic, Lower Part (FL, GA, NC, SC) | 0.81 | 0.50 |
| East South Central (AL, KY, MS, TN) | 0.88 | 0.57 |
| West South Central (AR, LA, OK, TX) | 0.85 | 0.55 |
| Mountain (AZ, CO, ID, MT, NV, NM, UT, WY) | 0.78 | 0.50 |
| Pacific (OR, WA, CA, AK, HI) | 0.87 | 0.52 |
| National Total | 0.75 | 0.45 |

Data are from individuals aged 65 and over in the 1963 National Health Survey. Sample size is 12,757. Minimum sample size for a subregion is 377.

Baker and Phibbs [2002]). However, the historical data have been largely ignored.

I exclude the approximately 5 percent of hospitals that are federally owned, producing a sample of about 6,500 hospitals per year. The analysis centers on six hospital outcomes: total expenditures, payroll expenditures, employment, beds, admissions, and patient days. Utilization and bed data are exclusive of newborns. I convert all expenditure variables to 1960 dollars using the CPI-U. Hospital expenditures consist of expenditures on inputs, and do not reflect hospital output prices. Employment and payroll expenditures exclude most physicians, since they are not employed directly by the hospital. The appendix provides a more detailed description of these variables and of the data quality.

Figure I shows the national time series patterns for each outcome based on aggregating the hospital-level data to the national level. Most outcomes are increasing over the entire sample period. However, beds and patient days began decreasing in the early 1960s as short-term hospitals took over many of the functions previously performed by long-term hospitals, such as treatment of tuberculosis patients [Somers and Somers 1967]; prior to this decline, long-term hospitals constituted above 10 percent of hospitals, but half of beds and patient days.

Table II reports mean hospital outcomes prior to Medicare

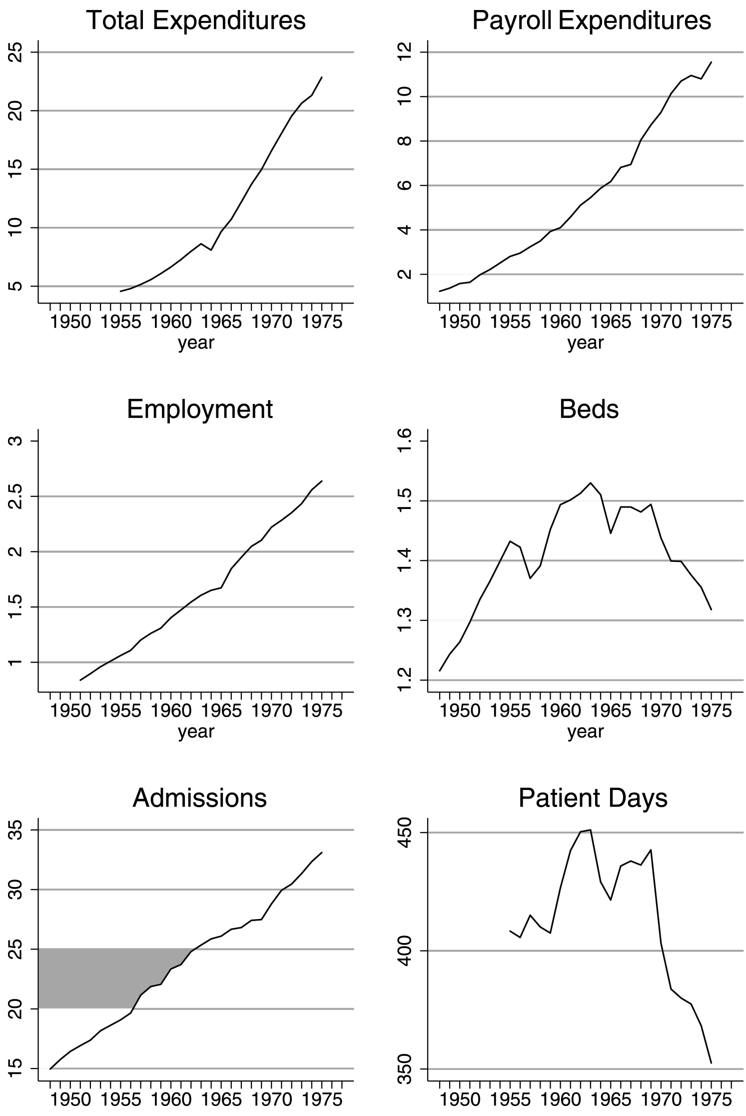


FIGURE I
National Time Series Patterns

Figure I graphs the national aggregates from the hospital-level data described in the text. *Y*-axis scale is in millions, except for expenditure variables for which it is in billions of constant (1960) dollars.

TABLE II
DESCRIPTION OF DEPENDENT VARIABLES FROM THE AHA DATA

| Outcome category | Dependent variable | First year data present | Sample mean (1962–1964) Full sample | North and Northeast | South and West |
|---|---|---|---|---|---|
| Expenditures | Total | 1955 | 1,486 | 1,782 | 1,180 |
| ($1960, ’000) | Payroll | 1948 | 976 | 1,196 | 748 |
| Major Inputs | Beds | 1948 | 228 | 283 | 174 |
| | Employment | 1951 | 253 | 307 | 200 |
| Utilization | Inpatient admissions | 1948 | 4,004 | 4,307 | 3,701 |
| | Inpatient days | 1955 | 70,371 | 88,303 | 52,370 |

All variables are measured annually at the hospital level for nonfederal hospitals. Employment and payroll expenditures exclude residents and interns. Expenditures are measured in thousands of (1960) dollars.

(1962–1964). It shows that prior to Medicare, average hospital outcomes were consistently higher in the North and NorthEast (where insurance coverage was comparatively high) than in the South and West (where insurance coverage was comparatively low). This is consistent with the evidence in the paper of an impact of insurance coverage on these outcomes, but may also reflect other differences across regions.

## III. IMPACT OF MEDICARE ON HOSPITAL UTILIZATION, INPUTS, AND SPENDING

### *III.A. Econometric Model*

The empirical strategy is to compare changes in outcomes in regions of the country where Medicare had a larger effect on the percentage of the elderly with health insurance to areas where it had less of an effect. Since this approach will not capture any effect of Medicare on the previously insured that operates via Medicare’s income effect, it will underestimate the full impact of Medicare.

Of course, private insurance rates prior to Medicare are not randomly assigned. Data from the 1960 census indicate that differences in socio-economic status can explain a substantial share of the variation in insurance coverage across subregions. Areas that differ in their socio-economic status may also differ in their level or growth of health care utilization and spending. The empirical approach is therefore to look at whether there is a break in any pre-existing differences in the level or trend of these outcomes around the time of Medicare's introduction in 1966. The identifying assumption is that absent Medicare, any preperiod differences would have continued on the same trends.

The basic estimating equation is

$$(1)\quad \log(y_{ijt}) = \alpha_j \times \mathbf{1}(\text{county}_j) + \delta_t \times \mathbf{1}(\text{Year}_t) + \sum_{t=1948}^{t=1975} \lambda_t(\text{Mcareimpact}_z) \times \mathbf{1}(\text{Year}_t) + X_{st}\beta + \varepsilon_{ijt}$$

The dependent variable is the log of outcome $y$ in hospital $i$ in county $j$ and year $t$; a level specification would constrain the outcomes to grow by the same absolute amount each year, which would be inappropriate given the considerable variation in size across hospitals. $\mathbf{1}(\text{County}_j)$ are county fixed effects; these control for any fixed differences across counties. $\mathbf{1}(\text{Year}_t)$ are year fixed effects; these control for any nationwide year effects. $\text{Mcareimpact}_z$ measures the percentage of the elderly in subregion $z$ without private Blue Cross hospital insurance in 1963 (Table I).

To account for possible serial correlation over time within areas, I allow for an arbitrary variance–covariance matrix in the error structure within each hospital market.[2] The existing literature suggests the use of the standard metropolitan statistical area (SMSA), defined in 1960, to approximate the hospital market (see, e.g., Makuc et al. [1991]; Dranove et al. [1992]; Gaynor and Vogt [2000]). This produces 210 separate markets; I also include fifty additional markets for the rural (non-SMSA) areas in each state.

The key variables of interest are the interactions of the year fixed effects with $((\text{Mcareimpact}_z) * 1(Year_t))$. The pattern of coefficients on these variables—the $\lambda_t$'s—shows the flexibly estimated pattern over time in the dependent variable in areas where Medicare had a larger impact on insurance coverage relative to areas where it had a smaller impact. The change in the trend of these $\lambda_t$'s before and after the introduction of Medicare provides an estimate of Medicare's impact. Crucially, (1) does not privilege 1965 relative to other years. Because I do not impose any ex-ante

2. Clustering at the market level allows for directly comparability of these results with those of subsequent analyses estimated at the market level. In practice, however, $p$-values are very similar if I instead cluster at the state level (see Finkelstein [2005]), or implement the randomized inference procedure described by Bertrand et al. [2004].

restrictions on when any structural breaks may occur, I allow the data to show where changes in the time pattern—if any—actually occur, and can gauge whether Medicare may plausibly have played a role.

To alleviate concerns that other things might also have been changing differentially over time across different areas of the country, (1) also includes a series of time-varying state-level covariates ($X_{st}$). Of particular concern is the potential confounding impact of Medicaid that, like Medicare, was also enacted in 1965. Medicaid accounted for less than one-third of combined Medicare and Medicaid hospital spending in the early 1970s [National Center for Health Statistics 2002].

Because the timing of Medicaid implementation—unlike Medicare—was left up to the individual states, I can try to separately control for any impact of Medicaid.[3] In all of the analyses, I therefore include a series of eight indicator variables for the number of years since (or before) the implementation of a Medicaid program in state $s$.[4] In practice, the estimated effects of Medicare are not sensitive to including these controls for Medicaid implementation. Of course, even controlling for the timing of Medicaid's introduction, (1) may overestimate the impact of Medicare if the size of a state's Medicaid program is positively correlated with $\text{Mcareimpact}_z$. In practice, however, there is a weak negative correlation between Medicaid spending per capita and $\text{Mcareimpact}_z$, reflecting the fact that states in the North and Northeast implemented more generous Medicaid programs [US Department of Health, Education and Welfare 1968; Stuart 1972].

Finally, it is important to highlight two limitations to using the results from (1) to gauge the aggregate impact of Medicare on national spending and utilization. First, hospital units of varying size are given equal weight in the estimation,

3. By July 1, 1966 (the date that Medicare was implemented) 22 states—accounting for half the national population—had implemented Medicaid. By January 1967 26 states (62 percent of the population) had implemented Medicaid. These numbers increased to thirty-seven states (77 percent of the population) by January 1968, 40 states (80 percent of the population) by January 1969, and forty-nine states (99 percent of the population) by January 1970 (US Department of Health, Education and Welfare [1970], Gruber [2003], and population estimates from the 1960 census).

4. Although in principle, estimates of (1) could shed light on the impact of Medicaid, in practice, the results suggest that the timing of state implementation of Medicaid was not random with respect to hospital outcomes, and analysis does not yield stable estimates of the impact of Medicaid.

but might have different responses to Medicare. Second, estimation at the hospital level may not capture any impact of Medicare that occurs via an effect on hospital entry and exit. Therefore, when I turn to the aggregate impact of Medicare in section III.D below, I present additional results aggregated to the hospital market level—which captures any net effects of hospital entry and exit—and weighted by market size. I also directly investigate the impact of Medicare on hospital entry and exit in section IV.B.[5]

### *III.B. Basic Results*

The core empirical findings are readily apparent in Figure II, which shows the $\lambda_t$'s from estimating (1) for six (log) dependent variables: admissions, patient days, employment, beds, payroll expenditures, and total expenditures. The time pattern of the $\lambda_t$'s identifies changes in the dependent variable in areas in which Medicare had a larger impact on insurance coverage *relative* to areas in which Medicare had a smaller impact. The dashed lines indicate the 95 percent confidence interval for each coefficient, which naturally increases with the distance from the reference year 1963. A vertical line demarcates 1965, the year in which Medicare was enacted.[6]

Consider first the results for hospital admissions (the upper-left graph in Figure II). There is a general downward trend over time in the $\lambda_t$'s up through 1965. This indicates that, prior to 1965, hospital admissions were not growing as fast in low insurance areas relative to high insurance areas. However, there is a dramatic reversal in this pattern after 1965, at which point admissions start to grow at the same rate or faster in the areas where Medicare's introduction had a larger impact on insurance coverage. The other five hospital outcomes examined in Figure II show the same pattern of a dramatic reversal of a generally downward or flat trend after 1965. The estimates for payroll and total expenses are somewhat noisier than the other estimates, which may reflect the greater noise in the expenditure measures

5. As I discuss below, estimation at the market level has its own disadvantage, namely increased noise in aggregated sums due to nontrivial amounts of missing data.

6. Data from year $t$ are from the survey period October $(t - 1)$ to September $(t)$. Since Medicare was enacted in July 1965 and implemented in July 1966, the year 1965 (i.e., Oct 1964 to Sept 1965) is treated as the year prior to Medicare. Any effects detected in 1966 (i.e., Oct 1965 to Sept 1966) may be anticipation or actual effects.

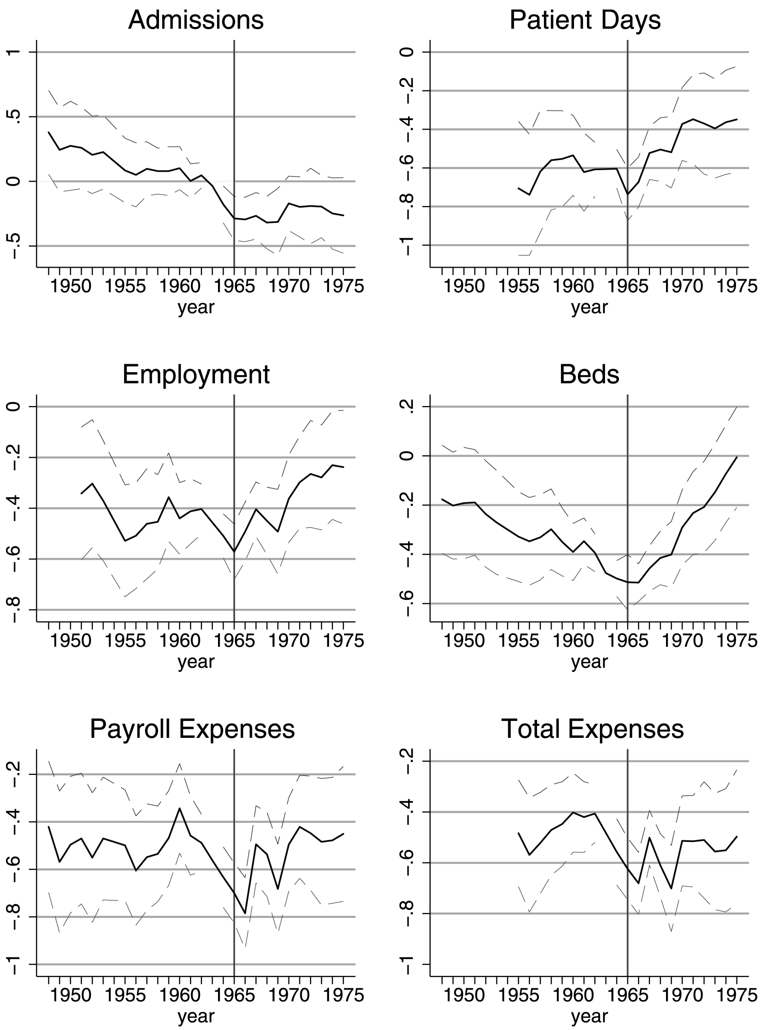


FIGURE II
Baseline Specification

Figure II graphs the pattern of the $\lambda_t$ coefficients from estimating (1) for the log of the dependent variable given above each graph. The scale of the graph is normalized so that in the reference year (1963) it is the average difference in the dependent variable between the south and west (where Medicare had a larger impact) relative to the north and northeast (where Medicare had a smaller impact). The dashed lines show the 95 percent confidence interval on each coefficient relative to the reference year (1963). Time varying state-level controls ($X_{st}$) in all analyses consist of eight indicator variables for the number of years before (or since) the implementation of Medicaid in state $s$ (see text for more details).

(see Appendix). The existence of a prior relative trend across more and less affected areas is not surprising given the differences across these areas in the level of hospital activity (and other characteristics) prior to Medicare, although the sign of this relative trend was not obvious *a priori*.

Motivated by the graphical results, I perform a variety of statistical tests of the $n$-year change in $\lambda_t$ after the introduction of Medicare relative to the $n$-year change in $\lambda_t$ before the introduction of Medicare. For example, the impact of Medicare in the first five years is calculated as follows:

$$\Delta 5 \equiv (\lambda_{1970} - \lambda_{1965}) - (\lambda_{1965} - \lambda_{1960}). \tag{2}$$

$\Delta 5$ thus denotes the five-year change in the hospital outcome after the introduction of Medicare relative to the five-year change prior to the introduction of Medicare in areas where Medicare had a greater impact on insurance coverage relative to areas where it had a smaller impact. The first three rows of Table III report the estimates for the two-year, five-year, and ten-year change in the outcome, respectively; $p$-values are reported in parentheses below each estimate. The results indicate that the introduction of Medicare is associated with a statistically significant increase in all of the dependent variables.

Because the reference (or pre-) period varies across the two-year, five-year, and ten-year tests, comparisons across the tests should not be interpreted as different effects at different time intervals. To compare the effects in different time intervals, the fourth row of Table III repeats the five-year test in the second row for the second five-year period 1970–1975, using the same reference period (1965–1960) as the first five year test. The results indicate that Medicare is associated with a further statistically significant increase in all of the outcomes in the second five year period.

The results from the first five-year test indicate an effect of Medicare for log admissions of 0.504, and for log total expenditures of 0.332. To translate these into the implied national impact of Medicare, I multiple them by 0.75, since nationwide, Medicare increased the proportion of the elderly with insurance coverage by 75 percentage points. The results therefore imply that the introduction of Medicare is associated with a nationwide increase between 1965 and 1970 in admissions and total spending of, respectively, 46 percent ($\sim$[exp(0.504 $\times$ 0.75) $-$ 1]), and 28 percent ($\sim$ [exp(0.332 $\times$ 0.75) $-$ 1]). In Section III.D below, I discuss some limitations to using the results in Table III to estimate the

TABLE III
IMPACT OF MEDICARE ON HOSPITAL BEHAVIOR

| | Utilization | | Inputs | | Expenditures | |
|---|---|---|---|---|---|---|
| | Log admissions | Log patient days | Log employment | Log beds | Log payroll expenditures | Log total expenditures |
| 1. First 2 years: (1967–1965 vs. 1965–1963) | 0.272** (0.040) | 0.402*** (0.0004) | 0.253*** (0.001) | 0.094 (0.363) | 0.342*** (0.003) | 0.279*** (0.006) |
| 2. First 5 years: (1970–1965 vs. 1965–1960) | 0.504*** (0.004) | 0.567*** (0.0001) | 0.340*** (0.009) | 0.346*** (0.006) | 0.560*** (0.0001) | 0.332** (0.011) |
| 3. First 10 years: (1975–1965 vs. 1965–1955) | 0.394* (0.096) | 0.419** (0.043) | 0.376** (0.015) | 0.693*** (0.0000) | 0.449*** (0.008) | 0.267* (0.093) |
| 4. Second 5 years: (1975–1970 vs. 1965–1960) | 0.295* (0.057) | 0.226* (0.096) | 0.256** (0.017) | 0.408*** (0.0001) | 0.401*** (0.003) | 0.239* (0.051) |
| *N* | 161,146 | 125,737 | 144,042 | 171,883 | 139,275 | 115,005 |

Table reports results from estimating (1) and calculating test statistics as shown in (2). Column heading shows dependent variable. Time varying state-level controls ($X_{st}$) in all analyses consist of eight indicator variables for the number of years before (or since) the implementation of Medicaid in state $s$ (see text for more details). $p$-values are in parentheses and are calculated allowing for an arbitrary variance–covariance matrix within each hospital market.

***, **, * denotes significance at the 1 percent, 5 percent, and 10 percent level, respectively. Sample covers first year of data availability through 1975. First year of data availability ranges from 1948 to 1955 depending on outcome; see Table II for details. Differences in sample size across the columns primarily reflect different starting years; however, to some extent they also reflect different proportions of missing data (see Appendix). Results are not sensitive to limiting all variables to a common sample.

implied national impact of Medicare, and present and discuss some alternative estimates.

### *III.C. Robustness*

I investigated the robustness of the preceding results to a number of alternative specifications. Overall the results were quite robust. This section briefly summarizes some of the more important robustness tests. Many of the results are presented in Table IV, where, to conserve space, I only report the five-year estimates. To make the results comparable across different specifications, I present the implied five-year impact of Medicare. Row 1 therefore takes the baseline results from Table III and multiplies them by 0.75 since, nationwide, Medicare increased the percent of the elderly by 75 percentage points.

A primary concern is the validity of the identifying assumption that absent the introduction of Medicare, the different sub-

TABLE IV
IMPACT OF MEDICARE ON HOSPITAL BEHAVIOR; ALTERNATIVE SPECIFICATIONS

| | Utilization | | Inputs | | Expenditures | |
|---|---|---|---|---|---|---|
| | Log admissions | Log patient days | Log employment | Log beds | Log payroll expenditures | Log total expenditures |
| 1. Baseline specification | 0.378*** (0.004) | 0.425*** (0.0001) | 0.255*** (0.009) | 0.260*** (0.006) | 0.420*** (0.0001) | 0.249** (0.011) |
| 2. State-specific linear trends | 0.355*** (0.006) | 0.490*** (0.0000) | 0.260*** (0.007) | 0.266*** (0.005) | 0.396*** (0.0002) | 0.254** (0.011) |
| 3. Additional Time varying covariates | 0.433*** (0.002) | 0.452*** (0.0001) | 0.283*** (0.005) | 0.251** (0.011) | 0.438*** (0.0002) | 0.322*** (0.001) |
| 4. Without 4 Southern subregions | 0.485*** (0.001) | 0.560*** (0.0000) | 0.234** (0.030) | 0.360*** (0.0004) | 0.329*** (0.005) | 0.264** (0.015) |
| 5. Urban- and rural-specific insurance | 0.390*** (0.0004) | 0.443*** (0.0000) | 0.260*** (0.004) | 0.318*** (0.0000) | 0.380*** (0.0001) | 0.254** (0.034) |
| 6. Indicator for ≥ 75% without Blue Cross insurance | 0.348*** (0.005) | 0.421*** (0.0001) | 0.235** (0.017) | 0.278*** (0.003) | 0.386*** (0.0003) | 0.192* (0.057) |
| 7. % without any private insurance | 0.275** (0.050) | 0.376*** (0.0005) | 0.270** (0.013) | 0.188* (0.063) | 0.438*** (0.0008) | 0.243** (0.019) |
| 8. % without Blue Cross insurance * % elderly | 0.338*** (0.007) | 0.418*** (0.0001) | 0.134 (0.144) | 0.252*** (0.006) | 0.310** (0.022) | 0.196* (0.065) |

Table reports implied impact of Medicare from estimating a variant of the baseline specification of (1), performing the "First 5 Years" test in (2), and the translating the test statistic into the implied impact of Medicare. This last step is done so that the estimates are directly comparable across specifications. $p$-values are in parentheses and are calculated allowing for an arbitrary variance–covariance matrix within each hospital market. ***, **, * denotes significance at the 1 percent, 5 percent, and 10 percent level, respectively.

Row 1: Baseline specification: Estimates are as shown in Table III, then multiplied by 0.75 (the nationwide impact of Medicare on insurance coverage as measured by percent of elderly without Blue Cross insurance in 1963). Time varying state-level controls ($X_{st}$) consist of eight indicator variables for the number of years before (or since) the implementation of Medicaid in state $s$ Sample covers first year of data availability through 1975. First year of data availability ranges from 1948 to 1955 depending on outcome; see Table II for details.

Row 2: Row 1 with state-specific linear trends included in the regression.

Row 3: Row 1 with additional time-varying state covariates (real per capita income, infant mortality rate, violent crime, and population) added to the regression.

Row 4: Row 1 without 4 Southern subregions (and therefore multiplying by 0.7, the average % of the elderly without Blue Cross insurance in the non-Southern United States in 1963).

Row 5: Row 1 but with urban- and rural-specific insurance rates within each subregion (see text for details).

Row 6: $\text{Mcareimpact}_z$ in (1) is measured with an indicator variable for subregion has ≥75% of elderly without Blue Cross insurance rather than a linear measure of % of the elderly without Blue Cross insurance as in the baseline specification. Estimates are multiplied by 2.7 since on average areas ≥75% or more of elderly without Blue Cross insurance have 28 percentage points less insurance than areas where <75% of the elderly are without Blue Cross insurance.

Row 7: $\text{Mcareimpact}_z$ is measured as the percent of the elderly without *any* private hospital insurance. Estimates are therefore multiplied by 0.45 (the percent of the elderly without any hospital insurance).

Row 8: $\text{Mcareimpact}_z$ uses variation in % of population that is elderly as well as % of elderly without Blue Cross insurance. Estimates are therefore multiplied by 0.15 (i.e., 0.75 × 0.20 where 0.75 is the average percentage point increase in elderly insurance due to Medicare and 0.20 is the average share of the elderly in hospital expenditures).

regions of the country would not have exhibited different changes in growth relative to their pre-Medicare growth patterns. Figure II suggests that in no year prior to 1965 (or after it for that matter) is there evidence of the dramatic reversal in trend in all outcomes that occurs after 1965. To examine this more systematically, I limit the data to the years prior to 1966 and re-estimate (1) and the two-year and five-year tests from Table III if—counter to fact—I assign some year prior to 1966 as the year in which Medicare was implemented. I tend not to find statistically or substantively large effects from these "false tests," which is broadly supportive of the identifying assumption (see Finkelstein [2005] for results). Further supporting the identifying assumption, row 2 of Table IV shows that the results are robust to including state-specific linear trends in (1), and row 3 shows that the results are robust to including additional time-varying covariates for real per capita state income, state infant mortality rate, the rate of violent crime, and state population.[7] Finally, since the introduction of Medicare coincided with a period of enormous social upheaval in the South—including the civil rights movement and the Hill Burton hospital construction program—row 4 shows that the results are robust to excluding the four southern subregions (about one-third of hospitals) from the sample. More generally, the results are robust to omitting any given subregion from the sample (see Finkelstein [2005] for results).

A related concern is that the estimated impact of Medicare might in part reflect the impact of increases in private health insurance for other age groups. However, I find no indication in the 1959, 1963, and 1970 NHS surveys of a relative increase in nonelderly private health insurance after Medicare's introduction relative to before in the more affected census regions relative to less affected census regions.

A final set of sensitivity analyses uses alternative sources of cross-sectional heterogeneity in the impact of Medicare on insurance coverage. Row 5 shows that the results are robust to using variation in insurance coverage at the subregion by-urban or subregion by-rural level instead of just variation at the subregion level.[8] Row 6 shows that the results are robust to replacing the

7 I am grateful to Larry Katz for providing these data. All variables are measured annually, except state population which is interpolated between censuses. See Katz, Levitt, and Shustorovitch [2003] for more details.

8. Insurance rates are uniformly lower in rural areas, but the geographic pattern across subregions is quite similar in rural and in urban areas. I do not use

linear measure of $\text{Mcareimpact}_z$ with an indicator variable for whether the subregion is one of the eight subregions above the national average in the impact of Medicare on insurance coverage. Row 7 shows the results are robust to measuring $\text{Mcareimpact}_z$ by the percent of elderly without any hospital insurance, rather than without Blue Cross hospital insurance (see Table I). Finally, row 8 shows that the results are quite similar if $\text{Mcareimpact}_z$ is measured as the share of hospital expenditures in the subregion covered by elderly insurance, which is calculated as the percent of the elderly without BC insurance times the proportion of hospital expenses that are elderly.[9] This is not surprising since in practice there is very little variation in the percent elderly across subregions (or even across counties).

### *III.D. The Magnitude of the Impact of Medicare's Introduction on Aggregate Spending*

There are two important limitations to using the results from estimating (1) at the hospital level to infer the aggregate impact of Medicare. First, the analysis will not capture any effects of Medicare that operate via an impact on hospital exit or entry. Second, the analysis treats hospital of different size equally, although they may have differential responses to Medicare. This section therefore estimates alternative models to address both of these potential issues.

One way to capture any impact of Medicare that operates via an impact on hospital exit or entry is to aggregate (i.e., sum) the outcomes to the hospital market. The disadvantage of analysis at the market-level is the increased noise in the estimates due to nontrivial amounts of missing data, particularly for the expenditure measures where only about two-thirds of hospitals report the information in a given year (see Appendix).[10] As a result, the

these urban- and rural-specific rates in the main specifications because of the small sample size of some rural subregions in the NHS and because there is some uncertainty about how to map the "rural" areas in the NHS data into the appropriate hospitals in the AHA.

9. The percent of hospital expenses that are elderly is based on the percent of the population in the subregion that is elderly, and an estimate from the 1963 Health Care Utilization Survey that hospital spending per individual aged 65 and over was 2.3 times that per individual under age 65.

10. There is no evidence of anything systematic determining which observations have missing data, either by the cross-sectional variation in Medicare's impact, the time period, or—perhaps more importantly—the interaction of the cross-sectional variation and time period that is used to identify the impact of Medicare.

flexibly estimated model in (1) often produces insignificant estimates at the market-level. I therefore estimate a more parametric, deviation-from-trend model with a trend break in 1965. This makes more efficient use of all the data in estimating the effect of Medicare than the point-to-point comparisons shown in Table III, which utilize only three years of data [see e.g., (2)]. A related advantage of the deviation-from-trend analysis over the more flexible estimation is that the point-to-point comparisons may produce misleading estimates if a particular point in the comparison is not in line with neighboring points. Because of the imposition of a functional form on the time series pattern, I restrict all of the deviation from trend analyses to the years 1960–1970.

I first estimate the deviation-from-trend analysis at the hospital level, to confirm that it produces broadly similar results to those from estimating (1). I estimate

$$(3)\quad \log(y_{ijt}) = \alpha_j \times \mathbf{1}(\text{county}_j) + \delta_t \times \mathbf{1}(\text{Year}_t) + \beta_1(t_t \times \text{Mcareimpact}_z) + \beta_2((t-1965)_t \times \text{Mcareimpact}_z) + X_{st}\beta + \varepsilon_{ijt}$$

As in the flexible (1), (3) includes a full set series of county fixed effects ($\alpha_j$'s) and year fixed effects ($\delta_j$'s). However, instead of interacting a full set of year dummies with the subregion's insurance coverage prior to Medicare as in (1), (3) interacts a linear time trend with the subregion's insurance coverage prior to Medicare ($t_t \times$ Mcareimpact$_z$) and allows for a trend shift after the introduction of Medicare that varies with the subregion's insurance coverage prior to Medicare (($t - 1965) \times$ Mcareimpact$_z$). The coefficient of interest is $\beta_2$; it indicates the differential slope shift in 1966 experienced by hospitals with more of an impact of Medicare on insurance coverage relative to those with less of an impact. The primary drawback to (3) is that it *ex-ante* restricts any shift to occur in 1966; the results from the preceding more flexible model in (1) together with the falsification tests done on the preperiod suggest that this is a reasonable restriction.

The results from estimating (3) are shown in the first row of Table V. The results are statistically significant for all outcomes and the implied five-year impact of Medicare (shown in bold in {curly brackets}) is similar to the implied five-year impact of Medicare from estimating (1) and performing the point-to-point test of (2) (see row 1 of Table IV).

When the outcomes are aggregated (summed) to the market-level, I estimate

TABLE V
AGGREGATE IMPACT OF MEDICARE

| | Log admissions | Log patient days | Log employment | Log beds | Log payroll expenditures | Log total expenditures |
|---|---|---|---|---|---|---|
| Hospital-level analysis | | | | | | |
| | 0.081** | 0.092*** | 0.046** | 0.077*** | 0.096*** | 0.056** |
| | (0.034) | (0.029) | (0.023) | (0.021) | (0.029) | (0.028) |
| | [$N$ = 66,669] | [$N$ = 66,376] | [$N$ = 66,510] | [$N$ = 70,534] | [$N$ = 59,905] | [$N$ = 60,842] |
| | **{0.35}** | **{0.41}** | **{0.19}** | **{0.33}** | **{0.43}** | **{0.23}** |
| Market-level analysis | | | | | | |
| Unweighted OLS | 0.072*** | 0.094*** | 0.099*** | 0.077*** | 0.094*** | 0.101*** |
| | (0.020) | (0.035) | (0.021) | (0.029) | (0.026) | (0.024) |
| | **{0.31}** | **{0.42}** | **{0.45}** | **{0.33}** | **{0.42}** | **{0.46}** |
| Weighted OLS | 0.077*** | 0.055 | 0.092*** | 0.060* | 0.105** | 0.082** |
| | (0.015) | (0.049) | (0.025) | (0.031) | (0.043) | (0.035) |
| | **{0.33}** | **{0.23}** | **{0.41}** | **{0.25}** | **{0.48}** | **{0.36}** |
| Weighted GLM | 0.074*** | 0.036 | 0.091*** | 0.060** | 0.104*** | 0.083** |
| | (0.015) | (0.053) | (0.023) | (0.029) | (0.040) | (0.034) |
| | **{0.32}** | **{0.14}** | **{0.41}** | **{0.25}** | **{0.48}** | **{0.37}** |
| $N$ | 2,846 | 2,846 | 2,846 | 2,847 | 2,844 | 2,844 |

Table reports coefficient on $(t - 1965) \times$ Mcareimpact (i.e., $\beta_2$) from estimating deviation-from-trend analysis in (3), (4), or (5). The first row reports the results from estimating (3) at the hospital level. The second and third rows report the results from estimating (4) at the market level. The bottom row reports the results from estimating (5) at the market level. For all estimates, the sample is limited to 1960 through 1970. All analyses include eight time-varying state-level indicator variables for the number of years before (or since) the implementation of Medicaid in state $s$. Weighted estimations (in rows 3 and 4) use the number of patient days in a given market in 1960 to weight each market's observations. Standard errors are (in parentheses) and are calculated allowing for an arbitrary variance–covariance matrix within each hospital market. Implied five-year (percent) aggregate impact of Medicare is in {curly brackets}. This is calculated based on the reported coefficient ($\beta_2$ from the relevant equation) and the translation ($\exp(\beta_2 \times 0.75 \times 5) - 1$); this translation accounts for the log specification, the fact that Medicare on average increased insurance coverage by 75 percentage points and that $\beta_2$ only gives a one year effect.

***,**, * denotes statistical significance at the 1, 5, and 10 percent levels, respectively.

$$\text{(4)} \quad \log(y_{mt}) = \alpha_m \times \mathbf{1}(\text{market}_m) + \delta_t \times \mathbf{1}(\text{Year}_t) + \beta_1(t_t \times \text{Mcareimpact}_z) + \beta_2((t - 1965)_t \times \text{Mcareimpact}_z) + X_{mt}\beta + \varepsilon_{mt}$$

where $m$ denotes the hospital market. The dependent variable now measures the log of total hospital spending (or inputs or utilization) in market $m$ and year $t$.[11] Recall that a hospital market is an SMSA, or the rural (non-SMSA) part of a state.[12]

The second row of Table V reports the results. All of the estimates are statistically significant. For admissions, patient days, beds, and payroll expenditures, the magnitudes are quite similar to the hospital-level estimates in the preceding row. However, for employment and total expenditures, the estimates at the market level are double the estimates at the hospital level. For example, for total spending, the coefficient on $(t - 1965) \times$ $\text{Mcareimpact}_z$ from (4) at the market level is 0.101, implying a five-year impact of Medicare on total spending of 46 percent ($\sim$ $[\exp(0.101 \times 0.75 \times 5) - 1]$). By contrast, the analogous coefficient from (3) at the hospital level is 0.056, implying a five-year impact of Medicare on total-spending of only 23 percent ($\sim[\exp(0.056 \times 0.75 \times 5) - 1]$). Nonetheless, for all but the employment estimates, each point estimate at the hospital (market) level lies within the 95 percent confidence interval for the market (hospital) level estimate.

While aggregation to the market level will capture any impact of Medicare that occurs via hospital entry or exit, it does not address the issue that markets of different size are given equal weight in the regression estimate, yet may have heterogeneous responses to Medicare. Although further aggregation to the national level would address this issue of potentially heterogeneous treatment effects, it would destroy the cross-sectional variation used to identify the effect of Medicare.[13] One potential solution is to weight each market by a

11. Twelve SMSA's cross a subregion border and eighteen cross a state border. In these cases, I assign the subregion and state-level variables the value in the subregion or state with the majority of hospitals. The results are not sensitive to assigning average values instead.

12. The market-level results are robust to the alternative specifications explored in Table IV (not shown). They are also robust to estimating equation (4) by a GLS procedure that allows for a separate variance–covariance matrix within each market as well as a market-specific AR(1) term. The point estimates from the GLS estimation are quite similar to the OLS estimates reported in Table V but the standard errors tend to be smaller.

13. A time series comparison of spending or admissions growth since 1965 relative to a pre-existing quadratic trend suggests that Medicare is associated with a 31 percent increase in spending by 1970, but no effect on admissions.

measure of its size. The third row of Table V therefore reports the results from estimating a weighted version of (4) in which each market's observations are weighted by the number of patient days in that market in the base year (1960). There is no systematic change in the results; some outcomes—such as admissions, employment, and payroll expenditures—are virtually unaffected. Others, such as beds and total expenditures are about 20 percent smaller. The one dramatic change is in patient days for which the effect declines in half and is no longer statistically significant.[14]

Finally, an issue with both the unweighted and weighted estimates is that they produce unbiased estimates of $E(\log(y|x))$, not $\log(E(y|x))$, which is the object of interest [Manning 1998]. A simple solution is to estimate a generalized linear model (GLM) with log links:

$$(5)\quad \log(E(y_m)) = \alpha_m \times \mathbf{1}(\text{market}_m) + \delta_t \times \mathbf{1}(\text{Year}_t) + \beta_1(t_t \times \text{Mcareimpact}_z) + \beta_2((t-1965)_t \times \text{Mcareimpact}_z) + X_{mt}\beta$$

The bottom row of Table V reports the results of estimating the conditional expectation function in (5) by maximum likelihood, assuming a gamma distribution of the error term (see e.g., McCullagh and Nelder [1989]). As in the prior row, I weight each market's observations by the number of patient days in the market in 1960, and allow for an arbitrary variance–covariance matrix within each market. With the exception of the estimate for patient days, which remains insignificant but declines even more in magnitude, the other estimates are virtually indistinguishable from the weighted OLS estimates in the previous row.

I use the results from the weighted GLM analysis (row 4 in Table V) as my central estimates of the aggregate effect of Medicare. Table V indicates that these results tend to be the same or

14. A potential issue with the weighted analysis is that the impact of Medicare on insurance coverage may also differ across markets of different size, making it difficult to distinguish heterogeneous treatment effects from heterogeneous treatments. An alternative approach would be to estimate the impact of Medicare separately for urban and rural areas, since the NHS provides separate estimates of insurance coverage for each area within each subregion. The separate estimates can then be averaged—using their relative contribution to national totals—to produce an estimate of the aggregate impact of Medicare. Deaton [1995] discusses the relative advantages of this approach and the weighting approach. Results using this alternative approach suggest that the impact of Medicare was greater in urban than in rural areas, and yield an implied national impact of Medicare on spending that is about 12 percent lower than the weighted estimate in Table V (not shown).

smaller than the results from alternative market-level specifications.

Using the results from the weighted GLM estimates at the market level, the coefficient 0.083 on $(t - 1965) \times \text{Mcareimpact}_z$ from the total expenditures regression (Table V, row 4, column 6) implies that the introduction of Medicare is associated with a 37 percent ($\sim[\exp(0.083 \times 0.75 \times 5) - 1]$) increase in hospital spending over its first five years. A similar analysis using the estimate of the impact of Medicare on admissions (coefficient of 0.074) suggests that in its first five years Medicare was associated with a 32 percent increase in admissions. Note that these estimates speak to the proportional effect of Medicare on hospital admissions and spending *for all ages*, not just the elderly. In 1965, the elderly constituted 10 percent of the population, and 20 percent of hospital expenditures.[15]

Data from the National Health Expenditure Accounts indicate that real hospital expenditures grew by 63 percent between 1965 and 1970, compared to only 41 percent over the previous five years. The estimates therefore suggest that Medicare can account for over half of the growth in hospital spending over this five year period, and all of the above-average growth relative to the previous five years.

## IV. Partial Equilibrium Versus General Equilibrium Effects of Health Insurance

### *IV.A. Comparison to the Rand HIE Estimates*

Several aspects of the Rand experiment facilitate comparison of my estimates with what the Rand estimates would predict for the impact of Medicare. The Rand experiment took place only shortly after the introduction of Medicare (it was conducted from 1974 to 1982). Like Medicare, it provided hospital insurance for free, so that both estimates incorporate a positive income effect on hospital spending. It also estimated the spending effects of health insurance separately for different types of health care, so that I can compare my estimates of the impact of Medicare on *hospital* spending to Rand estimates of the impact of health insurance on *hospital* spending. Finally, the Rand experiment specifically in-

15. Population estimates come from interpolating the 1960 and 1970 census estimates. The elderly's share of hospital expenditures is calculated using the 1963 Survey of Health Service Utilization and Expenditures.

vestigated the impact of shorter- versus longer-term changes in health insurance and found no differences, suggesting that the expected permanence of Medicare relative to the Rand experiment is unlikely to be an important factor.

The results from the Rand HIE indicate that moving someone from no insurance to a policy similar to Medicare's would increase their hospital spending by 37 percent.[16] Therefore, the Rand HIE would predict that moving 75 percent of the elderly from no insurance to Medicare would increase hospital spending among the elderly by 28 percent, or—as the elderly accounted 20 percent of hospital spending in 1965—total hospital spending by 5.6 percent. This is less than one-sixth the magnitude of the 37 percent effect of Medicare on hospital spending in its first five years that I estimated above; the confidence interval on my estimate rejects the Rand point estimate with more than 99% confidence.[17]

A potentially important caveat to this comparison is that the Rand experiment excluded individuals age 62 and over. It seems doubtful, however, that a larger spending response of the elderly relative to the nonelderly can explain the over six-fold higher estimated impact of Medicare. Indeed, *a priori*, it is not clear whether to expect that the elderly have a larger price elasticity of demand for health care (for example, because they tend to be poorer than the nonelderly), or a smaller price elasticity (for example, because their health problems are likely to be more severe.)

There are two broad classes of explanations for the empirical finding that market-wide changes in health insurance appear to have a disproportionately larger impact on the health care sector than small-scale changes in health insurance. The "fixed costs"

16. Medicare hospital insurance originally imposed a $40 deductible (in 1965 dollars) and no copayment for the first sixty days. The HIE estimates suggest that the effect of moving from no insurance to a policy with no copayment and this deductible (i.e., a $125 deductible in 1983 dollars, which are the dollars used in the reported HIE estimates) would be to increase spending from $500 to $685; see Keeler et al. [1988] and Newhouse et al. [1993], especially pages 129–130. Accounting for the fact that Medicare imposed a 25 percent copayment after sixty days in the hospital would only decrease the implied spending effect of Medicare from the Rand estimates. Note that although the HIE placed limits on maximum out of pocket spending, Keeler et al. [1988] describe how to estimate the effect of cost-sharing in the absence of such limits, and the estimates I use from the HIE follow this approach.

17. Utilization estimates from the HIE that adjust for the out of pocket maximums are not available. Nonetheless, the results of cruder comparisons also suggest that the HIE's implied impact of Medicare on hospital admissions would also be substantially lower than what I have estimated here (see Newhouse et al. [1993], Table 3.2).

hypothesis is that aggregate changes in health insurance may sufficiently change the nature and magnitude of the market demand for health care that they alter the incentives for hospitals to incur the fixed costs of entering the market or of adopting new practice styles. The "spillovers" hypothesis is that changes in insurance for one set of patients can have spillover effects to the treatment of other patients. Spillovers may arise from jointness in hospital production, medical ethics, fears of malpractice liability, or simply hospital income effects. Consistent with the presence of spillovers, several studies have found that, controlling for an individual's own insurance, average insurance coverage in a hospital or physician practice is systematically correlated with treatment intensity and spending on the individual [Baker 1997; Baker and Shankarkumar 1998; Glied and Graff Zivin 2002; Baicker and Staiger 2005; Dafny 2005].

These two hypotheses may be complementary, and therefore not necessarily separable in an accounting sense. For example, if Medicare induces a hospital to incur the fixed cost of adopting a new technology, the new technology, once adopted, may also be used on nonelderly individuals. The hypotheses are, however, conceptually distinct. The fixed costs hypothesis entails fundamental nonlinearities in the impact of health insurance on health spending. The spillovers hypothesis, by contrast, can operate even if the typical community health insurance has a linear impact on health spending (although there may also be important nonlinearities). The rest of this section provides suggestive empirical evidence for each hypothesis.

### *IV.B. Evidence for the "Fixed Costs" Hypothesis*

*The Impact of Medicare on Hospital Entry and Exit.* If Medicare sufficiently increases aggregate demand for hospital care, it may induce new hospitals to incur the fixed costs associated with market entry. Medicare may also affect hospital exit if, for example, increased market size increases the minimum efficient scale of a hospital, thereby inducing smaller hospitals to exit. I therefore examine the impact of Medicare on hospital entry and exit using the deviation-from-trend analysis (4); as before, I limit this analysis to the 1960–1970 period.

For the entry analysis, the dependent variable is the ratio of the number of hospitals in market $m$ that have entered between 1960 and year $t$ to the total number of hospitals in market $m$ in

1960. For the exit analysis, the dependent variable is the proportion of hospitals in market $m$ in 1960 that have left between 1960 and year $t$. (I use shares rather than logs because in most market-years there is no entry or exit.) On average, by 1970, new hospital entry had increased the number of hospitals in the market by 18 percent over the 1960 level; at the same time, 14 percent of hospitals that were in the market in 1960 had exited by 1970.[18]

The results are shown in Table VI. They suggest that the introduction of Medicare had a statistically significant effect on hospital entry. This is consistent with the larger estimates of the impact of Medicare at the market-level than at the hospital-level in Table V. By contrast, Medicare does not appear to have a substantively or statistically significant impact on hospital exit.

I can use the results in Tables V and VI to decompose Medicare's five-year spending effect into the portion due to Medicare-induced hospital entry. In doing so, I account for the fact that the data prior to 1965 indicate that, on average, five years after opening, a hospital's spending is only about 40 percent that of pre-existing hospitals. Therefore, the results from the entry analysis (columns (1) and (2) of Table VI) suggests that in its first five years, Medicare-induced hospital entry may be responsible for an 18 percent ($\sim[0.12 \times 0.75 \times 5 \times 0.4]$) increase in hospital spending, or about half of the overall 37 percent Medicare-induced increase in hospital spending. Since Medicare appears not to have affected hospital exit, the remaining 19 percent spending effect of Medicare presumably reflects growth within existing hospitals.

*Suggestive Evidence of the Impact of Medicare on Technology Adoption.* The large increase in aggregate demand associated with Medicare's introduction may also have encouraged hospitals

18. Identifying entry and exit requires linking hospitals across years based on name and location. To try to distinguish genuine exit and entry from apparent exit or entry stemming from hospital non-reporting or inadequate matching, I define a hospital as exiting in year $t$ if it is in the data in year $t - 1$ and not in any subsequent year through 1975. Analogously, I define a hospital as entering in year $t$ if it is in the data in year $t$ and not in any prior year back through 1948. As a reality check, I compared my estimate of hospital entry to an alternative estimate based on the hospital's establishment date; this does not require linking hospitals across time, but unfortunately, establishment date is not reported after 1964. Using the establishment date, I estimate that hospital entry increases the number of hospitals between 1955 and 1964 by 12 percent; using the panel data approach, the analogous estimate is 17 percent. This suggests that I may overestimate entry or exit by about 40 percent. However, there is no evidence of systematic differences across subregions in my estimate of entry using the panel data approach relative to the establishment date approach. It is therefore unlikely that the estimated impact of Medicare on entry or exit is biased.

TABLE VI
ANALYSIS OF EXIT AND ENTRY

| | Entry analysis (columns 1–2) | | Exit analysis (columns 3–4) | |
|---|---|---|---|---|
| | Unweighted OLS (1) | Weighted OLS (2) | Unweighted OLS (3) | Weighted OLS (4) |
| $(t - 1965) \times$ Mcareimpact | 0.116*** | 0.121*** | 0.011 | 0.013 |
| | (0.019) | (0.017) | (0.011) | (0.010) |
| Mean dep. var. in 1970 | 0.18 | 0.18 | 0.14 | 0.17 |

Table reports the coefficient on $(t - 1965) \times$ Mcareimpact (i.e., $\beta_2$) from estimating the OLS deviation-from-trend analysis at the market level (4). For the entry analysis, the dependent variable is the proportion of hospitals in market $m$ in 1960 that have entered between 1960 and year $t$. For the exit analysis, the dependent variable is the proportion of hospitals in market $m$ in 1960 that have left between 1960 and year $t$. For all estimates, the sample is limited to 1960 through 1970. All analyses include eight time-varying state-level indicator variables for the number of years before (or since) the implementation of Medicaid in state $s$. Weighted estimations (in columns 2 and 4) use the number of patient days in a given market in 1960 to weight each market's observations. Standard errors are in parentheses and are calculated allowing for an arbitrary variance–covariance matrix within each hospital market.

***,**, * denotes statistical significance at the 1, 5, and 10 percent levels, respectively. $N = 2{,}832$.

to incur the fixed costs associated with adopting new technologies. I investigate the impact of Medicare on the adoption of new cardiac technologies; these have had an important role in both the rise in health spending and the increase in life expectancy over the last several decades [Cutler 2003].

The AHA data provide information on two cardiac technologies: the open heart surgery facility and the cardiac intensive care unit (CICU). Virtually all hospitals with open heart surgery have a CICU, which performs necessary postoperative care for open-heart surgery patients. However, the CICU serves other purposes as well; only about one-fifth of hospitals with a CICU have an open heart surgery facility.

Unfortunately, AHA data on cardiac technologies do not exist prior to Medicare's introduction. As a result, I cannot directly examine the impact of Medicare's introduction on changes in each technology's geographic diffusion pattern. This important data limitation makes the analysis of Medicare's impact on technology adoption considerably more speculative than the previous analyses.

I proxy for what the geographic diffusion pattern of a cardiac technology might have looked like in the absence of Medicare by examining the geographic diffusion pattern of other technologies that reached roughly the same nationwide diffusion level *prior* to Medicare's introduction that a given cardiac technology reached *after* Medicare's introduction. The identifying assumption is that, absent Medicare, the geographic diffusion pattern of the cardiac technology would have looked similar to that of the older technology. The pronounced stability across time and across very different technologies in the geographic pattern of technology diffusion provides some support for this assumption [Skinner and Staiger 2005].

Open heart surgery had reached a 9 percent diffusion rate by 1975. Four technologies were at roughly this diffusion level prior to Medicare: the EEG (1950), the postoperative recovery room (1951), the diagnostic radioactive isotope therapy (1955), and the intensive care unit (1958). The CICU first appears in the data in 1969 with a diffusion rate of 35 percent.[19] Another form of intensive care—the postoperative recovery room—had diffused to this level by 1957.

Table VII presents the results. Columns (1) through (7) show the coefficient on $\text{Mcareimpact}_z$ from Probit estimation of

$$\text{Newtech}_{is} = \lambda \text{Mcareimpact}_z + X_s\beta + \epsilon_{is} \tag{6}$$

$\text{Newtech}_{is}$ is an indicator variable for whether hospital $i$ in state $s$ has acquired a given technology in the year of analysis (which varies across the technologies as described above). $\text{Mcareimpact}_z$ measures the increase in insurance coverage in subregion $z$ associated with the introduction of Medicare. $X_s$ controls for state-level socio-economic conditions in the year of analysis; these may help control for other factors—besides Medicare—that have changed over time and may affect technology adoption. The results indicate that neither open heart surgery nor the CICU is differentially diffused across areas with different $\text{Mcareimpact}_z$. By contrast, each of the control technologies is less likely to be adopted in areas with higher $\text{Mcareimpact}_z$, and this geographic pattern is often statistically significant.

Columns (8) and (9) of Table VII show the results from stacking each cardiac technology with its respective control technologies and estimating by Probit the difference-in-differences equation:

$$\begin{aligned}\text{Newtech}_{ist} = {} & \alpha \text{CARDIAC}_i + \delta \text{Mcareimpact}_z \\ & + \lambda(\text{Mcareimpact}_z \times \text{CARDIAC}_i) + X_{st}\beta + \epsilon_{ist}\end{aligned} \tag{7}$$

19. This is an aberration for the AHA data, as most technologies first appear with a diffusion rate of about 10 percent.

TABLE VII
MEDICARE AND THE ADOPTION OF NEW CARDIAC TECHNOLOGIES

| | Analysis of open heart surgery (columns 1–5) | | | | | Analysis of CICU (columns 6–7) | | Difference-in-differences analysis (columns 8–9) | |
|---|---|---|---|---|---|---|---|---|---|
| | *Open heart surgery facility* (1) | EEG (2) | Postop recovery room (3) | Diagnostic radioactive isotope (4) | Intensive care unit (5) | *CICU* (6) | Postoperative recovery room (7) | Open heart surgery vs. controls (8) | CICU vs. postoperative recovery room (9) |
| Without state-level covariates | *0.0004* | $-0.182^{***}$ | $-0.087^{**}$ | $-0.210^{***}$ | $-0.143^{***}$ | *−0.097* | $-0.341^{***}$ | $0.150^{***}$ | $0.243^{***}$ |
| | *(0.065)* | (0.059) | (0.044) | (0.068) | (0.053) | *(0.095)* | (0.106) | (0.046) | (0.077) |
| With state-level covariates | *0.015* | −0.087 | −0.049 | −0.118 | −0.054 | *0.102* | $-0.327^{**}$ | $0.123^{***}$ | $0.247^{***}$ |
| | *(0.063)* | (0.063) | (0.057) | (0.072) | (0.062) | *(0.096)* | (0.127) | (0.048) | (0.092) |
| Year of analysis | *1975* | 1950 | 1951 | 1955 | 1958 | *1969* | 1957 | | |
| Mean dependent variable | *0.09* | 0.10 | 0.12 | 0.10 | 0.13 | *0.35* | 0.35 | | |
| Estimating equation | *(6)* | (6) | (6) | (6) | (6) | *(6)* | (6) | (7) | (7) |

All estimates are marginal effects from probit estimation. Columns (1) through (7) report the marginal effect of Mcareimpact from estimation of (6); dependent variable is shown in column heading and results for cardiac technologies are in italic. Columns (8) and (9) report the marginal effect of the interaction of Mcareimpact with CARDIAC indicator from estimation of (7). CARDIAC is 1 for the cardiac technology in the analysis, (open heart surgery or CICU) and 0 otherwise. Standard errors (in parentheses) are adjusted for correlation within hospital markets. First row reports results from regressions without covariates. Second row reports results from a separate regression which adds controls for state-level socio-economic characteristics (specifically, real per capita state income, state infant mortality rate, violent crime rate, and state population).

$\text{CARDIAC}_i$ is an indicator variable for whether technology $i$ is the cardiac technology. The variable of interest is $\text{Mcareimpact}_z \times \text{CARDIAC}_i$. The results reject the null hypothesis that Medicare had no impact on cardiac technology adoption. The geographic adoption pattern of each cardiac technology is statistically significantly more skewed toward areas more affected by Medicare than the geographic adoption pattern of its control technologies. The results look similar if the sample is limited to hospitals that were built prior to Medicare (not shown).[20]

### *IV.C. Suggestive Evidence of Spillovers*

If health insurance spillovers are quantitatively important, estimates of the impact of an individual's health insurance could produce downward biased estimates of the aggregate impact of health insurance. One reason is that most empirical analyses of the impact of an individual's health insurance use other individuals in the same market with different health insurance as a comparison group; such analysis nets out any spillover effect. Even with an empirical design that avoids this problem, it is unlikely that spillovers would be captured in a study of the impact of an individual's health insurance on health spending; the marginal impact of one's own health insurance on the typical health insurance in the community is sufficiently small that even a large spillover effect would be virtually impossible to detect.

To provide a rough gauge of the potential importance of spillovers in the current context, I calculate an alternative estimate of the impact of Medicare based on changes in spending for the elderly relative to the nonelderly. The estimates are based on individual-level survey data from the 1963 and 1970 Surveys of Health Service Utilization and Expenditures.[21] Any impact of a change in typical insurance status will impact both age groups and therefore be netted out of the estimate; the differential spending change picks up only the direct impact of

20. The analysis is not well suited to gauging the magnitude of any impact of Medicare on technology adoption, since it conditions on the technologies reaching a given diffusion rate nationwide.

21. The 1963 survey is designed to be representative of the noninstitutionalized U.S. population; the 1970 survey also excludes the institutionalized population but over samples the elderly, rural areas, and the urban poor. Neither survey includes usable population weights. Spending data is based on individual self-reports, but attempts were made to verify insurance claims with third party payers. Neither survey contains geographic identifiers. For more details see ICPSR [1988, 2002].

one's own age group's insurance, conditional on average insurance coverage.

Consistent with potentially large spillovers, I find that analysis based on the age-variation in Medicare coverage produces substantially smaller estimates of the impact of Medicare on hospital spending than the analysis in Section III based on variation across subregions, which includes any spillover effects. Table VIII shows mean overall hospital spending in 1963 and in 1970 for individuals aged 65–74 and for individuals aged 55–64. The time series comparison shows increases in spending for both the elderly and nonelderly. The difference-in-differences estimate is more than seven times smaller than the time series increase in hospital spending for the elderly. It suggests that the introduction of Medicare is associated with an increase in hospital spending for the elderly relative to the nonelderly of 16 percent (with no covariate adjustment) or 30 percent (covariate adjusted). Even the larger estimate is less than one-fifth the magnitude of the implied estimate from Section III if the entire effect were limited to the elderly (185 percent, since the central estimate of the effect of Medicare in Section III is 37 percent and 20 percent of hospital spending prior to Medicare was for the elderly). Interestingly, it is quite similar to the 28 percent increase in elderly spending predicted by the Rand estimates.

## V. The Spread of Health Insurance and the Growth of Health Spending

Between 1950 and 1990, real per capita medical spending increased by a factor of six. Over the same period, the average coinsurance rate for the population (calculated as the ratio of national out of pocket health spending to national total health spending) fell by about 50 percentage points [Cooper at al. 1976; Gibson 1978; CMS 2004]. Using the estimates from the Rand experiment, Manning et al. [1987] and Newhouse [1992] conclude that the spread of health insurance can explain only a very small part—on the order of one-eighth to one-tenth—of the increase in spending over this period.[22] I reimplement the same back of the

22. To estimate the effect of the spread of health insurance on health spending, Manning et al. [1987] and Newhouse [1992] use the Rand's estimates of spending differences between various types of plans, but not the difference between no insurance and a Medicare-like policy that is the Rand estimate I compare my results to in the rest of the paper. Also, Manning et al. [1987] and Newhouse [1992] look at the predicted effect of the spread of health insurance on

TABLE VIII
CHANGES IN HOSPITAL SPENDING FOR INDIVIDUALS AGED 65–74 AND AGED 55–64

| | 1963 | 1970 | Difference | Difference (covariate-adjusted) | Difference-in-difference | Difference-in-difference (covariate adjusted) |
|---|---|---|---|---|---|---|
| Ages 65–74 | 281 | 919 | 639*** | 651*** | | |
| | | | (125) | (133) | | |
| Ages 55–64 | 245 | 840 | 595*** | 570*** | | |
| | | | (127) | (116) | | |
| Difference | 35 | 80 | | | 44 | 86 |
| | (63) | (167) | | | (178) | (171) |

All dollars are in year 2000 dollars. Data on overall hospital spending are from the 1963 and 1970 Surveys of Health Service Utilization and Expenditures. $N$ = 3,030 (pooled sample); see text for more detail on these surveys. Robust standard errors are in parentheses.

***, **, * indicates statistical significance at the 1 percent, 5 percent, and 10 percent level, respectively. Covariate-adjusted estimates control for gender, marital status, age, age-squared, and indicators for education group (6 or fewer years of school, between 6 and 12 years of school, and 12 or more years of school).

envelope calculation using my central estimate of the 37 percent spending increase associated with Medicare over its first five years. Medicare also decreased the average coinsurance rate in the population by about 7 percentage points.[23] Extrapolating from this relationship implies that the 50 percentage point decrease in coinsurance rates between 1950 and 1990 would increase spending by 264 percent. The overall spread of insurance may therefore be able to explain half of the six-fold increase in real per capita health spending over this period.

My findings therefore suggest that the spread of health insurance may have played a much larger role in the substantial growth in the health care sector over the last half century than the current conventional wisdom suggests. Of course, issues of

spending from 1950 to 1980 or 1950 to 1984, rather than 1950 to 1990, as I do here. However, their method is easily extrapolated out to 1990 and doing so does not change the estimated contribution of health insurance. I do not extend the extrapolation beyond 1990 due to the spread after this point of managed care, which may have very different effects on health spending than traditional fee for service health insurance.

23. Medicare increased insurance coverage among the elderly by 75 percentage points, but imposed an ~5 percent copay (i.e., a one day deductible with an average length of stay for the elderly prior to Medicare of twenty days)); therefore on average it decreased the coinsurance rate for the elderly by about 71 percentage points (~$0.75 \times 0.95$). The elderly were 10 percent of the population in 1965, therefore the average coinsurance rate for the population declined by about 7 percentage points.

external validity suggest that the exact result from this back-of-the envelope calculation should be viewed with considerable caution; it is primarily of interest in comparison to the results of the same calculation that had previously been performed using the Rand estimates.

One issue with external validity is that Medicare may have had more of an effect on spending than the spread of other public and private health insurance due to Medicare's generous reimbursement rates, including its generous reimbursement of capital spending [Somers and Somers 1967, United States Senate 1970; Feder 1977] that may have contributed to its apparently large effect on new hospital construction. On the other hand, it is possible that the long-run impact of Medicare is larger than the five-year impact used in the back of the envelope calculation. Indeed, the results in Table III indicate that the impact of Medicare on health spending rises over the second five years of its existence. Moreover, the suggestive evidence of an impact of Medicare on technology adoption raises the possibility that the increased market size for new technologies may have increased the incentives to develop new technologies, and thus the subsequent arrival rate of new technologies, as conjectured by Weisbrod [1991]. This dynamic feedback loop could produce long-run effects of Medicare on technological change and health spending beyond the ten-year post-Medicare window analyzed here. Although I cannot investigate this hypothesis directly, empirical evidence of the effect of increased expected demand on innovation in the pharmaceutical industry suggests that such a feedback mechanism may be present for hospital technologies as well [Acemoglu and Linn 2004; Finkelstein 2004].

## VI. Conclusion

By studying the introduction of Medicare, this paper has examined the impact of market-wide changes in health insurance on the health care sector. My central estimate is that Medicare is associated with a 37 percent increase in real hospital expenditures (for all ages) between 1965 and 1970. This estimate is over six times larger than what evidence from the impact of an individual's health insurance on health spending would have predicted. About half of the impact of Medicare on spending appears due to the induced entry of new hospitals, while the rest is due to growth in existing hospitals. The paper also presents suggestive evidence that market-wide changes in health insurance may fundamentally alter the character of medical care both for individuals who experience a change in insurance coverage, and for those who do not as well.

A back of the envelope calculation that extrapolates from the estimated impact of Medicare to the impact of the overall spread of health insurance more generally suggests that the spread of health insurance between 1950 and 1990 may be able to explain about half of the six-fold rise in real per capita health spending over this time period. This raises the natural question of whether a similar mechanism can explain why most other OECD countries have also experienced sustained growth in the health care sector over the last half-century [OECD 2004]. Interestingly, like the United States, many of these countries also established their national health insurance systems in the 1960s and 1970s [Cutler 2002]. An important question for further work is whether other health insurance systems had a similar impact on health spending, or whether idiosyncratic features of the Medicare system resulted in a uniquely high impact. In addition, if Medicare's impact on the practice of medicine in the United States influenced treatment practices or coverage decisions in other countries' national health care systems, it is also possible that the effect of Medicare on health spending may substantially exceed its impact within the United States. This is also an interesting avenue to explore in future research.

## Appendix: The AHA Historical Data

<u>Sample Definition and Time Period:</u> The data are from the August issue of Hospitals: The Journal of the American Hospital Association. Surveys were sent to every AHA-registered hospital in the US. For flow data (such as expenditures, employment, and patient days), the survey asks hospitals to report for the twelve-month period ending September 30th of the year prior to the publication year. For stock data (such as the number of beds, or whether the hospital has various facilities or technologies) it is less clear whether it is as of the survey response (i.e., before February of the publication year) or as of September 30th of the prior year. In all of the analysis, I take the year to be the year prior to publication year. Thus, for example, the 1966 data was published in the 1967 August issues of Hospitals and contains

flow data for the period October 1, 1965 through September 30th, 1966, and stock data as of the fall of 1966.

The AHA reports a response rate for the period I am studying of over 90 percent in all years, and often above 96 percent. This is considerably higher than the reported response rate in more recent decades. Conversations with the research librarian at the AHA suggest that this discrepancy may reflect the fact that the older statistics on response rate may include hospitals who respond to the survey with their name and address, even if they report no data (and are therefore not included in the published statistics). This appears corroborated by attempts by the author to track hospitals over time in the data, as hospitals often disappear for a year or two only to reappear. Extrapolating from the frequency of such occurrences suggests a response rate of closer to 80 percent, which is more in line with data from more recent decades.

Conditional on reporting any data, virtually all responding hospitals report bed information, and about 93 percent report information on admissions, patient days, and employment. However, only about 83 percent report payroll or total expenditure information; this is probably because such information is considered more proprietary by the hospital. Hospital expenditures are therefore likely to be measured with more error than the other variables. Data are more likely to be missing in smaller hospitals and in poorer areas of the country. There is no evidence of a change in reporting patterns associated with Medicare.

Variable definitions are consistent over the period used in this study. They are as follows:

*Total Expenditures:* These consist of payroll and non payroll expenses. Nonpayroll expenses are about 40 percent of total expenses and include employee benefits, professional fees, depreciation expenses, interest expenses, and other expenditures (supplies etc.). The AHA does not report hospital revenue during this time period; estimates of Medicare-induced changes in hospital expenditures therefore do not include any effect of the market-wide change in health insurance on the markup charged for health care services.

*Payroll Expenditures:* These include all salaries and wages for full time personnel and full-time equivalents of part-time personnel, except those paid to interns, residents, and students.

*Beds:* Excludes bassinets.

*Employment:* Includes all paid personnel (both full-time and full-time equivalents for part-time personnel) except residents, interns, and students. Does not include most physicians, since most physicians are not directly employed by the hospital. The 1964 data indicate that just over half of paid personnel are devoted to the "professional care of patients" (i.e., nurses and technicians); the remainder is divided among a variety of custodial and administrative functions. (This breakdown is not available in most years).

*Admissions:* Total inpatient admissions for a twelve-month period, excludes newborns.

*Average Daily Census:* Average number of inpatients receiving care each day during a twelve-month period, excludes newborns. The Patient Days measure used in paper is created by multiplying average daily census by 365.

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