The Aggregate Effects of Health Insurance:
Evidence from the Introduction of Medicare

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Abstract: 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 substantially larger than what the existing evidence from individual-level changes in health insurance would have predicted. Consistent with a disproportionately larger impact of aggregate changes in health insurance, the evidence suggests that the introduction of Medicare altered the practice of medicine. For example, I find that the introduction of Medicare is associated with an increase in the rate of adoption of then-new medical technologies. My estimate of the impact of market-wide changes in health insurance suggests that the spread of health insurance may be able to explain at least forty percent of the increase of real per capita health spending between 1950 and 1990.

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1. Introduction

This paper investigates the effect of market wide-changes in health insurance on the health care sector. In the 1970s, the Rand Health Insurance Experiment, one of the largest randomized, individual-level social experiments ever conducted in the United States, was undertaken to investigate the impact of health insurance on health care utilization and spending. Today, its results 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). In this paper, I argue that the aggregate impact of health insurance may be substantially larger than the estimates from such partial equilibrium analysis would suggest.

To do this, I examine the impact of the introduction of Medicare in 1965, the single largest change in medical insurance in the United States. I use the fact the elderly in different regions of the country had very different rates of private insurance coverage prior to Medicare to identify its effect on the hospital sector. I find that, in its first 10 years, Medicare is associated with a substantial increase in hospital utilization, employment, and capital inputs. I estimate that the introduction of Medicare was associated with a 22 percent increase in real hospital expenditures (for all ages) between 1965 and 1970, with no signs of abating in the subsequent five years. The five-year impact on spending alone is 4 times larger than what the estimates from the Rand health insurance experiment would have predicted.

One reason why the general-equilibrium impact of a market-wide change in health insurance may be much larger than what partial-equilibrium analysis, such as that in the Rand health insurance experiment, 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. I present some suggestive evidence that is consistent with this explanation. For example, the introduction of Medicare is associated with a growth in hospital expenditures *per patient day.* In contrast, the results from the Rand HIE experiment famously concluded that health insurance affected care utilization on the extensive margin, but not the intensity of care conditional on utilization (Newhouse et al., 1993). I also find that the introduction of Medicare is associated with increased adoption of two then-new cardiac technologies: open heart surgery and the cardiac intensive care unit.

The impact of health insurance on health spending is a crucial input for the optimal design of a health insurance system. It also has important implications for the role of health insurance in explaining the dramatic rise in real per capita health spending over the last half century. My estimates imply that Medicare can account for about one-third of the growth in hospital spending between 1965 and 1970, or all of the above-average growth in hospital spending over this 5 year period relative to the previous 5 years.

More generally, the results in this paper suggest that the overall spread of health insurance between 1950 and 1990 can explain at least 40 percent of the 5-fold increase in real per capita health spending over this time period, and potentially much more. Public policy played an important role in the spread of health insurance over this period, through public health insurance programs such as Medicare and Medicaid as well as the tax subsidy to employer provided health insurance. The results therefore indirectly suggest that U.S. policy figured prominently in the substantial growth in the health care sector over the last half century.

Of course, 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 and find that while Medicare appears to have had no impact on elderly mortality in its first 10 years, it did substantially reduce the right tail of out of pocket medical spending by the elderly; we estimate that this reduction in risk exposure may have produced considerable welfare gains.

The rest of the paper proceeds as follows. Section 2 describes the empirical strategy and data. Section 3 presents estimates of the effect of Medicare on hospital utilization, inputs and spending. Section 4 shows that the estimated impact of Medicare is substantially larger than what existing partial equilibrium analysis would have predicted, and presents some evidence for the likely explanations. Section 5 provides a back of the envelope calculation of 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. Section 6 shows that the major findings are robust to a wide range of alternative specifications. The last section concludes.

2. Studying the impact of Medicare: Approach and Data

Medicare is currently the largest health insurance program in the world, providing health insurance to 40 million people and comprising one-eighth of the federal budget and 2 percent of GDP (National Center for Health Statistics 2002, Newhouse 2002, US Congress 2000). Yet we know surprisingly little about the impact of this major change in health care financing on the health care sector. 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).

There are two major obstacles to any analysis of the impact of the introduction of Medicare on the health care sector: how to distinguish the impact of the national Medicare program separately from other co-incident secular changes, and where to find detailed, disaggregated, historical data on the health care sector. This section discusses the paper's approach to surmounting each of these hurdles.

2.1 Identifying the impact of Medicare: geographic variation in pre-Medicare insurance coverage

Medicare, enacted in July 1965, provides universal public health insurance for the elderly (coverage for the disabled was added in 1973). It was implemented nationwide on July 1 1966. It covered both inpatient hospital expenses (Part A) and physician expenses (Part B), reimbursing for both on the basis of "reasonable costs of services". Both the set of covered services and the reimbursement structure were very generous for that time (Somers and Somers 1967, Newhouse 2002).

Prior to Medicare, public health insurance coverage was practically non existent, and meaningful private health insurance for the elderly was also relatively rare (Stevens and Stevens, 1974, United States Senate 1963, Anderson and Anderson 1967, Epstein and Murray, 1967). In the absence of insurance coverage, many of the health expenditures for the elderly were paid out of pocket either by the patient or by non-elderly family members; some health care consumption was also provided free by hospitals (Finkelstein and McKnight, 2005, Anderson and Anderson 1967 and Epstein and Murray 1967).

Medicare had an enormous impact on health insurance coverage for the elderly. Based on data from the 1963 National Health Survey (NHS), I estimate that in 1963, only 25 percent of the elderly had meaningful private hospital insurance in 1963.[1] Upon the implementation of Medicare, hospital insurance coverage for the elderly rose virtually instantaneously to almost 100 percent (US Department of Health, Education and Welfare, 1969). As a result, the introduction of Medicare increased the proportion of the elderly with hospital insurance by 75 percentage points.

The impact of Medicare on elderly insurance coverage varied considerably across different areas of the country. Through a special request to the government, I obtained a version of the 1963 NHS that identifies the geographic location of the individual down to his sub-region. The data indicate that the proportion of the elderly (individuals aged 65 plus) without meaningful private hospital insurance prior to the introduction of Medicare ranged considerably, from a low of 49 percent in New England to a high of 88 percent in the East South Central United States. In 1965, the elderly constituted 10 percent of the population, but 20 percent of hospital expenditures.[2] As a result, the proportion of ex-ante hospital expenditures affected by Medicare varied across the country from about 9 percent to 16 percent.

This geographic variation allows me to identify the effect of Medicare separately from any underlying secular trends in the hospital sector.[3] Moreover, the 38 percentage point variation in the change in elderly insurance coverage – and the corresponding 8 percentage point variation in the proportion of ex-ante hospital expenditures newly covered by Medicare – provides an opportunity to study the impact of a market-wide change in insurance coverage.

A key criterion for using geographic variation in insurance coverage to identify the impact of Medicare is that to at least some extent private insurance coverage for the elderly was redundant of what

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[1] For more detailed information on the 1963 National Health Survey, see National Center for Health Statistics, 1964. I am extremely grateful to Will Dow for his work in unearthing these data.

[2] 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.

[3] Although variation in the percent elderly might seem a natural choice as well, in practice this provides very little additional variation. In Section 6, I show that the basic results are not affected by using this additional variation.

Medicare subsequently covered. Consistent with this, Finkelstein and McKnight (2005) present evidence of a substantial crowd-out effect of Medicare's introduction on private health insurance spending.

The NHS contains data not only on whether the individual had hospital insurance, but whether this plan was a Blue Cross insurance plan. This is particularly useful since much of the private insurance held by the elderly at this time was minimalist in nature (Epstein and Murray 1967, Anderson and Anderson 1967). However, the Blues plans had not only among the most generous– if not the most generous – benefit coverage (Anderson et al., 1963), but – perhaps even more importantly – Medicare's benefit and reimbursement structure was explicitly modeled on the existing Blue Cross and Blue Shield health insurance system (Ball, 1995, Newhouse 2002, Stevens and Stevens 1974, Stevens 1999). Thus the proportion of the elderly population with a Blue Plan provides a very good measure of the proportion of the elderly who had Medicare-equivalent coverage prior to Medicare. All of the statistics above on private hospital insurance coverage are based on coverage by Blue Cross hospital insurance.

Figure 1 and Table 1 shows the geographic distribution of the elderly without private Blue Cross (BC) hospital insurance coverage in 1963; such insurance would cover the hospital expenses subsequently covered by Medicare Part A.[4] Broadly speaking, insurance coverage is higher in the North East and North, and lower in the South and West. The geographic pattern in the percentage of individuals without *any* hospital insurance is similar to that for BC hospital insurance (although the national mean is substantially lower); below I show that the estimated impact of Medicare is robust to using either measure of hospital insurance.[5] Table 1 also indicates that within each sub-region, rural areas have lower insurance coverage rates than urban areas; I make use of this additional variation as well in some of the additional analysis below.

[4] The data also contain information on whether the individual has Blue Shield surgical insurance (which is subsequently covered by Medicare Part B). Since coverage patterns for hospital and surgical insurance are virtually identical, and the analysis in this paper is on the impact of Medicare on the hospital sector, I use the hospital insurance coverage rates in the analysis.

[5] The geographic pattern in BC hospital insurance coverage also appears to be extremely stable over time. The correlation across the four census regions of the percent of the elderly without BC hospital insurance in the 1959 and 1963 NHS is 0.99 (see National Center for Health Statistics 1960 for aggregate statistics on health insurance by region in the 1959 data).

The empirical strategy is to compare hospital outcomes before and after Medicare in areas of the country where Medicare had a larger effect on the percent of the elderly with health insurance to areas where it had less of an effect. Of course, private insurance rates prior to Medicare are not randomly assigned. Data from the 1960 census indicate that variation in socio-economic status can explain a substantial share of the variation in insurance coverage across sub-regions, and that socio-economic status and insurance coverage are highly positively correlated. Areas that differ in their socio-economic status are also likely to differ in terms of their desired level of medical spending, or growth in medical spending. The empirical approach is therefore to look for a break in any pre-existing differences around the time of Medicare's introduction in 1966; I discuss the econometric model in considerably more detail below.

2.2 Data: The American Hospital Association Annual Survey

I study the impact of Medicare on the hospital sector, which was the single largest component of health spending at the time of Medicare's introduction, as well as the single largest component of the subsequent growth in health spending (National Center for Health Statistics, 2002). For the analysis, I use 26 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 historical data, which I found 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 are widely used in studies of the hospital sector (e.g. Baker and Phibbs (2002), Cutler and Sheiner (1998), Duggan (2000)). However, the historical data have been largely ignored. Appendix A provides a detailed description of the data quality.

I limit the sample to the approximately three-quarters of hospitals that are private hospitals; about two-thirds of these private hospitals are non-profit. The sample therefore consists of about 4,500 hospitals per year. I exclude the public hospitals from the main analysis because the identification strategy is ill-suited to examining the effect of Medicare on public hospitals. Public hospitals serve a predominantly (or perhaps exclusively) poor and uninsured population, regardless of the overall rate of private insurance coverage in the sub-region (Stevens 1999). I show below, however, that the results are robust to including the public hospitals.[6]

The main analysis of the paper examines the impact of Medicare on six hospital outcomes: total expenditures, payroll expenditures, employment, beds (a common proxy for the hospital capital stock), admissions and number of patient days. Utilization and bed data are exclusive of newborns. All expenditure variables in the paper are converted to 1960 dollars using the CPI-U. Note that hospital expenditures do not include any hospital markup; this would be captured by hospital revenue, but this is not reported in the historical data. Appendix A provides a more detailed description of these variables.

Figure 2 shows the national time series patterns for each of these variables. The figure also shows a quadratic fitted to the pre period data (1965 and earlier). All of the variables are increasing over the entire period of the data. Most variables show increases after 1965 relative to the pre-existing trends. Of course, extrapolating off of the time series is potentially problematic. The mid-1960s were a period of great social change, as well as other important pieces of domestic legislation. The national price controls imposed in 1971 through 1974 are also likely to confound any time series analysis.

Table 2 provides summary statistics for all the outcome variables used in the study. It reports the hospital mean of the outcome variables in the period immediately prior to the introduction of Medicare (1962-1964). It also reports the means for 1962-1964 separately for hospitals in the North and NorthEast (where insurance coverage was comparatively high prior to Medicare) and for hospitals in the South and West (where insurance coverage was comparatively low prior to Medicare). Not surprisingly, the means are consistently higher in areas with a higher insurance rate prior to Medicare. Indeed, given that below I will show an effect of Medicare on almost all of these outcomes, it would be surprising if the levels were same in the pre-period when insurance rates were so different.

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[6] The non-random sorting of insured individuals across hospital types also suggests that the estimated effects on private hospitals may be biased down as well. We would like to measure the percent of potential patients without private insurance in private hospitals in a given area, but instead observe the percent of the population without private insurance in a given area; since the public hospitals attract almost entirely patients without insurance, the variation in our observed measure is actually greater than the variation in the true measure and thus our estimates will be biased down.

3. The Impact of Medicare on Hospital Utilization, Inputs and Spending

3.1 Econometric model

The empirical strategy is to compare changes in hospital-level 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. The basic estimating equation is given by:

$$\log(y_{ijt}) = \alpha_j * \mathbf{1}(county_j) + \delta_t * \mathbf{1}(Year_t) + \sum_{t=1948}^{t=1975} \lambda_t (pctuninsured)_z * \mathbf{1}(Year_t) + X_{st}\beta + \varepsilon_{ijt} \quad (1)$$

The dependent variable is the log of outcome y in hospital i in county j and year t. I estimate the equation in logs because the hospitals vary considerably in size, and therefore constraining the outcomes to all grow according to a series of common (level) year fixed effects seems inappropriate. $\mathbf{1}(County_j)$ are a series of county fixed effects that control for any fixed differences across counties in the outcome of interest. $\mathbf{1}(Year_t)$ are a series of year fixed effects that control for any common secular year effects for the whole nation. $pctuninsured_z$, the percentage of the elderly population in subregion z without private Blue Cross hospital insurance in 1963.

The key variables of interest are the interactions of the year fixed effects with the percentage of the elderly population in sub-region z without private health insurance in 1963 ($(pctuninsured)_z * \mathbf{1}(Year_t)$). The pattern of coefficients on these variables – the λ_t's – shows the flexibly estimated annual trend in the dependent variable over time in areas where all of the elderly lacked private insurance prior to Medicare relative to areas where none of the elderly lacked private insurance prior to Medicare. The change in the trend of these λ_t's before and after the introduction of Medicare can therefore provide an estimate of Medicare's impact. Crucially, equation (1) does not privilege 1965 relative to other years for when any changes might occur. I begin with this flexible specification in order to let the data show where changes in time pattern – if any – actually occur and to gauge whether Medicare may plausibly have played a role.[7]

Due to the possibility for correlation across hospitals over time within areas, I report results that allow for an arbitrary variance-covariance matrix within each state. I verified that the p-values are very similar if instead I implement the randomized inference procedure developed by Bertrand et al. (2004). Clustering at the sub-region produces substantially smaller p-values, although with only 11 sub-regions the desirable asymptotic properties of clustering may not obtain.

The empirical approach is to look for a break in any pre-existing differences in levels or trends in these hospital outcomes across areas that were differentially affected by Medicare around the time of Medicare's introduction in 1966. Since this approach will not capture any effect of Medicare on the previously-insured that operates via the income effect, it may underestimate of the full impact of Medicare.

The identifying (or, counterfactual) assumption is that absent Medicare, any pre-period differences would have continued. I use the almost 20 years of data prior to the introduction of Medicare to provide support for this identifying assumption (see Section 6). A central concern is that other things might have also have been changing differentially over time across different areas of the country. Equation (1) therefore also includes a series of time-varying state covariates (X_{st}). In the baseline specification, these covariates consist of a series of indicator variables for the number of years since (or before) the implementation of a Medicaid program in state *s*; all but one state enacted a Medicaid program (which provides public health insurance to the indigent) between 1966 and 1972 (Gruber, forthcoming). In practice, the estimated effects of Medicare are not sensitive to including these controls.[8] In the sensitivity

[7] In the sensitivity analysis in Section 6, I also show that the results are robust to interacting the year effect with an indicator variable for whether the area has more than a certain percent of the elderly without private insurance, rather than requiring the percent without insured to enter linearly, as in the baseline specification.

[8] In the period under examination Medicaid was a considerably smaller program than Medicare (National Center for Health Statistics, 2002). Although in principle*,* estimates of equation (1) could shed light on the impact of Medicaid, in practice, however, the results suggest that the timing of state implementation of Medicaid is not random with respect to hospital outcomes, and analysis does not yield stable estimates of the impact of Medicaid.

analysis in Section 6 I show the results are robust to adding other time-varying state-level covariates as well.

3.2 Basic results

The core empirical findings are readily apparent in Figure 3 which shows the λ_t's from estimating equation (1) for six different dependent variables: admissions and patient days (i.e. hospital utilization), employment and beds (i.e. hospital inputs) and payroll expenditures and total expenditures (i.e. hospital expenditures). These λ_t's are the coefficients on each of the year effects interacted with the percentage in that area uninsured in 1963. They therefore identify annual changes in the dependent variable in areas in which no one had Blue Cross insurance in 1963 *relative* to areas in which everyone had insurance. Since the coefficients identify only changes in the dependent variable relative to the omitted year (1965), I normalize λ_t in 1965 to the difference in the mean of the dependent variable in 1962-1964 between the south and the west (lower insurance areas) and the north and northeast (higher insurance areas). The circles indicate the 95 percent confidence interval for each coefficient.[9] A vertical line demarcates 1965, the year in which Medicare is enacted.[10]

Figure 3 indicates that prior to 1965, each hospital outcome is growing relatively more slowly in areas in which a lower proportion of the elderly had private insurance in 1963. This is not surprising, given the socio-economic difference across these areas discussed above. Strikingly, Figure 3 indicates that this slower growth in low insurance areas relative to high insurance areas reverses itself dramatically after 1965 (the year in which Medicare is enacted) for all six outcomes. Hospital outcomes begin to rise steadily in areas that previously had little insurance (i.e. areas where Medicare had a large impact on insurance coverage) relative to areas that previously had more insurance (i.e. areas where Medicare had less of an impact on insurance coverage).

[9] Since 1965 is the reference year, and the coefficients are on the interaction of year fixed effects with *pctunins*, the confidence intervals naturally tend to increase as we go further from 1965 in either direction.
[10] 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.

I perform a variety of statistical tests of the coefficients graphed in Figure 3. Motivated by the graphical results, all of these tests estimate the n-year change in λ_t after the introduction of Medicare relative to the n-year change in λ_t before the introduction of Medicare. For example, the impact of Medicare in the first five years is calculated as follows:

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

$\Delta 5$ thus denotes the 5-year change in the hospital outcome after the introduction of Medicare relative to the 5 years prior to the introduction of Medicare for areas that had 100 percent of the elderly without BC insurance in 1963 relative to areas that had none of the population without BC insurance in 1963.

The first three rows of Table 3 report the estimates for the 2-year, 5-year and 10-year change in the outcome, respectively. P-values are reported in parentheses below each estimate. Each column shows the result for a different dependent variable. The results provide statistical confirmation of the visual evidence in Figure 3; they uniformly indicate that the introduction of Medicare is associated with a substantial and statistically significant increase in all of the dependent variables.

Because the reference (or pre-) period changes with the 2- 5- and 10- year tests, comparisons across the test should not be interpreted as a different effect at different time intervals. To compare the effects in different time intervals, the fourth row of Table 3 repeats the five-year test for the period 1975 to 1970, using the *same* base period (1965 to 1960) as used in the first five year test in row 2 (which compares he period 1970 to 1965 to that of 1965 to 1960). The results indicate that Medicare is associated with a continued statistically significant increase in five of the six outcomes (all but patient days) in the second five year period.

Almost all of the analysis to date has focused on the consumer response to fee for service health insurance with little evidence, to my knowledge, of the impact of fee for service health insurance on the provider side. If hospitals have substantial excess capacity, increases in health insurance might increase consumer health care utilization without increasing hospital inputs. My findings, however, indicate that from the provider side, the introduction of Medicare is associated with increased hospital capital and labor inputs.

Table 4 provides further insight into the nature of the providers' response to Medicare. Column 1 indicates that Medicare is associated with a moderate (but not statistically significant) decline in average length of stay; this may be because the marginal patient admitted as a result of Medicare is less sick than the average pre-Medicare patient. Column 2 indicates that Medicare is associated with an initial increase in occupancy rates (presumably due to the lag in bed construction), followed by a subsequent decline in occupancy rates in the second five years. This decline in occupancy rates may be due to Medicare's original generous capital depreciation allowances which created incentives for inefficient expansion (Somers and Somers 1967, United States Senate 1970), or it may have been an unintended consequence of poor forecasting of the increase in demand. The evidence in column 3 that the introduction of Medicare is associated with an increase in hospital wages (as measured by payroll expenses / employment) is somewhat surprising since the labor supply of nurses, technicians, and custodial staff is presumably relatively elastic.[11] One possibility is that the increase in wages reflects an increase in the marginal product of the hospital employees – perhaps due to the adoption of new technologies or to hiring of higher quality labor. Another possibility is that labor shared in some of the rents that hospitals received from the introduction of Medicare.

Most interestingly, the last two columns indicate that – after its first five years – Medicare is associated with an increase in hospital expenditures *per patient day*, whether measured as payroll expenditures or total expenditures. The increase in treatment intensity appears to reflect an increase in both employment and beds per patient day (results not shown). Given the large increase in admissions associated with Medicare's introduction (see Table 3), it is likely that the marginal admission is substantially less sick than the average pre-Medicare admit. As a result, the actual increase in treatment intensity – measured on a risk adjusted basis – is likely to be considerably larger than what has been measured here.

[11] Hospital employment and payroll expenses tend not include physicians. See Appendix A for more information.

The impact of Medicare on spending per patient day is particularly striking in light of the notable finding of the Rand HIE that increases in an individual's health insurance affected his probability of care utilization, but not the amount of spending conditional on utilization (Newhouse et al., 1993). This disparity is consistent with market-wide changes in health insurance – such as those induced by Medicare – having effects on practice patterns that small-scale changes do not and may provide a clue as to why –as the next sub sections demonstrates – the estimated effects of the impact of Medicare on spending are substantially larger than what the Rand estimates would have implied.

3.3 The magnitude of the impact of Medicare's introduction on aggregate spending

The results in Table 3 depict the change in the outcome in areas in which no one had insurance prior to Medicare relative to areas in which everyone had insurance prior to Medicare. To translate them into an estimated impact of Medicare, they must be multiplied by 0.75, the average percentage point increase in insurance coverage associated with the introduction of Medicare. The results therefore imply that the introduction of Medicare is associated with an increase in admissions between 1965 and 1970 of 49 log points (63 percent) and in total spending of 40 log points (49 percent). The increases continue in the second five year period, suggesting that the long-run impact of Medicare is even larger.

These estimates were at the hospital-level. This is a natural level of analysis as hospitals are the supply-side decision makers who may potentially respond to the incentives provided by Medicare. However, when the focus turns to the national magnitude of the impact of Medicare on the health care sector, estimates of the average hospital-level impact of Medicare may give a misleading impression if Medicare is associated with a change in the number of hospitals, or if there is substantial heterogeneity across hospitals in their response to Medicare. To investigate this, I aggregate the data to the hospital-market level in each year. To define a hospital market, I follow the general insights from the existing literature which 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, Gaynor and Vogt 2000, Dranove et al. 1992). This results in 211 separate markets.[12]

In addition, the previous estimates were based on a flexible specification with year fixed effects interacted with *percent uninsured.* Although this approach has the advantage of not imposing on the data ex-ante where any break in trend must occur, it has the disadvantage of not using all of the pre-period data to estimate the counterfactual trend in relative spending that would have occurred in the absence of the introduction of Medicare. In order to do so, I implement a deviation-from-trend specification:

$$\begin{aligned}\log(y_{mt}) &= \alpha_j * \mathbf{1}(market_m) + \beta_1 year_t + \beta_2 (year_t * pctuninsured_z) \\ &+ \beta_3 Mcareyear_t + \beta_4 (Mcareyear_t * pctuninsured_z) + X_{st}\beta + \varepsilon_{mt}\end{aligned} \quad (3)$$

The equation includes an indicator variable for each market (1(*market)*), a common time trend *(year)* and an interaction of the time trend with the percent uninsured in the market (*year*pctuninsured)*, so that the time trend may differ across markets based on the percent of the elderly without private insurance in this market prior to Medicare. To allow for a change in the slope of the time trend when Medicare comes in, *Mcareyear* measures the number of years that Medicare is in effect. It takes the value 0 up through and including 1965, 1 in 1966, 2 in 1967 etc.

The key variable of interest is *Mcareyear*pctuninsured* which allows for the change in the slope of the time trend in 1966 to differ based on the percent of the elderly without private insurance in the market.[13] As before, the regression allows for time varying state covariates (X_{st}), which in the baseline

[12] Rural hospitals are excluded from the analysis, as they are in many similar analyses, since the definition of rural hospital markets is considerably less clear (see e.g. Dranove et al. 1992). In 1965, the urban markets accounted for 70 percent of hospital spending. I verified that the implied effects of Medicare from the market-level analysis are robust to using the slightly higher estimates of insurance coverage rates in urban areas of the sub-region only (see Table 1). I also tried adding the rural hospitals to the analysis. I defined the rural market somewhat arbitrarily as the non-SMSA areas in each state and used the urban or rural insurance rates within the subregion as appropriate (see Table 1); the results were similar to those reported below. However, when rural areas are included in the analysis, the results are sensitive to not recognizing the substantially lower insurance coverage rates of rural areas within a sub-region.

[13] Based on the evidence in Figure 3, the specification does not include an intercept shift in 1966 with Medicare's introduction; in practice, if an intercept shift that is allowed to differ with the percent without private insurance is included, it tends to be insignificant and to not affect the coefficient of interest.

specification consist of a series of indicator variables for the number of years since (or before) the implementation of a Medicaid program in state *s*.

Table 5 shows the results from estimating equation (3). The top panel reports the results for the whole sample. The bottom panel limits the sample to the period 1960 to 1970, to allow the estimation of the trends over a smaller period. Both panels indicate that the introduction of Medicare is associated with an increase in all six outcome measures; the estimated magnitudes are similar in both panels. In addition, the right hand column suggests that Medicare is associated with the building of new hospitals.[14]

Since the coefficient of interest – *Mcareyear*pctuninsured* – depicts the annual change in the outcome in markets in which no one had insurance prior to Medicare relative to markets in which everyone had insurance prior to Medicare, it must be multiplied by 0.75 – the average percentage point increase in insurance coverage associated with the introduction of Medicare (see Table 1) – to translate into an estimated effect of Medicare. The estimates from the top panel (full sample of data) therefore imply that the introduction of Medicare is associated, in its first five years, with an increase in total hospital spending of 4 percent per year, and in hospital admissions of 5 percent per year. The estimates from the bottom panel (estimated off of the 1960-1970 data only) are slightly larger, implying an impact of Medicare on hospital spending of 4.5 percent per year and on hospital admissions of 6 percent per year.

The smaller results from the full sample therefore imply that, in its first five years, the introduction of Medicare was associated with a 22 percent increase in total hospital spending and a 28 percent increase in hospital admission. According to the 1963 Survey of Health Care Utilization, the elderly constituted 20 percent of total hospital spending. As a result, – if the impact of Medicare on were limited to the elderly – Medicare would have increased the elderly's hospital spending by 110 percent and the elderly's hospital admissions by 140 percent after it had been in effect for five years. In the sensitivity analysis in Section 6 I show that the implied magnitude is comparable across alternative specifications.

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[14] The standard errors are calculated allowing for an arbitrary variance-covariance matrix within each market. I verified that the p-values are very similar if instead I implement the randomized inference procedure developed by Bertrand et al. (2004).

These estimates are less than half the implied impact of Medicare in the point-to-point, hospital-level results in Table 3. [15] I examined the reason for this discrepancy and determined that aggregation of the data to the market level reduces the implied impact of Medicare because the estimated impact of Medicare is larger on smaller hospitals (as measured by number of beds prior to the introduction of Medicare) than on bigger hospitals (results not shown). It is difficult to determine whether this reflects heterogeneity across hospital sizes in the treatment effect or unobserved heterogeneity in the treatment size, as I only have data on insurance coverage at the sub-region level, not by hospital size within sub-region.

Even the smaller estimates from the market-level analysis suggest a substantial impact of Medicare. 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 imply that Medicare can account for about one-third of the growth in hospital spending over this five year period and all of the above-average growth relative to the previous 5 years.

4. Partial Equilibrium vs. General Equilibrium Effects of Health Insurance

4.1 Comparison to the Rand HIE estimates

The estimated impact of Medicare on the hospital sector is substantially larger than what would have been predicted based on existing evidence from the small-scale changes in health insurance in the Rand HIE. If we apply the estimates from the Rand experiment to predicting the impact of Medicare, they imply that Medicare would increase hospital spending by about 5.6 percent; Appendix B provides the details behind this calculation. This estimate is about one-fourth the magnitude of the 22 percent effect of Medicare on health spending in its first five years that I estimated above. Interestingly, the estimated one-year impact of Medicare on hospital spending (4 percent increase) is quite similar to the Rand estimate. This is consistent with Medicare's disproportionately larger impact stemming from its effect on the

[15] Interestingly, the simple time series comparison of the growth of the hospital sector since 1965 relative to a pre-existing quadratic trend (see Figure 2) yields estimates that are quite comparable to those in Table 5; the time series estimates suggest that Medicare is associated with a 27 percent increase in hospital spending by 1970. However, the time series evidence shows no impact of an effect of Medicare on hospital admissions.

overall practice of medicine, which might be expected to occur with something of a lag. Indeed, the estimates above indicate that the impact of Medicare on health spending has not been fully realized in Medicare's first 5 years. The long-term impact of Medicare on health spending is likely to have been even higher than the 5-year estimate.

Several aspects of the design of the Rand experiment facilitate the comparison of my estimates with what the Rand estimates predict the impact of Medicare would have been. The Rand experiment took place only shortly after the introduction of Medicare (it was conducted from 1974 to 1982), so that the time period of the estimates is similar. Both provided hospital insurance for free to the original beneficiaries, so that both estimates incorporate this positive income effect on health spending.[16] The experiment provides estimates of the effect of a moving from no insurance policy to a policy similar to the original Medicare policy (see Appendix B). It also provides estimates separately for hospital spending. Finally, the Rand experiment specifically investigated the impact of shorter versus longer time 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. However, the Rand experiment excluded individuals age 62 and over, while Medicare covers individuals age 65 and over. It seems doubtful that differences in the spending response of the elderly and non-elderly alone could be large enough to explain the over 4-fold higher estimated impact of Medicare on hospital spending. Indeed, a priori, it is not clear whether to expect a higher spending response to price subsidies by the elderly (for example, because they tend to be poorer than the non-elderly), or a lower response (for example, because their health problems are likely to be more severe.)

As discussed in Appendix B, it is more difficult to translate the Rand estimates into the implied increase in admissions they predict would be associated with the introduction of Medicare. Nevertheless, the available evidence suggests that the admissions response estimated above is also substantially larger than what would be predicted by the Rand estimates.

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[16] Medicare's hospital insurance was financed with a payroll tax on workers, to which the first generation of elderly beneficiaries therefore did not contribute.

Theoretically, the impact of market-wide changes in health insurance could be disproportionately smaller than the effect of an individual's change in health insurance. For example, models of physician-induced demand suggest that while the substitution effect of health insurance encourages increased physician treatment intensity and the income effect will encourage decreased physician treatment intensity. Because the income effect will loom larger for market-wide changes in insurance coverage relative to individual changes, the impact of market-wide changes in health insurance may be disproportionately smaller than the impact of individual changes (McGuire and Pauly, 1991).

There are two classes of theoretical 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 what evidence from small-scale changes in health insurance would suggest: the "fixed costs" and "spillovers" hypotheses.[17] The "fixed costs" 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 or physicians to incur the fixed costs of adopting new practice styles. For example, market-wide changes in health insurance may expand aggregate demand to the point where it is now possible to cover the fixed costs of providing a technology for which demand was not previously sufficiently high.

The "spillovers" hypothesis is that the typical insurance coverage in a community determines a community standard of care that affects the treatment of all patients in the community. As a result, changes in insurance coverage for a subset of patients can, by affecting the average insurance in the community, have spillover effects onto a hospital's treatment of other patients whose insurance coverage has not changed. [18] Although the original exploration of this hypothesis by Newhouse and Marquis (1978) found little support for it, several subsequent studies have found that variation in the typical insurance

[17] Another possibility is that a market-wide increase in health insurance may allow hospitals to increase the mark up that they charge for their services. While such an effect may well be present, it will not captured in my estimates which are of the effect of Medicare on hospital expenses, not hospital revenues. See Appendix A for more detail.
[18] Although this discussion is couched in terms of the disproportionately larger effects of Medicare on spending, we note that both hypotheses may also contribute to the apparent disproportionately larger impact of Medicare on hospital utilization.

coverage in a physician's practice or market area affects treatment intensity and health spending for other individuals (Glied and Graff Zivin 2002, Baker 1997, Baker and Shankarkumar 1998).

The spillovers and fixed costs hypothesis may be complementary. 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 non-elderly individuals. "Spillovers" may also result for reasons other than the joint nature of hospital production, such as medical ethics, fears of malpractice liability, or simply hospital income effects. While the fixed costs hypothesis entails fundamental non-linearities 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 non-linearities). The next two sub-sections provide suggestive empirical evidence for each hypothesis.

4.2 Evidence for the "fixed costs" hypothesis: the impact of Medicare on technology adoption

Since medical technologies are an important component of hospital costs, any impact of Medicare on the adoption of new technologies may be an important component of why market-wide changes in health insurance appear to have disproportionately larger effects than individual-level changes.[19] Theoretically, however, it is not obvious whether Medicare would have an affect on technology adoption decisions. By lowering the marginal cost of technology use, fee for service health insurance might induce technology adoption (Weisbrod, 1991). However, it is also possible that the income effect for physicians of an aggregate increase in health insurance might slow the adoption of new medical technologies (McGuire and Pauly, 1991). Or, physicians' training may cause them to act according to a "technological imperative" to adopt the latest new medical technology regardless of the insurance environment; indeed, American hospitals' exceptionalism in the eagerness to adopt new technologies was already well-noted in

[19] An impact of Medicare on technology adoption is particularly well-suited for supporting the "fixed costs" hypothesis since this theory is one of discrete changes in practice style, not in the marginal addition of lumpy goods. By contrast, the above evidence of a substantial impact of Medicare on the construction of new hospital beds is less obviously a contributor to the larger aggregate impact of health insurance. Assuming that hospitals price based on average costs, an insurance-induced increase in hospital beds will only disproportionately increase spending if the increase in supply itself generates its own increase in demand, or if hospitals are operating on the increasing part of their average cost curve.

the decades prior to introduction of Medicare (Stevens, 1999). The existing empirical evidence indicates that areas with higher managed care penetration exhibit slower rates of diffusion of new technologies (Cutler and Sheiner 1998, Baker 2001, Baker and Phibbs 2002). However, it is unclear whether any such effect is due to the financial incentives per se that are embodied in managed care, or to the direct oversight and regulation of technology adoption that is part and parcel of the managed care approach (Glied 2000, Baker and Phibbs 2002).

I focus on the impact of Medicare on whether hospitals adopt the then-new, high cost cardiac technologies. New cardiac technologies have played an important role in both the rise of health spending and the improvement in life expectancy over the last several decades (Cutler, 2003). Moreover, if Medicare does have an effect on technology adoption, it is especially likely to show up in the adoption of cardiac technologies which are used disproportionately for the elderly. There are no such technologies in the AHA data prior to Medicare; however, two important new cardiac technologies entered the data after 1965: open-heart surgery and the cardiac intensive care unit (CICU).[20]

To assess whether Medicare had an impact on hospitals' adoption of these technologies, I examine the geographic patterns of hospital adoption of these technologies across the various sub-regions that were more or less affected by the introduction of Medicare.[21] I employ two different control strategies to proxy for what this geographic adoption pattern would have looked like in the absence of Medicare. Both use technologies that are at a similar point in their nationwide diffusion to the cardiac technologies but are less likely to be affected by Medicare. One control strategy studies the geographic diffusion pattern of technologies that reached the diffusion level of the cardiac technologies prior to Medicare's introduction. The other control strategy studies the geographic diffusion pattern of technologies that diffused to roughly

[20] Goldman and Cook (1984) estimate that the CICU was the single most important medical intervention behind the rapid decline in cardiovascular disease from 1968 to 1976.

[21] The AHA data contain only binary information on whether the hospital has a given technology. I will therefore no capture any impact of Medicare on other margins such as how many machines the hospital has, how often they are used, or when they were last upgraded. Despite these limitations, the AHA technology data have been widely used to study the impact of managed care on hospital technology adoption in the 1980s and 1990s (e.g. Cutler and Sheiner 1998, Baker 2001, Baker and Phibbs, 2002).

the same level as the cardiac technologies in the post-Medicare period, but whose patient base is heavily skewed away from Medicare patients.

Evidence that the geographic diffusion pattern of the cardiac technologies is more skewed toward areas with a larger impact of Medicare than the geographic pattern for the control technologies is evidence against the null hypothesis that Medicare had no effect on the adoption of these new cardiac technologies. However, the analysis is not well suited to gauging the magnitude of any impact of Medicare on technology adoption, since it conditions on technologies having reached a given diffusion rate nationwide, and therefore does not pick up any impact of Medicare on the timing at which this nationwide diffusion rate is reached.[22]

I begin with an analysis of the impact of Medicare on the adoption of open heart surgery. 10 percent of hospitals in the data had open heart surgery technology in 1975. For one set of controls, I use four other high-technology, costly technologies that had reached the same diffusion point prior to Medicare: the post-operative recovery room (which reached the 10 percent diffusion level in 1951), the electroencephalograph (EEG) (1953), the diagnostic radioactive isotope therapy (1955) and the Intensive Care Unit (ICU) (1958).[23] For the other set of controls I use two technologies that are at a similar national diffusion rate to open heart surgery in the immediate post-Medicare period but that were much less likely to have their use reimbursed by Medicare: renal dialysis inpatient facilities and renal dialysis outpatient facilities. Renal dialysis is used on patients with end stage renal disease (ESRD). Prior to the 1973 – in which Medicare began covering patients with ESRD regardless of their age – virtually all patients with ESRD were under 65 and therefore not covered by Medicare (Eggers, 2000).[24]

[22] An alternative approach is to examine the impact of Medicare on the subsequent diffusion of technologies that were already in the data prior to 1965 (such as the diagnostic radioactive isotope, the postoperative recovery room, the EEG, or the intensive care unity) using the hazard model approach of Baker and Phibbs (2002) and Baker (2001). The results from such analysis are quite sensitive to specification and do not provide compelling evidence that Medicare had an effect on the diffusion of these technologies. One reason may be that unlike the cardiac technologies, these technologies were not disproportionately used by the elderly (see e.g. Russell 1979) and therefore the incentive effects of Medicare on their adoption may have been weak.

[23] See Russell and Burke (1975), Russell (1977) and Russell (1979) for detailed qualitative descriptions of these technologies.

[24] Another obvious choice would be technologies used for infants or child birth. Unfortunately, the neo-natal intensive care unit analyzed by Baker and Phibbs (2002) doesn't enter the data until after the period of analysis.

Table 7 shows the results. The left-hand panel shows the coefficient λ on *Percentuninsured* from estimating the following equation separately for open heart surgery and for each control technology:

$$Newtech_{is} = \lambda * Percentuninsured_{z} + X_{s}\beta + \varepsilon_{is} \quad (7)$$

Newtech is an indicator variable for whether hospital *i* in state *s* has acquired the new technology in they year of analysis. $Percentuninsured_z$ measures the percent of the elderly in the hospital's sub-region (*z*) who had private Blue Cross hospital insurance prior to Medicare (i.e. in 1963). X_s controls for state-level socio-economic conditions (specifically real per capita state income, state infant mortality rate, the rate of violent crime and state population). The controls for state-level characteristics which vary over time (and hence with the year of analysis) may help control for the fact that other factors – besides Medicare – may have changed before and after Medicare and may affect the geographic pattern of technology adoption. I report results from probit analysis of equation (7) with and without the covariate controls.

The first column of Table 7 indicates that hospitals in areas without any insurance in the 1963 were 6 to 7 percentage points *more* likely to have adopted open heart surgery by 1975 than areas in which everyone had insurance in 1963. By contrast, the next six columns indicate that when the control technologies had diffused to about 10 percent nationwide, areas without any insurance were 3 to 28 percentage points *less* likely to have adopted each of these new technologies than areas in which everyone had insurance.[25]

The right hand panel of Table 7 shows the results from the difference in differences analysis:

$$Newtech_{ist} = TREAT_{i} + \delta Percentuninsured_{z} + \lambda(Percentuninsured_{z} * TREAT_{i}) + X_{st}\beta + \varepsilon_{ist} \quad (8)$$

TREAT is an indicator variable for whether technology *i* is open heart surgery. The key variable of interest is *Percentuninsured**TREAT.

The difference-in-differences analysis using the four older technologies as controls (column 8) indicate that the geographic adoption pattern of open heart surgery is statistically significantly more

[25] In results not reported, I confirmed that these results are not sensitive to changing the year of analysis by a few years on either side, subject to data availability. Renal dialysis inpatient first comes in in 1968 at 13 percent therefore I use the first year. Renal dialysis outpatient reaches 10 percent in 1973.

skewed toward areas affected by Medicare than the geographic adoption patterns of these older technologies. The assumption behind this analysis is that absent Medicare, the differential adoption of cardiac technologies across areas of the country would have looked similar to the differential adoption of older technologies across areas of the country prior to Medicare. To the extent that there are substantial technology-specific idiosyncrasies in the geographic adoption pattern or that the geographic adoption pattern would have changed substantially over time even absent the introduction of Medicare, the validity of the identifying assumption is suspect. Some supportive evidence for the identifying assumption comes from the evidence of pronounced stability across time and across very different technologies in the geographic pattern of who are the "early innovators." In particular, Skinner and Staiger (2004) find a remarkable consistency in the states who were early adopters of β-Blockers in 2001 and those who were early adopters of hybrid corn in the 1930s and 1940s.

The difference-in-difference analysis using the two renal dialysis technologies as controls (column 9) also provides evidence against the null hypothesis that Medicare had no impact on cardiac technology adoption. An advantage of this strategy is that the control technologies are examined at essentially the same time period as the cardiac technologies, reducing concerns that the analysis is confounding the impact of Medicare with other secular time trends. The disadvantage of this strategy is that faced with more resources on average, hospitals may choose to spend some of it on new technologies, even if these new technologies have not become much more profitable at the margin. Such spillover effects will bias the estimates against finding an impact of Medicare on technology adoption. Consistent with the introduction of Medicare having some impact on the adoption of the dialysis technologies via its income effect on hospitals, the point estimates from the difference in difference analysis are somewhat larger when the older technologies are used as a control group than when the dialysis technologies are.

Table 8 shows the analogous analysis of the impact of Medicare on the cardiac intensive care unit, which first appears in the data in 1969 with a diffusion rate of 41 percent.[26] I compare the geographic pattern of adoption of the CICU in 1969 to the geographic pattern of adoption of two other forms of intensive care. One, the postoperative recovery room, had diffused to 41 percent prior to Medicare in 1958. The other, the general intensive care unit, was only slightly more diffused than the cardiac intensive care unit in 1969 (48 percent vs. 41 percent) but was not as concentrated in Medicare patients. Once again, the results are evidence against the null hypothesis that Medicare had no effect on the diffusion of the CICU. Again too, the difference in difference estimates are smaller when a less elderly-specific technology in the post-Medicare period is used as the comparison group (column 5)) than when a technology in the pre-Medicare period is used (column 4) , which is consistent with Medicare having some spillover effects on general technologies.[27]

Finally, since there was evidence that Medicare is associated with increased hospital construction (see Table 5), I verified that the findings in Table 7 and 8 are not simply due to newly created hospitals furnishing themselves with state of the art technology. Specifically, I found that the results from estimating equation (7) and (8) are robust to restricting the sample to the set of hospitals that already existed prior to 1965.

4.3 Indirect evidence of spillovers

If health insurance spillovers are quantitatively important, estimates of the impact of an individual's health insurance on health spending could produce considerably downward biased estimates of the aggregate impact of health insurance on health spending. One reason is that the nature of most empirical analyses of the impact of an individual's health insurance is to use other individuals in the same market with different health insurance as a comparison group; such analysis nets out any spillover effect from the

[26] This is an aberration for the AHA data (most technologies first appear with a diffusion rate of about 10 percent), and may in part reflect the fact that for awhile prior to its appearance in the data, the cardiac intensive care unit may not have been distinguished in data collection from the intensive care unit (Russell, 1979).
[27] An obvious choice for a non-Medicare intensive technology would be a new technology used for infants, such as the neo-natal intensive care unit studied by Baker and Phibbs (2002). Unfortunately, no such technologies are in the data during the time period I am examining.

estimated impact of health insurance on health spending. 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 ever 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 compare the estimated effect of Medicare above – which is calculated based on changes in spending across sub-regions – to estimates based on changes in spending for the elderly relative to the non-elderly. In a comparison of spending changes by the elderly relative to the non-elderly, 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 one's own insurance, conditional on average insurance coverage.

Consistent with potentially large spillovers, I find that this analysis based on the age-variation in Medicare coverage produces substantially smaller estimates of the impact of Medicare on hospital spending than the analysis based on variation across sub-regions. Table 6 shows mean hospital spending in 1963 and in 1970 for individuals aged 65 to 74 and for individuals aged 55 to 64. The estimates are based on individual-level data from the 1963 and 1970 Surveys of Health Service Utilization and Expenditures.[28] The difference-in-differences estimate in Table 6 suggests that the introduction of Medicare is associated with a (statistically insignificant) increase in hospital spending for the elderly relative to the non-elderly of 16 percent (with no covariate adjustment) or 30 percent (covariate adjusted). Even the larger estimate is about one third the size of the estimates in Section 3. Interestingly, it is quite similar to the 28 percent increase in elderly spending predicted by the Rand estimates.

Moreover, Table 6 indicates that hospital spending increased substantially for both the non-elderly and the elderly following the introduction of Medicare. Indeed, the simple time series estimate of the change in health spending for the elderly between 1963 and 1970 is over 7 times larger than the

[28] See Finkelstein and McKnight (2005) for a detailed description of the data as well as additional estimates.

difference-in-differences estimates. This is further suggestive of a potentially large role for the "spillovers" hypothesis in reconciling the larger estimates in this paper with the smaller ones implied by the partial equilibrium analysis in the Rand health insurance experiment and in Table 6.

5. 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 five. Over the same period, the average coinsurance rate for the population as a whole fell by about 50 percentage points (Gibson 1978, Cooper at al., 1976 and CMS 2002). A belief that the spread of fee for service health insurance might play an important role in the growth of health spending was one of the factors motivating the subsequent move toward managed care. Based on the results of the Rand experiment, Newhouse (1992) concludes that the spread of health insurance was not quantitatively important in contributing to the increase in spending over this time period.

I implemented the same calculation as in Newhouse (1992), but using the larger spending elasticities for market-wide changes in health insurance estimated above. These suggested that, in its first five years, Medicare – which decreased the average co-insurance rate in the population by about 7 percentage points – was associated with a 22 percent increase in total hospital spending.[29] This implies that the decrease in co-insurance rates of 50 percentage points between 1950 and 1990 would increase health spending by about 150 percent and can therefore explain about forty percent of the increase in health spending over this period.[30]

Moreover, I believe these estimates represent a lower bound on the contribution of the spread of health insurance to the rise in health spending. The impact of Medicare on health spending rises over the second five years of its existence, suggesting that the five-year impact does not reflect the full extent of

[29] The 7 percentage point decrease in co-insurance comes from the fact that Medicare increased the proportion of the elderly with meaningful health insurance by 75 percentage points, the elderly were 10 percent of the population, and Medicare imposed roughly a 5% co-pay (i.e. a one-day deductible for the first 60 days; average length of stay for the elderly prior to Medicare was 20 days).

[30] Between 1990 and 2000, the average co-insurance rate declined by another 5 percentage points and real per capita spending grew by another 30 percent, suggesting that the spread of insurance may also explain about half of the rise of spending between 1990 and 2000. However, the rise of managed care over this time period makes the application of the estimates of Medicare's spending effect to the impact of health insurance in the 1990s more suspect.

the impact of Medicare on health spending. In addition, the evidence of an impact of Medicare on technology adoption suggests that, by increasing the adoption of new cardiac technologies and therefore the expected market size for new medical technologies, Medicare may have also affected the incentives to develop new medical technologies, and thus the subsequent arrival rate of these new technologies. This dynamic feedback loop could produce long-run effects of Medicare on technological change in medicine and on health spending beyond the 10-year post-Medicare window analyzed here. Although in this paper I can not investigate directly whether Medicare is associated with increased *creation* of new technologies, empirical evidence of the effect of increased demand on innovation pharmaceutical industry raises the possibility that such a feed back mechanism may be present for hospital technologies as well (Finkelstein 2004, Acemoglu and Linn 2004).

Although my findings suggest a much larger role for health insurance in explaining the growth in health spending than previously thought (Newhouse 1992), they are still consistent with the consensus among health economists that technological change has been the primary factor behind the rapid increase in health expenditures over the last half century (Newhouse 1992, Fuchs 1996, Cutler 2004). For my findings suggest that the impact of market wide changes in health insurance on technology adoption (and perhaps creation) is likely to be an important part of the reason for its disproportionately larger effect of health insurance on health spending than implied by individual-level changes in health insurance.

6. Robustness

I investigate the robustness of the paper's findings to a wide variety of alternative specifications. Overall the results are remarkably robust to all of these different analyses. In this section I discuss three of the major types of robustness analysis performed. I focus on the robustness of the results from Section 3. Sensitivity of the technology adoption estimates in Section 4 was similar (not shown).

6.1 Investigating the identifying assumption

A primary concern is the validity of the identifying assumption that absent the introduction of Medicare in 1966, the different sub-regions of the country would not have exhibited divergent growth from the pre-period patterns. Figure 3 suggests visually 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 in 1965. To examine this more formally, I limit the data to the years prior to 1966 and examine the results of the two-year and five-year tests shown in Table 3 if – counter to fact – I assign some year prior to 1966 as the year in which Medicare was implemented. I do this for all possible years prior to 1966 in which I have enough data to implement each test. The results of this "falsification exercise" are shown in Table 9. They are broadly supportive of the identifying assumption; where the "trust test" yields a statistically significant estimate, all but one of the 69 "false tests" produce smaller (and generally statistically insignificant) estimated effects.[31]

A related concern with the identifying assumption is that different sub-regions of the country might have experienced differential changes in growth in the mid-1960s even absent Medicare. In particular, the poorer South and West might have started to catch up with the richer parts of the country. I therefore verified that the results are robust to including state-specific linear trends in equation (1). This is shown in rows (1) and (2) of Table 10, where, to conserve space, I only report the 5-year estimates; other estimates look similar.

It is also possible that the analysis spuriously attributes to Medicare the impact of other Great Society policies implemented in the mid 1960s, or the impact of changing economic conditions around this time more generally. Row 3 shows that the results are robust to adding time-varying covariates for state-levels of socio-economic progress. Specifically, I include real per capita state income, state infant mortality rate, the rate of violent crime (perhaps an indicator of social cohesion), and state population.[32] I also re-estimated equation (1) using these state-level variables as the dependent variable (and including state instead of county fixed effects). Neither infant mortality nor violent crime shows any evidence of a change after the introduction of Medicare in more affected areas relative to less. However, starting in the early 1970s real state per capita income starts to rise more quickly than previously in these more affected

[31] Of course the various "false tests" shown in Table 9 are not independent since the may involve overlapping years.
[32] 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.

areas relative to the less affected areas. This reversal underscores the need for caution in using the empirical approach to estimate the impact of Medicare too far beyond its introduction; however it is unlikely that the short-term (5-year) estimates of the impact of Medicare are mistakenly picking up the effect of underlying changes in income growth.

6.2 Sensitivity to sample definition

A second set of sensitivity analyses concerns the sample of hospitals included in the analysis. Given the different growth trajectory of the South in the 50s and 60s, as well as the impact of the civil rights movement and the Hill Burton hospital construction program in the South around the time of Medicare's introduction, row 4 of Table 10 reports the results of estimating equation (1) without the four southern sub-regions in the sample (which constitute about 30 percent of the sample). The estimated impact of Medicare on admissions, employment and total expenditures remains very similar; it is noticeably larger for patient days and beds, and noticeably smaller for payroll expenditures. To look more generally if the results are driven by one particular sub-region of the country, I implemented an informal jackknife-like procedure in which each of the 11 sub-regions is dropped in turn from the sample and equation (1) is re-estimated. The point estimates tend to lie in a nice tight pattern around the whole sample, and are almost always statistically significant (results not shown).

All of the results in the paper have been estimated for private hospitals. As discussed above, I suspect that the identification strategy is poorly suited to detecting the impact of Medicare on public hospitals. Nevertheless, if Medicare is associated with a switch in patients from public to private hospitals, looking just at private hospitals could produce a misleading picture of the impact of Medicare. Row 5 of Table 10 shows that the results are qualitatively robust to including public hospitals in the analysis, suggesting that the estimated impact of Medicare on private hospitals represents a net increase in activity, and not merely a switch of health care provision from public to private hospitals.[33]

6.3 Sensitivity to alternative sources of variation

[33] Consistent with the intuition that the empirical strategy is less likely to detect the impact of Medicare on public hospitals, the estimated impact of Medicare on the sample of all hospitals is lower than that for private hospitals for admissions, employment, payroll and total expenditures (although higher for beds and patient days).

The final set of sensitivity analyses uses alternative sources of cross-sectional heterogeneity in the impact of Medicare to identify its effect. Row 6 shows that the results are robust to interacting the year fixed effects in equation (1) with an indicator variable for whether the sub-region is one of the eight that has 75 percent or more of the elderly without BC hospital insurance (see Table 1), rather than the linear measure of percent without insurance used in the baseline specification. Row 7 shows the results are also robust to interacting the year effects with the percent of elderly without *any* hospital insurance (rather than without Blue Cross hospital insurance).

Since in principle the impact of Medicare depends not only on the proportion of the elderly without insurance but also the proportion of the population that is elderly, Row 8 shows that the results are quite similar if this additional variation is used as well.[34] This is not surprising since in practice there is very little variation in the percent elderly. Across the 11 sub-regions, the percentage of the elderly ranges only from 7.7 to 11.2 (even across counties, the inter-quartile range in percentage elderly is only 8.3 to 12.6). To allow for the greater variation in percentage elderly than exists across sub-regions, I also tried re-estimating equation (1) using the variation in the percentage elderly and insurance coverage across counties; the results were very similar (not shown).

Finally, in results not reported, I also found that the estimates were not sensitive to using variation in insurance coverage at the state or county level instead of the sub-region level.[35]

6.4 Sensitivity of implied magnitude of aggregate impact of Medicare

Since the sensitivity of the market-level estimates in Table 5 is also of interest, Table 11 repeats the sensitivity analyses in Table 10 for these results. The results are again generally robust to the array of alternative specifications (and to the other specification tests done throughout this section that are not

[34] The year fixed effects in equation (1) are interacted with the share of hospital expenditures covered by elderly insurance. This is calculated as the percent of the elderly without BC insurance times the proportion of hospital expenses that are elderly. The latter is estimated based on the percent of the population in the sub-region that is elderly, and my 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.
[35] State- and county-level insurance estimates are imputed based on the sub-region in which the area is located and information from the 1960 census on the percent of the county or state that is urban. In the NHS I can calculate insurance coverage rates separately for urban and rural areas within a subregion (see Table 1).

reported). In particular, the estimate from the baseline specification of the impact of Medicare on annual hospital spending of 4.5 percent is quite similar in the alternative specifications. Several yield estimates within 0.3 percentage points or less. Even the lowest estimate (3.5 percent) is still substantial.

7. Conclusion

This paper has examined the impact of market-wide changes in health insurance on the health care sector by studying the single largest change in health insurance coverage in U.S. history: the introduction of Medicare in 1965. I find robust evidence that Medicare's introduction is associated with an increase in hospital utilization, measurable hospital inputs (i.e. employment and beds), hospital spending, and hospital technology adoption.

The estimated effects are large. I estimate that Medicare is associated with a 22 percent increase in real hospital expenditures (for all ages) between 1965 and 1970, and even larger effects if the analysis is extended through 1975. Even the 5-year estimate is 4 times larger than what evidence from the impact of an individual's health insurance on health spending would suggest. Consistent with a disproportionately larger impact on health spending of market-wide changes in health insurance relative to smaller scale changes, I find evidence that the introduction of Medicare was associated with changes in medical treatment practice. For example, the introduction of Medicare appears to be associated with an increase in treatment intensity, as measured by spending per patient day. Perhaps most importantly, I find that the introduction of Medicare is associated with an increased rate of initial adoption of two then-new cardiac technologies, open heart surgery and the cardiac intensive care unit.

The five-year spending estimates imply that the introduction of Medicare can account for all of the above-average growth in hospital spending between 1965 and 1970, relative to the prior 5 years. More generally, they imply that the spread of health insurance between 1950 and 1990 can explain at least 40 percent of the 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 2002b). Whether other health insurance systems have a similar impact on technological change and health spending as the U.S. Medicare program is an important question for further work. If Medicare's impact on technology use in the United States influences the treatment patterns or coverage decisions of other countries' national health care systems, it is also possible that the effect of Medicare on health spending may substantially exceed its direct impact within the United States. This is also an interesting avenue to explore in future research.

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Table 1: Percent of Elderly Without Hospital Insurance, 1963

	Any Insurance	Blue Cross	Blue Cross By: Urban	Rural
New England (CT, ME, MA, NH, RI, VT)	0.37	0.49	0.48	0.55
Middle Atlantic (NJ, NY, PA)	0.41	0.60	0.57	0.71
East North Central, Eastern Part (MI, OH)	0.32	0.55	0.51	0.68
East North Central, Western Part (IL, IN, WI)	0.42	0.75	0.71	0.84
West North Central (IA, KS, MN, MO, NE, ND, SD)	0.47	0.81	0.75	0.91
South Atlantic, Upper Part (DE, DC, MD, VA, WV)	0.45	0.75	0.70	0.84
South Atlantic, Lower Part (FL, GA, NC, SC)	0.50	0.81	0.77	0.86
East South Central (AL, KY, MS, TN)	0.57	0.88	0.83	0.91
West South Central (AR, LA, OK, TX)	0.55	0.85	0.85	0.86
Mountain (AZ, CO, ID, MT, NV, NM, UT, WY)	0.50	0.78	0.73	0.88
Pacific (OR, WA, CA, AK, HI)	0.52	0.87	0.86	0.91
NATIONAL TOTAL	0.46	0.73	0.68	0.83

Note: Data are from individuals aged 65 and over in the 1963 National Health Survey. Sample size is 12,757. Minimum sample size for a sub-region is 377. Minimum sample size for an urban (rural) sub-region is 177 (123).

Table 2: Description of Dependent Variables from the AHA data

Outcome Category	Dependent Variable	First year data present	Sample Mean (1962 – 1964) Full Sample	North & Northeast	South & West
Expenditures	Total	1955	1,410	1,679	1,098
($1960, '000)	Payroll	1948	870	1,054	658
Major Inputs	Beds	1948	125	152	96
	Employment	1951	229	278	175
Utilization	Inpatient Admissions	1948	4,448	4,958	3,895
	Inpatient Days	1955	36,410	45,254	26,809

Note: All variables are measured annually at the hospital level for private hospitals. Employment and payroll expenditures exclude residents and interns.

Table 3: Impact of Medicare on Hospital Behavior

	Log Admissions	Log Patient Days	Log Employment	Log Beds	Log Payroll Expenditures	Log Total Expenditures
1. First Two Years: (1967-1965 vs. 1965-1963)	0.302** (0.040)	0.298*** (0.009)	0.268*** (0.006)	0.074 (0.518)	0.421*** (0.0001)	0.330*** (0.0002)
2. First Five Years: (1970-1965 vs. 1965-1960)	0.646*** (0.004)	0.435** (0.014)	0.386** (0.012)	0.279* (0.093)	0.715*** (0.0000)	0.532*** (0.0009)
3. First 10 Years (1975-1965 vs. 1965-1955)	0.673** (0.014)	0.479** (0.036)	0.457*** (0.010)	0.644*** (0.0001)	0.572*** (0.009)	0.492*** (0.009)
4. Second Five Yrs: (1975-1970 vs. 1965-1960)	0.361** (0.028)	0.198 (0.206)	0.287** (0.018)	0.323*** (0.006)	0.554*** (0.0005)	0.414*** (0.0014)
N	112,323	86,401	99,523	119,402	94,789	77,598

Notes: Table reports results from estimating equation (1) and calculating test statistics as shown in equation (2). P-values in Parentheses. Column heading shows dependent variable. ***, **, * denotes significance at the 1 percent, 5 percent, and 10 percent level respectively. Differences in sample size across the columns primarily reflect different starting years for the various variables (see Table 2); however, to some extent they also reflect different proportions of missing data (see Appendix A). Results are not sensitive to limiting all variables to a common sample.

Table 4: Impact of Medicare on Hospital Behavior: Additional results

	Log Length of Stay (i.e. patient days / admissions)	Log Occupancy (i.e. patient days / beds)	Log Wages (i.e. payroll expenditures / employment)	Log Total Expenditures Per Patient Day	Log Payroll Expenditures Per Patient Day
1. First Two Years: (1967-1965 vs. 1965-1963)	0.062 (0.60)	0.183** (0.015)	0.097* (0.077)	-0.050 (0.47)	0.077 (0.25)
2. First Five Years: (1970-1965 vs. 1965-1960)	-0.162 (0.299)	0.088 (0.28)	0.188*** (0.006)	-0.025 (0.77)	0.148 (0.18)
3. First 10 Years (1975-1965 vs. 1965-1955)	-0.160 (0.57)	-0.194 (0.129)	0.094 (0.310)	0.068 (0.60)	0.141 (0.33)
4. Second Five Yrs: (1975-1970 vs. 1965-1960)	-0.153 (0.35)	-0.138* (0.068)	0.120** (0.040)	0.189** (0.017)	0.255*** (0.010)
N	86,308	86,400	85,670	77,369	75,999

Notes: Table reports results from estimating equation (1) and calculating test statistics as shown in equation (2). Column heading shows dependent variable. P-values in parentheses. ***, **, * denotes significance at the 1 percent, 5 percent, and 10 percent level respectively.

Table 5: Impact of Medicare on hospital markets, spline analysis

	Log Admissions	Log Patient Days	Log Employ-ment	Log Beds	Log Payroll Expenditures	Log Total Expenditures	Log Hospital #s
Whole Sample							
Mcareyear* pctuninsured	**0.066*** (0.009)**	**0.067*** (0.008)**	**0.056*** (0.009)**	**0.094*** (0.008)**	**0.085*** (0.011)**	**0.054*** (0.009)**	**0.033*** (0.008)**
Year	0.038*** (0.001)	0.029*** (0.003)	0.062*** (0.002)	0.026*** (0.001)	0.118*** (0.002)	0.091*** (0.003)	-0.005*** (0.0009)
Year* pctuninsured	-0.0004 (0.0003)	-0.0005 (0.0005)	-0.0006 (0.0004)	-0.0003 (0.0004)	-0.0006* (0.0003)	-0.0007* (0.0004)	-0.0003 (0.0005)
Mcareyear	-0.046*** (0.008)	-0.050*** (0.007)	-0.044*** (0.007)	-0.058*** (0.006)	-0.103*** (0.009)	-0.040*** (0.008)	-0.010 (0.007)
N	5,524	4,292	4,913	5,533	5,467	4,264	5,533
Sample limited to 1960-1970 only							
Mcareyear* pctuninsured	**0.073*** (0.018)**	**0.102*** (0.021)**	**0.071*** (0.019)**	**0.098*** (0.018)**	**0.045* (0.025)**	**0.060*** (0.021)**	**0.055*** (0.016)**
Year	0.029*** (0.005)	0.032*** (0.005)	0.053*** (0.006)	0.027*** (0.005)	0.091*** (0.007)	0.092*** (0.007)	-0.001 (0.004)
Year* pctuninsured	-0.0013** (0.0007)	-0.0014* (0.0007)	-0.002*** (0.0006)	-0.0015** (0.0007)	-0.0015** (0.0007)	-0.0017** (0.0007)	-0.0016** (0.0008)
Mcareyear	-0.052*** (0.016)	-0.069*** (0.019)	-0.037** (0.018)	-0.057*** (0.016)	-0.025 (0.022)	-0.012 (0.020)	-0.028* (0.015)
N	2,238	2,238	2,238	2,241	2,232	2,232	2,241

Notes: Table reports results from estimating equation (3). Each observation is a hospital market – year. Standard errors are in parentheses and allow for an arbitrary variance-covariance matrix within each market over time.

Table 6: Changes in hospital spending for individuals aged 65-74 and individuals aged 55-64

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

Note: All dollars are in year 2000 dollars. Data are from the 1963 and 1970 Surveys of Health Service Utilization and Expenditures. N=3,030 (pooled sample). 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).

Table 7: Medicare and the adoption of open heart surgery

	Open Heart Surgery	Earlier Technologies				Less Medicare-specific technologies (same time period)		Difference-in-Differences Analysis	
		Post-op recovery room	EEG	Diagnostic Radioactive Isotope	Intensive Care Unit	Renal Dialysis, Inpatient	Renal Dialysis, Outpatient	Earlier Technologies As Control	Less Medicare-specific (but same time period) as control
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
W/o covariates	**0.069**	-0.089**	-0.18***	-0.275***	-0.097**	-0.137**	-0.082	0.230***	0.151**
	(0.054)	(0.038)	(0.048)	(0.066)	(0.046)	(0.059)	(0.053)	(0.046)	(0.061)
With covariates	**0.062**	-0.072*	-0.074*	-0.150**	-0.034	-0.086*	-0.033	0.185***	0.145**
	(0.044)	(0.043)	(0.040)	(0.062)	(0.057)	(0.059)	(0.037)	(0.055)	(0.057)
Year	**1975**	1951	1953	1955	1958	1968	1973		
Mean dep var	**0.106**	0.010	0.097	0.108	0.101	0.128	0.101		

Note: All estimates are marginal effects from probit estimation. Left hand panel reports the marginal effect of *Pctuninsured* from estimation of equation (7); dependent variable is shown in column heading. Right hand panel reports marginal effect of the interaction of *Pctuninsured* with TREAT indicator from estimation of equation 8. TREAT is 1 for open heart surgery, 0 otherwise. Standard errors (in parentheses) are adjusted for correlation within states. First row reports results from regressions without covariates. Second row reports results from a separate regression in which controls for state-level socio-economic characteristics are included.

Table 8: Medicare and the adoption of the Cardiac Intensive Care Unit

	Cardiac Intensive Unit	Post-operative recovery room	Intensive care unit	Difference-in-Differences	
				Control: Post-Op Recovery Room	Control: Intensive Care Unit
W/o covariates	**-0.094**	-0.44***	-0.291*	0.346***	0.289**
	(0.246)	(0.154)	(0.165)	(0.113)	(0.129)
With covariates	**0.158**	-0.364**	-0.143	0.369***	0.287**
	(0.113)	(0.147)	(0.112)	(0.110)	(0.133)
Year	**1969**	1958	1969		
Mean dep var	**0.41**	0.41	0.48		

Note: See notes to Table 7. In the difference-in-differences analysis, TREAT is 1 for CICU technology.

Table 9: Falsification exercise on pre-period (actual estimates from Table 3 are given in **bold**).

Log Admissions	Log Patient Days	Log Employment	Log Beds	Log Payroll Expenditures	Log Total Expenditures
Distribution of estimates from two-year test					
-0.208*			-0.106	-0.438***	
-0.154			-0.056	-0.240	
-0.111		-0.154	-0.054	-0.077	
-0.078		-0.128	-0.053	-0.033	
-0.073	-0.150	-0.121	-0.046	-0.016	-0.155
-0.050	-0.043	-0.104	-0.028	-0.001	-0.140
-0.054	-0.026	-0.032	0.015	0.091	-0.120
-0.044	-0.025	0.003	0.042	0.107	-0.081
-0.013	-0.020	0.009	0.050	0.137	0.005
0.032	-0.012	0.101	**0.074**	0.145	0.019
0.290	0.019	**0.268*****	0.078	0.145	0.167
0.302**	**0.298*****	0.269**	0.160	**0.421*****	**0.330*****
Distribution of estimates from five-year test					
-0.283			-0.129	-0.648***	
0.061			0.028	-0.057	
0.098		-0.225	0.032	0.055	
0.243		0.035	0.081	0.138	
0.398*		0.213	0.097	0.246	
0.483**	-0.196	0.306*	0.109	0.319	-0.396*
0.646***	**0.435*****	**0.386****	**0.279***	**0.715*****	**0.532*****

Note: Table reports the results from estimating the first two years test and the first five years test (see Table 3 and equation 2 for details) when equation (1) is estimated on a sample restricted to years 1965 and earlier, and a randomly assigned year prior to 1965 is selected for the test. **Bold** estimates are the actual estimate from Table 3. ***, **, * denotes significance at the 1 percent, 5 percent, and 10 percent level respectively.

Table 10: Estimated Impact of Medicare in First Five Years; Alternative Specifications

		Utilization		Inputs		Expenditures	
		Log Admissions	Log Patient Days	Log Employment	Log Beds	Log Payroll Expenditures	Log Total Expenditures
1.	**Baseline specification**	**0.485*** (0.004)**	**0.326** (0.014)**	**0.290** (0.012)**	**0.209* (0.093)**	**0.536**** (0.0000)**	**0.399*** (0.0009)**
2.	State-specific linear trends	0.458*** (0.006)	0.362*** (0.008)	0.283** (0.014)	0.212* (0.087)	0.504*** (0.0001)	0.386*** (0.002)
3.	Time varying covariates	0.564*** (0.002)	0.372*** (0.006)	0.332*** (0.005)	0.230* (0.066)	0.591*** (0.0000)	0.469*** (0.0001)
4.	w/o 4 south'n sub-regions	0.532*** (0.0006)	0.454*** (0.0002)	0.286** (0.013)	0.310*** (0.010)	0.396*** (0.009)	0.386*** (0.002)
5	Including public hospitals	0.378** (0.018)	0.425*** (0.0001)	0.254** (0.017)	0.346** (0.048)	0.420*** (0.0006)	0.249** (0.039)
6.	Indictor for 75% or more w/o BC ins	0.402** (0.015)	0.321** (0.016)	0.262** (0.025)	0.257** (0.029)	0.467*** (0.002)	0.324*** (0.008)
7.	% w/o *any* private insurance	0.444** (0.033)	0.309** (0.038)	0.301** (0.030)	0.128 (0.376)	0.636*** (0.0001)	0.453*** (0.002)
8.	pctuninsured * elderly share of hosp spend	0.360** (0.019)	0.302** (0.017)	0.178 (0.114)	0.210* (0.068)	0.382** (0.014)	0.298*** (0.006)

Notes: Table cells report the implied impact of Medicare based on estimating some variant of equation (1), performing the "First Five Years" test in equation (2), and then performing the appropriate adjustment to this test statistic to translate it into an estimated impact of Medicare. This last step is done so that the estimates are directly comparable across specifications. Columns show the dependent variable. P-values are in parentheses. ***, **, * denotes significance at the 1 pct, 5 pct, and 10 pct level respectively. Each row reports the results from a deviation from the baseline specification as follows (additional details are in text):

- Row 1: Baseline specification: Estimates of equation 1 as shown in Table 3, then multiplied by 0.75 (the average % of the elderly without BC insurance prior to Medicare).
- Row 2: Row 1 with state-specific linear trends included in the regression.
- Row 3: Row 1 with 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 sub-regions (and therefore multiplying by 0.7, the average % of the elderly without BC insurance in the non-Southern United States).
- Row 5: Row 1 with public hospitals added in
- Row 6: Interacts year effect w/ indicator variable for sub-region has >=75% of elderly w/o BC insurance. Estimates are multiplied by 2.7 since on average areas with >=75% of elderly w/o BC insurance have 28 percentage points less insurance than areas where <75% of the elderly are without BC insurance.
- Row 7: Interacts year effect with percent of the elderly without *any* private hospital insurance, and therefore multiplies by 0.45 (the percent of the elderly without any hospital insurance).
- Row 8: Uses variation in % of population that is elderly as well as % of elderly w/o BC insurance. Coefficient is 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.

Table 11: Estimated Impact of Medicare on Hospital Markets Alternative Specifications

		Log Admiss-ions	Log Patient Days	Log Employ-ment	Log Beds	Log Payroll Expends	Log Total Expends	Log Hosp #s
1.	**Baseline specification**	**0.055*** (0.000)**	**0.077*** (0.000)**	**0.053*** (0.000)**	**0.074*** (0.000)**	**0.034* (0.070)**	**0.045*** (0.005)**	**0.041*** (0.001)**
2.	State-specific linear trends	---	---	---	---	---	---	---
3.	Time varying covariates	0.048*** (0.001)	0.065*** (0.000)	0.042*** (0.006)	0.050*** (0.000)	0.022 (0.26)	0.042** (0.016)	0.013 (0.032)
4.	w/o 4 south'n sub-regions	0.036*** (0.000)	0.053*** (0.002)	0.038** (0.012)	0.046*** (0.000)	0.021 (0.23)	0.036** (0.034)	0.014 (0.30)
5	Includes public hospitals	0.068*** (0.000)	0.069*** (0.000)	0.076*** (0.000)	0.066*** (0.000)	0.068*** (0.000)	0.081*** (0.000)	0.069*** (0.000)
6.	Indictor for 75% or more w/o BC ins	0.054*** (0.000)	0.076*** (0.000)	0.057*** (0.000)	0.076*** (0.000)	0.035* (0.068)	0.046*** (0.008)	0.030** (0.024)
7.	% w/o *any* private insurance	0.059*** (0.000)	0.085*** (0.000)	0.055*** (0.000)	0.081*** (0.000)	0.045** (0.035)	0.049*** (0.006)	0.058*** (0.000)
8.	pctuninsured* elderly share of hosp spend	0.026** (0.043)	0.048*** (0.002)	0.028** (0.048)	0.046*** (0.001)	0.032* (0.083)	0.035** (0.041)	0.010* (0.043)

Notes: Table cells report the coefficient on *Medicare*pctuninsured* from estimating equation (3) on the years 1960 – 1970. Columns show the dependent variable. P-values are in parentheses. ***, **, * denotes significance at the 1 pct, 5 pct, and 10 pct level respectively. Each row reports the results from a deviation from the baseline specification as follows (additional details are in text):

- Row 1: Baseline results from Table 5, multiplied by 0.75 (the average % of the elderly without BC insurance prior to Medicare in urban areas).
- Row 2: Row 1 with state-specific linear trends included in the regression. Not included in spline specification.
- Row 3: Row 1 with time-varying covariates included in the regression.
- Row 4: Row 1 without 4 Southern sub-regions (and therefore multiplying by 0.70, the average % of the elderly without BC insurance in the non-Southern United States).
- Row 5: Row 1 with public hospitals added in
- Row 6: Interacts year effect w/ indicator variable for sub-region has >=75% of elderly w/o BC insurance. Estimates are multiplied by 2.7 since on average urban areas with >=75% of elderly w/o BC insurance have 28 percentage points less insurance than areas where <75% of the elderly are without BC insurance.
- Row 7: Interacts year effect with percent of the elderly without *any* private hospital insurance, and therefore multiplies by 0.45 (the percent of the elderly in urban areas without any hospital insurance).
- Row 8: Uses variation in % of population that is elderly as well as % of elderly w/o BC insurance. Coefficient is 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.

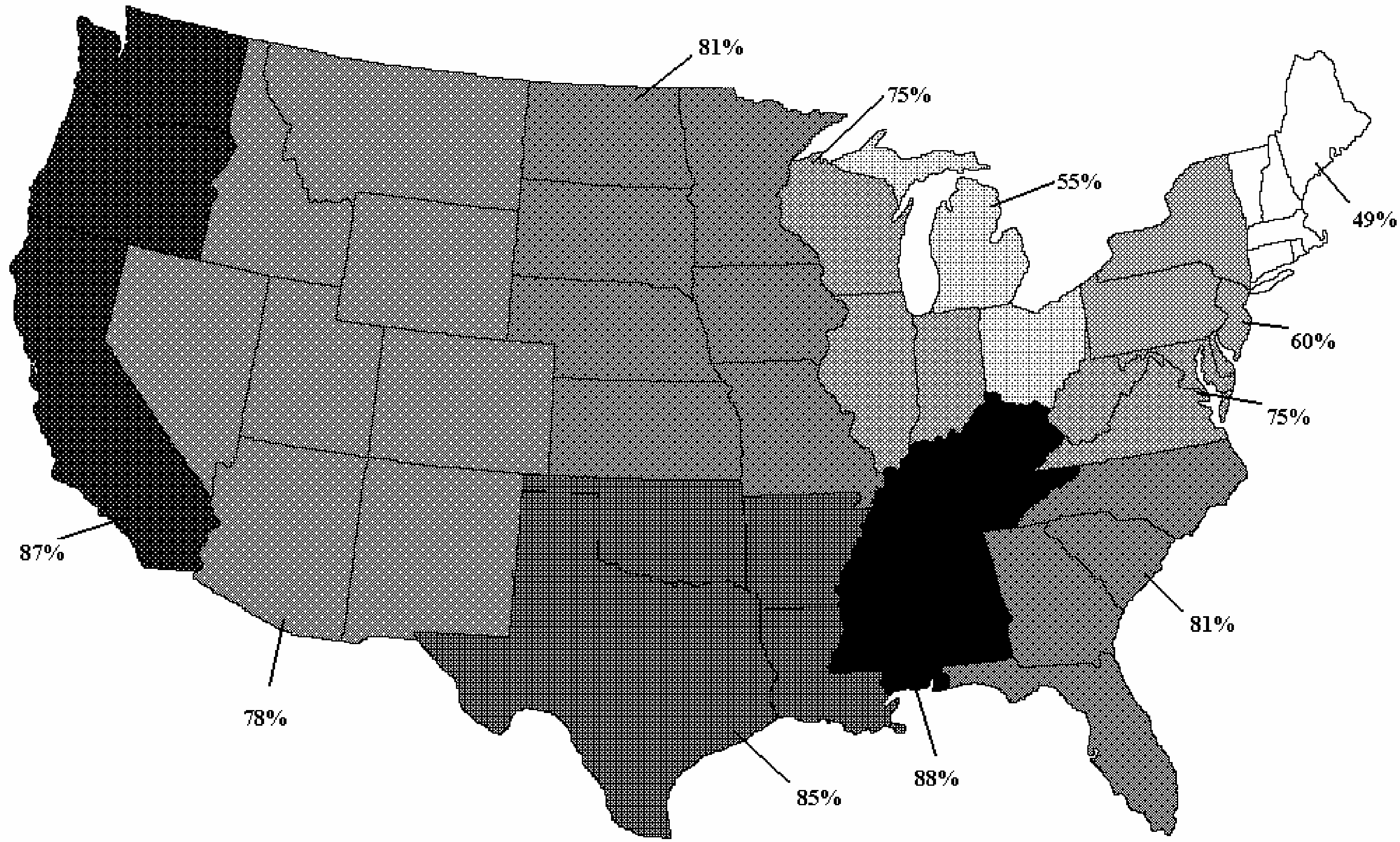
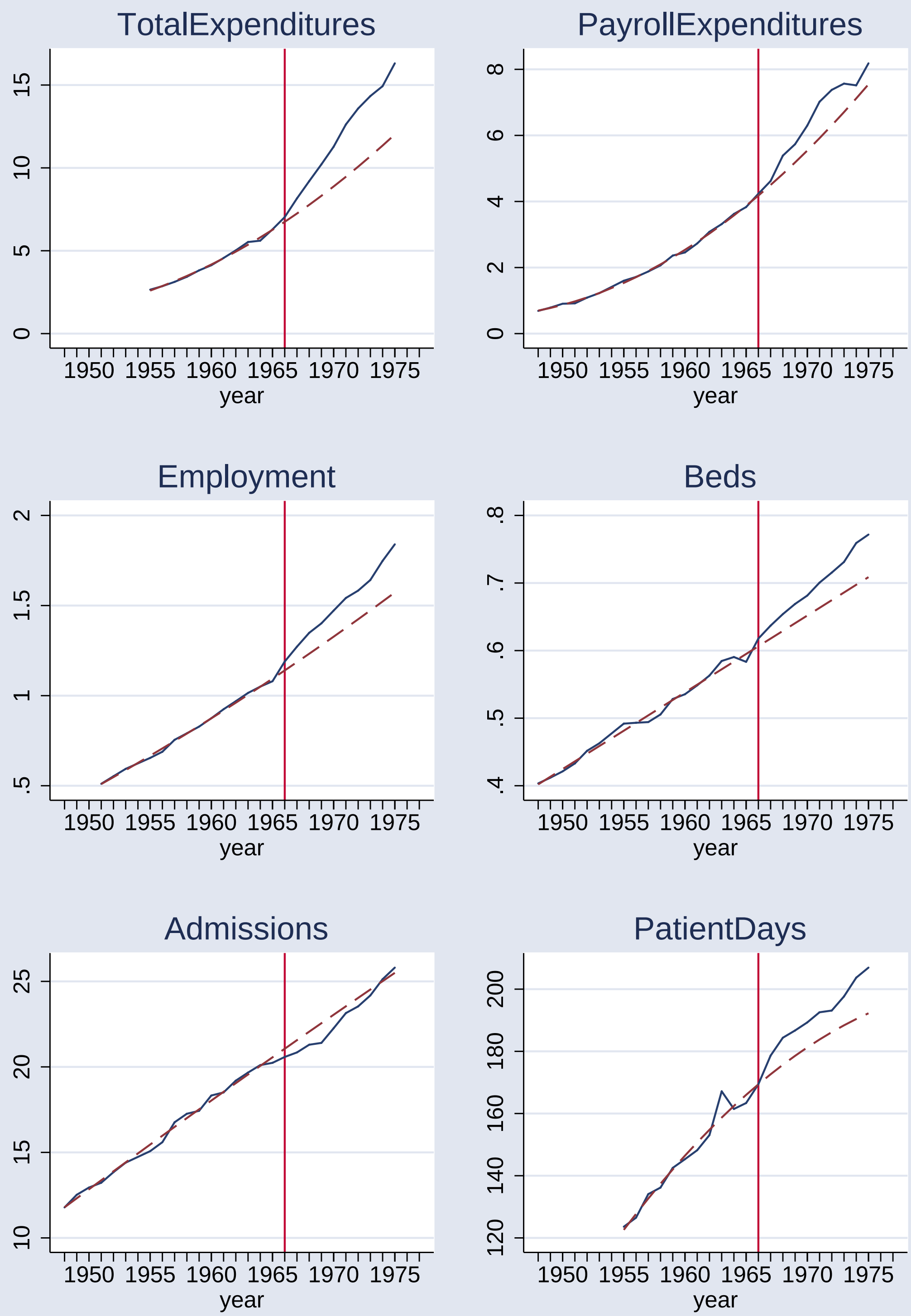


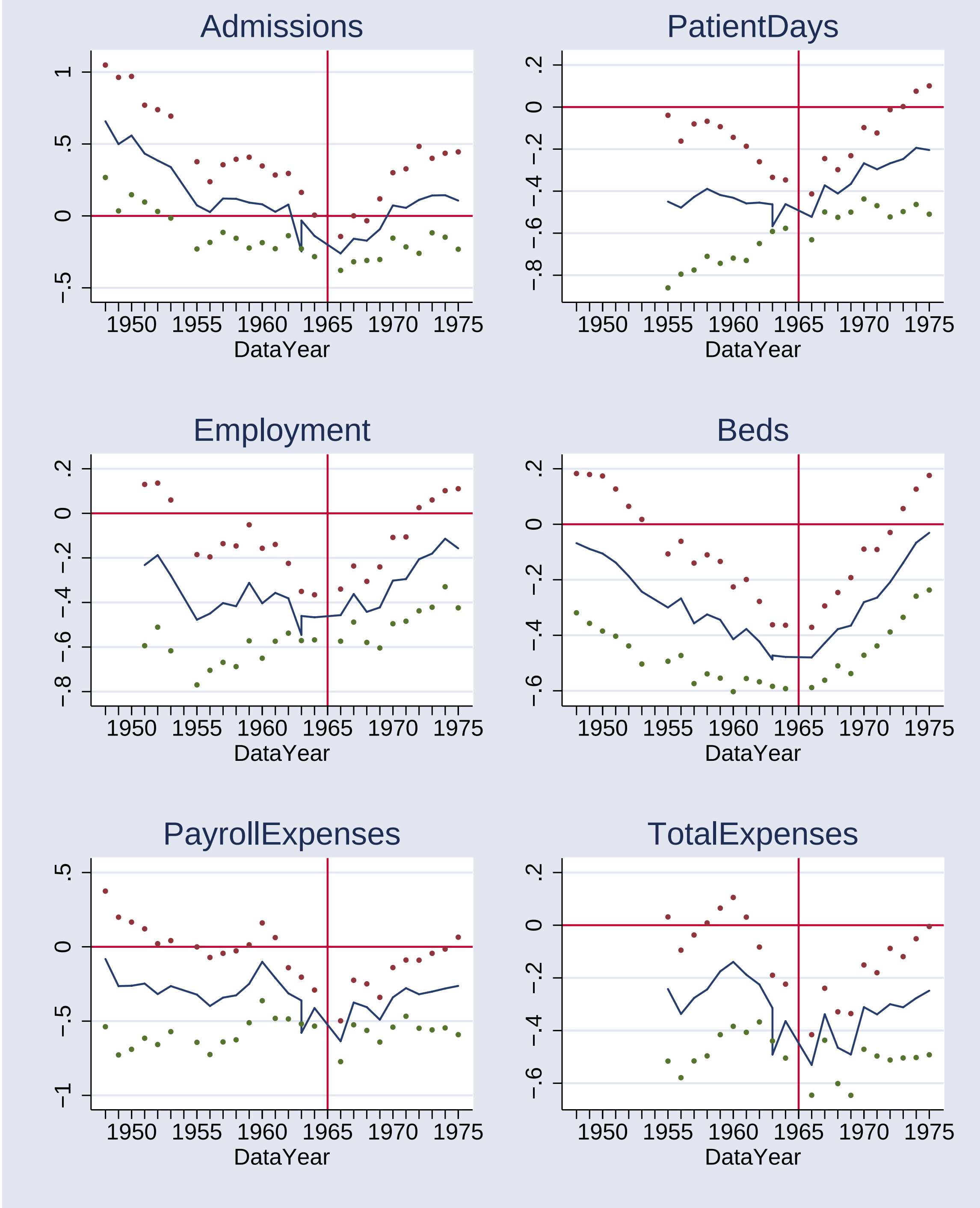
Figure 1: Percent of Elderly Without Blue Cross Hospital Insurance by Sub-region. Data are from 1963 National Health Interview Survey. Darker areas denote a higher percent without Blue Cross Health Insurance; Lighter areas denote a lower percent without Blue Cross health insurance.

Figure 2: National Time Series Patterns



Note: Y-axis scale is in millions. Expenditure variables are in constant (1960) dollars. Dashed line shows quadratic time series trend predicted off of the pre-1965 data.

Figure 3: Baseline Specification



Note: Figure 3 graphs the pattern of the λ_t coefficients from estimating equation (1) for the dependent variable given above each graph; all dependent variables are in logs. The dots show the 95 percent confidence interval on each coefficient; the omitted year is 1965. The scale of the graph is normed in the reference year (1965) to the average difference in the dependent variable between the south and west relative to the north and northeast.

Appendix A: The American Hospital Association (AHA) Historical Data.

The American Hospital Association Data are taken from the hospital-level data published in the annual August issue of *Hospitals: The Journal of the American Hospital Association*.

Sample definition and time period
The results are based on receipt of hospital surveys sent to every AHA-registered hospital in the United States. Receipt of the survey is generally required by February for inclusion in the August issue. For flow data (such as expenditures, employment, and patient days) , the survey asks hospitals to report for the 12-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 issue 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 response rate to the AHA surveys for the period I am studying is over 90 percent in all the years in question (and often above 96 percent).[36] 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.

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

Total Expenditures: Total expenses for a 12 month period.

- These consist of payroll and non payroll expenses. Non-payroll expenses include employee benefits, professional fees, depreciation expenses, interest expenses, and other expenditures (supplies etc.). Non-payroll expenses are about 40 percent of total hospital expenditures.
- 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: Payroll expenses for a 12-month period.

- Payroll expenses include all salaries and wages (for both full time and full-time equivalents of part-time personnel) except for 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.

- Paid personnel do not include most physicians, since most physicians are not directly employed by the hospital.
- The 1964 data indicate that just over half of the paid personnel are devoted to the "professional care of patients" (i.e. nurses and technicians); the remainder are divided among a variety of custodial and administrative functions.

Admissions: Total inpatient admissions for a 12-month period, excludes newborns.

Average Daily Census: Average number of inpatients receiving care each day during a 12-month period; excludes newborns.

- Patient Days measure used in paper is created by multiplying average daily census by 365

[36] This is considerably higher than the response rate in more recent decades, and likely reflects the fact that the earlier surveys asked for considerably less detailed information than more recent surveys.

Appendix B: Predictions from the Rand HIE for the impact of Medicare on hospital spending

To calculate what the Rand HIE estimates imply for the predicted impact of Medicare, I use the Rand estimates of the impact of moving from no insurance to a policy that approximates the original Medicare policy. Medicare hospital insurance originally imposed a $40 deductible (in 1965 dollars) and then no co-payment for the first 60 days.[37] Since the average length of a hospital stay for the elderly in 1963 was only 20 days – and only 5 percent of elderly hospital visits entailed stays of more than 60 days – it seems reasonable to approximate Medicare's hospital coverage as having a 0 co-pay[38]. Accounting for the fact that Medicare introduced an 25% co-pay after 60 days would only decrease the implied spending effect of Medicare further).

The results from the Rand experiment suggest that the effect of an individual moving from no insurance to a policy with no co-pay and a $125 deductible (in 1983 dollars, which is equivalent to a $40 deductible in 1965 dollars) would be to increase hospital spending by 37 percent . These estimates can be found in Keeler et al. (1988) and Newhouse et al. (1993), especially pages 129- 130. Therefore, the implied effect from the Rand experiment of moving 75 percent of the elderly from no insurance to Medicare's hospital insurance would be to increase hospital spending among the elderly by 28 percent, or total hospital spending by about 5.6 percent.

Importantly, these estimates represent the "pure" effects of cost-sharing in which – as is the case with Medicare – there are no limits on the individual's out of pocket payments. In practice, The Rand experiment placed limits on the maximum out of pocket spending the individual could have. Keeler et al. (1988) describe how the cost-sharing effects in the absence of such limits can be estimated.

It is more difficult to perform a similar comparison to the implied impact of Medicare from the Rand HIE on hospital admissions or patient days. Unlike for spending, estimates for health care utilization were not calculated to allow computation of the "pure" effects of cost-sharing separately from the effects of limits to maximum out of pocket expenditures. Nonetheless, the results of cruder comparisons still suggest that the implied impact of Medicare from the Rand experiment on hospital admissions would be substantially lower than what I have estimated here (see e.g. Newhouse et al., 1993 Table 3.2).

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[37] The deductible was set to be the cost of a day of hospital care (Somers and Somers, 1967).

[38] Author's estimates on elderly length of stay are based on the 1963 Survey of Health Service Utilization and Expenditures.