Income and Health Spending: Evidence from Oil Price Shocks*

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Abstract

Health expenditures as a share of GDP have more than tripled over the last half century. A common conjecture is that this is primarily a consequence of rising real per capita income, which more than doubled over the same period. We investigate this hypothesis empirically by instrumenting for local area income with time-series variation in global oil prices between 1970 and 1990 interacted with cross-sectional variation in the oil reserves across different areas of the Southern United States. This strategy enables us to capture both the partial equilibrium and the local general equilibrium effects of an increase in income on health expenditures. Our central estimate is an income elasticity of 0.7, with an elasticity of 1.1 as the upper end of the 95 percent confidence interval. Point estimates from alternative specifications fall on both sides of our central estimate, but are almost always less than 1. Consistent with our finding that health spending does not appear to be a luxury good, we do not find a significant effect of increased income on hospital technolgy adoption; this suggests that there are unlikely to be substantial global general equilibrium effects (which would not be estimated by our empirical strategy) of rising income on health spending via induced innovation. Our overall reading of the evidence is that rising income is unlikely to be a major driver of the rising health share of GDP.

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1 Introduction

The dramatic rise in health care expenditures is one of the notable economic trends of the postwar era. As seen in Figure 1, health care expenditure as a share of GDP in the United States has more than tripled over the last half century, from 5 percent in 1960 to 16 percent in 2005 (CMS, 2006). A common conjecture is that the rise in the share of income spent on health care expenditures is a direct, or at least a natural, consequence of the secular increase in living standards —because health care is a luxury good. The *Economist* magazine stated this as a "conventional wisdom" in 1993, writing:

> "As with luxury goods, health spending tends to rise disproportionately as countries become richer." (quoted in Blomqvist and Carter, 1997, p. 27).

This view has recently been forcefully articulated by Hall and Jones (2007). They argue that the optimal share of spending on health increases as incomes rise, since spending money on life extension allows individuals to escape diminishing marginal utility of consumption within a period. The view that health care is a luxury good also receives support from the very high estimates of the value of life and value of health provided by Nordhaus (2003) and Murphy and Topel (2003, 2006). The fact that most other OECD countries have also experienced substantial growth in their health sector over the last half century (OECD 2004) also suggests that rising income, rather than idiosyncratic features of the institutional structure of the U.S. health care system, is a natural candidate to explain the rise in the health share of GDP in the U.S.

Understanding the extent to which the rise in the health share of GDP is a direct consequence of the rise in living standards is important for several reasons. First, it is necessary for a proper accounting of the notable growth in the US (and OECD) health care markets over the last half century. Second, it is necessary for forecasting how health care spending will likely evolve in coming years. Finally, understanding the role that rising income plays in the rising health share of GDP is a crucial first step towards an assessment of the optimality of the growth of the health care sector. In particular, if demand for health care is strongly increasing in income, so that rising income can explain most or all of the rising health share, it would be more likely that the increasing share of GDP allocated to health is socially optimal (although of course a systematic analysis of social optimality would require taking into account the informational and institutional constraints in the health care market).

The relationship between income and health spending is the subject of a voluminous empirical literature. Nevertheless, almost all of the existing evidence comes from simple correlations of income and health care spending, across individuals, across countries, or over time. These correlations are consistent with income elasticities ranging from close to zero to substantially above one.[1] In light of the paucity of existing evidence, Hall and Jones (2007) conclude their paper by stating that "Our model makes the strong prediction that if one looks hard enough and carefully enough, one ought to be able to see income effects [with elasticities above 1] in the micro data. Future empirical work will be needed to judge this prediction."

Our objective is to provide causal estimates of the effect of income on aggregate health spending. There are (at least) two important challenges in this exercise. The first important challenge is that income and health covary at the individual or regional level for a variety of reasons. Therefore, simple correlations are unlikely to correspond to causal estimates of the effect of income on health spending.

A second important challenge is that a thorough investigation of the role that rising income plays in the growth of the health care sector requires incorporating the effects of income on health spending in general equilibrium. Partial and general equilibrium income elasticities may differ for a variety of reasons. On the one hand, because an increase in the demand for health care from a community (a "general equilibrium change") may prompt changes in medical practice styles, including the adoption (and possibly development) of new technologies, the general equilibrium effect of rising income may be larger than the partial equilibrium effect. Indeed, Finkelstein (2007) estimates that, for this reason, the general equilibrium effect of health insurance coverage on health spending is larger than the partial equilibrium effect. On the other hand, if the supply of health care is less than perfectly elastic, an increase in the demand for health care from a community will increase price; if it is also the case that the price elasticity of health care demand is greater than one, the increase in health care expenditure in general equilibrium will be less than in partial equilibrium. Many of the potential general equilibrium effects are "local" in the sense that they result from the increase in incomes in a particular region or local economy. These effects can be detected by looking at increased health spending and technology adoption in the local economy. In addition, there may also exist "global" general equilibrium effects, related to the development of new technologies for the entire United States or the world economy.

[1] OECD (2006) provides a recent survey of the large empirical literature on the correlation between income and health spending (see particularly Annex 2B). The cross-sectional relationship across individuals between income and health spending tends to be small or negative (e.g. Newhouse and Phelps 1976). In contrast, cross-country analysis tends to suggest income elasticities greater than 1 (e.g., Newhouse 1977, Gerdtham and Jonsson 2000), as do time series analysis of the relationship between income growth and growth in health spending over a country's development (e.g., Fogel 1999).

We confront both of these challenges. By exploiting potentially exogenous variation in local incomes, we attempt to estimate causal elasticities that incorporate local general equilibrium effects. On the basis of our estimates, we also argue below that global general equilibrium effects are unlikely to be significant in this instance.

Our strategy is to exploit the time-series variation in global oil prices between 1970 and 1990, which impacted incomes differentially across different parts of the (Southern) United States that vary in the oil intensity of the local economy. We approximate local economies by Economic Sub Regions (ESRs), which consist of groups of counties within a state that have strong economic ties. We focus on the South of the United States to increase the comparability of the ESRs, in particular to minimize the likelihood of differential trends in health care expenditure driven by other factors. Our empirical strategy exploits the interaction between global oil prices and ESR-level importance of oil in the economy as an instrument for income. Our main proxy for the importance of oil is the size of pre-existing oil reserves in an ESR. The identifying assumption is that the interaction between global oil price changes and local oil reserves should have no effect on changes in the demand for health care, except through income. We provide several pieces of evidence that are supportive of the validity of this identifying assumption. Using this instrumental-variable strategy we estimate the elasticity of hospital spending with respect to income. Because our instrument impacts incomes at the ESR level (rather than individual income), our estimates correspond to local general equilibrium effects of income changes.

Our baseline estimate is a statistically significant elasticity of ESR hospital spending with respect to ESR income of 0.71 (standard error = 0.21). This point estimate suggests that rising income would be associated with a modest decline in the health share of GDP. Perhaps more informatively, the upper end of our 95 percent confidence interval allows us to reject the hypothesis that rising real income explains more than 0.3 percentage points of the 11 percentage point increase in the health share of US GDP between 1960 and 2005. Point estimates from a wide range of alternative specifications fall on both sides of this baseline estimate, but are almost always less than 1. On the basis of this evidence, our assessment is that the secular rise in US incomes is unlikely to have been a major cause of the increase in the health share of GDP in the US.

We note at the outset (and explore in greater depth in the paper) at least two potentially important caveats to our conclusions regarding the role of income in contributing to the rising health share of GDP. First, as already explained above, our strategy estimates local general equilibrium effects. If the growth of the health care market resulting from the rise in national or global incomes induced more innovation, our estimates would not incorporate the implications of these induced innovations on health expenditures. Nevertheless, we believe that significantly larger elasticities resulting from these global general equilibrium effects are unlikely for two reasons. First, the same induced innovation effects working at the national or global level should manifest themselves as increased technology adoption or entry of new hospitals at the local (ESR) level. However, we find no statistically or substantively significant effects of local income on hospital entry or on various measures of technology adoption at the ESR level. In this light, a significant *global* induced innovation effect seems unlikely. Second, technological change should be more rapid for sectors that are expanding faster than others (e.g., Acemoglu, 2002, Acemoglu and Linn, 2004). Since health care appears as a necessity good (with an income elasticity less than one), induced innovations should relatively favor its complement. If innovation in the health care sector is subject to "threshold effects" that are absent in other sectors, health care innovations may be more responsive to market size than innovation in other sectors over a certain range. However, there appears to be no strong theoretical or empirical basis for such differential threshold effects in health care. Consequently, we believe that induced innovation at the global level is unlikely to increase the income elasticity of health care expenditures significantly above our estimates.[2]

Second, because of data availability, our empirical work focuses primarily on hospital expenditures from the American Hospital Association data (rather than on total health expenditures). Hospital spending is the single largest component of total health care spending, and the time-series evidence in Figure 1 suggests that hospital and nonhospital components of health care have grown proportionally over the last half century. If income elasticities were substantially higher for the non-hospital components of health expenditures, and if the rise in income over this time period were the major driver of the increase in health expenditures, we should (all else equal) instead see a decline in the hospital share of total health expenditures. This suggests that income elasticities of hospital and non-hospital components of health expenditures should be similar. In addition, using complementary data sources we provide evidence suggesting that the income elasticities of hospital spending and overall health expenditures are similar. While there are reasons to trust these additional data sources less than our benchmark data on hospital spending, this evidence bolsters our belief that our elasticity estimates for hospital spending are likely to be representative of those for total health expenditures.

[2] Note that this argument does not imply that there are no induced innovation effects in health care. In fact, the evidence in Acemoglu and Linn (2004) shows that the introduction of new drugs for different age groups is strongly responsive to changes in the *relative* expected market sizes. However, these results are silent on whether total pharmaceutical—or medical—innovation responds to rising incomes; in fact, they suggest that if rising incomes increase the relative market sizes of other sectors more than that of health care, induced innovations should be relatively directed towards these other sectors.

We are only aware of two studies that attempt to estimate the "causal effect" of income on health spending. Moran and Simon (2006) use the Social Security notch cohort to examine the effect of plausibly exogenous variation in an elderly individual's income on prescription drug use; they estimate an elasticity of drug use with respect to income of above one. The Rand Health Insurance Experiment provides experimental estimates of the impact of income on health expenditures from the so-called Super Participation Incentive in which a sub-sample of individuals were given an unanticipated, small (a maximum of $250 in the mid 1970s) additional lump sum payment in the penultimate year of the experiment; the experiment found no significant impact of this additional income on utilization or expenditures, possibly because the experiment provided only a temporary (one year) income increase (Newhouse et al, 1993, p. 78).[3] Both of these studies focus on the partial equilibrium effect of an increase in own income on own health spending. As a consequence, they are silent on potential general equilibrium effects, which are central to an evaluation of the role of rising aggregate incomes in the growth of the health care sector.[4]

The rest of the paper proceeds as follows. Section 2 describes our empirical strategy and data. Section 3 shows the first-stage relationship between ESR income and our instrument, and presents our instrumental variable estimates of the income elasticity of hospital expenditures and its components. This section also discusses the implications of our elasticity estimate for the role of income in explaining the rise in the health share of GDP. Section 4 discusses some of the most salient threats to extrapolating from our estimates to the role of rising income in explaining the rising health share of GDP. Section 5 explores the robustness of our instrumental-variables estimates along a number of dimensions and examines the validity of our identifying assumption. Section 6 concludes.

2 Empirical Strategy and Data

2.1 Empirical Strategy

Our empirical strategy is to instrument for income in different geographic areas (approximating local economies) with time-series variation in oil prices interacted with cross-sectional variation

[3] Note that this sub-experimnent was not designed to estimate the income elasticity of demand for health care but rather to test the whether the income side payments made to families as part of the experimental design (whose focus was to estimate the effect of cost sharing) impacted utilization.

[4] Our empirical strategy is related to that used by Michaels (2007), to estimate the long-run consequences of resource-based specialization, and to those in Buckley (2003) and Black, McKinnish and Sanders (2005). Michaels also exploits variation in oil abundance across county groups within the U.S. South and studies the consequences of the availability of greater oil resources on changes in the sectoral composition of employment and in education. Buckley (2003) exploited the same source of variation within Texas to investigate the effect of income on marriage and divorce. Black, McKinnish and Sanders (2005) use a similar strategy focusing the coal boom and bust. None of these papers study the effect of income on health care expenditures and technology.

in the oil intensity of the different local economies. We then examine the relationship between the resulting changes in income and changes in hospital spending using panel data on area-level hospital spending. The structural relationship of interest is modeled as:

$$\log h_{jt} = \alpha_j + \gamma_t + \beta \log y_{jt} + \mathbf{X}_{jt}^T \boldsymbol{\phi} + \varepsilon_{jt}, \tag{1}$$

where h_{jt} is hospital expenditures in area j and year t, y_{jt} denotes income in area j in year t, and $\mathbf{X}_{jt}$ denotes a vector of other covariates that are included in some of our specifications (and $\mathbf{X}_{jt}^T$ denotes its transpose). In our baseline specification, there are no $\mathbf{X}_{jt}$s. The α_js are area fixed effects measuring any time-invariant differences across the different geographic areas. The γ_ts are year fixed effects which capture any common (proportional) changes in health care spending each year. For simplicity, equation (1) assumes a linear form and constant proportional effects of income on health expenditure.[5]

The simplest strategy would be to estimate β in equation (1) using ordinary least squares (OLS). However, OLS estimates of β are likely to be biased. Moreover, the sign of the bias is a priori ambiguous. For example, if income is positively correlated with health and healthier areas have lower health care expenditures, the OLS estimates are likely to be biased downwards. If, on the other hand, income is positively correlated with insurance coverage and insurance coverage encourages increased health care spending, OLS estimates are likely to be biased upwards.

Our empirical strategy attempts to isolate potentially exogenous sources of variation in local area income, y_{jt}. In particular, we instrument for changes in area income by exploiting the differential impact of (global) changes in oil prices across areas of the country in which oil plays a more or less significant role in the local economy. In particular, we instrument for $\log y_{jt}$ in equation (1) with the following first-stage regression:

$$\log y_{jt} = \alpha'_j + \gamma'_t + \delta(\log p_{t-1} \times I_j) + \mathbf{X}_{jt}^T \boldsymbol{\phi}' + u_{jt}, \tag{2}$$

where p_{t-1} is the global spot oil price in the previous year, and I_j is a (time-invariant) measure of the role of oil in the local economy. The α'_js and γ'_ts are defined similarly to the α_js and γ_ts in equation (1). In our baseline specifications, I_j will be proxied by the total amount of oil reserves in area j. Throughout, we use oil prices dated $t-1$ in the regression for income as time t to allow for a lag in the translation of oil price changes into income changes. We show in Section 5 that the estimates and implied elasticities are similar (in fact smaller) when we instead use oil prices at time t.

[5] It also seems preferable to specify the dependent variable (log hospital expenditures) in logs rather than in levels since the distribution of hospital expenditures across areas is very right skewed (see Figure 4b discussed below), and it does not seem appropriate for year fixed effects to require a constant (level) change in spending over time across all areas.

Our identifying assumption is that, absent oil price changes, health expenditures in areas with different oil reserves would have grown at similar rates. This is reasonable since both global oil prices and the location of oil reserves are not affected by, and should not be correlated with, changes in an area's demand for health care. Naturally, areas with different amounts of oil reserves do differ in ways that could affect hospital expenditures. For example, we find that areas in our baseline sample with more than one million barrels of oil in large oil wells had roughly two times the population, labor force, labor income, hospital expenditures, and hospital beds in 1970 than areas in our baseline sample without any large oil wells. Nevertheless, any such differences that are time-invariant should be captured by the area fixed effects (the α_js) in equations (1) and (2).

Only differential trends in hospital spending or health expenditures across these areas would be a threat to the validity of our instrumental-variables strategy. As a basic step to increase comparability across areas and to limit potential differential trends, our baseline analysis focuses on the Southern United States—which contains about 50% of the oil in the United States (Oil and Gas Journal Data Book, 2000). More importantly, in Section 5, we provide a variety of evidence to support are identifying assumption that there were no major differential trends in health expenditures across local economies correlated with oil reserves.

Our baseline specification focuses on the period 1970-1990, which encompasses the major oil boom and bust, and uses Economic Sub Regions (ESRs) as our geographic units (local economies). We construct ESRs by splitting the Economic Sub Regions produced by the Census ("Census ESRs") so that our ESRs do not straddle state boundaries. Census ESRs are commonly used geographic aggregations that were last revised for the 1970 Census; they consist of groupings of State Economic Areas (SEAs).[6] There are 247 ESRs in the United States overall, and 99 in our sample of 16 Southern states.[7] We discuss below the results of analyses at both higher levels of aggregation (in particular, state) and lower levels of aggregation (in particular, SEA) and also explore the implications of expanding the analysis to include longer time periods and other parts of the United States.

2.2 Data and Descriptive Statistics

Estimation of equations (1) and (2) requires time-series data on oil prices, cross-sectional data on the oil intensity of the local economy, panel data on the income in each area, and panel

[6] ESRs frequently cross state boundaries. In contrast, SEAs do not cross state boundaries and are defined on the basis of a combination of topographic and natural resource considerations.

[7] Our baseline sample is 2065 observations instead of $99\times21=2079$ observations because of four ESR-years of missing AHA data and because Washington D.C. does not appear in AHA until 1980. Restricting the sample to include only ESRs that appear in all years does not affect results.

data on health expenditures in each area. We briefly describe the construction of our main data series here. Table 1 provides summary statistics on our main variables.

Oil prices For oil prices we use average annual spot oil prices from the West Texas Intermediate series.[8] Figure 2 shows the time series of average annual spot oil prices from 1950 to 2005. Oil prices rose dramatically over the 1970s from $3.35 per barrel in 1970 to a high of $37.38 per barrel in 1980. This oil boom was followed by an oil bust; oil prices declined starting in 1980 to a trough of $15.04 per barrel in 1986. We focus primarily on the period 1970-1990, as these two decades encompass the major oil boom and bust. We discuss below the effects of extending the analysis to include the later oil boom that began at the end of the 1990s as well as the results of falsification exercises during the pre-boom 1950s and 1960s.

Oil price shocks appear to be permanent. Using the time-series data shown in Figure 2, a regression of the log oil price at time t on its one year lag produces a coefficient of 1.009 (standard error = 0.043). Augmented Dickey-Fuller unit-root tests are reported in Appendix Table A1, which all fail to reject the null hypothesis that log oil prices follow a unit root.[9] This evidence suggests that our empirical strategy will be informative about the effects of permanent (vs. transitory) changes in income on health care expenditures.

Oil intensity Our primary measure of the oil intensity of area j, is an estimate of the total oil reserves in that area (since discovery). We draw on data from the 2000 Edition of the Oil and Gas Journal Data Book, which includes information on all 306 oil wells in the United States of more than 100 million barrels in total size. Total oil reserves are calculated as estimated remaining reserves plus total cumulative oil production as of 1998; they are thus not affected by the prior intensity of oil extraction in the area. Throughout, we refer to these as "large" oil wells. Our baseline analysis is limited to the Southern United States, which contains 161 of the 306 large oil wells in the United States and 51% of the total oil reserves in these oil wells.[10]

Figure 3 shows the cross-sectional variation in oil reserves across different areas of the South. It indicates that the importance of oil to the local economy varies substantially across different areas of the South. For example, approximately 70 percent (69 out of 99) of the ESRs in the Southern United States have no large oil wells. Conditional on having a large oil well, the standard deviation in oil reserves across ESRs in the Southern U.S. is more than 2500

[8] These are available at: http://research.stlouisfed.org/fred2/series/OILPRICE/downloaddata?cid=98

[9] Kline (2008) conducts a more detailed analysis of the time-series behavior of oil prices and concludes that oil prices are "well approximated by a pure random walk".

[10] According to the 2000 Edition of the Oil and Gas Data Book, there is only one large well in the South that is listed as having been discovered after 1970 (Giddings, TX in 1971). Excluding this well has no effect on our results. There are also 60 (out of the 306) oil wells that are located off-shore and thus were not assigned to any county. These off-shore wells account for 12% of the oil reserves in the data.

million barrels (relative to a mean reserve conditional on having any reserves of 1700 million barrels). As a result, as we shall see, different areas experienced differential changes in income in response to changing oil prices. This provides our first-stage variation in income.

In some of our analyses we also draw on data from the 1970 Census on the mining share of employment in 1970 to help measure oil intensity of an area. The mining share includes all workers in oil mining, natural gas and coal mining; it is unfortunately not available separately for oil mining.[11]

Area income Our primary data on ESR income comes from aggregating up county-level annual payroll (for all establishments) from the County Business Patterns (CBP).[12] We also obtain ESR-level employment data from the CBP. The CBP data are attractive for our purposes because of their level of disaggregation, enabling us to construct ESR-level measures of income. Figure 4a provides a histogram of the logarithm (log) of income from the CBP across ESRs. The distribution of log income appears to be well approximated by a normal distribution.

A potential drawback of these data is that they do not include capital income. To investigate whether the exclusion of capital income has a systematic effect on our results, we also repeat our analysis at the state level using annual data on gross state product (GSP), which includes both labor and capital income. We also use industry-specific GSP estimates as a dependent side variable to provide comparative estimates of income elasticities in different industries.[13]

Area health spending Our primary data on area health spending are obtained by aggregating up hospital level data from the American Hospital Association's (AHA) annual census of all U.S. hospitals. We use these data to construct our main dependent variable: total hospital expenditures in area j and year t. Figure 4b shows a histogram of the logarithm of hospital spending from the AHA, which also has the standard shape of a normally-distributed variable.

The AHA data also contain other measures of hospital activity, which we use below to investigate which components of health expenditure respond to the rise in income, and to investigate the impact of rising income on hospital technology adotion. Specifically, the AHA data contain total hospital expenditures, payroll expenditures, full time equivalent employ-

[11] Mining share of employment is defined based on the 1970 Census of Population (Volume 1: Characteristics of the Population, Table 123, Parts 2-9 & 11-52).

[12] The CBP is an annual establishment survey of all establishments in the Business Register at the Census Bureau. The CBP data are available on-line at the Geospatial & Statistical Data Center at the University of Virginia for the years 1977 through 1997 (http://fisher.lib.virginia.edu/collections/stats/cbp/county.html) and at the U.S. Census Bureau for the years 1998 through 2006 (http://censtats.census.gov/cbpnaic/cbpnaic.shtml). Earlier years were hand-entered from bound volumes available at the MIT Library Storage Annex. For more information on these data see http://www.census.gov/epcd/cbp/view/cbpmethodology.htm).

[13] GSP data are from the Bureau of Economic Analysis (http://www.bea.gov/regional/gsp/).

ment, admissions, inpatient days, beds, and a series of binary indicator variables for whether the hospital has a variety of different technologies. For about three quarters of the years, we also have information full-time equivalent employment of two types of nurses in the data: Registered Nurses (RNs) and Licensed Practitioner Nurses (LPNs); together these constitute about 20% of total hospital employment. RNs are considerably more skilled than LPNs and we use the ratio of RNs to RNs and LPNs combined as a proxy for the skill mix.[14]

There are two key advantages of the AHA data. First, they allow us to conduct our analysis at a level of aggregation below the state; this is attractive given the substantial within state variation in oil intensity seen in Figure 3a. Second, they allow us to measure other components of health care activity besides spending. In particular, we can measure hospital technology adoption decisions which allows us to shed light on whether there are likely to be potential global general equilibrium effects from rising income that our analysis would not capture.

The major drawback of the AHA data is that they do not contain information on non-hospital components of health expenditures. To investigate whether the focus on hospital spending is likely to produce misleading estimates of the income elasticity of total health expenditures, in some specifications we analyze data from the Health Care Financing Administration (HCFA), which produces state-level estimates of total personal health care expenditures and its components for a subset of our study years (Levit, 1982, 1985).[15]

Population Finally, to investigate potential confounding effects of migration in response to income variation, we construct annual data on area total population, as well as area population of different age groups (since hospital use varies dramatically by age). We construct these data using the county level population data from the Current Population Reports (CPR).[16] Crucially, for our purposes, population in intercensal years is not interpolated but rather imputed based on a variety of administrative data sources including data on births, deaths, school enrollment, and tax returns (US Census Bureau, various states and years, and Siegal 2002). To gauge the relative intensity of hospital use among individuals of different age groups, we use data from National Health Interview Survey, the NHIS (pooled between 1973 and 1991).

[14] RN certification requires about twice as many years of training as LPN certification, and are paid substantially higher hourly wages (see Acemoglu and Finkelstein, 2008).

[15] Data from the 1970s were obtained from Levit (1982, 1985). Data for 1980 - 1990 were obtained from the Centers of Medicare & Medicaid Services on-line at http://www.cms.hhs.gov/NationalHealthExpendData/05_NationalHealthAccountsStateHealthAccountsResidence.asp#TopOfPage The data include total health expenditures and expenditures on the following components (which sum to the total) Hospital Care, Physicians' Services, Dentists' Services, Drugs and Other Medical Nondurables, Eyeglasses and appliances, Nursing Home Care, and Other Health Services (which include Home Health Care, Other Professional Services, and Other Personal Services).

[16] The Current Population Reports data are available on-line at the U.S. Census Bureau (http://www.census.gov/popest/archives/pre-1980/ and http://www.census.gov/popest/archives/1980s/).

3 Main Results

3.1 First Stage

Table 2 shows the relationship between ESR income and our instrument. The first column shows the results from estimating equation (2). In this and all subsequent estimates, we allow for an arbitrary variance covariance matrix within each state.[17] The results in column 1 indicate a positive and strong first stage: ESRs with greater oil reserves experience greater changes in income in response to oil price changes than areas with less oil. The F-statistic is 18.74. We defer a discussion of the magnitude of the first stage until a little later in this section.

To examine the source of the increase in income, column 2 re-estimates the first-stage equation (2) using log area employment on the left hand side instead of log area income. The results indicate that areas with more oil also experience greater change in employment when oil prices change. The coefficient δ_1 on the instrument is of approximately the same magnitude in columns 1 and 2, suggesting that all (or most) of the changes in income associated with oil price movements across areas with different levels of oil reserves comes from changes in employment at constant wages. This is consistent with our prior expectations that oil workers should be close substitutes to other workers and have a relatively elastic labor supply in the local labor market. It is also consistent with the stylized fact that labor income changes at short-run frequencies (e.g., over the business cycle) are largely driven by employment changes, with little movements in wage per worker.[18] In contrast to our source of income variation, about half of the growth in income between 1960 to 2005 is due to increased employment, while the other half is due to increased wages per employee (U.S. Census Bureau 2008). In Section 4, we discuss the possible implications of extrapolating from these income changes to the effects of the secular increase in incomes in the U.S. economy.

The impact of our instrument on employment and existing evidence on migration responses to local economic conditions (e.g., Blanchard and Katz, 1992) suggest that are instrument may also affect area population. Any increase in population in high oil areas relative to low oil areas may increase health expenditures directly, potentially over-stating the effect of increased income on hospital spending among a (constant) population. Column 3 explores this issue by re-estimating equation (2) with log population as the new dependent variable. The results indicate that our instrument also predicts population, so that part of the increase in area income we estimate likely reflects increases in area population; a comparison of columns 2 and

[17] Because of concerns of the small sample properties of clustering with only 16 states, we experimented with alternative small sample corrections. We discuss this in detail in the robustness analysis in Section 5 below.

[18] See, for example, Abraham and Haltiwanger (1995). This does not imply that the wage per efficiency unit of labor is constant, since there may be composition effects (see, Solon, Barsky and Parker, 1994).

3 suggests that about one third of the effect of the instrument on employment can be accounted for by its effects on population.

A natural solution is to convert both income (our endogenous right-hand side variable) and hospital expenditures (our dependent variable of interest) into per capita terms, so that the structural equation focuses on the impact of income per capita on hospital spending per capita (the same instrument now used for income per capita in the first stage). The first-stage results with income per capita on the left-hand side are shown in column 4. Consistent with a comparison of columns 1 and 3, the per-capita specification shows a statistically significant but smaller first-stage effect than unadjusted specification in column 1. In particular, the first-stage coefficient is smaller than that in column 1 by 5 log points or by about 40 percent.

While the per capita specification is natural, it may in turn under-state the effect of increased income on hospital spending because the population changes associated with our instrument are from disproportionately low users of hospital care. This can be seen in columns 5 and 6 which indicate that the population response to our instrument is concentrated among the non-elderly (those under 55). In fact, it appears that the population response is concentrated among those younger than 45 (not shown in Table 3 to save space). Younger individuals consume disproportionately lower hospital care than the elderly. To illustrate this, Figure 5 shows the average annual number of hospital days for individuals in five year age buckets from the NHIS (pooled between 1973 and 1991). The under 55 average 0.6 hospital days per year, while individuals aged 55 and older average 2.3 hospital days per year. As a result, even though the 55 and older are only 23% of the population, they consume 38% of hospital days.

To obtain more accurate estimates of the impact of rising incomes on health expenditures (and to try to err away from the direction of potentially under-estimating the effects of income), in our baseline analysis we correct for the changes in the composition of the population rather than simply using per capita estimates. In particular, we construct a measure of "hospital utilization weighted population" in area j in year t, denoted by $HUWP_{jt}$. This measure is computed as the inner product of the vector of populations in each five year age bin in area j and year t (pop_{ajt}) with our estimate of the national time period average from the NHIS of hospital days used by that age bin ($hospdays_a$). Namely:

$$HUWP_{jt} = \sum_a pop_{ajt} \times hospdays_a \tag{3}$$

Our preferred specification is therefore to adjust (i.e., divide) income in both the structural equation (1) and the first-stage equation (2) and hospital expenditures in the structural equation (1) by $HUWP_{jt}$ as constructed in equation (3). This leads to our baseline version of our structural equation (1):

$$\log \tilde{h}_{jt} = \alpha_j + \gamma_t + \beta \log \tilde{y}_{jtjt} + \mathbf{X}_{jt}^T \boldsymbol{\phi} + \varepsilon_{jt}, \tag{4}$$

our baseline version of the first-stage equation (2):

$$\log \tilde{y}_{jt} = \alpha'_j + \gamma'_t + \delta'(\log p_{t-1} \times I_j) + \mathbf{X}_{jt}^T \boldsymbol{\phi}' + u_{jt}, \tag{5}$$

where adjusted income ($\tilde{y}_{jt}$) and adjusted hospital expenditure ($\tilde{h}_{jt}$) are defined

$$\tilde{y}_{jt} \equiv \frac{y_{jt}}{HUWP_{jt}} \text{ and } \tilde{h}_{jt} \equiv \frac{h_{jt}}{HUWP_{jt}}.$$

Intuitively, both income and hospital expenditures (or other outcomes) are adjusted for hospital-use weighted population (HUWP) to capture any direct effect of our instrument on hospital-use weighted population.

The first-stage result from the estimation of equation (5) are shown in column 7. Its magnitude lies (mechanically) in between the first-stage estimates without any migration adjustment (column 1) and the per capita adjustment in column 4. In practice, the magnitude is about one third of the way from the per capita adjustment to the unadjusted specification. The two stage least squares estimate of the effect of income on hospital spending using the hospital use weighted population adjustment should therefore similarly lie in between the unadjusted estimates and the per capita adjusted estimates (and we find below that it does).

When we use the hospital use weighted adjustment as opposed to no adjustment, the additional assumption required to interpret the coefficient β in our IV estimation of equation (4) as the income elasticity of hospital expenditures is that the hospital spending-age profile of the marginal individuals who move across areas in response to oil price changes is similar to the national average hospital use-age profile. Based on this reasoning, we take the estimates from equations (5) and (4) as our baseline/preferred specification. Nevertheless, because even conditional on age migrants may be healthier than the general population, the estimates from (5) and (4) might understate the effects of income on health expenditures. We therefore also report results without any adjustment for migration as well as results using the per capita adjustment. One might consider the unadjusted estimates as an upper bound on the income elasticity, while the per capita adjusted estimates are a lower bound (provided that the marginal migrant into a high-oil area in response to an oil price increase is "healthier" than the average population in the area, which seems like a reasonable assumption).[19]

To gauge the magnitude of the first stage, we calculated the impact on area income of the change in oil prices experienced between 1970 and 1980 across areas with different amounts of

[19] This last presumption is both intuitive and also consistent with the fact that migrants are considerably younger than the average person (see Table 2).

oil reserves. In our preferred specification (column 7) the oil price change from 1970 to 1980 is associated with a 3.6 percent larger increase in area income in areas with a one standard deviation larger amount of oil.

3.2 Income elasticity of hospital spending and components

Table 3 presents our central estimates of the impact of income on hospital expenditures. Column 1 reports the OLS estimate of equation (4) in which both hospital expenditures and income are adjusted for HUWP as shown. The estimate of β, the coefficient on adjusted per capita income $\log \tilde{y}_{jt}$, is 0.065 (standard error = 0.116). This indicates that when income in the area rises by 10 percent, hospital expenditures rise by about 0.6 percent. This relationship is statistically indistinguishable from zero. As previously discussed, the OLS correlation between income and hospital spending may be biased in either direction relative to the causal effect of income on hospital spending. The subsequent analyses suggest that in our setting the OLS estimate is downward biased.

Column 2 shows the results from the reduced form corresponding to (4) and (5) (without covariates):

$$\log \tilde{h}_{jt} = \alpha''_j + \gamma''_t + \delta''(\log p_{t-1} \times I_j) + \varepsilon''_{jt}. \tag{6}$$

This reduced-form estimation shows a (positive) and statistically significant relationship between our instrument and log hospital expenditures.

Column 3 presents our baseline IV estimate of equation (4), using our baseline instrument and first-stage specification. The estimated elasticity of health expenditure with respect to income is 0.707, with a standard error of 0.211.[20] Columns 4 and 5 show results without any population adjustment and with a per capita population adjustment, respectively. As discussed in Section 3.1, these can be interpreted as upper and lower bounds on the income elasticity of hospital spending. In both alternative specifications the income elasticity remains statistically significant and ranges between 0.666 and 0.801, suggesting that these bounds are reasonably tight.

Columns 4 through 7 of Table 4 investigate which components of hospital expenditures are affected by income changes. Table 4 reports the results from IV estimation of equation (4) using different hospital outcomes as the dependent variable.[21] Several interesting findings

[20] Since we have only one instrument and one endogenous right-hand side variable, the point estimate in the IV specification can also be obtained by dividing the reduced-form estimate in column 2 by the first-stage estimate from column 7 of Table 2.

[21] As detailed in the notes to Table 4, we adjust both the dependent variable and income for hospital-utilization weighted population (HUWP) to account for population migration in response to our instrument. The exceptions are in columns 4 and in columns 8-11 in which income is still adjusted for (i.e., divided by) HUWP, so that we are measuring the increase in income per adjusted population, but the dependent variable, is not adjusted

emerge.

First, the results in columns 1 and 2 suggest that the impact of income on hospital payroll expenditures (which are about one half of total hospital expenditures) can explain all (or more) of the effect of income on total hospital expenditures. There is no evidence in column 3 of an economically or statistically significant effect of income on hospital employment. This suggests that the increase in payroll expenditures comes from some combination of an improvement in the quality of employees and/or a bidding up of the wages of (quality-adjusted) employees.

Second, we find evidence of economically and statistically significant skill upgrading associated with increased income. Column 4 shows an increase in the skill composition of employment, which we proxy by the ratio of skilled nurses (RNs) to all RNs and LPNs.[22] This does not rule out wage (price) effects, but does suggest that at least some of the increase in payroll expenditures in column 2 comes from quality improvements. More importantly, it suggests that our empirical strategy is able to uncover (at least some) general equilibrium effects; skill upgrading of hospitals is likely to be a response to the ESR-level increase in the demand for hospital services.

Third, we find no evidence that rising income is associated with an increase in hospital utilization (as measured by either admissions or patient days) or in hospital capacity (as measured by beds). These results are shown in columns 5 through 7.[23] The remaining columns of Table 4 show the impact of rising income on hospital entry and technology adoption; we defer a discussion of these results until Section 4.

3.3 Implications for the role of income in explaining rise in health share

The point estimates of the income elasticity of hospital spending reported in Table 3 (as well as most of those we will report subsequently from additional analyses) are less than one, suggesting that health care does not appear to be a luxury good. To provide some context

for HUWP. In column 4 the dependent variable is a ratio (of skilled nurses to total nurses) which would not increase mechanically with population; in columns 8-11, the dependent variables (number of hospitals, number of technologies, or indicator for specific technologies) are count variables or indicators, which would not be expected to scale linearly with population in the same way as e.g., spending or admissions are likely to. For these reasons, we do not adjust these dependent variables for population. As discussed above, not adjusting for migration could be interpreted as providing upper bound estimates of responsiveness to income.

[22] We only have information on RN and LPN employment for the following years: 1970, 1972, 1974, 1976, 1978, 1980-2005. Our baseline elasticity estimate for hospital expenditures declines to 0.458 (s.e. 0.167) when the odds years in the 1970s are excluded.

[23] The point estimates are uniformly negative and in the case of admissions and patient days they are statistically significant. We caution against putting too much weight on the suggestive evidence of a decline in utilization, since the statistical significance of these estimates is not as robust across alternative specifications as that of the other results reported in Table 4. Nonetheless, we find a decline in hospital utilization associated with the increase in incomes to be plausible, since rising income should not worsen health, and all else equal, should not encourage patients to have longer hospital spells.

for the magnitude of the income elasticity we estimate, consider the results from our baseline specification (Table 3, column 3), which are roughly in the middle of the range of elasticities we will report in various alternative specifications below. The point estimate of an elasticity of 0.7 suggests that the approximate doubling of real per capita GDP between 1960 and 2005 (from $19,212 to $41,874 in $2005) would actually cause a *decline* in the health share of GDP from 5 percent to about 4 percent. The upper end of the 95 percent confidence interval from our baseline estimate is an income elasticity of 1.1. This allows us to reject a role of rising income in increasing the health share of GDP by more than 0.3 percentage points between 1960 and 2005, or, in other words, of explaining more than 3 percent of the 11 percentage point increase in health share over this time period. At the same time, the point estimate suggests that rising real per capita income may be able to explain about 15 percent of the rise in real per capita health spending, while the upper end of the 95 percent confidence interval allows us to reject a role for rising real per capita income in explaining more than one quarter of the rise in real per capita health spending.[24] Therefore, our results suggest that while rising income may be an important component of growing health expenditures, it is unlikely to have contributed much to the increase in the *share* of GDP spent on health care.

It strikes us as a prima facie plausible result that health care may be a necessity rather than a luxury good. For example, suppose that we were to model the relationship between income and expenditures with the widely-used Stone-Geary preferences. Then, the utility of an individual could be written as

$$\sum_{g=1}^{G} \chi_g \log \left(c_g - \bar{c}_g \right),$$

where $g = 1, ..., G$ denotes the various goods, χ_g's are positive constants, $\bar{c}_g$ is the minimum necessary level of consumption of good g, and $c_g > \bar{c}_g$ is the actual level of consumption. With these preferences, goods with high levels of $\bar{c}_g$ will be necessities, and those with low or negative levels of $\bar{c}_g$ will be luxuries. One would naturally expect health care related goods to have high $\bar{c}_g$'s, since some minimal level of health care is necessary even at low income levels. Naturally, one could consider more general preferences than Stone-Geary, so that goods that are necessary to consume at low levels of income might later become luxuries. Nevertheless, this widely-used functional form suggests that thinking of health care as a potential necessity good is quite plausible; we also discuss in more detail below some suggestive evidence that the income elasticity of health care is not rising with income.

[24] On the basis of the existing correlation studies (described in the Introduction), past studies that have attempted to decompose the causes of the rising in health spending have concluded that the rise in income may account anywhere from 5 percent (Cutler, 1995) to a quarter (Newhouse, 1992) of the spending growth.

4 Discussion

We now discuss several potential concerns and caveats with the out of sample extrapolation of our estimates to the role of income in explaining the rising health share of GDP.

4.1 Endogenous Technology Responses

A major threat to the computations in subsection 3.3 and our conclusion that rising incomes are not a significant contributor to the rising health share of GDP is the possibility of global (or national) technology responses to income changes. While our estimates incorporate the impact of income on technology adoption and entry of new hospitals at the ESR level, they may under-state the effects of rising incomes if these induced the development of major new global technologies, which then led to a sizable expansion in health expenditures. This concern is particularly important since technological change in health care is commonly believed to be one of the key drivers of rising health care expenditures (e.g., Newhouse, 1992, Fuchs, 1996, Congressional Budget Office, 2008).

In this subsection, we argue that induced technology responses are unlikely to have contributed to a sizable increase in health care spending. Our argument has two parts. First, if present and economically significant, an induced innovation response to rising income should also manifest itself at the ESR level in the form of entry of new hospitals (which presumably embody new technologies) and/or adoption of new technologies at existing hospitals. In particular, even though innovations take place at the national or global level, the same mechanism that would cause induced innovation at the national or global level should also lead to faster adoption of these technologies in areas with greater increases in demand (e.g., Acemoglu, 2002, 2007). Second, existing theory suggests that induced innovations should be directed to sectors that are otherwise expanding rapidly (see in particular Appendix A below), while our estimates suggest that, all else equal, health expenditure increases less than proportionately with income.

Let us start with the empirical evidence. Columns 8 through 11 of Table 4 show no evidence that rising income is associated with an increase in hospital entry or technology adoption. Column 8 of this table shows a negative and statistically insignificant impact of income on the number of hospitals (so that the number of hospitals appears to have grown relatively more in areas experiencing slower income growth).

The rest of Table 4 turns to technology adoption. The AHA data contain binary indicators for whether the hospital has various "facilities", such as a blood bank, open heart surgery facilities, CT scanner, occupational therapy services, dental services, and genetic counseling services. These data have been widely used to study technology adoption decisions in hospitals (see, for example, Cutler and Sheiner, 1998, Baker and Phibbs, 2002, Finkelstein, 2007, Acemoglu and Finkelstein, 2008). Since they contain only indicator variables for the presence or absence of various facilities, we cannot investigate the potential upgrading of existing technology or the intensity of technology use, but we can study the response of the total number of facilities to changes in income, which proxy for technology adoption decisions on the extensive margin.

During the time period we study, the AHA collects information on the presence of 172 different technologies or "facilities". These are listed, together with their sample means (the fraction of ESRs each technology is in) and the years in which they are available, in Appendix Table A2. On average, a given facility is reported in the data for 7 out of the possible 21 years; only nine of the technologies are in the data for all years. Moreover, as is readily apparent from Appendix Table A2, the list encompasses a range of very different types of facilities. Given these two features of the data, we pursue two complementary approaches to analyzing technology adoption with the AHA data (see Acemoglu and Finkelstein, 2008 for a similar strategy).

Our first approach to investigating the impact of income on technology adoption, which is shown in column 9, treats all facilities equally and measures technology as the number of unique technologies in a given ESR. The year fixed effects in our IV estimate of equation (4) adjust for the fact that the set of technologies reported each year differs. The results show no substantively or statistically significant evidence of an increase in the number of unique technologies in the area in response to the increase in income. In fact, the point estimate on income is negative and statistically insignificant. It is also substantively small, suggesting that a 10 percent increase in area income is associated with a statistically insignificant decrease in the number of technologies in the area of 0.013, or about 0.03 percent relative to the average number of technologies in the area.[25]

A drawback to this approach is that it treats all technologies as perfect substitutes. As an alternative approach, we estimated hazard models of the time to adoption for specific technologies that are in the data for at least 15 years of our 21 year sample period. As in Acemoglu and Finkelstein (2008), we limit our analysis to technologies that were identified as "high tech" by previous researchers (Cutler and Sheiner, 1998, Baker, 2001, and Baker and Phibbs, 2002). Unfortunately, there are only two technologies that meet these criteria: open

[25] To provide some context for comparison, using the same technology measure (but at the hospital level rather than the ESR level) Acemoglu and Finkelstein (2008) show that, in its first three years, the introduction of Medicare PPS was associated with, on average, the adoption of one new technology at the hospital level (about a 4 percent increase in the average number of unique technologies that the hospital has).

heart surgery and diagnostic radioisotope facility. Both of these technologies were diffusing over our sample period, though open heart surgery started from a lower prevalence and diffused more rapidly.[26] To investigate the impact of ESR income on local technology adoption decisions, we estimated a semi-parametric Cox hazard models for these two technologies as functions of our baseline instrument.[27] In particular, the conditional probability that ESR j adopts the technology in question at time t (meaning that at least one hospital in the ESR adopts the technology conditional on there being no hospital in the area that had previously adopted this technology) is modeled as

$$\lambda_t = \lambda_0 \exp(\delta' \log p_{t-1} \times I_j + \mathbf{X}_{jt}^T \boldsymbol{\phi}'), \tag{7}$$

where λ_0 is a fully flexible, non-parametric baseline hazard, $\log p_{t-1} \times I_j$ is our baseline instrument, the interaction between oil prices and oil reserves in the ESR, and the vector $\mathbf{X}_{jt}$ includes region fixed effects for the three census regions within the South, total hospital expenidtures in 1970, and total hospital beds in 1970 (we do not include year fixed effects in the model since the fully flexible baseline hazard in the Cox model is specified with respect to calendar time). Columns 10 and 11 in Table 4 show the results of this estimation. The results show no evidence of a significant effect of our income on technology adoption. The coefficient for open-heart surgery is negative, small and insignificant, while the coefficient for radioisotope therapy is positive, also small and highly insignificant.[28]

Next, turning to the theoretical argument, Appendix A outlines a simple model of induced innovations and demonstrates that development of new technologies is likely to be directed toward sectors that are expanding more rapidly. The implications of this theory are consistent with existing empirical evidence, which indicate that medical innovation responds to expected market size (e.g., Acemoglu and Linn, 2004, Finkelstein, 2004). In the present context, these theoretical expectations imply that innovations induced by the secular rise in incomes should not be favoring the health care sector. In particular, our point estimates suggest that, ignoring induced technology effects, health care expenditures increase less than proportionately with aggregate income. Thus, as income increases, the market size for health care technologies will increase less than the market size for a range of other technologies. As a consequence, the

[26] Open heart surgery is in our data for all 21 years (1970-1990) and diagnostic radioisotope therapy is in our data for 19 years (1972-1990). Only 30 percent of ESRs had open heart surgery technology in 1970, whereas over three quarters of ESRs did so by 1990. About three quarters of ESRs had diagnostic radioisotope faciltiies in 1972 and 92 percent had it by 1990.

[27] For now, we focus on the reduced-form relationship rather than the IV estimate of the Cox hazard model, since the former is both simpler to interpret and more straightforward to estimate.

[28] By contrast, Acemoglu and Finkelstein (2008) find significant increases in the adoption of both of these technologies in response to a change in Medicare's hospital reimbursement policy for labor inputs; this suggests that the adoption of these technologies are generally responsive to economic incentives.

induced technology channel suggests that there should not be disproportionate technological advances in the health care sector in response to the secular increase in incomes. As the model in Appendix A highlights, the main exception to this conclusion is that even a less than proportionate increase in the size of the market for health care technologies might jump-start medical technological advances if technological change in the health care sector was unprofitable prior to income reaching a certain minimum threshold. This exception seems implausible (at least to us) given that advances in medical technologies have been ongoing for more than a century and mortality has been declining at a roughly constant rate over this same period (Cutler and Meara, 2003).[29]

Limited income-induced technology effects for the health care sector are also consistent with the results reported in Table 4, which show no significant effects on hospital entry or technology adoption driven by ESR-level income changes. The lack of a response in hospital entry and technology adoption bolsters the argument that, because the relative market size for the health care sector does not increase disproportionately following an increase in income, the induced technology effects should also be limited.

Overall, therefore while we cannot rule out major national or global induced technology responses accompanying the secular increase in income in the United States (which could in turn have further effect on health expenditures), our empirical evidence and theoretical expectations suggest that these effects should be relatively small and thus should not change our basic conclusion that the increase in income is unlikely to be the major factor in the run-up in the share of GDP spent on health care.

4.2 Hospital Spending Versus Total Health Expenditure

A major potential limitation of our estimates is that the dependent variable measures hospital expenditures rather than total health expenditures, which may have different income elasticities. Hospital expenditures are the single largest component of health care expenditures, accounting for close to two-fifths of the total; by contrast, spending on physicians accounts for about one fifth of total health expenditures, and spending on drugs accounts for about one-tenth; these shares have been roughly constant since 1960 (CMS, 2006).

Our reading of the available evidence is that total health expenditures are unlikely to have a significantly higher income elasticity than hospital spending. The first piece of suggestive evidence comes from Figure 1, which shows that the hospital share of total health expenditures

[29] The specific nature of medical technological progress may have varied over time. For example, improvements in sanitation and other public health measures were a primary factor in mortality declines early in the 20th century, while penicillin and other antibiotics were a key factor mid-century, and medical interventions that reduce cardiovascular disease mortality were critical in the latter part of the century (Cutler and Meara, 2003).

has been roughly constant over the last half century. If income elasticities were higher for the non-hospital components of health expenditures, and if the rise in income over this time period were the major driver of the increase in health expenditures, we should see (all else equal) a decline in the share of hospital spending in overall health expenditure. The fact that Figure 1 shows no such decline supports our overall conclusion.

Our second piece of evidence comes from estimates of income elasticities of overall health care expenditures and non-hospital components thereof, based on several complementary data sources. In particular, we have state-level data on total health expenditures and its components from the Health Care Financing Administration (HCFA) for 1972, 1976-1978 and 1980-1990 (instead of our baseline sample 1970-1990). The HCFA estimates are based on a combination of administrative and survey data. An important caveat to these data is that they are interpolated (within each component) where data are missing between years (Levit, 1982, 1985). Such interpolation is likely to understate the standard errors and may even also bias the estimated coefficients, so the results from this data set have to be interpreted with caution. Our main interest is to see whether there are any reasons to expect overall health expenditures to be more responsive than hospital spending to potentially exogenous changes in income.

Table 5 presents estimates from the HCFA data. Since we lose some variation by aggregating from the ESR level to the state level, we report results both for our basline sample of the 16 the Southern states (Panel A) and for the entire United States (Panel B). We are reassured that the results are broadly similar.

Column 1 shows that our first stage is robust to state-level analysis for the sub-set of years for which we have HCFA data. Columns 2 and 3 show our estimated income elasticity for total health expenditures and the hospital sub component respectively. Both estimated income elasticities are positive but statistically insignificant. The point estimates suggest a similar, but slightly larger income elasticity for hospital expenditures than for total health expenditures.[30] Columns 4 through 9 present results for the other components of health expenditures, and provide some intuition for why hospital and total health expenditure income elasticities are similar. These estimates suggest that the income elasticity of spending on physician services, on

[30] The hospital expenditure data in the HCFA series are derived from the AHA data for non-federal hospitals, but use unpublished Federal agency data for federal hospital expenditures (Levit 1982). There are also several differences between how we use the AHA data and how they are used in the HCFA estimates. Most importantly, the HCFA estimates are base on interpolating missing data (Levit 1982, 1985). When we estimate the income elasticity of hospital expenditures using our AHA hospital data but at the state level for the full US and for the HCFA available years (i.e. the analog of Table 5 column 3 panel B) we estimate a statistically significant elasticity of 0.514 (standard error = 0.226). If we further restrict to non-federal hospitals this elasticity falls to 0.474 (standard error = 0.255) which is statistically indistinguishable from the HCFA estimate of 0.148 (standard error = 0.162). We suspect differences between estimates using the AHA data and the HCFA data on hospital expenditures reflect primarily the interpolation of missing data.

dental services, on drugs and other medical non durables, and on vision products is greater than the income elasticity of hospital spending, while nursing home care and other health services have large negative income elasticities. The large negative income elasticity for nursing home care is intuitive since wealthier individuals can more easily pay for assistance in the house. Overall, we read the results in Table 5 as suggestive of similar income elasticities for total health expenditures and hospital expenditures.

Results from several other data sources are also consistent with this conclusion. We examined the income elasticity of state-level Health Services Gross State Product (GSP) from 1970-1990. Health services GSP account for roughly 26% of total health expenditures. Our estimates using health services GSP show no evidence of a greater income elasticity than that for hospital spending.[31]

We also examined the elasticity of various components of state-level health care utilization from the National Health Interview Surveys (NHIS). The NHIS data cover 1973-1990 (data before 1973 do not have state identifiers) and are not interpolated, which is a clear advantage relative to the HCFA data. On the other hand, the NHiS only measures utilization on the extensive margin. This implies that NHIS data will not be informative of increases in expenditure on the intensive margin. As in the AHA data, we find no evidence in the NHIS of a positive income elasticity of hospital utilization. We also find no evidence of a positive income elasticity of doctor visits (indeed, the point estimates are negative, though not statistically significant).[32]

Overall, while there are important limitations to each data source, a number of complementary data sets with information on state-level health expenditures indicate that the income elasticity of overall health expenditures is unlikely to be significantly higher than the income elasticity of hospital spending. This is also consistent with the time-series evidence in Figure 1. We therefore believe that our estimates of the income elasticity of hospital spending are likely to be representative of the income elasticity of total health expenditures.

4.3 Labor Income Versus Total Income

Our baseline income measure captures only the effect of our instrument on labor income. If capital income and labor income do not respond proportionately to our instrument, we may be understating (or over-stating) our first stage, and thus over-stating (or under-stating) the income elasticity in the second stage. Unfortunately, annual data on labor and capital income

[31] These results are shown in Table 7, column 6. We discuss this table in subsection 4.4 below. We are currently investigating exactly what components of health-care spending are included in health GSP. The definition given on-line is available here: http://www.census.gov/epcd/naics/NSIC8B.HTM#S80.

[32] These results are not reported to save space but are available upon request.

do not exist for our time period at a level of disaggregation below the state, while, as discussed, our preferred specification is at a sub-state level.

To investigate whether our restriction to labor income biases our estimate of the income elasticity, we aggregated our data (and regressions) to the state level and explored how the first-stage and IV estimates change when we use Gross State Product (GSP) at the state level; GSP includes both labor and capital income. Table 6 shows the results of this exercise. Panel A shows the first-stage estimates and Panel B the corresponding IV estimates. Column 1 shows our baseline results, which use CBP labor (payroll) income at the ESR level as our income measure. Column 2 shows the equivalent regression, again using labor income, estimated at the state level. The first stage remains strong at the state level, and the IV estimate declines slightly (elasticity of 0.564 with a standard error of 0.223) but is on the whole very similar to that in column 1.[33]

Column 3 shows the results at the state level when we use state GSP as our income measure rather than CBP payroll data. The first stage results suggest that non labor income appears to rise by the same proportion or slightly more proportionally in response to our instrument than our primary measure of labor income (compare columns 2 and 3 of Panel A). If anything, the results suggest that the estimates using labor income only may be slightly over-stating the income elasticity of health expenditures (compare columns 2 and 3 of Panel B). Since, as discussed, we lose variation by aggregating to the state level, we also report results at the state level when we include the entire U.S. in the sample rather than just the 16 states in the South. Column 4 shows the results when we use labor income (from the CBP payroll data) as our measure of income and column 5 shows the results when we the GSP measure, which include capital income. Once again the results suggest that non-labor income may rise slightly more than proportionately with labor income, so that our income elasticities in our baseline estimates may be slightly overstated.[34]

[33] A comparison of columns 1 and 2 of Table 6 indicate that our results are generally robust when data are aggregated up from the ESR to the state. However, we also found that the first-stage relationship is not robust to analyzing the data at a lower level of aggregation than the ESR. For example, we explored analyses conducted at the level of the State Economic Area (SEA); there are 194 SEAs in our sample of Southern States compared to 99 ESRs. The major concern with the SEAs is that some of them are closely linked to each other economically and residentially, thus would not be experiencing independent income variation. In this case, we would expect a significant amount of attenuation in the first stage. Consistent with this expectation, the first stage becomes weaker, with an F-statistic of only 2.06 at the SEA level. As a result, we do not report IV estimates for lower levels of aggregation.

[34] The results in column 4 also suggest that our estimates are not sensitive to using the entire U.S. In later robustness analysis we show this is true at the ESR level as well (see Table 9 below).

4.4 Heterogeneity in Income Elasticities

Another potential concern with our conclusions concerning the role of rising incomes in explaining the rising health share of GDP is that our IV estimate are based on a specific area of the country and time period, as well as a specific type of income variation. If there is substantial heterogeneity in the income elasticity of health expenditures across any of these dimensions, out of sample extrapolations may be particularly unreliable. We therefore explored (to the extent possible) whether there appears to be substantial heterogeneity in our estimated income elasticity. All in all, we read the available evidence as suggesting that the quantitative estimates are reasonably similar across different geographic samples, time periods, and different sources of income variation; we therefore don't see any reason to suspect that heterogeneous elasticities are likely to lead to a serious underestimation of the effect of rising incomes on health care expenditures.

Source and extent of income variation At a general level, one might be concerned that the source and range of the variation in income that we are exploiting may be insufficient to estimate (or detect) income elasticities significantly greater than one. To alleviate this concern, we would like to estimate similar IV regressions with spending on a good that can be classified as a luxury on a priori grounds (e.g., recreation). Although we do not have data on spending on other goods at the ESR level, we can pursue this strategy at the state level where we have data on industry-specific Gross State Products (GSP) for other service industries. Specifically, we examine the income elasticity of four potential luxuries: "amusement and recreation services," "hotels and other lodging places," "legal services" and "other services," which includes (among other things) record production, actuarial consulting, music publishing, and other consulting.[35] We also estimate the income elasticity of "food and kindred products" (a subset of non durable goods)—which we expect to be a necessity, and, for comparative purposes, health services GSP, which we already discussed in subsection 4.2.[36]

The results are shown in Table 7 and suggest that our source of variation in income is strong enough to uncover elasticities greater than one at the state level.[37] Legal services and "other" services both appear to be strong luxuries. Amusement services and hotels also show an income elasticity of close to or above 1. By contrast, food stores appear to be a necessity,

[35] For a complete definition of "other services" see: http://www.osha.gov/pls/imis/sic_manual.display?id=1014&tab=description

[36] More information on each of these categories can be found here: http://www.bea.gov/regional/gsp/default.cfm?series=SIC.

[37] First stage results for this same specification are shown in Table 6, Panel A, columns 2 and 4. Second stage results for this same specification using the AHA hospital expenditure data as the dependent variable can be found in Table 6, Panel B, columns 2 and 4.

with an income elasticity that is virtually the same as what we estimate for health services (see column 6).

A more specific concern is that, as discussed in Section 3.1, we cannot reject that our income variation at the ESR level comes entirely from changes in employment at roughly constant wages (see Table 2), while about half of income growth in the U.S. over the last half century comes from increased wages per employed individual (U.S. Census Bureau, 2008).[38] This raises the potential concern that, if the elasticity of health spending with respect to income is increasing in income, the elasticity of health care spending with respect to rising wages may be larger than the elasticity with respect to rising employment.

Table 8 investigates whether there is any evidence of this type of convexity in the income elasticity. Column 1 reports results from the baseline IV specification, while column 2 adds an interaction of the ESR's (log) income with its (log) income in 1970. This strategy allows the effect of changes in income to vary based on initial income levels and provides a simple check against the possibility that the income elasticity of health expenditures may vary systematically with the level of income of the area. We instrument for log income and the interaction of log income with 1970 log income with our standard instrument (oil reserves times log oil prices) and its interaction with log income in the area in 1970. The results show no evidence that the Engel curve for health expenditures is convex; if anything the point estimates suggest a (statistically insignificant) concave Engel curve.

As another check on the potential convexity of the relationship between income and hospital spending, we looked for nonlinearities in the reduced-form relationship. Column 3 reproduces the baseline reduced-form results for comparison and column 4 reports the results of a modified reduced-form specification, which also includes the square of the baseline instrument (i.e., $(\log p_{t-1} \times I_j)^2$ as well as $\log p_{t-1} \times I_j$). The estimates in column 4 also show no evidence of a convex relationship between income and health expenditures. The lack of any convexity in the relationship between income and health spending further suggests that the income elasticity of health expenditures is unlikely to be significantly greater at higher levels of income or for larger income changes.

Different areas and time period Table 9 explores the sensitivity of our estimates to defining the sample based on different geographic regions and different time periods. Panel A shows the first-stage results, and Panel B shows the corresponding IV estimates. Column 1 reproduces our baseline estimates, which are for the 16 Southern states focusing on the time

[38] At the state level we estimate that our instrument is associated with a statistically significant increase in wages, alhough the increase in income is still predominantly due to an increase in employment (not shown).

period 1970-1990.

As discussed above, we chose to limit our baseline sample to the Southern United States both because the oil reserves are concentrated in the South and because the ESRs in this region are more comparable, thus less likely to experience differential trends in hospital spending owing to other reasons. In column 2 we further limit the sample to the 7 Southern states that have oil reserves in our data; the results are quite similar. In column 3 we go in the opposite direction, and look at the entire United States. The results in this column show that expanding the sample to the entire United States (not including Alaska and Virginia) results in a very similar point estimate of the income elasticity (0.799 vs 0.707 in the baseline), though the estimate is less precise (standard error = 0.621 compared to 0.211 in the baseline).[39]

We also explored whether within the South our estimates were sensitive to excluding a particular state. Appendix Table A3 shows the results from estimating our baseline specification (from column 1) dropping one of the 16 states at a time. The results indicate that the estimates are generally quite robust both in terms of magnitude and statistical significance to the omission of a single state. The exception occurs when we exclude Texas. In this case, reported in column 16, the point estimate falls by about 40 percent, and combined with the increase in standard error, this makes the estimate of the income elasticity of hospital expenditure is no longer significant at the 5% level. This is not surprising since much of the variation in oil intensity in our sample is within Texas (see Figure 3).

Our baseline time period is for 1970-1990 and covers the original oil boom and bust. In column 4 of Table 9, we return to our baseline Southern states sample, but now expand the time period 1970-2005 (thus including all available years with data). Figure 2 shows that oil prices experienced a second boom starting in 1999. Nevertheless, we lose the first stage when we include the post 1990 years (and therefore do not report the corresponding IV estimate). This weaker first-stage relationship appears to reflect the inadequacy of imposing constant ESR fixed effects over a 36 year period. Indeed, when this assumption is relaxed by including state-specific time trends, the estimates again become statistically significant. This is shown in column 5, which shows a first-stage relationship and an IV estimate of similar magnitude to the baseline.

4.5 Short-Run Versus Long-Run Income Elasticities

Another concern with extrapolating from our estimates to the impact of rising income on the health share of GDP is that the short-run response of health expenditures to income may be

[39] We do not include Alaska because of the Alaska Permanent Fund (established in 1976), as well as the difficulty in forming consistent data by ESR between 1970 and 1990. We do not include Virginia because of the difficulty in forming consistent data by ESR between 1970 and 1990.

more limited than their long-run responsiveness, thus leading to dynamics in the changes in health spending following income shocks. For example, increased demand may result in the short run in higher prices, with the response of quantities emerging with a delay as capacity expands. Depending on the price elasticity of demand for health care, these dynamics will then influence overall expenditures differentially in the short and the long run.

There are no strong theoretical reasons to expect the long-run income elasticity to be substantially greater than the short-run elasticity. For example, if health care demand is inelastic (with price elasticity less than one, which is plausible, for example, because of insurance), as capacity expands in the long run in the face of rising incomes, overall health expenditures will increase less than in the short run. In addition, if long-run increases income also increase overall health, the long-run increase in health expenditures may again be less than in the short run. Nevertheless, even though there are no a priori reasons to expect long-run effects to be greater than short-run effects, it is important to understand whether our empirical strategy is estimating the former or the latter.

To investigate this issue, we repeated our regressions using decadal observations, thus removing the source of variation due to short-run changes in our instrument. Table 10 compares our baseline results—which use annual observations from 1970-1990 in columns 1 through 3—with the estimates using only decadal observations (1970, 1980, 1990) in columns 4 through 6. The first stage is slightly weaker with only the decadal observations but still reasonably strong. The IV elasticity estimate from the decadal estimate is similar to the baseline annual estimate (0.839 compared to 0.707) and statistically significant. These results therefore suggest that the long-run income elasticity is similar to the short-run elasticity.

This conclusion also receives support from the lack of capacity responses. If long-run effects were significantly different than short-run effects, we would expect to see hospitals expanding capacity (either simultaneously with the increase in health expenditures or gradually as they reach their capacity constraints). However, Table 4 showed no evidence of an increase in hospital capacity or utilization (in particular, there was no increase in admissions, patient days, hospital beds, and hospital entry in response to the rise in local income).

A related issue is that there might be heterogeneity in the adjustment dynamics of hospital spending in response to increases in income. For example, suppose that some of the ESRs respond immediately to increases in income, while other ESRs take one or two years to respond. In this case, results using the annual panel and assuming immediate and complete adjustment would underestimate the true long-run income elasticity. We show in Appendix B that specifications using 3-year averages can perform better when adjustment dynamics vary by ESR. Thus in column 7 we report results based on 3-year averages. The estimated elasticity increases slightly (from 0.707 to 0.828).

5 Robustness

In this section, we provide some robustness checks of our baseline estimates, focusing on whether our causal estimates of the effect of income on health care expenditures might be spurious and whether they may be underestimating the elasticity of the response.

5.1 Identifying assumption

Our identifying assumption is that absent oil price changes, ESRs with different levels of oil reserves would have experienced the same proportional changes in hospital expenditures. In Table 11 we explore a variety of alternative specifications designed to investigate the validity of this identifying assumption. As usual, Panel A shows the first-stage results, while Panel B shows the corresponding IV estimates. Column 1 replicates our baseline estimates.

Column 2 shows the results of a natural falsification test: we repeat the baseline analysis of equation (5) (corresponding to column 1), but also include a 5-year *lead* of the instrument, that is, $\log p_{t+5} \times I_j$ (where I_j again denotes oil reserves in ESR j). To the extent that our instrument captures the impact of rising oil prices on the area's income rather than picking up differential trends across areas with different levels of oil reserves, future oil prices should not predict current income changes. The results for the first stage in Panel A, column 2 indicate a statistically insignificant positive coefficient on the lead of the instrument. While it is reassuring that this result is not statistically significant, its magnitude is large (about 60 percent of that on the instrument). This raises some concerns serial correlation in the first-stage residual. We explore issues of serial correlation in greater depth in the next subsection. To preview, even if there is such serial correlation in the first stage, this does not necessarily create a bias in the two-stage least squares estimates. In addition, our robustness checks in the next sub-section show that the statistical and quantitative properties of our estimates are reasonably robust in alternative specifications that explicitly recognize the possibility of serial correlation in first-stage residuals.

The results from the IV estimates that include the 5-year lead of the instrument (both in the first and second stages) are reassuring for our identification strategy. Panel B column 2 shows that the estimate of income elasticity in this specification remains statistically significant and increases slightly in magnitude relative to the baseline in column 1. The negative and marginally statistically significant coefficient on the 5-year lead of the instrument raises some concerns about mean reversion, which we will also address in more depth in the next subsection.

Column 3 shows the results from an alternative check on our identification strategy, in which we additionally control for interactions between oil prices ($\log p_{t-1}$) and fixed ESR characteristics. In particular, we add separate interactions between log oil prices in year t-1 and each of log hospital expenditures in 1969, log hospital beds in 1969, log population in 1970, log area income in 1970 and log area employment in 1970 to the right-hand side. This "horse race" between our instrument and other interactions of oil prices and baseline area characteristics is useful for two complementary reasons. First, it provides greater confidence that it is the interaction between oil price shocks and availability of oil reserves leading to the source of income variation that we are exploiting. Second, it indirectly controls for differential pre-existing trends in health expenditures (and income) across ESRs, which are the main threat to our identification strategy. The results of this horse race show that both our first-stage and second-stage estimates are robust in magnitude and precision to the (simultaneous) inclusion of all of these interaction terms. Very similar estimates are obtained when we include each interaction term one by one (not shown).

Column 4 shows the results of adding region-specific linear trends for the three Census regions within the South. Column 5 shows the results of adding state-specific linear trends. These two specifications allow different regions (respectively, different states) within the South to be on different linear time trends. The first stage is reasonably robust while the IV estimates decline in magnitude, and in the case of state specific linear trends, they are no longer statistically significant. Although this last result raises some concerns about the magnitude and precision our estimates of the income elasticity, if anything, it suggests that models that do not control for state-specific trends might lead to over-estimates (rather than under-estimates) of this elasticity.

Finally, as another natural falsification exercise, we checked the implications of estimating our models on health expenditures data from 1955 through 1969 while assuming that the oil price changes took place 15 years prior (more precisely, the year 1955 is assigned the oil price for 1970, the year 1956 is assigned to the oil price in 1971, etc.).[40] The period before 1970 shows virtually constant oil prices before 1970 (see Figure 2). Therefore, if our identifying assumption is valid, we should not see any differential changes in health expenditures across areas with different oil reserves prior to 1970, and in particular, we should not see more rapid

[40]The AHA data do not contain information on hospital expenditures prior to 1955, which is why we could not extend this analysis even further back in time. We report only reduced form results for this falasification exercise because we do not have income data for the entire period from 1955 to 1969. Our primary source of income data, CBP, extends back annually to 1964 and is available irregularly dating back to 1946. Also, before 1970 only first quarter payroll and employment data are available.

increases in health expenditures in areas with greater oil reserves. Column 6 shows the first-stage and reduced-form results for our baseline specification if we limit it to the 1970 to 1984 period. The first-stage remains as does the reduced form, though the implied IV estimate is about one half the size of our baseline estimate (which uses the entire 1970-1990 period). Column 7 shows the result for the comparable falsification exercise. Reassuringly, there is no evidence of a significant reduced form in this falsification exercise. Indeed, the point estimate is the negative and not statistically significant. This finding is consistent with the identifying assumption that, absent changes in oil prices, areas of the South with different levels of oil intensity would have experienced similar trends in their hospital expenditures.

Overall, we read the results in Table 11 as broadly supportive of our identifying assumption.

5.2 Alternative specifications of the instrument

We explored the robustness of our results to alternative specifications of the instrument. Table 12 shows the results. Panel A again shows the first-stage estimates and Panel B shows the corresponding IV estimates.

Column 1 replicates our baseline first-stage specification, in which the instrument is the interaction of the total oil reserves and the log of the (lagged) oil price, i.e., $\log p_{t-1} \times I_j$, with again I_j measured as oil reserves. Column 2 reports results in which the instrument is constructed as the interaction between the level of (lagged) oil prices and oil reserves (i.e., $p_{t-1} \times I_j$ instead of $\log p_{t-1} \times I_j$).[41] Column 3 reports results when we use the log oil price at time t rather than its one year lag (i.e., $\log p_t \times I_j$ instead of $\log p_{t-1} \times I_j$). With both alternative functional forms for oil prices we continue to estimate a strong first stage and a statistically significant income elsticity in the second stage. We estimate an income elasticity of 0.53 and 0.56 in columns 2 and 3 respectively; this is slightly smaller than the baseline estimate of 0.71.

Columns 4 through 6 report results using different ways of measuring the oil intensity of the area. Recall that this variable, I_j, was proxied by total (cumulative) oil reserves in area j in our baseline specification. Figure 3B shows that the oil reserve distribution is highly skewed and one may be concerned that using the level of oil reserves might give disproportionate weight to the ESRs with the highest all reserves. Moreover, the effect of oil reserves on the demand for labor, and thus on income, may be nonlinear, with large and very large oil reserves leading to similar effects on income when oil prices rise. Motivated by these considerations, in column 4 we report results with an alternative measure of I_j, where oil reserves are censored at the 95th

[41] By contrast, the log-log specification used in equation (4) is natural, since it allows us to estimate the elasticity directly.

percentile of oil reserve distribution (the instrument is again constructed by interacting this measure with $\log p_{t-1}$). The results are very similar to the baseline. We continue to estimate a strong first stage, and a statistically signficant income elasticity; the estimated income elasticity of 0.619 (standard error = 0.203) is slightly smaller than the baseline estimate. We also obtain similar estimates if instead we censor oil reserves at the 90th or the 99th percentiles (not shown).

As another check on possible nonlinearities, column 5 measures oil intensity by an indicator variable for whether there are any large oil wells in the ESR (i.e., the instrument is now $\mathbf{1}(I_j > 0)$). The first-stage is now slightly weaker (F-statistic of about 8), and the estimated income elasticity rises to 1.06 (standard error = 0.639); it is not statistically significantly different from zero at conventional levels.

Finally, as an alternative measure of oil intensity, in column 6 we measure oil intensity as the (de-meaned) mining share of employment in the ESR in 1970, interacted with an indicator variable for whether there are any large oil wells in the ESR.[42] Our first stage is now marginally stronger than in the preceding specification (F-statistic of about 12), but we estimate a statistically insignificant income elasticity of 0.839 (standard error = 0.851).

5.3 Serial correlation

Our baseline estimates allow for an arbitrary variance-covariance matrix at the state level. In this subsection we explore other specifications that allow and control for serial correlation and mean reversion in the second stage. Table 13 reports the results from alternative adjustments to the standard errors. Columns 1 and 2 reproduce our baseline first-stage and IV estimates, respectively. A potential concern with our baseline specification is that since it includes only 16 states, these standard errors may be downward biased because of the relatively small number of clusters (Cameron, Gelbach and Miller, 2008).

One potential way to address this concern is to allow for an arbitrary variance-covariance matrix at the ESR level (rather than the state level). These standard errors may have better asymptotic properties, since we now have 99 clusters (instead of 16 as in our baseline). A possible disadvantage is that this specification does not allow for correlation across ESRs within the same state, which may be important in practice.[43] Columns 3 and 4 report the

[42] We include the indicator variable for whether there are an large oil wells because mining employment is defined in the data to include all workers in oil mining, natural gas and coal mining. The indicator for oil wells is designed to separate out high mining share areas that are most likely not oil mining (such as coal mining areas of West Virginia).

[43] For example, a boom in an oil-rich ESR may attract in-migration from other ESRs within the same state, reducing total payroll income in these ESRs and also potentially affecting health care expenditures through this and other channels. The result will be a negative correlation in ESR-level residuals within a state.

results. Clustering at the ESR level leads to a weaker first-stage (F-statistic = 5.18) and a correspondingly less precise IV estimate, which is now significant at the 6 percent level.

Another strategy to correct for potential biases from the small number of clusters at the state level is the wild bootstrap procedure suggested by Cameron, Gelbach and Miller (2008).[44] Column 5 reports the results of a wild bootstrap in which we resample states with replacement; we find reassuringly similar (indeed somewhat smaller) p-values to our baseline specification.[45] Column 6 reports the results of a wild bootstrap that resamples ESRs with replacement. Analogously to clustering at the ESR level compared to clustering at the state level, the p values are somewhat higher when we bootstrap at the ESR level rather than when we bootstrap at the state level, though the estimated elasticity is still statistically significant at the 5 percent level.

An alternative strategy to address concerns about potential serial correlation is to directly model the dynamics of the error term in our structural equation (4) and then estimate this extended model using instrumental-variables Generalized Least Squares (IV-GLS). In all of our IV-GLS specifications we allow for heteroscedasticity in the second-stage error term as well as for serial correlation. We also experiment with various assumptions regarding the nature of the autocorrelation. The details of the implementation of IV-GLS and the procedure for the computation of the standard errors are discussed in Appendix B. Table 14 reports the results. Column 1 reports estimates from our baseline specification, but using a subsample of our original data. We limit the sample to the 96 (out of 99) ESRs that have data in the full 21 years from 1970 to 1990. Column 1 verifies that this has no meaningful effect on our baseline results. Column 2 reports IV-GLS results assuming a common autocorrelation coefficient across all ESRs (the estimate of this common autocorrelation coefficient is 0.570). These estimates imply an elasticity of 0.681 (standard error = 0.241), which is very similar to the baseline results in column 1 (elasticity of 0.682, standard error of 0.214). Column 3 reports results assuming an AR(2) specification of the residuals and common autocorrelation coefficients (which are estimated to be 0.508 and 0.116). The estimated elasticity declines to 0.606 (from 0.682) but it is still precisely estimated (standard error = 0.238). Columns 4 and 5 report results assuming AR(1) errors but with state-specific and ESR-specific autocorrelation coefficients, respectively. The coefficients are always smaller than our baseline estimates but they are more precisely estimated (this pattern is not surprising, since rather than allowing for an arbitrary variance-covariance matrix, we are imposing a parametric model). In any case,

[44] We are very grateful to Doug Miller for providing sample code and discussing it with us.

[45] In their Monte Carlo study, Cameron et al find it is important to calculate p-values based on t-statistics rather than parameter estimates. We find even lower p-values when we use the parameter estimates; we report the p-value from t-statistics following their recommendation.

these results provide no evidence of larger income elasticities than our baseline estimates.

Finally column 6 reports results from including a lagged dependent variable on the right-hand side to allow for possible mean reversion. In this extended model, the estimate of income elasticity is similar to the baseline and the lagged dependent variable is not significant. However, the estimator in column 6 is inconsistent because of the presence of the lagged dependent variable on the right-hand side. Reassuringly, when we instrument for the lagged dependent variable with further lags, we obtain similar point estimates, though much larger standard errors (results not shown).[46]

6 Conclusion

This paper has explored the role of the secular rise in incomes in the dramatic run-up in the health share of GDP in the United States, which increased from 5 percent of GDP in 1960 to 16 percent in 2005. A common conjecture is that rising incomes have played a primary role in the increase in the health share of GDP. A finding of a primary role for rising incomes would have important implications for forecasting the future growth of the health share of GDP as well as potential implications for the optimality of the rising health share of GDP. Yet, surprisingly, there are virtually no empirical estimates of the causal effect of aggregate income on health spending.

We attempt to estimate the causal effect of aggregate income on aggregate health expenditures by instrumenting for local area income with time-series variation in oil prices interacted with cross-sectional variation in the oil reserves in different areas of the Southern United States. This strategy is attractive not only because it isolates a potentially exogenous source of variation in incomes but also because it incorporates local general equilibrium effects, as we estimate the response of health expenditures in the area to an aggregate change in incomes. Across a wide range of specifications, we estimate a positive and statistically significant income elasticity of hospital expenditures that is almost always less than 1. Our central estimate is an income elasticity of 0.71 (standard error = 0.21). This estimate is reasonably robust to a range of alternative specifications.

[46] On the other hand, if we estimate the same model using first differences, the results are significantly different (and lead to even smaller elasticities) than our baseline specification with ESR fixed effects. The difference between first-difference and fixed-effects estimators may suggest either serial correlation in residuals or potentially other forms of misspecification, for example because of heterogeneous adjustment dynamics. Nevertheless, we do not think that this difference between the two estimators is a major cause for concern for two reasons. First, the IV-GLS and lagged dependent variable models reported in Table 14 are more flexible and general than estimation in first differences, and yield results very similar to our baseline estimates. Second, the results using three-year averaged data in Table 9 also yield estimates that are very similar to our baseline results and the Monte Carlo evidence reported in Appendix B suggests that this estimation method is most robust against heterogeneous adjustment dynamics.

Our central point estimate suggests that rising income did not contribute to the rise in the health *share* of GDP between 1960 and 2005. Our 95 percent confidence interval—which includes at its upper end an income elasticity of 1.1—suggests that we can reject a role of rising income of explaining more than a very small part, 0.4 percentage points, of the 11 percentage point increase in the health share of GDP over that time period. Although considerable caution is warranted in extrapolating estimates from a particular source of variation, time period, and part of the country to the overall impact of rising income in the post war period, we provided suggestive evidence for why many of the most salient potential concerns are not likely to pose major threats to our conclusions.

While our findings suggest that the increase in income is unlikely to be a primary driver of the increase in the health share of GDP, they do not provide an answer to the question of what is behind this notable trend. There is general consensus that rapid progress in medical technologies in the major driver of increasing health expenditures (e.g., Newhouse, 1992, Fuchs, 1996, Cutler, 2003, Congressional Budget Office, 2008), though presumably technological progress itself is being spurred by other factors. Our analysis suggests that the rise in income is unlikely to be the major driver of medical innovations either. An interesting possibility is that institutional factors, such as the spread of insurance coverage, have not only directly encouraged increased spending but also induced the adoption and diffusion of new medical technologies (Weisbrod 1991, Finkelstein 2004, Finkelstein 2007, Acemoglu and Finkelstein, 2008). This channel of induced innovation could not only account for the increase in the health share of GDP in the United States, but could also be a major contributor to the similar trends experienced by other OECD countries, if technological advances in the United States spread relatively rapidly to other advanced economies. An investigation of this possibility, as well as more general analyses of the determinants of technological change in the health care sector, are important and interesting areas for further work.

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Appendix A: Induced Innovation Effects

In this Appendix, we present a simple model to illustrate why we expect that any induced innovatoin effects in the health care sector due to rising income are unlikely to be large, given that we estimate an income elasticity of health expenditure that is less than one. We start with a brief review of a simple model with endogenous technology responses to changes in market size. To economize on space, the reader is referred to Acemoglu (2002, 2007, 2009) or Acemoglu and Linn (2004) for the details (and microfoundations for various assumptions imposed here for simplicity).

Consider an infinite-horizon, continuous-time economy with $g = 1, ..., G$ goods. To communicate the basic ideas, we take expenditures on these goods as given, represented by $[E_g(t)]_{t=0}^{\infty}$ for good g (in terms of some numeraire). We also assume that all of these goods have unit price elasticity (otherwise, we could not take these expenditures as given). We then ask how changes in these expenditure levels affect the types of technologies developed by profit-maximizing firms. These assumptions imply that at time t the demand for good g will be

$$D_g(p_g(t), t) = \frac{E_g(t)}{p_g(t)},$$

where $p_g(t)$. Suppose, in particular, that each good can be supplied in different qualities, denoted by $q_g(t) \in \mathbb{R}_+$, and consumers will purchase whichever variety of the good has the highest price-adjusted quality, that is, among varieties of good g, g_1,..., g_V, available in the market, they will choose the one with highest $q_{g_v}(t)/p_{g_v}(t)$. This implies that whichever firm has the highest quality variety for good g at time t will generate revenues equal to $E_g(t)$. Suppose also that all goods, regardless of quality, can be produced at marginal cost equal to 1 (in terms of the numeraire). This implies that the firm with the highest price-adjusted quality for good g at time t (presuming that there is a single such firm) will make profits equal to

$$\pi_g(t) = (p_g(t) - 1)\frac{E_g(t)}{p_g(t)}. \tag{8}$$

Innovation and technological progress are modeled as in the quality ladder models of Aghion and Howitt (1992) and Grossman and Helpman (1991) (see also Acemoglu, 2009, for a textbook treatment). Suppose that starting from leading-edge quality $q_g(t)$ at time t, R&D directed to good g generates (stochastic) innovations for this good. An innovation creates a new leading-edge quality $\lambda q(t)$, where $\lambda > 1$. There is free entry into R&D and each firm has access to an R&D technology that generates a flow rate δ_g of innovation for every dollar spent for research on good g. So if R&D expenditure at time t for good g is $z_g(t)$, the flow rate of innovation is

$$\delta_g z_g(t).$$

Differences in δ_g's introduce the possibility that technological progress is scientifically more difficult for some goes than for others. A firm that makes an innovation has a perpetual patent on the good that it invents, and will be able to sell it until a better good comes to the market.

Consider good g, where current quality is $q_g(t)$. Since consumers will purchase from the highest price-adjusted quality and, by definition, the next best firm must have quality $q_g(t)/\lambda$ and can price as low as its marginal cost, 1. This implies that the leading-edge producer must set a limit price

$$p_g(t) = \lambda \text{ for all } g \text{ and } t. \tag{9}$$

Then (8) give the time t profits of the firm with the leading-edge variety of good g, with quality $q_g(t)$ as

$$\pi_g(q_g(t)) = \frac{\lambda - 1}{\lambda} E_g(t). \tag{10}$$

Firms are forward-looking, and discount future profits at the interest rate r. We assume that this interest rate is constant. The discounted value of profits for firms can be expressed by a standard dynamic programming recursion. $V_g(t \mid q_g)$, the value of a firm that owns the most advanced variety of good g with quality q_g at time t, is

$$rV_g(t \mid q_g) - \dot{V}_g(t \mid q_g) = \pi_g(q_g(t)) - \delta_g z_g(t) V_g(t \mid q_g), \tag{11}$$

where $\pi_g(q_g(t))$ is the flow profits given by (10), and $z_g(t)$ is R&D effort at time t on this line by other firms. Throughout, we assume that the relevant transversality conditions hold and discounted values are finite. Moreover, because of the standard replacement effect first emphasized by Arrow (1962), the firm with the best technology does not undertake any R&D itself (see, for example, Aghion and Howitt, 1992, Acemoglu, 2009). Intuitively, the value of owning the best technology in line g, $rV_g(t \mid q_g)$, is equal to the flow profits, $\pi_g(q_g(t))$, plus the potential appreciation of the value, $\dot{V}_g(t \mid q_g)$, and takes into account that at the flow rate $\delta_g z_g(t)$ there will be a new innovation, causing the current firm to lose its leading position and to make zero profits thereafter.

Free entry into R&D for developing new technologies for each good implies that there will be entry as long as additional R&D is profitable. Therefore, free entry requires the following complementary slackness condition to hold:

$$\text{if } z_g(t) > 0, \text{ then } \delta_g V_g(t \mid q_g) = 1 \text{ for all } g \text{ and } t \tag{12}$$

(and if $z_g(t) = 0$, $\delta_g V_g(t \mid q_g) \leq 1$ and there will be no innovation for this good at time t).

An equilibrium in this economy is given by sequences of prices $p_g(t)|_{g=1,..G}$ that satisfy (9), and R&D levels $z_g(t)|_{g=1,..g}$ that satisfy (12) with $V_g(\cdot)$ given by (11).

An equilibrium is straightforward to characterize. The free entry condition must hold at all t. Supposing that it holds as the quality in some interval $[t', t'']$, we can differentiate this equation with respect to time, which yields $\dot{V}_g (t \mid q_g) = 0$ for all g and t (as long as $z_g (t) > 0$). Substituting this equation and (12) into (11) yields the levels of R&D effort in the unique equilibrium as

$$z_g (t) = \max \left\{ \frac{\delta_g (\lambda - 1) \lambda^{-1} E_g (t) - r}{\delta_g} ; 0 \right\} \text{ for all } g \text{ and } t. \tag{13}$$

Equation (13) highlights the market size effect in innovation: the greater is expenditures on good g, $E_g (t)$, the more profitable it is to be a supplier of that good, and consequently, there will be greater research effort to acquire this position. In addition, a higher productivity of R&D as captured by δ_g also increases R&D, and a higher interest rate reduces R&D since current R&D expenditures are rewarded by future revenues.

Given equation (13), we can now ask how a rise in overall income in the economy will affect the direction of technological change. Such a change will shift the expenditures from $\left\{ [E_g (t)]_{t=0}^{\infty} \right\}_{g=1,\dots,G}$ to $\left\{ \left[\tilde{E}_g (t) \right]_{t=0}^{\infty} \right\}_{g=1,\dots,G}$. However, expenditures on some good will increase by more, in particular, those that are "luxury goods" will see their expenditures increase by more. Equation (13) then implies that innovations will be tend to be directed towards those goods.

To highlight the implications of this type of induced technological change for our purposes, suppose that the economy consists of two goods, health care and the "rest". Suppose also that equation (13) leads to positive R&D for both groups of goods. Moreover, let us parameterize expenditures on these two groups of goods as $E_{health} (t) = a_{health} (t) Y (t)$ and $E_{rest} (t) = a_{rest} (t) Y (t)$, where $Y (t)$ is total income (GDP). Our ESR-level estimates imply that, without the induced technology responses, $a_{rest} (t) > a_{health} (t)$, so that with the rising incomes $E_{rest} (t)$ increases more than $E_{health} (t)$. Equation (13) then implies that $z_{rest} (t)$ will increase (proportionately) by more than $z_{health} (t)$, or that $z_{rest} (t) / z_{health} (t)$ will increase. Importantly, this conclusion is independent of the values of the δ_g's as long as they are such that both $z_{rest} (t) > 0$ and $z_{health} (t) > 0$. This result is the basis of our argument that, given the patterns relationship between health care expenditures and income we observe at the ESR level, national-level directed technological change is unlikely to significantly increase the responsiveness of health care expenditures to aggregate income changes.

Equation (13) also highlights the conditions under which this conclusion needs to be modified. If it happens to be the case that $z_{health} (t) = 0$ and $z_{rest} (t) > 0$ to start with, then an increase in $E_{health} (t)$ that is proportionately less than that in $E_{rest} (t)$ may still have a disproportionate effect on innovation in the health care sector by making $z_{health} (t) > 0$. Intuitively, before the changes in expenditures, technological change in the health care sector would have been unprofitable, and as the market size passes a certain threshold (in this case equal to $\delta_g^{-1}(\lambda - 1)^{-1}\lambda r$), innovation jumps up from zero to the positive level. While this is theoretically possible, we believe that it is unlikely to be important in the context of the health care sector, since as discussed earlier in the main text, throughout the 20th century technological change in the health care sector was positive and in fact quite rapid (Cutler and Meara, 2003).

Appendix B: Econometric Issues

In this Appendix, we discuss a number of econometric issues related to the correction for serial correlation and dynamics.

Implementation of IV GLS

We now provided details of the implementation of the IV-GLS estimator used in subsection 5.3. In particular, we use the following procedure for this estimation. First, we recover estimates of the residuals ($\hat{\varepsilon}_{jt}$) from the baseline IV specification. Then we use these residuals to estimate the autocorrelation coefficients. For example, when we estimate ESR-specific autocorrelation coefficients, we run the following regression of $\hat{\varepsilon}_{jt}$ on its lag ($\hat{\varepsilon}_{j,t-1}$) for each ESR to recover an estimate of the ESR-specific autocorrelation coefficient, $\hat{\rho}_j$:

$$\hat{\varepsilon}_{jt} = \rho_j \hat{\varepsilon}_{j,t-1} + \xi_{jt}$$

These autocorrelation coefficients are used to create adjusted (LHS and RHS) variables as follows:

$$\begin{aligned} \tilde{x}_{jt} &= x_{jt} - \hat{\rho}_j x_{j,t-1} \\ \tilde{y}_{jt} &= y_{jt} - \hat{\rho}_j y_{j,t-1} \end{aligned}$$

Finally, to adjust for ESR-level heteroskedasticity, we run IV again using the adjusted variables above to recover a new set of residuals ($\hat{\varepsilon}'_{jt}$) and then we create a weighting matrix $\hat{\mathbf{\Omega}}$ using these residuals:

$$\hat{\mathbf{\Omega}} = \mathbf{I}\left(N_T\right) \otimes \mathbf{diag}\left(\frac{1}{T}\sum_{t=1}^{T}(\hat{\varepsilon}'_{1,t}), \frac{1}{T}\sum_{t=1}^{T}(\hat{\varepsilon}'_{1,t}), \ldots, \frac{1}{T}\sum_{t=1}^{T}(\hat{\varepsilon}'_{J,t})\right)$$

where $\mathbf{I}(\cdot)$ creates an identity matrix and $\mathbf{diag}(\cdot)$ creates a diagonal matrix from a vector. Using this weighting matrix, the IV-GLS estimator is given as follows:

$$\hat{\boldsymbol{\beta}}_{IV-GLS} = (\mathbf{X}'\hat{\mathbf{\Omega}}^{-1}\mathbf{Z}(\mathbf{Z}'\hat{\mathbf{\Omega}}^{-1}\mathbf{Z})^{-1}\mathbf{Z}'\hat{\mathbf{\Omega}}^{-1}\mathbf{X})^{-1}\mathbf{X}'\hat{\mathbf{\Omega}}^{-1}\mathbf{Z}(\mathbf{Z}'\hat{\mathbf{\Omega}}^{-1}\mathbf{Z})^{-1}\mathbf{Z}'\hat{\mathbf{\Omega}}^{-1}\mathbf{y}$$

Performance of different estimators with heterogeneous adjustment dynamics

We now describe results from a simple Monte Carlo study to investigate the performance of various estimators under heterogeneous long-run adjustment dynamics. Our Monte Carlo results suggest that heterogeneous adjustment dynamics may lead traditional fixed effects instrumental variables (FE-IV) estimators to underestimate the true long-run effect. We show that using 3-year averages can reduce this bias. Reassuringly, our 3-year average results are similar to our baseline results (Table 9, column 7). The remainder of this section describes the set of our Monte Carlo study and our results.

We define the following variables for our simulation:

$$
\begin{aligned}
z_{jt} &= N(0,1) \\
a_{jt} &= N(0,1) \\
x_{jt} &= N(0,1) + z_{jt} + a_{jt} \\
\delta_j &= N(0,1) \\
\varepsilon_{jt} &= \rho\varepsilon_{j,t-1} + \xi_{jt} \\
y_{jt} &= x_{jt} + a_{jt} + \delta_j + \varepsilon_{jt}
\end{aligned}
$$

where j indexes one of the J panels and t indexes on of the T time periods within a panel. $N(0,1)$ represents an i.i.d. standard normal random variable, z_{jt} represents a valid instrumental variable for x_{jt}, a_{jt} is the unobserved variable that induces a correlation between x_{jt} and the error term in the endogenous fixed effects regression of y_{jt} on x_{jt}, and δ_j is an unobserved fixed effect. ε_{jt} is the error term in the model which follows an AR(1) process ($|\rho| < 1$). We also experiment with serveral other ways to construct y_{jt}:

$$
\begin{aligned}
y_{jt} &= x_{j,t-1} + a_{jt} + \delta_j + \varepsilon_{jt} \\
y_{jt} &= \begin{cases} x_{jt} + a_{jt} + \delta_j + \varepsilon_{jt} & \text{if } j < J/2 \\ x_{j,t-1} + a_{jt} + \delta_j + \varepsilon_{jt} & \text{if } j \geq J/2 \end{cases} \\
y_{jt} &= \begin{cases} x_{jt} + a_{jt} + \delta_j + \varepsilon_{jt} & \text{if } j < J/3 \\ x_{j,t-1} + a_{jt} + \delta_j + \varepsilon_{jt} & \text{if } J/3 \leq j < 2J/3 \\ x_{j,t-2} + a_{jt} + \delta_j + \varepsilon_{jt} & \text{if } j \geq 2J/3 \end{cases}
\end{aligned}
$$

We experimented with the following estimators in in our Monte Carlo study:

1. (FE-IV) Fixed effects IV regression of y_{jt} on x_{jt}, instrumenting x_{jt} by z_{jt}

2. (FD-IV) First differences IV regression of $(y_{jt} - y_{j,t-1})$ on $(x_{jt} - x_{j,t-1})$, instrumenting $(x_{jt} - x_{j,t-1})$ by $(z_{jt} - z_{j,t-1})$

3. (FE-IV-LAG) Fixed effects IV regression of y_{jt} on $x_{j,t-1}$, instrumenting $x_{j,t-1}$ by $z_{j,t-1}$

4. (FE-IV-3YR) Fixed effects IV regression of $\tilde{y}_{js}$ on $\tilde{x}_{js}$ instrumenting $\tilde{x}_{js}$ by $\tilde{z}_{js}$ (where $\tilde{v}_{js}$ denotes the three-year averages of v_{jt} and s represents a three-year groups of years)

5. (FD-IV-3YR) First differences IV regression of $(\tilde{y}_{js} - \tilde{y}_{j,s-1})$ on $(\tilde{x}_{js} - \tilde{x}_{j,s-1})$, instrumenting $(\tilde{x}_{js} - \tilde{x}_{j,s-1})$ by $(\tilde{z}_{js} - \tilde{z}_{js})$

Finally, we choose $J = 10$ and $T = 30$, and we experiment with three values of ρ $(0.1, 0.5, 0.9)$.

The results (based on 500 simulations) are given in Appendix Table A4. There are five panels of results corresponding to each of the five estimators mentioned above. The resuls are the mean of the estimates across each of the simulations and the standard deviation of the parameter estimates (in parentheses underneath). The first panel reports the FE-IV results. As would be expected, the standard deviation of the parameter estimates is larger when there are higher amounts of serial correlation. The second panel reports FD-IV results, where (also as expected) the standard deviation of the parameter estimates goes down as there is more serial correlation. The third panel reports FE-IV-LAG results, and the last two columns report the two sets of 3-year average results (FE-IV-3YR and FD-IV-3YR).

Each panel reports results for the same set of four models. The first row is the standard model where all panels adjust instantly. All estimators except FE-IV-LAG perform very well (the average of the parameter estimates is very close to the true value of 1.000). The second row reports results using a model where all panels take one time period to adjust. For this model the FE-IV and FD-IV results perform very poorly, while FE-IV-LAG unsurprisingly performs optimally. Interestingly, FE-IV-3YR still performs reasonably well, though for all degrees of serial correlation the estimates are roughly 2/3 of the true value.

The final two rows (rows 3 and 4) report results when there heterogeneity in the adjustment dynamics (where a random set of panels responds instantly and another random set of panels does not respond instantly). For all estimators the results are attentuated away from the true coefficient, but the FE-IV-3YR estimator always performs best, even when there is substantial serial correlation.

We conclude two things from this simulation exercise: (1) heterogeneous adjustment dynamics can lead standard estimators (FE-IV and FD-IV) to underestimate the true long-run effect and (2) estimators using 3-year averages appear to be reasonably robust to a moderate amount of heterogeneity in adjustment dynamics.

Table1: Descriptive Statistics

Variable	Mean	Standard Deviation
County Business Patterns Data		
Total Income (Payroll); ($millions)	2916.9	6066.7
Total Employment (millions)	0.21	0.35
Current Population Reports and National Health Interview Survey Data		
Population (millions)	0.69	0.90
HUWP (millions)	0.61	0.85
Oil and Gas Data Book Data		
Oil Reserves (million barrels)	532.3	1596.1
AHA Hospital Data		
Total Expenditures ($millions)	292.6	636.3
Hospital Payroll ($millions)	139.9	284.1
Admissions (millions)	0.11	0.16
In-Patient Days (millions)	1.08	1.47
Beds (000's)	4.15	5.65
Full-time Equivalents (000's)	9.58	14.55
RN / (LPN + RN)	0.63	0.12
# of Technologies	47.0	18.1
# of Hospitals	24.67	26.58
BEA GSP Data (all in $millions)		
Total GSP	54559.5	60731.7
(Industry-Specific GSPs)		
Health Services	1639.9	2182.0
Amusement and Recreation Services	150.3	266.4
Hotels and Other Lodging	237.7	343.6
Legal Services	312.9	575.2
Other Services	590.0	978.2
Food	521.1	485.2

Notes: Summary statistics are for the baseline sample of 99 Economic Subregions (ESRs) in the South between 1970 and 1990. All statistics are at the ESR-Year level except for GSP which is at the state-year. Source for variables is given in italics. N = 2065 at ESR - Year except for RN/(LPN+RN) which is 1576 and Inpatient days which is 1967) See text for more details on construction of data set. Data on RNs and LPNs is only available in 1970, 1972, 1974, 1976, 1978, and 1980-1990. Data on in-patient days is not available in 1979. HUWP is a hospital-utilization weighted measure of population.

Table 2: First Stage

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Total Income	Total Employment	Population	Income per capita	Population < 55	Population > 55	Income per HUWP
Oil Reserves$_j$ ×	12.900	15.542	5.211	7.689	6.421	1.549	9.436
log(oil price)$_{t-1}$	(2.980)	(2.572)	(1.479)	(1.954)	(1.756)	(1.530)	(2.293)
	[0.001]	[0.000]	[0.003]	[0.001]	[0.002]	[0.328]	[0.001]
R^2	0.994	0.969	0.997	0.984	0.997	0.996	0.983
N	2065	2065	2065	2065	2065	2065	2065
F-statistic	18.74	36.53	12.41	15.49	13.37	1.02	16.93

Notes: Table reports results from estimating variants of equation (2) and (5) by OLS. Dependent variables are defined in column headings and are all in logs; in column 7 the dependent variable is Income (as measured by payroll) divided by a hospital-utilization weighted measure of population (HUWP). The sample is all Southern states between 1970 and 1990. Unit of observation is an Economic Sub Region (ESR)-year. All models include ESR and year fixed effects. Standard errors, adjusted to allow for an arbitrary variance-covariance matrix for each state over time, are in parentheses and p-values are in brackets.

Table 3: Hospital Expenditures

	(1)	(2)	(3)	(4)	(5)
		Reduced Form		No Pop. Adjust.	Per Capita Pop. Adjust.
	OLS	OLS	IV	IV	IV
$\log(\text{Income})_{jt}$	0.065		0.707	0.801	0.666
	(0.116)		(0.211)	(0.155)	(0.261)
	[0.583]		[0.004]	[0.000]	[0.022]
Oil Reserves_j ×		6.673			
$\log(\text{oil price})_{t-1}$		(2.166)			
		[0.008]			
R^2	0.903	0.903	0.900	0.989	0.970
N	2065	2065	2065	2064	2064

Notes: Table reports results of estimating equations (1), (4) or (6) by OLS or IV as indicated. Dependent variable is log hospital expenditures. In all columns income is measured by payroll. In columns (1) through (3) both hospital expenditures and income are divided by a hospital-utilization weighted measure of population (HUWP) before taking logs (see equations 4 and 6). In column (4) hospital expenditures and income are not adjusted, and in column (5) both hospital expenditures and income are divided by the total population before taking logs. The sample is all Southern states between 1970 and 1990. Unit of observation is an Economic Sub Region (ESR)-year. All models include ESR and year fixed effects. Standard errors, adjusted to allow for an arbitrary variance-covariance matrix for each state over time, are in parentheses and p-values are in brackets.

Table 4: Other Hospital Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Dependent Variable:	Total Hospital Expenditures	Total Hospital Payroll	FTE	RN/ (RN+LPN)	Admissions	In-Patient Days	Beds	Num Hospitals	#Techs	Open-Heart Surgery	Radio-isotope Therapy
$\log(\text{Income})_{jt}$	0.707	0.935	0.058	0.352	-0.402	-0.993	-0.665	-0.542	-0.130		
	(0.211)	(0.228)	(0.217)	(0.098)	(0.188)	(0.475)	(0.446)	(0.350)	(0.216)		
	[0.004]	[0.001]	[0.793]	[0.003]	[0.050]	[0.054]	[0.156]	[0.142]	[0.559]		
Oil Reserves_j ×										-0.030	0.061
$\log(\text{oil price})_{t-1}$										(0.034)	(0.175)
										[0.373]	[0.727]
R^2	0.900	0.958	0.894	0.863	0.795	0.887	0.874	0.982	0.945		
N	2065	2064	2065	1576	2065	1967	2065	2065	2065	849	262

Notes: Columns 1 through 9 report IV estimates of equation (4) with the first stage given by equation (5). Column 1 reproduces baseline results from column 3 in Table 3. Columns 10 and 11 report Cox proportional hazard model estimates of equation (7). Each column shows results for a different dependent variable, as indicated in the column heading. Dependent variables in columns 1-3 and 5-7 are in logs and are divided (before taking logs) by a hospital-utilization weighted measure of population (HUWP). Dependent variables in columns 8 and 9 are in logs but not adjusted by any population measure. Dependent variable in columns 10 and 11 is an indicator variable for whether an at-risk Ecnomic Sub Region (ESR) adopts the technology in that year. In columns 1 through 9 income is measured by the wage bill divided by HUWP. In column 10, there are 62 ESRs that have not adopted open-heart surgery technology by 1970 and 24 ESRs that have not adopted by 1990. In column 11, there are 31 ESRs that have not adopted radioisotope therapy by 1972 (the first year data are available) and 8 ESRs that have not adopted by 1990. Data for RNs and LPNs only exist in 1970, 1972, 1974, 1976, 1978, and 1980-1990. Data for in-patient days do not exist in 1979. The sample is all Southern states between 1970 and 1990. Unit of observation is an Economic Sub Region (ESR)-year. All models include ESR and year fixed effects, except columns 10 and 11 which have region fixed effects, year fixed effects and controls for total hospital beds and hospital expenditures in 1970. Standard errors, adjusted to allow for an arbitrary variance-covariance matrix for each state over time, are in parentheses and p-values are in brackets.

Table 5: Hospital Spending Versus Overall Health Spending

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Regression	First Stage OLS	IV	IV	IV	IV	IV	IV	IV	IV
Dependent variable	Income	Total Health Care Exp.	Hosp. Exp.	Physician and Other Services	Dental Services	Drugs and Other Medical Non-durables	Vision Products	Nursing Care	Other Health Services
Panel A: Southern States Only									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Oil Reserves$_j$ × log(oil price)$_{t-1}$	3.719								
	(0.781)								
	[0.000]								
log(Income)$_{jt}$		0.079	0.090	0.199	0.631	0.267	1.182	-1.244	-0.325
		(0.080)	(0.155)	(0.154)	(0.099)	(0.119)	(0.504)	(0.309)	(0.213)
		[0.342]	[0.570]	[0.215]	[0.000]	[0.041]	[0.033]	[0.001]	[0.148]
R^2	0.983	0.998	0.995	0.995	0.99	0.992	0.911	0.926	0.961
N	236	236	236	236	236	236	236	236	236
F-statistic	22.67627								
Share of Total Health Exp.			46.30%	24.73%	5.17%	11.33%	1.80%	7.02%	3.44%
Panel B: All U.S.									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Oil Reserves$_j$ × log(oil price)$_{t-1}$	3.195								
	(0.607)								
	[0.000]								
log(Income)$_{jt}$		0.107	0.148	0.372	0.653	0.314	0.751	-1.914	-0.934
		(0.182)	(0.162)	(0.192)	(0.177)	(0.117)	(0.820)	(1.013)	(0.786)
		[0.558]	[0.366]	[0.058]	[0.001]	[0.010]	[0.364]	[0.065]	[0.241]
R^2	0.98	0.996	0.964	0.973	0.986	0.989	0.877	0.921	0.914
N	729	729	729	729	729	729	729	729	729
F-statistic	27.73108								
Share of Total Health Exp.			45.06%	25.04%	6.07%	10.40%	2.02%	8.57%	3.39%

Notes: Standard errors, adjusted to allow for an arbitrary variance-covariance matrix for each state over time, are in parentheses and p-values are in brackets. All dependent variables are in logs and divided by a hospital-utilization weighted measure of population (HUWP). In all columns income is measured by payroll divided by HUWP. Sample is Southern states in Panel A and All US (except Alaska and Virginia) in Panel B. Data from Health Care Finance Administration (HCFA) is available in 1972, 1976 - 1978, and 1980-1990. Column 1 estimates the first stage. Columns 2 through 9 report IV estimates of equation (4). In all columns, observations are at the state-year level.

Table 6: Labor Income vs. All Income

Panel A: First Stage

Dependent Variable: Income

	(1)	(2)	(3)	(4)	(5)
Oil Reserves$_j$ ×	9.436	2.645	3.209	2.280	2.956
log(oil price)$_{t-1}$	(2.293)	(0.510)	(0.842)	(0.462)	(0.697)
	[0.001]	[0.000]	[0.002]	[0.000]	[0.000]
R^2	0.983	0.989	0.989	0.985	0.982
N	2065	326	326	1015	1015
F-statistic	16.93	26.93	14.54	24.32	17.99
% increase in income given the rise in oil prices from 1970 to 1980 for the following differential in Oil Reserves					
1 standard deviation	3.64%	5.93%	7.19%	3.36%	4.36%

Panel B: IV

Dependent Variable: Hospital Expenditures

	(1)	(2)	(3)	(4)	(5)
log(Income)$_{jt}$	0.707	0.564	0.465	0.747	0.577
	(0.211)	(0.223)	(0.156)	(0.353)	(0.263)
	[0.004]	[0.023]	[0.009]	[0.040]	[0.033]
R^2	0.900	0.992	0.992	0.980	0.981
N	2065	326	326	1015	1015

Specification

	(1)	(2)	(3)	(4)	(5)
Income definition	Payroll	Payroll	GSP	Payroll	GSP
Geographic level of analysis	ESR	State	State	State	State
Geographic sample	South	South	South	USA	USA

Notes: Table reports estimates of variants of estimating equation (5) by OLS in Panel A and equation (4) by IV in Panel B. In all specifications income and hospital expenditures are divided by hospital-utilization weighted measure of population (HUWP) and then logged. Bottom rows define the specification variants; these are the definition of income (Payroll or GSP), the geographic level of analysis (ESR or State) and the geographic sample (South or all US). In all columns the years of analysis are 1970 - 1990. The sample is all Southern states between 1970 and 1990 in columns (1) through (3); columns (4) and (5) expand sample to all US (except Alaska and Virginia). Column (1) reproduces baseline results from column (3) in Table 3. Unit of observation is an ESR-year in column (1) and a state-year in columns (2) through (5). Column (1) includes Economic Subregion (ESR) and year fixed effects; columns (2) through (5) include state and year fixed effects. Standard errors, adjusted to allow for an arbitrary variance-covariance matrix for each state over time, are in parentheses and p-values are in brackets.

Table 7: Income Elasticity of Other Goods

	(1) Amuse-ment	(2) Hotels	(3) Legal Services	(4) Other Services	(5) Food	(6) Health Services
	Panel A: Southern States Only					
$\log(\text{Income})_{jt}$	0.903	0.840	1.615	1.364	0.023	-0.016
	(0.373)	(0.309)	(0.306)	(0.376)	(0.407)	(0.179)
	[0.029]	[0.016]	[0.000]	[0.002]	[0.956]	[0.929]
R^2	0.984	0.983	0.991	0.989	0.965	0.996
N	326	326	326	308	324	326
	Panel B: All U.S.					
$\log(\text{Income})_{jt}$	1.078	0.941	1.729	1.389	0.275	0.228
	(0.375)	(0.387)	(0.281)	(0.264)	(0.354)	(0.415)
	[0.006]	[0.019]	[0.000]	[0.000]	[0.441]	[0.585]
R^2	0.975	0.978	0.988	0.983	0.977	0.994
N	1013	1015	1015	989	1013	1015

Notes: Table reports results from estimating equation (4) by IV. Dependent variables are given in column headings. All dependent variables are in logs, and all dependent variables and income are divided by a hospital-utilization weighted measure of population (HUWP) before taking logs. In all columns income is measured by payroll. The sample is all Southern states between 1970 and 1990 in Panel A and all US states (except Alaska and Virginia) between 1970 and 1990 in Panel B. Unit of analysis is a state-year. Dependent variable is the Gross State Product for various industries, as indicated by column headings. Standard errors, adjusted to allow for an arbitrary variance-covariance matrix for each state over time, are in parentheses and p-values are in brackets.

Table 8: Decomposition and Tests for Nonlinear Effects

	(1)	(2)	(3)	(4)
	IV	IV	OLS	OLS
Geographic level of analysis	ESR	ESR	ESR	ESR
Dependent variable	Total Hospital Expenditures	Total Hospital Expenditures	Total Hospital Expenditures	Total Hospital Expenditures
Oil Reserves$_j$ ×			6.673	10.598
log(oil price)$_{t-1}$			(2.166)	(7.753)
			[0.008]	[0.192]
log(Income)$_{jt}$	0.710	0.833		
	(0.213)	(0.421)		
	[0.005]	[0.068]		
log(Income)$_{jt}$ ×		-0.070		
log(Income)$_{j,t=1970}$		(0.165)		
		[0.680]		
{ Oil Reserves$_j$ ×				-492.563
log(oil price)$_{t-1}$ }^{2}				(737.899)
				[0.515]
R^2	0.897	0.895	0.903	0.903
N	2054	2054	2065	2065
-1 standard deviation	0.710	0.864	6.673	11.899
Marginal Effect at Mean	0.710	0.833	6.673	10.598
+1 standard deviation	0.710	0.801	6.673	9.297

Notes: Table reports IV estimates of variants of equation (4) in columns 1 and 2 and OLS estimates of a variant of equation (6) in columns 4 and 5. In all columns dependent variable is hospital expenditures divided by a hospital-utilization weighted measure of population (HUWP) before taking logs; income (measured by payroll) is also divided by HUWP before taking logs. The unit of anlaysis is an Economic Sub Region (ESR)-year and the regressions include ESR fixed effects and year fixed effects. The sample is all Southern states between 1970 and 1990. Note that the results in columns 1 and 3 differ slightly from baseline resutls in Table 3 because the sample does not include Washington, DC (DC is dropped because there is no data for DC in the 1970s). Standard errors, adjusted to allow for an arbitrary variance-covariance matrix for each state over time, are in parentheses and p-values are in brackets.

Table 9: Heterogeneity Across Geography and Time

Panel A: First Stage

Dependent Variable: Income

	(1)	(2)	(3)	(4)	(5)
Oil Reserves$_j$ ×	9.436	6.260	7.305	1.633	7.861
log(oil price)$_{t-1}$	(2.293)	(1.635)	(2.582)	(1.896)	(2.012)
	[0.001]	[0.009]	[0.007]	[0.403]	[0.001]
R^2	0.983	0.984	0.982	0.983	0.986
N	2065	1070	4915	3547	3547
F-statistic	16.93	14.67	8.00	0.74	15.26
% increase in income given the rise in oil prices from 1970 to 1980 for the following differential in Oil Reserves					
1 standard deviation	3.64%	3.20%	2.47%	0.63%	3.03%

Panel B: IV

Dependent Variable: Hospital Expenditures

	(1)	(2)	(3)	(4)	(5)
log(Income)$_{jt}$	0.707	0.700	0.799	*N/A*	0.794
	(0.211)	(0.367)	(0.621)		(0.434)
	[0.004]	[0.105]	[0.205]		[0.088]
R^2	0.900	0.967	0.921		0.941
N	2065	1070	4915		3547

Specification

	(1)	(2)	(3)	(4)	(5)
Years	1970-1990	1970-1990	1970-1990	1970-2005	1970-2005
Geographic level of analysis	ESR	ESR	ESR	ESR	ESR
Geographic sample	South	Southern States w/ Large Oil Wells	All US	South	South
State-specific time trends	N	N	N	N	Y

Notes: Table reports results from estimating variants of equation 5 (Panel A) and equation (4) (Panel B). All dependent variables are in logs and divided by a hospital-utilization weighted measure of population (HUWP). In all columns income is measured by payroll divided by HUWP. Bottom rows define the specification variants. The baseline sample is all Southern states between 1970 and 1990. Column (1) reproduces baseline results from column (7) in Table 2 and column (3) in Table 3. Unit of analysis is an Economic Sub Region (ESR)-year in all columns except column (4) where it is State Economic Area (SEA)-year. All columns include ESR fixed effects and year fixed effects except for column (4) which includes SEA fixed effects and year fixed effects. Standard errors, adjusted to allow for an arbitrary variance-covariance matrix for each state over time, are in parentheses and p-values are in brackets. Because there is no statistically significant first stage in column (5), the IV results are not reported.

Table 10: Short-run versus Long-run Effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent variable	Income Baseline FS OLS	Hospital Expenditures Baseline RF OLS	Hospital Expenditures Baseline IV	Income 10-year FS OLS	Hospital Expenditures 10-year RF OLS	Hospital Expenditures 10-year IV	Hospital Expenditures 3-year avg. IV
Oil Reserves$_j$ ×	9.436	6.673		11.854	9.941		
log(oil price)$_{t-1}$	(2.293)	(2.166)		(4.283)	(3.056)		
	[0.001]	[0.008]		[0.014]	[0.005]		
log(Income)$_{jt}$			0.707			0.839	0.828
			(0.211)			(0.258)	(0.227)
			[0.004]			[0.005]	[0.002]
R^2	0.983	0.903	0.900	0.668	0.756	0.857	0.976
N	2065	2065	2065	296	296	296	690

Notes: Table reports results of estimating equations (4), (5) or (6) by OLS or IV as indicated. All dependent variables are in logs. In all columns income is measured by payroll and both hospital expendiutres and income are divided by a hospital-utilization weighted measure of population (HUWP) before taking logs. Columns 1 through 3 are the baseline sample of all Southern states between 1970 and 1990; in columns (4) through (6), only observations from 1970, 1980, and 1990 are included. Unit of analysis is an Economic Sub Region (ESR)-Year, and all columns include ESR fixed effects and year fixed effects. Column (7) uses 3-year averages of all variables (see Appendix B for more details). Standard errors, adjusted to allow for an arbitrary variance-covariance matrix for each state over time, are in parentheses and p-values are in brackets.

Table 11: Robustness of Identifying Assumption

	(1) Baseline	(2) 5-Year Lead	(3) Horse Race	(4) Region Trends	(5) State Trends	(6) 1970-1984 Subsample	(7) Falsification Test
Panel A: First Stage Results							
Dependent Variable: Income							
Oil Reserves$_j$ × log(oil price)$_{t-1}$	9.436	8.376	8.754	11.940	13.860	14.415	
	(2.293)	(2.146)	(2.331)	(3.124)	(4.059)	(3.536)	
	[0.001]	[0.001]	[0.002]	[0.002]	[0.004]	[0.001]	
Oil Reserves$_j$ × log(oil price)$_{t+5}$		4.822					
		(3.345)					
		[0.170]					
R^2	0.983	0.983	0.984	0.983	0.985	0.986	
N	2065	2065	2054	2065	2065	1471	
Panel B: Reduced Form and IV Results							
Dependent Variable: Hospital Expenditures							
	IV	IV	IV	IV	IV	RF	RF
log(Income)$_{jt}$	0.707	1.087	0.708	0.328	0.100		
	(0.211)	(0.303)	(0.265)	(0.183)	(0.108)		
	[0.004]	[0.003]	[0.018]	[0.093]	[0.369]		
Oil Reserves$_j$ × log(oil price)$_{t-1}$						4.641	-3.107
						(1.783)	(4.044)
						[0.020]	[0.455]
Oil Reserves$_j$ × log(oil price)$_{t+5}$		-16.286					
		(8.075)					
		[0.062]					
R^2	0.900	0.895	0.899	0.903	0.905	0.851	0.980
N	2065	2065	2054	2065	2065	1471	1487

Notes: Table reports results from estimating variants of equation (5) by OLS (Panel A) and equation (4) by IV (Panel B), except in columns 6 and 7 which show variants of equation (6) estimated by OLS in Panel B. All dependent variables are in logs. In all columns income is measured by payroll and both hospital expenditures and income are divided by a hospital-utilization weighted measure of population (HUWP) before taking logs. Unit of observation is an Economic Sub Region (ESR) - year and all columns include ESR and year fixed effects. In columns (1) through (5) the sample is all Southern states between 1970 and 1990. Column (1) reproduces baseline results from column (3) of Table 3. Column (2) includes a 5-year lead of the instrument as a control variable. Column (3) includes several additional interaction terms as control variables in a "horse race" (the interaction terms are the log oil price interacted with each of the following: hospital expenditures in 1969, hospital beds in 1969, population in 1970, wage bill in 1970, employment in 1970). Columns (4) adds region-specific linear time trends for the three Census regions in the South. Column (5) includes state-specific linear time trends for the 16 Southern states. Column (6) produces the first stage and reduced form results for 1970 to 1984 as comparison to the falsification test in column (7), which "grafts" the same oil price series in 1970 to 1984 onto the hospital data in 1955 to 1969. Standard errors, adjusted to allow for an arbitrary variance-covariance matrix for each state over time, are in parentheses and p-values are in brackets.

Table 12: Alternative Specifications of Instrument

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: First Stage						
Dependent Variable: Income						
Oil Reserves$_j$ ×	9.436					
log(oil price)$_{t-1}$	(2.293)					
	[0.001]					
Oil Reserves$_j$ ×		0.902				
oil price$_{t-1}$		(0.206)				
		[0.001]				
Oil Reserves$_j$ ×			10.284			
log(oil price)$_t$			(2.567)			
			[0.001]			
max(Oil Reserves,				12.935		
95th percentile) ×				(2.599)		
log(oil price)$_{t-1}$				[0.000]		
1{Oil Reserves > 0} ×					0.043	
log(oil price)$_{t-1}$					(0.015)	
					[0.011]	
1{Oil Reserves > 0} ×						0.821
Mining share of labor force in 1970 ×						(0.237)
log(oil price)$_{t-1}$						[0.004]
R^2	0.983	0.983	0.983	0.983	0.983	0.983
N	2065	2065	2065	2065	2065	2065
F-statistic	16.93	19.11	16.04	24.77	8.44	11.96
% Change given the rise in oil prices from 1970 to 1980 for following differential in Oil Reserves						
1 standard dev. difference	3.64%	4.91%	3.97%	4.15%	4.74%	3.95%
Panel B: IV						
Dependent Variable: Hospital Expenditures						
	(1)	(2)	(3)	(4)	(5)	(6)
log(Income)$_{jt}$	0.707	0.529	0.564	0.619	1.061	0.839
	(0.211)	(0.146)	(0.203)	(0.203)	(0.639)	(0.851)
	[0.004]	[0.002]	[0.014]	[0.008]	[0.117]	[0.340]
R^2	0.900	0.901	0.901	0.901	0.895	0.898
N	2065	2065	2065	2065	2065	2065

Notes: Table reports results from estimating variants of equation (5) (Panel A) by OLS and equation (4) (Panel B) by IV. The specifications vary in their definition of the instrument, which is given in the left hand column of Panel A. All dependent variables are in logs. Unit of observation is an ESR-year. In all columns income is measured by payroll and both hospital expenditures and income are divided by a hospital-utilization weighted measure of population (HUWP) before taking logs. The sample is ESRs in Southern states between 1970 and 1990. Column (1) reproduces baseline results (see column (7) of Table 2 and column (3) of Table 3). **1**(Oil Reserves > 0) is an indicator variable for whether the ESR has any large oil wells. Unit of analysis is an Economic Sub Region (ESR)-year, and all columns include fixed effects and year fixed effects. Standard errors, adjusted to allow for an arbitrary variance-covariance matrix for each state over time, are in parentheses and p-values are in brackets.

Table 13: Bootstrap Results

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable	Income FS	Hosp Exp IV	Income FS	Hosp Exp IV	Hosp Exp IV	Hosp Exp IV
Standard Errors	Cluster at State Level	Cluster at State Level	Cluster at ESR Level	Cluster at ESR Level	Wild bootstrap at State Level	Wild bootstrap at ESR Level
Oil Reserves$_j$ × log(oil price)$_{t-1}$	9.436		9.436			
	(2.293)		(4.147)			
	[0.001]		[0.025]			
log(Income)$_{jt}$		0.707		0.707	0.707	0.707
		(0.211)		(0.369)		
		[0.004]		[0.058]	[0.0016]	[0.0492]
R^2	0.983	0.900	0.983	0.900	0.900	0.900
N	2065	2065	2065	2065	2065	2065
F-statistic	16.93		5.18			

Notes: Table reports results from estimating equation (5) by OLS or equation (4) by IV as indicated. The sample is all Southern states between 1970 and 1990. Unit of observation is an Economic Sub Region (ESR)-year. All columns include Economic Sub Region (ESR) fixed effects and year fixed effects. All dependent variables in logs. In all columns, Income is measured by payroll and is divided by a hospital-utilization weighted measure of population (HUWP) before taking logs. The dependent variable is also always adjusted by HUWP. Columns (1) and (2) reproduce baseline results from column (7) in Table 2 and column (3) in Table 3. Columns (3) and (4) report results clustering at the ESR level instead of the state level. Column (5) reports wild bootstrap results sampling states with replacement, and column (6) reports wild bootstrap results sampling ESRs with replacement. All bootstrap results based on 10,000 iterations. Standard errors are in parentheses; bootstrapped standard errors are not reported in (5) and (6) because inference is done on bootstrapped t-statistics. P-values are shown in square brackets; p-values in columns (1) through (4) are based on asymptotic t-tests, while p-values in columns (5) and (6) are bootstrapped p-values based on simulated t-statistics.

Table 14: IV-GLS and Lagged Depedendent Variable

	Dependent Variable: Hospital Expenditures					
	(1)	(2)	(3)	(4)	(5)	(6)
	Baseline IV	IV-GLS	IV-GLS	IV-GLS	IV-GLS	Lagged Dep. Var. IV
Within-panel serial correlation	Cluster at State	Common AR(1)	Common AR(2)	State-specific AR(1)	ESR-specific AR(1)	Cluster at State
$\log(\text{Income})_{jt}$	0.682	0.681	0.606	0.448	0.340	0.717
	(0.214)	(0.241)	(0.238)	(0.198)	(0.190)	(0.185)
	[0.007]	[0.005]	[0.011]	[0.024]	[0.075]	[0.001]
$\log(\text{Total Hosp. Exp.})_{t-1}$						0.044
						(0.094)
						[0.646]
N	2016	2016	2016	2016	2016	1966

Notes: Table reports results from estimating variants of equation (4) by IV. The sample is all Southern states between 1970 and 1990. Unit of observation is an Economic Sub Region (ESR)-year. All specifications are at the Economic Sub Region (ESR)-year level and include ESR fixed effects and year fixed effects. In all columns, Income is measured by payroll divided by a hospital-utilization weighted measure of population (HUWP) before taking logs. The dependent variable is always log hospital expenditures adjusted by HUWP. Baseline sample is modified to only include the 96 (of 99) ESRs with data for all 21 years between 1970 and 1990 for columns (1) through (5). Column (1) produces baseline IV results with this modified sample. Columns (2) through (5) report IV-GLS results. Column (6) include a lagged dependent variable as a control.

Appendix Table A1: Augmented Dickey-Fuller Tests

	Dependent Variable: log(oil price)$_t$ - log(oil price)$_{t-1}$							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
log(oil price)$_{t-1}$	0.034	0.005	0.014	0.010	-0.090	-0.156	-0.151	-0.175
	(0.054)	(0.057)	(0.060)	(0.063)	(0.089)	(0.093)	(0.101)	(0.107)
	[0.537]	[0.927]	[0.816]	[0.880]	[0.315]	[0.098]	[0.141]	[0.107]
log(oil price)$_{t-1}$ - log(oil price)$_{t-2}$		0.249	0.254	0.264		0.318	0.319	0.351
		(0.158)	(0.160)	(0.167)		(0.156)	(0.159)	(0.166)
		[0.120]	[0.119]	[0.121]		[0.046]	[0.050]	[0.041]
log(oil price)$_{t-2}$ - log(oil price)$_{t-3}$			-0.121	-0.123			-0.038	-0.034
			(0.166)	(0.170)			(0.167)	(0.169)
			[0.469]	[0.474]			[0.819]	[0.840]
log(oil price)$_{t-3}$ - log(oil price)$_{t-4}$				0.047				0.125
				(0.172)				(0.170)
				[0.786]				[0.467]
t					0.111	0.142	0.141	0.157
					(0.064)	(0.065)	(0.070)	(0.075)
					[0.088]	[0.035]	[0.050]	[0.040]
N	55	54	53	52	55	54	53	52
Dickey-Fuller test statistic	0.621	0.092	0.234	0.151	-1.014	-1.686	-1.498	-1.642
Approximate p-value	0.988	0.966	0.974	0.969	0.942	0.757	0.830	0.776

Notes: Table based on annual data on oil prices from 1950 to 2005. Standard errors are in parentheses and p-values are in brackets.

Appendix Table A2: Hospital Technologies

Hospital Technology	First Year	Last Year	Years of Data	Fraction Adopted
Emergency Department	1970	1990	21	0.998
Histopathology Services	1970	1990	21	0.964
Home care Program / Department	1970	1990	21	0.701
Hospital Auxiliary	1970	1990	21	0.993
Inhalation Therapy Department (Respiratory)	1970	1990	21	0.993
Occupational Therapy	1970	1990	21	0.852
Physical Therapy Department	1970	1990	21	0.993
Psychiatric Partial Hospitalization Program	1970	1990	21	0.727
X-Ray Therapy	1970	1990	21	0.873
Blood Bank	1970	1990	20	0.993
Open Heart Surgery Facilities	1970	1990	20	0.528
Psychiatric Emergency Services (Outpatient)	1970	1990	20	0.788
Psychiatric Emergency Services	1970	1990	19	0.887
Rehabilitation Outpatient Unit	1970	1990	19	0.764
Organized Outpatient Department	1970	1988	18	0.940
Social Work Department	1970	1989	17	0.966
Cardiac Intensive Care	1970	1985	16	0.970
Family Planning Service	1970	1985	16	0.630
Psychiatric Foster And/Or Home Care	1970	1986	16	0.393
Self Care Unit	1970	1985	16	0.503
Premature Nursery	1970	1985	15	0.943
Rehabilitation Inpatient Unit	1970	1985	15	0.592
Postoperative Recovery Room	1970	1982	13	0.993
Electroencephalography	1970	1981	12	0.921
Hemodialysis / Renal Dialysis (Impatient)	1970	1981	12	0.682
Hemodialysis / Renal Dialysis (Outpatient)	1970	1981	12	0.675
Organ Bank	1970	1981	12	0.337
Pharmacy with FT Registered Pharmacist	1970	1981	12	0.974
Pharmacy with PT Registered Pharmacist	1970	1981	12	0.942
Psychiatric Inpatient Unit	1970	1980	11	0.750
Intensive Care Unit (Mixed)	1970	1979	10	0.973
Cobalt and Radium Therapy	1970	1978	9	0.669
Radium Therapy	1970	1978	9	0.837
Cobalt Therapy	1970	1977	8	0.693
Extended Care Unit	1970	1974	5	0.810
Basic Emergency Department	1970	1970	1	0.975
Major Emergency Department	1970	1970	1	0.743
Provisional Emergency Unit	1970	1970	1	0.962
Radioisoptope Facility	1970	1970	1	0.852
Genetic Counseling Service	1971	1990	20	0.441
Radioisoptope Facility (Diagnostic)	1971	1990	20	0.967
Radioisoptope Facility (Therapeutic)	1971	1990	20	0.836
Volunteer Services Department	1971	1990	20	0.956
Psychiatric Consultation and Education	1971	1986	16	0.799
Burn Care	1971	1985	15	0.472
Speech Therapist Services / Pathology	1972	1990	19	0.877
Clinical Psychologist Services	1972	1986	15	0.847
Dental Services	1972	1985	14	0.968
Podiatrist Services	1972	1985	13	0.796

Chronic Obstructive Pulmonary Disease	1975	1990	16	0.783
Alcohol / Chemical Dependency (Outpatient)	1975	1990	15	0.742
Skilled Nursing or Long Term Care Unit	1975	1985	11	0.852
Alcohol / Chemical Dependency (Impatient)	1975	1985	10	0.723
Neonatal Intensive Care	1976	1985	10	0.743
Pediatric Unit (Impatient)	1977	1978	2	0.951
Patient Representative Services	1978	1990	13	0.958
Abortion Service (Impatient)	1978	1981	4	0.794
Abortion Service (Outpatient)	1978	1981	3	0.638
Radioactive Implants	1979	1990	12	0.811
Megavoltage Radiation Therapy	1979	1990	11	0.781
Computerized Tomography Scanner (Head or Body)	1979	1990	10	0.859
Pediatric Intensive Care	1979	1985	7	0.773
Cardiac Catheterization	1980	1990	11	0.722
Hospice	1980	1990	11	0.715
Recreational Therapy	1980	1990	11	0.869
Ultrasound Facility (Diagnostic)	1980	1990	11	0.976
Kidney Transplant	1980	1990	7	0.327
Organ Transplant (Other than Kidney)	1980	1990	7	0.377
Chaplaincy Services	1980	1985	6	0.987
Electrocardiography	1980	1985	6	1.000
Intermediate Care for Mentally Retarded	1980	1985	6	0.439
Intravenous Admixture Services	1980	1985	6	0.993
Medical/Surgical Acute Care	1980	1985	6	1.000
Medical/Surgical Intensive Care	1980	1985	6	0.998
Newborn Nursery	1980	1985	6	1.000
Obstetrical Care	1980	1985	6	1.000
Other Long-Term Care / Intermediate Care Facility	1980	1985	6	0.838
Pediatric Acute Care	1980	1985	6	1.000
Pharmacy Unit Dose System	1980	1985	6	0.990
Psychiatric Acute Care	1980	1985	6	0.953
Psychiatric Long Term Care	1980	1985	6	0.568
General Surgical Services	1980	1985	5	1.000
General Laboratory Services	1980	1985	4	1.000
Health Science Library	1980	1990	3	0.968
Psychiatric Intensive Care	1980	1982	3	0.679
Ambulance Services	1980	1981	2	0.930
Anesthesia Service	1980	1981	2	1.000
Autopsy Services	1980	1981	2	0.989
C.T. Scanner (Body Unit)	1980	1981	2	0.761
C.T. Scanner (Head Unit)	1980	1981	2	0.570
Cancer/Tumor	1980	1981	2	0.894
Electromyography	1980	1981	2	0.826
Hemodialysis (Home Care/ Mobile Unit)	1980	1981	2	0.464
NeuroSurgery	1980	1981	2	0.769
Physical Rehabilitation	1980	1982	2	0.856
Pulmonary Function Laboratory	1980	1981	2	0.987
Toxicology	1980	1981	2	0.983
Intravenous Therapy	1980	1980	1	0.886
Medical/Surgical Acute Care (Inpatient)	1980	1980	1	0.335
Rehabilitation	1980	1980	1	0.953
Residential Care	1980	1980	1	0.547

Residential Care (Inpatient)	1980	1980	1	0.280
Day Hospital	1981	1987	7	0.822
Pediatric Psychiatric Services	1981	1986	6	0.777
Health Promotion	1981	1985	5	0.964
Optometric Services	1981	1985	5	0.857
Other Special Care	1981	1985	5	0.877
Sheltered Care	1981	1985	5	0.419
Ambulator Surgical Services	1981	1981	1	1.000
Podiatrist Services (Inpatient)	1981	1981	1	0.873
Podiatrist Services (Outpatient)	1981	1981	1	0.835
Hemodialysis Services	1982	1990	9	0.850
Outpatient Surgery	1982	1990	8	1.000
Abortion Services	1982	1985	4	0.825
Pharmacy Services	1982	1985	4	1.000
Comprehensive Geriatric Assessment Services	1983	1990	8	0.805
Nuclear MRI Facility	1983	1990	8	0.542
Psychiatric Liaison Services	1983	1990	8	0.819
Trauma Center	1984	1990	7	0.751
Alcohol / Chemical Acute Care (Inpatient)	1984	1984	1	0.903
Alcohol / Chemical Subacute Care (Inpatient)	1984	1984	1	0.852
Birthing Room	1985	1990	6	0.970
Extracorporeal Shock-Wave Lithotripter	1985	1990	6	0.395
X-Ray (Diagnostic)	1985	1989	5	0.999
Unknown Technology	1985	1985	1	0.678
Adult Day Care	1986	1990	5	0.567
Community Health Promotion	1986	1990	5	0.984
Fertility Counseling	1986	1990	5	0.608
Fitness Center	1986	1990	5	0.746
Geriatric Acute-Care Unit	1986	1990	5	0.754
Occupational Health Services	1986	1990	5	0.869
Patient Education	1986	1990	5	0.992
Respite Care	1986	1990	5	0.803
Sports Medicine Clinic / Service	1986	1990	5	0.775
Sterilization	1986	1990	5	0.945
Women's Center	1986	1990	5	0.762
Worksite Health Promotion	1986	1990	5	0.959
Organ Transplant (Including Kidney)	1986	1989	4	0.467
AIDS Services	1986	1987	2	0.926
Continuing Care Case Management	1986	1987	2	0.773
Contraceptive Care	1986	1987	2	0.646
Genetic Counseling Screening	1986	1987	2	0.532
Satellite Geriatric Clinics	1986	1987	2	0.278
Child Adolescent Psychiatric Services	1987	1990	4	0.872
Geriatric Psychiatric Services	1987	1990	4	0.839
Psychiatric Education	1987	1990	4	0.887
AIDS (Outpatient)	1988	1990	3	0.414
AIDS General Inpatient Care	1988	1990	3	0.980
AIDS/ARC Unit	1988	1990	3	0.247
AIDS/HIV Testing	1988	1990	3	0.969
Alzheimer's Diagnostic Assessment Services	1988	1990	3	0.596
Emergency Response for Elderly	1988	1990	3	0.932
Geriatic Clinic	1988	1990	3	0.496

In Vitro Fertilization	1988	1990	3	0.379
Medicare Certified Distinct Part Skilled Nursing Unit	1988	1990	3	0.886
Organized Social Work Services	1988	1990	3	0.989
Other Skilled Nursing Care	1988	1990	3	0.891
Senior Membership Program	1988	1990	3	0.737
Angioplasty	1989	1990	2	0.708
Arthritis Treatment Center	1989	1990	2	0.485
Emergency Social Work Services	1989	1990	2	0.911
Freestanding Outpatient Center	1989	1990	2	0.686
Hospital Based Outpatient Care Center	1989	1990	2	0.998
Orthopedic Surgery	1989	1990	2	0.972
Outpatient Social Work Services	1989	1990	2	0.939
Bone Marrow Transplant	1990	1990	1	0.301
Cardiac Rehabilitation	1990	1990	1	0.924
Non-Invasive Cardiac Assessment	1990	1990	1	0.970
Positron Emission Tomography Scanner	1990	1990	1	0.267
Single Photo Emission Computed Tomography	1990	1990	1	0.754
Stereotactic Radiosurgery	1990	1990	1	0.415
Tissue Transplant	1990	1990	1	0.432

Notes: This table lists the 172 unique technologies from the AHA annual surveys between 1970 and 1990. For each technology, this table reports the first year the technology appears, the last year the technology appears, and the fraction of Economic Sub Region (ESR)-year observations that contain at least one hospital that has adopted the technology.

Appendix Table A3: Results Leaving Out Each State in Census South

Panel A: First Stage

Dependent Variable: Income

	All South	Drop AL	Drop AR	Drop DE	Drop DC	Drop FL	Drop GA	Drop KY	Drop LA	Drop MD	Drop MS	Drop NC	Drop OK	Drop SC	Drop TN	Drop TX	Drop WV
Oil Reserves$_j$ ×	9.436	9.535	9.588	9.361	9.392	9.433	9.517	10.074	9.156	9.158	9.565	8.824	9.111	9.230	9.362	21.773	8.442
log(oil price)$_{t-1}$	(2.293)	(2.466)	(2.389)	(2.298)	(2.292)	(2.366)	(2.439)	(2.450)	(2.060)	(2.288)	(2.384)	(2.193)	(1.779)	(2.344)	(2.397)	(4.785)	(1.918)
	[0.001]	[0.002]	[0.001]	[0.001]	[0.001]	[0.001]	[0.002]	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]	[0.000]	[0.001]	[0.002]	[0.000]	[0.001]
R^2	0.983	0.983	0.983	0.983	0.983	0.983	0.984	0.983	0.984	0.983	0.983	0.983	0.982	0.983	0.982	0.984	0.984
N	2065	1877	1918	2044	2054	2002	1900	1897	1939	1981	1939	1918	1897	1939	1918	1813	1939
F-statistic	16.93	14.95	16.11	16.60	16.79	15.89	15.22	16.90	19.75	16.02	16.09	16.19	26.23	15.50	15.26	20.71	19.37
% Change given the rise in oil prices from 1970 to 1980 for following differential in Oil Reserves																	
1 std. dev. difference	3.60%	3.80%	3.80%	3.60%	3.60%	3.70%	3.80%	4.00%	3.60%	3.60%	3.80%	3.50%	3.60%	3.70%	3.70%	3.00%	3.30%

Panel B: IV

Dependent Variable: Total Hospital Expenditures

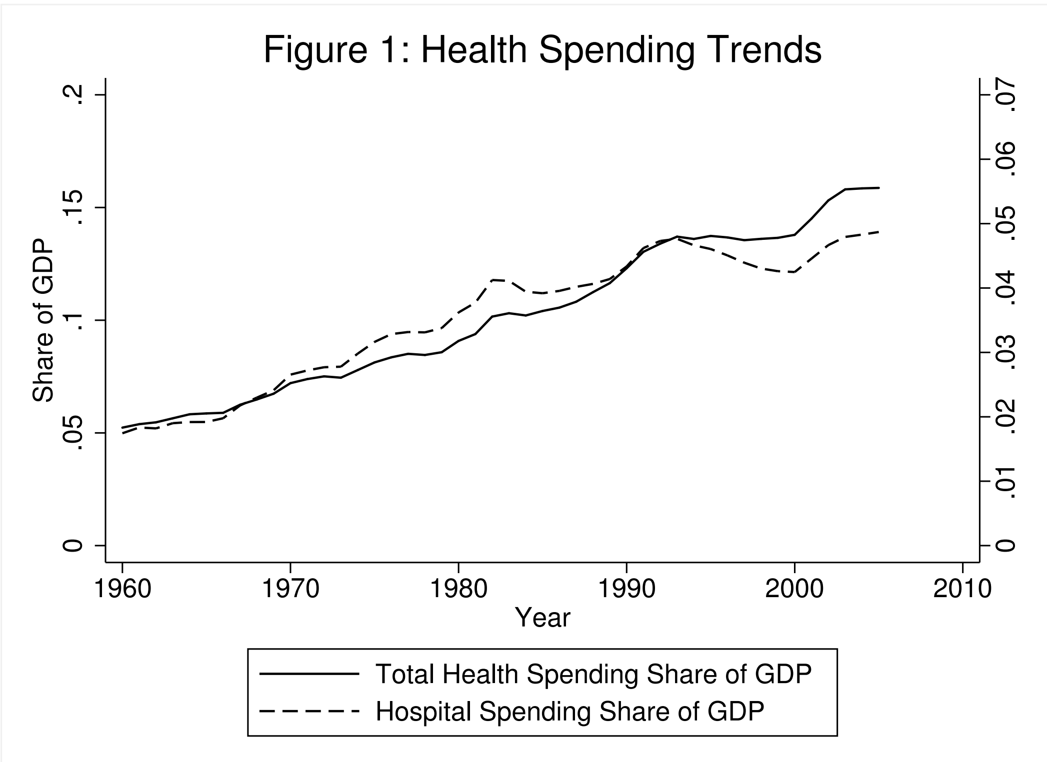
	All South	Drop AL	Drop AR	Drop DE	Drop FL	Drop FL	Drop GA	Drop KY	Drop LA	Drop MD	Drop MS	Drop NC	Drop OK	Drop SC	Drop TN	Drop TX	Drop WV
log(Income)$_{jt}$	0.707	0.686	0.709	0.679	0.710	0.679	0.818	0.719	0.690	0.638	0.765	0.658	0.806	0.745	0.663	0.437	0.731
	(0.211)	(0.222)	(0.213)	(0.214)	(0.213)	(0.216)	(0.181)	(0.209)	(0.269)	(0.212)	(0.210)	(0.232)	(0.178)	(0.220)	(0.219)	(0.701)	(0.244)
	[0.004]	[0.008]	[0.005]	[0.007]	[0.005]	[0.007]	[0.000]	[0.004]	[0.023]	[0.009]	[0.003]	[0.013]	[0.000]	[0.004]	[0.009]	[0.543]	[0.010]
R^2	0.900	0.895	0.894	0.900	0.897	0.897	0.893	0.968	0.895	0.899	0.893	0.896	0.895	0.895	0.894	0.898	0.896
N	2065	1877	1918	2044	2054	2002	1900	1897	1939	1981	1939	1918	1897	1939	1918	1813	1939

Notes: Table reports estimates of variants of estimating equation (5) by OLS in Panel A and equation (4) by IV in Panel B. In all specifications income (measured by Payroll) and hospital expenditures are divided by hospital-utilization weighted measure of population (HUWP) and then logged. First column shows results from our baseline sample of all Southern States from 1970 - 1990. Subsequent columns show the results when the state specified in the column heading is ommitted from the analysis. Unit of observation is an Economic Sub Region (ESR)-year; all regressions include ESR and year fixed effects. Standard errors, adjusted to allow for an arbitrary variance-covariance matrix for each state over time, are in parentheses and p-values are in brackets.

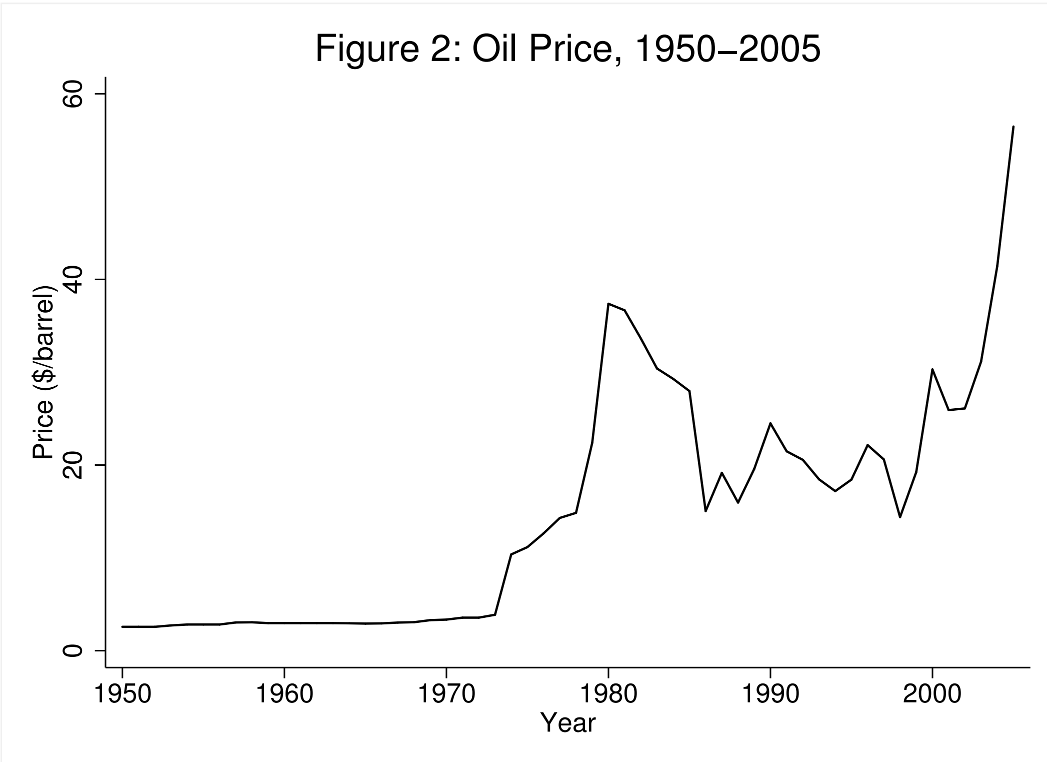
Appendix Table A4: Monte Carlo Simulation Results

	FE-IV			FD-IV			FE-IV-LAG			FE-IV-3YR			FD-IV-3YR		
	ρ=0.1	ρ=0.3	ρ=0.9	ρ=0.1	ρ=0.3	ρ=0.9	ρ=0.1	ρ=0.3	ρ=0.9	ρ=0.1	ρ=0.3	ρ=0.9	ρ=0.1	ρ=0.3	ρ=0.9
$y_{jt} = x_{jt} + a_{jt} + \delta_j + \varepsilon_{jt}$	1.008	1.009	1.012	1.010	1.009	1.009	-0.041	-0.038	-0.033	1.020	1.026	1.032	1.029	1.032	1.034
	(0.094)	(0.097)	(0.127)	(0.111)	(0.102)	(0.095)	(0.111)	(0.117)	(0.141)	(0.181)	(0.220)	(0.352)	(0.228)	(0.243)	(0.245)
$y_{jt} = x_{j,t-1} + a_{jt} + \delta_j + \varepsilon_{jt}$	-0.033	-0.031	-0.029	-0.490	-0.491	-0.491	0.993	0.996	1.001	0.626	0.632	0.638	0.496	0.499	0.501
	(0.138)	(0.143)	(0.158)	(0.127)	(0.120)	(0.115)	(0.089)	(0.095)	(0.124)	(0.228)	(0.260)	(0.378)	(0.291)	(0.310)	(0.324)
$y_{jt} = x_{j,t} + a_{jt} + \delta_j + \varepsilon_{jt}$	0.482	0.484	0.486	0.253	0.253	0.253	0.479	0.482	0.486	0.810	0.816	0.821	0.743	0.746	0.748
or $x_{j,t-1} + a_{jt} + \delta_j + \varepsilon_{jt}$	(0.135)	(0.139)	(0.159)	(0.155)	(0.149)	(0.146)	(0.146)	(0.148)	(0.166)	(0.229)	(0.258)	(0.371)	(0.294)	(0.312)	(0.322)
$y_{jt} = x_{jt} + a_{jt} + \delta_j + \varepsilon_{jt}$	0.307	0.309	0.311	0.161	0.161	0.160	0.309	0.311	0.316	0.626	0.632	0.637	0.480	0.483	0.486
or $x_{j,t-1} + a_{jt} + \delta_j + \varepsilon_{jt}$	(0.139)	(0.144)	(0.163)	(0.153)	(0.149)	(0.146)	(0.147)	(0.151)	(0.168)	(0.257)	(0.284)	(0.388)	(0.313)	(0.330)	(0.340)
or $x_{j,t-2} + a_{jt} + \delta_j + \varepsilon_{jt}$															

Notes: Results from Monte Carlo simulation described in Appendix B. Mean of parameter estimates over 500 simulations are reported; standard deviation of parameter estimates are in parentheses.

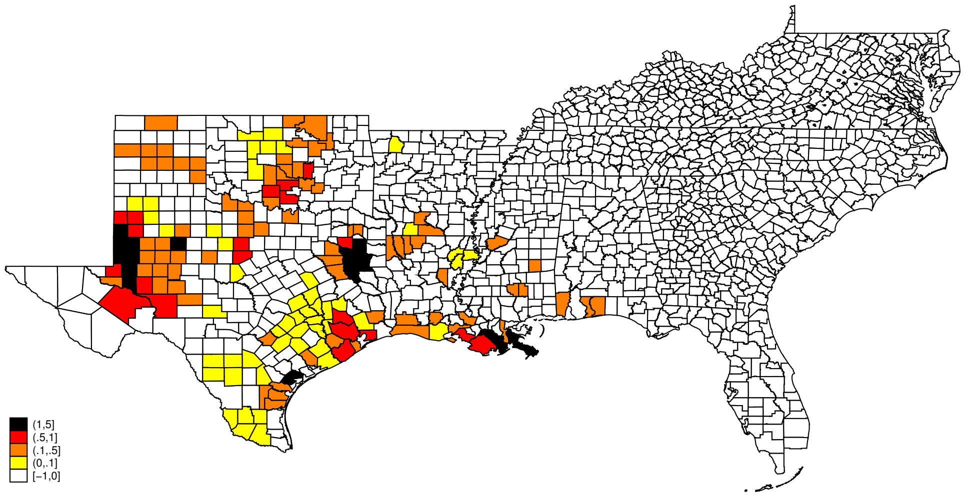


Note: This graph displays the trends in hospital spending from 1960 until 2005. Source: CMS (2006).



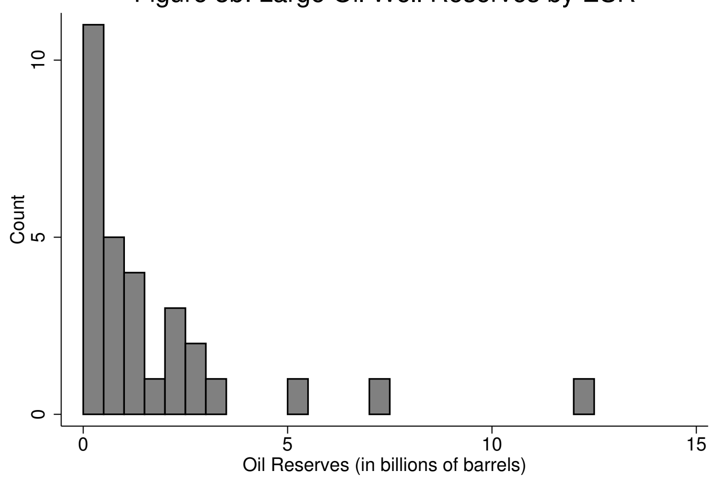
<u>Note:</u> This graph displays the annual average oil price, calculated from the monthly spot prices in the West Texas Intermediate series. The data are available here: `http://research.stlouisfed.org/fred2/series/OILPRICE/downloaddata?cid=98`.

Figure 3a: Large Oil Well Reserves by County



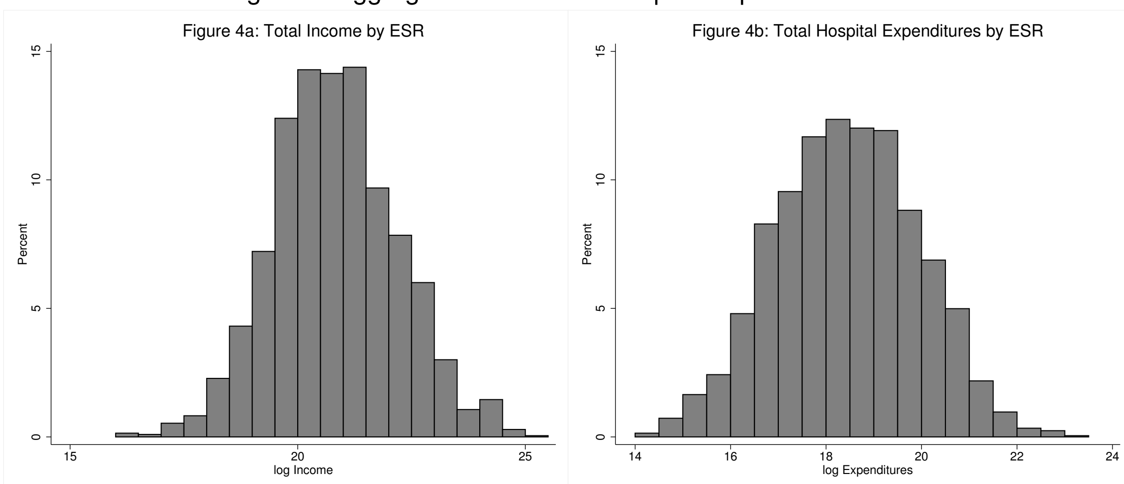
Note: This map displays the total amount of oil in large oil wells for each county in the South. Large oil wells are defined as having ever had more than 100 million barrels of oil. The data come from the 2000 Edition of the Oil and Gas Data Book.

Figure 3b: Large Oil Well Reserves by ESR

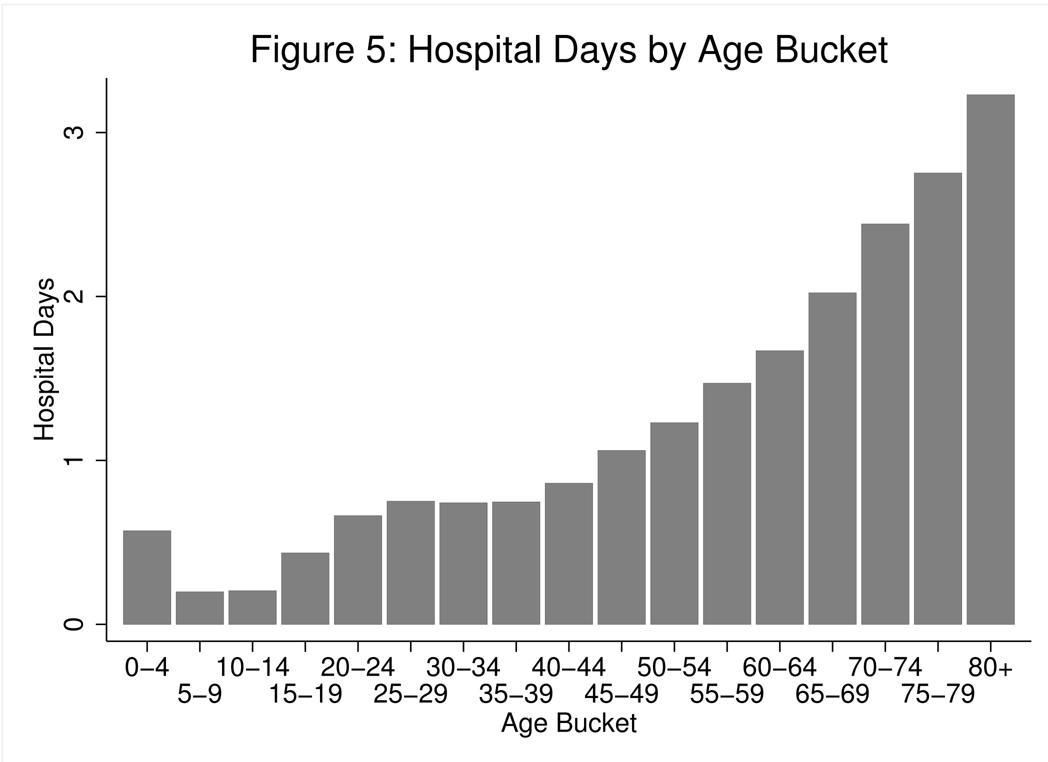


Note: This figure displays the cross-sectional distribution of oil reserves by Economic Sub Region (ESR) among the ESRs containing large wells. Of the 99 ESRs in the South, 69 ESRs do not have any large oil wells. This figure shows the amount of oil reserves (in billions of barrels) for the 30 ESRs with large oil wells. The data come from the 2000 Edition of the Oil and Gas Data Book.

Figure 4: Aggregate Income and Hospital Expenditure Data



Note: This figure contains histograms of the total income and total hospital expenditures by Economic Sub Region (ESR). Income is measured using the payroll data from the County Business Patterns (CBP), and the total hospital expenditures come from the American Hospital Association (AHA) Annual Surveys. Both variables are displayed in logs. The data displayed are for ESRs in the South for the years 1970 to 1990.



Note: This chart displays the average annual number of hospital days for various age buckets. The data come from the National Health Interview Survey (NHIS) for years 1973 to 1991.