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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. We also present evidence suggesting that there are unlikely to be substantial national or global general equilibrium effects of rising income on health spending, for example through 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".[1] 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 Hall-Jones view also receives indirect 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 makes the secular rise in incomes a natural candidate to explain the rise in the health share of GDP in the United States.

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 enables a proper accounting of the notable growth in the US (and OECD) health care sector over the last half century. Second, it is necessary for forecasting how health care spending is likely to evolve in coming years. Finally, it is a crucial first step towards an assessment of the optimality of the growth of the health care sector. In particular, if health spending 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.[2]

[1] Throughout we use the term "luxury good" to designate an empirical income elasticity greater than one (and similarly "necessity" refers to an elasticity less than one). This responsiveness to income may result from preferences, policy or other factors.

[2] Of course a large role for income would only be suggestive, not dispositive. A systematic analysis of social optimality would also have to consider potential externalities in health provision and in health R&D, as well as informational and institutional constraints in the health care market.

The relationship between income and health spending is the subject of a voluminous empirical literature. Remarkably, however, virtually all existing estimates are based on 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.[3] 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 is that income and health co-vary at the individual or regional level for a variety of reasons. Therefore, simple correlations are unlikely to reveal the causal effect of income on health spending.

A second challenge is that an investigation of the role that rising income plays in the growth of the health care sector requires incorporating the general equilibrium effects of income on health spending. Partial and general equilibrium income elasticities may differ for a variety of reasons. For example, the general equilibrium effect of rising income may be larger than the partial equilibrium effect if an increase in the demand for health care from a community (a "general equilibrium change") prompts changes in medical practices, including the adoption (and possibly development) of new technologies.[4] Alternatively, if the supply of health care is less than perfectly elastic and the price elasticity of demand for health care is greater than one, the responsiveness of health care expenditures to an increase in income may be lower in general equilibrium than in partial equilibrium. In addition, changes in income may also affect health care policy through a variety of political economy channels, either magnifying or curtailing the direct effect of income on health expenditures. Many of the potential general equilibrium effects are "local" in the sense that they result from changes in incomes in a particular region or local economy. These effects can be detected by looking at the response of health spending to income in the local economy. In addition, there may also exist national or even global general equilibrium effects, which will be harder to detect empirically.

[3] 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 analyses of the relationship between income growth and growth in health spending for individual countries (e.g., Fogel 1999).

[4] Finkelstein (2007), for example, argues that, for such reasons, the general equilibrium effect of health insurance coverage on health spending is larger than the partial equilibrium effect.

We confront both of these challenges. By exploiting potentially exogenous variation in local area incomes, we attempt to estimate causal elasticities that incorporate local general equilibrium effects. On the basis of our estimates and additional evidence, we also argue below that national or 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. In our baseline specification 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 health expenditures 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.[5]

Our baseline estimate is a statistically significant elasticity of ESR hospital spending with respect to ESR income of 0.72 (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 0.5 percentage points of the 11 percentage point increase in the health share of US GDP between 1960 and 2005. Point estimates of the income elasticity from a wide range of alternative specifications fall on both sides of our baseline estimate, but are almost always less than 1.

We note at the outset (and explore in greater depth in the paper) two potentially important caveats to our conclusions. The first caveat is that 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

[5] We also present results at the state rather than ESR level. This reduces our cross-sectional variation in oil intensity but alllows us to capture general equilibrium effects at a higher level of geography than the ESR. The results are similar.

spending, and the time-series evidence in Figure 1 suggests that hospital and non-hospital 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) 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. We also draw on additional data sources to provide suggestive empirical evidence that the income elasticities of hospital expenditures and overall health expenditures are similar. This evidence bolsters our belief that our elasticity estimates for hospital spending are likely to be representative of those for total health expenditures.

A second potentially important caveat is that our strategy estimates local general equilibrium effects, but will not capture any global or national general equilibrium effects. Of particular concern is that if the growth of the health care market resulting from the rise in 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 induced innovation 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 to have an income elasticity less than one, induced innovations should relatively favor the non-health sectors that have an income elasticity above one.[6]

Relatedly, our estimates also leave out any general equilibrium effects of rising income operating through a national policy response. Nevertheless, as we discuss in more detail below, since a significant portion of health care policy is determined at the sub-national level, much of the impact of rising income on health care policy should be incorporated in our estimates. We also present some suggestive evidence that any national policy responses not captured by our analysis are likely to be quantitatively small.

[6]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.

A final point that warrants emphasis at the outset is that our empirical strategy estimates the effect of rising incomes on health care spending in the recent US context. This empirical relationship is undoubtedly partly shaped by several specific institutional features of the US health care system. Our evidence does not therefore directly address the question of whether health care is a "luxury good" in households' utility function as hypothesized by Hall and Jones (2007).

To our knowledge, our paper represents the first empirical attempt to estimate the causal general equilibrium income elasticity of health spending.[7] Indeed, we are only aware of two prior studies that attempt to estimate the "causal effect" of income on health spending; both estimate the partial equilibrium effect of income on own 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 the elderly's prescription drug use; they estimate an elasticity of drug use with respect to income of above one. The Rand Health Insurance Experiment finds that a small, unanticipated, temporary increase in own income has no significant impact on own health expenditures or utilization (Newhouse et al., 1993, p. 78).[8]

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 their components. Section 4 discusses the implications of our elasticity estimate for the role of rising income in explaining the rise in the health share of GDP in the United States; it also discusses in some depth some of the most salient potential threats to extrapolating from our estimates in this manner. 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.

[7] 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 US 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. Davis and Haltiwanger (2001) investigate the impact of changes in oil prices on sectoral job creation and job destruction. Kilian (2008) provides a review of the literature on the economic effects of energy price shocks on a variety of different sectors and macroeconomic aggregates. None of these papers study the effect of income on the health care sector.

[8] These results are from the so-called Super Participation Incentive in which a sub-sample of families were given an unanticipated, small (a maximum of $250 in the mid 1970s) additional lump sum payment for one year in the penultimate year of the experiment. Note that this sub-experiment was not designed to estimate the income elasticity of demand for health care but rather to test 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.

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

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

where h_{jt} is health care 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 and h_{jt} is measured by hospital expenditures. The α_js are area fixed effects measuring any time-invariant differences across the different geographic areas. The γ_ts are year fixed effects, capturing 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.[9]

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 (unobserved) health and healthier areas have lower health care expenditures, the OLS estimates would be biased downwards. If, on the other hand, income is positively correlated with insurance coverage and insurance encourages increased health care spending, OLS estimates would be biased upwards.

Our empirical strategy attempts to isolate potentially-exogenous sources of variation in local area income, y_{jt}. 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:

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

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

[9] The specification with the dependent variable, hospital expenditures, in logs rather than in levels is attractive both because the distribution of hospital expenditures across areas is highly right skewed (see Figure 4b below) and because it implies that year fixed effects correspond to constant proportional (rather than constant level) changes in health spending across all areas.

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 at 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 when we instead use oil prices at time t. The year fixed effects in both the first and second stage will capture any common (proportional) effects of oil price changes on area income and health care expenditures that are independent of the role of oil in the local economy, which may be operating, for example, through the effects of oil prices on costs of living or production.

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 may differ in ways that could affect health expenditures. Any such differences that are time-invariant will be captured by the area fixed effects (the α_js and α'_js) in equations (1) and (2). Only differential trends in 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). We show in the next subsection that areas of the Southern United States that differ in terms of the role of oil in the local economy (I_j in (1) and (2)) have similar levels of income and hospital expenditures at the start of our sample period (when oil prices had been relatively constant for at least 20 years). More importantly, in Section 5, we provide a variety of evidence to support our identifying assumption that there were no major differential trends in health expenditures across local economies correlated with their oil intensity.

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 our 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).[10] There are 247 ESRs in the United States overall, and 99 in our sample of 16 Southern states.[11] We discuss below the results of

[10] ESRs frequently cross state boundaries. In contrast, SEAs do not cross state boundaries and are defined on the basis of a combination of demographic, economic, agricultural, topographic and natural resource considerations. In metropolitan areas, SEAs are based on standard metropolitan areas (SMSAs); for SMSAs that straddle two or more states, each part becomes a separate SEA.

[11] Our baseline sample is 2065 observations instead of $99\times21=2079$ observations because of four ESR-years of missing hospital data and because Washington D.C. does not appear in the hospital data until 1980. Restricting

analyses at different levels of aggregation (in particular, state) 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 data on health expenditures in each area. We briefly describe the construction of our main data series here. Table 1a provides summary statistics on some of our main variables.

Oil prices We measure oil prices by the average annual spot oil price from the West Texas Intermediate series.[12] Figure 2 shows the time series of average annual spot oil prices from 1950 to 2005. We focus primarily on the period 1970-1990, as these two decades encompass the major oil boom and bust. 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 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.[13] This evidence suggests that our empirical strategy will be informative about the effects of permanent (rather than 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 sample to include only ESRs that appear in all years does not affect results.

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

[13] 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". See also Hamilton (2008) for a similar conclusion.

the 306 large oil wells in the United States and 51% of the total oil reserves of these oil wells.[14]

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, including substantial within state variation. 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 US is more than 2500 million barrels (relative to a mean reserve conditional on having any reserves of 1700 million barrels). As a result of this variation, as we shall see, different areas experienced differential changes in income in response to changing oil prices; this is the basis of our first stage.

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 not available separately for oil mining).[15]

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).[16] We also obtain ESR-level employment data from the CBP in the same manner. 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

[14] 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.

[15] 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).

[16] 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`).

variable to provide comparative estimates of income elasticities in different industries.[17]

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 US 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 adoption. Specifically, the AHA data contain total hospital expenditures, payroll expenditures, full time equivalent employment, 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 on the levels of full-time equivalent employment of two types of nurses in the data: Registered Nurses (RNs) and Licensed Practitioner Nurses (LPNs), which together 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.[18]

There are three key advantages of the AHA data. First, they are extremely high quality. Relatedly, they appear to be unique among annual sub-national data on health expenditures from our time period in that they are constructed independently each year, and therefore do not rely on some degree of interpolation between years. Second, they allow us to conduct our analysis at a level of aggregation below the state and thus to exploit the substantial within-state variation in oil intensity shown in Figure 3a. Third, they allow us to measure other components of health care activity. In particular, using these data we can measure hospital technology adoption decisions and thereby investigate potential global general equilibrium effects through induced innovation.

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 may lead to biased estimates of the income elasticity of total health expenditures, we use data from the Health Care Financing Administration (HCFA), which produces state-level estimates of total personal health care expenditures and its components, although only for a subset of our study years (Levit, 1982, 1985).[19] In addition, we also examine decadal

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

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

[19] As we discuss in more detail in Section 4 below, the HCFA data are constructed partly based on in-

state-level Census data on the earnings of various groups of health care providers.

Population To investigate the extent of migration in response to our income variation, we use annual data on total area population and on area population by five year age groups from the Current Population Reports (CPR). Crucially, for our purposes, population is not interpolated between censuses but rather is imputed annually 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).[20]

Finally, to gauge the relative intensity of hospital use among individuals of different age groups, we use data on the age profile of hospital use constructed from the National Health Interview Survey (NHIS), which we pool between 1973 and 1991.

Comparison across areas with different oil intensity Table 1b examines whether there are significant differences in income and various measures of hospital activity in 1970 across ESRs with different levels of oil reserves. We look at this relationship in our baseline sample of the 16 Southern United States. Columns 3 and 4 of this table show that there is no statistically or economically significant relationship between oil reserves and any (or all) of population, total employment, hospital expenditures, hospital beds and total income. In each case, the association with oil reserves is statistically indistinguishable from zero and the magnitude of variation is small (one standard deviation change in oil reserve is associated with only about one tenth of one standard deviation change in each of these variables). This offers some preliminary support for our exclusion restriction that, absent the oil price changes in the 1970s and 1980s, ESRs with different levels of oil reserves would have been on similar trends in terms of their hospital expenditures and utilization. Section 5 provides a more systematic investigation of the validity of our exclusion restriction.

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terpolation between years, which is an important caveat for regression analysis based on these data. Data from 1972 and 1976-1978 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).

[20] 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.[21] 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 sources 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 on our instrument, δ, 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 may be due to 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.[22] 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 (US Census Bureau, 2008). In Section 4, we discuss the possible implications of extrapolating from our income changes to the effects of the secular increase in incomes in the US 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 our 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 reflects increases in area population; a comparison of columns 2 and 3

[21] Because of concerns of the small sample properties of clustering with only 16 states, we experimented with alternative small sample corrections, as well as alternative strategies to correct for potential serial correlation. The alternative procedures produce similar results, and are discussed in Section 5.

[22] 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).

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 from estimating equation (2) with log 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, in which we estimate equation (2) using as the dependent variable the log of the total population under 55 and the log of the total population 55 and over, respectively. The results 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 2 to save space). Younger individuals consume disproportionately lower amounts of hospital care than the elderly. To illustrate this, Figure 5 shows the average annual number of hospital days for individuals in five-year age brackets estimated from the National Health Interview Survey (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, if anything, to err on the side of over-estimating, rather than under-estimating, income elasticities), 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 average of hospital days used by that age bin ($hospdays_a$) from the pooled 1973-1991 NHIS. Namely:

$$HUWP_{jt} = \sum_a pop_{ajt} \times hospdays_a \qquad (3)$$

Our preferred specification adjusts (i.e., divides) 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 structural equation:

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

and our baseline first-stage equation:

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

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

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

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 estimates of the first-stage coefficient, δ', from equation (5) are shown in column 7. Its magnitude lies (mechanically) in between the first-stage estimates without any migration adjustment (column 1) and with the per capita adjustment (column 4). In practice, the magnitude is about one third of the way from the per capita adjustment to the unadjusted specification. The IV estimate of the effect of income on hospital spending using the hospital utilization 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).

In what follows, we take the estimates from equations (5) and (4), which correct for the age-adjusted hospital utilization of the population, as our baseline/preferred specification. Because even conditional on age migrants may be healthier than the general population, the estimate of β from (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, and 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).[23] In practice, we will see below that these "bounds" on the income elasticity are relatively tight.

Finally, column 8 shows the HUWP-adjusted first stage but now aggregated to the state level (rather than the ESR level as in column 7); the first stage is robust to aggregation to the

[23] This last presumption is both intuitive and consistent with the fact that migration is concentrated among younger individuals (see Table 2).

state level (F-statistic = 24.05).[24]

To gauge the magnitude of the first stage, we calculated that 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. The first stage in our preferred specification has an F-statistic of 16.58.

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. The estimate of β in (4) is -0.027 (standard error = 0.074). This indicates that when income in an area increase by 10 percent, hospital expenditures fall by about 0.3 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. Our subsequent analysis suggests 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). The estimated elasticity of health expenditure with respect to income is 0.723, with a standard error of 0.214.[25]

Columns 4 and 5 show IV results without any population adjustment and with a per capita population adjustment, respectively, to both hospital expenditures and income. As discussed in Section 3.1, these estimates 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 0.665 and 0.801, suggesting that these bounds are reasonably tight.

[24] Although the first stage is robust to aggregating up from ESR to state, it is not robust to dis-aggregating the data to a lower level of aggregation than the ESR (not shown). 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.

[25] 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.

The last column of Table 3 reports the results from our baseline, HUWP-adjusted specification (from column 3) but now aggregated to the state level. We estimate an income elasticity at the state level of 0.550 (standard error = 0.230). The point estimate at the state level is similar to our estimate at the ESR level of 0.723 (see column 3). We provide a more detailed discussion of state-level results in Section 4 but note here that, among other things, the state-level estimates allow us to capture potential general equilibrium effects, such as political economy effects, that may be more likely to occur at the level of the state than at the sub-state ESR.

Table 4 investigates which components of hospital expenditures are affected by income changes. It reports the results from IV estimation of equation (4) using different hospital outcomes as the dependent variable.[26] Several interesting findings 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 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 a 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, proxied by the ratio of skilled nurses (RNs) to all RNs and LPNs.[27] This does not rule out wage (price) effects, but suggests that at least some of the increase in payroll expenditures in column 2 comes from quality improvements. More importantly, evidence of skill upgrading also 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

[26] 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 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.

[27] 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 0.449 (s.e. 0.181) when the odd years in the 1970s are excluded.

measured by beds). These results are shown in columns 5 through 7.[28] The remaining columns of Table 4 document the impact of rising income on hospital entry and technology adoption; we defer a discussion of these results until Section 4.[29]

4 The Role of Income in Rising Health Share of GDP

We now present the implications of our estimates for the role of rising income in explaining the rising health share in the United States. The bulk of the section is then devoted to a discussion of several potential concerns and caveats with this out-of-sample extrapolation exercise.

4.1 Income and the Rising Health Share of GDP

Let us focus on the results from our baseline specification (Table 3, column 3), which are roughly in the middle of the range of elasticities we report in various alternative specifications below.

The point estimate of an elasticity of 0.72 implies that the approximate doubling of real per capita GDP between 1960 and 2005 (from $19,212 to $41,874 in $2005) should have caused 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 1.13. This allows us to reject a role of rising income in increasing the health share of GDP by more than 0.5 percentage points between 1960 and 2005, i.e., it does not explain more than 5 percent of the overall increase in health share over this time period.

We can also interpret our estimates in terms of their implications for rising income in explaining rising health expenditures (rather than the rising health share of GDP). 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 expenditures, while the upper end of the 95 percent confidence

[28] 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 may improve health.

[29] We also explored the relationship between our income variation and public funding of health care, using data from the Regional Economic Information System; these data are available at the ESR level annually for our entire study period. Public spending on health care appears to fall as income rises, with Medicaid spending falling substantially more than Medicare spending. Since the income of either Medicare or Medicaid beneficiaries should not be affected much by our instrument (the former are predominantly retirees with a pre-determined income stream and the latter are, by definition, constrained to be very low income), these results likely reflect a potential crowding out of scarce hospital resources from those whose incomes have risen and perhaps also policy responses of state governments to changing incomes. The decline in Medicaid spending may further reflect reductions in eligibility for Medicaid resulting from the increase in employment. These results are available upon request.

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.[30]

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 in the United States. We next turn to several potential concerns with this extrapolation exercise.

4.2 National and Global General Equilibrium Effects

A thorough empirical examination of the role that rising income plays in the growth of the health care sector requires incorporating any general equilibrium effects of income on health care spending. While our empirical strategy is designed to capture (and indeed - as evidence by the skill upgrading results in Table 4 - appears to capture) general equilibrium effects that occur at the level of the local economy, it does not incorporate any general equilibrium effects at the national or global level.[31]

Two such general equilibrium effects that could potentially increase the income elasticity of health expenditures above what we have estimated are induced innovation effects (which could occur at the national or global level) and national political economy responses to rising income. We discuss each in turn.

Endogenous technology responses While our estimates incorporate the impact of income on technology adoption and entry of new hospitals at the ESR level, they may understate 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 an induced technology response to rising income is 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,

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

[31] Indeed, we find it difficult to imagine a convincing empirical straetgy that could capture national and global general equilibirum effects of rising income.

the same mechanism leading to induced innovations 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 expenditures increase less than proportionately with income.

Turning to the first component of the argument, we find no evidence that rising income is associated with an increase in hospital entry or technology adoption. These results are summarized in columns 8 through 11 of Table 4. 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 previously used to study technology adoption decisions in hospitals, and in particular hospital responsiveness to economic incentives including the insurance regime and relative factor prices (see, e.g., Cutler and Sheiner, 1998, Baker and Phibbs, 2002, Finkelstein, 2007, Acemoglu and Finkelstein, 2008). Since they contain only indicator variables for the presence of various facilities, we cannot investigate the potential upgrading of existing technology or the intensity of technology use, but we can study the impact of changes in income on the total number of facilities, proxying for technology adoption decisions on the extensive margin.

During the time period we study, the AHA collects information on the presence of 172 different "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 the relationship between income and 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 log of the number of distinct technologies in a given ESR in a given year. The year fixed effects in our IV estimate of equation (4) adjust for the fact that the set of technologies reported in each year differs. The results show no substantively or statistically significant evidence of an increase in the number of distinct 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 1.3 percent.[32]

A drawback of this approach is that it treats all technologies as perfect substitutes. As an alternative, 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 in our sample: open heart surgery and diagnostic radioisotope facility. Both have been found in other work to be responsive to economic incentives (Finkelstein, 2007, Acemoglu and Finkelstein, 2008). Both of these technologies were diffusing over our sample period, though open heart surgery started from a lower prevalence and diffused more rapidly.[33] To investigate the impact of ESR income on local technology adoption decisions, we estimate semi-parametric Cox hazard models for these two technologies as functions of income. 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

$$\lambda_{jt} = \lambda_{0t} \exp(\beta \log \tilde{y}_{jt} + \mathbf{X}_j^T \phi), \tag{7}$$

where λ_{0t} is a fully flexible, non-parametric baseline hazard, $\tilde{y}_{jt}$ is our baseline measure of (HUWP-adjusted) income, and $\mathbf{X}_j$ is a vector of (time-invariant) covariates. Since we have at most a single transition (adoption) for each ESR, we cannot include ESR fixed effects in the hazard model. Instead, we include time-invariant ESR characteristics in the vector $\mathbf{X}_j$, in particular, region fixed effects for the three census regions within the South, total hospital expenditures in 1970, and total hospital beds in 1970. The fully flexible baseline hazard in the Cox model is specified with respect to calendar time and thus controls for time effects. As in our baseline specification, income is an endogenous right-hand side variable, which we instrument

[32] To provide some context for comparison, using the same technology measure (but at the hospital level rather than at 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 distinct technologies that the hospital has).

[33] Open heart surgery is in our data for all 21 years (1970-1990) and diagnostic radioisotope therapy for 19 years (1972-1990). Only 43 percent of ESRs had open heart surgery technology in 1970, whereas about 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.

with $\log p_{t-1} \times I_j$. We implement our instrumental variables estimator using a control function approach (Newey, Powell, and Vella, 1999). Specifically, we include the residual ($\hat{u}_{jt}$) from the first stage regression in equation (5) as an additional covariate in equation (7). We report bootstrapped standard errors and p-values for this two-step estimator. The results reported in columns 10 and 11 in Table 4 show no evidence of a significant increase in technology adoption associated with an increase in income. The point estimates suggest a negative relationship between log income and adoption of open-heart surgery, and a positive relationship between log income and adoption of the diagnostic radioisotope facility. However, both estimates are imprecise and not statistically different from zero.[34]

Next, turning to the theoretical argument, Appendix A outlines a simple model of induced innovations and demonstrates that development of new technologies will tend 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 incomes rise, the market size for health care technologies will increase less than the market size for a range of other technologies. As a consequence, the 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 plausibly at roughly a constant rate (as mortality has been declining at a roughly constant rate over this same period (Cutler and Meara, 2003)).[35]

Limited income-induced technology effects for the health care sector are also consistent

[34] By contrast, Acemoglu and Finkelstein (2008) find statistically 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 is generally responsive to economic incentives.

[35] Of course, the specific nature of medical technological progress has 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).

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, while we cannot conclusively rule out major national or global induced technology responses to the secular increase in income in the United States, which could in turn have further effects 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 rising incomes are unlikely to be the major factor in the run-up in the share of GDP spent on health care.

Political economy effects of rising incomes Although our empirical strategy would not capture any effect of income on health care expenditures that operate via a national political economy response to rising income, our state-level results incorporate potential responses at the state and sub-state levels. The similarity between the estimate of the income elasticity at the state level (see Table 3 column 6) and our baseline estimate at the ESR level (Table 3 column 3) suggests that these state-level policy responses do not significantly increase the responsiveness of health care expenditures to income, although there may be substantial sub-state level policy responses captured by both our ESR- and state-level estimates.[36]

While our empirical strategy does not incorporate national political economy effects resulting from rising incomes, health policy in the United States is highly decentralized, with much of the public involvement occurring at the state (or lower) level of government. Therefore our empirical strategy likely captures much of the potential political economy responses. This holds for both public provision and public financing of health care, both of which are potentially affected by changes in income.

In terms of public provision of health care, about one third of hospitals in the United States (accounting for about one third of hospital expenditures) are publicly owned. About 85 percent of these hospitals (constituting about three-quarters of public hospital expenditures) are non-federal (i.e., state-, county-, or city-owned). Thus most of any effect that income changes have on public support for hospital financing would be incorporated into our state-level analysis.

In terms of public financing of health care, by far the two largest sources are Medicare and

[36] This observation also underscores that, as already emphasized in the Introduction, the empirical relationship between income and health spending in the United States in the latter half of the 20th century, which we are exploring, may reflect a variety of institutional factors beyond the willingness of households to spend more on health care as their incomes grow.

Medicaid, which have similar levels of spending (CMS, 2006). Medicaid is jointly financed by the federal and state governments but the states are given considerable autonomy in the design of program eligibility and benefit requirements (Gruber, 2003). Political economy effects of changing income on Medicaid design are likely to be captured by our estimates using state-level variation.

Medicare, in contrast, is a fully federal program, so that any political economy effects of income on Medicare design would not be captured by our estimates. This is a potentially important channel through which rising income may affect health spending, and not one that we can directly estimate. Nevertheless, it is reassuring in this regard that Medicare spending per beneficiary over our time period has not risen faster than overall health spending per capita.[37] If the national political economy response of Medicare policy were an important part of the mechanism through which the secular increase in income contributes to the growth of health share of GDP, Medicare spending per beneficiary should have been rising much faster than overall health spending per capita over the past several decades. The fact that it has not suggests that any potential political economy responses to rising incomes working through Medicare does not introduce a serious downward bias in our estimate of the role of income growth in the run-up of the health share of GDP.

4.3 Hospital Spending Versus Total Health Expenditure

An important 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 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

[37] We compared the growth in per capita health expenditures to the growth in per beneficiary Medicare spending from 1975 to 2005. We started in 1975 to allow the Medicare program (which only began in 1965 and expanded to cover SSDI recipients starting in 1973) to be fully phased in. Between 1975 and 2005 Medicare spending per beneficiary grew at an average annualized rate of 7.86%, while health spending per capita grew at 7.62%. Data on total and Medicare health expenditures and Medicare beneficiaries can be found at `http://www.cms.hhs.gov/nationalhealthexpenddata/` and `http://www.cms.hhs.gov/MedicareEnRpts/`.

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 of the hospital- and non-hospital components thereof, based on several complementary data sources. We use these data to investigate whether there is any evidence that overall health expenditures are more responsive than hospital expenditures to changes in income. To preview, although estimates from the other available data sources are often quite imprecise (motivating our preference for the AHA data set), we do not find any evidence that overall health expenditures are more income elastic than hospital expenditures.

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 problem with these data is that each component is interpolated whenever data are missing between years (Levit, 1982, 1985). Such interpolation may bias the estimated coefficients, so the results from this data set have to be interpreted with caution.

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 baseline sample of the 16 the Southern states (Panel A) and for the entire United States (Panel B). Column 1 shows that our first stage is robust to state-level analysis for the subset of years for which we have HCFA data. Columns 2 and 3 show our estimated income elasticity from the HCFA data for total health expenditures and the hospital subcomponent, respectively. Both estimated income elasticities are positive but quantitatively small and imprecise, and thus statistically insignificant. The income elasticity of hospital spending using the HCFA data is also noticeably smaller than that estimated using the AHA data.[38]

However, most importantly for our purposes, the point estimates in columns 2 and 3 of Table 5 suggest similar income elasticities for hospital expenditures and total health expenditures. Columns 4 through 9 present results for the other components of health expenditures, and

[38] The hospital expenditure data in the HCFA series are estimated using 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 creating the HCFA data. Most importantly, the HCFA estimates interpolate missing data (Levit, 1982, 1985). Average state-year hospital expenditures are similar in the two data sets ($2,641 million from the HCFA data compared to $2,333 million for the same state-years in the AHA data). Log hospital expenditures are also highly correlated across the two data sets at the state-year level (correlation = 0.98). However, conditional on state and year fixed effects, the correlation in the residual log hospital expenditures is only 0.67. This presumably helps explain why the income elasticity estimates differ. Using our AHA hospital data at the state level for the full United States and limiting the sample to the years for which the HCFA data are available (i.e., the analog of Table 5 column 3 panel B), we estimate a statistically significant income elasticity of 0.509 (standard error = 0.225). This is statistically indistinguishable from the HCFA estimate of 0.139 (standard error = 0.151).

provide some intuition for why hospital and total health expenditure income elasticities may be similar. The point estimates suggest that the income elasticities of spending on physician services, on dental services, on drugs and other medical non-durables, and on vision products are greater than the income elasticity of hospital spending, while nursing home care and other health services have large negative income elasticities.[39] Overall, the results in Table 5 are generally imprecisely estimated, but the point estimates are uniformly consistent with similar income elasticities for total health expenditures and for hospital expenditures.

Results from several other data sources are also consistent with this conclusion, though again are similarly imprecise. 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; indeed the point estimates are considerably smaller than our estimates for hospital expenditures, although they are quite imprecise.[40]

We also examined the impact of area income on the income of different groups of health care providers (results available on request). If non-hospital components of health care expenditures—such as physician expenditures—are substantially more income elastic than hospital expenditures, we would expect to find that the earnings of the non-hospital based health care providers are also substantially more income elastic than hospital expenditures and than the earnings of health care providers that contribute to hospital expenditures, such as nurses and health care technicians. Using decadal Census data aggregated to the state level, we estimated the income elasticity of the earnings of the following groups of health care providers: physicians, nurses, health care technicians (including clinical laboratory technicians and therapy assistants), and other health services workers (including health aids, nursing aids and attendants).[41] Our IV point estimates show no evidence that physician earnings are more responses to area income than hospital expenditures or than the earnings of other health care providers. However, the

[39] The large negative income elasticity for nursing home care strikes us as intuitive. Wealthier individuals can more easily pay for assistance at home to substitue for nursing home care (which Medicaid will cover) than can poor individuals.

[40] The results for state-level Health Services GSP are shown in Table 7, column 6, Panel A and B). The rest of that table is discussed in subsection 4.5 below. Comparable state-level estimates for hospital expenditures are show in Table 6 Panel A, columns 1 and 3. The Gross State Product (State GDP) estimates are produced annually by the Bureau of Economic Analysis. The specific industries within health services (SIC code 80) are listed at `http://www.census.gov/epcd/naics/NSIC8B.HTM#S80`. The major source of state data for the health services GSP estimates are sales and payrolls from the (quinquennial) census of service industries; intercensal years are interpolated and extrapolated using wages and salaries reported annually to the BEA (see `http://www.bea.gov/regional/pdf/gsp/GDPState.pdf`).

[41] Our first stage is robust to aggregation to the state level and to decadal (vs annual) analysis; the IV estimate of AHA hospital expenditures in this specification is generally similar in magnitude although somewhat less precise than that in our baseline specification.

estimates using the Census income data—particularly those for physician income—are noticeably less precise than those from comparable specifications using the AHA data on hospital expenditures, so that one should not place too much emphasis on these results.[42]

Overall, while there are important limitations to each data source, a number of complementary data sets with information on state-level health expenditures suggest 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 conclude that our estimates of the income elasticity of hospital spending are likely to be representative of the income elasticity of total health expenditures.

4.4 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 under-stating (or over-stating) the first-stage relationship, and consequently, over-stating (or under-stating) the income elasticity in the second stage. Unfortunately, annual data on labor and capital income do not exist for our time period at a level of disaggregation below the state.

We therefore investigate how our estimates at the state level change when we use Gross State Product (GSP) as our measure of income, rather than our baseline payroll measure; unlike payroll, GSP includes both labor and capital income. Table 6 shows the results of this exercise. Panel A shows the IV estimates, and Panel B shows the first-stage estimates. Columns 1 and 2 compare results at the state level when labor (payroll) income and GSP are used, respectively, as our income measure. The first stage suggests that, in response to our instrument, non-labor income appears to rise by the same proportion, or by slightly more, then our primary measure of labor income (compare columns 1 and 2 of Panel B). If anything, therefore, the results suggest that the estimates using labor income only may be slightly over-stating the income elasticity of health expenditures (compare columns 1 and 2 of Panel A).

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 US in the sample rather than just the 16 states in the South. Column 3 shows the results when we use labor income (from the CBP payroll

[42] We also examined the elasticity of various components of state-level health care utilization from the 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 about 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). Results available on request.

data) as our measure of income and column 4 shows the results when we use the GSP measure, which incorporates 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.[43]

4.5 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 estimates are based on a specific type of income variation as well as a specific area of the country and time period. 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 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 sources of income variation, geographic samples, time periods, and time horizons; we therefore do not 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 estimated similar IV regressions with spending on goods that can be classified as a luxury on a priori grounds (e.g., recreation). Since we do not have data on spending on other goods at the ESR level, we pursued this strategy at the state level using data on industry-specific Gross State Products (GSP) for other service industries. Specifically, we used our instrument at the state level to examine the income elasticity of four potential luxury goods: "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.[44] We also estimated the income elasticity of "food and kindred products," which we expect to be a necessity (health services GSP, which was already discussed in subsection 4.3, is also included in this table).

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.[45] Legal services and

[43] The results in column 3 also suggest that our estimates are not sensitive to using the entire United States. In later robustness analysis we show this is true at the ESR level as well (see Table 9 below).

[44] A complete definition of "other services" can be found here: `http://www.osha.gov/pls/imis/sic_manual.display?id=1014&tab=description`.

[45] More information on each of these categories can be found here: `http://www.bea.gov/regional/gsp/`

"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, 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 United States over the last half century comes from increased wages per employed individual (US Census Bureau, 2008).[46] 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 increases in wages may be larger than the elasticity with respect to increases in employment.

Table 8 investigates whether there is any evidence of this type of convexity in Engel curves for health expenditures. 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 ESR log income with our standard instrument (oil reserves times log oil prices) and the interaction of this instrument with 1970 ESR log income. 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.

Finally, we note that because oil prices both rise and fall over our time period, our instru-

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`default.cfm?series=SIC`. First-stage results for this same specification are shown in Table 6, Panel B, columns 1 and 3. Second stage results for this same specification using the AHA hospital expenditure data as the dependent variable can be found in Table 6, Panel A, columns 1 and 3.

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

ment predicts both increases and decreases in income. From a purely estimation standpoint, this is a strength of our instrument, since it makes it less likely that it simply captures differential (monotonic) trends across different areas of the country. Nevertheless, since much of the motivation of our paper is related to the effects of rising incomes on health care expenditures, we also investigated whether the effects of rises and declines in income are asymmetric. In particular, we re-estimated our baseline models allowing positive and negative changes (between t and $t-1$) in income to have different effects (and we instrumented these income variables with our baseline instrument interacted with an indicator for whether oil prices rose between dates t and $t-1$). We found no evidence of such asymmetric effects (results available upon request).

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 IV estimates and Panel B shows the corresponding first-stage results. Column 1 reproduces our baseline estimates, which are for the 16 Southern states focusing on the time 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 (0.804 vs. 0.723 in the baseline), though the estimate is less precise (standard error = 0.631 compared to 0.214 in the baseline).[47]

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 each 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, the point estimate falls by about 40 percent; combined with the increase in standard error, this makes the estimate of the income elasticity of hospital expenditure no longer significant at the 5% level. This is not surprising since much of the variation in oil intensity in our sample is

[47] 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.

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.

Short-run versus long-run income elasticities We estimate the short-run response of health expenditures to income, which may be different from their long-run response. 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. However, there are no strong theoretical reasons to expect the long-run income elasticity to be 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 in income also improve 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 re-estimated 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. With only the decadal observations, the first stage is only slightly weaker (compare columns 4 and 1). The IV elasticity estimate from the decadal estimate is similar to the baseline annual estimate (0.794 compared to 0.723) and still 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 larger 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 typically perform better when there are heterogeneous adjustment dynamics by ESR. Thus in column 7 we report results based on 3-year averages. The estimated elasticity increases slightly (from 0.723 to 0.826).

5 Robustness

In this section, we provide several robustness checks of our baseline estimates, particularly 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 income elasticity of health care expenditures.

5.1 Exclusion Restriction

The exclusion restriction of our IV strategy 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 IV estimates, while Panel B shows the corresponding first-stage results. 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 differential trends across areas with different levels of oil reserves, future oil prices should not predict current income changes. Column 2 in Panel B shows that the first-stage relationship is robust to including the lead of the instrument. The coefficient on the lead of the instrument is positive and large (about 60 percent of that on the instrument), though statistically insignificant. The magnitude of this coefficient raises some concerns about potential serial correlation. We explore issues of serial correlation in greater detail in the subsection 5.3. To preview, even if there is serial correlation in the first stage, this does not necessarily create a bias in the IV estimates. In addition, our robustness checks in the next subsection show that the statistical and quantitative properties of our estimates are reasonably robust in alternative specifications that explicitly recognize the possibility of serial correlation.

The results from the IV estimates that include the five-year lead of the instrument (both in the first and second stages) are shown in Panel A column 2. The estimate of income elasticity in this specification remains statistically significant and increases somewhat in magnitude relative to the baseline in column 1. The negative (and statistically insignificant) coefficient on the five-year lead of the instrument indicates that our IV estimates are unlikely to be capturing pre-existing trends.

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 control for 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. 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 additional evidence 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. Consistent with the limited differences in various ESR characteristics shown in Table 1b, 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. The IV estimates decline considerably 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 of our estimates of the income elasticity, if anything, it suggests that our baseline model which does not control for state-specific trends might lead to over-estimates (rather than under-estimates) of this elasticity.

Finally, as another natural and important 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, and so on through the year 1969 which is assigned to the oil price of 1984).[48] 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 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 falsification exercise. Reassuringly, this falsification exercise shows no evidence of a significant reduced-form relationship between our instruments and health expenditures; the point estimate is negative (opposite sign from the "actual" estimate in column 6) and not statistically significant. This finding supports the validity of 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 also explored the robustness of our results to alternative specifications of the instrument. Table 12 shows the results. Panel A again shows the IV estimates and Panel B shows the corresponding first-stage 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. The remainder of the columns show results for alternative (plausible) specifications of the instrument; they tend to produce smaller income elasticities than our baseline specification.

Columns 2 and 3 report results using different functional forms for oil prices. 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 $\log p_{t-1} \times I_j$ as in our baseline

[48] 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 falsification 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. However, before 1970 only first quarter payroll and employment data are available from CBP.

specification). 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 strong first stages and statistically significant income elasticities in the second stage that are similar to, though slightly smaller than, our baseline estimate (the income elasticity estimates are 0.49 and 0.64 in columns 2 and 3 respectively, compared to 0.72 in our baseline).

Columns 4 through 6 report results using different ways of measuring the oil intensity of the area. Recall that in our baseline specification we proxied oil intensity of area j by its total (cumulative) oil reserves. 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 oil 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 percentile of oil reserve distribution (the instrument is then 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 significant income elasticity; the estimated income elasticity of 0.632 (standard error = 0.205) is only 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 1.10 (standard error = 0.67), but is no longer statistically significantly at the 5 percent level.

Finally, 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.[49] Our first stage is now marginally stronger than in the preceding specification (F-statistic of about 11), and we estimate a statistically insignificant income elasticity of 0.860 (standard error = 0.870).

[49] We include the indicator variable for whether there are any 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 included to separate out high mining share non-oil areas (such as coal mining areas of West Virginia).

5.3 Serial Correlation and Standard Errors

In our baseline model we cluster our standard errors at the state level; the standard errors are therefore computed from a variance-covariance matrix that allows both for arbitrary correlation in residuals across ESRs within a state and for serial correlation at the state or ESR level. However, because we only have 16 states in our baseline (South only) sample, these standard errors may be downward biased due to the relatively small number of clusters (Cameron, Gelbach and Miller, 2008). As a simple robustness check, we computed the standard errors allowing for an arbitrary variance-covariance matrix at the ESR level (rather than the state level). A possible disadvantage of these standard errors is that they do not allow for correlation across ESRs within the same state, which may be important in practice.[50] Clustering at the ESR level increases the standard errors substantially, so that the first-stage F-statistic is now 5.50 (instead of 16.58 with clustering at the state level). The standard errors for the second stage are also larger, but our IV estimate is still statistically significant at the 6 percent level (results available upon request).

Another strategy to correct for potential biases in the standard errors resulting from the small number of clusters at the state level is the wild bootstrap procedure suggested by Cameron, Gelbach and Miller (2008).[51] We performed wild bootstraps resampling states with replacement. In this case, we find reassuringly similar (indeed somewhat smaller) p-values to our baseline specification with state-level clustering.[52] In particular, using wild bootstraps we find that both the first stage and the second stage estimates are statistically significant at the less than 1 percent level (results available upon request).

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; we also experiment with various assumptions regarding the nature of any 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 13 reports the results. Column 1 shows 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

[50] 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 would be a negative correlation in ESR-level residuals within a state.

[51] We thank Doug Miller for suggestions and for providing us with a sample code.

[52] 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 also computed p-values using parameter estimates, and found these to be even lower (thus leading to more precise results) than the results reported here based on t-statistics.

1 verifies that this has no notable effect on our baseline results. Column 2 reports IV-GLS results assuming a common AR(1) autocorrelation coefficient across all ESRs. Column 3 reports results assuming an AR(2) specification of the residuals with common autocorrelation coefficients. In both specifications the point estimate rises relative to the baseline, but is also considerably less precise. Columns 4 and 5 report results assuming state-specific AR(1) and AR(2) errors respectively. Here the point estimates are very similar to the baseline specification both in magnitude and in precision. Overall we interpret these results as supportive of the robustness of the baseline specification.

As a final strategy to control for serial correlation, columns 6 and 7 include a lagged dependent variable on the right-hand side. In column 6, this model is estimated with ordinary least squares and leads to a long-run elasticity of 0.859 (standard error = 0.213), which is slightly higher than our baseline estimate. However, the least squares estimator in column 6 is inconsistent because of the presence of the lagged dependent variable on the right-hand side. Column 7 estimates the same model using the Arellano-Bond GMM dynamic panel estimator. This GMM procedure estimates the same model in first differences using further lags of the dependent variable as instruments. This leads to a considerably smaller long-run elasticity (= 0.142, standard error = 0.080) than in our baseline. Such smaller long-run elasticities make it even less likely that rising incomes over the past half a century could be the primary driver of the increase in the health share of GDP in the United States.[53]

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. It would also provide crucial input into an investigation of the potential optimality (or sub-optimality) of rising health share of GDP. Yet, surprisingly, little is known about the empirical impact of rising aggregate incomes on health spending.

We attempted to estimate the causal effect of aggregate income on aggregate health expenditures by instrumenting for local area income with time-series variation in global oil prices

[53] If we estimate our baseline model in first differences (and thus without further lagged dependent variables on the right-hand side), the results are similar to those reported in column 7 from the GMM procedure. In particular, the point estimate is 0.078 (standard error = 0.106). As we discuss in Appendix B, heterogeneous adjustment dynamics can introduce significant downward bias in first-difference estimates, and we thus put less weight on this estimate.

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 0.72 (standard error = 0.21). This estimate is reasonably robust to a range of alternative specifications.

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, 0.5 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 incomes in the post-war period, we provided additional evidence suggesting that many of the most salient potential concerns with such extrapolation 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 is a (or "the") major driver of increasing health expenditures (e.g., Newhouse, 1992, Fuchs, 1996, Cutler, 2002, Congressional Budget Office, 2008), though presumably technological progress itself is being spurred by other factors. Our analysis thus indirectly also suggests that rising incomes are 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 provided that technological advances in the United States spread relatively rapidly to other advanced economies, it could also be a major contributor to the similar trends experienced by other OECD countries. 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, given an income elasticity of health expenditure less than one, any induced innovation effects in the health care sector due to rising income are unlikely to be large. We first present a simple model incorporating 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 goods 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 $q_g(t)$. 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 for good 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 equality 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 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 a 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:

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

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:

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

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 (see Table 10, 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 $(\tilde{z}_{js} - \tilde{z}_{js})$

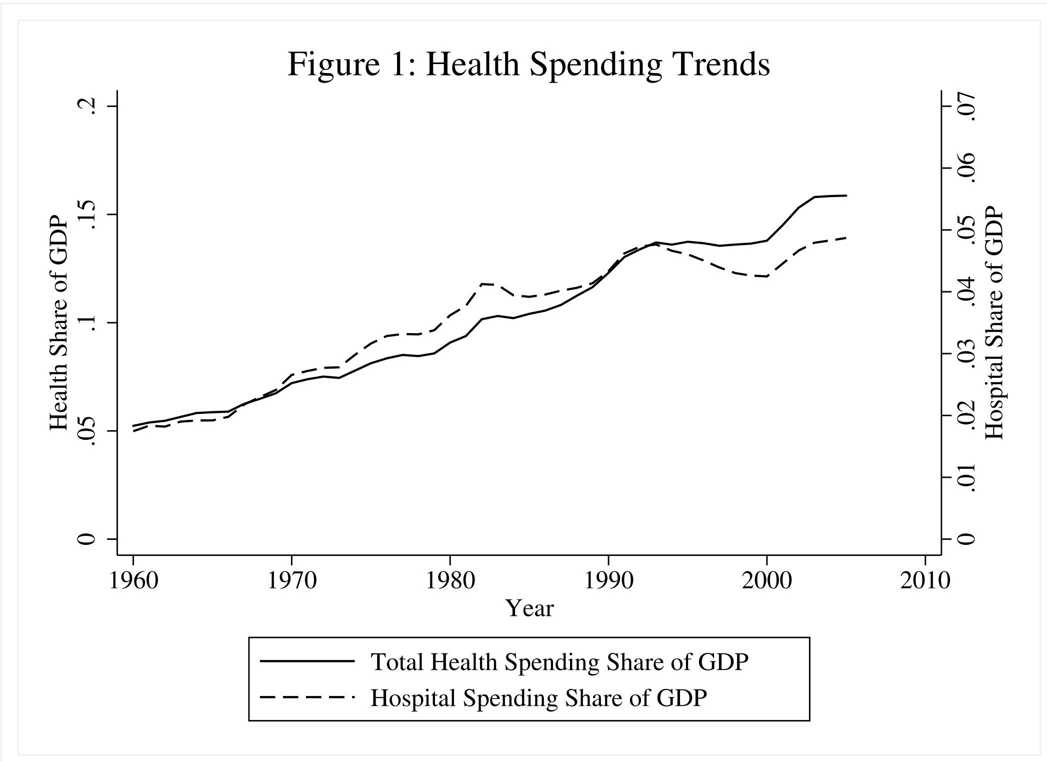
Finally, we choose $J = 10$ and $T = 30$, and we experiment with three values of ρ $(0.1, 0.5, 0.9)$.

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).

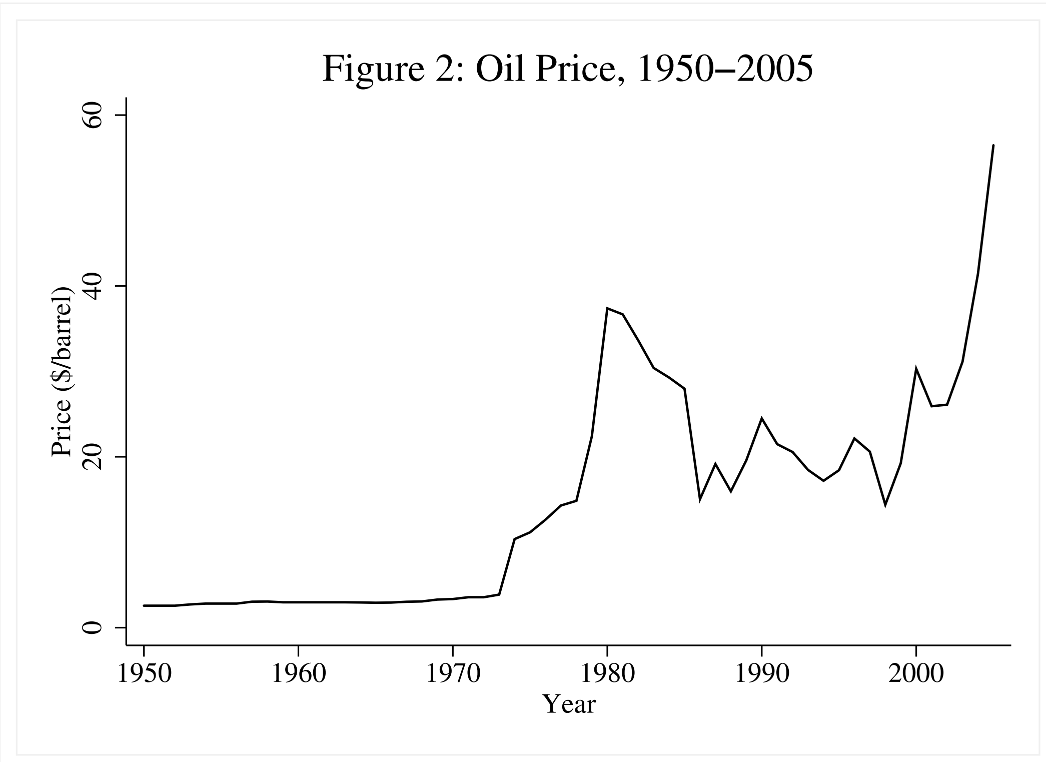
Each panel reports results for the same set of four models. The first row is the standard model where all panels adjust instantly. All estimators except FE-IV-LAG perform very well (the average of the parameter estimates is very close to the true value of 1.000). The second row reports results using 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 is 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.

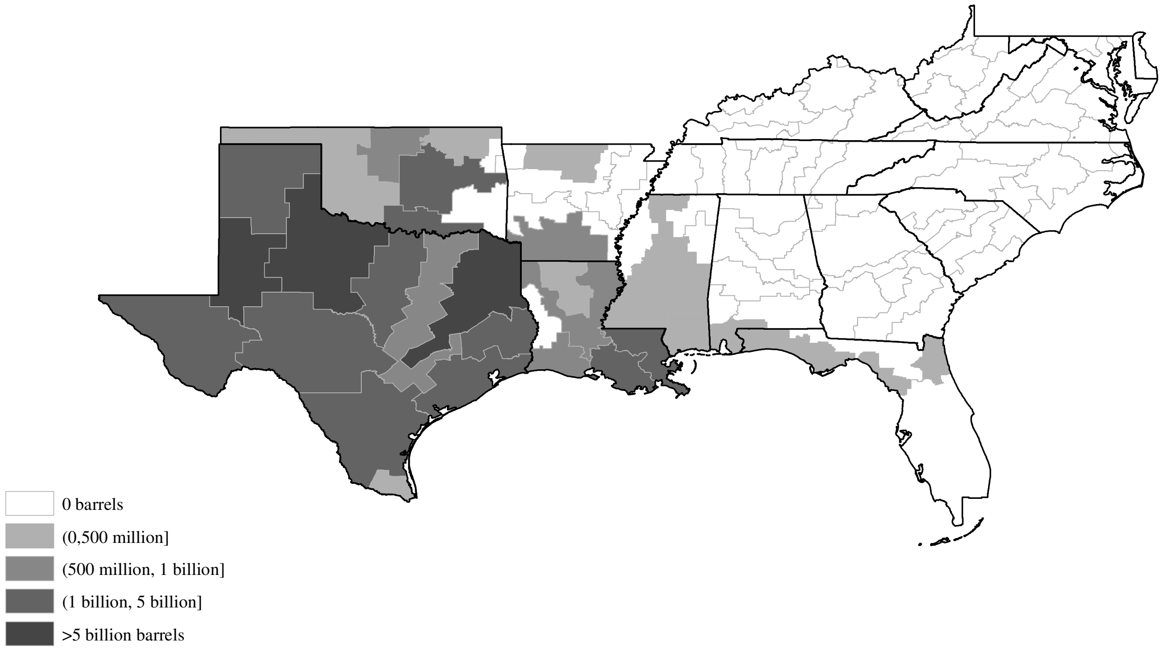


Note: This graph displays the trends in hospital spending from 1960 until 2005. Source: CMS (2006).

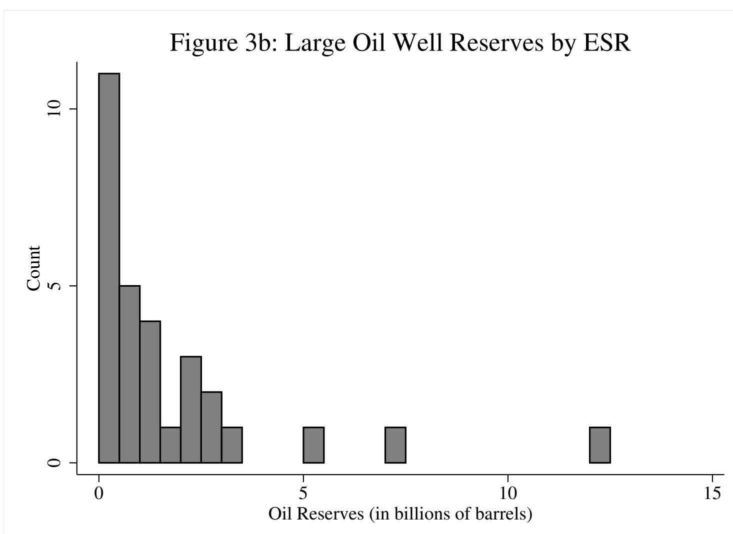


Note: This graph displays the annual average oil price, calculated from the monthly spot prices in the West Texas Intermediate series. The data are available here: `http://research.stlouisfed.org/fred2/series/OILPRICE/downloaddata?cid=98`.

Figure 3a: Map of Large Oil Well Reserves by ESR

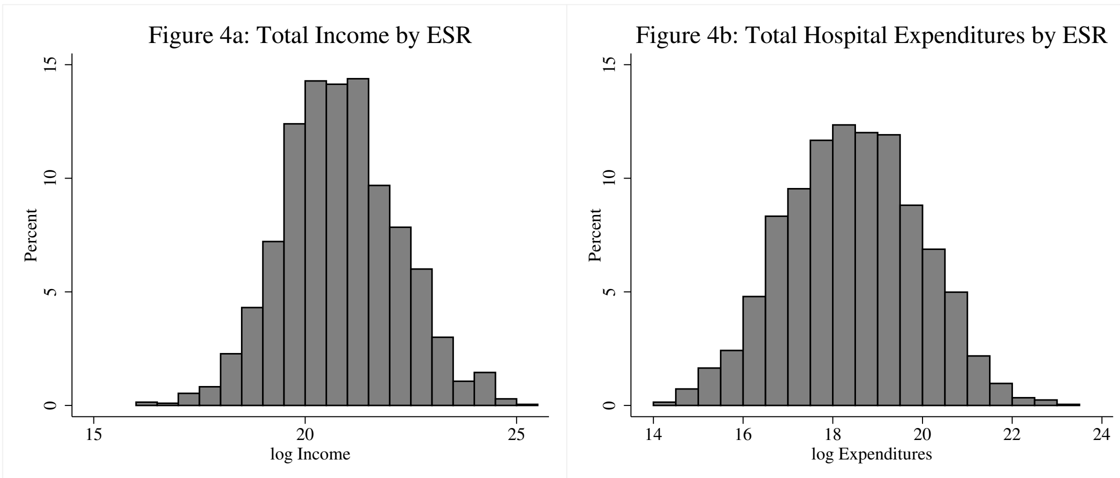


<u>Note:</u> This map displays the total amount of oil in large oil wells for each Economic Sub Region in the South. Large oil wells are defined as having ever had more than 100 million barrels of oil. The data come from the 2000 Edition of the Oil and Gas Data Book.

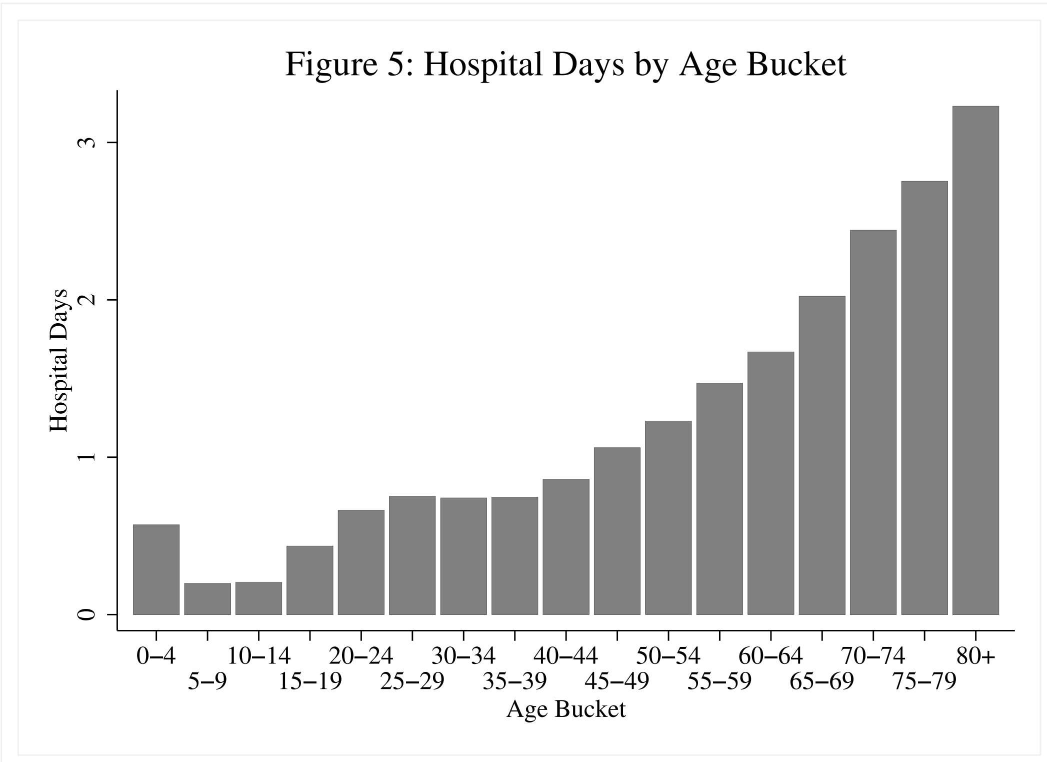


<u>Note:</u> This figure displays the cross-sectional distribution of oil reserves by Economic Sub Region (ESR) among the ESRs containing large wells. Of the 99 ESRs in the South, 69 ESRs do not have any large oil wells. This figure shows the amount of oil reserves (in billions of barrels) for the 30 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.

Table 1a: Descriptive Statistics

Variable	ESR-year data Mean	ESR-year data Standard Deviation	State-year data Mean	State-year data Standard Deviation
Oil and Gas Data Book Data				
Oil Reserves (million barrels)	532.3	1596.1	3371.7	9124.3
County Business Patterns Data				
Total Income (Payroll); ($millions)	2916.9	6066.7	18494.4	20751.0
Total Employment (millions)	0.21	0.35	1.32	1.13
AHA Hospital Data				
Total Expenditures ($millions)	292.61	636.31	1854.22	2257.94
Hospital Payroll ($millions)	139.87	284.12	886.40	1011.38
Admissions (millions)	0.11	0.16	0.73	0.55
Inpatient Days (millions)	1.08	1.47	6.85	4.76
Beds (thousands)	4.15	5.65	26.29	19.06
Full-time Equivalents (thousands)	9.58	14.55	60.72	48.58
RN / (LPN + RN)	0.63	0.12	0.69	0.09
# of Technologies	46.98	18.10	48.37	19.53
# of Hospitals	24.67	26.57	156.43	126.71
Current Population Reports and NHIS Data				
Population (millions)	0.68	0.89	4.44	3.40
HUWP (millions)	0.60	0.84	3.88	3.02
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			624.5	995.6
Food			524.3	485.0
Health Care Financing Administration (HCFA) Data (all in $millions)				
Total Health Care Expenditures			5923.8	6447.2
Hospital Expenditures			2641.1	2663.7
Physician and Other Services			1626.0	2065.4
Dental Services			303.3	330.7
Drugs and Other Medical Non-durables			654.7	685.4
Vision Products			106.2	130.8
Nursing Care			390.7	382.2
Other Health Services			201.8	425.2

Notes: Summary statistics in columns 1 and 2 are for the baseline sample of 99 Economic Sub Regions (ESRs) in the 16 Southern states between 1970 and 1990 (i.e. all statistics are ESR-year); columns 3 and 4 report summary statistics for the State-year data for the same baseline sample of 16 southern states between 1970 and 1990. Source for variables is given in italics. BEA and HCFA data are only available at state level. N = 2065 at ESR-Year except for RN/(LPN+RN) which is 1576 and Inpatient Days which is 1967. N = 326 at State-Year except for HCFA data and except for RN/(LPN+RN) which is 251 and Inpatient Days which is 311. Data on RNs and LPNs are only available in 1970, 1972, 1974, 1976, 1978, and 1980-1990. Data on Inpatient Days are not available in 1979. N = 236 at State-Year for HCFA data which are only available in 1972, 1976-1978, and 1980-1990. HUWP is a hospital-utilization weighted measure of population. See text for more details.

Table 1b: Comparing Economic Sub Regions in 1970 With Different Oil Reserves

Variable	(1) Mean for ESRs with Large Oil Wells	(2) Mean for ESRs without Large Oil Wells	(3) Coefficient	(4) *p* -value
Population (in millions)	0.687	0.521	0.113	0.155
Total Employment (in millions)	0.168	0.137	0.075	0.306
Hospital Expenditures (in $thousands)	0.059	0.050	0.072	0.356
Hospital Beds (in thousands)	4.671	3.940	0.094	0.184
Total Income (in $thousands)	0.989	0.778	0.077	0.298
p-value of *F* -test of joint significance (F-statistic = 1.12 for $F(5,92)$)				0.357

Notes: All results based on 1970 cross-section of the ESRs in the baseline sample (i.e. the 16 Southern states). Column 3 reports the coeffient from a regression of Oil Reserves on the variable in the row header and a constant term; in these regressions in column 3, both dependent and independent variables are standardized to have standard deviation of 1. Column 4 reports the associated p-value (based on heteroskedasticity-robust standard errors). The final row of table reports results from a regression of Oil Reserves on all of the variables listed in table and a constant term. N = 98 in the regressions reported in columns 3 and 4 because AHA data for Washington, DC are not available in 1970. N = 30 in column 1 and 68 in column 2.

Table 2: First Stage

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Geographic level of analysis:	Economic Sub Region							State
Dependent variable:	Total Income	Total Employment	Population	Income per capita	Population < 55	Population $\geq$ 55	Income per HUWP	Income per HUWP
Oil Reserves$_j$ ×	12.900	15.542	5.252	7.648	6.421	1.545	9.245	2.564
log(oil price)$_{t-1}$	(2.980)	(2.572)	(1.491)	(1.937)	(1.756)	(1.531)	(2.271)	(0.523)
	[0.001]	[0.000]	[0.003]	[0.001]	[0.002]	[0.329]	[0.001]	[0.000]
R^2	0.994	0.969	0.997	0.984	0.997	0.996	0.983	0.989
N	2065	2065	2065	2065	2065	2065	2065	326
F -statistic	18.74	36.53	12.40	15.58	13.37	1.02	16.58	24.05

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 and 8 the dependent variable is income 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 except in column 8 where it is State-year. All models include ESR (or state in column 8) 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)	(6)
Geographic level of analysis:	Economic Sub Region					State
Population adjustment:	HUWP	HUWP Reduced Form	HUWP	None	Per Capita	HUWP
	OLS	OLS	IV	IV	IV	IV
log(Income)$_{jt}$	-0.027		0.723	0.801	0.665	0.550
	(0.074)		(0.214)	(0.155)	(0.263)	(0.230)
	[0.723]		[0.004]	[0.000]	[0.023]	[0.030]
Oil Reserves$_j$ × log(oil price)$_{t-1}$		6.680				
		(2.048)				
		[0.005]				
R^2	0.973	0.973	0.968	0.989	0.970	0.992
N	2065	2065	2065	2065	2065	326

Notes: Table reports results of estimating equations (1), (4) or (6) by OLS or IV as indicated. Dependent variable is log hospital expenditures. In columns 1, 2, 3, and 6, both hospital expenditures and income are divided by a hospital-utilization weighted measure of population (HUWP) before taking logs (see equations (4) through (6)). In column 4 hospital expenditures and income are not adjusted before taking logs, 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 except in column 6 where it is a state-year. All models include ESR (or state in column 6) 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

Dependent Variable:	(1) Total Hospital Expenditures	(2) Total Hospital Payroll	(3) FTE	(4) RN/ (RN+LPN)	(5) Admissions	(6) In-Patient Days	(7) Beds	(8) # of Hospitals	(9) # of Technologies	(10) Open-Heart Surgery	(11) Radioisotope Therapy
$\log(\text{Income})_{jt}$	0.723	0.934	0.039	0.329	-0.430	-1.034	-0.698	-0.552	-0.132	-3.195	1.084
	(0.214)	(0.233)	(0.222)	(0.089)	(0.193)	(0.488)	(0.455)	(0.358)	(0.221)	(11.112)	(2.566)
	[0.004]	[0.001]	[0.862]	[0.002]	[0.042]	[0.051]	[0.146]	[0.144]	[0.558]	[0.170]	[0.545]
R^2	0.968	0.958	0.893	0.868	0.788	0.884	0.871	0.981	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. Unit of observation is an Economic Sub Region (ESR)-year. The baseline sample is all Southern states between 1970 and 1990. 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 column 4 is not adjusted by any population measure and is not in logs. Columns 10 and 11 report results from an instrumental variables estimator of the Cox proportional hazard model shown in equation (7). Dependent variable in columns 10 and 11 is an indicator variable for whether an at-risk ESR adopts the technology in that year and sample size reflects the number of ESRs "at risk" for adoption in each year. In column 10, there are 56 ESRs that have not adopted open-heart surgery technology by 1970 and 22 ESRs that have not adopted by 1990. In column 11, there are 21 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 (column 4) only exist in 1970, 1972, 1974, 1976, 1978, and 1980-1990. Data for in-patient days (column 6) do not exist in 1979. All models include ESR and year fixed effects, except columns 10 and 11 which have region fixed effects and controls for total hospital beds and hospital expenditures in 1970. In all columns income is divided by HUWP before taking logs. 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; in columns 10 and 11 the standard errors and p-values are bootstrapped (clustered by state).

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.	Hospital 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)$_{t-1}$	3.626								
	(0.776)								
	[0.000]								
log(Income)$_{jt}$		0.055	0.067	0.179	0.622	0.248	1.187	-1.302	-0.359
		(0.077)	(0.157)	(0.152)	(0.100)	(0.120)	(0.516)	(0.321)	(0.228)
		[0.484]	[0.675]	[0.257]	[0.000]	[0.057]	[0.036]	[0.001]	[0.137]
R^2	0.985	0.998	0.995	0.996	0.991	0.993	0.914	0.926	0.963
N	236	236	236	236	236	236	236	236	236
F-statistic	21.81								
Share of Total Health Care 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)$_{t-1}$	3.162								
	(0.586)								
	[0.000]								
log(Income)$_{jt}$		0.098	0.139	0.365	0.650	0.307	0.748	-1.944	-0.953
		(0.167)	(0.151)	(0.186)	(0.173)	(0.112)	(0.824)	(0.968)	(0.758)
		[0.558]	[0.361]	[0.056]	[0.000]	[0.009]	[0.368]	[0.050]	[0.214]
R^2	0.98	0.996	0.965	0.974	0.986	0.989	0.879	0.918	0.915
N	729	729	729	729	729	729	729	729	729
F-statistic	29.11								
Share of Total Health Care Exp.			45.06%	25.04%	6.07%	10.40%	2.02%	8.57%	3.39%

Notes: Table reports first stage results of estimating equation (5) by OLS in column 1; remaining columns report estimates of variants of estimating equation (4) by IV. Unit of observation is a State-year in all columns. Dependent variables are various measures of health care expenditures from the Health Care Finance Administration (HCFA). HCFA data are available in 1972, 1976 - 1978, and 1980-1990. All dependent variables and income are in logs and divided by a hospital-utilization weighted measure of population (HUWP). In all columns income is divided by HUWP before taking logs. Sample is Southern states in Panel A and All U.S. (except Alaska and Virginia) in Panel B. 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 6: Labor Income vs. All Income

Panel A: IV Results

Dependent Variable: Hospital Expenditures

	(1)	(2)	(3)	(4)
log(Income)$_{jt}$	0.550	0.451	0.740	0.568
	(0.230)	(0.160)	(0.359)	(0.263)
	[0.030]	[0.013]	[0.045]	[0.036]
R^2	0.992	0.993	0.981	0.982
N	326	326	1015	1015

Panel B: First Stage Results

Dependent Variable: Income

	(1)	(2)	(3)	(4)
Oil Reserves$_j$ ×	2.564	3.128	2.220	2.895
log(oil price)$_{t-1}$	(0.523)	(0.851)	(0.443)	(0.682)
	[0.000]	[0.002]	[0.000]	[0.000]
R^2	0.989	0.990	0.985	0.983
N	326	326	1015	1015
F-statistic	24.05	13.50	25.10	18.05

Specification

	(1)	(2)	(3)	(4)
Income definition	Payroll	GSP	Payroll	GSP
Geographic sample	South	South	USA	USA

Notes: Table reports estimates of variants of estimating equation (4) by IV in Panel A and equation (5) by OLS in Panel B. Unit of observation is a State-year in all columns. 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 as in the baseline specification or Gross State Product (GSP)) 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 and 2; columns 3 and 4 expand sample to all US (except Alaska and Virginia). Column 1 reproduces results from column 6 in Table 3. All regressions 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)	(2)	(3)	(4)	(5)	(6)
	Industry-specific Gross State Product					
	Amuse-ment	Hotels	Legal Services	Other Services	Food	Health Services
	Panel A: Southern States Only					
log(Income)$_{jt}$	0.900	0.835	1.635	1.375	-0.009	-0.048
	(0.385)	(0.319)	(0.317)	(0.387)	(0.416)	(0.181)
	[0.034]	[0.019]	[0.000]	[0.003]	[0.984]	[0.793]
R^2	0.984	0.984	0.991	0.989	0.965	0.996
N	326	326	326	308	324	326
	Panel B: All U.S.					
log(Income)$_{jt}$	1.080	0.940	1.749	1.400	0.255	0.207
	(0.384)	(0.397)	(0.291)	(0.270)	(0.356)	(0.412)
	[0.007]	[0.022]	[0.000]	[0.000]	[0.477]	[0.617]
R^2	0.975	0.978	0.988	0.984	0.977	0.994
N	1013	1015	1015	989	1013	1015

Notes: Table reports results from estimating variants of 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. 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. All columns include state and year fixed effects. 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

Dependent Variable: Hospital Expenditures				
	(1)	(2)	(3) Reduced Form	(4) Reduced Form
Regression:	IV	IV	OLS	OLS
Oil Reserves$_j$ ×			6.680	10.567
log(oil price)$_{t-1}$			(2.099)	(7.511)
			[0.006]	[0.180]
log(Income)$_{jt}$	0.725	0.833		
	(0.216)	(0.369)		
	[0.005]	[0.040]		
log(Income)$_{jt}$ ×		-0.066		
log(Income)$_{j,t=1970}$		(0.143)		
		[0.652]		
{ Oil Reserves$_j$ ×				-487.728
log(oil price)$_{t-1}$ }^{2}				(717.177)
				[0.507]
R^2	0.967	0.965	0.973	0.973
N	2054	2054	2065	2065
-1 standard deviation	0.725	0.862	6.680	11.855
Marginal Effect at Mean	0.725	0.833	6.680	10.567
+1 standard deviation	0.725	0.804	6.680	9.278

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 3 and 4. The unit of anlaysis is an Economic Sub Region (ESR)-year, and the regressions include ESR fixed effects and year fixed effects. All dependent variables are in logs. In all columns hospital expenditures and income are divided by a hospital-utilization weighted measure of population (HUWP) before taking logs. The sample is all Southern states between 1970 and 1990. Note that the results in columns 1 and 3 differ slightly from baseline results 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

	(1)	(2)	(3)	(4)	(5)
	Panel A: IV Results				
	Dependent Variable: Hospital Expenditures				
	(1)	(2)	(3)	(4)	(5)
log(Income)$_{jt}$	0.723	0.700	0.804	*N/A*	0.853
	(0.214)	(0.368)	(0.633)		(0.439)
	[0.004]	[0.106]	[0.210]		[0.071]
R^2	0.968	0.967	0.956		0.970
N	2065	1070	4915		3547
	Panel B: First Stage Results				
	Dependent Variable: Income				
	(1)	(2)	(3)	(4)	(5)
Oil Reserves$_j$ ×	9.245	6.237	7.094	1.481	7.966
log(oil price)$_{t-1}$	(2.271)	(1.655)	(2.375)	(1.882)	(1.930)
	[0.001]	[0.009]	[0.004]	[0.443]	[0.001]
R^2	0.983	0.985	0.982	0.984	0.986
N	2065	1070	4915	3547	3547
F-statistic	16.58	14.21	8.92	0.62	17.04
	Specification				
	(1)	(2)	(3)	(4)	(5)
Years	1970-1990	1970-1990	1970-1990	1970-2005	1970-2005
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 estimates of variants of estimating equation (4) by IV in Panel A and equation (5) by OLS in Panel B. All dependent variables and income are in logs and divided by a hospital-utilization weighted measure of population (HUWP) before taking logs. Unit of analysis is an Economic Sub Region (ESR)-year in all columns, and all columns include ESR fixed effects and year fixed effects. 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. 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 4, 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$_j$ ×	9.245	6.680		7.621	6.050		
log(oil price)$_{t-1}$	(2.271)	(2.099)		(2.643)	(2.628)		
	[0.001]	[0.006]		[0.011]	[0.036]		
log(Income)$_{jt}$			0.723			0.794	0.826
			(0.214)			(0.411)	(0.231)
			[0.004]			[0.073]	[0.003]
R^2	0.983	0.973	0.968	0.986	0.986	0.981	0.976
N	2065	2065	2065	296	296	296	690
F-statistic	16.577			8.318			

Notes: Table reports results of estimating equations (4), (5) or (6) by OLS or IV as indicated. All dependent variables are in logs. Unit of analysis is an Economic Sub Region (ESR)-Year, and all columns include ESR fixed effects and year fixed effects. In all columns income and hospital expenditures 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. 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: IV and Reduced Form OLS Results							
Dependent Variable: Hospital Expenditures							
	IV	IV	IV	IV	IV	RF	RF
$\log(\text{Income})_{jt}$	0.723	0.992	0.697	0.352	0.131		
	(0.214)	(0.306)	(0.283)	(0.192)	(0.118)		
	[0.004]	[0.005]	[0.027]	[0.088]	[0.286]		
Oil Reserves$_j$ × $\log(\text{oil price})_{t-1}$						4.980	-3.107
						(1.656)	(4.044)
						[0.009]	[0.455]
Oil Reserves$_j$ × $\log(\text{oil price})_{t+5}$		-11.322					
		(7.830)					
		[0.169]					
R^2	0.968	0.964	0.970	0.972	0.976	0.966	0.980
N	2065	2065	2054	2065	2065	1471	1487
Panel B: First Stage Results							
Dependent Variable: Income							
Oil Reserves$_j$ × $\log(\text{oil price})_{t-1}$	9.245	8.186	8.219	11.722	13.774	14.172	
	(2.271)	(2.157)	(2.387)	(3.004)	(3.951)	(3.481)	
	[0.001]	[0.002]	[0.004]	[0.001]	[0.003]	[0.001]	
Oil Reserves$_j$ × $\log(\text{oil price})_{t+5}$		4.821					
		(3.291)					
		[0.164]					
R^2	0.983	0.983	0.984	0.984	0.985	0.986	
N	2065	2065	2054	2065	2065	1471	
F-statistic	16.577	14.396	11.853	15.222	12.154	16.571	

Notes: Table reports results from estimating variants of equation (4) by IV in Panel A, except in columns 6 and 7 which show variants of equation (6) estimated by OLS in Panel A; table reports results from estimating variants of equation (5) by OLS in Panel B. All dependent variables are in logs. In all columns 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 (see column 7 of Table 2 and 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 variables: hospital expenditures in 1969, hospital beds in 1969, population in 1970, wage bill in 1970, employment in 1970. Column 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

Panel A: IV Results

Dependent Variable: Hospital Expenditures

	(1)	(2)	(3)	(4)	(5)	(6)
log(Income)$_{jt}$	0.723	0.491	0.640	0.632	1.095	0.860
	(0.214)	(0.145)	(0.194)	(0.205)	(0.670)	(0.870)
	[0.004]	[0.004]	[0.005]	[0.008]	[0.123]	[0.339]
R^2	0.968	0.971	0.969	0.970	0.962	0.966
N	2065	2065	2065	2065	2065	2065

Panel B: First Stage Results

Dependent Variable: Income

	(1)	(2)	(3)	(4)	(5)	(6)
Oil Reserves$_j$ ×	9.245					
log(oil price)$_{t-1}$	(2.216)					
	[0.001]					
Oil Reserves$_j$ ×		0.886				
oil price$_{t-1}$		(0.200)				
		[0.000]				
Oil Reserves$_j$ ×			10.080			
log(oil price)$_t$			(2.467)			
			[0.001]			
max(Oil Reserves,				12.646		
95th percentile) ×				(2.523)		
log(oil price)$_{t-1}$				[0.000]		
1{Oil Reserves > 0} ×					0.041	
log(oil price)$_{t-1}$					(0.014)	
					[0.012]	
1{Oil Reserves > 0} ×						0.808
Mining share of labor force in 1970 ×						(0.240)
log(oil price)$_{t-1}$						[0.004]
R^2	0.983	0.984	0.983	0.983	0.984	0.983
N	2065	2065	2065	2065	2065	2065
F-statistic	17.41	19.71	16.69	25.12	8.22	11.36

Notes: Table reports estimates of variants of estimating equation (4) by IV in Panel A and equation (5) by OLS in Panel B. The specifications vary in their definition of the instrument, which is given in the left-hand column of Panel B. Unit of analysis is an Economic Sub Region (ESR)-year. All dependent variables are in logs. In all columns 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. All columns include ESR 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: IV-GLS and Lagged Depedendent Variable

	Dependent Variable: Hospital Expenditures						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Baseline IV	IV-GLS	IV-GLS	IV-GLS	IV-GLS	Lagged Dep. Var. IV	Arellano-Bond IV
Within-panel serial correlation	Cluster at State	Common AR(1)	Common AR(2)	State-specific AR(1)	State-specific AR(2)	Cluster at State	Cluster at State
log(Income)$_{jt}$	0.697	0.963	1.111	0.724	0.770	0.493	0.120
(*A*)	(0.216)	(0.505)	(0.681)	(0.263)	(0.287)	(0.134)	(0.067)
	[0.006]	[0.057]	[0.103]	[0.006]	[0.007]	[0.002]	[0.075]
log(Total Hospital Exp.)$_{t-1}$						0.428	0.154
(*B*)						(0.088)	(0.047)
						[0.000]	[0.001]
Implied long-run effect						0.859	0.142
(*A*/(1-*B*))						(0.213)	(0.080)
						[0.001]	[0.077]
N	2016	2016	2016	2016	2016	1966	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 and hospital expenditures are divided by a hospital-utilization weighted measure of population (HUWP) before taking logs. For columns 1 through 5, the baseline sample is modified to only include the 96 (of 99) ESRs with data for all 21 years between 1970 and 1990. Column 1 produces baseline IV results with this modified sample. Columns 2 through 5 report IV-GLS results. In column 2, ρ_1 is estimated to be 0.585. In column 3, ρ_1 is estimated to be 0.508 and ρ_2 is estimated to be 0.127. In column 4, ρ_1 is estimated separately by state; estimated values of ρ_1 range from 0.155 to 0.887 with mean 0.604 and s.d. 0.240. In column 5, ρ_1 and ρ_2 are estimated separately by state; estimated values of ρ_1 range from 0.118 to 0.747 with mean 0.487 and s.d. 0.200, and estimated values of ρ_2 range from 0.041 to .341 with mean 0.192 and s.d. 0.083. Column 6 includes a lagged dependent variable as a control. Column 7 uses the Arellano-Bond GMM dyamic panel estimator. In columns 6 and 7 the standard error on the implied long-run effect is estimated using the delta method.

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: IV Results

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)$_{jt}$	0.723	0.702	0.725	0.695	0.725	0.694	0.838	0.714	0.706	0.655	0.782	0.677	0.823	0.764	0.680	0.461	0.750
	(0.214)	(0.226)	(0.216)	(0.216)	(0.216)	(0.219)	(0.183)	(0.212)	(0.272)	(0.215)	(0.214)	(0.235)	(0.184)	(0.223)	(0.222)	(0.695)	(0.248)
	[0.004]	[0.008]	[0.005]	[0.006]	[0.005]	[0.007]	[0.000]	[0.005]	[0.021]	[0.009]	[0.003]	[0.012]	[0.001]	[0.004]	[0.009]	[0.518]	[0.009]
R^2	0.968	0.969	0.967	0.969	0.967	0.968	0.967	0.968	0.969	0.970	0.967	0.968	0.967	0.969	0.968	0.974	0.969
N	2065	1877	1918	2044	2054	2002	1900	1897	1939	1981	1939	1918	1897	1939	1918	1813	1939

Panel B: First Stage Results

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$_j$ ×	9.245	9.312	9.386	9.182	9.205	9.236	9.347	9.874	8.997	9.007	9.349	8.660	8.892	9.051	9.164	21.641	8.236
log(oil price)$_{t-1}$	(2.216)	(2.375)	(2.303)	(2.222)	(2.215)	(2.285)	(2.356)	(2.363)	(2.015)	(2.221)	(2.303)	(2.127)	(1.678)	(2.265)	(2.311)	(4.879)	(1.831)
	[0.001]	[0.002]	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]	[0.000]	[0.001]	[0.001]	[0.001]	[0.001]
R^2	0.983	0.983	0.983	0.983	0.983	0.983	0.984	0.983	0.984	0.984	0.983	0.983	0.983	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	17.41	15.37	16.61	17.08	17.26	16.33	15.73	17.45	19.93	16.45	16.48	16.58	28.09	15.97	15.73	19.67	20.24

Notes: Table reports estimates of variants of estimating equation (4) by IV in Panel A and equation (5) by OLS in Panel B. In all specifications income 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 (see column 7 of Table 2 and column 3 of Table 3). Subsequent columns show the results when the state specified in the column heading is omitted 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: This table reports results from the Monte Carlo study described in Appendix B. Each cell displays the mean of the parameter estimates from 500 simulations; standard deviation of parameter estimates is reported below in parentheses.