

INCOME AND HEALTH SPENDING: EVIDENCE FROM OIL PRICE SHOCKS

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Abstract—Health expenditures as a share of GDP in the United States have more than tripled over the past half-century. A common conjecture is that this is a consequence of rising income. We investigate this hypothesis by instrumenting for local area income with time series variation in oil prices interacted with local oil reserves. This strategy enables us to capture both partial equilibrium and local general equilibrium effects of income on health expenditures. Our central income elasticity estimate is 0.7, with 1.1 as the upper end of the 95% confidence interval, which suggests that rising income is unlikely to be a major driver of the rising health expenditure share of GDP.

I. 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 past half-century, from 5% in 1960 to 18% in 2009 (CMS, 2010). A common conjecture is that the rise in the share of income spent on health care expenditures is a direct, or at least a natural, consequence of the secular increase in living standards because health care is a “luxury good.” The *Economist* magazine stated this as “conventional wisdom” in 1993: “As with luxury goods, health spending tends to rise disproportionately as countries become richer” (quoted in Blomqvist & 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 past half-century (OECD, 2004) 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 U.S. (and OECD) health care sector over the past half-century. Second, it is necessary for forecasting how health care spending is likely to evolve. Finally, it is a crucial first step toward 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.¹

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 0 to substantially above 1.² In light of the paucity of existing evidence, Hall and Jones (2007) conclude their paper by stating, “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 covary 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

Received for publication May 20, 2010. Revision accepted for publication May 18, 2012.

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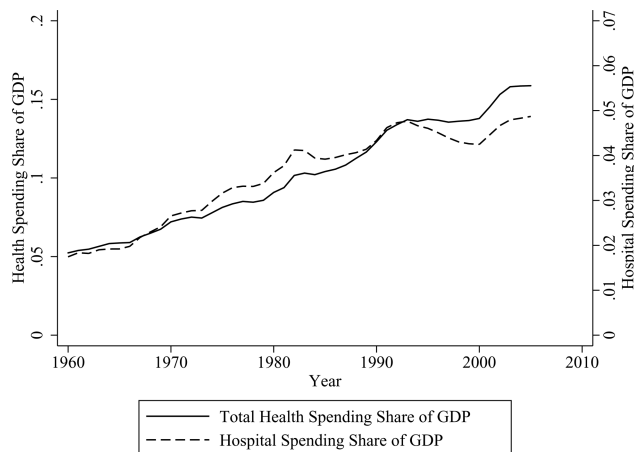
We are grateful to Amitabh Chandra, Bob Hall, Chad Jones, Guy Michaels, Joe Newhouse, Mark Watson (the editor), two anonymous referees, and participants at the NBER Health Care meetings and NBER Labor Studies Summer Institute for helpful comments, to James Wang for outstanding research assistance, and to the National Institute of Aging (NIA grant numbers P30-AG012810 and T32-AG000186) for financial support.

A supplemental appendix is available online at http://www.mitpressjournals.org/doi/suppl/10.1162/REST_a_00306.

¹ Of course, a large role for income would be only 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.

² 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 (Newhouse & Phelps, 1976). By contrast, cross-country analysis tends to suggest income elasticities greater than 1 (Newhouse, 1977; Gerdtham & Jonsson, 2000), as do time series analyses of the relationship between income growth and growth in health spending for individual countries (Fogel, 1999).

FIGURE 1.—HEALTH SPENDING TRENDS, 1960–2005



Source: CMS (2006).

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 or because, in line with Baumol (1967), health care is complementary to other sectors but subject to slower productivity growth.³ Alternatively, if the supply of health care is less than perfectly elastic and the price elasticity of demand for health care is greater than 1, 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 important national or even global general equilibrium effects, which will be harder to detect empirically.

We confront both of these challenges: By exploiting plausibly exogenous variation in local area incomes, we attempt to estimate causal elasticities that incorporate local general equilibrium effects. Our strategy is to exploit the time series variation in global oil prices between 1970 and 1990 that affected 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 subregions (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

³ 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.

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 preexisting 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 an elasticity of ESR-level hospital spending with respect to ESR-level income of 0.72 (standard error = 0.21). 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 use our local general equilibrium income elasticity estimate to perform a back-of-the-envelope calculation of the role that rising income may have played in the rising U.S. health share. Our central point estimate of 0.72 suggests that rising income would be associated with a modest decline in the health share of GDP. Perhaps more informatively, the upper end of the 95% confidence interval of this estimate is 1.13; this 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 U.S. GDP between 1960 and 2005.

We discuss several important caveats to this out-of-sample extrapolation. One set of concerns revolves around the fact that our estimates correspond to local general equilibrium effects of income changes but will not capture any global or national general equilibrium effects. Two such effects that could potentially increase the income elasticity of health expenditures above what we have estimated are induced innovations (which could occur at the national or global level) and national political economy responses to rising income. While we cannot rule out an important role for such mechanisms, we present empirical and theoretical evidence suggesting that national or global general equilibrium income effects may not be substantively large in this setting. A second set of concerns relates to the fact 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; substantial heterogeneity in the income elasticity of health expenditures would suggest caution in any out-of-sample extrapolations. Again, our ability to address this is naturally limited, but we present a variety of estimates that we interpret as suggesting that heterogeneity in elasticities is not likely to lead to serious underestimation in our extrapolation to the effect of rising incomes on health expenditures. Finally, a third set of concerns relates to the nature of our data, particularly the fact that our empirical work focuses primarily on hospital expenditures rather than on total health expenditures. Here too the evidence suggests that our elasticity estimates for hospital spending are likely to be representative of those for total health expenditures.

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 U.S. context. This empirical relationship is undoubtedly partly shaped by several specific institutional features of the U.S. 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). Throughout we use the term *luxury good* to designate an empirical income elasticity greater than 1 (and similarly, *necessity* refers to an elasticity less than 1) while recognizing that this responsiveness to income may result from preferences, policy, or other factors.

To our knowledge, our paper represents the first empirical attempt to estimate the causal general equilibrium income elasticity of health spending.⁴ Indeed, we are aware of only 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 1. The Rand Health Insurance Experiment finds that a small, unanticipated, temporary increase in one’s income has no significant impact on one’s own health expenditures or utilization (Newhouse and the Insurance Experiment Group, 1993, p. 78).⁵

The rest of the paper proceeds as follows. Section II describes our empirical strategy and data. Section III contains our main results. It shows the first-stage relationship between ESR income and our instrument and presents our instrumental variable estimates of the local general equilibrium income elasticity of hospital expenditures and their components. Section IV 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 then discusses in some depth some of the most salient potential

threats to extrapolating from our estimates in this manner. Section V concludes. The online appendixes provide further theoretical and empirical results.

II. Empirical Strategy and Data

A. Motivating Theory

We start with a simple theoretical model that provides a framework for interpreting our results. The model clarifies the distinction among three different income elasticities: the partial equilibrium income elasticity that measures the responsiveness of health spending to a change in an individual’s income, the local general equilibrium income elasticity from a change in an area’s income, and the global general equilibrium income elasticity that measures the responsiveness of health spending to national or global income changes.

Consider an individual i residing in area j at time t with a utility function given by

$$\pi(Q_{jt}h_{ijt})u(c_{ijt}), \quad (1)$$

where h_{ijt} denotes this individual’s health expenditures and c_{ijt} corresponds to his nonhealth consumption expenditures; Q_{jt} is the quality of health care per unit of health care expenditure in area j at time t . By *area*, we mean the geographic areas approximating local health care markets. In our empirical work, we will look at economic subregions and then aggregate the data to the state level to investigate whether some of these technology and policy responses might be more pronounced at a higher level of aggregation. The functional form in equation (1), that is, multiplicatively separable in health and nonhealth consumption, is adopted both to simplify the exposition and to link our equations to Hall and Jones (2007), whose dynamic model also has a static representation identical to this equation (1). The budget constraint of the individual is written as

$$c_{ijt} + h_{ijt} \leq y_{ijt}, \quad (2)$$

where both c_{ijt} and h_{ijt} are expenditures (and we therefore have no prices on the right-hand side; the relative price of health care is already incorporated into Q_{jt}). Assuming that both π and u are concave and differentiable, the individual’s optimal demand for health expenditures leads to the following simple equation for the share of income spent on health,

$$\frac{h_{ijt}}{y_{ijt}} = \frac{\eta_{\pi_{ijt}}/\eta_{u_{ijt}}}{1 + \eta_{\pi_{ijt}}/\eta_{u_{ijt}}}, \quad (3)$$

where $\eta_{\pi_{ijt}} \equiv Q_{jt}h_{ijt}\pi'(Q_{jt}h_{ijt})/\pi(Q_{jt}h_{ijt})$ and $\eta_{u_{ijt}} \equiv c_{ijt}u'(c_{ijt})/u(c_{ijt})$ are the elasticities of the π and u functions evaluated at the expenditure levels of individual i in area j and time t .

As Hall and Jones (2007) emphasized, we expect the share of income spent on health to increase as incomes rise if $\eta_{u_{ijt}}$

⁴Our empirical strategy is related to that used by Michaels (2007) to estimate the long-run consequences of resource-based specialization and to those in Buckley (2003) and Black, McKinnish, and Sanders (2005). Michaels also exploits variation in oil abundance across county groups within the U.S. South and studies the consequences of the availability of greater oil resources on changes in the sectoral composition of employment and in education. Buckley (2003) exploited the same source of variation within Texas to investigate the effect of income on marriage and divorce. Black et al. (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 sectors and macroeconomic aggregates. None of these papers studies the effect of income on the health care sector.

⁵These results are from the so-called Super Participation Incentive in which a subsample 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 subexperiment 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) affected utilization.

decreases more rapidly than $\eta_{\pi_{ijt}}$ with income. However, the behavior of the quality of health care in the area, Q_{jt} , also plays an important role in the evolution of health expenditures. To see this more clearly, we take logs of both sides of equation (3) and then take a first-order Taylor expansion of $\log[(\eta_{\pi_{ijt}}/\eta_{u_{ijt}})/(1 + \eta_{\pi_{ijt}}/\eta_{u_{ijt}})]$ in terms of $\log h_{ijt}$, $\log Q_{jt}$, and $\log y_{ijt}$ and rearrange to write

$$\log h_{ijt} = \zeta \log Q_{jt} + \tilde{\beta} \log y_{ijt} + \xi_{ijt}, \quad (4)$$

where ξ_{ijt} is an error term capturing approximation errors as well as any omitted factors. In practice, we expect the error term to have a representation of the form $\xi_{ijt} = \tilde{\alpha}_j + \tilde{\gamma}_t + \tilde{\varepsilon}_{ijt}$, with $\tilde{\alpha}_j$ and $\tilde{\gamma}_t$ corresponding to systematic differences in the demand for health care across areas and over time. In equation (4), $\tilde{\beta}$ measures the individual income elasticity for health expenditures holding Q_{jt} constant. This is the elasticity we would measure if we could have random variation in individual incomes within an area, holding quality of health care Q_{jt} constant; it thus corresponds to what we referred to as the partial equilibrium income elasticity.

In general equilibrium, income changes may affect the quality of health care.⁶ In this context, it is important to distinguish between local general equilibrium effects—corresponding to the effects of changes in income in area j on health expenditures working through their effects on Q_{jt} —and national (or global) general equilibrium effects—whereby changes in national (or global) income have an impact on health expenditures through their effect on some frontier quality or the aggregate of the area qualities, that is, the aggregate of the Q_{jt} s. Examples of local general equilibrium effects of area income on area health care quality would include hospital entry and technology adoption decisions in response to changes in local income and local health policy decisions (such as funding of public hospitals or state-level public health insurance eligibility rules) that are responsive to local area income. Examples of national or global general equilibrium effects of income would include the development of new technologies induced by national or global income changes and the responsiveness of national health policy decisions (such as Medicare policy) to national income.

To capture these two distinct mechanisms, we write

$$\log Q_{jt} = \bar{\alpha}_j + \kappa_0 \log y_{jt} + \kappa_1 \log y_t + \lambda_1 s_t, \quad (5)$$

where y_{jt} is average (per capita) income in area j at time t and y_t is average national income, κ_0 measures local general equilibrium effects, and κ_1 captures national or global general equilibrium effects.⁷ In addition, $\bar{\alpha}_j$ captures other sources of variation (orthogonal to income) in the quality of health care across areas, and s_t captures other factors (orthogonal to income) affecting the quality of health care in the aggregate,

⁶ Our use of the term *quality* does not imply a normative assessment of the net social benefit of changes in Q_{jt} . Instead, Q_{jt} stands for factors increasing the demand for health care.

⁷ For induced innovation, we could also take y_t to represent average income in the OECD.

such as autonomous scientific advances. Substituting equation (5) into equation (4), averaging across all individuals within area j , and proxying the average of logs with the log of the average, we obtain

$$\log h_{jt} \simeq \alpha_j + \gamma_t + \beta \log y_{jt} + \varepsilon_{jt}, \quad (6)$$

where h_{jt} is average health expenditure in area j at time t , and we have $\alpha_j \simeq \bar{\alpha}_j + \zeta \bar{\alpha}_j$, $\gamma_t \simeq \tilde{\gamma}_t + \zeta(\kappa_1 \log y_t + \lambda_1 s_t)$, and $\beta \simeq \tilde{\beta} + \zeta \kappa_0$. Note also that equation (6) could have been equivalently written in its aggregate form, with the log of total area health expenditure, $\log H_{jt}$, on the left-hand side and the log of total area income, $\log Y_{jt}$, on the right-hand side. In our empirical work, it will be more convenient to start with this version, though our main estimates will come from equations expressed in variants of per capita units as in equation (6).

Equation (6) emphasizes that the income elasticity β we estimate will differ from the partial equilibrium income elasticity ($\tilde{\beta}$) due to local general equilibrium effects ($\zeta \kappa_0$). For example, when the income of a single individual in an area increases, the types of health care that this individual has access to will remain constant, and this may limit his willingness to spend on health care. In contrast, if the entire area becomes more prosperous, local hospitals may adopt new technologies or new practices that increase the willingness of (a subset of) the local population to spend on health care. Thus, while the partial equilibrium income elasticity $\tilde{\beta}$ might be small, the local general equilibrium elasticity β could be substantially larger.⁸

Equation (6) also emphasizes that national or global general equilibrium income effects, such as induced innovation or national policy responses, are absorbed by the time effects, the γ_t s, and are thus not captured in our estimates of β . To the extent that these national or global general equilibrium income effects are quantitatively important, our estimates will understate the national or global relationship between income and health. We view this as an important but inevitable limitation of our empirical strategy, which attempts to obtain causal estimates of the impact of income on health expenditures. We are not aware of alternative empirical strategies that could generate convincing estimates of national and global general equilibrium effects of rising income; in particular, any pure time series strategy would confound the effects of income with those represented by s_t in equation (5). Instead, our strategy is to provide credible estimates that incorporate both partial equilibrium and local general equilibrium income effects. We then draw on supplementary evidence to try to gauge whether there are likely to be quantitatively important national general equilibrium income effects not captured by our analysis. As we discuss in section IVB, this supplementary evidence and analysis suggest that the national relationship between income and health expenditures is not significantly different from the relationship estimated from our empirical approach.

⁸ As noted in section I, β could also be smaller than $\tilde{\beta}$.

B. Empirical Strategy

Let us first write equation (6) in its aggregate form and with covariates as follows,

$$\log H_{jt} = \alpha_j + \gamma_t + \beta \log Y_{jt} + \mathbf{X}_{jt}^T \boldsymbol{\phi} + \varepsilon_{jt}, \quad (7)$$

where Y_{jt} is total area income, \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 total hospital expenditures in area j at time t . The α_j s are area fixed effects measuring any time-invariant differences across the different geographic areas. The γ_t s are year fixed effects, capturing any common (proportional) changes in health care spending each year.⁹ For convenience and transparency, we begin by estimating this aggregate form of equation (6) and then turn to estimating variants of the per capita specification shown in this equation.

The simplest strategy would be to estimate β in equation (7) 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 downward. If instead income is positively correlated with insurance coverage and insurance encourages increased health care spending, OLS estimates would be biased upward.

Our empirical strategy attempts to isolate potentially exogenous sources of variation in local area income, Y_{jt} (or equivalently in local per capita income, y_{jt} , in later specifications). 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 production plays a more or less significant role in the local economy. In particular, we instrument for $\log Y_{jt}$ in equation (7) with the following first-stage regression:

$$\log Y_{jt} = \alpha'_j + \gamma'_t + \delta(\log p_{t-1} \times I_j) + \mathbf{X}_{jt}^T \boldsymbol{\phi}' + u_{jt}, \quad (8)$$

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'_j s and γ'_t s are defined similarly to the α_j s and γ_t s in equation (7). 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 a lag in the translation of oil price changes into income changes. We show in the online appendix (section B) 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; these 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_j s and α'_j s) in equations (7) and (8). 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 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 section IIC that areas of the southern United States that differ in terms of the role of oil in the local economy— I_j in equation (8)—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 twenty years). More important, in the online appendix (section B), 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 to 1990, which encompasses the major oil boom and bust and uses economic subregions (ESRs) as our geographic units (local economies). We construct our ESRs by splitting these subregions 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

C. Data and Descriptive Statistics

Estimation of equations (7) and (8) 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 1 provides summary statistics on some of our main variables.

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 2,065 observations instead of $99 \times 21 = 2,079$ 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 the sample to ESRs that appear in all years does not affect results.

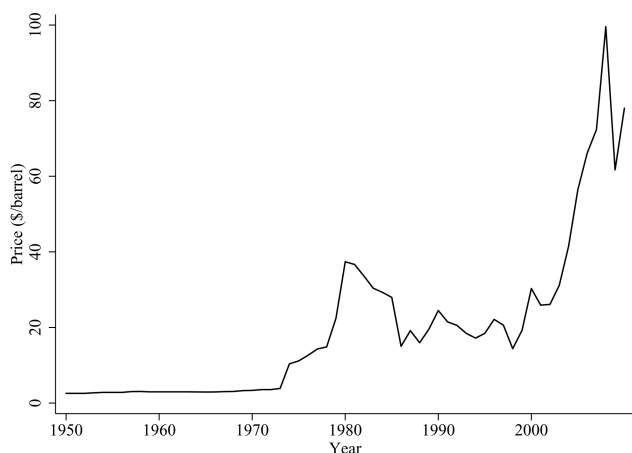
⁹ The specification with the dependent variable, hospital expenditures, in logs rather than in levels is attractive both because the distribution of hospital expenditures across areas is highly right skewed (see figure 5 below) and because it implies that year fixed effects correspond to constant proportional (rather than constant level) changes in health spending across all areas.

TABLE 1.—DESCRIPTIVE STATISTICS

Variable	ESR-Year Data		State-Year Data	
	Mean	SD	Mean	SD
<i>Oil and Gas Data Book Data</i>				
Oil reserves (million barrels)	532.3	1,596.1	3,371.7	9,124.3
<i>County Business Patterns Data</i>				
Total income (payroll) (\$millions)	2,916.9	6,066.7	18,494.4	20,751.0
Total employment (millions)	0.21	0.35	1.32	1.13
<i>AHA hospital data</i>				
Total expenditures (\$millions)	292.61	636.31	1,854.22	2,257.94
Hospital payroll (\$millions)	139.87	284.12	886.40	1,011.38
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
Number of technologies	46.98	18.10	48.37	19.53
Number of hospitals	24.67	26.57	156.43	126.71
<i>Current population Reports and NHIS data</i>				
Population (millions)	0.68	0.89	4.44	3.40
HUWP (millions)	0.60	0.84	3.88	3.02
<i>BEA GSP data (all in \$millions)</i>				
Total GSP (industry-specific GSPs)			54,559.5	60,731.7
Health services			1,639.9	2,182.0
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
<i>Health Care Financing Administration data (all in \$millions)</i>				
Total health care expenditures			5,923.8	6,447.2
Hospital expenditures			2,641.1	2,663.7
Physician and other services			1,626.0	2,065.4
Dental services			303.3	330.7
Drugs and other medical nondurables			654.7	685.4
Vision products			106.2	130.8
Nursing care			390.7	382.2
Other health services			201.8	425.2

Summary statistics in columns 1 and 2 are for the baseline sample of 99 ESRs in the sixteen southern states between 1970 and 1990 (all statistics are ESR-year); columns 3 and 4 report summary statistics for the state-year data for the same baseline sample of sixteen southern states between 1970 and 1990. BEA and HCFA data are available only at the state level. $N = 2065$ at ESR-year except for RN/(LPN+RN), which is 1,576, 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 available only 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 available only in 1972, 1976–1978, and 1980–1990. HUWP is a hospital-utilization weighted measure of population. See the text for details.

FIGURE 2.—ANNUAL OIL PRICES, 1950–2010

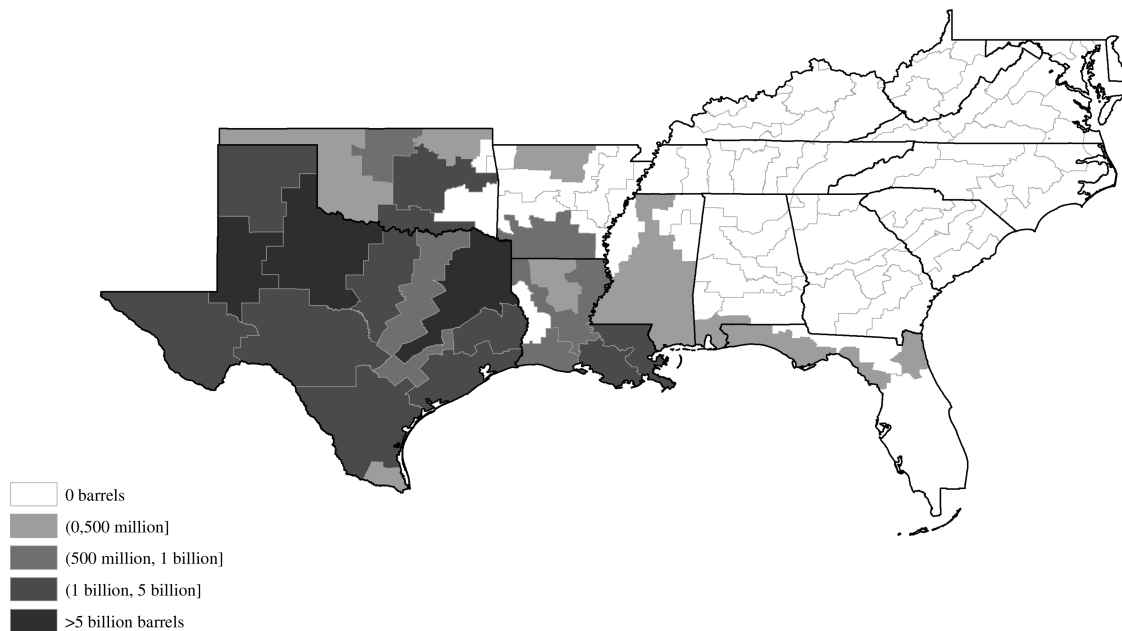


Average annual oil prices calculated from the monthly spot prices in the West Texas Intermediate series. The data are available at <http://research.stlouisfed.org/fred2/series/OILPRICE/downloaddata?cid=98>.

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 to 1990, the two decades that 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. As documented by several researchers (Hamilton, 2009; Kline, 2008), oil price shocks appear to be permanent. In section C.3 in the online appendix, we present our own analysis of the time series, which is also consistent with changes in oil prices being permanent. This suggests that if individuals correctly understand oil price changes to be permanent, our empirical strategy will be informative about the effects of permanent (rather than transitory) changes in income on health care expenditures.

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

FIGURE 3.—MAP OF LARGE OIL WELL RESERVES



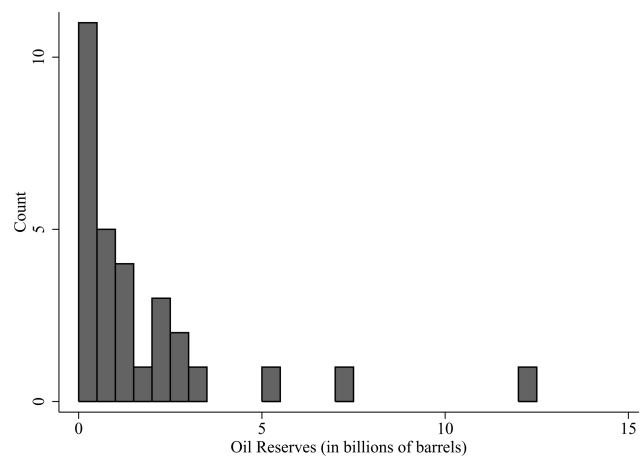
This map displays the total amount of oil in large oil wells for each economic subregion (ESR) in the South. Large oil wells are defined as having ever had more than 100 million barrels of oil. Source: The data come from the 2000 edition of the *Oil and Gas Data Book*.

Oil intensity. Our primary measure of the oil intensity of area j is an estimate of the total oil reserves in that area (since discovery). We draw on data from the 2000 edition of the *Oil and Gas Journal Data Book*, which includes information on all 306 oil wells in the United States of more than 100 million barrels in total size. Total oil reserves are calculated as estimated remaining reserves plus total cumulative oil production as of 1998; they are thus not affected by the prior intensity of oil extraction in the area. Throughout, we refer to these as large oil wells. Our baseline analysis is limited to the southern United States, which contains 161 of the 306 large oil wells in the United States and 51% of the total oil reserves of these oil wells.¹³

Figures 3 and 4 show the cross-sectional variation in oil reserves across different areas of the South. These figures indicate 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% (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 United States is more than 2,500 million barrels (relative to a mean reserve conditional on having any reserves of 1,700 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.

¹³ 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, Texas, in 1971). Excluding this well has no effect on our results. There are also 60 (out of the 306) oil wells that are located offshore and thus were not assigned to any county. These offshore wells account for 12% of the oil reserves in the data.

FIGURE 4.—DISTRIBUTION OF LARGE OIL WELL RESERVES BY ESR

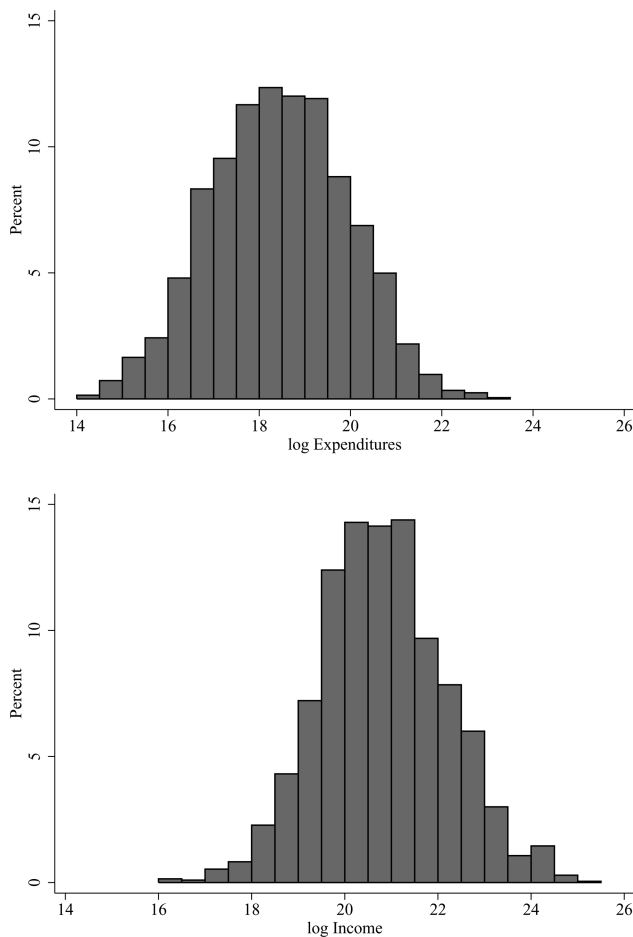


This figure displays the cross-sectional distribution of oil reserves by economic ESR among the ESRs containing large wells. Of the 99 ESRs in the South, 69 ESRs do not have any large oil wells. This figure shows the amount of oil reserves (in billions of barrels) for the 30 ESRs with large oil wells. Source: 2000 edition of the *Oil and Gas Data Book*.

Area income. Our primary data on ESR income come from aggregating up county-level annual payroll (for all establishments) from the County Business Patterns (CBP).¹⁴ We also obtain ESR-level employment data from the CBP in

¹⁴ The CBP is an annual establishment survey of all establishments in the Business Register at the Census Bureau. The CBP data are available online at the Geospatial & Statistical Data Center at the University of Virginia for 1977 through 1997 (<http://fisher.lib.virginia.edu/collections/stats/cbp/county.html>) and at the U.S. Census Bureau for 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>.

FIGURE 5.—AGGREGATE HOSPITAL EXPENDITURE AND INCOME DATA, 1970–1980



Histograms of total hospital expenditures and total income by ESR. Total hospital expenditures come from the American Hospital Association Annual Surveys, and income is measured using the payroll data from the County Business Patterns (CBP). Both variables are displayed in logs.

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 5 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.

Area health spending. Our primary data on area health spending are obtained by aggregating up hospital-level data from the American Hospital Association's (AHA) annual census of all U.S. hospitals. We use these data to construct our main dependent variable: total hospital expenditures in area j and year t . Figure 5 shows a histogram of the log 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 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 LPNs, combined as a proxy for the skill mix.¹⁵

There are three key advantages of the AHA data. First, they are extremely high quality. Relatedly, they appear to be unique among annual subnational 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 3. 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 nonhospital components of health expenditures. In section IVB, we draw on several additional data sources to provide suggestive evidence that the income elasticity of overall health expenditures is not greater than that of hospital expenditures.

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 (U.S. Census Bureau, various states and years; Siegal, 2002).¹⁶

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 2 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 sixteen southern

¹⁵ RN certification requires about twice as many years of training as LPN certification, and RNs are paid substantially higher hourly wages (see Acemoglu & Finkelstein, 2008).

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

TABLE 2.—COMPARING ECONOMIC SUBREGIONS WITH DIFFERENT OIL RESERVES, 1970

Variable	ESRs with Large Oil Wells (1)	ESRs without Large Oil Wells (2)	Coefficient (3)	p-Value (4)
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

All results based on 1970 cross-section of the ESRs in the baseline sample (the 16 southern states). Column 3 reports the coefficient from a regression of oil reserves on the variable in the row heading and a constant term; in these regressions, both dependent and independent variables are standardized to have a standard deviation of 1. Column 4 reports the associated p-value of the test whether the coefficient in column 3 is statistically significantly different from 0, using heteroskedasticity-robust standard errors. The final row of the table reports results from a regression of oil reserves on all of the variables listed in the 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 3.—FIRST STAGE

Geographic Level of Analysis	Economic Subregion (ESR)							State
	Total Income (1)	Total Employment (2)	Population (3)	Income per Capita (4)	Population < 55 (5)	Population > 55 (6)	Income per HUWP (7)	Income per HUWP (8)
Oil Reserves _j × log(oil price) _{t-1}	12.900 (2.980) [0.001]	15.542 (2.572) [0.000]	5.252 (1.491) [0.003]	7.648 (1.937) [0.001]	6.421 (1.756) [0.002]	1.545 (1.531) [0.329]	9.245 (2.271) [0.001]	2.564 (0.523) [0.000]
R ²	0.994	0.969	0.997	0.984	0.997	0.996	0.983	0.989
N	2,065	2,065	2,065	2,065	2,065	2,065	2,065	326
F-statistic	18.74	36.53	12.40	15.58	13.37	1.02	16.58	24.05

The table reports results from estimating variants of equations (8) and (11) by OLS. Dependent variables are defined in column headings and are all in logs; in columns 7 and 8, the dependent variable is income divided by a HUWP. The sample is all southern states between 1970 and 1990. The unit of observation is an ESR-year except in column 8, where it is state-year. All models include ESR fixed effects (or state fixed effects 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.

states in the 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 0, and the magnitude of variation is small (1 standard deviation change in oil reserve is associated with only about one-tenth of 1 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. The online appendix (section B) provides a much more systematic investigation of the validity of our exclusion restriction.

III. Main Results

A. First Stage

Table 3 shows the relationship between ESR income and our instrument. The first column shows the results from estimating equation (8). In this and all subsequent estimates, we allow for an arbitrary variance-covariance matrix within each state.¹⁷ The results in column 1 indicate a positive and strong

first stage: ESRs with greater oil reserves experience greater changes in income in response to oil price changes than areas with less oil. The F-statistic is 18.74. We defer a discussion of the magnitude of the first stage until later in this section.

To examine the sources of the increase in income, column 2 reestimates the first-stage equation (8) 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.¹⁸ In contrast to our source of income variation, about half of the growth in income between 1960 and 2005 is due to increased employment, while the other half is due to increased wages per employee (U.S. Census Bureau, 2008). In section IV, we discuss the possible implications of extrapolating from

¹⁷ Because of concerns of the small sample properties of clustering with only sixteen 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 the online appendix (section B).

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

our income changes to the effects of the secular increase in incomes in the U.S. economy.

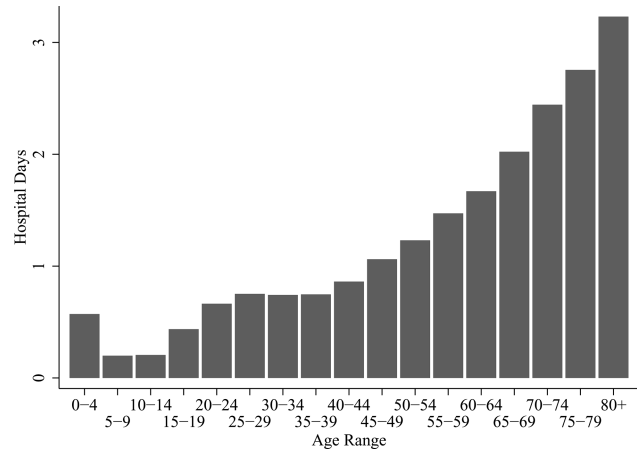
The impact of our instrument on employment and existing evidence on migration responses to local economic conditions (Blanchard & Katz, 1992) suggest that our instrument may also affect area population. Any increase in population in high oil areas relative to low oil areas may increase health expenditures directly, potentially overstating the effect of increased income on hospital spending among a (constant) population. Column 3 explores this issue by reestimating equation (8) 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 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). This also matches more closely our estimating equation (6) from the motivating theory. The first-stage results from estimating equation (8) 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%.

While the per capita specification is natural, it may in turn understate 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 (8) using as the dependent variable the log of the total population under age 55 and the log of the total population age 55 and over, respectively. The results indicate that the population response to our instrument is concentrated among the nonelderly (those under 55). In fact, it appears that the population response is concentrated among those younger than age 45 (not shown in table 3 to save space). Younger individuals consume disproportionately lower amounts of hospital care than the elderly. To illustrate this, figure 6 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. Those 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, although 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 overestimating rather than underestimating

FIGURE 6.—HOSPITAL USAGE BY AGE



Average annual number of hospital days for various age ranges. Source: The data come from the National Health Interview Survey for years 1973 to 1991.

income elasticities), we correct in our baseline analysis 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 (HUWP) 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:

$$HUWP_{jt} = \sum_a pop_{ajt} \times hospdays_a. \tag{9}$$

Our preferred specification adjusts (divides) income in both the structural equation (7) and the first-stage equation (8) and hospital expenditures in the structural equation (7) by $HUWP_{jt}$ as constructed in equation (9). This leads to our baseline structural equation, closely resembling our motivating theoretical equation, (6):

$$\log \tilde{h}_{jt} = \alpha_j + \gamma_t + \beta \log \tilde{y}_{jzt} + \mathbf{X}_{jt}^T \boldsymbol{\phi} + \varepsilon_{jt}, \tag{10}$$

and our baseline first-stage equation:

$$\log \tilde{y}_{jt} = \alpha'_j + \gamma'_t + \delta' (\log p_{t-1} \times I_j) + \mathbf{X}_{jt}^T \boldsymbol{\phi}' + u_{jt}, \tag{11}$$

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 HUWP to capture any direct effect of our instrument on this population.

The estimates of the first-stage coefficient, δ' , from equation (11) 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

TABLE 4.—HOSPITAL EXPENDITURES

Geographic Level of Analysis	Economic Subregion (ESR)					State
	HUWP OLS (1)	HUWP Reduced-Form OLS (2)	HUWP IV (3)	None IV (4)	Per Capita IV (5)	HUWP IV (6)
log(Income) _{7Y}	-0.027 (0.074) [0.723]		0.723 (0.214) [0.004]	0.801 (0.155) [0.000]	0.665 (0.263) [0.023]	0.550 (0.230) [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	2,065	2,065	2,065	2,065	2,065	326

The table reports the results of estimating equations (7), (10), or (12) by OLS or IV, as indicated. The dependent variable is log hospital expenditures. In columns 1, 2, 3, and 6, both hospital expenditures and income are divided by HUWP before taking logs; see equations (10) through (12). 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. The unit of observation is an ESR-year except in column 6, where it is a state-year. All models include ESR fixed effects (or state fixed effects 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.

(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 HUWP adjustment should therefore similarly lie 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 (11) and (10), which correct for the age-adjusted hospital utilization of the population, as our baseline or preferred specification. Because migrants may be healthier than the general population (even conditional on age), the estimate of β from equation (10) 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 person in the area, which seems like a reasonable assumption.¹⁹ In practice, we will see that these bounds on the income elasticity are relatively tight.

Finally, column 8 shows the HUWP-adjusted first stage, now aggregated to the state level (rather than the ESR level as in column 7); the first stage is robust to aggregation to the state level (*F*-statistic = 24.05).²⁰

To gauge the magnitude of the first stage, we calculate in our preferred specification (column 7) that the oil price change from 1970 to 1980 is associated with a 3.6% larger increase in area income in areas with a 1 standard

deviation larger amount of oil. The first stage in our preferred specification has an *F*-statistic of 16.58.

B. Income Elasticity of Hospital Spending and Components

Table 4 presents our central estimates of the impact of income on hospital expenditures. We start with the OLS estimates. As the existing literature has documented (see note 2), the relationship between income and health spending can vary greatly depending on the source of variation. For example, as can be seen in figure 1, the pure time series at the national level implies an income elasticity of health spending that is substantially above 1. Similarly, in our sample of southern states and in our time period (1970–1990), the implied income elasticity of hospital spending is also substantially above 1. Yet column 1 reports the OLS estimate of equation (10) in which both hospital expenditures and income are adjusted for HUWP and the income elasticity estimate of β in (10) is -0.027 (standard error = 0.074). This indicates that when income in an area increases by 10%, hospital expenditures fall by about 0.3%. This relationship is statistically indistinguishable from 0.²¹ 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 equations (10) and (11) (without covariates):

$$\log \tilde{h}_{jt} = \alpha_j'' + \gamma_t'' + \delta''(\log p_{t-1} \times I_j) + \varepsilon_{jt}''. \quad (12)$$

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 (10). The estimated elasticity of health expenditure

¹⁹ This last presumption is both intuitive and consistent with the fact that migration is concentrated among younger individuals (see table 3).

²⁰ Although the first stage is robust to aggregating up from ESR to state, it is not robust to disaggregating 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, and 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.

²¹ Without ESR fixed effects, the OLS estimate would suggest an income elasticity that is considerably higher (about 0.75). A similar pattern emerges at the state level where, with state fixed effects, the OLS estimate of the income elasticity is 0.154 (standard error = 0.104), but without state fixed effects, it rises to 0.711 (standard error = 0.328).

TABLE 5.—OTHER HOSPITAL OUTCOMES

Dependent Variable	Total Hospital Expenditures (1)	Total Hospital Payroll (2)	FTE (3)	RN/ (RN + LPN) (4)	Admissions (5)	In-Patient Days (6)	Beds (7)	Number of Hospitals (8)	Number of Technologies (9)	Open Heart Surgery (10)	Radioisotope Therapy (11)
$\log(\text{Income})_{jt}$	0.723 (0.214) [0.004]	0.934 (0.233) [0.001]	0.039 (0.222) [0.862]	0.329 (0.089) [0.002]	-0.430 (0.193) [0.042]	-1.034 (0.488) [0.051]	-0.698 (0.455) [0.146]	-0.552 (0.358) [0.144]	-0.132 (0.221) [0.558]	-3.163 (11.334) [0.169]	1.083 (2.575) [0.545]
R^2	0.968	0.958	0.893	0.868	0.788	0.884	0.871	0.981	0.945		
N	2,065	2,064	2,065	1,576	2,065	1,967	2,065	2,065	2,065	849	262

Columns 1 through 9 report IV estimates of equation (10) with the first stage given by equation (11). Column 1 reproduces baseline results from column 3 in table 4. The unit of observation is an economic subregion (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 the (HUWP). The dependent variables in columns 8 and 9 are in logs but not adjusted by any population measure; the 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 variable estimator of the Cox proportional hazard model shown in equation (13). The 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, 56 ESRs had not adopted open heart surgery technology by 1970 and 22 ESRs had not adopted by 1990. In column 11, 21 ESRs had not adopted radioisotope therapy by 1972 (the first-year data are available), and 8 ESRs had not adopted by 1990. Data for RNs and LPNs (column 4) exist only in 1970, 1972, 1974, 1976, 1978, and 1980–1990. Data for in-patient days (column 6) did 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).

with respect to income is 0.723, with a standard error of 0.214.²²

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 IIIA, these estimates can be interpreted as upper and lower bounds on the income elasticity of hospital spending. In both alternative specifications, the income elasticity ranges between 0.665 and 0.801, suggesting that these bounds are reasonably tight.

The last column of table 4 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 IV 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 substate ESR. In addition, to the extent one is concerned that hospital utilization may occur outside of one's ESR, this concern is mitigated at the state level.

Table 5 investigates which components of hospital expenditures are affected by income changes. It reports the results from IV estimation of equation (10) using different hospital outcomes as the dependent variable.²³ Several interesting

findings emerge. First, the results in columns 1 and 2 suggest that the impact of income on hospital payroll expenditures (about one-half of total hospital expenditures) can explain most 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 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.²⁴ 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 important, evidence of skill upgrading also suggests that our empirical strategy is able to uncover (at least some) local general equilibrium effects; skill upgrading of hospitals is likely to be a response to the ESR-level increase in the demand for hospital services.

Third, we find no evidence that rising income is associated with an increase in hospital utilization (as measured by either admissions or patient-days) or in hospital capacity (as measured by beds). These results are shown in columns 5 through 7. The point estimates are uniformly negative. For admissions and patient-days, the estimates are statistically significant, though this is far from a robust result. In the robustness analysis in the online appendix (section B), we document that in contrast to the other statistically significant results in table 5, which are highly robust, both the

²² 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 3.

²³ As detailed in the notes to table 5, we adjust both the dependent variable and income for HUWP to account for population migration in response to our instrument. The exceptions are in columns 4 and 8 to 11 in which income is still adjusted for (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 to 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, say, 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.

²⁴ We have information on RN and LPN employment only for the following years: 1970, 1972, 1974, 1976, 1978, and 1980 to 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.

statistical significance of the estimated declines of admissions and patient-days and the sign of their point estimates vary across specifications; in addition, the coefficient on beds changes sign (and is rarely statistically significant) in alternative specifications.²⁵ We therefore interpret these results as showing no response of hospital utilization or capacity to changes in income. This pattern is consistent with the time series evidence suggesting that hospital utilization has not been increasing as incomes have risen; indeed, age-adjusted admissions rates appear to have been roughly constant since 1960, while length of stay has fallen (Newhouse, 1992).

The remaining columns of table 5 document the impact of rising income on hospital entry and technology adoption. We discuss these results briefly in section IVB below and in detail in the online appendix (section C.1).²⁶

C. Robustness

We performed a large number of robustness checks of our baseline estimates, designed to explore the robustness of our instrumental-variables estimates along a number of dimensions and to examine the validity of our identifying assumption. Specifically, we explored a variety of alternative specifications designed to investigate the validity of our identifying assumption, we examined the robustness of our results to alternative specifications of our instrument, and we explored alternative ways to address potential serial correlation in the residuals. The results from these additional analyses were in general quite reassuring. They are presented in detail in the online appendix (section B and tables A3, A4, A5, and A13).

IV. 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.

A. Income and the Rising Health Share of GDP

We focus on the results from our baseline specification (table 4, column 3), which are roughly in the middle of

²⁵ See in particular appendix table A13 for a summary of the results of the robustness analysis for these variables.

²⁶ 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 predetermined 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 on request.

the range of elasticities we report in various alternative specifications.

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% to about 4%. The upper end of the 95% 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, that is, it does not explain more than 5% of the overall increase in health share over this 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% of the rise in real per capita health expenditures, while the upper end of the 95% confidence interval allows us to reject a role for rising real per capita income in explaining more than one-quarter of the rise in real per capita health spending.²⁷

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.

B. Potential Concerns with Extrapolation

National and global general equilibrium effects. Our empirical strategy is designed to capture (and, as indicated by the skill upgrading results in table 5, *does* appear to capture) general equilibrium effects that occur at the level of the local economy. However, 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 that occur at the national or global level. Two such effects that could potentially increase the income elasticity of health expenditures above what we have estimated are induced innovations (which could occur at the national or global level) and national political economy responses to rising income.

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 global 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

²⁷ On the basis of the existing correlation studies (described in section I), 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% (Cutler, 1995) to a 25% (Newhouse, 1992) of the spending growth.

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).

Without observing exogenous variation in national (or global) income, it is of course difficult to conclusively rule out major national- or global-induced technology responses to the secular increase in income in the United States, which could have further effects on health expenditures. However, the empirical evidence we do have and our theoretical expectations suggest that significantly larger elasticities resulting from these induced innovation general equilibrium effects may be 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. In particular, even though innovations take place at the national or global level, 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 (Acemoglu, 2002, 2007). 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; we present these results in detail in our online appendix, section C.1.

Second, existing theory suggests that induced innovations should be directed to sectors that are otherwise expanding faster than others (see in particular our online appendix, section A); the implications of this theory are consistent with existing empirical evidence, which indicate that medical innovation responds to expected market size (Acemoglu & Linn, 2004; Finkelstein, 2004). However, our estimates suggest that all else equal, health expenditures increase less than proportionately with 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.²⁸

As the model in the online appendix highlights, the main exception to this conclusion is that even a less than proportionate increase in the size of the market for health

²⁸ The fact that income-induced innovations in the health care sector are not likely to be quantitatively important does not imply that induced innovation more generally is not important in the health care sector. Indeed, there is substantial empirical evidence of induced innovation effects in the health care sector arising through such mechanisms such as expected market size (Acemoglu & Linn, 2004; Finkelstein, 2004, 2007) and relative factor prices (Acemoglu & Finkelstein, 2008). Moreover, the evidence in Acemoglu and Linn (2004) suggests that whether rising income produces induced innovation effects in the health care sector will depend on whether the health sector expands relative to other sectors in response to rising income. Given that our estimates suggest that rising incomes increase the relative market sizes of other sectors more than that of health care, this suggests that induced innovations arising from increased income are more likely to be directed toward these other sectors rather than health care.

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 & Meara, 2004).²⁹

Political economy effects of rising incomes. Although our empirical estimates at the state level (table 4, column 6) capture any state- or substate-level political economy effects of rising income, they will not capture any effect of income on health care expenditures that operate via a national political economy response to rising income. 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.³⁰ The major exception is Medicare, a fully federal program that accounts for about half of total expenditures by public health insurance programs. 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.³¹ If rising income had quantitatively important national political economy effects in terms of Medicare generosity, one

²⁹ 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 twentieth century, while penicillin and other antibiotics were a key factor midcentury, and medical interventions that reduce cardiovascular disease mortality were critical in the later part of the century (Cutler & Meara, 2004).

³⁰ 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% of these hospitals (constituting about three-quarters of public hospital expenditures) are nonfederal (state, county, or cityowned). 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 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.

³¹ 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/>.

might expect to see Medicare spending per beneficiary growing faster than overall health spending per capita as incomes have risen.

Hospital spending versus total health expenditures. 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 (CMS, 2010). We are able to draw on several additional data sources to provide suggestive evidence of the income elasticity of overall health care expenditures and of the hospital and nonhospital components thereof. We describe the data and estimates in detail in the online appendix, section C.3.

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. This is consistent with the time series evidence in figure 1, which indicates that hospital and nonhospital components of health care have grown proportionally over the past half-century. If income elasticities were substantially higher for the nonhospital 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.

Labor income versus total income. Another potential concern is that our baseline income measure captures only the effect of our instrument on labor income. If capital income and labor income do not respond proportionately to our instrument, we may be understating (or overstating) the first-stage relationship and, consequently, overstating (or understating) 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. However, we were able to 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. These results are discussed in the online appendix, section C.3. They suggest that, if anything, the baseline estimates using only labor income may be slightly overstating the income elasticity of health expenditures.

Heterogeneity in income elasticities. 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, albeit only to the extent possible within our sample, whether there appears to be important heterogeneity in our estimated income elasticity that could lead to serious

underestimation of the effect of rising incomes on health care expenditures. Overall, we view the available evidence as reassuring on this point. We discuss the results in detail in the online appendix, section C.3, and here simply briefly summarize the main concerns, our attempts to investigate them, and the nature of our findings.

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 1. To alleviate this concern, we estimated similar IV regressions with spending on goods that can be classified as a luxury on a priori grounds (such as recreation); we found that our source of variation in income is indeed strong enough to uncover elasticities greater than 1.

A more specific concern is that, as discussed in section IIIA, we cannot reject that our income variation at the ESR level comes entirely from changes in employment at roughly constant wages (see table 3), while about half of the income growth in the United States over the past half-century comes from increased wages per employed individual (U.S. Census Bureau, 2008). 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. Within our sample, however, we found no evidence of a convex relationship between income and health expenditures; this also suggests that the income elasticity of health expenditures is unlikely to be significantly greater at higher levels of income or for larger income changes. Of course, if there are important nonlinearities in the effect of income at the individual level, these might not be captured by these exercises.

We also explored the sensitivity of our results to other samples. We find similar point estimates (albeit with less precision) if we expand the sample to the entire United States.

Short-run versus long-run income elasticities. Since we focus on annual variation, our empirical strategy estimates the short-run response of health expenditures to (permanent changes in) income. This may naturally be different from the long-run response of health expenditures, although a priori, the sign of this difference is not obvious. To investigate this issue, we reestimated our regressions using decadal rather than annual data. Although naturally our estimates become less precise, our point estimate of the income elasticity remains virtually the same. This suggests that the long-run income elasticity may be similar to the short-run elasticity, a conclusion that is consistent with the lack of a capacity response that we found in table 5.

V. 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% of GDP in 1960 to 16% 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 suboptimality) of rising health share of GDP. Yet surprisingly little is known empirically about the impact of rising aggregate incomes on aggregate 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 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), which 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% 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 postwar period, we provided additional evidence suggesting that many of the most salient potential concerns with such extrapolation may not 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 (Newhouse, 1992; Fuchs, 1996; Cutler, 2002; Congressional Budget Office, 2008), though presumably technological progress itself is being spurred by other factors. Our analysis thus also indirectly 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, 2007; Acemoglu & 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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