On-line Appendix for Income and Health Spending: Evidence from Oil Price Shocks (not for publication)

Daron Acemoglu
MIT and NBER

Amy Finkelstein
MIT and NBER

Matthew J. Notowidigdo
University of Chicago
Booth School of Business
and NBER

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Section A: Induced Innovation Effects

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

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

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

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

$$\pi_g(t) = (p_g(t) - 1)\frac{E_g(t)}{p_g(t)}. \tag{14}$$

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

$$\delta_g z_g(t).$$

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

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

$$p_g(t) = \lambda \text{ for all } g \text{ and } t. \tag{15}$$

Then (14) gives the time t profits of the firm with the leading-edge variety of good g, with quality $q_g(t)$ as

$$\pi_g(q_g(t)) = \frac{\lambda - 1}{\lambda} E_g(t). \tag{16}$$

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

$$rV_g(t \mid q_g) - \dot{V}_g(t \mid q_g) = \pi_g(q_g(t)) - \delta_g z_g(t) V_g(t \mid q_g), \tag{17}$$

where $\pi_g(q_g(t))$ is the flow profits given by (16), and $z_g(t)$ is R&D effort at time t on this line by other firms. Throughout, we assume that the relevant transversality conditions hold and discounted values are finite. Moreover, because of the standard replacement effect first emphasized by Arrow (1962), the firm with the best technology does not undertake any R&D itself (see, for example, Aghion and Howitt, 1992, Acemoglu, 2009). Intuitively, the value of owning the best technology for good g, $rV_g(t \mid q_g)$, is equal to the flow profits, $\pi_g(q_g(t))$, plus the potential appreciation of the value, $\dot{V}_g(t \mid q_g)$, and takes into account that at the flow rate $\delta_g z_g(t)$ there will be a new innovation, causing the current firm to lose its leading position and to make zero profits thereafter.

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

$$\text{if } z_g(t) > 0, \text{ then } \delta_g V_g(t \mid q_g) = 1 \text{ for all } g \text{ and } t \tag{18}$$

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

An equilibrium in this economy is given by sequences of prices $p_g(t)|_{g=1,..G}$ that satisfy (15), and R&D levels $z_g(t)|_{g=1,..g}$ that satisfy (18) with $V_g(\cdot)$ given by (17).

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

Substituting this equation and (18) into (17) yields the levels of R&D effort in the unique equilibrium as

$$z_g(t) = \max\left\{\frac{\delta_g(\lambda - 1)\lambda^{-1}E_g(t) - r}{\delta_g}; 0\right\} \text{ for all } g \text{ and } t. \tag{19}$$

Equation (19) highlights the market size effect in innovation: the greater is expenditures on good g, $E_g(t)$, the more profitable it is to be a supplier of that good, and consequently, there will be greater research effort to acquire this position. In addition, a higher productivity of R&D as captured by δ_g also increases R&D, and a higher interest rate reduces R&D since current R&D expenditures are rewarded by future revenues.

Given equation (19), we can now ask how a rise in overall income in the economy will affect the direction of technological change. Such a change will shift the expenditures from $\left\{[E_g(t)]_{t=0}^{\infty}\right\}_{g=1,\ldots,G}$ to $\left\{\left[\tilde{E}_g(t)\right]_{t=0}^{\infty}\right\}_{g=1,\ldots,G}$. However, expenditures on some good will increase by more, in particular, those that are "luxury goods" will see their expenditures increase by more. Equation (19) then implies that innovations will be tend to be directed towards those goods.

To highlight the implications of this type of induced technological change for our purposes, suppose that the economy consists of two goods, health care and the "rest". Suppose also that equation (19) leads to positive R&D for both groups of goods. Moreover, let us parameterize expenditures on these two groups of goods as $E_{health}(t) = a_{health}(t)Y(t)$ and $E_{rest}(t) = a_{rest}(t)Y(t)$, where $Y(t)$ is total income (GDP). Our ESR-level estimates imply that, without the induced technology responses, $a_{rest}(t) > a_{health}(t)$, so that with the rising incomes $E_{rest}(t)$ increases more than $E_{health}(t)$. Equation (19) then implies that $z_{rest}(t)$ will increase (proportionately) by more than $z_{health}(t)$, or that $z_{rest}(t)/z_{health}(t)$ will increase. Importantly, this conclusion is independent of the values of the δ_g's as long as they are such that both $z_{rest}(t) > 0$ and $z_{health}(t) > 0$. This result is the basis of our argument that, given the relationship between health care expenditures and income we observe at the ESR level, national-level directed technological change is unlikely to significantly increase the responsiveness of health care expenditures to aggregate income changes.

Equation (19) also highlights the conditions under which this conclusion needs to be modified. If it happens to be the case that $z_{health}(t) = 0$ and $z_{rest}(t) > 0$ to start with, then an increase in $E_{health}(t)$ that is proportionately less than that in $E_{rest}(t)$ may still have a disproportionate effect on innovation in the health care sector by making $z_{health}(t) > 0$. Intuitively, before the changes in expenditures, technological change in the health care sector would have been unprofitable, and as the market size passes a certain threshold (in this case equal to $\delta_g^{-1}(\lambda - 1)^{-1}\lambda r$), innovation jumps up from zero to a positive level. While this is theoretically possible, we believe that it is unlikely to be important in the context of the health care sector, since as discussed earlier in the main text, throughout the 20th century technological change in the health care sector was positive and in fact quite rapid (Cutler and Meara, 2003).

Section B: Robustness

In this Appendix, we provide several robustness checks of our baseline estimates, particularly focusing on whether our causal estimates of the effect of income on health care expenditures might be spurious and whether they may be underestimating the income elasticity of health care expenditures. In the interest of brevity, we focus our discussion on the robustness of our main dependent variable: hospital expenditures. Appendix Table A13 summarizes results from the alternative specifications shown in the previous tables as well as results from the main alternative specifications pursued below, for each of the components of hospital expenditures analyzed in Table 5.

B.1 Exclusion Restriction

The exclusion restriction of our IV strategy is that absent oil price changes, ESRs with different levels of oil reserves would have experienced the same proportional changes in hospital expenditures. In Table A3 we explore a variety of alternative specifications designed to investigate the validity of this identifying assumption. As usual, Panel A shows the IV estimates, while Panel B shows the corresponding first-stage results. Column 1 replicates our baseline estimates.

Column 2 shows the results of a natural falsification test: we repeat the baseline analysis of equation (11) (corresponding to column 1), but also include a 5-year *lead* of the instrument, that is, $\log p_{t+5} \times I_j$ (where I_j again denotes oil reserves in ESR j). To the extent that our instrument captures the impact of rising oil prices on the area's income rather than differential trends across areas with different levels of oil reserves, future oil prices should not predict current income changes. Column 2 in Panel B shows that the first-stage relationship is robust to including the lead of the instrument. The coefficient on the lead of the instrument is positive and large (about 60 percent of that on the instrument), though statistically insignificant. The magnitude of this coefficient raises some concerns about potential serial correlation. We explore issues of serial correlation in greater detail in the subsection B.3. To preview, even if there is serial correlation in the first stage, this does not necessarily create a bias in the IV estimates. In addition, our robustness checks in the next subsection show that the statistical and quantitative properties of our estimates are reasonably robust in alternative specifications that explicitly recognize the possibility of serial correlation.

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

Column 3 shows the results from an alternative check on our identification strategy, in which we additionally control for interactions between oil prices ($\log p_{t-1}$) and fixed ESR characteristics. In particular, we control for separate interactions between log oil prices in year t-1 and each of log hospital expenditures in 1969, log hospital beds in 1969, log population in 1970, log area income in 1970 and log area employment in 1970. This "horse race" between our instrument and other interactions of oil prices and baseline area characteristics is useful for two complementary reasons. First, it provides additional evidence that it is the interaction between oil price shocks and availability of oil reserves leading to the source of income variation that we are exploiting. Second, it indirectly controls for differential pre-existing trends in health expenditures (and income) across ESRs, which are the main threat to our identification strategy. Consistent with the limited differences in various ESR characteristics shown in Table 2, the results of this horse race show that both our first-stage and second-stage estimates are robust in magnitude and precision to the (simultaneous) inclusion of all of these interaction terms. Very similar estimates are obtained when we include each interaction term one by one (not shown).

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

Finally, as another natural and important falsification exercise, we checked the implications of estimating our models on health expenditures data from 1955 through 1969 while assuming that the oil price changes took place 15-years prior (more precisely, the year 1955 is assigned the oil price for 1970, the year 1956 is assigned to the oil price in 1971, and so on through the year 1969 which is assigned to the oil price of 1984).[1] The period before 1970 shows virtually constant oil prices before 1970 (see Figure 2). Therefore, if our identifying assumption is valid, we should not see any differential changes in health expenditures across areas with different oil reserves prior to 1970, and in particular, we should not see more rapid increases in health expenditures in areas with greater oil reserves. Column 6 shows the first-stage and reduced-form results for our baseline specification if we limit it to the 1970 to 1984 period. The first-stage remains as does the reduced form, though the implied IV estimate is about one half the size of our baseline estimate (which uses the entire 1970-1990 period). Column 7 shows the result for the falsification exercise. Reassuringly, this falsification exercise shows no evidence of a significant reduced-form relationship between our instruments and health expenditures; the point estimate is negative (opposite sign from the "actual" estimate in column 6) and not statistically significant. This finding supports the validity of the identifying assumption that, absent changes in oil prices, areas of the South with different levels of oil intensity would have experienced similar trends in their hospital expenditures.

Overall, we read the results in Table A3 as broadly supportive of our identifying assumption.

B.2 Alternative Specifications of the Instrument

We also explored the robustness of our results to alternative specifications of the instrument. Table A4 shows the results. Panel A again shows the IV estimates and Panel B shows the corresponding first-stage estimates. Column 1 replicates our baseline first-stage specification, in which the instrument is the interaction of the total oil reserves and the log of the (lagged) oil price, i.e., $\log p_{t-1} \times I_j$, with again I_j measured as oil reserves. The remainder of the columns

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

show results for alternative (plausible) specifications of the instrument; they tend to produce smaller income elasticities than our baseline specification.

Columns 2 and 3 report results using different functional forms for oil prices. Column 2 reports results in which the instrument is constructed as the interaction between the level of (lagged) oil prices and oil reserves (i.e., $p_{t-1} \times I_j$ instead of $\log p_{t-1} \times I_j$ as in our baseline specification). Column 3 reports results when we use the log oil price at time t rather than its one year lag (i.e., $\log p_t \times I_j$ instead of $\log p_{t-1} \times I_j$). With both alternative functional forms for oil prices we continue to estimate strong first stages and statistically significant income elasticities in the second stage that are similar to, though slightly smaller than, our baseline estimate (the income elasticity estimates are 0.49 and 0.64 in columns 2 and 3 respectively, compared to 0.72 in our baseline).

Columns 4 through 6 report results using different ways of measuring the oil intensity of the area. Recall that in our baseline specification we proxied oil intensity of area j by its total (cumulative) oil reserves. Figure 3b shows that the oil reserve distribution is highly skewed and one may be concerned that using the level of oil reserves might give disproportionate weight to the ESRs with the highest oil reserves. Moreover, the effect of oil reserves on the demand for labor, and thus on income, may be nonlinear, with large and very large oil reserves leading to similar effects on income when oil prices rise. Motivated by these considerations, in column 4 we report results with an alternative measure of I_j, where oil reserves are censored at the 95th percentile of oil reserve distribution (the instrument is then constructed by interacting this measure with $\log p_{t-1}$). The results are very similar to the baseline. We continue to estimate a strong first stage, and a statistically significant income elasticity; the estimated income elasticity of 0.632 (standard error = 0.205) is only slightly smaller than the baseline estimate. We also obtain similar estimates if instead we censor oil reserves at the 90th or the 99th percentiles (not shown).

As another check on possible nonlinearities, column 5 measures oil intensity by an indicator variable for whether there are any large oil wells in the ESR (i.e., the instrument is now $\mathbf{1}(I_j > 0)$). The first stage is now slightly weaker (F-statistic of about 8), and the estimated income elasticity rises to 1.10 (standard error = 0.67), but is no longer statistically significantly at the 5 percent level.

Finally, in column 6 we measure oil intensity as the (de-meaned) mining share of employment in the ESR in 1970, interacted with an indicator variable for whether there are any large oil wells in the ESR.[2] Our first stage is now marginally stronger than in the preceding specification (F-statistic of about 11), and we estimate a statistically insignificant income elasticity of 0.860 (standard error = 0.870).

B.3 Serial Correlation and Standard Errors

In our baseline model we cluster our standard errors at the state level; the standard errors are therefore computed from a variance-covariance matrix that allows both for arbitrary correlation in residuals across ESRs within a state and for serial correlation at the state or ESR level. However, because we only have 16 states in our baseline (South only) sample, these standard errors may be downward biased due to the relatively small number of clusters (Cameron,

[2] Mining share of employment is defined based on the 1970 Census of Population (Volume 1: Characteristics of the Population, Table 123, Parts 2-9 & 11-52). The mining share includes all workers in oil mining, natural gas and coal mining (it is not available separately for oil mining). We therefore include the indicator variable for whether there are any large oil wells to separate out high mining share non-oil areas (such as coal mining areas of West Virginia).

Gelbach and Miller, 2008). As a simple robustness check, we computed the standard errors allowing for an arbitrary variance-covariance matrix at the ESR level (rather than the state level). A possible disadvantage of these standard errors is that they do not allow for correlation across ESRs within the same state, which may be important in practice.[3] Clustering at the ESR level increases the standard errors substantially, so that the first-stage F-statistic is now 5.50 (instead of 16.58 with clustering at the state level). The standard errors for the second stage are also larger, but our IV estimate is still statistically significant at the 6 percent level (results available upon request).

Another strategy to correct for potential biases in the standard errors resulting from the small number of clusters at the state level is the wild bootstrap procedure suggested by Cameron, Gelbach and Miller (2008).[4] We performed wild bootstraps resampling states with replacement. In this case, we find reassuringly similar (indeed somewhat smaller) p-values to our baseline specification with state-level clustering.[5] In particular, using wild bootstraps we find that both the first stage and the second-stage estimates are statistically significant at the less than 1 percent level (results available upon request).

An alternative strategy to address concerns about potential serial correlation is to directly model the dynamics of the error term in our structural equation (10) and then estimate this extended model using instrumental-variables Generalized Least Squares (IV-GLS). In all of our IV-GLS specifications we allow for heteroscedasticity in the second-stage error term; we also experiment with various assumptions regarding the nature of any autocorrelation. The details of the implementation of IV-GLS and the procedure for the computation of the standard errors are discussed in Section C. Table A5 reports the results. Column 1 shows estimates from our baseline specification, but using a subsample of our original data; we limit the sample to the 96 (out of 99) ESRs that have data in the full 21 years from 1970 to 1990. Column 1 verifies that this has no notable effect on our baseline results. Column 2 reports IV-GLS results assuming a common AR(1) autocorrelation coefficient across all ESRs. Column 3 reports results assuming an AR(2) specification of the residuals with common autocorrelation coefficients. In both specifications the point estimate rises relative to the baseline, but is also considerably less precise. Columns 4 and 5 report results assuming state-specific AR(1) and AR(2) errors respectively. Here the point estimates are very similar to the baseline specification both in magnitude and in precision. Overall we interpret these results as supportive of the robustness of the baseline specification.

As a final strategy to control for serial correlation, columns 6 and 7 include a lagged dependent variable on the right-hand side. In column 6, this model is estimated with ordinary least squares and leads to a long-run elasticity of 0.859 (standard error = 0.213), which is slightly higher than our baseline estimate. However, the least squares estimator in column 6 is inconsistent because of the presence of the lagged dependent variable on the right-hand side. Column 7 estimates the same model using the Arellano-Bond GMM dynamic panel estimator. This GMM procedure estimates the same model in first differences using further lags of the dependent variable as instruments. This leads to a considerably smaller long-run elasticity (= 0.142, standard error = 0.080) than in our baseline. Such smaller long-run elasticities make it

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

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

[5] In their Monte Carlo study, Cameron et al find it is important to calculate p-values based on t-statistics rather than parameter estimates. We also computed p-values using parameter estimates, and found these to be even lower (thus leading to more precise results) than the results reported here based on t-statistics.

even less likely that rising incomes over the past half a century could be the primary driver of the increase in the health share of GDP in the United States.[6]

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

Section C: Additional results

C.1 Hospital entry and technology adoptoin

As discussed in Section 4.2, if an induced inovation response to rising income is present and economically signfiicant, it should also manifest iteself at the ESR level in the form of entry of new hospitals (which presumably embody new technologies) and/or the adoption of new technologies at existing hospitals. However, we find no evidence that rising income is associated with an increase in hospital entry or technology adoption. These results are summarized in columns 8 through 11 of Table 5. Column 8 of this table shows a negative and statistically insignificant impact of income on the number of hospitals (so that the number of hospitals appears to have grown relatively more in areas experiencing slower income growth).

The rest of Table 5 turns to technology adoption. The AHA data contain binary indicators for whether the hospital has various "facilities", such as a blood bank, open heart surgery facilities, CT scanner, occupational therapy services, dental services, and genetic counseling services. These data have been previously used to study technology adoption decisions in hospitals, and in particular hospital responsiveness to economic incentives including the insurance regime and relative factor prices (see, e.g., Cutler and Sheiner, 1998, Baker and Phibbs, 2002, Finkelstein, 2007, Acemoglu and Finkelstein, 2008). Since they contain only indicator variables for the presence of various facilities, we cannot investigate the potential upgrading of existing technology or the intensity of technology use, but we can study the impact of changes in income on the total number of facilities, proxying for technology adoption decisions on the extensive margin.

During the time period we study, the AHA collects information on the presence of 172 different "facilities". These are listed, together with their sample means (the fraction of ESRs each technology is in) and the years in which they are available in Appendix Table A1. On average, a given facility is reported in the data for 7 out of the possible 21 years; only nine of the technologies are in the data for all years. Moreover, as is readily apparent from Appendix Table A1, the list encompasses a range of very different types of facilities. Given these two features of the data, we pursue two complementary approaches to analyzing the relationship between income and technology adoption with the AHA data.

Our first approach to investigating the impact of income on technology adoption, which is shown in column 9, treats all facilities equally and measures technology as the log of the number of distinct technologies in a given ESR in a given year. The year fixed effects in our IV estimate of equation (10) adjust for the fact that the set of technologies reported in each year differs. The results show no substantively or statistically significant evidence of an increase in the number of distinct technologies in the area in response to the increase in income. The point estimate on income is negative and statistically insignificant. It is also substantively small, suggesting that a 10 percent increase in area income is associated with a statistically insignificant decrease in the number of technologies in the area of 1.3 percent.[7]

A drawback of this approach is that it treats all technologies as perfect substitutes. As an alternative, we estimated hazard models of the time to adoption for specific technologies that are in the data for at least 15 years of our 21 year sample period. As in Acemoglu and Finkelstein (2008), we limit our analysis to technologies that were identified as "high tech" by

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

previous researchers (Cutler and Sheiner, 1998, Baker, 2001, and Baker and Phibbs, 2002). Unfortunately, there are only two technologies that meet these criteria in our sample: open heart surgery and diagnostic radioisotope facility. Both have been found in other work to be responsive to economic incentives (Finkelstein, 2007, Acemoglu and Finkelstein, 2008). Both of these technologies were diffusing over our sample period, though open heart surgery started from a lower prevalence and diffused more rapidly.[8] To investigate the impact of ESR income on local technology adoption decisions, we estimate semi-parametric Cox hazard models for these two technologies as functions of income. In particular, the conditional probability that ESR j adopts the technology in question at time t (meaning that at least one hospital in the ESR adopts the technology conditional on there being no hospital in the area that had previously adopted this technology) is modeled as

$$\lambda_{jt} = \lambda_{0t} \exp(\beta \log \tilde{y}_{jt} + \mathbf{X}_j^T \phi), \tag{20}$$

where λ_{0t} is a fully flexible, non-parametric baseline hazard, $\tilde{y}_{jt}$ is our baseline measure of (HUWP-adjusted) income, and $\mathbf{X}_j$ is a vector of (time-invariant) covariates. Since we have at most a single transition (adoption) for each ESR, we cannot include ESR fixed effects in the hazard model. Instead, we include time-invariant ESR characteristics in the vector $\mathbf{X}_j$, in particular, region fixed effects for the three census regions within the South, total hospital expenditures in 1970, and total hospital beds in 1970. The fully flexible baseline hazard in the Cox model is specified with respect to calendar time and thus controls for time effects. As in our baseline specification, income is an endogenous right-hand side variable, which we instrument with $\log p_{t-1} \times I_j$. We implement our instrumental variables estimator using a control function approach (Newey, Powell, and Vella, 1999). Specifically, we include the residual ($\hat{u}_{jt}$) from the first-stage regression in equation (11) as an additional covariate in equation (20). We report bootstrapped standard errors and p-values for this two-step estimator. The results reported in columns 10 and 11 in Table 5 show no evidence of a significant increase in technology adoption associated with an increase in income. The point estimates suggest a negative relationship between log income and adoption of open-heart surgery, and a positive relationship between log income and adoption of the diagnostic radioisotope facility. However, both estimates are imprecise and not statistically different from zero.[9]

C.2 The income elasticity of total health expenditures

We bring several complementary data sources to bear to try to shed some light on whether overall health expenditures may be more responsive to changes in income than hospital expenidtures, which are the focus of our main analysis. To preview, although estimates from the other available data sources are often quite imprecise (motivating our preference for the AHA data set), we do not find any evidence that overall health expenditures are more income elastic than hospital expenditures.

We have state-level data on total health expenditures and its components from the Health Care Financing Administration (HCFA) for 1972, 1976-1978 and 1980-1990 (instead of our

[8] Open heart surgery is in our data for all 21 years (1970-1990) and diagnostic radioisotope therapy for 19 years (1972-1990). Only 43 percent of ESRs had open heart surgery technology in 1970, whereas about three quarters of ESRs did so by 1990. About three quarters of ESRs had diagnostic radioisotope faciltiies in 1972 and 92 percent had it by 1990.

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

baseline sample 1970-1990).[10] The HCFA estimates are based on a combination of administrative and survey data. An important problem with these data is that each component is interpolated whenever data are missing between years (Levit, 1982, 1985). Such interpolation may bias the estimated coefficients, so the results from this data set have to be interpreted with caution.

Table A6 presents estimates from the HCFA data. Since we lose some variation by aggregating from the ESR level to the state level, we report results both for our baseline sample of the 16 the Southern states (Panel A) and for the entire United States (Panel B). Column 1 shows that our first stage is robust to state-level analysis for the subset of years for which we have HCFA data. Columns 2 and 3 show our estimated income elasticity from the HCFA data for total health expenditures and the hospital subcomponent, respectively. Both estimated income elasticities are positive but quantitatively small and imprecise, and thus statistically insignificant. The income elasticity of hospital spending using the HCFA data is also noticeably smaller than that estimated using the AHA data.[11]

However, most importantly for our purposes, the point estimates in columns 2 and 3 of Table A6 suggest similar income elasticities for hospital expenditures and total health expenditures. Columns 4 through 9 present results for the other components of health expenditures, and provide some intuition for why hospital and total health expenditure income elasticities may be similar. The point estimates suggest that the income elasticities of spending on physician services, on dental services, on drugs and other medical non-durables, and on vision products are greater than the income elasticity of hospital spending, while nursing home care and other health services have large negative income elasticities.[12] Overall, the results in Table A6 are generally imprecisely estimated, but the point estimates are uniformly consistent with similar income elasticities for total health expenditures and for hospital expenditures.

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

[10] Data from 1972 and 1976-1978 were obtained from Levit (1982, 1985). Data for 1980-1990 were obtained from the Centers of Medicare & Medicaid Services on-line at `http://www.cms.hhs.gov/NationalHealthExpendData/05_NationalHealthAccountsStateHealthAccountsResidence.asp#TopOfPage`. The data include total health expenditures and expenditures on the following components (which sum to the total): Hospital Care, Physicians' Services, Dentists' Services, Drugs and Other Medical Nondurables, Eyeglasses and Appliances, Nursing Home Care, and Other Health Services (which include Home Health Care, Other Professional Services, and Other Personal Services).

[11] The hospital expenditure data in the HCFA series are estimated using the AHA data for non-federal hospitals, but use unpublished Federal agency data for federal hospital expenditures (Levit, 1982). There are also several differences between how we use the AHA data and how they are used in creating the HCFA data. Most importantly, the HCFA estimates interpolate missing data (Levit, 1982, 1985). Average state-year hospital expenditures are similar in the two data sets ($2,641 million from the HCFA data compared to $2,333 million for the same state-years in the AHA data). Log hospital expenditures are also highly correlated across the two data sets at the state-year level (correlation = 0.98). However, conditional on state and year fixed effects, the correlation in the residual log hospital expenditures is only 0.67. This presumably helps explain why the income elasticity estimates differ. Using our AHA hospital data at the state level for the full United States and limiting the sample to the years for which the HCFA data are available (i.e., the analog of Table A6 column 3 panel B), we estimate a statistically significant income elasticity of 0.509 (standard error = 0.225). This is statistically indistinguishable from the HCFA estimate of 0.139 (standard error = 0.151).

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

considerably smaller than our estimates for hospital expenditures, although they are quite imprecise.[13]

We also examined the impact of area income on the income of different groups of health care providers (results available on request). If non-hospital components of health care expenditures—such as physician expenditures—are substantially more income elastic than hospital expenditures, we would expect to find that the earnings of the non-hospital based health care providers are also substantially more income elastic than hospital expenditures and than the earnings of health care providers that contribute to hospital expenditures, such as nurses and health care technicians. Using decadal Census data aggregated to the state level, we estimated the income elasticity of the earnings of the following groups of health care providers: physicians, nurses, health care technicians (including clinical laboratory technicians and therapy assistants), and other health services workers (including health aids, nursing aids and attendants).[14] Our IV point estimates show no evidence that physician earnings are more responses to area income than hospital expenditures or than the earnings of other health care providers. However, the estimates using the Census income data—particularly those for physician income—are noticeably less precise than those from comparable specifications using the AHA data on hospital expenditures, so that one should not place too much emphasis on these results.[15]

Overall, while there are important limitations to each data source, a number of complementary data sets with information on state-level health expenditures suggest that the income elasticity of overall health expenditures is unlikely to be significantly higher than the income elasticity of hospital spending.

C.3 Incorporating a broader measure of income

Our baseline income measure captures only the effect of our instrument on labor income. If capital income and labor income do not respond proportionately to our instrument, we may be under-stating (or over-stating) the first-stage relationship, and consequently, over-stating (or under-stating) the income elasticity in the second stage. Unfortunately, annual data on labor and capital income do not exist for our time period at a level of disaggregation below the state. 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

[13] The results for state-level Health Services GSP are shown in Table 8, column 6, Panels A and B). The rest of that table is discussed in subsection C.4 below. Comparable state-level estimates for hospital expenditures are show in Table A7, Panel A, columns 1 and 3. The Gross State Product (State GDP) estimates are produced annually by the Bureau of Economic Analysis. The specific industries within health services (SIC code 80) are listed at `http://www.census.gov/epcd/naics/NSIC8B.HTM#S80`. The major source of state data for the health services GSP estimates are sales and payrolls from the (quinquennial) census of service industries; intercensal years are interpolated and extrapolated using wages and salaries reported annually to the BEA (see `http://www.bea.gov/regional/pdf/gsp/GDPState.pdf`).

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

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

payroll measure; unlike payroll, GSP includes both labor and capital income. [16]

Table A7 shows the results of this exercise. Panel A shows the IV estimates, and Panel B shows the first-stage estimates. Columns 1 and 2 compare results at the state level when labor (payroll) income and GSP are used, respectively, as our income measure. The first stage suggests that, in response to our instrument, non-labor income appears to rise by the same proportion, or by slightly more, then our primary measure of labor income (compare columns 1 and 2 of Panel B). If anything, therefore, the results suggest that the estimates using labor income only may be slightly over-stating the income elasticity of health expenditures (compare columns 1 and 2 of Panel A).

Since, as discussed, we lose variation by aggregating to the state level, we also report results at the state level when we include the entire US in the sample rather than just the 16 states in the South. Column 3 shows the results when we use labor income (from the CBP payroll data) as our measure of income and column 4 shows the results when we use the GSP measure, which incorporates capital income. Once again the results suggest that non-labor income may rise slightly more than proportionately with labor income, so that our income elasticities in our baseline estimates may be slightly overstated.[17]

C.4 Exploring potential 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 whether there appears to be substantial heterogeneity in our estimated income elasticity. All in all, we read the available evidence as suggesting that the quantitative estimates are reasonably similar across different sources of income variation, geographic samples, time periods, and time horizons; we therefore do not see any reason to suspect that heterogeneous elasticities are likely to lead to a serious underestimation of the effect of rising incomes on health care expenditures.

Source and extent of income variation At a general level, one might be concerned that the source and range of the variation in income that we are exploiting may be insufficient to estimate (or detect) income elasticities significantly greater than one. To alleviate this concern, we estimated similar IV regressions with spending on goods that can be classified as a luxury on a priori grounds (e.g., recreation). Since we do not have data on spending on other goods at the ESR level, we pursued this strategy at the state level using data on industry-specific Gross State Products (GSP) for other service industries. Specifically, we used our instrument at the state level to examine the income elasticity of four potential luxury goods: "amusement and recreation services," "hotels and other lodging places," "legal services" and "other services," which includes (among other things) record production, actuarial consulting, music publishing, and other consulting.[18] We also estimated the income elasticity of "food and kindred products," which we expect to be a necessity. The results are shown in Table A8.[19]

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

[17] The results in column 3 also suggest that our estimates are not sensitive to using the entire United States. In later robustness analysis we show this is true at the ESR level as well (see Table A10 below).

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

[19] An estimate for health services GSP, which was already discussed in subsection C.2, is also included in this table.

The results suggest that our source of variation in income is strong enough to uncover elasticities greater than one at the state level.[20] Legal services and "other services" both appear to be strong luxuries. Amusement services and hotels also show an income elasticity of close to or above 1. By contrast, food stores appear to be a necessity, with an income elasticity that is virtually the same as what we estimate for health services (see column 6).

A more specific concern is that, as discussed in Section 3.1, we cannot reject that our income variation at the ESR level comes entirely from changes in employment at roughly constant wages (see Table 3), while about half of income growth in the United States over the last half century comes from increased wages per employed individual (US Census Bureau, 2008).[21] This raises the potential concern that, if the elasticity of health spending with respect to income is increasing in income, the elasticity of health care spending with respect to increases in wages may be larger than the elasticity with respect to increases in employment.

Table A9 investigates whether there is any evidence of this type of convexity in Engel curves for health expenditures. Column 1 reports results from the baseline IV specification, while column 2 adds an interaction of the ESR's (log) income with its (log) income in 1970. This strategy allows the effect of changes in income to vary based on initial income levels and provides a simple check against the possibility that the income elasticity of health expenditures may vary systematically with the level of income of the area. We instrument for log income and the interaction of log income with 1970 ESR log income with our standard instrument (oil reserves times log oil prices) and the interaction of this instrument with 1970 ESR log income. The results show no evidence that the Engel curve for health expenditures is convex; if anything the point estimates suggest a (statistically insignificant) concave Engel curve.

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

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

[20] More information on each of these categories can be found here: `http://www.bea.gov/regional/gsp/default.cfm?series=SIC`. First-stage results for this same specification are shown in Table A7, Panel B, columns 1 and 3. Second stage results for this same specification using the AHA hospital expenditure data as the dependent variable can be found in Table A7, Panel A, columns 1 and 3.

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

Different areas and time period Table A10 explores the sensitivity of our estimates to defining the sample based on different geographic regions and different time periods. Panel A shows the IV estimates and Panel B shows the corresponding first-stage results. Column 1 reproduces our baseline estimates, which are for the 16 Southern states focusing on the time period 1970-1990.

As discussed above, we chose to limit our baseline sample to the Southern United States both because the oil reserves are concentrated in the South and because the ESRs in this region are more comparable, thus less likely to experience differential trends in hospital spending owing to other reasons. In column 2 we further limit the sample to the 7 Southern states that have oil reserves in our data. The results are quite similar. In column 3 we go in the opposite direction, and look at the entire United States. The results in this column show that expanding the sample to the entire United States (not including Alaska and Virginia) results in a very similar point estimate of the income elasticity (0.804 vs. 0.723 in the baseline), though the estimate is less precise (standard error = 0.631 compared to 0.214 in the baseline).[22]

We also explored whether within the South our estimates were sensitive to excluding a particular state. Appendix Table A2 shows the results from estimating our baseline specification (from column 1) dropping each one of the 16 states at a time. The results indicate that the estimates are generally quite robust both in terms of magnitude and precision to the omission of a single state. The exception occurs when we exclude Texas. In this case, the point estimate falls by about 40 percent; combined with the increase in standard error, this makes the estimate of the income elasticity of hospital expenditure no longer significant at the 5% level. This is not surprising since much of the variation in oil intensity in our sample is within Texas (see Figure 3).

Our baseline time period is for 1970-1990 and covers the original oil boom and bust. In column 4 of Table A10, we return to our baseline Southern states sample, but now expand the time period 1970-2005 (thus including all available years with data). Figure 2 shows that oil prices experienced a second boom starting in 1999. Nevertheless, we lose the first stage when we include the post 1990 years (and therefore do not report the corresponding IV estimate). This weaker first-stage relationship appears to reflect the inadequacy of imposing constant ESR fixed effects over a 36 year period. Indeed, when this assumption is relaxed in column 5 by including state-specific time trends, the first-stage relationship is again statistically significant and leads to an IV estimate of similar magnitude to the baseline.

Permanent versus transitory elasticities The interpretation of our estimates depend on whether oil price changes are permanent or transitory. This is investigated in Table A11 using the time-series data shown in Figure 2. Column 1 shows that a regression of the log oil price at time t on its one year lag produces a coefficient of 1.009 (standard error = 0.043). The augmented Dickey-Fuller unit-root test reported at the bottom comfortably fails to reject the null hypothesis that log oil prices follow a unit root. The remaining columns of this table show several different specifications, all indicating that we cannot reject that changes in oil prices are permanent. These findings are consistent with those of previous researchers.[23] The available evidence therefore suggests that our empirical strategy speaks to the effects of permanent

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

[23] Kline (2008) conducts a more detailed analysis of the time-series behavior of oil prices and concludes that oil prices are "well approximated by a pure random walk". See also Hamilton (2008) for a similar conclusion.

(rather than transitory) changes in income on health care expenditures.

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

To investigate this issue, we re-estimated our regressions using decadal observations, thus removing the source of variation due to short-run changes in our instrument. Table A12 compares our baseline results—which use annual observations from 1970-1990 in columns 1 through 3—with the estimates using only decadal observations (1970, 1980, 1990) in columns 4 through 6. With only the decadal observations, the first stage is only slightly weaker (compare columns 4 and 1). The IV elasticity estimate from the decadal estimate is similar to the baseline annual estimate (0.794 compared to 0.723) although the standard error of the decadal estimate is roughly double what we obtain with annual data. We read these results as suggestive of a long-run income elasticity that is similar to the short-run elasticity.

This conclusion also receives support from the lack of capacity responses. If long-run effects were significantly larger than short-run effects, we would expect to see hospitals expanding capacity (either simultaneously with the increase in health expenditures or gradually as they reach their capacity constraints). However, Table 5 showed no evidence of an increase in hospital capacity or utilization (in particular, there was no increase in admissions, patient days, hospital beds, and hospital entry in response to the rise in local income).

A related issue is that there might be heterogeneity in the adjustment dynamics of hospital spending in response to increases in income. For example, suppose that some of the ESRs respond immediately to increases in income, while other ESRs take one or two years to respond. In this case, results using the annual panel and assuming immediate and complete adjustment would underestimate the true long-run income elasticity. We show in the Section D below that specifications using 3-year averages typically perform better when there are heterogeneous adjustment dynamics by ESR. Thus in column 7 we report results based on 3-year averages. The estimated elasticity increases slightly (from 0.723 to 0.826).

Section D: Econometric Issues

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

D.1 Implementation of IV GLS

We now provided details of the implementation of the IV-GLS estimator used in subsection B.3. In particular, we use the following procedure for this estimation. First, we recover estimates of the residuals ($\hat{\varepsilon}_{jt}$) from the baseline IV specification. Then we use these residuals to estimate the autocorrelation coefficients. For example, when we estimate state-specific autocorrelation coefficients, we run the following regression of $\hat{\varepsilon}_{jt}$ on its lag ($\hat{\varepsilon}_{j,t-1}$) for each state to recover an estimate of the state-specific autocorrelation coefficient, $\hat{\rho}_s$:

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

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

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

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

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

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

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

D.2 Performance of different estimators with heterogeneous adjustment dynamics

We now describe results from a simple Monte Carlo study to investigate the performance of various estimators under heterogeneous long-run adjustment dynamics. Our Monte Carlo results suggest that heterogeneous adjustment dynamics may lead traditional fixed effects instrumental variables (FE-IV) estimators to underestimate the true long-run effect. We show that using 3-year averages can reduce this bias. Reassuringly, our 3-year average results are similar to our baseline results (see Table A12, column 7). The remainder of this section describes the set of our Monte Carlo study and our results.

We define the following variables for our simulation:

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

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

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

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

1. (FE-IV) Fixed effects IV regression of y_{jt} on x_{jt}, instrumenting x_{jt} by z_{jt}
2. (FD-IV) First differences IV regression of $(y_{jt} - y_{j,t-1})$ on $(x_{jt} - x_{j,t-1})$, instrumenting $(x_{jt} - x_{j,t-1})$ by $(z_{jt} - z_{j,t-1})$
3. (FE-IV-LAG) Fixed effects IV regression of y_{jt} on $x_{j,t-1}$, instrumenting $x_{j,t-1}$ by $z_{j,t-1}$
4. (FE-IV-3YR) Fixed effects IV regression of $\tilde{y}_{js}$ on $\tilde{x}_{js}$ instrumenting $\tilde{x}_{js}$ by $\tilde{z}_{js}$ (where $\tilde{v}_{js}$ denotes the three-year averages of v_{jt} and s represents a three-year groups of years)
5. (FD-IV-3YR) First differences IV regression of $(\tilde{y}_{js} - \tilde{y}_{j,s-1})$ on $(\tilde{x}_{js} - \tilde{x}_{j,s-1})$, instrumenting $(\tilde{x}_{js} - \tilde{x}_{j,s-1})$ by $(\tilde{z}_{js} - \tilde{z}_{js})$

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

The results (based on 500 simulations) are given in Appendix Table A14. There are five panels of results corresponding to each of the five estimators mentioned above. The resuls are the mean of the estimates across each of the simulations and the standard deviation of the parameter estimates (in parentheses underneath). The first panel reports the FE-IV results. As would be expected, the standard deviation of the parameter estimates is larger when there are higher amounts of serial correlation. The second panel reports FD-IV results, where (also as expected) the standard deviation of the parameter estimates goes down as there is more serial correlation. The third panel reports FE-IV-LAG results, and the last two columns report the two sets of 3-year average results (FE-IV-3YR and FD-IV-3YR).

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

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

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

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Appendix Table A1: Hospital Technologies

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

Podiatrist Services	1972	1985	13	0.796
Chronic Obstructive Pulmonary Disease	1975	1990	16	0.783
Alcohol / Chemical Dependency (Outpatient)	1975	1990	15	0.742
Skilled Nursing or Long Term Care Unit	1975	1985	11	0.852
Alcohol / Chemical Dependency (Impatient)	1975	1985	10	0.723
Neonatal Intensive Care	1976	1985	10	0.743
Pediatric Unit (Impatient)	1977	1978	2	0.951
Patient Representative Services	1978	1990	13	0.958
Abortion Service (Impatient)	1978	1981	4	0.794
Abortion Service (Outpatient)	1978	1981	3	0.638
Radioactive Implants	1979	1990	12	0.811
Megavoltage Radiation Therapy	1979	1990	11	0.781
Computerized Tomography Scanner (Head or Body)	1979	1990	10	0.859
Pediatric Intensive Care	1979	1985	7	0.773
Cardiac Catheterization	1980	1990	11	0.722
Hospice	1980	1990	11	0.715
Recreational Therapy	1980	1990	11	0.869
Ultrasound Facility (Diagnostic)	1980	1990	11	0.976
Kidney Transplant	1980	1990	7	0.327
Organ Transplant (Other than Kidney)	1980	1990	7	0.377
Chaplaincy Services	1980	1985	6	0.987
Electrocardiography	1980	1985	6	1.000
Intermediate Care for Mentally Retarded	1980	1985	6	0.439
Intravenous Admixture Services	1980	1985	6	0.993
Medical/Surgical Acute Care	1980	1985	6	1.000
Medical/Surgical Intensive Care	1980	1985	6	0.998
Newborn Nursery	1980	1985	6	1.000
Obstetrical Care	1980	1985	6	1.000
Other Long-Term Care / Intermediate Care Facility	1980	1985	6	0.838
Pediatric Acute Care	1980	1985	6	1.000
Pharmacy Unit Dose System	1980	1985	6	0.990
Psychiatric Acute Care	1980	1985	6	0.953
Psychiatric Long Term Care	1980	1985	6	0.568
General Surgical Services	1980	1985	5	1.000
General Laboratory Services	1980	1985	4	1.000
Health Science Library	1980	1990	3	0.968
Psychiatric Intensive Care	1980	1982	3	0.679
Ambulance Services	1980	1981	2	0.930
Anesthesia Service	1980	1981	2	1.000
Autopsy Services	1980	1981	2	0.989
C.T. Scanner (Body Unit)	1980	1981	2	0.761
C.T. Scanner (Head Unit)	1980	1981	2	0.570
Cancer/Tumor	1980	1981	2	0.894
Electromyography	1980	1981	2	0.826
Hemodialysis (Home Care/ Mobile Unit)	1980	1981	2	0.464
NeuroSurgery	1980	1981	2	0.769
Physical Rehabilitation	1980	1982	2	0.856
Pulmonary Function Laboratory	1980	1981	2	0.987
Toxicology	1980	1981	2	0.983
Intravenous Therapy	1980	1980	1	0.886
Medical/Surgical Acute Care (Inpatient)	1980	1980	1	0.335
Rehabilitation	1980	1980	1	0.953

Residential Care	1980	1980	1	0.547
Residential Care (Inpatient)	1980	1980	1	0.280
Day Hospital	1981	1987	7	0.822
Pediatric Psychiatric Services	1981	1986	6	0.777
Health Promotion	1981	1985	5	0.964
Optometric Services	1981	1985	5	0.857
Other Special Care	1981	1985	5	0.877
Sheltered Care	1981	1985	5	0.419
Ambulator Surgical Services	1981	1981	1	1.000
Podiatrist Services (Inpatient)	1981	1981	1	0.873
Podiatrist Services (Outpatient)	1981	1981	1	0.835
Hemodialysis Services	1982	1990	9	0.850
Outpatient Surgery	1982	1990	8	1.000
Abortion Services	1982	1985	4	0.825
Pharmacy Services	1982	1985	4	1.000
Comprehensive Geriatric Assessment Services	1983	1990	8	0.805
Nuclear MRI Facility	1983	1990	8	0.542
Psychiatric Liaison Services	1983	1990	8	0.819
Trauma Center	1984	1990	7	0.751
Alcohol / Chemical Acute Care (Inpatient)	1984	1984	1	0.903
Alcohol / Chemical Subacute Care (Inpatient)	1984	1984	1	0.852
Birthing Room	1985	1990	6	0.970
Extracorporeal Shock-Wave Lithotripter	1985	1990	6	0.395
X-Ray (Diagnostic)	1985	1989	5	0.999
Unknown Technology	1985	1985	1	0.678
Adult Day Care	1986	1990	5	0.567
Community Health Promotion	1986	1990	5	0.984
Fertility Counseling	1986	1990	5	0.608
Fitness Center	1986	1990	5	0.746
Geriatric Acute-Care Unit	1986	1990	5	0.754
Occupational Health Services	1986	1990	5	0.869
Patient Education	1986	1990	5	0.992
Respite Care	1986	1990	5	0.803
Sports Medicine Clinic / Service	1986	1990	5	0.775
Sterilization	1986	1990	5	0.945
Women's Center	1986	1990	5	0.762
Worksite Health Promotion	1986	1990	5	0.959
Organ Transplant (Including Kidney)	1986	1989	4	0.467
AIDS Services	1986	1987	2	0.926
Continuing Care Case Management	1986	1987	2	0.773
Contraceptive Care	1986	1987	2	0.646
Genetic Counseling Screening	1986	1987	2	0.532
Satellite Geriatric Clinics	1986	1987	2	0.278
Child Adolescent Psychiatric Services	1987	1990	4	0.872
Geriatric Psychiatric Services	1987	1990	4	0.839
Psychiatric Education	1987	1990	4	0.887
AIDS (Outpatient)	1988	1990	3	0.414
AIDS General Inpatient Care	1988	1990	3	0.980
AIDS/ARC Unit	1988	1990	3	0.247
AIDS/HIV Testing	1988	1990	3	0.969
Alzheimer's Diagnostic Assessment Services	1988	1990	3	0.596
Emergency Response for Elderly	1988	1990	3	0.932

Geriatic Clinic	1988	1990	3	0.496
In Vitro Fertilization	1988	1990	3	0.379
Medicare Certified Distinct Part Skilled Nursing Unit	1988	1990	3	0.886
Organized Social Work Services	1988	1990	3	0.989
Other Skilled Nursing Care	1988	1990	3	0.891
Senior Membership Program	1988	1990	3	0.737
Angioplasty	1989	1990	2	0.708
Arthritis Treatment Center	1989	1990	2	0.485
Emergency Social Work Services	1989	1990	2	0.911
Freestanding Outpatient Center	1989	1990	2	0.686
Hospital Based Outpatient Care Center	1989	1990	2	0.998
Orthopedic Surgery	1989	1990	2	0.972
Outpatient Social Work Services	1989	1990	2	0.939
Bone Marrow Transplant	1990	1990	1	0.301
Cardiac Rehabilitation	1990	1990	1	0.924
Non-Invasive Cardiac Assessment	1990	1990	1	0.970
Positron Emission Tomography Scanner	1990	1990	1	0.267
Single Photo Emission Computed Tomography	1990	1990	1	0.754
Stereotactic Radiosurgery	1990	1990	1	0.415
Tissue Transplant	1990	1990	1	0.432

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

Appendix Table A2: Results Leaving Out Each State in Census South

Panel A: IV Results

Dependent Variable: Total Hospital Expenditures

	All South	Drop AL	Drop AR	Drop DE	Drop FL	Drop FL	Drop GA	Drop KY	Drop LA	Drop MD	Drop MS	Drop NC	Drop OK	Drop SC	Drop TN	Drop TX	Drop WV
$\log(\text{Income})_{jt}$	0.723	0.702	0.725	0.695	0.725	0.694	0.838	0.714	0.706	0.655	0.782	0.677	0.823	0.764	0.680	0.461	0.750
	(0.214)	(0.226)	(0.216)	(0.216)	(0.216)	(0.219)	(0.183)	(0.212)	(0.272)	(0.215)	(0.214)	(0.235)	(0.184)	(0.223)	(0.222)	(0.695)	(0.248)
	[0.004]	[0.008]	[0.005]	[0.006]	[0.005]	[0.007]	[0.000]	[0.005]	[0.021]	[0.009]	[0.003]	[0.012]	[0.001]	[0.004]	[0.009]	[0.518]	[0.009]
R^2	0.968	0.969	0.967	0.969	0.967	0.968	0.967	0.968	0.969	0.970	0.967	0.968	0.967	0.969	0.968	0.974	0.969
N	2065	1877	1918	2044	2054	2002	1900	1897	1939	1981	1939	1918	1897	1939	1918	1813	1939

Panel B: First Stage Results

Dependent Variable: Income

	All South	Drop AL	Drop AR	Drop DE	Drop DC	Drop FL	Drop GA	Drop KY	Drop LA	Drop MD	Drop MS	Drop NC	Drop OK	Drop SC	Drop TN	Drop TX	Drop WV
Oil Reserves_j ×	9.245	9.312	9.386	9.182	9.205	9.236	9.347	9.874	8.997	9.007	9.349	8.660	8.892	9.051	9.164	21.641	8.236
$\log(\text{oil price})_{t-1}$	(2.216)	(2.375)	(2.303)	(2.222)	(2.215)	(2.285)	(2.356)	(2.363)	(2.015)	(2.221)	(2.303)	(2.127)	(1.678)	(2.265)	(2.311)	(4.879)	(1.831)
	[0.001]	[0.002]	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]	[0.000]	[0.001]	[0.001]	[0.001]	[0.001]
R^2	0.983	0.983	0.983	0.983	0.983	0.983	0.984	0.983	0.984	0.984	0.983	0.983	0.983	0.983	0.982	0.984	0.984
N	2065	1877	1918	2044	2054	2002	1900	1897	1939	1981	1939	1918	1897	1939	1918	1813	1939
F-statistic	17.41	15.37	16.61	17.08	17.26	16.33	15.73	17.45	19.93	16.45	16.48	16.58	28.09	15.97	15.73	19.67	20.24

Notes: Table reports estimates of variants of estimating equation (10) by IV in Panel A and equation (11) by OLS in Panel B. In all specifications income and hospital expenditures are divided by hospital-utilization weighted measure of population (HUWP) and then logged. First column shows results from our baseline sample of all Southern states between 1970 and 1990 (see column 7 of Table 3 and column 3 of Table 4). Subsequent columns show the results when the state specified in the column heading is omitted from the analysis. Unit of observation is an economic sub-region (ESR)-year; all regressions include ESR and year fixed effects. Standard errors, adjusted to allow for an arbitrary variance-covariance matrix for each state over time, are in parentheses and p-values are in brackets.

Appendix Table A3: Examination of Identifying Assumption

	(1) Baseline	(2) 5-year Lead	(3) Horse Race	(4) Region Trends	(5) State Trends	(6) 1970-1984 Subsample	(7) Falsification Test
	Panel A: IV and Reduced Form OLS Results						
	Dependent Variable: Hospital Expenditures						
	IV	IV	IV	IV	IV	RF	RF
$\log(\text{Income})_{jt}$	0.723	0.992	0.697	0.352	0.131		
	(0.214)	(0.306)	(0.283)	(0.192)	(0.118)		
	[0.004]	[0.005]	[0.027]	[0.088]	[0.286]		
Oil Reserves_j ×						4.980	-3.107
$\log(\text{oil price})_{t-1}$						(1.656)	(4.044)
						[0.009]	[0.455]
Oil Reserves_j ×		-11.322					
$\log(\text{oil price})_{t+5}$		(7.830)					
		[0.169]					
R^2	0.968	0.964	0.970	0.972	0.976	0.966	0.980
N	2065	2065	2054	2065	2065	1471	1487
	Panel B: First Stage Results						
	Dependent Variable: Income						
Oil Reserves_j ×	9.245	8.186	8.219	11.722	13.774	14.172	
$\log(\text{oil price})_{t-1}$	(2.271)	(2.157)	(2.387)	(3.004)	(3.951)	(3.481)	
	[0.001]	[0.002]	[0.004]	[0.001]	[0.003]	[0.001]	
Oil Reserves_j ×		4.821					
$\log(\text{oil price})_{t+5}$		(3.291)					
		[0.164]					
R^2	0.983	0.983	0.984	0.984	0.985	0.986	
N	2065	2065	2054	2065	2065	1471	
F-statistic	16.577	14.396	11.853	15.222	12.154	16.571	

Notes: Table reports results from estimating variants of equation (10) by IV in Panel A, except in columns 6 and 7 which show variants of equation (12) estimated by OLS in Panel A; table reports results from estimating variants of equation (11) by OLS in Panel B. All dependent variables are in logs. In all columns hospital expenditures and income are divided by a hospital-utilization weighted measure of population (HUWP) before taking logs. Unit of observation is an economic sub-region (ESR)-year, and all columns include ESR and year fixed effects. In columns 1 through 5 the sample is all Southern states between 1970 and 1990. Column 1 reproduces baseline results (see column 7 of Table 3 and column 3 of Table 4). Column 2 includes a 5-year lead of the instrument as a control variable. Column 3 includes several additional interaction terms as control variables in a "horse race"; the interaction terms are the log oil price interacted with each of the following variables: hospital expenditures in 1969, hospital beds in 1969, population in 1970, wage bill in 1970, and employment in 1970. Column 4 adds region-specific linear time trends for the three Census regions in the South. Column 5 includes state-specific linear time trends for the 16 Southern states. Column 6 produces the first stage and reduced form results for 1970 to 1984 as comparison to the falsification test in column 7, which "grafts" the same oil price series in 1970 to 1984 onto the hospital data in 1955 to 1969. Standard errors, adjusted to allow for an arbitrary variance-covariance matrix for each state over time, are in parentheses and p-values are in brackets.

Appendix Table A4: Alternative Specifications of Instrument

Panel A: IV Results

Dependent Variable: Hospital Expenditures

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

Panel B: First Stage Results

Dependent Variable: Income

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

Notes: Table reports estimates of variants of estimating equation (10) by IV in Panel A and equation (11) by OLS in Panel B. The specifications vary in their definition of the instrument, which is given in the left-hand column of Panel B. Unit of analysis is an economic sub-region (ESR)-year. All dependent variables are in logs. In all columns hospital expenditures and income are divided by a hospital-utilization weighted measure of population (HUWP) before taking logs. The sample is ESRs in Southern states between 1970 and 1990. Column 1 reproduces baseline results (see column 7 of Table 3 and column 3 of Table 4). 1(Oil Reserves > 0) is an indicator variable for whether the ESR has any large oil wells. All columns include ESR fixed effects and year fixed effects. Standard errors, adjusted to allow for an arbitrary variance-covariance matrix for each state over time, are in parentheses and p-values are in brackets.

Appendix Table A5: IV-GLS and Lagged Depedendent Variable

	Dependent Variable: Hospital Expenditures						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Baseline IV	IV-GLS	IV-GLS	IV-GLS	IV-GLS	Lagged Dep. Var. IV	Arellano-Bond IV
Within-panel serial correlation	Cluster at State	Common AR(1)	Common AR(2)	State-specific AR(1)	State-specific AR(2)	Cluster at State	Cluster at State
log(Income)$_{jt}$	0.697	0.963	1.111	0.724	0.770	0.491	0.120
(*A*)	(0.216)	(0.505)	(0.681)	(0.263)	(0.287)	(0.135)	(0.067)
	[0.006]	[0.057]	[0.103]	[0.006]	[0.007]	[0.002]	[0.075]
log(Total Hospital Exp.)$_{t-1}$						0.426	0.154
(*B*)						(0.088)	(0.047)
						[0.000]	[0.001]
Implied long-run effect						0.856	0.142
(*A*/(1-*B*))						(0.214)	(0.080)
						[0.001]	[0.077]
N	2016	2016	2016	2016	2016	1966	1966

Notes: Table reports results from estimating variants of equation (10) by IV. The sample is all Southern states between 1970 and 1990. Unit of observation is an economic sub-region (ESR)-year. All specifications include ESR fixed effects and year fixed effects. In all columns, income and hospital expenditures are divided by a hospital-utilization weighted measure of population (HUWP) before taking logs. For columns 1 through 5, the baseline sample is modified to only include the 96 (of 99) ESRs with data for all 21 years between 1970 and 1990. Column 1 produces baseline IV results with this modified sample. Columns 2 through 5 report IV-GLS results. In column 2, ρ_1 is estimated to be 0.585. In column 3, ρ_1 is estimated to be 0.508 and ρ_2 is estimated to be 0.127. In column 4, ρ_1 is estimated separately by state; estimated values of ρ_1 range from 0.155 to 0.887 with mean 0.604 and s.d. 0.240. In column 5, ρ_1 and ρ_2 are estimated separately by state; estimated values of ρ_1 range from 0.118 to 0.747 with mean 0.487 and s.d. 0.200, and estimated values of ρ_2 range from 0.041 to .341 with mean 0.192 and s.d. 0.083. Column 6 includes a lagged dependent variable as a control. Column 7 uses the Arellano-Bond GMM dynamic panel estimator. In columns 6 and 7 the standard error on the implied long-run effect is estimated using the delta method.

Appendix Table A6: Hospital Spending Versus Overall Health Spending

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Regression:	First Stage OLS	IV	IV	IV	IV	IV	IV	IV	IV
Dependent Variable:	Income	Total Health Care Exp.	Hospital Exp.	Physician and Other Services	Dental Services	Drugs and Other Medical Non-durables	Vision Products	Nursing Care	Other Health Services
Panel A: Southern States Only									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Oil Reserves$_j$ × log(oil price)$_{t-1}$	3.626								
	(0.776)								
	[0.000]								
log(Income)$_{jt}$		0.055	0.067	0.179	0.622	0.248	1.187	-1.302	-0.359
		(0.077)	(0.157)	(0.152)	(0.100)	(0.120)	(0.516)	(0.321)	(0.228)
		[0.484]	[0.675]	[0.257]	[0.000]	[0.057]	[0.036]	[0.001]	[0.137]
R^2	0.985	0.998	0.995	0.996	0.991	0.993	0.914	0.926	0.963
N	236	236	236	236	236	236	236	236	236
F-statistic	21.81								
Share of Total Health Care Exp.			46.30%	24.73%	5.17%	11.33%	1.80%	7.02%	3.44%
Panel B: All U.S.									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Oil Reserves$_j$ × log(oil price)$_{t-1}$	3.162								
	(0.586)								
	[0.000]								
log(Income)$_{jt}$		0.098	0.139	0.365	0.650	0.307	0.748	-1.944	-0.953
		(0.167)	(0.151)	(0.186)	(0.173)	(0.112)	(0.824)	(0.968)	(0.758)
		[0.558]	[0.361]	[0.056]	[0.000]	[0.009]	[0.368]	[0.050]	[0.214]
R^2	0.98	0.996	0.965	0.974	0.986	0.989	0.879	0.918	0.915
N	729	729	729	729	729	729	729	729	729
F-statistic	29.11								
Share of Total Health Care Exp.			45.06%	25.04%	6.07%	10.40%	2.02%	8.57%	3.39%

Notes: Table reports first stage results of estimating equation (11) by OLS in column 1; remaining columns report estimates of variants of estimating equation (10) by IV. Unit of observation is a State-year in all columns. Dependent variables are various measures of health care expenditures from the Health Care Finance Administration (HCFA). HCFA data are available in 1972, 1976 - 1978, and 1980-1990. All dependent variables and income are in logs and divided by a hospital-utilization weighted measure of population (HUWP). In all columns income is divided by HUWP before taking logs. Sample is Southern states in Panel A and All U.S. (except Alaska and Virginia) in Panel B. Standard errors, adjusted to allow for an arbitrary variance-covariance matrix for each state over time, are in parentheses and p-values are in brackets.

Appendix Table A7: Labor Income vs. All Income

	Panel A: IV Results			
	Dependent Variable: Hospital Expenditures			
	(1)	(2)	(3)	(4)
log(Income)$_{jt}$	0.550	0.451	0.740	0.568
	(0.230)	(0.160)	(0.359)	(0.263)
	[0.030]	[0.013]	[0.045]	[0.036]
R^2	0.992	0.993	0.981	0.982
N	326	326	1015	1015
	Panel B: First Stage Results			
	Dependent Variable: Income			
	(1)	(2)	(3)	(4)
Oil Reserves$_j$ × log(oil price)$_{t-1}$	2.564	3.128	2.220	2.895
	(0.523)	(0.851)	(0.443)	(0.682)
	[0.000]	[0.002]	[0.000]	[0.000]
R^2	0.989	0.990	0.985	0.983
N	326	326	1015	1015
F-statistic	24.05	13.50	25.10	18.05
	Specification			
	(1)	(2)	(3)	(4)
Income definition	Payroll	GSP	Payroll	GSP
Geographic sample	South	South	USA	USA

Notes: Table reports estimates of variants of estimating equation (10) by IV in Panel A and equation (11) by OLS in Panel B. Unit of observation is a State-year in all columns. In all specifications income and hospital expenditures are divided by hospital-utilization weighted measure of population (HUWP) and then logged. Bottom rows define the specification variants; these are the definition of income (Payroll as in the baseline specification or Gross State Product (GSP)) and the geographic sample (South or all US). In all columns the years of analysis are 1970 - 1990. The sample is all Southern states between 1970 and 1990 in columns 1 and 2; columns 3 and 4 expand sample to all US (except Alaska and Virginia). Column 1 reproduces results from column 6 in Table 4 (Panel A) and column 8 of Table 3 (Panel B). All regressions include state and year fixed effects. Standard errors, adjusted to allow for an arbitrary variance-covariance matrix for each state over time, are in parentheses and p-values are in brackets.

Appendix Table A8: Income Elasticity of Other Goods

	(1)	(2)	(3)	(4)	(5)	(6)
	Industry-specific Gross State Product					
	Amuse-ment	Hotels	Legal Services	Other Services	Food	Health Services
	Panel A: Southern States Only					
log(Income)$_{jt}$	0.900	0.835	1.635	1.375	-0.009	-0.048
	(0.385)	(0.319)	(0.317)	(0.387)	(0.416)	(0.181)
	[0.034]	[0.019]	[0.000]	[0.003]	[0.984]	[0.793]
R^2	0.984	0.984	0.991	0.989	0.965	0.996
N	326	326	326	308	324	326
	Panel B: All U.S.					
log(Income)$_{jt}$	1.080	0.940	1.749	1.400	0.255	0.207
	(0.384)	(0.397)	(0.291)	(0.270)	(0.356)	(0.412)
	[0.007]	[0.022]	[0.000]	[0.000]	[0.477]	[0.617]
R^2	0.975	0.978	0.988	0.984	0.977	0.994
N	1013	1015	1015	989	1013	1015

Notes: Table reports results from estimating variants of equation (10) by IV. Dependent variables are given in column headings. All dependent variables are in logs, and all dependent variables and income are divided by a hospital-utilization weighted measure of population (HUWP) before taking logs. The sample is all Southern states between 1970 and 1990 in Panel A and all US states (except Alaska and Virginia) between 1970 and 1990 in Panel B. Unit of analysis is a state-year. All columns include state and year fixed effects. Dependent variable is the Gross State Product for various industries, as indicated by column headings. Standard errors, adjusted to allow for an arbitrary variance-covariance matrix for each state over time, are in parentheses and p-values are in brackets.

Appendix Table A9: Decomposition and Tests for Nonlinear Effects

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

Notes: Table reports IV estimates of variants of equation (10) in columns 1 and 2 and OLS estimates of a variant of equation (12) in columns 3 and 4. The unit of anlaysis is an economic sub-region (ESR)-year, and the regressions include ESR fixed effects and year fixed effects. All dependent variables are in logs. In all columns hospital expenditures and income are divided by a hospital-utilization weighted measure of population (HUWP) before taking logs. The sample is all Southern states between 1970 and 1990. Note that the results in columns 1 and 3 differ slightly from baseline results in Table 4 because the sample does not include Washington, DC (DC is dropped because there is no data for DC in the 1970s). Standard errors, adjusted to allow for an arbitrary variance-covariance matrix for each state over time, are in parentheses and p-values are in brackets.

Appendix Table A10: Heterogeneity Across Geography and Time

Panel A: IV Results

Dependent Variable: Hospital Expenditures

	(1)	(2)	(3)	(4)	(5)
$\log(\text{Income})_{jt}$	0.723	0.700	0.804	*N/A*	0.853
	(0.214)	(0.368)	(0.633)		(0.439)
	[0.004]	[0.106]	[0.210]		[0.071]
R^2	0.968	0.967	0.956		0.970
N	2065	1070	4915		3547

Panel B: First Stage Results

Dependent Variable: Income

	(1)	(2)	(3)	(4)	(5)
Oil Reserves_j ×	9.245	6.237	7.094	1.481	7.966
$\log(\text{oil price})_{t-1}$	(2.271)	(1.655)	(2.375)	(1.882)	(1.930)
	[0.001]	[0.009]	[0.004]	[0.443]	[0.001]
R^2	0.983	0.985	0.982	0.984	0.986
N	2065	1070	4915	3547	3547
F-statistic	16.58	14.21	8.92	0.62	17.04

Specification

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

Notes: Table reports estimates of variants of estimating equation (10) by IV in Panel A and equation (11) by OLS in Panel B. All dependent variables and income are in logs and divided by a hospital-utilization weighted measure of population (HUWP) before taking logs. Unit of analysis is an economic sub-region (ESR)-year in all columns, and all columns include ESR fixed effects and year fixed effects. Bottom rows define the specification variants. The baseline sample is all Southern states between 1970 and 1990. Column 1 reproduces baseline results from column 7 in Table 3 and column 3 in Table 4. Standard errors, adjusted to allow for an arbitrary variance-covariance matrix for each state over time, are in parentheses and p-values are in brackets. Because there is no statistically significant first stage in column 4, the IV results are not reported.

Appendix Table A11: Augmented Dickey-Fuller Tests

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

Notes: Table based on annual data on oil prices from 1950 to 2005 (see Figure 2). Standard errors are in parentheses and p-values are in brackets.

Appendix Table A12: Short-run versus Long-run Effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent variable:	Income	Hospital Expenditures	Hospital Expenditures	Income	Hospital Expenditures	Hospital Expenditures	Hospital Expenditures
	Baseline	Baseline	Baseline	10-year	10-year	10-year	3-year avg.
	FS	RF		FS	RF		
	OLS	OLS	IV	OLS	OLS	IV	IV
Oil Reserves$_j$ ×	9.245	6.680		7.621	6.050		
log(oil price)$_{t-1}$	(2.271)	(2.099)		(2.643)	(2.628)		
	[0.001]	[0.006]		[0.011]	[0.036]		
log(Income)$_{jt}$			0.723			0.794	0.826
			(0.214)			(0.411)	(0.231)
			[0.004]			[0.073]	[0.003]
R^2	0.983	0.973	0.968	0.986	0.986	0.981	0.976
N	2065	2065	2065	296	296	296	690
F-statistic	16.577			8.318			

Notes: Table reports results of estimating equations (10), (11) or (12) by OLS or IV as indicated. All dependent variables are in logs. Unit of analysis is an economic sub-region (ESR)-year, and all columns include ESR fixed effects and year fixed effects. In all columns income and hospital expenditures are divided by a hospital-utilization weighted measure of population (HUWP) before taking logs. Columns 1 through 3 are the baseline sample of all Southern states between 1970 and 1990; in columns 4 through 6, only observations from 1970, 1980, and 1990 are included. Column 7 uses 3-year averages of all variables (see Appendix Section B for more details). Standard errors, adjusted to allow for an arbitrary variance-covariance matrix for each state over time, are in parentheses and p-values are in brackets.

Appendix Table A13: Replication of Robustness Analysis for Other Dependent Variables

Dependent Variable:	(1) Total Hospital Expenditures	(2) Total Hospital Payroll	(3) FTE	(4) RN/ (RN+LPN)	(5) Admissions	(6) In-Patient Days	(7) Beds	(8) Number of Hospitals	(9) Number of Technologies	(10) Open-Heart Surgery	(11) Radioisotope Therapy
Panel A: Baseline Results (reproduced from Table 5)											
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
$\log(\text{Income})_{jt}$	0.723	0.934	0.039	0.329	-0.430	-1.034	-0.698	-0.552	-0.132	-3.163	1.083
	(0.214)	(0.233)	(0.222)	(0.089)	(0.193)	(0.488)	(0.455)	(0.358)	(0.221)	(11.334)	(2.575)
	[0.004]	[0.001]	[0.862]	[0.002]	[0.042]	[0.051]	[0.146]	[0.144]	[0.558]	[0.169]	[0.545]
N	2065	2064	2065	1576	2065	1967	2065	2065	2065	849	262
Panel B: No Population Adjustment (see Table 4, column (4))											
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
$\log(\text{Income})_{jt}$	0.801	0.953	0.311	0.266	-0.025	-0.451	-0.217	-0.395	-0.095	-1.154	1.537
	(0.155)	(0.167)	(0.175)	(0.072)	(0.138)	(0.324)	(0.295)	(0.262)	(0.161)	(3.929)	(3.467)
	[0.000]	[0.000]	[0.096]	[0.002]	[0.861]	[0.184]	[0.474]	[0.152]	[0.564]	[0.187]	[0.259]
N	2065	2064	2065	1576	2065	1967	2065	2065	2065	849	262
Panel C: Per Capita Population Adjustment (see Table 4, column (5))											
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
$\log(\text{Income})_{jt}$	0.665	0.920	-0.161	0.415	-0.728	-1.468	-1.053	-0.667	-0.160	-2.450	1.894
	(0.263)	(0.282)	(0.297)	(0.112)	(0.265)	(0.645)	(0.584)	(0.447)	(0.272)	(9.014)	(3.297)
	[0.023]	[0.005]	[0.595]	[0.002]	[0.015]	[0.038]	[0.092]	[0.156]	[0.565]	[0.194]	[0.278]
N	2065	2064	2065	1576	2065	1967	2065	2065	2065	849	262
Panel D: State-level Results (see Table 4, column (6))											
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
$\log(\text{Income})_{jt}$	0.550	0.865	-0.119	0.326	-0.333	0.258	0.294	-0.028	0.819	N/A	N/A
	(0.230)	(0.187)	(0.181)	(0.095)	(0.187)	(0.204)	(0.212)	(0.287)	(1.672)		
	[0.030]	[0.000]	[0.520]	[0.004]	[0.095]	[0.226]	[0.186]	[0.924]	[0.631]		
N	326	326	326	251	326	311	326	326	322		
Panel E: State GSP instead of Income, Census South (see Table A7, column (2))											
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
$\log(\text{Income})_{jt}$	0.451	0.709	-0.098	0.274	-0.273	0.212	0.241	-0.023	0.673	N/A	N/A
	(0.160)	(0.126)	(0.154)	(0.100)	(0.160)	(0.165)	(0.172)	(0.236)	(1.323)		
	[0.013]	[0.000]	[0.534]	[0.015]	[0.109]	[0.218]	[0.182]	[0.924]	[0.619]		
N	326	326	326	251	326	311	326	326	322		
Panel F: State-level Results, All U.S. (see Table A7, column (3))											
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
$\log(\text{Income})_{jt}$	0.740	0.863	-0.086	0.407	-0.188	-0.009	0.109	0.098	1.054	N/A	N/A
	(0.359)	(0.398)	(0.244)	(0.112)	(0.342)	(0.268)	(0.395)	(0.295)	(1.367)		
	[0.045]	[0.035]	[0.726]	[0.001]	[0.585]	[0.973]	[0.783]	[0.741]	[0.444]		
N	1015	1015	1015	777	1015	967	1015	1015	1011		
Panel G: State GSP instead of Income, All U.S. (see Table A7, column (4))											
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
$\log(\text{Income})_{jt}$	0.568	0.662	-0.066	0.331	-0.144	-0.007	0.084	0.075	0.812	N/A	N/A
	(0.263)	(0.298)	(0.189)	(0.102)	(0.264)	(0.205)	(0.303)	(0.225)	(1.023)		
	[0.036]	[0.031]	[0.728]	[0.002]	[0.588]	[0.973]	[0.783]	[0.740]	[0.431]		
N	1015	1015	1015	777	1015	967	1015	1015	1011		
Panel H: Drop States with No Large Oil Wells (see Table A10, column (2))											
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
$\log(\text{Income})_{jt}$	0.700	0.852	-0.042	0.487	-0.630	-2.089	-1.816	-1.098	-0.047	-2.760	2.996
	(0.368)	(0.378)	(0.544)	(0.124)	(0.305)	(0.578)	(0.626)	(0.513)	(0.237)	(22.066)	(10.381)
	[0.106]	[0.065]	[0.940]	[0.008]	[0.085]	[0.011]	[0.027]	[0.076]	[0.850]	[0.299]	[0.377]
N	1070	1070	1070	815	1070	1019	1070	1070	1070	399	128
Panel I: ESR-level Results, All U.S. (see Table A10, column (3))											
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
$\log(\text{Income})_{jt}$	0.804	0.882	0.030	0.503	-0.330	-0.598	-0.595	-0.383	-0.191	-2.135	1.292
	(0.633)	(0.557)	(0.352)	(0.126)	(0.406)	(0.439)	(0.605)	(0.320)	(0.325)	(9.394)	(7.234)
	[0.210]	[0.120]	[0.932]	[0.000]	[0.420]	[0.180]	[0.330]	[0.237]	[0.560]	[0.302]	[0.168]
N	4915	4914	4915	3749	4915	4681	4915	4915	4915	1906	503
Panel J: 1970-2005 + state-specific linear time trends (see Table A10, column (5))											
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
$\log(\text{Income})_{jt}$	0.853	0.935	0.591	0.281	-0.038	-0.486	-0.064	-0.650	0.039	N/A	N/A
	(0.439)	(0.565)	(0.761)	(0.063)	(0.197)	(0.230)	(0.350)	(0.410)	(0.110)		
	[0.071]	[0.119]	[0.450]	[0.000]	[0.850]	[0.052]	[0.858]	[0.134]	[0.729]		
N	3547	3546	3547	3058	3547	3449	3547	3547	2164		
Panel K: Decadal Panel (see Table A12, column (6))											
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
$\log(\text{Income})_{jt}$	0.794	0.921	0.071	0.625	-0.510	-1.562	-0.771	-0.728	-0.311	N/A	N/A
	(0.411)	(0.541)	(0.548)	(0.250)	(0.306)	(0.846)	(0.689)	(0.491)	(0.355)		
	[0.073]	[0.109]	[0.898]	[0.025]	[0.117]	[0.085]	[0.281]	[0.159]	[0.396]		
N	296	296	296	296	296	296	296	296	296		

Panel L: 3-year Averages (see Table A12, column (7))

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
$\log(\text{Income})_{jt}$	0.826	0.954	0.235	0.259	-0.369	-0.719	-0.614	-0.635	-0.162	N/A	N/A
	(0.231)	(0.216)	(0.169)	(0.101)	(0.175)	(0.429)	(0.434)	(0.371)	(0.255)		
	[0.003]	[0.001]	[0.185]	[0.022]	[0.052]	[0.115]	[0.178]	[0.107]	[0.534]		
N	690	690	690	592	690	690	690	690	690		

Panel M: Include 5-year Lead (see Table A3, column (2))

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
$\log(\text{Income})_{jt}$	0.992	1.188	-0.023	0.360	-0.563	-0.979	-0.824	-0.761	-0.175	N/A	N/A
	(0.306)	(0.263)	(0.224)	(0.086)	(0.271)	(0.588)	(0.592)	(0.518)	(0.321)		
	[0.005]	[0.000]	[0.918]	[0.001]	[0.056]	[0.117]	[0.185]	[0.163]	[0.594]		
N	2065	2064	2065	1576	2065	1967	2065	2065	2065		

Panel N: Horse Race (see Table A3, column (3))

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
$\log(\text{Income})_{jt}$	0.697	0.748	0.117	0.284	-0.617	-0.380	0.084	-0.676	0.314	N/A	N/A
	(0.283)	(0.284)	(0.275)	(0.142)	(0.278)	(0.346)	(0.318)	(0.397)	(0.226)		
	[0.027]	[0.020]	[0.678]	[0.065]	[0.044]	[0.290]	[0.794]	[0.110]	[0.187]		
N	2054	2053	2054	1565	2054	1956	2054	2054	2054		

Panel O: Region-specific linear time trends (see Table A3, column (4))

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
$\log(\text{Income})_{jt}$	0.352	0.439	0.053	0.262	-0.014	0.279	-0.740	-0.382	-0.040	-2.699	2.023
	(0.192)	(0.254)	(0.287)	(0.062)	(0.127)	(0.071)	(0.371)	(0.210)	(0.134)	(11.844)	(6.711)
	[0.088]	[0.104]	[0.855]	[0.001]	[0.913]	[0.001]	[0.065]	[0.090]	[0.769]	[0.312]	[0.239]
N	2065	2064	2065	1576	2065	1967	2065	2065	2065	849	262

Panel P: Oil price in levels (see Table A1, column (2))

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
$\log(\text{Income})_{jt}$	0.491	0.648	-0.038	0.230	-0.258	-0.657	-0.506	-0.393	-0.110	-1.392	2.089
	(0.145)	(0.135)	(0.127)	(0.061)	(0.119)	(0.298)	(0.294)	(0.243)	(0.159)	(8.824)	(3.380)
	[0.004]	[0.000]	[0.768]	[0.002]	[0.047]	[0.044]	[0.106]	[0.127]	[0.499]	[0.479]	[0.191]
N	2065	2064	2065	1576	2065	1967	2065	2065	2065	849	262

Panel Q: Oil price at time t instead of t-1 (see Table A4, column (3))

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
$\log(\text{Income})_{jt}$	0.640	0.823	0.103	0.342	-0.315	-0.996	-0.605	-0.469	-0.173	-3.064	2.016
	(0.194)	(0.233)	(0.222)	(0.104)	(0.171)	(0.458)	(0.403)	(0.306)	(0.210)	(13.207)	(7.030)
	[0.005]	[0.003]	[0.649]	[0.005]	[0.085]	[0.046]	[0.154]	[0.147]	[0.423]	[0.240]	[0.242]
N	2065	2064	2065	1576	2065	1967	2065	2065	2065	849	262

Panel R: max(Oil Reserves, 95th Percentile) (see Table A4, column (4))

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
$\log(\text{Income})_{jt}$	0.632	0.888	-0.032	0.336	-0.410	-1.025	-0.711	-0.459	-0.145	-2.717	2.003
	(0.205)	(0.227)	(0.206)	(0.095)	(0.187)	(0.482)	(0.454)	(0.335)	(0.225)	(10.449)	(3.490)
	[0.008]	[0.001]	[0.879]	[0.003]	[0.045]	[0.050]	[0.138]	[0.190]	[0.527]	[0.237]	[0.255]
N	2065	2064	2065	1576	2065	1967	2065	2065	2065	849	262

Panel S: Has Large Oil Wells Dummy (see Table A4, column (5))

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
$\log(\text{Income})_{jt}$	1.095	1.377	0.076	0.297	-0.169	0.078	0.014	0.384	-0.322	0.597	-0.669
	(0.670)	(0.661)	(0.507)	(0.210)	(0.416)	(0.753)	(0.681)	(0.505)	(0.343)	(6.238)	(4.904)
	[0.123]	[0.055]	[0.883]	[0.177]	[0.691]	[0.919]	[0.984]	[0.459]	[0.362]	[0.458]	[0.631]
N	2065	2064	2065	1576	2065	1967	2065	2065	2065	849	262

Panel T: Oil Wells Dummy x Mining Share of Labor Force in 1970 (see Table A4, column (6))

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
$\log(\text{Income})_{jt}$	0.860	0.975	-0.219	0.189	-0.391	-0.236	0.005	-0.113	0.344	-1.141	-0.212
	(0.870)	(0.805)	(0.484)	(0.162)	(0.450)	(0.877)	(0.871)	(0.584)	(0.604)	(9.114)	(3.217)
	[0.339]	[0.245]	[0.658]	[0.262]	[0.399]	[0.792]	[0.995]	[0.849]	[0.577]	[0.278]	[0.869]
N	2065	2064	2065	1576	2065	1967	2065	2065	2065	849	262

Panel U: Lagged Dependent Variable (see Table A5, column (6))

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
$\log(\text{Income})_{jt}$	0.856	0.951	0.157	0.048	-0.373	-0.505	-0.638	-0.611	-0.114	N/A	N/A
	(0.214)	(0.181)	(0.156)	(0.019)	(0.168)	(0.518)	(0.414)	(0.353)	(0.239)		
	[0.001]	[0.000]	[0.331]	[0.024]	[0.042]	[0.344]	[0.144]	[0.104]	[0.641]		
N	1963	1961	1963	988	1963	1768	1963	1963	1963		

Notes: This table shows robustness results across all of the dependent variables in Table 5 (Panel A reproduces baseline results in Table 5 for comparison). The table and column number in each panel heading references the specification that is being shown; see notes in main tables for details on the various specifications. In all panels, the first column replicates the robustenss analysis shown in the referenced table for total hospital expenditures. In all panels, the dependent variable in columns 4, 8, and 9 is not adjusted for population. 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 some panels, there is not enough variation to estimate the Cox proportional hazard models in columns (10) and (11); we place "N/A" in these cells. We do not include robustness tests for the IV-GLS results in Table A5 because several of the alternative dependent variables are missing data for various years, making estimation of the AR(1) and AR(2) coefficients difficult because of the "gaps" in the panel data set. The results reported in Panel U (lagged dependent variable specification) are the implied long-run effects.

Appendix Table A14: Monte Carlo Simulation Results

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

Notes: This table reports results from the Monte Carlo study described in Appendix (Section D). Each cell displays the mean of the parameter estimates from 500 simulations; standard deviation of parameter estimates is reported below in parentheses.