Published in final edited form as:
Q J Econ. 2005 January ; 120(1): 345–386.

Fiscal Shenanigans, Targeted Federal Health Care Funds, and Patient Mortality

Katherine Baicker and
Dartmouth College and NBER

Douglas Staiger
Dartmouth College and NBER

Abstract

We explore the effectiveness of matching grants when lower levels of government can expropriate some of the funds for other uses. Using data on the Medicaid Disproportionate Share program, we identify states that were most able to expropriate funds. Payments to public hospitals in these states were systematically diverted and had no significant impact on patient mortality. Payments that were not expropriated were associated with significant declines in patient mortality. Overall, subsidies were an effective mechanism for improving outcomes for the poor, but the impact was limited by the ability of state and local governments to divert the targeted funds.

I. Introduction

The general theory of fiscal federalism suggests that intergovernmental matching grants are an important mechanism for internalizing externalities across local jurisdictions, while maintaining the benefits of local control to satisfy heterogeneous demands for public goods (Oates, 1999). There are many examples of government programs that use intergovernmental grants to subsidize (or tax) spending controlled by lower levels of government. The federal Medicaid program (and until recently AFDC) is administered by the states but subsidized with federal matching grants. Similarly, school finance equalization schemes use state funds to subsidize and tax school spending at the local level (Hoxby, 2001). The effectiveness of these mechanisms is, however, limited when lower levels of government are able to misrepresent their contributions to the program. For example, local schools may move various discretionary expenses into or out of the school budget depending on whether such expenses are subsidized or taxed, while states may shift expenses from previously state-funded programs to Medicaid in order to reap the federal match. These kinds of fiscal manipulations by local jurisdictions increase the cost of the program to higher levels of government with little real change in the provision local public goods, thereby limiting the ability of higher levels of government to influence local spending through taxes or subsidies.

We investigate how such fiscal shenanigans limit the effectiveness of matching grants. Using a simple model, we show that the ability of a lower level of government to use such schemes has two distinct effects. First, it increases the cost of the program to the granting government, since localities are able to increase the effective match rate. Second, it decreases the effect of the matching grant on total program resources, since localities are able to avoid increasing their own contribution. Taken together, fiscal shenanigans lead to a more expensive yet less effective program.

We evaluate the empirical implications of this model for a large federal program that subsidizes hospitals serving the poor. We focus on two related questions. First, how does this program distort the behavior of state and local governments who wish to expropriate the funds for other uses? Second, to the extent that the program does increase resources devoted to the targeted population, do patients benefit? The answers to these two questions shed light on whether (and at what cost) federal matching funds are able to achieve their goals.

Our empirical analysis uses nationwide data on the Medicaid Disproportionate Share Hospital (DSH) program, a federal-state program targeted to hospitals serving the poor. Surprisingly little is known about the impact of this program on patient care, despite spending of nearly $200 billion during the 1990s. We begin by investigating the extent to which state governments expropriated these DSH funds through creative financing mechanisms. Recent reports by the GAO (2000) and Coughlin et al. (2000) suggest that state governments were able to capture much of the DSH payments through various mechanisms, and we present additional direct evidence that these mechanisms were used most in government-owned hospitals in states with the most to gain. We use the results of this analysis to net out funds captured by the state, and then relate the amount of “effective” DSH payments (spending that was not captured by the state and was available for patient care) to changes in patient mortality over the decade in which the DSH program was introduced. We find that effective spending was significantly related to declines in patient mortality. This effect came primarily through improvements in survival during hospitalization, not through reductions in later mortality or through declines in the incidence or severity of disease, suggesting that improved medical care in the hospital was the causal factor.

Our evidence highlights the importance of heterogeneity in state responses to program incentives. For example, previous work by Duggan (2000) evaluating California’s DSH program found that infant mortality rates were unaffected because subsidies of over $1 billion per year did not translate into increased spending on patients. Our results suggest that most of the DSH money in California was captured by the state, so that there was little net impact on hospital resources or patient care. In contrast, however, we find evidence that other states were less able to divert the targeted funds, and DSH money in these states was associated with improved patient outcomes. Overall, our analysis suggests that federal matching grants can be an effective mechanism for improving medical care and outcomes for the poor, but that the impact is limited by the ability of state and local government to divert the targeted funds.

Section II describes Medicaid and the DSH program in more detail. Section III develops a simple model of the incentives states face under the program. Section IV uses this framework to identify which states expropriated DSH funds for other uses. Section V estimates what fraction of DSH resources were diverted from patient care in states that were expropriating these funds. Section VI uses this information to examine the effect of DSH funds that were actually available for patient care on patient outcomes. Section VII concludes, discussing the implications of these findings for public programs in a federal system. An appendix provides a detailed description of the data sources and variables used.

II. The Disproportionate Share Hospital Program

Medicaid is a joint federal-state program providing public health insurance to the poor. Each state must follow broad guidelines set by the federal government, but is otherwise free to determine eligibility criteria for its Medicaid recipients, the generosity of coverage, and the formula determining payments to hospitals. The federal government then matches state Medicaid expenditures at a rate based on state per capita income, with wealthier states receiving a match of 50 percent and the poorest states receiving a match rate of up to 82 percent.

Traditional Medicaid rules required that each hospital be reimbursed based primarily on the cost of care, and did not allow a state to pay higher rates to hospitals simply because they served a poor population. In response to growing financial pressure on hospitals serving the poor, the

Medicaid Disproportionate Share Hospital (DSH) program was introduced in 1989. The DSH program allowed states to pay additional reimbursement to hospitals that served a large number of Medicaid or uninsured patients (relative to fully-insured patients). Medicaid DSH grew rapidly, reaching payments of roughly $17 billion by 1992 before stabilizing. In 1998 Medicaid DSH payments were $16 billion – representing 9 percent of federal Medicaid vendor payments (Centers for Medicare & Medicaid Services, 2002). Medicare, the federally funded and controlled health insurance program for the elderly, established a smaller DSH program in 1987 that cost $4.5 billion by 1998.

The structure of the Medicaid DSH program provided the opportunity for savvy states to extract greater federal matching funds without necessarily increasing their net contribution to Medicaid. The primary mechanism used by states to do this in the 1990s involved making DSH payments to government-owned hospitals, and then diverting a large fraction of these payments back to the state in the form of an intergovernmental transfer (IGT).[1] For example, suppose a state made a $100 million Medicaid DSH payment to a county hospital, of which $50 million was reimbursed by the federal match. If the county then transferred $50 million back to the state in the form of an IGT, the state would have made no net contribution. What appeared to be a $100 million DSH payment financed with a 50% federal matching grant was in reality a $50 million payment financed entirely by the federal government, once the IGT was taken into account.

Of course, states could also expropriate even more of the DSH payment. For example, the GAO (1994) documented a $277 million DSH payment by the state of Michigan to a county nursing facility (half of which was reimbursed by the federal government), which wired $271 million back to the state *the same day*. More generally, interviews with state officials suggest that most of the DSH payments to state-owned hospitals were expropriated by the state (through direct reductions in state contributions to the hospital's budget) and resulted in no net increase of funds for patient care (Ku and Coughlin, 1995).[2] Overall, however, it appears that these extreme examples were the exception rather than the rule. Coughlin et al. (2000) estimate that states were able to capture 19 percent of DSH payments using IGTs and similar mechanisms.

Some simple descriptive statistics suggest that states may have directed DSH payments disproportionately toward government-owned hospitals to exploit the IGT mechanism, and that the extent of this practice varied considerably across states. Information on DSH payments to individual hospitals has been compiled by the Centers for Medicare & Medicaid Services since 1998, with usable data for 43 states representing roughly two-thirds of all DSH payments (see the Data Appendix for details).[3] Table 1 shows the breakdown of total DSH payments in our data made to different types of hospitals. Whereas state-owned non-acute-care facilities represented only 2.1 percent of Medicaid patient days (Coughlin et al., 2000), these facilities received 19 percent of DSH payments. Similarly, while private hospitals account for almost 70 percent of Medicaid patient days, they received only 35 percent of DSH payments. Forty one counties containing less than two percent of the United States population received more than $200 *per capita* in payments to public hospitals – representing more than $1.7 billion in DSH payments per year or roughly 18 percent of all DSH payments observed in our data. This

[1]Throughout the 1990s, states exploited different loopholes in the federal DSH statutes to increase the effective federal match rate. As the GAO reports (1994, 2000) and Coughlin, Ku, and Kim (2000) show, many states extracted billions in "extra" Medicaid DSH payments through these loopholes. 1991 and 1993 legislation curtailed the use of many of these schemes, and particularly limited the ability of states to divert DSH funds paid to private hospitals (Coughlin et al., 2000).

[2]This type of self-dealing with state-owned facilities was most common in mental health and long-term care facilities where state ownership is common. Fewer than 5% of acute-care hospital beds are in state owned facilities (Coughlin et al, 2000).

[3]As described in the Data Appendix, states were required to report DSH payments made by hospital beginning in 1998. Of the roughly $16 billion in DSH payments made in 1998, about $10 billion were posted by CMS. Several states did not comply with reporting requirements (such as Ohio and Georgia), while others only partially complied. We were able to match 95 percent of the reported dollars to hospital characteristics (such as county location and ownership), resulting in usable data on $9.5 billion in DSH payments.

NIH-PA Author Manuscript

NIH-PA Author Manuscript

skewed distribution is what would be expected if large DSH payments were being directed to public hospitals in order to divert some of those funds back to the state.

There is also a great deal of heterogeneity across states in both the size of DSH payments and in the degree to which the payments were channeled to state and county hospitals. Figure 1 illustrates the mix of DSH payments by hospital ownership in each state. In New Hampshire less than 14 percent of DSH funds went to state hospitals while the remainder (nearly $100 per capita) went to private hospitals. In contrast, 96 percent of DSH payments in Louisiana went to state hospitals, totaling over $150 per capita. In many other states (including such populous states as California, Florida and Texas) a large fraction of DSH payments went to county and district hospitals. This variation is not driven solely by differences in the states' existing hospital structures: for example, only 11 percent of Louisiana's hospitals are state-owned.

III. A Model of State Behavior

To better understand the incentives faced by state governments in designing their DSH payments, we develop a simple model of state behavior. The model has three key features. First, we assume that the state derives some benefit from paying subsidies to hospitals that serve the poor, presumably in the form of improved access and health outcomes among the population served by these hospitals. Second, we assume that the state can expropriate any amount of DSH funds paid to public (but not private) hospitals through the use of IGT. Finally, we assume that the federal rules constrain the state so that it must make similar DSH payments to all public and private hospitals that serve a similar proportion of poor patients. Thus, the state's problem is to determine the level of the DSH payment (if any) along with the amount of IGT to divert from the public hospitals, as a function of the proportion of poor patients served by the hospital.

Let X represent the net payment per patient made to a hospital (DSH net of any IGT), and let ρ represent the proportion of poor patients served in the hospital. Suppose that the benefits of the payments are given by $\rho f(X)$, where $f'>0$ and $f''<0$. In other words, the benefits of these payments are larger in hospitals that serve more poor patients, and are increasing in the amount of the payment but with declining marginal benefit. There are a number of reasons that the benefits of these payments to hospitals that serve more poor patients could be larger. For example, hospitals serving the poor have lower average revenue per patient and correspondingly spend less on patient care, so that the marginal benefit of additional resources devoted to patients may be high. Alternatively, the state may simply value redistribution of medical care toward poor populations for equity (as opposed to efficiency) reasons. Moreover, the state could not have achieved such redistribution prior to DSH because traditional Medicaid rules did not allow a state to pay higher rates to hospitals simply because they served more poor patients.

In private hospitals, which pay no IGT, the net payment is simply the DSH amount (per patient), so that X=DSH. In public hospitals, the payment is net of IGT so that X=DSH−IGT (where IGT is also per patient). If public hospitals account for a proportion π_ρ of all hospitals with a given ρ, then the total benefits of DSH payments to hospitals with a given ρ are given by:

$$Benefits = (1-\pi)\rho\ f(DSH) + \pi\rho\ f(DSH - IGT) \tag{1}$$

where the subscript on π has been suppressed for convenience. Thus, the benefits are a weighted average of the benefits at private and public hospitals.

The net cost of DSH payments depends on two factors. First, the federal government pays a portion of all Medicaid costs (the Federal Medical Assistance Percentage or FMAP), which varies from 0.5 to 0.82 depending on the income of each state. Second, the state receives a proportion of the DSH money back in the form of IGT, where the amount depends on the proportion of hospitals that are public. Thus, the total net cost to the state is given by:

$$Costs = DSH(1 - FMAP) - \pi\, IGT \tag{2}$$

The first term represents the state direct contribution, while the second term represents the funds diverted back to the state through the IGT mechanism.

For each value of ρ, the state chooses the DSH payment going to all hospitals and the IGT payment coming from public hospitals to maximize its benefits net of costs. Thus, whereas Medicaid payments to hospitals prior to DSH could not depend on ρ, now the optimal DSH and IGT payments are functions of ρ. As a benchmark, we begin by considering a state with no public hospitals (π=0) or, equivalently, a state that chooses not to use the IGT mechanism. In this case, the state chooses DSH to satisfy the first order condition given by:

$$\rho\, f'(DSH) = 1 - FMAP \tag{3}$$

The left-hand side of equation 3 represents the marginal benefits of payments to the hospitals, while the right-hand side represents the marginal cost of these payments to the state (i.e., the state share). Because of the federal subsidy, the state increases DSH payments to the point where the marginal benefit of an additional dollar is less than a dollar.

Equation 3 defines an implicit function between DSH and ρ that the state would use to optimally determine DSH. Because the marginal benefit of DSH is increasing in ρ but declining in DSH ($f''<0$), the state will choose to make larger DSH payments to hospitals serving a larger proportion of poor patients. For hospitals with a sufficiently low ρ, the state will be at a corner solution with DSH=0, i.e. the state will choose to make no DSH payments to hospitals serving few poor patients. Actual DSH allocation rules generally follow this pattern, with no DSH payments below some threshold and payments that increase with the proportion of poor patients above the threshold. This is also consistent with the pattern of expenditures seen in Table 1: since ρ is generally higher for public hospitals than private hospitals, public hospitals receive even more DSH dollars than their share of all poor patient-days would suggest.

In the more general case in which the state uses IGTs and has public hospitals, the state chooses DSH and IGT to satisfy two first order conditions that can be simplified to be:[4]

$$\rho\, f'(DSH - IGT) = 1 \tag{4a}$$

$$\rho\, f'(DSH) = 1 - \frac{FMAP}{1 - \pi} \tag{4b}$$

As with equation 3, these two equations determine the state's optimal choice of DSH and IGT as a function of ρ (and π and FMAP). These two first order conditions have a very natural interpretation. Equation (4a) states that the marginal benefit of the net payments made to public hospitals is equal to 1 – since the state controls the net amount going to public hospitals through the unsubsidized IGT, the state will use the IGT to reduce the net payment until the marginal dollar returns one dollar in marginal benefits. Thus, any increase in the DSH payment is undone dollar-for-dollar by IGT, and the federal subsidy has no effect on net resources going to the public hospitals.

Equation (4b) implies that the marginal benefit of the payment made to private hospitals is set equal to the net marginal cost of this payment to the state. When there are no public hospitals

[4]The first-order conditions set equal to zero the derivatives of (Benefits-Costs) with respect to IGT and DSH, where Benefits and Costs are defined in equations (1) and (2). Equation (4a) is the first order condition with respect to IGT. Equation (4b) is derived from the first order condition with respect to DSH, using equation (4a) to substitute 1 for ρf (DSH-IGT).

(π=0), the marginal cost is simply the state share (1-FMAP). When there are public hospitals (π>0), the net marginal cost to the state is lower, since public hospitals fully pay back any increase in DSH payments they receive. This increases the implicit federal subsidy and thereby increases the DSH amount the state is willing to pay to private hospitals. In the extreme, when the proportion of hospitals that are private ($1-\pi$) is smaller than the federal match, the marginal cost of higher DSH payments becomes negative, i.e. at the margin the state finances more than the entire state contribution through the IGT mechanism. In this case, federal caps on DSH payments to hospitals would be binding, as states would otherwise increase DSH payments without bound. Thus, the proportion of DSH payments that fall into private hands (and thereby cannot be recovered through IGT) is the key cost that limits the size of the DSH program.

Figure 2 illustrates the solution to the model graphically for a given value of ρ. As the state raises the DSH payment, the marginal benefit declines. The state sets DSH payments so that the marginal benefit equals the marginal cost in private hospitals ($1 - \text{FMAP}/(1-\pi)$), and then sets IGT so that the marginal benefit equals the marginal cost in public hospitals (1). The use of the IGT mechanism lowers the marginal cost in private hospitals compared to what it would have been in the absence of IGT (1-FMAP), and therefore increases the DSH payments that a state is willing to make.

This simple model captures several realistic features of the current DSH program. States have a great deal of latitude in creating payment formulas, but must generally treat hospitals with similar proportions of poor patients similarly. For example, California makes payments only to hospitals with a proportion of uninsured and Medicaid patients above a certain threshold, and the payments are an increasing function of that proportion above the threshold. There are caps on DSH payments that limit a state’s ability to extract federal dollars. Finally, the most widely publicized examples of financial shenanigans involving IGT payments have occurred in non-acute care hospitals, were public ownership is the norm (π is high) so that states are likely to face a negative marginal cost of increasing DSH payments.

The assumption that the proportion of poor patients at each hospital (ρ) is exogenous is less realistic. Duggan (2000) found that the California DSH program gave many hospitals strong financial incentives to admit more poor patients, and these incentives led to increases in the proportion of poor patients being served at these hospitals and declines at their competitors. In a more complete model of state behavior, states would take such incentives into account in designing their DSH payment mechanism. While it is not obvious how such a model would affect the optimal level of DSH, this complication is not likely to affect the basic insight of the model regarding IGT – namely that IGT will be used to offset DSH going to public hospitals and lower the implicit cost of providing DSH to private hospitals.

From an empirical perspective, our model implies that DSH funds paid to public hospitals will have less of an impact on patient outcomes in states that used intergovernmental transfers to expropriate DSH funds. This apparent ineffectiveness of DSH funds is the result of the state expropriating the funds through IGT, leaving public hospitals with a net payment (DSH net of IGT) much smaller than the original DSH payment. If one could identify the states using IGT and estimate the net payments actually going to public hospitals, then the net funds paid to public hospitals would have an impact on patient outcomes that was at least as large as the impact of DSH funds paid to private hospitals (since the marginal benefit of net payments is higher in public hospitals).

While it is difficult to observe such fiscal shenanigans directly, the model has a number of implications that help to identify empirically those states that were most likely using the IGT mechanism. First, the model suggests that having a large proportion of public hospitals, particularly if those public hospitals are more likely to serve a large proportion of poor patients, will encourage states to use the IGT mechanism. Among states that use the IGT mechanism we would expect higher DSH payments, particularly in public hospitals. Moreover, holding the proportion of poor patients in a hospital (ρ) constant, states using the IGT mechanism will raise DSH payments more if a larger proportion of hospitals are public. Thus, states using the IGT mechanism will tend to pay a larger proportion of their overall DSH payments to public hospitals.

IV. Which States Engage in Fiscal Shenanigans?

For obvious reasons, a direct and reliable measure of the extent to which each state expropriated DSH funds is not available. We consider three alternative state-level proxies that should be associated with the extent to which a state expropriated DSH funds. Our most direct proxy is whether the state self-reported that it used IGT to finance DSH payments in a survey of state Medicaid programs conducted by the Urban Institute (Coughlin et al, 2000). While this serves as a potential marker for states that were using the IGT mechanism, it is only available for the subset of states that responded to the survey, and one might question the accuracy of self-reports on this issue in the face of ongoing investigations into such schemes by the GAO and others. Our second proxy is the size of DSH payments made to county hospitals in each state relative to the number of Medicaid and uninsured patient hospital days, also from Coughlin. A state that expropriated a larger fraction of county DSH funds for its own uses had more incentive to increase the size of its DSH program, so the overall size of the county hospital DSH program (relative to the patients it was intended to serve) should be higher in states using the IGT mechanism. Our final proxy is the fraction of all DSH payments to acute care hospitals that went to county and other local-government hospitals, based on our DSH data from the Centers for Medicare & Medicaid Services. Any state that was expropriating DSH funds from county hospitals had a strong incentive to funnel funds toward these hospitals, so the proportion of these funds going to county hospitals should be higher in states using the IGT mechanism.

Our model suggests that there will be variation in the degree to which different states expropriated DSH payments through IGT. We explore the importance of three key determinants of states' behavior. First, to the extent that there were returns to scale in running the IGT scheme, states with larger populations should be more likely to use IGTs. Such returns to scale would arise if setting up such a scheme required fixed costs in terms of time or hiring staff with sufficient financial savvy, a point that has been made more generally in recent work by Mulligan and Shleifer (2004) exploring the determinants of state regulatory policy. This factor seems particularly important here given that the smallest states received less than half a million dollars annually in DSH payments – and this is an upper bound on the net benefit to the state of implementing an IGT scheme. A second important characteristic that should have facilitated the use of IGT was whether county hospitals accounted for a large fraction of hospital beds in the state. States with a larger share of county hospitals had relatively more county hospitals on which to operate the IGT mechanism (increasing the benefit of using IGT to the state) and relatively fewer private hospitals that might potentially also qualify for DSH payments (reducing the cost to the state). A final characteristic that should have facilitated the use of IGT was whether county hospitals differed from private hospitals in the proportion of patients that were on Medicaid or uninsured. Since state rules for allocating DSH payments had to be at least superficially consistent with the original purpose of benefiting hospitals with a disproportionate share of poor patients, it was much easier for states to target DSH payments to county hospitals if their patient pool was much poorer than that of private hospitals.

We thus estimate:

$$\begin{aligned} \textit{Proxy for IGT Use}_s = {} & \beta_0 + \beta_1 \textit{Poverty of Public Hosp Patients Relative to Priv Hosp Patients}_s \\ & + \beta_2 \textit{Public Share of Hosp Beds}_s + \beta_3 \ln(\textit{Population}_s) + \Gamma X_s + \varepsilon_s \end{aligned} \tag{5}$$

The dependent variables for this regression are our three proxies for state use of IGT: the share of state DSH contributions that were financed by local IGT; the amount of DSH going to county hospitals per Medicaid and uninsured persons in the state; and the share of DSH payments going to county hospitals as opposed to private hospitals. The poverty of public hospital patients relative to private hospital patients is calculated by first finding the median public hospital in each state based on the share of each hospital's patients that are poor, and then calculating the fraction of private hospitals with a smaller share of their patients that are poor, using the Medicare *Impact* file for 1999. We also use this data source to calculate the share of each state's hospital beds that are in public hospitals. The data that we use are summarized in Table 2. A more detailed description of data sources and construction is included in the Data Appendix.

We report the results of this estimation in Table 3. As predicted, a larger population, a larger share of hospital beds in county hospitals, and a larger difference between county and private hospitals in the proportion of poor patients served are positively related to all three proxies for a state's IGT use. We include additional controls in the even columns to see whether these results are driven by unmeasured confounders, including each state's per capita income and unemployment rate, and whether or not the state ran a deficit in the period before the DSH usage is measured (in 1992 to 1994, when many states were under fiscal distress). These covariates are usually not significant, and their inclusion does not change the estimates of the coefficients of interest. Although the number of observations limits the number of covariates we can include simultaneously, results are robust to the inclusion of other covariates such as percent of the population that is white, percent living in poverty, or percent with a high school diploma.

As another plausibility check on our three proxies, Table 4 compares the characteristics of states that report the use of inter-governmental transfers to fund DSH to those states that do not. The justification for these proxies was that states using IGT would have incentives to have larger DSH programs (relative to the population it was intended to serve) and to funnel funds towards county hospitals in particular. As expected, states reporting the use of IGT to fund DSH spent more than twice as much per capita on DSH, spent more than five times as much per Medicaid or uninsured patient, and spent a larger fraction of their DSH funds on county hospitals.

Overall, the empirical evidence suggests that states use IGT in a way that is predictable, and that is captured by our proxies. All three proxies are positively related to state characteristics that should facilitate the use of IGT (and are correlated with each other), as our model would predict. In the remaining analyses, we use these proxies to identify states that are most likely to be diverting DSH funds.

V. How Much of DSH Spending is Diverted?

To evaluate the impact that net resources (DSH less IGT) had on patient outcomes, we must estimate the proportion of DSH payments to county hospitals that was diverted through the IGT mechanism. We use the relationship between DSH payments and the amount of net IGT observed in county financial data to estimate this proportion. Because county hospitals are part of the parent county government, their DSH payments appear as an intergovernmental revenue (from the state to the county) in county financial data. In the absence of any diversion by the state, every dollar of DSH funds will result in a dollar increase in net IGT. If the state diverts DSH funds through an intergovernmental transfer from the county to the state (either through reductions in revenues that would have otherwise come from the state or increases in the funds the county sends back to the state), then every dollar of DSH funds will result in less than a dollar increase in net IGT. Therefore, the impact of DSH funds on net IGT provides an estimate of the proportion of DSH payments to county hospitals that remained available for patient care.

We would expect the proportion to be near one in states that were least likely to divert DSH funds, and below one in states that were most likely to divert DSH funds.

More precisely, we estimate the proportion of DSH payments to county hospitals that remained available for patient care based on the regression:

$$\Delta\, netIGT_{i,(97\,to99)-(87\,to89)} = a_s + \delta_1 DSH_{i,(98\,to00)} + \delta_2(lowexpropriation)_s DSH_{i,(98\,to00)} + \Gamma\Delta X_{i,(2000)-(1990)} + \varepsilon_i \quad (6)$$

The unit of observation is the county. The dependent variable is the change in net intergovernmental transfers (intergovernmental revenues, including DSH dollars and other revenues, minus intergovernmental expenditures), measured in real per capita dollars. The change is measured as the average for 1997 to 1999 minus the average for 1987 to 1989. We difference the data at the county level to remove any fixed county-level differences in net IGT, and we use a long difference beginning just prior to the introduction of the DSH program to focus on the long-run impacts of DSH payments. The data on net IGT, described in the Data Appendix, come from the *Annual Survey* and *Census of Government Finances*.

The key right hand side variable in this regression is the amount of DSH per capita going to county hospitals in the late 1990s (measured as the average of available data for 1998 to 2000). Note that this variable is in effect the difference in DSH payments between the late 1980s and the late 1990s, since there were no sizable DSH payments until the early 1990s. We interact the DSH variable with a dummy variable (*low expropriation*) that is equal to 1 if the state has lower than average IGT use, based on one of our three proxies.[5] In specifications without the interaction term, the coefficient on DSH (δ_1) represents the net change in real resources available to the county and its hospitals for each dollar of DSH revenue it receives. In specifications that include the interaction term, δ_1 is the net change in resources for counties in states that do the most expropriation of DSH funds, while $\delta_1+\delta_2$ is the net change in resources for counties in states that do the least expropriation of DSH funds. We expect δ_1 to be positive but less than 1, δ_2 to be positive, and the sum to be 1 or less. In alternate specifications, we interact DSH with a continuous measure defined as $[1 - X/\max(X)]$ where X is one of our three proxies for IGT use. The results using the continuous measure can be interpreted similarly to the results using the dummy variable (*low expropriation*): both measures are equal to 0 in the states that are most likely to expropriate DSH funds and equal to one in the states that are least likely to expropriate DSH funds.

Finally, we control for several other factors that may affect county resources and DSH spending. We include the change in the fraction of patient-days used by Medicaid recipients (to capture the effect of any Medicaid eligibility expansions) and the change in Medicare DSH payments.[6] The regressions also control for state fixed effects (to capture any state-specific trends in fiscal conditions, etc.) and changes in percent white, unemployment, percent living in poverty, real median house value, percent holding a high school diploma, and real per capita income at the county level. These data come from the *Area Resource File*, and are measured as differences between 1990 and 2000.

Table 5 presents estimates of equation (6). Column (1) shows that each dollar of DSH payment going to county hospitals increased average net county resources by 57 cents. This estimate suggests that the average state expropriated the remaining 43 cents through IGT. Since the state

[5]Recall from section IV that the three proxies are: (1) share of state funds from local IGT, (2) county DSH spending per Medicaid or uninsured patient, and (3) share of DSH spending to county hospitals.

[6]The biggest changes in Medicaid eligibility and Medicare DSH took place before the period we study (see Currie and Gruber, 1996; Nicholson and Song, 2001). The exclusion of these variables does not change the estimated coefficients reported below, nor are the coefficients on these variables themselves significant.

share of Medicaid is at most 50%, this estimate is consistent with the view that states largely recouped their original contribution to the DSH payments (i.e., largely avoided providing any net matching funds for DSH payments to county hospitals). Column (2) adds DSH payments made to state and private hospitals located in the county. The coefficient on DSH payments to county hospitals changes little from column (1). As expected, we find no significant relationship between DSH payments to state or private hospitals and net IGT: these hospitals are independent of county governments and DSH payments to them are unrelated to county budgets.

The funds remaining with the county represent a net increase in resources available to the county overall, but we have limited information about whether these funds were spent on hospitals themselves or other county functions. Because county hospitals are financed as part of the parent county government, we do not observe transfers between the county government and its hospitals. The 57 cent increase in county resources associated with each dollar of DSH payments is likely to be an upper bound on the amount of resources that eventually went to county hospitals. While data on county spending on narrower budget categories is limited in the *Survey* and *Census of Government Finances*, we estimate the effect of DSH spending on county spending on hospitals for the 557 counties in which hospital spending is reported.[7] Each dollar of DSH revenue increased hospital spending in these counties by 60 cents (s.e. $0.29). By contrast, DSH spending was not associated with an increase in spending in other major budget categories, such as education (13 cents, s.e. $0.12) or highways (less than 1 cent, s.e. $0.08). This suggests that a substantial portion of the funds remaining in counties may have actually been devoted to hospital resources, and that intergovernmental transfers were the main mechanism for diverting funds to other uses.[8]

The remaining columns of Table 5 add an interaction term between DSH and various measures of whether a state is less likely to expropriate the DSH payments going to county hospitals. We present results for the three alternative methods of identifying states that are likely to expropriate more DSH, using both discrete and continuous measures. Again, each interaction term is defined such that the baseline coefficient on DSH (δ_1) is the proportion of DSH funds kept by counties in states that do the most expropriation of DSH funds, with the coefficient on the interaction term (δ_2) showing the incremental amount that counties in states that do less expropriation will keep. The results for these alternative methods are qualitatively similar.[9] In states that are most likely to expropriate DSH payments, we estimate that the proportion of DSH payments to county hospitals that remain in the county is around 0.5 and is significantly below 1, implying that these states are expropriating roughly half of DSH payments to county hospitals. Counties in states that are less likely to expropriate DSH funds themselves do indeed see a greater increase in net IGT: all of the interaction terms are positive and significant.[10]

[7]Unfortunately, the structure of the data does not allow us to separate counties not reporting their hospital spending from counties that have zero hospital spending. If the actual net increase in hospital resources is smaller, our later estimates of the effect of DSH dollars on patient outcomes will be biased towards zero.

[8]Another mechanism for diverting funds independent of intergovernmental transfers would be the reduction of direct state expenditures on a program coupled with increased county responsibility for that program – such as is if the state stopped giving housing vouchers to poor residents but required the county to do so, or increased the county cost sharing. These changes would not show up as intergovernmental expenditures or revenues, but changes in state and county direct expenditures. We do not see evidence of these alternative mechanisms at work here.

[9]We also estimate equation (6) including the “low state expropriation” measures interacted with DSH spending on each type of hospital (state, county, and private) separately. Only the interaction with county DSH dollars is significant, and it is not substantially changed by the inclusion of the additional interactions. For example, using the specification from column (5) of Table 5, the estimated coefficient on county DSH spending itself is .52 (s.e. .18), the coefficient on the interaction of low expropriation with county DSH is 1.87 (.51), the interaction with state hospital DSH is −.02 (.23), and with private hospital DSH is −.08 (.30).

[10]The sum of δ_1 and δ_2 is larger than 1 in several cases, although generally not significantly so. This is consistent with the fact that DSH spending seems to be underreported to CMS (and we were unable to match some of the DSH spending that was reported). If our measure of DSH spending is too low, the observed coefficients might be too high. We discuss robustness to different methods of incorporating δ_1 and δ_2 in footnote 14.

The results in Table 5 suggest that our proxies for state expropriation are capturing real differences in state behavior. Counties in states where our proxies indicate there was little opportunity to redirect DSH payments got to keep the full amount of the DSH payments they received, while counties in states where our proxies indicate greater possibilities for redirection saw their net intergovernmental revenues rise by only 50 cents for each dollar of DSH payment received. In other words, it appears that roughly half of DSH payments to county hospitals were diverted through in the states that were most likely to be using the IGT mechanism.

VI. How Does DSH Spending Affect Patient Outcomes?

We now turn to the question of whether DSH payments had an effect on patient outcomes. We begin by estimating the relationship between DSH payments and changes in morality among infants and heart attack patients. After establishing these relationships, we investigate the mechanisms through which those effects might occur.

To estimate the relationship between DSH payments and patient mortality, we decompose total DSH payments into effective DSH (payments to acute care hospitals net of IGT) and ineffective DSH (all other payments). Effective DSH payments should have a beneficial impact on patient outcomes, while ineffective DSH payments should have no effect on resources or patient outcomes in acute care hospitals. We measure effective DSH payments as all payments to private acute care hospitals plus all payments to county acute care hospitals in states that do low expropriation (defined as states devoting a lower-than average share of DSH dollars to county rather than private hospitals) or all payments to private acute care hospitals plus 53 percent of payments to county acute care hospitals in states that do high expropriation. Ineffective DSH spending is all payments to state-owned hospitals, all payments to non-acute care hospitals, and 47 percent of payments to county acute care hospitals in states that do high expropriation. This classification of which DSH dollars were effective in county hospitals uses the results of column (5) from Table 5 as our baseline specification, although subsequent results are robust to using any specification from Table 5.

A. The Impact of DSH Payments on Patient Mortality

We analyze two key measures of patient outcomes: infant mortality and post-heart attack mortality. We choose these measures because mortality rates in both of these patient populations are believed to be sensitive to the quality of hospital care, and increased DSH payments are likely to improve the quality of hospital care for all patients, not just those covered by Medicaid. Many measures of overall hospital quality developed by the Medicare Quality Improvement Organization focus on the treatment of heart attack patients (Jencks, 2003), and Shen (2003) finds that heart attack mortality increases when hospital resources are reduced. Currie and Gruber (1996) find that Medicaid expansions decreased infant mortality.

We estimate regressions of the form:

$$\Delta MR_{i,(98\,to00)-(88\,to90)} = \alpha_s + \beta_1 DSH_{i,effective,(98\,to00)} + \beta_2 DSH_{i,ineffective,(98\,to00)} + \Gamma \Delta X_{i,(2000)-(1990)} + \varepsilon_i \qquad (7)$$

The dependent variable is either (a) the change in the percent of infants that died within 28 days of birth from 1988–1990 (averaged) to 1998–2000 (averaged), estimated using natality data from the *Area Resource File*, or (b) the change in the risk-adjusted percent of patients over age 65 who died within 90 days of having a heart attack from 1989–1991 (averaged) to 1998–2000 (averaged), estimated using Medicare Claims data. Additional detail on how these variables are constructed is provided in the Data Appendix. Our analysis is done at the county (rather than hospital) level to avoid issues of patient selection across hospitals and because both the county financial data and the infant mortality data are only available at the county level. The regressions also control for state fixed effects and changes in the same county-level covariates as above.

The key independent variables are effective and ineffective DSH per capita, where the coefficient on ineffective DSH is expected to be zero and the coefficient on effective DSH is expected to be negative (associated with declines in mortality). Decomposing DSH payments in this way allows us both to gauge more accurately the impact of DSH spending that reaches its intended targets (low-income hospitals, rather than state general funds) and allows us to verify whether our characterization of the effectiveness of that targeting is borne out in the data.

Table 6 presents estimates of equation (7). Column (1) of Table 6 shows that for each additional $100 per capita of DSH payments made to hospitals within a county, there was a statistically significant reduction in 28-day infant mortality of .062 percentage points, or .62 infant deaths per thousand births. Column (2) decomposes DSH dollars into effective and ineffective payments, showing that all of the impact is associated with effective DSH dollars, which are estimated to reduce infant mortality by .101 percentage points. In contrast, the estimate for ineffective DSH dollars is a third the size and statistically indistinguishable from zero. Column (3) further decomposes effective and ineffective DSH payments into those made to county acute care hospitals versus other hospitals. We see that effective DSH payments to both private and county hospitals have effects on infant mortality that are similar in magnitude and individually significant, while ineffective DSH payments to both county and other types of hospitals have smaller estimated effects that are statistically insignificant. These estimates indicate that our decomposition of county hospital DSH into effective and ineffective payments accurately identifies states in which such payments had less impact on patient outcomes.[11]

Similarly, column (4) shows that an additional $100 per capita in DSH payments reduced 90-day post-heart attack mortality by 1.17 percentage points, or 11.7 deaths per thousand heart attacks. Column (5) shows that effective DSH dollars were associated with a larger decline of 2.78 percentage points, while ineffective DSH dollars had virtually no effect. Column (6) again shows that effective payments to public and private hospitals had similar effects, resulting in significant improvements in post-heart attack mortality. Overall, the results from Table 6 imply that DSH payments for acute care hospitals that were not expropriated by the state resulted in significant reductions in patient mortality.

How big are these reductions in mortality? A simple calculation suggests that the reductions in mortality are modest given the amount of money spent on the DSH program. The .062 percentage point reduction in infant mortality associated with each $100 per capita increase in DSH spending implies that each $12 million in DSH spending resulted in one baby saved, while the 1.17 percentage point reduction in post-heart attack mortality associated with a $100 per capita increase in DSH spending implies that $9 million in DSH spending resulted in one life saved (a larger reduction in mortality for a smaller population of patients).[12] A similar calculation based only on effective DSH payments would cut these costs roughly in half. These crude calculations understate the total benefits to the extent that they do not count benefits that accrue to other patient groups or from other uses to which DSH funds are diverted. Nevertheless, these estimates are in line with similar calculations done for other increases in

[11]The estimated impact of effective DSH dollars going to public hospitals is slightly larger than those going to private hospitals, as predicted by our model, but the difference is not significant for either infants or heart attacks.

[12]Each $100 in DSH spending per capita reduces infant mortality by .62 babies per 1000 births. In our data, there are 3.1 million births per year (for 1998–2000, averaged), and 225 million people. Reducing infant deaths by 1 would require $100 per capita * 224 million/(3.1 million * .00062) = $11.7 million. Each $100 in DSH spending per capita reduces post-heart attack mortality by 1.17 deaths per 100 heart attacks. There are 218,000 heart attacks per year in our data (for 1998–2000, averaged). Reducing heart attack deaths by 1 would require $100 per capita * 224 million/(218,000 * .0117) = $8.8 million.

Medicaid spending. For example, Currie and Gruber (1996) study the effects of Medicaid eligibility expansions on infant mortality, and find that while the most cost-effective targeted expansions cost almost $1 million per infant saved, broader expansions cost more than $4 million per infant saved. Moreover, they note that these figures are significantly less than the cost of other policies that are routinely implemented. Our cost-per-life for heart attacks victims is quite consistent with Shen (2003), who analyzes the effect of decreases in hospital resources driven by changes in Medicare payments on heart attack survival.[13] Thus, our estimated effects are well within the range of the existing literature.

Improving access to high quality care is particularly important for poor and disadvantaged populations, given the well-documented racial disparities in health care (Smedley et al., 2003) and the fact that poor and minority populations often receive care at hospitals with below-average quality of care (Skinner et al., 2003; Weech-Maldonado et al., 2003; Morales et al., 2004). While we would like to examine the differential effect of DSH dollars on patients of different races, cell sizes for black infants and heart attack patients are too small (and standard errors thus too big) to draw significant distinctions. For example, each $100 per capita in effective DSH reduces black infant mortality by .16 percentage points with a standard error of .13, and reduces white infant mortality by .06 with a standard error of .02.

B. Robustness

We test the robustness of our results to a number of alternate specifications, and investigate whether they may be driven by spurious correlation between DSH payments and county-level trends in mortality. First, we repeat our primary specification, regressing changes in infant and post-heart attack mortality on effective and ineffective DSH (as in column 2 of Table 6), but using alternative methods of identifying states in which DSH payments to county hospitals are likely to be ineffective because of state expropriation. We identify high-diversion states using the other two definitions from Table 5 (those reporting a high share of non-federal funding for DSH coming from local government IGTs and those with high DSH per Medicaid or uninsured patient as having ineffective DSH going to public hospitals). Identifying ineffective DSH using either of these alternate methods does not substantially alter the results.[14] Thus, our results do not appear to be particularly sensitive to the metric used to identify states that expropriate DSH payments.

Second, a key empirical concern is that counties receiving DSH payments are likely to differ systematically from other counties (for example, having a poorer population and perhaps higher infant or heart attack mortality) and that these pre-existing differences may generate different trends in mortality independent of DSH payments per se. Our finding that only effective DSH payments are related to mortality alleviates some of this concern. Table 7 estimates a variety of additional specifications to further allay concerns that our results are driven by such spurious correlations. The first columns of each panel of Table 7 replicate our base results from Table 6 for comparison. In columns (2) and (7), we include a dummy variable for whether a county

[13]Shen (2003) finds that each $1000 per patient in reduced Medicare funds caused by the implementation of Medicare's Prospective Payment System (PPS) increased 90-day post-heart attack mortality by about one percentage point (with about 36 million patients admitted to hospitals each year). While her implied cost per life is somewhat larger than ours, the increase in funds that we study is targeted to a population that is likely to have a higher marginal benefit. Staiger and Gaumer (1992) also make a similar calculation, obtaining a similar cost per life from the implementation of PPS. Cutler (1995) finds that the price changes created by PPS affected both the treatment and outcomes of Medicare beneficiaries.

[14]For 28-day infant mortality, coefficients change from the original results of −.106 for effective DSH and −.033 for ineffective DSH to −.108 and −.049 for the first alternative and −.106 and −.032 for the second. For post-heart attack mortality, coefficients change from −2.13 for effective DSH and −.09 for ineffective DSH to −1.91 and −.19 for the first, and to −1.98 and −.16 for the second. We also calculate effective and ineffective DSH using the continuous measures of low state expropriation (so that each state has a different share of county hospital spending classified as effective or ineffective, rather than using the dummies to group states as "high" or "low" expropriators), with very similar results. For example, for 28-day infant mortality using the continuous measure of effective DSH based on the share of DSH going to county hospitals, the coefficient on effective DSH is −.099 and ineffective DSH is −.032.

received any DSH payments at all to control for broad differences in trends in such counties. Adding this dummy variable to the specification yields an insignificant coefficient and has no effect on our basic estimates. Thus, there is a clear dose-response relationship – changes in mortality rates are proportional to the amount of DSH payment a county receives. We also estimate mortality effects using a spline in DSH payments to test for non-linear effects, but are unable to reject that the coefficients on the segments are equal.

We next control for initial mortality, to test whether poorer hospitals with higher mortality may have experienced both larger declines over this period and higher DSH payments, without any causal connection between the two.[15] The coefficients on the lags in columns (3) and (8) are both negative and significant (suggesting that there was mean reversion in both infant and post-heart attack mortality), but estimates of the effect of DSH payments on infant mortality are not appreciably different. The effect of DSH payments on heart attack mortality is somewhat lower after controlling for mean reversion, but still statistically significant. We also test the sensitivity of the results to an alternate functional form, using the change in logs of mortality as the dependent variables in columns (4) and (8). These estimations produce very consistent results, with virtually identical implied effects at the means.[16] Finally, we might be concerned that we are capturing unusual trends in infant mortality because of the rapid but uneven improvements seen in the 1980s and early 1990s with the introduction of surfactants. In column (5) we re-estimate the same specification, but substituting changes in one-year infant mortality between 1976–80 and 1988–90. Subsequent DSH dollars have no effect on mortality changes from this pre-period, with a small positive and insignificant estimated coefficient.[17] The results seem robust to these alternative specifications.

C. Mechanisms

There are two fundamentally different mechanisms by which DSH funds might have reduced mortality rates among hospital patients. First, patients may have received better hospital care as the result of the additional DSH funds, either because of improved care at all hospitals or because of patients receiving their care at better hospitals (as argued in Duggan, 2000). Previous research suggests that both infant and post-heart attack mortality are sensitive to hospital resources (see Currie and Gruber, 1997, and Shen, 2003). Alternatively, the additional DSH funds may have been used for public health and outreach programs that affected hospital mortality rates through changes in patient risk factors (such as prenatal care or other health behaviors) or selection in the underlying population being admitted to the hospital (as would occur if the incidence of heart attacks declined).

We take two approaches to disentangle these stories. First, we look at the incidence of the risk factors for the mortality outcomes we examine: the fraction of infants born with low birth weight (and then mortality conditional on that fraction), and the incidence of heart attacks (and then mortality conditional on that incidence). The fraction of low birth weight babies should capture an important component of patient risk within hospitals. Furthermore, advancements in medical care are almost entirely manifested in reductions in mortality conditional on birth weight, not in reductions in the incidence of low birth weight. Reductions in the incidence of

[15] In these specification, we include mortality in the earlier period (1988–1990 for infant mortality, 1989–1991 for heart attack mortality) on the right-hand side, and instrument for this variable (with infant mortality in 1985–1987 or heart attack mortality for 1992–1994) to correct the estimates for measurement error. Unfortunately 28-day infant mortality is not available for this period in the ARF, so we use 1-year mortality. We use later heart attack mortality because the first year of data we have for this variable is 1989. For this reason we are also not able to replicate column (4) for heart attack mortality.

[16] There are a number of small counties with 0 observed mortality that drop out of the log specification. These counties receive very little weight in the original regressions, so their omission here is not a great concern, but we also estimate a panel Poisson regression with very similar results. For example, for 28-day infant mortality the estimated coefficient on effective DSH is −0.206 (s.e. 0.061) and on ineffective DSH is −0.063 (s.e. 0.053).

[17] Earlier data on heart attack mortality is not available to do a similar robustness check.

low birth weight can largely be attributed to improvements in maternal health and prenatal care. Similarly, the incidence of heart attacks within a county should capture any important selection effects that would alter the underlying health status of patients admitted with a heart attack.

Columns (1) and (2) of Table 8 explore the effect of DSH payments on the change in the incidence of low birth weight between the late 1980s and the late 1990s. Total DSH payments in general and effective DSH spending in particular seem to have a small but significant effect on low birth weight. The estimate implies that a $100 increase in effective DSH per capita is associated with a 0.26 percentage point decline in the fraction of babies born with low birth weight, relative to a base of 7.5 percent. Thus, there is some evidence that effective DSH may have reduced low birth weight, presumably through improved prenatal care. Columns (3) and (4) re-estimate the effect of DSH payments on 28-day infant mortality holding the fraction low birth weight constant. The results are quite similar to those found in Table 6: the effect of DSH payments comes through the hospital treatment of babies even holding constant the primary risk factor. In other words, the effect of DSH payments on low birth weight is much too small to account for the relationship between DSH payments and infant mortality. Similarly, columns (7) and (8) show that there is virtually no effect of DSH payments on the incidence of heart attacks, and columns (9) and (10) show that holding the incidence of heart attacks constant does not change the effect of DSH payments on post-heart attack mortality.

Second, we examine longer-run mortality to see whether mortality reductions seem to come from in-hospital care or from broader factors. Most of the effect of care within the hospital should be seen in the period immediately following hospital admission, rather than months later. Columns (5) and (6) of Table 8 replicate the main specifications from Table 6 but with infant mortality between 28 days and one year as the dependent variable, while columns (11) and (12) use post-heart attack mortality between 90 days and one year. We see no significant effects of DSH payments here – the effect of these payments on mortality seems to come through care in the hospital, not post-discharge. Overall, these results suggest that the effect of DSH payments on infant and heart attack mortality operates primarily through improved hospital care – not through prenatal care or selection or through other factors that affect mortality post-discharge.

VII. Conclusion

Federal subsidies can be an effective mechanism for improving medical care and outcomes for the poor, but their impact is limited by the ability of state and local government to divert the targeted funds. While funds that are diverted to other uses may result in other benefits to society (such as tax abatement or subsidies of other government programs), this dilutes the intended impact of the subsidies and thereby reduces their cost-effectiveness in terms of the program's stated goals. Clearly, ongoing legislative attacks reflect the belief by many that the amount of diversion involved in the DSH program is excessive. Nevertheless, our analysis suggests that while the cup may be half empty, it is still half full. Despite a significant amount of diversion by the states, the Medicaid DSH program appears to have contributed to significant declines in patient mortality in many areas during the 1990s.

These findings shed light on a broad range of programs that use matching grants to encourage state and local spending. School finance reform is a particularly salient case: the impact of school finance equalizations on the resources available to low-income school districts may be overstated because of similar fiscal shenanigans. Cullen and Loeb (2001) find that school districts in Michigan relabeled existing expenditures to qualify for equalization funds. Similarly, Downes (1992) finds that school districts in California used off-budget mechanisms such as activity fees and bake sales to avoid limitations on school spending. Baicker and Gordon

(2004) find that states offset mandated increases in state education aid to low income districts by reducing spending on other programs for the poor in those districts. Gordon (2004) finds that federal increases in Title I spending to low income school districts are offset by reductions in local spending. In each of these cases, apparent increases in program expenditures were in fact offset by changes in local spending, so that the federal grants did not lead to increased resources for the targeted population. This diversion of funds also makes it appear that increased resources do not improve student outcomes – when, in fact, net resources did not increase by much. In this light, it is not surprising that estimates of the effect of school finance on student performance are mixed (Card and Payne, 2002; Clark, 2003; Downes, 1992). These distortions imply that school finance equalizations relying on matching formulas are likely to be ineffective at redistributing resources.

More generally, these distortions change the tradeoff between matching grants and block grants. The standard theory of fiscal federalism views block grants as a means to redistribute tax revenue, whereas matching grants serve as a means of increasing local spending on public programs that have positive externalities, such as Medicaid and welfare (Oates, 1999). Legislation in 1996 that converted the federal welfare program funded by matching grants (AFDC) to a system of block grants (TANF) and similar proposals being discussed for Medicaid (Pear, 2004) are at odds with this principle. Our results may help to explain these moves toward block grant funding: matching grant programs are both more expensive and less effective when lower levels of government are able to misrepresent their contributions.

Acknowledgements

This research was funded by NIA grant P01 AG19783. The authors thank Amitabh Chandra, Mark Duggan, Alan Durell, Lawrence Katz, Jonathan Skinner, two anonymous referees, and participants at several seminars for helpful suggestions.

References

Baicker, Katherine; Gordon, Nora. The Effect of Mandated State Education Spending on Total Local Resources. NBER Working Paper 10701; 2004;

Card, David; Abigail Payne, A. School Finance Reform, the Distribution of School Spending, and the Distribution of Student Test Scores. Journal of Public Economics 2002;83

[accessed January 15, 2004]. Centers for Medicare and Medicaid Services, Medicaid Disproportionate Share Hospital Reports, http://www.cms.gov/dsh/default.asp

Centers for Medicare and Medicaid Services. CMS Statistics. Department of Health and Human Services; 2002.

Clark, Melissa A. Education Reform, Redistribution, and Student Achievement: Evidence from the Kentucky Education Reform Act. Oct. 2003 manuscript

Coughlin, Teresa; Liska, David. Changing State and Federal Payment Policies for Medicaid Disproportionate Share Hospitals. Health Affairs 1998;17(3)

Coughlin, Teresa; Ku, Leighton; Kim, Johnny. Reforming the Medicaid Disproportionate Share Hospital Program. Health Care Financing Review Winter;2000 22(2)

Cullen, Julie Berry; Loeb, Susanna. Fiscal Substitution in the Context of School Finance Reform. 2000. manuscript

Currie, Janet; Gruber, Jonathan. Saving Babies: The Efficacy and Cost of Recent Changes in the Medicaid Eligibility of Pregnant Women. Journal of Political Economy 1996;104(6)

Currie, Janet; Gruber, Jonathan. The Technology of Birth: Health Insurance, Medical Interventions, and Infant Health. NBER Working Paper 1997;5985

Cutler, David. The Incidence of Adverse Medical Outcomes Under Prospective Payment. Econometrica January;1995 63(1):29–50.

Downes, Thomas. Evaluating the Impact of School Finance Reform on the Provision of Public Education: The California Case. National Tax Journal 1992;45(4):405–419.

Duggan, Mark. Hospital Ownership and Public Medical Spending. Quarterly Journal of Economics. Nov; 2000

General Accounting Office. Letter Report, GAO/HEHS-94-133. Aug 1. 1994 Medicaid: States Use Illusory Approaches to Shift Program Costs to Federal Government.

General Accounting Office. Allen before the Senate Committee on Finance, GAO/T-HEHS-00-193. Sept 6. 2000 Medicaid: State Financing Schemes Again Drive Up Federal Payments, Statement of Kathryn G.

Gordon, Nora. Do Federal Grants Boost School Spending? Evidence from Title I. Journal of Public Economics 2004;88(910):1771–1792.

Gruber, Jonathan. Medicaid. In: Moffitt, R., editor. Means-Tested Transfer Programs in the United States. University of Chicago Press; Chicago: 2003.

Hoxby, Caroline M. All School Finance Equalizations Are Not Created Equal. Quarterly Journal of Economics November;2001 16(4)

Jencks SF, Huff ED, Cuerdon T. Change in the Quality of Care Delivered to Medicare Beneficiaries, 1998–1999 to 2000–2001. Journal of the American Medical Association 15 January;2003 :305–312. [PubMed: 12525231]

Kominski, Gerald; Long, Stephen. Medicare's Disproportionate Share Adjustment and the Cost of Low-income Patients. Journal of Health Economics 1997;16

Ku, Leighton; Coughlin, Teresa. Medicaid Disproportionate Share and Other Financing Programs. Health Care Financing Review 1995;16(3)

Mulligan, Casey; Shleifer, Andrei. NBER Working Paper 10234. Jan. 2004 Population and Regulation.

National Center for Health Workforce Analysis. Department of Health and Human Services. 2003. Area Resource File.

Newhouse, Joseph. NBER Working Paper 8531. Oct. 2001 Medicare Policy in the 1990s.

Nicholson, Sean; Song, David. The Incentive Effects of the Medicare Indirect Medical Education Policy. Journal of Health Economics 2001;20

Oates, Wallace E. An Essay on Fiscal Federalism. Journal of Economic Literature September;1999 37:1120–1149.

Pear, Robert. The New York Times. February 5. 2003 The President's Budget Proposal: The Poor; Aid To Poor Faces Tighter Scrutiny. Late Edition - Final, Section A, Column 1

Skinner, Jonathan; Weinstein, James N.; Sporer, Scott M.; Wennberg, John E. Racial, Ethnic, and Geographic Disparities in Rates of Knee Arthroplasty among Medicare Patients. New England Journal of Medicine October 2;2003 349(14):1350–1359. [PubMed: 14523144]

Shen, Yu-Chu. The Effect of Financial Pressure on the Quality of Care in Hospitals. Journal of Health Economics March;2003 22(2):243–69. [PubMed: 12606145]

Staiger, DO.; Gaumer, G. Quality of Care in Hospitals: Post-Admission Mortality Under Medicare's Prospective Payment System, Working Paper. Jan. 1992

Smedley, Brian, et al., editors. National Academy Press. 2003. Unequal Treatment: Confronting Racial and Ethnic Disparities in Healthcare.

Weech-Maldonado RJ, Morales LS, Elliot MN, Spritzer K, Marshall G, Hays RD. Race/Ethnicity, Language, and Patients' Assessments of Care in Medicaid Managed Care. Health Services Research 2003;38(3):789–808. [PubMed: 12822913]

Data Appendix

Data for this analysis come from several different sources, and are summarized in Table 2. Analysis is performed at the county level, using data from the late 1980s and the late 1990s.

A. Medicaid DSH Payments

Beginning in 1998, CMS requested that each state make available an annual report of the hospitals receiving DSH payments and the amount they received. Most states have complied with this request in at least one year since 1998. We matched the hospitals listed in these reports with their American Hospital Association identification number whenever possible, but some listings were ambiguous, and some states (such as Alabama and Michigan) reported aggregated figures that could not be used. Overall, we were able to match 90 percent of DSH dollars reported. Hospital allocations were then aggregated to the county level. When multiple years of data were available, the county values were averaged across years. After discarding Alaska, Hawaii, and the District of Columbia, we were left with data on DSH payments to 2579 (of the 3042) US counties. By matching hospital payments to AHA provider information from the Hospital File, we were able to calculate DSH payments at the county level by hospital ownership (public (state, county, district) versus private) and by hospital service (general, children's, psychiatric, etc.). We supplement this data with details on state-level DSH allocations and financing gathered by Coughlin et al. (2000), including information on the degree to which states used intergovernmental transfers from localities to finance their spending on DSH, and the total spending on the DSH program relative to the number of Medicaid and uninsured patients in the state in 1997. These data come in large part from a survey conducted by the Urban Institute.

B. County Finances

Data on county budgets come from the annual *Survey of Government Finances* and the *Census of Government Finances* conducted by the Bureau of the Census. We use real (1999=100) per capita three-year average spending and revenue figures for 1987–1989 and 1997–1999. All counties are included in the survey years of 1987 and 1997, but only 1/2 to 2/3 are included in (non-Census) Survey years. Not all counties report all categories of revenues or expenditures in any given year.

To examine the effect on county finances of increases in DSH payments to county hospitals, we subtract intergovernmental expenditures from intergovernmental revenues to generate net intergovernmental revenues. DSH payments to county hospitals will appear as intergovernmental revenues in the county budget, and any funds that the county hospitals return to the state will appear as intergovernmental expenditures.

Data from the *Survey* and *Census of Government Finances* is notoriously noisy, especially for smaller spending and revenue categories. We implement a conservative trimming strategy, dropping observations where spending or revenues jump by more than a factor of 5 in any year. This results in dropping 27 observations for net intergovernmental revenues. Results are not sensitive to their exclusion.

Missing observations are more of a problem for smaller budget categories. For example, only 557 counties report any hospital spending. Unfortunately, the way in which the data are reported makes it impossible to distinguish between 0s and missing observations.

C. Infant Mortality and Low Birth Weight

Data on infant births, birth weight, and deaths are reported at the county level in the 2003 *Area Resource File* compiled by the National Center for Health Workforce Analysis. We use several different measures of infant mortality. We construct 28-day and 1-year mortality in 1988–1990 and 1998–2000 from reported 3-year averages of births and deaths by race. For earlier periods we use the reported 5-year average infant mortality rate. These data are summarized in Table 1. There was a significant decline in infant mortality during this period, with 28-day mortality dropping from 5.8 per thousand in 1988–1990 to 4.6 in 1998–2000. We similarly construct the average incidence of low birth weight from reported 3-year averages of the number of low birth weight babies.

D. Heart Attacks and Post-heart Attack Mortality

Data on the incidence of heart attacks (acute myocardial infarctions, or AMIs) and post-AMI mortality are constructed from the Medicare Claims data from 1989–2000, along with data through 2001 on mortality. These data include 20 percent of Medicare admissions for 1989 to 1991, and 100 percent from 1992 to 2000. We use every fee-for-service heart attack admission to create a longitudinal cohort of 2.5 million fee-for-service enrollees age 65 or over coded with acute myocardial infarction. We assign patients to counties based on their residence (rather than where they were treated), and use linked death certificate data to see whether patients survived a 90-day or one-year window.

From this micro-data we construct a risk-adjusted county-year level measure of post-AMI mortality by regressing 1-year and 90-day mortality on a full set of age (5-year age categories, 65–69, 70–74, etc.), race, and sex interactions, and ten co-morbidities (including cancer (metastatic and non-metastatic), diabetes, liver dysfunction, vascular disease, pulmonary disorders, dementia, and severity of heart attack). Over this period there was a marked decline in the severity of diagnosed AMIs, with less-fatal subendocardial or non-q wave AMI rising from 25% of all AMIs in 1989 to half in 2000 (in part because of better detection techniques). We therefore control for whether heart attacks are q wave or non-q-wave in our regression analysis. Thus secular changes in demographic composition, severity of AMI, and health status are controlled for. We then calculate the residual mortality for each year in our sample, 1989 to 2000. The measures of heart attack mortality we use in the rest of our analysis thus refer to demographic and illness adjusted post-AMI mortality among Medicare recipients over age 65. We use three-year averages (1989–1991 and 1998–2000) of this mortality in our analysis. In 1998–2000, 23 percent of Medicare heart attack victims died within 90 days, which represented a five-percentage-point decline from 1989–1991.

We construct the county-year level incidence of heart attacks similarly, adjusting for the age, sex, and race of county populations. An average of 0.78 percent of the population suffered a heart attack in 1998–2000, down from 0.81 in 1989–1991.

E. Medicare DSH Payments and Hospital Composition

We use the Medicare DSH payments made to each hospital from the Medicare *Impact* files for 1990 and 2000. We also calculate the number of beds in hospitals of different ownerships and types and the fraction of each hospital’s patients that are poor from the 1999 Medicare *Impact* files. We use this information to calculate our measure of the overlap in populations served by public and private hospitals: we array public hospitals based on what fraction of their patients are poor and choose the median public hospital. We then calculate what fraction of private hospitals have at least that fraction of their patients poor.

F. Covariates

Other county-level covariates come from the *Area Resource File*, including the unemployment rate, per capita income, percent of the population that is white, percent of adults holding a high school diploma, median home value, percent of population living in poverty, fraction of households that are single parent, and total population. We also obtain the fraction of patient-days in each county’s hospitals accounted for by Medicare and Medicaid patients. These covariates are measured as the county-level change from 1990 to 1999 (unemployment rate, real income, poverty) or 1990 to 2000 (single parent households, percent white, real home value, education, patient days) based on data availability. We deflate using CPI.

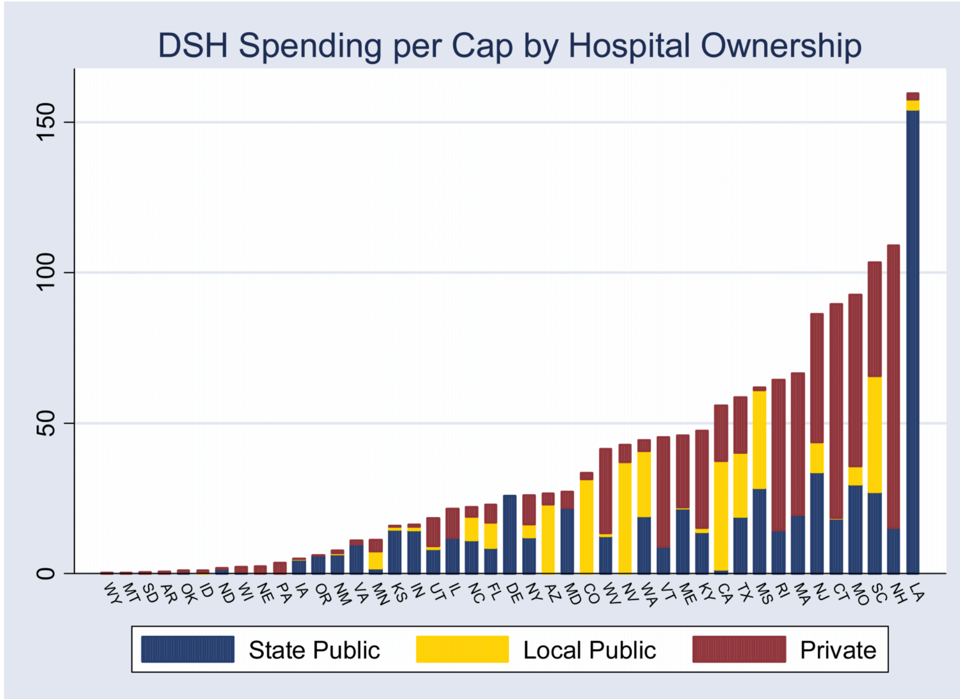

Figure 1.

Notes: Medicaid Disproportionate Share Hospital spending per capita by state and type of hospital (State-owned public, local-owned public (county and city), and privately-owned (for-profit and not-for-profit)). Data come from *Centers for Medicare & Medicaid Services*, as reported by state agencies, averaging available data for 1998, 1999, and 2000.

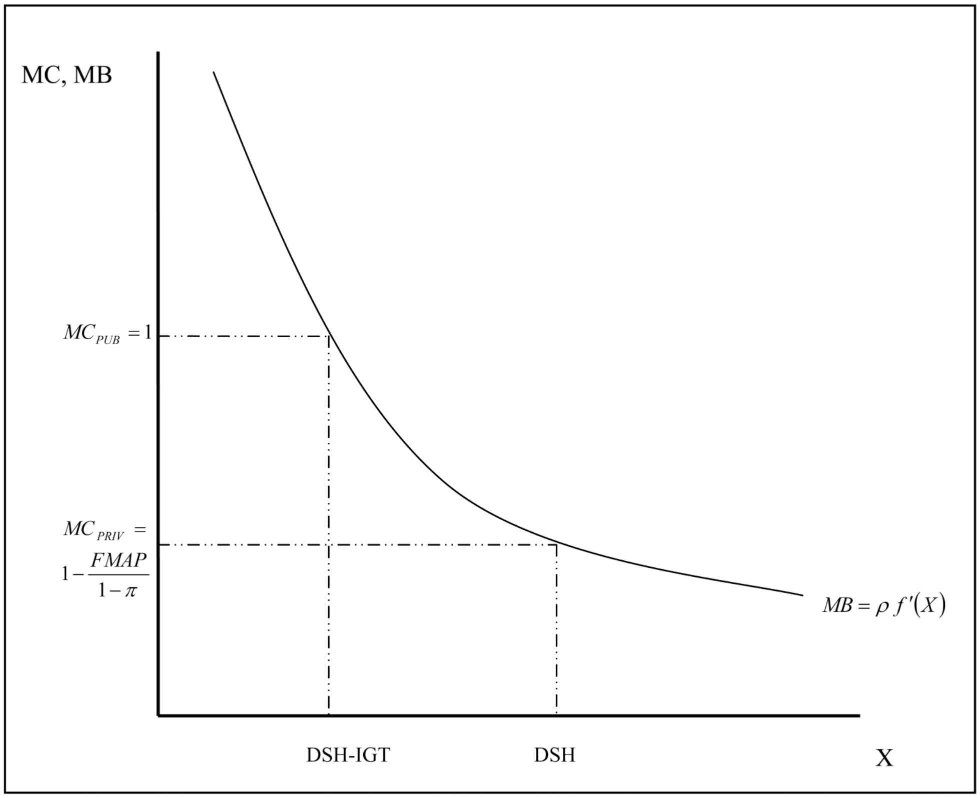

Figure 2.
Model of State Choice of DSH Spending and Intergovernmental Transfers

Table 1

DSH Payments by Type of Hospital

	Millions of Dollars (percent of total)			
	State	**County & Local Public**	**Not-For-Profit**	**For-Profit**
Acute Care Hospitals	1,324 (14.0%)	2,708 (28.6%)	2,862 (30.2%)	354 (3.7%)
Other Hospitals	1,809 (19.1%)	57 (0.6%)	31 (0.3%)	89 (0.9%)

Total for our sample (matched to AHA IDs): $9.5 billion

Notes: DSH data from the Centers for Medicare & Medicaid Services, averaged for 1998–2000, merged with American Hospital Association information on hospital location, type, and service of hospital, and aggregated to county level.

Table 2

Summary Statistics

	Mean	Std Dev	N
County Budgets (real per capita)			
Total Revenues: Annual Average, 1997–1999	1120	1285	2498
Total Revenues: Change, 1987–1989 to 1997–1999	163	230	2491
Intergovernmental Revenues: Annual Average, 1997–1999	404	487	2486
Intergovernmental Revenues: Change, 1987–1989 to 1997–1999	65	101	2471
Intergovernmental Expenditures: Annual Average, 1997–1999	68	108	1583
Intergovernmental Expenditures: Change, 1987–1989 to 1997–1999	19	46	1397
Disproportionate Share Hospital Payments (real per capita)			
Annual Average, 1998–2000	31	37	2526
Infant Mortality (within 28 days, per thousand births)			
Annual Average, 1998–2000	4.5	1.8	2524
Change, 1988–1990 to 1998–2000	−1.3	2.0	2520
Low Birth Weight (per thousand births)			
Annual Average, 1998–2000	73.1	14.1	2758
Change, 1988–1990 to 1998–2000	6.4	10.2	2753
Post-Heart Attack Mortality (within 90-days, risk adjusted)			
Annual Average, 1998–2000	0.23	0.03	2520
Change, 1989–1991 to 1998–2000	−0.05	0.07	2478
Incidence of heart attack (per thousand, age-sex-race adjusted)			
Annual Average, 1998–2000	7.8	2.7	2524
Change, 1989–1991 to 1998–2000	−0.3	2.4	2522
Covariates (change in real values, 1990 to 2000)			
Change in percent white	−0.04	0.03	2525
Change in unemployment rate	−1.16	1.44	2525
Change in per capita income	10,921	7,081	2525
Change in percent poor	−0.52	2.27	2525
Change in percent with HS Diploma	4.55	3.37	2525
Change in median home value	50,380	93,745	2525
Change in percent single parent households	0.03	0.01	2524
Change in share of patients covered by Medicare	0.04	0.10	2526
Change in share of patients covered by Medicaid	0.03	0.08	2526
Change in Medicare DSH payments	1,537,955	1,945,373	2526
State-level Variables			
Fraction of state DSH coming from local intergovernmental transfers, 1997	0.17	0.33	37
DSH payments to county hosps per Medicaid/Uninsured Patient, 1997	20.96	39.84	43
DSH to county hosps over total DSH to county and private hosps, 1998–2000	0.20	0.29	42
Unemployment rate (1999)	3.88	1.43	48
Per capita income (1999)	26,131	8,721	48
Percent white (2000)	76.96	22.36	48
Percent below poverty line (2000)	11.20	4.30	48
Deficit indicator (1992–1994)	0.13	0.33	48
Population (1999)	5,091,053	6,105,402	48

Notes: Budget data are measured in real (1999=100) per capita dollars and come from the Survey of Government Finances. DSH data from the Centers for Medicare & Medicaid Services, merged with American Hospital Association information on hospital location, type, and service of hospital, then aggregated to county level.

Infant mortality, low birth weight, and covariate data are from the Area Resource File.

Post-heart attack mortality and incidence are from Medicare Claims data, adjusted for age, sex, race, and illness.

All county means are weighted by 1998 population, except mortality (weighted by birth or heart-attack incidence).

Table 3

Predictors of Financial "Shenanigans"

	Share of State Funds from Local Intergovernmental		County DSH per Medicaid/Uninsured Person		Share of County and Private Hospital DSH Going to Counties	
	(1)	(2)	(3)	(4)	(5)	(6)
Fraction of private hospitals with a lower ratio of poor patients than the median public hospital	0.44 (.24)	0.49 (.25)	50.71 (24.41)	45.58 (23.45)	0.24 (.16)	0.24 (.18)
Public Share of Hospital Beds	1.11 (.44)	1.18 (.46)	94.71 (42.44)	96.14 (50.08)	0.92 (.31)	0.92 (.37)
Log of Population	0.15 (.04)	0.20 (.06)	10.89 (4.84)	7.25 (7.45)	0.08 (.04)	0.08 (.05)
Per Capita Income ($thousands)		−0.016 (.009)		1.527 (1.318)		0.001 (.009)
Unemployment Rate		0.01 (.06)		8.61 (4.49)		0.01 (.04)
State Operating in Deficit in 1992–1994		0.27 (.13)		0.40 (14.58)		0.02 (.10)
Mean of Dependent Variable	0.18	0.18	21.0	21.0	0.20	0.20
N	34	34	43	43	42	42

Notes: State-level analysis. Robust standard errors in parentheses.

DSH spending data aggregated to state-level, based on hospital-level DSH data from the Centers for Medicare & Medicaid Services, 1998–2000, merged to American Hospital Association data on hospital location and characteristics.

Data on overlap of poverty distribution (based on Medicare DSH adjustment factor) and on public share of hospital beds from Medicare Impact Reports for 1999. Overlap of poverty distribution (first row) measured by calculating the fraction of each hospital's patient pool that is below the poverty line, choosing the median public hospital, and then seeing what proportion of private hospitals have a lower fraction than that.

State-level reported IGT use (for columns 1 and 2) and size of DSH per Medicaid enrollee (for columns 3 and 4) for 1997 from Coughlin, Ku, and Kim (2000).

State deficit information from Survey and Census of Government Finances, in real per capita dollars. Covariates from Area Resource File, for 2000.

Table 4

Characteristics of States that Use Intergovernmental Transfers to Fund DSH

	States Not Using IGT Mean	Std Dev	States Using IGT Mean	Std Dev
DSH ($ Per Capita)	22.9	34.7	57.1	30.4
To State Hospitals	13.4	31.7	18.6	10.2
To County Hospitals	2.0	5.5	11.5	13.6
DSH to County Hospitals Per Medicaid/ Uninsured Patient	8.3	22.2	50.4	46.2
County Share of DSH to County and Private Hospitals	0.17	0.27	0.34	0.32
N	23		11	

Notes: State-level data.

DSH expenditures are measured in real per capita dollars.

State-level size of DSH per Medicaid enrollee and reported IGT use for 1997, from Coughlin, Ku, and Kim (2000).

DSH spending data aggregated to state-level, based on hospital-level DSH data from the Centers for Medicare & Medicaid Services, measured as the average of 1998–2000 (in real per capita 1999 dollars), merged to American Hospital Association data on hospital location and characteristics.

Table 5

The Effect of DSH Spending on County Budgets

	Change in Net Intergovernmental Revenues (1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Change in DSH to County Hospitals	0.57 (.18)	0.57 (.20)	0.51 (.13)	0.53 (.17)	0.52 (.17)	0.36 (.11)	0.48 (.28)	0.01 (.39)
Change in DSH to State Hospitals		0.09 (.08)	0.10 (.08)	0.10 (.08)	0.11 (.08)	0.14 (.09)	0.13 (.08)	0.12 (.08)
Change in DSH to Private Hospitals		−0.04 (.13)	−0.01 (.12)	−0.02 (.13)	0.02 (.13)	0.02 (.11)	−0.04 (.11)	−0.01 (.12)
Interactions (baseline is counties in states that do the greatest appropriation of DSH								
Change in DSH to County Hospitals Interacted with Dummy if State has:								
Low Local Intergovernmental Transfer Share			1.81 (.70)					
Low County DSH Spending per Medicaid/Uninsured Patient				1.44 (.90)				
Low Share of County and Private Hospital Spending to Counties					1.90 (.47)			
Change in DSH to County Hospitals Interacted with Continuous Measures of How **Low** Each State's Use is of:								
Local Intergovernmental Transfers						1.97 (.55)		
County DSH Spending per Medicaid/Uninsured Patient							3.20 (1.02)	
Share of County and Private Hospital Spending Going to Counties								1.65 (.92)
State fixed effects and covariates	yes	yes	yes	yes	yes	yes	yes	yes
Observations	1392	1392	1236	1392	1392	1236	1392	1392

Notes: County-level analysis. Standard errors in parentheses, clustered at state level. Regressions weighted by population.

County budget data from annual Survey of Government Finances. Difference measured as average of 1997–1999 minus average of 1987–1989 (in real 1999 per capita dollars).

DSH spending data aggregated to county-level, based on hospital-level DSH data from the Centers for Medicare & Medicaid Services, merged to American Hospital Association data on hospital location and characteristics. Measured as the average of 1998–2000 (in real per capita 1999 dollars).

State-level size of DSH per Medicaid enrollee and reported IGT use (interaction terms) from Coughlin, Ku, and Kim (2000). Covariates include fraction white, unemployment rate, per capita income, percent of population with income below the poverty line, percent of the population with less than a high school degree, and fraction of families headed by a single parent, from the Area Resource File, and state fixed effects.

Interaction terms: Dummy variables are indicators for states with lower-than-average values for the three measures. Continuous variables are defined as [1 − (each state's value/maximum state's value)] - always between 0 and 1. By using these definitions, the top row always shows the minimum increase in net IGT (for counties in state doing maximum appropriation), while interaction terms show the additional increment in resources received by counties in states doing less appropriation.

Table 6

The Effect of Change in Per Capita DSH Spending on Mortality

	Change in 28-day Infant Mortality (Effect of $100 Change)			Change in 90-day Post-Heart Attack Mortality (Effect of $100 Change)		
	(1)	(2)	(3)	(4)	(5)	(6)
Change in DSH to All Hospitals	−0.062 (.018)			−1.17 (.75)		
Change in "Effective" DSH (see note)		−0.101 (.026)			−2.78(.69)	
Change in DSH to Private Acute Care Hospitals			−0.093 (.035)			−1.90 (.89)
Change in Effective DSH to County Acute Care Hospitals			−0.122 (.026)			−4.88 (1.54)
Change in "Ineffective" DSH (see note)		−0.034 (.022)			−0.16 (.82)	
Change in Ineffective DSH to County Acute Care Hospitals			−0.008 (.065)			0.63 (3.01)
Change in Other Ineffective DSH (to State-Owned and Non-Acute Care Hospitals)			−0.035 (.022)			−0.13 (.82)
Probability Effective DSH = Ineffective DSH		0.049			0.019	
State fixed effects and covariates	yes	yes	yes	yes	yes	yes
Observations	2519	2519	2519	2476	2476	2476

Notes: County-level analysis. Standard errors in parentheses, clustered at state level. Regressions weighted by births or heart attacks in each period. Sample includes counties in 42 states from column (5) of Table 3.

Infant mortality (within 28 days) from Area Resource File. Change calculated as average for 1998–2000 minus average for 1988–1990, in percentage points.

Heart attack mortality (within 90 days) from Medicare Claims Data, adjusted for age, sex, race, and ten measures of illness. Change calculated as average for 1998–2000 minus average for 1989–1991, measured in percentage points. "Effective" DSH defined based on column (5) of Table 5: In states with a low fraction of DSH dollars going to county hospitals (low "expropriators"), effective DSH is defined as all DSH going to acute care hospitals. In states with a high fraction of DSH dollars going to county hospitals (high "expropriators"), effective DSH is defined as all DSH going to private acute care hospitals plus 53 percent of DSH going to county acute care hospitals.

DSH spending data aggregated to county-level, based on hospital-level DSH data from the Centers for Medicare & Medicaid Services, merged to American Hospital Association data on hospital location and characteristics. Measured as the average of 1998–2000 (in real per capita 1999 dollars).

Covariates include fraction white, unemployment rate, per capita income, percent of population with income below the poverty line, percent of the population with less than a high school degree, and fraction of families headed by a single parent, from the Area Resource File, and state fixed effects.

Table 7

Robustness Checks

	Infant Mortality: Change in 28-day infant mortality (1988–90 to 1998–00)			Change in Ln (28-day mortality)	Change in 1-year mortality (1976–80 to 1988–90)	Post-Heart Attack Mortality: Change in 90-day post heart attack mortality (1989–91 to 1998–00)			Change in Ln (90-day mortality)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Change in Effective DSH (Effect of $100 change)	−0.101 (.026)	−0.101 (.027)	−0.081 (.019)	−0.198 (.047)	0.033 (.030)	−2.775 (.689)	−2.785 (.712)	−1.563 (.554)	−0.098 (.026)
Change in Other DSH (Effect of $100 change)	−0.034 (.022)	−0.035 (.022)	−0.015 (.016)	−0.074 (.043)	0.042 (.028)	−0.160 (.821)	−0.203 (.861)	−0.147 (.487)	−0.017 (.035)
Dummy for any DSH		0.002 (.016)					0.001 (.005)		
Infant mortality in 1988/90 (instrumented with mortality in 1985/87)			−0.39 (.10)						
Heart attack mortality in 1989/91 (instrumented with mortality in 1992/1994)								−0.44 (.10)	
Implied effect of $100 change in effective DSH at mean base mortality				−0.115					−2.747

Notes: County-level analysis. Standard errors in parentheses, clustered at state level. Regressions weighted by births or heart attacks in each period. Sample includes counties in 42 states from column (5) of Table 3.

Infant mortality (within 28 days) from Area Resource File. Change calculated as average for 1998–2000 minus average for 1988–1990, in percentage points.

Heart attack mortality (within 90 days) from Medicare Claims Data, adjusted for age, sex, race, and ten measures of illness. Change calculated as average for 1998–2000 minus average for 1989–1991, measured in percentage points.

"Effective" DSH defined based on column (5) of Table 5: In states with a low fraction of DSH dollars going to county hospitals (low "expropriators"), effective DSH is defined as all DSH going to acute care hospitals. In states with a high fraction of DSH dollars going to county hospitals (high "expropriators"), effective DSH is defined as all DSH going to private acute care hospitals plus 53 percent of DSH going to county acute care hospitals.

DSH spending data aggregated to county-level, based on hospital-level DSH data from the Centers for Medicare & Medicaid Services, merged to American Hospital Association data on hospital location and characteristics. Measured as the average of 1998–2000 (in real per capita 1999 dollars). Covariates include fraction white, unemployment rate, per capita income, percent of population with income below the poverty line, percent of the population with less than a high school degree, and fraction of families headed by a single parent, from the Area Resource File, and state fixed effects.

Table 8

How DSH Spending Affects Mortality

	Infant Mortality Outcomes								Post-Heart Attack Outcomes			
	Change in fraction born LBW		Change in 28-day mortality		Change in mortality between 28 days and one year		Change in incidence of heart attacks		Change in 90-day post-heart attack mortality		Change in mortality between 90 days and one year	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Change in DSH to All Hospitals (Effect of $100 change)	−0.257 (.051)		−0.046 (.017)		0.000 (.010)		−0.02 (.03)		−1.20 (.76)		−0.35 (.33)	
Change in "Effective" DSH (Effect of $100 change)		−0.385 (.120)		−0.078 (.028)		−0.005 (.014)		−0.02 (.05)		−2.81 (.73)		−0.08 (.46)
Change in "Ineffective" DSH (Effect of $100 change)		−0.170 (.081)		−0.025 (.019)		0.003 (.016)		−0.01 (.04)		−0.20 (.80)		−0.52 (.57)
Change in Fraction Low Birth Weight			0.060 (.005)	0.059 (.005)								
Change in incidence of heart attacks									−1.89 (.58)	−1.90 (.59)		
State fixed effects and covariates	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes

Notes: County-level analysis. Standard errors in parentheses, clustered at state level. Regressions weighted by births or heart attacks in each period. Sample includes counties in 42 states from column (5) of Table 3.

Infant mortality (within 28 days) and incidence of low birth weight from Area Resource File. Change in mortality calculated as average for 1998–2000 minus average for 1988–1990, in percentage points.

Heart attack mortality (within 90 days) and incidence from Medicare Claims Data, adjusted for age, sex, race, and ten measures of illness. Change in mortality calculated as average for 1998–2000 minus average for 1989–1991, measured in percentage points.

"Effective" DSH defined based on column (5) of Table 5: In states with a low fraction of DSH dollars going to county hospitals (low "expropriators"), effective DSH is defined as all DSH going to acute care hospitals. In states with a high fraction of DSH dollars going to county hospitals (high "expropriators"), effective DSH is defined as all DSH going to private acute care hospitals plus 53 percent of DSH going to county acute care hospitals.

DSH spending data aggregated to county-level, based on hospital-level DSH data from the Centers for Medicare & Medicaid Services, merged to American Hospital Association data on hospital location and characteristics. Measured as the average of 1998–2000 (in real per capita 1999 dollars).

Covariates include fraction white, unemployment rate, per capita income, percent of population with income below the poverty line, percent of the population with less than a high school degree, and fraction of families headed by a single parent, from the Area Resource File, and state fixed effects.