THE BUDGETARY REPERCUSSIONS OF CAPITAL CONVICTIONS

Katherine Baicker*
Dartmouth College and NBER

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ABSTRACT

Control of public spending and revenues is increasingly being left to states and localities. In order to understand the consequences of such a movement on the distribution of social spending, it is necessary to understand how fiscal distress will affect state and local budgets. This paper exploits the large and unexpected negative shock to county budgets imposed by the presence of capital crime trials, first to understand the real incidence of the cost of capital convictions, and second to uncover the effects of local fiscal distress on the level and distribution of public spending and revenues. I show that these trials are quite costly relative to county budgets, and that the costs are borne primarily by increasing taxes (although perhaps in part by decreases in spending on police and highways). The results highlight the vulnerability of county budgets to fiscal shocks: each trial causes an increase in county spending of more than $2 million, implying an increase of more than $6 billion in both expenditures and revenues between 1982 and 1997. Using these trials as a source of exogenous variation to examine inter-jurisdictional spillovers, I find significant spillovers of both spending and revenues between counties.

I. INTRODUCTION

The 1990s have seen a series of radical changes in the funding of social insurance programs, including a movement away from federal entitlements and towards increasing state and local responsibility for the maintenance of a social safety net and other public spending. In 1997 the state of Florida sued the U.S. government in an effort to recover funds for SSI and food stamp benefits for legal immigrants cut by the 1996 Welfare Reform Act. The mayor of Dade county alleged that "the new federal law is likely to cost Dade County $300 million a year" (Morgan, St. Petersburg Times, 1997). In nearby Pinellas County officials want to ensure that the state of Florida picks up the tab (as most other states do) for the children's health insurance program to offset the more than $800,000 the local agency has contributed in matching funds since 1996 (Krueger, St. Petersburg Times, 2001). Localities face a related problem when federal funds for ongoing programs such as increased police forces dry up. The $8.8 billion federal Community Oriented Police Services program that funded 100,000 additional police officers is expiring, and finding funds to maintain the expanded force puts a great strain on local budgets (Ortega, Columbus Dispatch, 1997).

In order to understand the likely consequences of such a movement on the distribution of public spending, it is necessary to understand how fiscal distress affects state and local budgets. How will local jurisdictions react to budget shocks? Will they cut back welfare spending or capital projects, or will they raise taxes? If taxes are raised, on whom does the burden of those increases fall? Will housing prices change? Will this affect mobility between localities? How will neighboring jurisdictions react? How long

1

do these changes persist? What are the implications of these changes for the well-being of residents?

This research builds on several strands of the public economics literature, including the effect of fiscal institutions on states' ability to respond to shocks (see Poterba, 1994, and Rueben, 1998), the effect of one state's spending on the spending of neighboring states (see Case, Hines, and Rosen, 1993, Besley and Case, 1995, Figlio, Van Kolpin, and Reid, 1999, and Baicker, 2000), and the effect of selective migration and population composition on the public bundle (see Poterba, 1997, Borjas and Hilton, 1996, Meyer, 1998, and Levine and Zimmerman, 1999). There is a wide literature on the "flypaper effect," or the tendency of shocks to be disproportionately absorbed in the budget in which they originate, rather than being spread across all categories according to marginal propensities to consume – although there are many potential explanations for such observed behavior (see Hines and Thaler, 1995, Strumpf, 1998, Baicker, 2001, Knight, 2002, for example). Together this research suggests that the shift in control of funds from the federal government to the states and localities will have a profound effect on the landscape of public spending.

When control of funds is shifted from the federal to the state or local level, fiscal institutions and demographics may play a much larger role in the distribution of resources, and externalities or spillovers between jurisdictions may create very different regional equilibria. Therefore the effect of fiscal shocks and the ability of states and localities to shift funds between categories will be of increasing importance in determining the distribution of social spending both within and between jurisdictions. It is unclear, however, whether or not the effects documented in this literature are peculiar

to state budgets or to fiscal shocks originating in welfare programs themselves. Will shocks to other parts of the budget be accommodated by decreases in welfare spending too? Are local budgets fundamentally different from state budgets? The next logical step in this line of research is to examine other sources of fiscal distress at the local level. This is important not just for understanding the implications of welfare reform, but for understanding the interplay among all state and local spending categories and the effect of other financial reforms. Other kinds of fiscal stress and stress at the local level may produce different budget spillovers, both between budget categories and between jurisdictions.

Trying capital crimes is an increasing source of fiscal distress for counties, and the cost of paying for these very expensive convictions has fueled a public debate about their effectiveness in deterring crime and on whom the burden of paying for them should fall. The estimated cost of a death penalty case is over $2 million (Dieter, 1994). Documenting the ultimate incidence of the cost of these trials would be a valuable exercise in and of itself, but they also provide a particularly apt setting in which to study the intra- and interjurisdictional effects of local fiscal distress. As shown below, capital trials are unexpected (most counties, even in states with capital punishment provisions, saw no such convictions between 1983 and 1997), they represent a significant expense to the county relative to its budget, they are likely to be uncorrelated with prior spending and revenue decisions, and they offer no localized change in services associated with living in a particular county. This paper takes advantage of the shock these trials impose to investigate these questions empirically. I explore this source of variation first to determine which areas of local budgets absorb the shocks, the spillover effects of the

budget changes, and where the ultimate incidence of capital convictions lies. The optimal financial design for public programs depends crucially on how each jurisdiction absorbs shocks and on how shocks affect the total bundle of resources available to different sectors of the population.

II.   EMPIRICAL STRATEGY

In order to investigate the effects of fiscal distress on program spending we need to identify some exogenous source of budgetary stress not generated within these programs. It is insufficient (and potentially misleading) merely to examine the relationships between different categories of spending without abstracting from economic and political conditions. In order to uncover causal relationships, rather than correlations, it is necessary to find shocks to local budgets that are unexpected and uninfluenced by things like local economic conditions. Capital crime trials provide just such a source of financial stress.

Background on Capital Crime Trials

Trying capital murder cases can pose a significant financial burden on localities (see Dieter, 1994, for a review). An Advisory Commission on Intergovernmental Relations report (1993, p. 24) notes that court systems in most states "receive less than a third of their budgets from the state. In most systems, counties provide the remaining funds." Costs for seeing a case through to a death sentence are estimated at more than $2 million, with some estimates ranging as high as $7 million to execution (Burnett, 1999). This is as much as 10 times more than life in prison, and most of these costs accrue at the

trial level, contrary to much public discourse about the cost of "endless appeals" (Dieter).

Since nine out of ten defendants in capital crime trials are indigent, counties must often

pay legal costs for both prosecution and defense (Moneyline, 1995). The median county

had a $12 million budget in 1997, while the tenth percentile spent $2 million. The

distribution of county spending is shown in Figure 1. In 1991 NJ spent $16 million to

impose the death penalty, and the next year the state laid off 500 police officers because

they could not afford to pay them (Dieter).

Counties in many states complain of the financial hardships the trials impose.

Jasper County, Texas claims to have already raised property taxes by 8 percent to pay for

the trial of the three men accused of killing James Byrd, and will have to delay new

computer purchases and construction (Burnett). According to the Jasper County Auditor

(as reported by Gold, 2002), the county has already spent more than a million dollars

(including 28 percent on defense attorneys, 17 percent on salary for extra prosecutors,

and 16 percent on jury expenses, courthouse security, and the court reporter). One Texas

county tried to raise taxes to pay for a high-profile capital trial and the taxpayers revolted

and voted for a tax rollback, which forced the county commissioners to cut funding to fire

and ambulance services in the county, while another case caused a border dispute

between counties trying to avoid the cost of a particular trial (Dieter). Jasper County,

Mississippi (no relation) spent three times more on a capital trial in 1995 than it spent on

its libraries, and, lacking even parking meters to raise revenue, had to increase property

and automobile taxes to raise the funds (Moneyline). Dieter reports several more

examples:

> In Lincoln County, Georgia . . . the county commissioners refused to pay
> the defense costs when the attorney won a new trial for a death row inmate

> [and] were sent to jail. [The] chair of the County Commission explained:
> "We're a rural county of 7,500 people with a small tax base. We had to
> raise taxes once already for this case when it was originally tried, and now
> we are going to have to raise taxes again. It's not fair." (p. 6)

> Quitman County, the poorest in Mississippi . . . had to raise taxes and
> borrow money to try [two cases]. [The county clerk says] 'I'm thinking
> we'll become even poorer and I'm also thinking that a lot of people are
> going to move out of the county because of the increased tax burden and
> move over to other counties where the taxes are not quite as high.'

Gold (2002) reports that Quitman county took out loans and raised taxes a total of three times. "Now, the county is having trouble attracting a new tenant to a vacant warehouse because it has higher property taxes than any nearby county." Kolbert (2001) reports that the cost of security alone in one trial in New York prompted the county to impose a sales tax.

While these cases are quite expensive, contrary to the impression given by much media coverage they are fairly uncommon. Over the period used in this analysis, 1983 to 1997, 80% of counties saw no such convictions. This seems like an insurable risk, and we might expect some form of intergovernmental risk sharing. Indeed, several states (such as Utah, Texas, Idaho, Wyoming, Washington, and Mississippi) have implemented or are currently considering legislation to offset local costs, but (perhaps because of lags in adapting to the increasing financial burden) during the period of study few such reimbursements occurred.

This anecdotal evidence suggests that the effects of paying for these trials should be discernable in county budgets. It also suggests that we pay particular attention to local taxes, and to capital and police protection expenditures. By examining the effects of

paying for these extraordinary and often unexpected expenses on local finances, we may gain valuable insight into the effects of both temporary and permanent exogenous increases in local expenditures and taxes, as well as a better understanding of the incidence of this expensive policy.

Estimation Framework

A simple economic model would predict that the cost of a negative windfall would be borne based on the marginal propensity to consume public goods and private goods out of income. Given the low fraction of personal income devoted to county budgets (less than 3 percent in this period), we would expect the bulk of the costs to be borne by increased taxes, and potentially smoothed over time by borrowing. (See Hines and Thaler, 1995, for evidence that many shocks tend to be absorbed disproportionately within closely related budget categories, known as the "flypaper effect.") I estimate the effect of paying for a capital crime conviction on total revenues, total spending, and spending on specific public goods.

Several different models are estimated here, but each includes county effects (α_i), time effects (β_t), demographic and economic controls including population (X_{it}), and some also include a state-specific time trend (*state* * *t*). The dependent variable is spending on a particular category or tax revenue from a particular source, and the independent variable of interest is the presence of a capital conviction.

$$spending_{it} = \alpha_i + \beta_t + X_{it}\gamma + state_i * t + \delta\, capital\; conviction_{it} + \varepsilon_{it} \qquad (1)$$

$$taxation_{it} = \alpha_i + \beta_t + X_{it}\gamma + state_i * t + \delta\, capital\; conviction_{it} + \varepsilon_{it} \qquad (2)$$

We might want to limit the analysis to counties with few of these trials (so that they are least expected), to counties in states with the death penalty (using just counties in death-penalty states, as opposed to all counties without a case, as controls), or to smaller counties (which may have less of a cushion and potentially even higher variable costs). I also present analysis using a log specification, but since we would like to put a dollar price tag on the cost of capital convictions I begin with this model.

III.   DATA

County Budgets

County budget data comes from two sources. The first is the *Census of Governments*, conducted every 5 years by the Bureau of the Census, and the second is the *Annual Survey of Government Finances*, conducted most years on a sample of county governments. County government data is used, rather than aggregations of all sub-governments to the county-area level. There are several logistical difficulties involved in using this data, some of which can be overcome and some of which cannot. Disaggregation in the early years is limited, and in some years missing observations and 0 values are indistinguishable. The number of counties in the survey is around 2100 for years before 1992 and 1500 in the years after.

Capital Convictions

The *National Corrections Reporting Program* surveys all inmates admitted to correctional facilities and is available annually from 1983 to 1997. It includes information on the county and date of conviction as well as the severity of the sentence.

From this information I have compiled the number of prisoners admitted to prison under sentence of death by county by year. Because the survey is limited to inmates, those acquitted in capital trials will be omitted from the analysis. The source of variation I use is therefore the presence of a capital conviction. While data on the fraction of capital trials leading to convictions is not readily available, a Department of Justice study of the 75 largest counties in 1998 found that 77% of murder trials resulted in convictions.

While the NCRP survey has much detail on the individual prisoners and their sentences, it has only sentencing dates, not the time span of the trial itself. Capital trials can often take more than a year, so a prison admission in year t may very well affect county budgets in year $t-1$. While we can use the data to help inform us about the proper time horizon, this noise is likely only to bias results towards zero. Several states do not participate in the survey in several years. Again, these missing observations should not bias the results.

One possible concern with the use of capital trials as an exogenous shock is that a prosecutor may have discretion over whether or not to charge a defendant with a capital crime, and that his or her decision may be influenced by the financial circumstances of the jurisdiction. Gold reports that "Costs notwithstanding, county officials say they pursue the death penalty when the crime warrants it." Furthermore, a judge in Ohio recently ruled (after being ordered to review the case by the appeals court) that costs could not prevent a prosecutor from seeking the death penalty (AP, 2002). This and the other anecdotal evidence cited above suggests that this is not the case, and while this proposition is difficult to test rigorously, the data suggest that capital convictions are not

predictable with the observed local conditions.[1] The probability of having a capital conviction in this period is 5.23 percent in counties with a deficit last period, and 5.12 percent in counties without (with a chi-squared test yielding a 70 percent probability that these are the same). A regression of the capital conviction dummy on the county unemployment rate and the budget deficit or surplus explains only .0064 of the variation in capital convictions.

Data is summarized in Table 1. As the first panel shows, there is a great deal of variation between counties in the number of prisoners convicted of capital crimes. Over the 18 years sampled, most counties (more than 80 percent) had no death penalty convictions and more than 10 percent had exactly 1 year with a death penalty conviction between 1983 and 1997. Thus, for the vast majority of counties these convictions are rare occurrences. Of course, some large counties have several convictions each year. While costs for frequent capital conviction counties may still be variable, they are bound to be more anticipated. Furthermore, the (relatively fixed) cost is likely to be a bigger shock to smaller counties, which have a smaller tax base and are less likely to have the requisite personnel already on staff. Even for relatively large counties (those over 75,000) these events are relatively rare: 53 percent had none, and 17 percent had only 1. (Fortunately, from our perspective, they are still common enough to provide ample observations for estimation: more than 1,300 county-years saw at least one capital conviction.)

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[1] Ideally there would be an instrument for capital convictions that was not otherwise correlated with spending, but such an instrument is hard to find. One possibility would be to use the existence of capital punishment in the state at all – such as the legality of capital punishment, whether or not anyone has been put to death in the state, or the number of prisoners on death row. Unfortunately these instruments produce a very weak first stage and very large standard errors in the second stage.

Covariates

County covariates are available from several sources. The *County and City Data Book*, published annually by the Bureau of the Census, includes variables at the county-year level such as median age, personal income, land area, population, unemployment, and employment by sector (such as within government or construction). Further demographic breakdowns (such as population by race) are available only in the decennial census years. The *Statistical Abstract of the United States* provides some useful state-level data. County-level crime rates (both murders and an index of "serious crimes") come from the Department of Justice *Uniform Crime Reports.*

IV. RESULTS

Capital Convictions and Spending

Table 2 presents estimates of equations (1) and (2) above. I focus on total non-education expenditures (because in some states counties are responsible for school spending but in others school districts are separate governmental units), and on corrections and judicial expenditures. More detailed reported budget categories at the county level are sufficiently heterogeneous and inconsistently categorized that it is not clear exactly where the expenditures associated with the trials would appear, and further disaggregation of the data results in many missing observations. Capital convictions appear on the right-hand side as a dummy variable indicating the presence of an admission in year *t* or year *t+1*.[2]

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[2] This functional form is suggested by the data, as described above. While including a series of leads and lags forces the dropping of several years of data, the cumulative effect is the same as the coefficient presented here, and the most significant years are *t* and *t+1*.

The presence of a death penalty conviction has a significant effect on expenditures, coming through judicial and corrections spending. The presence of such a conviction increases judicial and corrections spending (and total spending) by more than $2 million, which is quite consistent with outside estimates of the cost of a death penalty case.[3] Controls in all columns include population, population density, the murder rate, the uniform crime report index of serious crimes, and county and year fixed effects. All columns except (3) include state-specific time trends. Column (4) includes only counties in state-years with death penalty legislation in place, effectively excluding non-death penalty states as controls. Each produces similar results: capital convictions impose a significant burden on county budgets, consistent with previous estimates of their total cost. Applying this estimate to the total number of convictions during this period implies an increase in county budgets of more than $6 billion between 1982 and 1997.

These costs are borne primarily by raising revenues. Column (5) reports the effect of a capital conviction on revenues (excluding intergovernmental transfers). Counties appear to finance these trials almost exclusively by raising taxes.

There are several reasons that a log specification might be preferable. First, we might be concerned that we are not adequately controlling for population in these specifications. If population ought to appear in a non-linear form, and if population (positively) affects both the probability of having a death penalty case and the amount a county spends, then the coefficient on death penalty convictions might be biased up. In this case, it might be more appropriate to examine per capita spending. Second, if

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[3] Since the right hand side variable is a dummy for the presence of a case in either of two years, we need to multiply the effect on spending by 2, but the average number of cases conditional on the dummy being positive is 1.6, so the total effect on spending implied by a coefficient of 2.5 million would be roughly 3.2 million per capital conviction.

spending and revenues in general grow by percentages, not by dollar amounts, then the log model will better fit county budget trends. Third, as Figure 1 shows, expenditures are asymmetric, with a long right tail. The disadvantage of this specification is that capital convictions might cost less for larger jurisdictions, not more. This issue is addressed below.

$$\ln(spending)_{it} = \alpha_i + \beta_t + X_{it}\gamma + \delta(capital\ conviction)_{it} + \varepsilon_{it} \tag{3}$$

Table 3 thus estimates equation (4), with different categories of spending and revenues as the dependent variable (and robust standard errors in parentheses). In this specification I decompose spending and revenues into several broad categories. X_{it} included the log of the population, murders, and crime index. The top panel uses all counties.

Here, too, total expenditures and revenues increase significantly with the presence of a capital conviction. The presence of a trial increases spending by 1.3 percent and revenues by 2.3 percent. Columns (2) through (6) estimate equation 4 for several subcategories of spending. While there is much more noise in the individual categories, judicial and corrections spending increases significantly (although not as much as total spending or revenues), while police and highway expenditures decrease significantly (about $515,000 together). Why would police protection and highways in particular face cuts? The fact that this spending decreases is consistent with the stories told by local officials and the anecdotal evidence above, and may be driven by the fact that these funds are either on hand and more easily accessible (police) or more easily delayed (capital

spending on highways). Delaying capital projects is akin to borrowing against future revenues. There may also be political factors driving these decisions – a reduction in services most visible to taxpayer-consumers may be the best way to motivate future tax increases. These decreases are only about 35% of the increase in total spending at the mean: the main way in which the trials are financed is through increased revenues to offset the increase in total expenditures.[4]

While previous research suggested that negative shocks to the welfare budgets of states resulted in cutbacks in other welfare spending, welfare budgets do not seem to bear the cost here. Spending on welfare, hospitals, and the like may be difficult to adjust in the short-run, because of more complex rules and legislation. This negative finding suggests that welfare programs are most at risk when jurisdictions face increases in spending on similar programs such as Medicaid.

One concern with this specification is that capital trials will represent a smaller proportional shock for bigger counties (and perhaps even a smaller shock in dollars if they experience a smaller increase in fixed costs). The lower panel presents the same estimations with the sample of counties limited to those with fewer than 100,000 residents, capital convictions in 3 or fewer years, and no capital convictions in the last two years. These restrictions are meant to limit the sample to counties for which the presence of a trial represents a real marginal shock. Results are similar, although

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[4] This analysis does not incorporate the dynamics of the budget adjustment process. The analysis discussed above suggests that the biggest impact of these trials is in the first two years and that spending then returns to pre-trial levels, but a more sophisticated econometric approach is required to separate out noise in the timing of the expense from a dynamic adjustment process. Unfortunately, adding additional leads or lags of the death conviction variable reduces the number of years available, making a more dynamic analysis difficult with the current limited time span. Another potential measure of the longer-term effect on county financial health is county bond ratings. Unfortunately, there is very little within-county variability of bond ratings during this period (as measured by Moody's ratings, for example), and many counties receive no rating at all, even if they issue bonds.

standard errors are larger (especially on subcategories). The elasticities of spending and revenues are similar, although this translates to a smaller dollar response at the mean. There is less evidence of an off-set in police and highway expenditure. If anything, these counties raise revenues differently: they seem more likely to utilize increases in charges, and less likely to use sales taxes (and indeed are much less likely to have a sales tax in the first place).

Thus, the observed local reaction to these shocks is consistent with the predictions of the theory: taxes increase in the short run, offset in part perhaps by a delay in capital expenditures. The next section examines the broader ripple effects of this shock to revenues.

Spillovers Between Counties

The "cost" of obtaining a capital conviction may extend well beyond the prosecuting county's borders. There are many reasons to think that one county's spending might influence another's, such as tax competition and selective migration (see Case, Hines, and Rosen, 1993, and Besley and Case, 1995). If one county raises its taxes it may enable the voters of a neighboring county to raise taxes (and thereby expenditures) without fear of high-income taxpayers moving out of the jurisdiction. Thus, estimating the full budgetary implication of these trials requires an examination of spillovers to other counties.

These spillovers are difficult to estimate, however, because an OLS regression of one county's spending on that of its neighbors could be biased by omitted controls for local political and economic conditions, correlated mismeasurement, and the like. If

capital convictions significantly increase spending and tax rates, we can use this as an exogenous source of variation to examine the jurisdictional spillover effects.

Table 4 presents both OLS estimates and IV estimates where neighboring counties' revenues are instrumented with the presence of neighbors' capital convictions. In this table a county's "neighbor" is the population-weighted average of geographically contiguous counties, but there are certainly there are other measure of "neighborliness" that would be equally reasonable. (Similar results are obtained when neighbors' spending is used, instead of neighbors' revenues.)

The OLS regressions in columns (1) and (3) suggest that a 10 percent increase in neighbors' revenues induces a 1.8 percent increase in own revenues and a 1.7 percent increase in own spending. These results are slightly lower than most estimates of state-to-state spillovers discussed above, but are consistent with the literature in general. The IV results are very similar, with elasticities of .23 for revenues and .27 for expenditures, and are measured quite precisely. The inclusion of these terms does not substantially change the estimated effects of a county's own capital convictions.

From this analysis we learn that county spending and revenue decisions have significant spillovers to neighboring jurisdictions, even when the potential endogeneity of neighbors' budget decisions is taken into account. It seems that a shock to one county's taxes, even in the absence of an accompanying increase in services, loosens a constraint on taxes and spending in neighboring counties. The extent to which this change in neighboring behavior is a "cost" of a capital conviction depends on the extent of other inter-county externalities.

V. CONCLUSION

This project explores the effects of the large negative shock to county budgets posed by the presence of a capital crime trial, first to understand the real incidence of these capital conviction costs and second to analyze the effects of local fiscal distress on the level and distribution of public spending and revenues. Analysis shows that counties bear the large and unexpected burden of capital convictions in part by raising taxes and in part by decreasing expenditures on police, and highway spending, while health and welfare spending seem to be maintained. The estimated increase in taxes and expenditures is significant, amounting to more than $6.5 billion over a 15-year period. This is true for large and small counties alike. These convictions have effects beyond county borders, consistent with the literature on local spillovers and "yardstick competition": using capital convictions as an instrument for neighboring counties' spending and revenues shows the presence of significant inter-jurisdictional spillovers (while abstracting from correlated economic conditions and the like). These results imply that the implementation of state programs to help offset these costs would be welfare enhancing.

This analysis suggests several interesting extensions. First, we could see which factors, such as demographics, affect the ways in which counties respond to such shocks. Second, the presence of a capital conviction could be an instrument for several endogenous right-hand side variables of interest. For example, since the convictions appear to affect taxes and police expenditures, they could serve as an instrument to examine the effect of property taxes on inter-county mobility or the effect of policing on crime.

As states and localities are given greater control over public funds, the distribution of spending and the well-being of residents will increasingly depend on the way states and localities accommodate shocks, their ability to shift funds between budget categories, and on the spillover effects of shocks within and between jurisdictions. Only through understanding the reactions of state and local governments to different fiscal conditions can we gauge the effectiveness and equity of the local provision of important safety net programs and infrastructure, both in terms of the distribution and the stability of resources.

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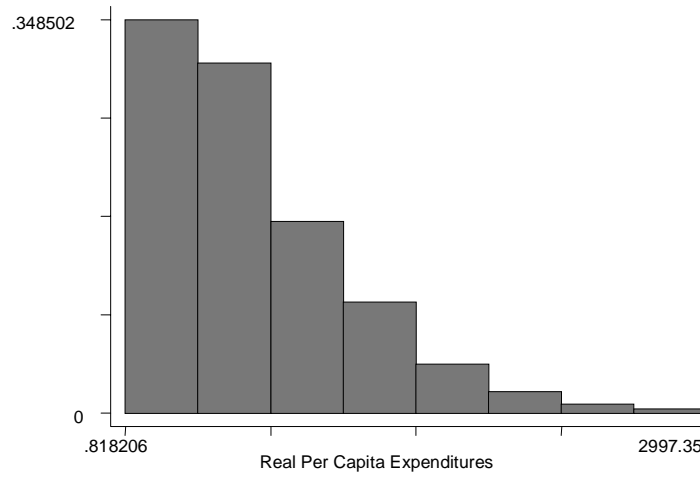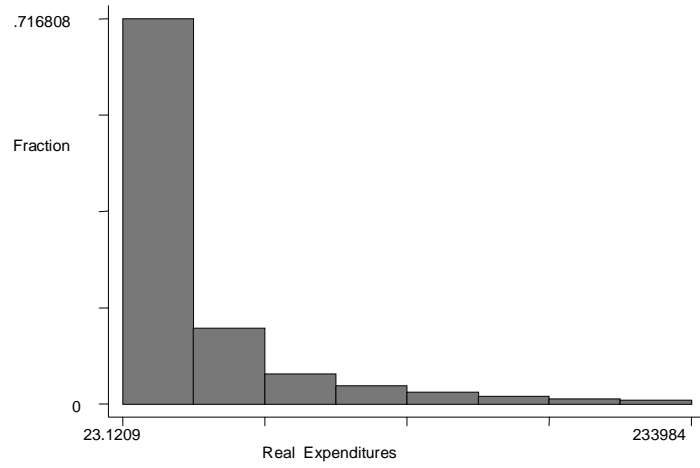**Figure 1: The Distribution of Total County Government Expenditures**

Table 1: Summary Statistics

	Mean	Standard Deviation	Minimum	Maximum	N
Capital Convictions					
Number of prisoners convicted of death sentence	0.06	0.41	0	16	34,280
Number of Years in which county had at least one death conviction	0.45	1.35	0	15	55,406
Observations with at least one death conviction	0.04	0.20	0	1	34,280
Observations where death penalty legislation in place	0.80	0.40	0	1	55,406
County Budgets (real $2000, thousands)					
Total Expenditures	66,897	294,986	23	13,536,065	39,768
Survey counties only	72,875	306,079	23	13,536,065	27,654
Non-Education Expenditures	58,142	277,386	23	13,200,000	39,762
Public Welfare	11,015	88,835	1	5,074,326	35,121
Health and Hospitals	11,357	58,330	1	2,778,790	38,104
Corrections and Judicial	5,704	34,880	1	1,750,669	37,672
Police	3,316	19,468	1	1,132,872	39,649
Highways	4,922	11,278	1	385,033	36,134
Total revenues	68,834	316,995	26	15,116,010	35,803
Survey counties only	76,576	333,780	31	15,116,010	23,687
Own sources	44,149	172,629	22	6,276,371	35,767
Property Taxes	17,432	72,155	1	3,149,595	39,877
Sales Taxes	8,473	32,811	1	702,271	20,319
Charges	10,827	41,928	1	1,718,596	39,296
Population					
All	79,023	261,756	51	9,126,131	55,350
Budget Data Present	94,000	290,555	84	9,126,131	39,747

Notes: Sample is counties from 48 continental states from 1980 to 1997.
Data reported correspond to counties with available budget data.
Capital Sentence data come from the annual NCRP, 1983 to 1997, not available for all counties.
County Spending data come from the Census and Survey of Governments, 1980 to 1997

Table 2: Capital Trials and Spending

	Level Specification				
	Total Expenditures (Non-education)	Judicial and Corrections Expenditures	Judicial and Corrections Expenditures	Judicial and Corrections Expenditures	Total Revenues (Own Sources)
	(1)	(2)	(3)	(4)	(5)
Death Penalty Conviction (in period t or t+1)	2,646 (1224)	2,410 (348)	2,290 (352)	2,312 (381)	2,568 (891)
Year Fixed Effects	yes	yes	yes	yes	yes
County Fixed Effects	yes	yes	yes	yes	yes
Other Covariates	yes	yes	yes	yes	yes
State-Specific Time Trends	yes	yes	no	yes	yes
Death Penalty Regime States Only	no	no	no	yes	no
Mean of Dependent Variable	58,142	5,704	5,704	6,149	44,149
Number of Observations	38,616	36,739	36,739	28,916	34,976

Notes: Sample is counties from 48 continental states from 1980 to 1997.
Capital Sentence data come from the annual NCRP, 1983 to 1997, not available for all counties.
County Spending data come from the Census and Survey of Governments, 1980 to 1997, measured in real thousands of $2000.
Other covariates include population, population density, murder rate, and uniform crime report index.

Table 3: The Effect of Capital Trials on County Budgets

	Log Specification									
	Total Expenditures (Non-Education)	Judicial and Corrections Expenditures	Public Welfare Expenditures	Health and Hospital Expenditures	Police Expenditures	Highway Expenditures	Total Revenues (Own Sources)	Revenues from Sales Taxes	Revenues from Property Taxes	Revenues from Charges
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
All Counties										
Death Penalty Conviction	0.013	0.039	0.001	0.033	-0.031	-0.033	0.023	0.066	0.009	0.044
(in period t or t+1)	(.008)	(.017)	(.023)	(.021)	(.010)	(.012)	(.008)	(.022)	(.008)	(.018)
Year Fixed Effects	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
County Fixed Effects	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Other Covariates	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Mean of Dependent Variable	58,142	5,704	11,015	11,357	3,316	4,922	44,149	8,473	17,432	10,827
Number of Observations	38,616	36,739	34,134	37,059	38,516	35,038	34,976	20,319	38,734	38,182
Counties with Death Convictions in 3 or Fewer Years (with none in the last two years) and Population less than 100,000										
Death Penalty Conviction	0.016	0.028	-0.019	0.001	-0.013	-0.016	0.028	0.015	0.012	0.113
(in period t or t+1)	(.013)	(.024)	(.041)	(.040)	(.019)	(.027)	(.014)	(.040)	(.013)	(.036)
Year Fixed Effects	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
County Fixed Effects	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Other Covariates	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Mean Level of Dependent Variable	16,615	5,207	2,562	3,884	1,036	2,778	13,556	2,630	5,207	4,218
Number of Observations	14,468	14,549	12,641	13,755	14,484	12,866	14,382	8,781	14,778	14,778

Notes: Sample is counties from 48 continental states from 1980 to 1997.
Capital Sentence data come from the annual NCRP, 1983 to 1997, not available for all counties.
County Spending data come from the Census and Survey of Governments, 1980 to 1997, measured as the log of real $2000.
Controls include log of population, murder rate, and uniform crime report index.

Table 4: Spillovers Between Counties

	Log Specification			
	Instrument for Neighbors' Revenues is Neighbors' Capital Conviction			
	Total Revenues		Total Expenditures (Non-Education)	
	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)
Ln (Neighbors' Revenues)	0.177	0.228	0.163	0.272
	(.007)	(.027)	(.008)	(.028)
Death Penalty Conviction (in period t or t+1)	0.015	0.014	0.019	0.017
	(.007)	(.007)	(.007)	(.007)
Ln (Population)	1.198	1.151	1.168	1.065
	(.024)	(.033)	(.025)	(.035)
Year Fixed Effects	yes	yes	yes	yes
County Fixed Effects	yes	yes	yes	yes

Notes: Sample is counties from 48 continental states from 1980 to 1997.
Capital Sentence data come from the annual NCRP, 1983 to 1997, not available for all counties.
County Spending data come from the Census and Survey of Governments, 1980 to 1997,
measured in real thousands of $2000.