13. Monitoring and enforcement in federal alcohol and drug abuse block grants

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1 INTRODUCTION

Block grants to fund state mental health and substance abuse services started with President Reagan's omnibus Budget Reconciliation Act of 1981. In 1982, the Alcohol, Drug Abuse and Mental Health (ADAMH) block grants replaced ten mental health and substance abuse grant programs. Before that time, federal funds were directly allocated to specific programs for drug abuse and prevention, community mental health, alcohol treatment and rehabilitation, and other services. Being direct allocations, these funds were unrelated to the mediatory roles of the states. Then in 1982, the Budget Reconciliation Act effectively combined the fundings of various mental health and related programs, reduced them by about 20 percent, and then allocated them entirely at the state level.[1]

Block grants to the states give the states more autonomy in targeting and assessing program needs and priorities. This mechanism may allow states better use of the resources to target clients as well as to reallocate existing funding among different services. In return, the overall amount of federal funding for substance abuse and mental health services was reduced from $585 million in 1981 to $432 million in 1982 (GAO 1985). The reduction meant that states would have to be more efficient than before if the same levels of services were to be maintained. The level of funding increased gradually over time, but it did not attain the real funding level of 1981 until the late 1980s.

To ensure that ADAMH block grants are used for the intended services, block grants are subject to 'set-aside' restrictions. Set-asides require that a specified minimum percentage of a block grant must be spent on particular groups of persons (or organizations), such as services for

women. They also determine the split of the ADAMH block grant between mental health and substance abuse services. For example, after 1989, states were supposed to have 10 percent of the full ADAMH block grant earmarked for alcohol and drug abuse services for women; states could not use more than 10 percent of the grant for administration, and after 1989, this limit was further reduced to 5 percent. For fiscal years 1981 through 1988, the set-asides were based on a 'hold-harmless' criterion – the amount and distribution between mental health and substance abuse being based on the respective allocations before 1981. These set-asides restrict states' control over block grants. Nevertheless, the set-asides have been offset by various transfer provisions, allowing states some freedom to move funds across services. In summary, the proportion of the ADAMH block grants for which a state had complete control over the distribution between mental health and substance abuse services changed over the years, with a maximum of 25 percent in 1985.

Clearly, the various restrictions on states' ability to reallocate funds reflects the major concern with the use of ADAMH block grants: a state may choose not to use ADAMH block grants for substance abuse and mental health services. In fact, once a state receives the block grants, it may choose to regard the funds as general revenue. Median voter theory (Musgrave and Musgrave 1989), which posits that politicians will allocate funds to maximize the preferences of the median voter, implies that the effect of block grants on behavioral health services will be very small if the preferences of the median voter do not value such services. However, earlier researchers have discovered a so-called 'fly-paper' effect (Wyckoff 1988). That is, states have actually responded by spending funds according to their intended use. Given these contrary views, and because states' responses to block grants fundings directly affect their efficacy, it is important to assess empirically the effectiveness of this funding mechanism.

Congress began requiring enforcement of block grant spending in the late 1980s. The Anti-Drug Abuse Act of 1986 legislated that 1 percent of block grant funds be used for data collection and evaluation. Congress imposed more restrictions in 1988: 20 percent of substance abuse portion as set-asides for prevention, and at least 10 percent of the drug portion to intravenous drug treatments (Institute of Medicine 1990). By 1990, enforcement of block grants was stepped up. The Center for Substance Abuse Treatment (CSAT) was asked to enforce maintenance-of-effort provisions. By 1992 and 1993, a new block grant application or report form was phased in, and states' accountability was increased. CSAT also sent audit teams to states to monitor compliance.

This chapter considers the effect of such monitoring and enforcement

efforts using data on state spending. Earlier research has indicated that after federal enforcement of block grants was supposedly tightened in 1989, state spending on substance abuse services responded more to block grant funding. A previous paper by Jacobsen and McGuire (1996) used a post-1989 year dummy to capture the enhanced enforcement effect statistically. For this research, we collected data on technical review and waiver applications from CSAT, in an attempt to measure enforcement effects more precisely. We have used a post-1989 year dummy as well as the technical review and waiver application dummies for all states and all years for which these are applicable.

Because our new data are directly related to enforcement, our statistical analysis may be used to assess the effectiveness of these enforcement mechanisms. In addition, we collected data for years before and after the previous study. Our results are broadly consistent with earlier findings. State ADAMH spendings did respond to block grants, and this magnitude increased after 1989. Our results, however, indicate that technical reviews and waiver applications do not appear to explain states' responses. In other words, these specific enforcement mechanisms, after the step-up in enforcement has been controlled for, do not appear to lead to an increase in spending. There are two possible interpretations of these results. First, the effectiveness of general enforcement does not seem to be enhanced by the particular mechanisms under investigation. Second, states' compliance may depend more on a continuing relationship with the federal government or reputation rather than ex post auditing or inspection.

The next section contains some background information and describes our data set. The third section presents the regression results and their interpretations. We use a fixed-effects regression with interacted terms for our main model, and provide a set of specification checks. Concluding remarks are in the final section.

2   BACKGROUND AND DATA

The general background of the funding of substance abuse services by block grants to the states has been made available by earlier papers: GAO (1992), Hudson and Dubey (1985), Jacobsen and McGuire (1996), and Gamhkar and Sim (1999). Our research interest lies with the aspect of monitoring and enforcement of block grants. Therefore, we will review this particular issue only. First, federal block grants are subject to maintenance-of-effort requirements. When a state applies for a substance abuse block grant, the state governor pledges that appropriate steps will

be taken to ensure that funded block grants will be spent according to the application. As we have said in the previous section, substance abuse (SA) block grants may contain set-asides, which direct fractions of the funds to provide services for certain groups, and states comply with these set-aside requirements unless they apply for waivers.[2] We have collected data on which states have applied for a waiver during our sample period.

Second, beginning in 1985, states that received over $100 000 in federal assistance must conduct an audit. The state is responsible for having an audit done by an independent firm.[3] CSAT is notified when the audit discovers a problem but does not automatically receive a report. However, we were unable to obtain any data on such notifications. Third, technical assistance reviews may be carried out. A site visit by a CSAT contractor helps to review states' needs with respect to types of substance abuse services and programs. Maintenance of efforts are included in these reviews. We have been able to collect data on technical reviews for our data period.

Our data set extends that used in Jacobsen and McGuire (1996). The substance abuse spending data for years 1984–94 were obtained from the annual survey of state alcohol and drug agencies – the State Alcohol and Drug Abuse Profile (SADAP), collected by the National Association of State Alcohol and Drug Abuse Directors (NASADAD) under contract to the National Institute on Drug Abuse (NIDA). Data of the ADAMH block grant allocations for substance abuse were obtained from the Substance Abuse and Mental Health Services Administration (SAMHSA).

All spending data are expressed in real terms, in constant 1988 dollars, using the US consumer price index as the deflator, and on a per capita basis. State income and population data were collected from the Statistical Abstract of the US. Additional data on state waiver application and approval over the time period and the data on phase one technical review are collected from CSAT.[4]

We use five main independent variables. Income refers to state per capita income, also in constant 1988 dollars. SABG is the substance abuse block grants. The next three variables are different measures of monitoring and enforcement of block grant administrations. Each of these variables has been interacted with the SABG variable.[5] The Post89 variable, a dummy that takes the value of 1 for years after 1989, simply captures the fact that enforcement has been stepped up since 1989. The 0–1 dummy variable that is used to create the Waiver variable takes a value of 1 when a state applies for a waiver. Similarly, for the TechRev variable, the corresponding 0–1 dummy takes a value of 1 when a technical review is carried out. Table 13.1 contains the definitions of the independent variables.[6] The regression analysis will also employ state and year fixed effects.

Table 13.1 Definitions and summary statistics of variables

Variables	Description
Dependent variable	Real state per capita SA expenditure
Income	Real state per capita income
SABG	Real state per capita SA revenue from ADAMH block grant
Waiver	Equals 1 if waiver in state i year j, interacted with SABG
TechRev	Equals 1 if technical review in state i year j, interacted with SABG
Post89	Equals 1 for years after 1989, interacted with SABG

	Observations	Mean	Std. dev.	Minimum	Maximum
Dependent variable	550	9.363	8.887	0.593	62.410
Income	550	1.578	1.076	0.153	8.381
SABG	561	2.017	1.136	0.371	9.899

Note: Means and standard deviations are statistics over all states and years. Dependent variable and income data are missing for the following states: Wyoming, 7 years; New Mexico, 1 year; District of Columbia, 1 year; Oregon, 2 years.

For six states, we graph the state total per capita substance abuse expenditures, and their shares of SA block grant in total SA expenditures; see Figure 13.1. Clearly, over time both per capita total substance abuse spendings as well as the percentages of block grants in total spendings change. These also vary according to the states, although they are seldom more than 40 percent. Some summary statistics of the enforcement variables are presented in Tables 13.2 and 13.3. Since 1989, only a minority of states have applied for waivers, although the majority of these waiver applications were accepted. On the other hand, the number of technical reviews on states since 1992 tends to be higher. For example in 1993, 40 percent of the states underwent technical reviews. There seems to be a downward trend in the number of states' waiver applications, but this does not appear to apply to the number of technical reviews during the four years (within our data set) for which they were applicable. Overall, we do observe some variations in the enforcement data that we analyze.
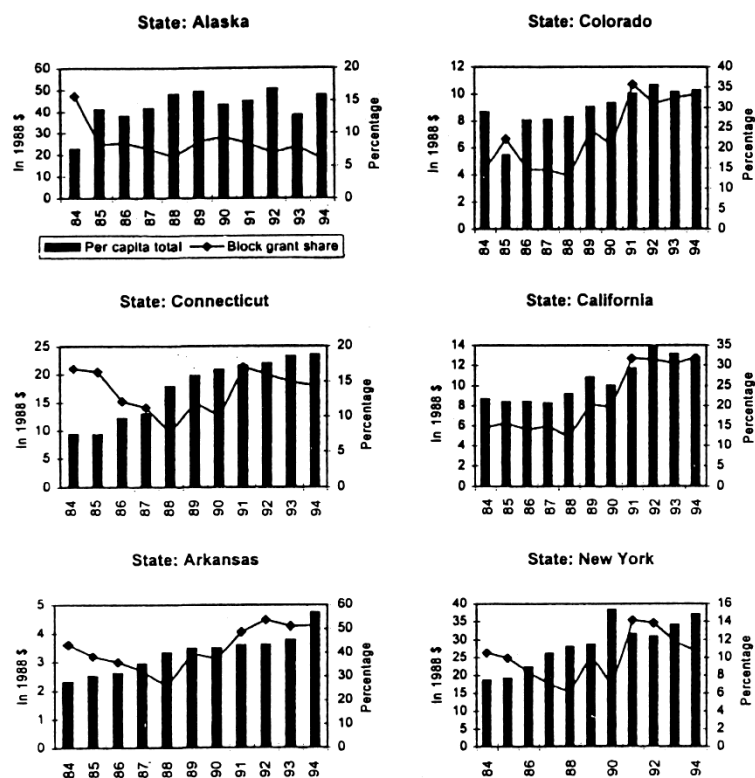
Figure 13.1 State substance abuse expenditure and the share of block grants for selected states

Table 13.2 Summary of state waiver applications

Fiscal year	Number of states applied	Approved	Rejected
1989	6	4	2
1990	8	7	1
1991	6	6	0
1992	5	5	0
1993	0	0	0
1994	2	2	0

Table 13.3 Summary of technical reviews

Fiscal Year	Number of states implemented
1992	11
1993	20
1994	5
1995	10

3   HYPOTHESES AND STATISTICAL RESULTS

Previously, researchers have confronted significant methodological problems when assessing the effect of block grants and state substance abuse spendings. The usual method of using cross-sectional data to estimate the effect of interest suffers from potential bias due to omitted variables. Very often only a few of the many factors influencing state spending can be included in a cross-sectional regression. It is by now well recognized that the omitted variable bias is empirically serious.[7] For our model, the variable measuring block grant funds may be correlated with omitted variables that also influence expenditure patterns (such as 'need').

Following Jacobsen and McGuire (1996), we use a 'fixed-effects model' to identify the regulatory effects of block grants on state SA expenditures. The fixed-effects approach enables us to control for many of these influences on state substance abuse spending without explicitly modelling them. Our data set spans a period of 11 years. This allows us to control for unspecified factors which influence substance abuse spending

Table 13.4 Regression results for state total spending on substance abuse, 1984–94

Variables	Eqn. 1	Eqn. 2	Eqn. 3	Eqn. 4	Eqn. 5
Income	−0.00004 (0.00029)	−0.00010 (0.00029)	−0.00004 (0.00029)	−0.00004 (0.00029)	−0.00010 (0.00029)
SABG	0.6598 (0.2426)	−0.0444 (0.2801)	0.6518 (0.2425)	0.6641 (0.2435)	−0.0429 (0.2804)
Post89		1.5554 (0.3294)			1.5829 (0.3335)
Waiver			−0.3181 (0.3500)		−0.2243 (0.3440)
TechRev				−0.0360 (0.1837)	−0.1637 (0.1818)
N	550	550	550	550	550
Overall-R^2	0.102	0.075	0.104	0.102	0.076

Note: Standard errors are in parentheses under each estimate.

and which are similar across observations of the same state. Each regression in this chapter includes a dummy variable for each state; the estimated coefficients on these variables represent the state fixed effects. Similarly, because we have many states in the data, we can include a fixed effect for each year. Given the fixed-effects variables, regression estimates of the effects of independent variables are identified by the changes in those variables. So the estimates measure how an increase or decrease in block grant funds affects changes in state substance abuse spending.

We estimate a number of regressions with the state agency substance abuse spending per capita in constant 1988 dollars as the dependent variable. The full model includes a number of independent variables. Common in all five regressions are states' real per capita incomes and real per capita SA block grants. Our full model makes use of all the independent variables (Table 13.4, equation 5). Each of the other four regression equations contains the per capita state income and block grant variables; these are the only variables for the benchmark model (Table 13.4, equation 1). Then the enforcement interaction variables are alternatively added to the benchmark model (equations 2–4). Table 13.4 presents the regression results.

Earlier research by Jacobsen and McGuire (1996) using a subset of the data here (for the years between 1987 and 1992) found that states did respond to increased SA block grants by increasing SA spending, and that this response was stronger after 1989, when federal enforcement was stepped up. Recall that the enforcement variables (Post89, Waiver and TechRev) are all interacted with the SABG variable. To obtain the full effect of a change in SA block grant on SA spending (the dependent variable), then the coefficient of the SABG variable should be added to those of the enforcement variables where relevant. For example, in equation 5 (Table 13.4) a $1 increase in SA block grant is estimated to lead to $ (−0.0429 + 1.5829 − 0.2243 − 0.1637) = $1.152 increase in SA spending. We have performed a partial F test on the null hypothesis that the sum of the coefficients of SABG, Post89, Waiver and TechRev is equal to one. The F-statistic (with 1 and 484 degrees of freedom) turns out to be 0.09 and the *p* value is 0.7589. Therefore, we cannot reject the null hypothesis. In fact, we have also computed the asymptotic confidence interval, and it is [0.16, 2.14].

Equation 1 in Table 13.4 can be interpreted as a test of the median voter theory. Median voter theory asserts that block grants are just like general revenue, and predicts the regression coefficients of the income and block grant variables to be identical. If an SA block grant is regarded as general revenue, then an increase in SABG should not produce an increase in SA spending different from an increase in Income, and this is clearly rejected by the result in equation 1. In equation 1 (Table 13.4), the estimate of 0.6598 on the SABG variable should be interpreted as the 'average' response of SA expenditure to SA block grants, where the averaging is meant to apply over all states and all years in the sample period. We see that in both equations 2 and 5 in Table 13.4, the Post89 variable has a strongly positive estimate; this is consistent with earlier findings in Jacobsen and McGuire. From equation 2, we conclude that the block grants do have a significant effect on states' SA spendings, and this effect is stronger after 1989, when enforcement was supposed to have strengthened. Equation 5 reinforces this result: with all enforcement variables included, the estimated coefficient of the Post89 variable remains highly positive and significant. Equations 3 and 4 in the table omit the Post89 variable; instead each of these includes a more specific enforcement variable. In equation 3, the Waiver variable is used. The SABG estimates in both equations are similar to that in equation 1. Neither of the two enforcement variables is statistically significant. Our interpretation is that the specific enforcement information that we are able to gather does not appear to strengthen the compliance of SA block grants. Equation 5 again reinforces this point: there both the estimates of the

Table 13.5 Estimated within-state correlation matrix

Years	1984	1985	1986	1987	1988	1989	1990	1991	1992	1993
1984	1.0000									
1985	0.3544	1.0000								
1986	0.3782	0.6365	1.0000							
1987	0.4020	0.6917	0.7657	1.0000						
1988	0.4308	0.7992	0.8882	1.0000	1.0000					
1989	0.4377	0.7957	0.8870	1.0000	1.0000	1.0000				
1990	0.4523	0.8079	0.9378	1.0000	1.0000	1.0000	1.0000			
1991	0.4232	0.7673	0.8704	0.9652	1.0000	1.0000	1.0000	1.0000		
1992	0.4042	0.7663	0.8365	0.9245	1.0000	1.0000	1.0000	1.0000	1.0000	
1993	0.2517	0.4442	0.4724	0.5722	0.6594	0.7125	0.6296	0.6312	0.6813	1.0000
1994	0.3915	0.7387	0.8123	0.9344	1.0000	1.0000	1.0000	1.0000	1.0000	0.7609

Waiver and TechRev variables are negative and insignifcant, as in equations 3 and 4, and the Post89 variable captures most of the effect of block grants.

We perform a number of robustness tests on our regression analysis. We first address the issue of serial correlation and heteroskedasticity. We now discuss the methods and explain their results. It has been argued by Gamkhar and Sim (1999) that serial correlation could be a potential problem with the analysis of block grant spendings. To investigate, we begin with the decomposition of the error term of the basic regression: $\epsilon_{it} = \upsilon_i + \eta_t + \varepsilon_{it}$, where i is an index for the states, and t is an index for the years. In other words, the error term ϵ_{it} consists of the state-specific component, υ_i, time-specific component, η_t, as well as the residual ε_{it}. We first omit the time component, and use maxmium likelihood to estimate the correlation matrix of the state-specific component together with the residuals. This correlation matrix is reported in Table 13.5.

The result in Table 13.5 suggests that the correlation does not tend to decline over time. We further check the ratio of the variances of the state-specific components and the residuals. The value of this ratio is high, about 2.46. This indicates that variance of the state-specific component dominates that of the residual. We therefore conclude that serial correlation is unimportant. In other words, the contribution by the state-specific component, υ_i, to the error term, ϵ_{it}, is much stronger than the residual ε_{it}.

Table 13.6 Specification checks for serial correlation and heteroskedasticity

Variables	Reg 1	Reg 2	Reg 3	Reg 4	Reg 5	Reg 6
Income	0.0006 (0.0002)	0.0008 (0.0002)	0.0010 (0.0002)	0.0007 (0.0002)	0.0008 (0.0003)	−0.0001 (0.0006)
SABG	0.5388 (0.3627)	0.1240 (0.2451)	0.3338 (0.2531)	0.0440 (0.1947)	0.1240 (0.2717)	−0.0444 (0.3649)
Post89	0.5643 (0.2476)	0.7861 (0.1648)	0.5978 (0.1671)	0.7885 (0.1455)	0.7861 (0.2152)	1.5554 (0.5504)
	unstructured error term	state-fixed effects	random effects	Reg 2 + AR(1)	Reg 2 + heteroskedasticity	Reg 5 + year dum
N	550	550	550	550	550	550

We further perform a number of specification checks. Estimation with an unstructured error term is reported in regression 1 in Table 13.6. Regression 2 in Table 13.6 presents the basic equation with only state fixed effects, whereas regression 3 presents the random effects model; the estimates differ somewhat. In the next regression, we report the results when an AR(1) process is assumed for the residual. Consistent with our previous analysis that serial correlation is unimportant, estimates in regression 4 are very similar to those in regression 2. Correcting for heteroskedasticity does not change the results. Regression 5 adds the heteroskedasticity correction but its estimates are the same as those in regression 2. Finally, regression 6 also adds the year fixed effects, and the estimates are similar to those in equation 2 of Table 13.4. Although the estimates in Table 13.6 change somewhat according to the statistical assumptions used, the result that in the post-1989 period, block grants are associated with about a dollar-for-dollar increase in spending emerges for all specifications.

4   CONCLUSION

Our results suggest that the precise way that enforcement affects ADAMH block grant spendings may not be directly related to the waiver and review monitoring that CSAT carried out. Rather, we hypothesize that the *threat* of rigorous enforcement may have been at work. Given that ADAMH block grants typically make up only a small percentage of a state's budget, perhaps it is not worthwhile for a state to acquire a bad

reputation by using block grant funds inappropriately. A bad reputation in ADAMH block grant spendings may affect a state's ability to apply successfully for other funds. In other words, the benefit of 'cheating' may be too small and any potential penalties a state may experience in its ability to secure future funding may outweigh the gain.

Policy implications of our results are several. First, the use of waiver and review monitoring in substance abuse block grants may be regarded more as information gathering or assistance to states rather than explicit methods of reinforcement. While our results do not show that these particular enforcements have significant effects on substance abuse spendings originating from block grants, we do not want to imply that they are not worthwhile. These activities may well be useful to help states plan the ways they intend to use the block grant fundings. When states apply for waivers and are subject to reviews, it is expected that the effort that is being devoted to planning is increased. Second, we do not want to imply that results in our chapter can be extrapolated to other block grants. This is because the shares of substance abuse block grants in state budgets are typically very small. Again, our hypothesis of a reputation effect – that explicit enforcement may not be as powerful as a general 'announcement' effect – may work well for block grants that are relatively small. Third, our results do tend to support the efficacy of block grants as a means to fund substance abuse services provided by the states. The positive effect of block grants fundings on actual substance abuse spendings is actually quite robust: the earlier results in Jacobsen and McGuire (1996) are confirmed again in this analysis, which uses a larger data set and more stringent robustness checks.

NOTES

1   More background about the ADAMH block grants can be found in Jacobsen and McGuire (1996).

2   In unusual economic circumstances, states may be automatically granted a

waiver; for example, in a 'financial crisis' in which the total tax revenue declines at least 1.5 percent, and either unemployment increases by at least 1 percentage point or employment declines by at least 1.5 percentage points. See Federal Register Section 45 CFR Subtitle A, Part 96 – Block Grants; subpart L on 'Substance Abuse Prevent and Treatment Block Grant'.

3   Neither the state nor the federal government actually conducts the audit.

4   We thank James Sayers and Judith Uriyu from CSAT for kindly providing the waiver and technical review data.

5   Interaction is obtained by multiplying variables together.

6   Jacobsen and McGuire (1996) also used a block grant share variable in similar regressions. This variable was the ratio between the SA block grants and the state budget in 1988 and interacted with SABG; it measures the importance of SA block grants as a source of revenue. We have used some similar 'share' variables but discovered that regression results were very sensitive depending on the definitions. We suspect that an endogeneity problem led to this lack of robustness and have decided not to use it.

7   Blank et al. (1994) in a different context, found that model identification using changes over time within states gives different estimates from those obtained using only cross-sectional variations.

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