

Finders keepers: Forfeiture laws, policing incentives, and local budgets [☆]

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Abstract

To encourage anti-drug policing, the federal government and many state governments have enacted laws that allow police agencies to keep a substantial fraction of the assets that they seize in drug arrests. We use rich new data on police seizures and local spending to explore the reactions of both governments and police to the incentives created by these policies. We find that local governments offset police seizures by reducing their other allocations to police, partially undermining the incentives laid out in statute and diverting the earmarked funds to other purposes. Police, in turn, respond to the real net incentives for seizures, once local offsets are taken into account, by increasing the drug arrest rate. Heroin prices also increase, suggesting that the increased emphasis on anti-drug policing raises the supply costs of illicit drugs. These findings highlight both the promise and pitfalls of using financial incentives to solve agency problems in a federal system: both local agents and intervening governments have sophisticated responses to financial incentives, and these responses must be taken into account in both designing effective policies and evaluating their consequences.

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1. Introduction

Agency problems are as common in the provision of public goods as they are in private markets. Local school boards may try to undo the effects of state-level school finance reforms (Baicker and Gordon, 2006). Bureaucrats may have private agendas at odds with those of their constituents (Brennan and Buchanan, 1977). Teachers may resist the curriculum dictated by the school board, while welfare caseworkers may not strictly enforce the eligibility criteria included in welfare reform. In some circumstances, perfect contracts or laws can be written to solve these agency problems and competitive contracts for private production of public goods can improve efficiency (Hart et al., 1997). When perfect contracts or laws are not possible (such as when the agents' actions are unobservable or multi-year commitments cannot be made), financial incentives may be used to induce the desired behavior. This strategy is more common in the private sector, but monetary incentives are increasingly common in the provision of public goods — with performance bonuses for individual employees such as teachers or public health workers or system-wide performance incentives for institutions such as schools or hospitals.¹

Because of the many levels of government involved, “asset forfeiture” laws provide a particularly informative example of the implications of using financial incentives to influence the provision of public goods in a federal system. In an effort to induce police to do more anti-drug policing, both the federal government and many state governments introduced laws in the 1980s that allow law enforcement agencies to keep a substantial fraction of the assets that they seize in drug arrests. This drug-related asset forfeiture is a source of considerable controversy, as the legal hurdles for forfeiture are often lower than for criminal conviction and those subject to seizures can find it difficult to recover their property, even when they are found not guilty of related criminal charges.² Many claim (and our data confirm) that forfeitures have become a major revenue source for some local police and prosecutors, giving them an incentive not just to deter crime but also to raise funds.

The potential response of localities to these laws highlights a fundamental problem in the use of financial incentives to solve agency problems in the provision of public goods in a federal system: different levels of government may have competing goals and constraints. Federal and state governments introduced forfeiture incentives to increase anti-drug policing, but counties can undermine those incentives by adjusting their allocations to police. In fact, some states' laws explicitly acknowledge that local governments could (but should not!) reduce their police allocations in response to seizures.³ Similar agency problems arise in a range of other programs. For example, Baicker and Staiger (2005) show that federal health care funds targeted to poor hospitals are often expropriated by state governments for other uses when the opportunity arises and Duggan (2000) finds that state funds channeled to county and district hospitals treating a

¹ See Ballou (2001) and Hoxby (2002) for discussion of performance-based pay in public schools and Smith and York (2004) and Rosenthal et al. (2004) for a discussion of its use in various public health care settings. Examples of system-wide pay for performance include the “No Child Left Behind” Bonus Fund, which rewards schools for closing their achievement gap, and the “Physician Group Practice” demonstration, which will award bonuses for cost-effective care of Medicare beneficiaries.

² Although this is a common criticism of forfeiture laws, this outcome is consistent with the laws' intent. Law enforcement officials maintain that asset forfeiture is a powerful tool that allows them to “disrupt the ‘working capital’ of criminal organizations” (Stellwagen, 1985) and “take the profit out of crime” (Cassella, 1997), thereby deterring future drug crimes as well as punishing current criminals.

³ Michigan's law, for example, includes a provision stating that proceeds from asset forfeiture should not supplant funds normally provided to police by counties. As is the case for TANF and Medicaid, however, such Maintenance of Effort requirements are difficult to enforce when spending is generally increasing in nominal dollars.

“disproportionate share” of publicly insured patients are largely offset by reductions in local spending. Similarly, [Gordon \(2004\)](#) finds that increases in federal spending on low-income school districts are offset by reductions in local spending.

In this paper, we use new and original data on drug-related seizures combined with detailed data on county budgets and arrests to explore the effect of asset forfeiture laws on the behavior of both local governments and law enforcement. We first assess whether asset forfeiture laws really increase law enforcement budgets, or whether local governments undermine those incentives with offsetting changes in police budgets. We also consider whether local governments are more likely to offset seizures in some circumstances (such as when under fiscal distress) than others. We find that the relationship between police seizures and local allocations to the police budget is more complex than a naïve interpretation of forfeiture statutes would suggest. Local governments partially offset police seizures by reducing their spending on police the following year. When faced with a budget deficit, counties reduce their allocations in response to seizures much more, and allocate those funds to public welfare programs. Thus, our results suggest that local governments partially undermine the incentives created by state and federal forfeiture laws by diverting seizure revenues for other uses.⁴

In the second part of the paper, we analyze how police change the level and composition of arrests in response to the *de jure* incentives created by forfeiture laws and the *de facto* incentives left in place after county offsetting behavior is taken into account. By distinguishing between *de jure* and *de facto* sharing rules, our analysis refines previous studies of asset forfeiture policies (such as [Benson et al., 1995](#); [Mast et al., 2000](#)) and presents a more complete picture of responses to this complex set of incentives. We find that when law enforcement agencies get to keep more of the assets they seize, they respond by devoting substantially more of their effort to anti-drug policing and away from other petty crimes. This results in not only an increase in drug arrests, and specifically a category of arrests that includes heroin, but also an increase in the price of heroin. In contrast, we find no evidence that these incentives affect overall arrest rates for marijuana, a larger but less lucrative market. This shift in policing towards heroin arrests relative to marijuana arrests suggests that we may be capturing real incentive effects of the law, rather than a coincident increase in overall drug demand. Thus, forfeiture laws seem to be effective in changing public agents’ behavior, albeit with substantial diversion of resources by localities.

2. Background on forfeiture

Private assets connected, sometimes quite loosely, with drug possession or trafficking offenses can be seized through both state and federal “asset forfeiture” laws. These civil asset forfeiture laws are commonly traced back to the medieval English “law of deodands,” with both premised on the “guilt” of the object involved in the crime, irrespective of the guilt or innocence of its owner ([Levy, 1996](#)). Although in principle forfeiture laws can cover a wide range of offenses, in practice most are drug-related. The seized assets, whether real property, cash, or other financial instruments, may be processed through the Department of Justice or a state program. Some portion of these assets, depending on the specific law used, is then returned to the local law enforcement agency.

⁴ As we discuss below, these results could also capture increased seizures by police in response to anticipated budget shortfalls. We take three approaches to disentangling this potential reverse causality, all of which point to the relative importance of county offsets of seizures, rather than police “fundraising” in response to budget shortfalls.

2.1. Federal asset forfeiture law

Federal drug-related civil forfeiture law dates back to the Comprehensive Drug Abuse Prevention and Control Act of 1970. That law limited forfeiture to illicit drugs, manufacturing and storage equipment, and the conveyances used to transport the drugs. Starting in the late 1970s, the federal government expanded law enforcement's forfeiture authority, enabling them to seize cash and later to seize not only the property used in the commission of a drug crime but also that equal in value to "forfeitable assets that are no longer available" (Blumenson and Nilsen, 1998a,b, p. 45).⁵ In 1984, with the passage of the Comprehensive Crime Control Act, the federal government established its most controversial forfeiture policy, an "equitable sharing" provision, whereby state and local agencies could request that the Department of Justice (DOJ) "adopt" and then share a drug-related asset seizure. The explicit motivation for the DOJ-sharing provision was to provide law enforcement at all levels with an incentive to pursue drug crimes. State and local agencies can request federal adoption if (1) a federal agency was involved in the seizure or (2) the seizure was made pursuant to the commission of a federal crime that provides for seizure, as is the case with any drug offense. After the seizure is "adopted" by DOJ, it can return up to 80% (90% before 1990) of the proceeds back to the seizing agency. DOJ does, however, set minimums on the value of seizures it will adopt, including \$20,000 for real property and \$5000 for cash or cars.

2.2. State asset forfeiture laws

DOJ's "success" in seizing assets in the early 1980s and its introduction of the equitable sharing program brought asset forfeiture policies to the attention of state governments. Many states responded by passing their own forfeiture laws or by tapping in to existing laws on the books.⁶ Currently, every state has a drug-related civil forfeiture law. These laws apply only to seizures processed by state agencies. If a state agency has the DOJ adopt its seizure then federal sharing provisions apply.

State laws vary widely in the fraction of seizures returned to the local agency, the way different types of property are treated, and restrictions on the use of funds. While many states return the bulk of funds to the seizing agency, others contribute them to a general law enforcement fund, earmark them for specific uses, or pool them with general revenues. A few states have specific constitutional provisions requiring seized assets be devoted to education (e.g. Indiana and Missouri). Several others have recently limited the fraction of seizures that police can keep (e.g. Nevada) or outlawed forfeiture without a criminal conviction (e.g. Oregon). These restrictions are in part a response to reports of abuses, which abound in both the popular press (e.g. Dillon, 2000) and the academic literature (Blumenson and Nilsen, 1998a,b).⁷

Despite the specific provisions laid out in statute, both federal and state agencies can exercise significant discretion in determining sharing. Local agencies themselves typically choose whether to process a seizure through state authority or to have it adopted by DOJ. One would assume that local agencies would always choose the route with the more generous statutory sharing provision (which varies according to the provisions and timing of state laws). Conversations with specific

⁵ Under federal law, assets can be forfeited in three ways: administratively, in a civil proceeding, or in a criminal proceeding.

⁶ See Blumenson and Nilsen (1998a,b) for a thorough overview of state laws.

⁷ See Blumenson and Nilsen (2001) and Di Eduardo (2001) for a discussion of recent state reforms and Short (2002) for information on a recent federal reform, The Civil Asset Forfeiture Reform Act (CAFRA) of 2000.

state agencies, however, suggest that DOJ often makes “deals” with local agencies to adopt seizures, and that state agencies have the discretion to negotiate with local agencies. However, while agencies have some latitude to bargain, their bargaining power is likely to be affected by the statutory sharing rule.

2.3. Reactions to forfeiture laws

Past work demonstrates that police respond to monetary incentives in ways predicted by theory, such as by reducing their work effort when they are paid below their reference point (Mas, 2006). Several studies have directly examined the effect of forfeiture laws on policing, with the prevailing claim being that police respond to incentives by seizing more. Most do not adequately control for policy endogeneity or draw inferences about the broader budgetary implications of forfeiture policies. Benson et al. (1995) find a positive correlation between police seizures and police expenditures in a cross-section of some Florida agencies, but the causal connection is not clear. Mast et al. (2000) find a positive correlation between statutory sharing rules and drug arrests as a fraction of total arrests in large cities. They have no data on the magnitude of seizures, so must examine instead state–year level variation in the fraction of seized assets police retain by statute.

As these studies confirm, law enforcement clearly has the opportunity to increase their budgets through drug-related forfeitures. The federal government once touted such “benefits” of forfeiture (see Stellwagen and Wylie, 1985), and opinion data suggest law enforcement understood the message. Worrall (2001) surveyed law enforcement executives and found that 30 to 45% of them agreed that “forfeiture is necessary as a budgetary supplement.”

At the same time, anecdotal evidence suggests that the relationship between asset forfeiture and public budgets is complicated by the ability of local governments to offset some of what police seize. For example, a former San Jose police chief recalled one year when his department’s new budget had no funds for equipment; when the chief asked the city manager why “he kind of waved his hand dismissively and said ‘Well, you guys seized \$4 million last year’” (Dillon, 2000). We use data on actual seizures to assess quantitatively the relationship between seizures and police budgets. We next present a framework for understanding how federal and state forfeiture policies might affect both the budgetary decisions of county governments and the law enforcement activity of local police.

3. Conceptual framework and empirical strategy

The choices faced by states, counties, and police agencies can be thought of as a repeated principal–agent problem with uncertainty. The police, the agents, seek to maximize some function of their effort, which they dislike, and their budget, which they want to increase (perhaps because they care about the crime reduction their budget finances, because of Leviathan motivation, because their job is more pleasant with greater resources, or because they benefit directly from higher salaries or perks (see Gordon and Wilson, 1999)). The county, the principal, seeks to maximize some function of the public goods that it funds, such as reductions in crime, education, highways, and the like, subject to a budget constraint that includes revenues from seizures (and other sources).

In each period, the police first choose how much (costly) anti-drug policing effort to exert. This effort results in a seizure amount (which might be determined entirely by effort, or might be the product of imperfectly observed effort and noise). The county then chooses how much money to allocate to policing — implicitly choosing how much of the seized funds to leave with the police,

rather than expropriate for county budgets through reduced police allocations.⁸ If police seizures were exogenous, the county would treat its share of the seizure proceeds as unconstrained income, and would allocate it accordingly (with presumably only a small share to policing). Because police value their own resources and their (costly) effort affects seizures, however, the county is motivated to leave seizure incentives in place. Thus, changes in county allocations to police should not completely offset the marginal seizures made by police.

We can derive some comparative statics by looking at simpler cases within this broader framework. For example, consider the case where counties are simply trying to maximize their seizure revenue. If S is the amount police seize, which is a function of T , the implicit tax rate (the amount that the county expropriates through reduced allocation to the police when the police seize more), then the county is trying to set T to maximize Revenue = $T * S(T)$. As the tax rate rises, seizures will decline. Thus, the county will pick an implicit tax rate that maximizes revenues, taking this response into account (much like other optimal tax problems).

In a more complicated world, the county might also value the seizure effort exerted by the police beyond its effect on the county's budget — if, for example, it values the resulting reduction in crime directly. In this case, the county might even further subsidize this kind of policing by allowing the police to keep a greater fraction of the proceeds than laid out in statute. Alternatively, if different police agencies have different opportunities to seize (known to the police but unobserved by the county), the county will choose a tax schedule that balances the distortions induced in effort (where high-opportunity police departments may masquerade as low-opportunity ones) against its revenues.⁹ Furthermore, the cost of raising outside funds relative to funds from seizures may vary. If the cost to the county of raising revenue from other sources increases in times of fiscal distress (and if its preferences for spending are convex), then during those times it will value revenues from seizures more highly. Institutional constraints may also affect the degree to which counties can actually expropriate police seizures, making the statutory sharing rate (rather than the de facto sharing implied by county offsets of police budgets) the binding constraint. For example, the substantial “flypaper effect” literature suggests that it may be costly for governments to reallocate funds between budget categories, making initial budget allocations and grants less fungible than they would appear.¹⁰

These extensions complicate the relationships among statutory sharing, county offsets, police effort, and seizures and make them difficult to sign.¹¹ We take an empirical approach to estimating county reactions to police seizures and police responses to the implied incentives. While our analysis cannot speak to the efficiency implications of such offsetting behavior (without putting normative weights on the preferences of local versus state versus federal voters), it can shed light generally on the implications for using financial incentives in a federal setting and specifically on the impact of real incentives, net of any offsetting, on policing.

⁸ As noted below, county allocations to police are sufficiently large that budgetary authorities could offset the full amount of police seizures through reductions if they chose.

⁹ Analogous to the Mirrlees (1971) income tax problem in a world of heterogeneous unobserved ability, the optimal tax schedule may be non-linear.

¹⁰ See Hines and Thaler (1995) for an overview and Knight (2002) for alternate explanations.

¹¹ Although the intent of forfeiture policies is to encourage anti-drug policing, in principle, these incentives could reduce enforcement if the income effect generated by seizures swamps the substitution effect: since police are made “richer” by forfeiture policies at any level of drug arrests, they may choose to expend less effort on drug enforcement. Moreover, it is not clear what the seizure revenue-maximizing level of drug arrests actually is, let alone how effort, revenues, and crime reduction enter into the police utility function.

3.1. County offsets of police seizures

We first examine the net effect of seizures made by local law enforcement agencies on their budgets. Although many forfeiture laws were written so as to provide a “windfall” to law enforcement (Stellwagen and Wylie, 1985), this intent can be effectively undone if a parent/county government reduces its own police spending in response. We thus estimate:

$$\text{Police Budget}_{ist} = \alpha_{is} + \alpha_t + \delta_s t + \beta_1 \text{Seizures}_{is,t-1} + X_{ist} \Gamma + \varepsilon_{ist} \quad (1)$$

where i indexes counties, s indexes states, and t indexes time. We use lagged seizures as our main independent variable in an attempt to isolate the causal effect of seizures on budgets.¹² Because data on seizures made through state statutes are quite sparse, our main analysis considers seizures made through the DOJ. As a supplement to our primary analysis, however, we consider seizures made through programs in five states for which we have gathered data: Arizona, California, Florida, New York, and Pennsylvania.

In all models, we include county and year fixed effects as well as linear state-specific time trends. We also control for annual county unemployment rates and the arrest rate for serious (“index”) crimes to capture the opportunity for seizures and the overall law enforcement environment. Police budgets and seizures are expressed in real per capita terms. All regressions are weighted by county population (as the data are noisier for smaller counties). We discuss the sensitivity of our estimates to these choices below. Finally, to correct for serial correlation in the error terms, we allow for an arbitrary correlation at the state level (Bertrand et al., 2004).

Since the seizures police make are not included in the budgets they receive from their parent county governments, β_1 captures the degree to which counties change police budgets in response to seizures. If counties do not respond to increased seizures by changing their budgetary allocations to police we would expect β_1 to be zero. Counties might also reduce their allocations to police by a lump sum based on expected or potential seizures, however, rather than as a fraction of the amount that police actually seize. Although this would achieve the same diversion of funds by the county for other uses, it would have a very different effect on police incentives at the margin. We test for this possibility by limiting our sample to those counties with positive seizures, controlling for previous seizures, and examining county reactions to “unexpected” police seizures, described in more detail below.

We are also interested in the heterogeneity of offsetting behavior. To examine differential responses to local seizures, we include the interaction of seizures with local deficits (measured as non-police spending minus revenues):

$$\text{Police Budget}_{ist} = \alpha_{is} + \alpha_t + \delta_s t + \beta_1 \text{Seizures}_{is,t-1} + \beta_2 \text{Seizures} * \text{Deficit}_{is,t-1} + X_{ist} \Gamma + \varepsilon_{ist} \quad (2)$$

To the extent that counties reduce their allocations to police, they have extra funds to spend on other programs or to reduce tax revenues. To explore the use of these funds, we include other spending categories as alternate dependent variables.

¹² This specification implies that police agencies receive their share the year after the seizure. While Federal documents indicate a lag between seizures and disbursements, we are unsure of the precise timing (Office of the Attorney General, 1994). A population-weighted regression of per capita DOJ disbursements on per capita DOJ deposits and the one and two year lags of these deposits suggests that our assumption of a one year lag may not be far off: the coefficient is largest on and only statistically significant for the one-year lag of deposits.

Clearly, causality could run in the other direction: police may alter their seizure activity in response to changes in their budget. We take several approaches to disentangling this reverse causality. First, throughout the paper, we use the timing of seizures and budget allocations, observing how lagged seizures affect current allocations. We also use linear state-specific time trends to control for any smooth pre-existing trends in budgetary allocations to police that are correlated with the timing of forfeiture law adoption and asset seizures. While the timing of these reactions helps give some insight into causal pathways, the persistence and potential non-linearity of both policing patterns and county budgets make it difficult to rely on timing alone to determine causality in a simultaneous-equation framework.

To further test the causality, we examine seizure “windfalls,” defined as seizures that are more than one standard deviation away from the mean seizure level in the county, and “unexpected” seizures, defined as the residual or unexplained portion of seizures from a regression of seizures in period $t-1$ on seizures in period $t-2$, county and year fixed effects, and the same covariates described above. “Windfalls” and “unexpected” seizures are likely to surprise both the county government and the police, and thus enable us to better capture exogenous variation in seizures. We estimate the response of county spending to these surprises. This specification also allows us to evaluate whether counties respond to fluctuations in actual police seizures incrementally, rather than through lump-sum reductions based on perceived seizure opportunities.

Lastly, we use the mayoral election cycle (following Levitt, 1997; McCrary, 2002) as an instrument for police budgets to estimate the responsiveness of seizures to (exogenous) changes in police budgets. We can use those estimates to put a bound on the extent to which any estimates of county “offsets” actually capture the reaction of police to anticipated budgetary changes. This approach relies, however, on the difficult to test assumption that election years affect seizures only through police spending and is thus more suggestive than conclusive.

3.2. Police responses to (net) seizure incentives

The second half of this paper evaluates the responsiveness of police to forfeiture laws. We first consider the gross incentives created by state laws (which, unlike DOJ policy, vary across states and over time) to see whether police seize more and focus more on anti-drug policing when the statutory sharing rate is higher. To capture relative police effort, we estimate:

$$\text{Policing Behavior}_{ist} = \alpha_{is} + \alpha_t + \delta_s t + \beta_1 \text{Sharing}_{st} + X_{ist} \Gamma + \varepsilon_{ist} \quad (3)$$

where policing behavior includes the arrest rate and the composition of those arrests. We begin with a simple coding of the statutory sharing provisions in effect in each year. Because many police resources are fungible, we treat statutory sharing percentages with and without restrictions on the use of the proceeds in the same way. Thus, for example, we do not change the coding of Arizona’s sharing percentage after 1988, when the statute was changed to require returned assets be used for the investigation of racketeering or drug crimes.

Because localities may act to offset police seizures through reductions in their allocations to police, the de facto incentives faced by police may be much smaller than the share laid out in statute. We use the results on the degree to which counties offset DOJ seizures from the first part of the paper to create a measure of de facto sharing. Specifically, we use the normalized coefficients generated by including county-specific interactions with DOJ program seizures in

Eq. (1) to capture county-specific reductions in police budgets in response to seizures.¹³ To evaluate how police respond to the real relative to the statutory incentives created by sharing provisions, we estimate the effect of these two measures (first separately and then simultaneously) on our measures of policing behavior:

$$\begin{aligned} \text{Policing Behavior}_{ist} = & \alpha_{is} + \alpha_t + \delta_{st} + \beta_1 \text{ Statutory Sharing}_{ist} \\ & + \beta_2 \text{ Real Net Sharing}_{ist} + X_{ist}\Gamma + \varepsilon_{ist} \end{aligned} \quad (4)$$

We next consider the broader implications of forfeiture laws on the market for illicit drugs. The often-explicit intention of asset forfeiture laws is to “take the profit out of drug crime.” If these provisions affect drug markets, we might expect to see a change in the price of drugs. Thus, we follow the analysis of the effect of financial incentives on arrest patterns with an investigation of their ultimate effect on drug prices.

4. Data

We merge forfeiture, budget and arrest data from several sources to create a panel of county-level observations.

4.1. Federal Department of Justice forfeitures

Data on seizures through the federal program are available annually from 1990 to 1998 at the judicial district level. Since judicial districts are sometimes much bigger than counties, we allocate seizures to counties based on population.¹⁴ A population-weighted regression of the log of DOJ disbursements to counties on the log of population, year fixed effects, and the covariates discussed above yields a coefficient on the log of population of 1.06 (with a standard error of 0.04) and an *R*-squared of 0.7, suggesting that disbursements flow to counties roughly in proportion to their population. As shown in Table 1, Panel A, police seize almost two dollars per capita through the federal statute. Thus, DOJ-processed seizures are 3.6% the size of county allocations to police.

4.2. Forfeitures through state programs

Information on assets seized through state programs is not collected nationally, and different states have different reporting requirements and data availability. We gathered data on assets seized by local law enforcement agencies in (parts of) the 1990s for California, Florida, Pennsylvania, Arizona, and New York. Details on the form and scope of these data are available in a data appendix upon request. We aggregated these seizures to the county–year level.

¹³ By construction, this measure equalizes a jurisdiction’s tendency to offset seizures across all years, and thus captures a locality’s average offsetting behavior. In principle, however, offsetting may vary over time in response to changes in the economy or anti-drug sentiment. Data limitations prevent us from specifying this more flexible model of offsetting.

¹⁴ We explore the validity of this allocation using supplemental data on DOJ disbursements to individual local agencies, which are available from 1998 to 2001. When agencies make seizures, the funds are deposited in a central account before being disbursed back to the local agencies based on the sharing rules. Disbursements (unlike deposits, which get reported in the year of seizure) occur with lags, depending on the timing of the disposition of the case. For this reason, and because of the limited data we have on disbursements through federal and state programs, we focus on seizures.

Table 1
Summary statistics

	Mean	Std dev	<i>N</i>
<i>Panel A: Full sample</i>			
Seizures through DOJ (\$real per capita)	\$2.08	\$3.11	17,408
County budgets (\$real per capita)			
Total spending	\$1068.05	\$1239.28	17,408
Allocations to police	\$57.66	\$72.07	17,408
Deficits (non-police spending — revenue)	−\$36.90	\$158.04	17,408
Arrests (per 100,000 residents)			
“Index” arrests (from county records)	9417	5140	17,408
Total arrests (from MSA records)	5266	2434	14,791
Drug arrests (from MSA records)	459	338	14,791
Price per pure gram of heroin	1855	7184	1033
Statutory sharing rate	0.61	0.39	17,408
De facto sharing rate	0.05	0.03	17,408
<i>Panel B: 5-state sample</i>			
Seizures through states (\$real per capita)	\$1.20	\$2.07	1071
Seizures through DOJ (\$real per capita)	\$2.63	\$2.83	817
County budgets (\$real per capita)			
Total spending	\$1761.88	\$1848.94	1071
Allocations to police	\$109.60	\$113.74	1071
Deficits (non-police spending — revenue)	−\$85.33	\$248.15	1071
Arrests (per 100,000 residents)			
“Index” arrests (from county records)	7418	4769	1071
Total arrests (from MSA records)	5279	1658	2705
Drug arrests (from MSA records)	584	312	2705

Notes: The full sample includes counties in the 48 contiguous states. The 5-state sample is limited to counties in AZ, CA, FL, NY, and PA.

DOJ program seizures are reported by federal judicial districts (and allocated to counties based on population) for the years 1990–1998. State program seizures are available for the 5-state sample only for various years spanning 1994–2001.

County budget data from Bureau of the Census, 1990–2001.

Arrest data from Uniform Crime Reports, 1977–2001. Price data are from STRIDE, 1977–1999.

All observations are at the county–year level and are weighted by population.

As shown in Panel B of Table 1, police in these five states seize roughly one dollar per capita per year, or about 1.2% of their annual police budget, through state statutes. Seizures per capita through the DOJ program are larger in these 5 large industrialized states than in the full sample but represent a similar fraction of police budgets. Federally-adopted seizures represent about 77% of the value of a county’s total annual seizures, although they are not necessarily the police’s preferred method of forfeiture.¹⁵

4.3. State forfeiture sharing provisions

We follow the work of Mast et al. (2000) and Worrall (2001) in codifying the sharing rules in each state according to the fraction of seizures that is to be returned to the seizing (police) agency.

¹⁵ Indeed, although some maintain that DOJ-sharing rules offer law enforcement a generous alternative to strict state sharing provisions (Blumenson and Nilsen, 1998a,b), conversations with several officials in such states (e.g. NJ) reveal that police typically prefer to process their seizures through the state. Furthermore, DOJ will only adopt seizures above a threshold size. Major drug stings, the cases most likely to net significant assets, typically involve federal agencies, even when carried out by or with local police, and DOJ tries to exert its authority in these cases.

For example, in California the statute dictates that 65% of net proceeds be returned to the police agency, while in Florida 100% is returned. We do this for all states, not just those for which we have state seizure data. Details on our coding of state sharing provisions are available as an appendix table upon request.

4.4. County budget data

We use publicly available Census Bureau data on county expenditures for 1991 to 2001. Data are available for all localities every 5 years from the *Census of Government Finances*, and for a sample (roughly half to two-thirds of counties) annually from the *Survey of Government Finances*. All revenues and expenditures are inflation-adjusted using the CPI. Annual police budgets are roughly \$55 per capita for the full sample and \$93 per capita for the 5-state sample.

Counties are not the only unit of local government to finance policing: sub-county governments such as cities do as well.¹⁶ We use the available panel of cities and other local sub-county governments to perform sensitivity checks. Because dramatic changes from year to year in the sample of sub-county governments included in the *Survey* prevent consistent sub-county analysis over time, our main analysis focuses on county governments.

4.5. Arrest data

We use arrest data from the *Uniform Crime Reports* (UCR), which are available annually at the county and at the agency level for Metropolitan Statistical Areas (MSAs). In our analyses of police budgets, we include “index I” (murder, rape, robbery, aggravated assault, burglary, larceny, and motor vehicle theft) arrests per 100,000 from the 1991–1999 annual county-level files. There are approximately 5266 total arrests (defined as the sum of all arrests excluding those unclassified by crime type) per 100,000 residents, about 459 of which are drug-related.

In our analysis of the impact of forfeiture laws on policing activity, we use the UCR agency-level arrest data (aggregated to the county level) for 1977 to 2001. We rely on the agency data because the county data do not report drug arrests until 1985, after many states had already passed forfeiture laws. This long panel of arrests allows us to exploit within-state variation in the adoption of forfeiture laws to identify their impact on policing activity. But, because the agency-level data are only available for MSAs, only about 600 counties (associated with an MSA) are represented in the data.

4.6. Drug price data

We use data from the Drug Enforcement Administration’s *System to Retrieve Information from Drug Evidence* (STRIDE) from 1977 through 1999 to create median county-level cocaine/crack and heroin prices. STRIDE records purchases and seizures of illegal drugs made by undercover DEA agents and informants. Horowitz (2001) points out that STRIDE data are not from independent random samples of illicit drug prices but rather are the result of criminal investigations conducted by the DEA, and thus may not represent the real street prices that would be ideal for this analysis.¹⁷

¹⁶ We do not analyze state spending on policing as it is a relatively small share (about a fifth) of local spending and is generally targeted to activities (e.g. highway patrols) that do not uniquely benefit particular localities.

¹⁷ He further demonstrates that prices obtained from different sources have different levels and trends, although, as Grogger (2001) notes, these series may be measuring different underlying prices (wholesale versus retail).

Others note, though, that STRIDE data are highly correlated with series from unrelated sources, implying some signal in the data (Caulkins, 2001). Since STRIDE remains the only long-term source of drug prices and the best available source of information on cocaine and heroin prices, we use it to provide some evidence of the impact of forfeiture laws on cocaine and heroin markets.¹⁸ Because of the many problems with STRIDE, however, we view this analysis as suggestive and meant merely to complement the other analyses.

4.7. Other covariates

We use county-level data on unemployment from the Bureau of Labor Statistics and on other county population characteristics (such as race urbanicity, per capita income, and age) from the [National Center for Health Workforce Analysis Area Resource File](#).

5. Results

5.1. County offsets of police seizures

We begin by estimating how much counties offset seizures by reducing their allocations to police. If parent governments fully offset the financial “gains” from seizures, then each dollar the police gain through seizures would result in a one dollar reduction in county allocations; if they do not offset at all there would be no estimated change in allocations.

Table 2 present results of the net effect of seizures through the DOJ program on county allocations to police budgets estimated by Eq. (1). All regressions control for index crime arrests per capita, county unemployment rates, county and year fixed effects, and state-specific time trends.¹⁹ The results indicate that increases in seizures within a county are associated with reductions in budgetary allocations to police the following year. As column (1) shows, each dollar police seize is followed by an 82 cent reduction in the police budget — consistent with a total offset of the 80 cents returned by the DOJ per dollar seized. As discussed below, some of these funds are returned to the county corrections and judicial budgets. These estimates suggest that while federally-adopted seizures may be returned to law enforcement, county governments use the budgetary authority at their disposal to capture much of the gains from seizures.

We next consider whether localities co-opt more of the funds police seize when they are facing fiscal distress. To test this hypothesis, we include as regressors the size of a county’s deficit (or surplus) and the interaction between the deficit and seizures made by police in the county through the DOJ forfeiture program, both lagged one year. As column (2) shows, the greater the fiscal stress on a county (as measured by the size of its deficit — real per capita non-police expenditures minus real per capita revenues), the more it offsets seizures by reducing its allocations to police.²⁰ Increasing a county’s deficit by \$100 per capita would increase the offset rate of DOJ program seizures by 30 to 40 cents. Consistent with these results, we find that offsetting is higher in counties with higher poverty rates: at the mean (population-weighted) county poverty rate of

¹⁸ Since STRIDE is a much weaker source for marijuana prices, with most observations coming from DC and few containing purity information (see Jacobson (2004) for more detail), we limit this analysis to cocaine and heroin.

¹⁹ Results with county-specific time trends are quite consistent, but are computationally intensive and in some cases lack power because of the limited number of observations we have per county.

²⁰ County deficit is defined as non-police spending minus total revenues; thus the variable is positive in times of fiscal distress. The results are virtually identical if we use total spending less revenues as our measure of deficits.

Table 2
The effect of seizures on public spending

Dependent variable is per capita spending on:	Police budgets		Corrections and judicial		Public welfare	
	(1)	(2)	(3)	(4)	(5)	(6)
Lagged DOJ seizures	-0.82** (0.39)	-0.78 (0.66)	0.55** (0.10)	0.48** (0.18)	-0.88 (0.61)	0.03 (0.89)
County deficit		-0.0065* (0.0035)		0.0032 (0.0033)		0.0180 (0.0233)
Lagged DOJ seizures* county deficit		-0.0034** (0.0010)		0.0003 (0.0013)		0.0099** (0.0023)
Observations	17,408	15,083	17,495	15,066	15,288	13,441
R-squared	0.98	0.98	0.93	0.93	0.99	0.99
State-specific time trends	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The sample consists of counties from the 48 contiguous states for the years 1991–1999. The dependent variable is a county's per capita police spending. The independent variables of interest are the per capita values of seizures processed through the DOJ the previous year, the county deficit, defined as non-police spending minus revenue, and their interaction. All regressions also include county and year fixed effects, linear, state-specific time trends, per capita index crime arrests, and county unemployment rate. All regressions are weighted by population. Standard errors are clustered by state and given in parentheses. *Significant at the 10% level; **significant at the 5% level.

9.5% in our sample, we estimate a 70 cent offset, but a county with 20% higher poverty rate of 11.4 would have a 97 cent offset. Further exploration of the heterogeneity of local responses provides insight into the political economy of the appropriation of seizure resources by localities. For example, we find substantially more offsetting in localities with larger black populations (55 cents at the mean (of 11.6%) versus 77 cents for counties 20% above the mean).

If counties usurp DOJ-processed seizures from police budgets, where do they spend the extra money? Columns (3) to (6) of Table 2 explore the possible reallocation of police budgetary offsets by county governments. To the extent that they reallocate any funds to local law enforcement-related programs, counties appear to reallocate police budget offsets primarily to other criminal justice programs.²¹ In particular, a one dollar increase in DOJ seizures is associated with an almost 50 cent increase in allocations to correctional and judicial budgets. This puts the total offset of police seizures in column (1) into a more complete perspective. If police and other criminal justice budgets are planned simultaneously, columns (1) and (3) imply that local police and criminal justice budgets go up by 50 cents on net for every dollar seized.²²

Seizures appear to be related to budget reductions for public welfare programs, although the main estimate is fairly imprecise. And in times of fiscal distress, public welfare programs benefit from seizure offsets: a county with a deficit of \$50 per capita would increase public welfare spending by 53 cents for each dollar seized, compared with an insignificant 3 cents for a county with no deficit. Spending on other budget categories, such as fire protection and health and hospitals (not shown here), does not respond to seizures, overall or in times of distress.

²¹ This is consistent with Baicker's (2004) finding that the shock of financing a capital crime trial may be absorbed in part by decreases in police spending, suggesting that spending on these two budget categories may be linked.

²² The 80 cents DOJ returns per dollar seized is fully offset; but 50 cents is funneled to corrections. Estimates using aggregated police, corrections and judicial budgets are only marginally significant in most specifications.

5.2. Specification tests and causality

Table 3 reports the results from several robustness checks. Results are quite consistent across specifications using state–year fixed effects instead of year fixed effects and state-specific time trends (or just omitting state–time trends),²³ omitting population weights and including a control for population,²⁴ and omitting the crime controls.²⁵ This consistency makes it unlikely that unmeasured underlying crime or other smoothly trending unobservables are driving the results.

Since our analysis relies primarily on county-level data even though sub-governments may be important, we repeat the estimation using the (limited) data available on police spending by city governments or by all local government units (aggregated to the county level in both cases). These results are qualitatively similar, although generally smaller in magnitude. That the county offset estimates are consistently larger in magnitude than the sub-county estimates suggests that county governments may account for a substantial share of seizure offsets, even if they do not account for the majority of police spending.

We next explore the causal interpretation of our main results, which relies on understanding the source of variation in police seizures. Certainly police effort is important, but anecdotal evidence suggests considerable idiosyncrasy in the size of seizures realized at any given time.²⁶ We confirm these claims more systematically below by showing that seizures are fairly unpredictable from year to year. Our results may still, however, capture police reactions to reduced budgets, rather than just county reactions to police seizures. We try to disentangle the possibility of police “fundraising” from county offsetting in several ways. First, we use lagged seizures as a crude way to focus on budget responses. If we run a similar regression of seizures on lagged budgets, we find virtually no effect (a coefficient of -0.002 and robust s.e. of 0.003). This evidence is far from conclusive, however: given the autocorrelation of budgets and the potential for measurement error, it is difficult to prove causality with timing alone.

As a second approach, we focus on “windfalls” or seizures that are more than a standard deviation away from the norm for police within a county and “unexpected” seizure amounts, or the residual from a regression that predicts seizures in $t-1$ based on seizures in $t-2$, our basic set of covariates and county and year fixed effects.²⁷ Specifically, we include lagged windfall

²³ Estimates with county-specific time trends or state-specific year effects are similar to our preferred model, implying that we are soaking up much of the idiosyncratic variation specific to localities, but the estimates without state-specific trends are substantially larger and thus may not adequately control for unobserved heterogeneity.

²⁴ Unweighted results are similar here, but in other specifications and samples (particularly those with fewer observations) population weights increase precision. We experiment with a control for population here because Worrall (2001) suggests that large police agencies, which typically correspond to police agencies in large jurisdictions, report greater reliance on and use of asset forfeiture.

²⁵ Inadequately controlling for underlying crime (and seizure opportunity) would bias our results towards 0 if in times of higher (unmeasured) crime people spent more on policing. But the results are similar when we control for lagged or contemporaneous arrests and are also robust to the exclusion of crime controls altogether.

²⁶ As one critique of asset forfeiture puts it, “While law enforcement officials make a big deal out of the occasional large drug bust, the majority are relatively small-time dealers or users who do not have lots of valuable property (Meeker-Lowry, 1996).” An investigative report confirms this claim, noting that despite law enforcement boasts of regularly seizing “mansions, planes, and millions in cash,” the “DEA’s own database contradicts the official line. It showed that big-ticket items — valued at more than \$50,000 were only 17% of the total 25,297 items seized by the DEA” over an 18 month period (Schneider and Flaherty, 1991).

²⁷ In fact, seizures look fairly unpredictable from year to year. Regressing this year’s state seizures on last year’s state seizures, including county and year fixed effects and our basic set of covariates, yields a coefficient of -0.04 (with a standard error of 0.03). (Mean reversion would tend to bias these coefficients down).

Table 3
Robustness checks of the effect of seizures on police budgets

	Preferred specification	Alternate covariates:				Dependent variable:		Independent variable: unpredictable seizures			
		No state-specific time trends	State-by-year FEs	No crime controls	Unweighted	City spending	Sub-county spending	“Windfall” seizures		“Unexpected” seizures	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Lagged DOJ seizures	-0.82** (0.39)	-1.30** (0.39)	-0.89** (0.37)	-0.81** (0.36)	-0.83** (0.32)	-0.21 (0.23)	-0.41** (0.18)	-1.10** (0.39)	-0.57 (0.43)	-0.69** (0.09)	-0.46** (0.08)
Observations	17,408	17,408	17,408	17,408	17,408	11,959	15,269	15,272	15,272	17,408	17,408
R-squared	0.98	0.98	0.98	0.98	0.98	0.98	0.96	0.98	0.98	0.98	0.98

The sample consists of counties from the 48 contiguous states for the years 1991–1999. The dependent variable in columns (1)–(5) and (8)–(11) is per capita county police spending. The dependent variable in column (6) is per capita police spending by all cities within a county and in column (7) is per capita police spending by all sub-county units within a county. The independent variable of interest is the per capita values of seizures processed through the DOJ the previous year. In columns (8) and (9) the independent variable is “Windfall” per capita seizures that are more than one standard deviation above the county’s mean per capita seizure. In columns (10) and (11) the independent variable is “Unexpected” seizures that are the residual from a regression of seizures in $t-1$ on seizures in $t-2$ (with the same set of covariates used in the main regression and year and county fixed effects). All regressions also include county fixed effects and unemployment rates. Most regressions include year fixed effects and state-specific time trends. Regressions in columns (2), (6), and (8) exclude state-specific time trends. Regression in column (3) excludes state-specific time trends and year fixed effects. All but the regression in column (4) include per capita index crime arrests. All regressions except that reported in column (5) are weighted by county population. The regression in column (5) includes county population as an independent regressor. Standard errors are clustered by state and given in parentheses. * Significant at the 10 percent level; ** significant at the 5 percent level. There are fewer observations in columns (5) and (6) because of differences in the sub-county sampling frame.

seizures or lagged unexpected seizures, instead of lagged seizures, to estimate the same basic regression in Table 2. These results, shown in Table 3, imply very similar offsets.²⁸

That localities change their allocations to police in response to surprise seizures helps rule out the possibility that localities reduce police budgets lump sum in response to the introduction of sharing laws rather than offsetting incrementally based on the amount police actually seize.²⁹

As a third strategy for isolating the response of county allocations to police seizures, we exploit the timing of mayoral election cycles to see how the systematic increase in police spending in election years affects the value of asset seizures. Counties are assigned the mayoral election year of the largest city in the jurisdiction.³⁰ We find that mayoral elections are associated with an increase in per capita county-level police spending of roughly two dollars in election years.³¹ Using this variation in timing of the mayoral election cycle to instrument for police budgets, we find that increases in spending are associated with small (but precisely estimated) reductions in the value of seizures: a one dollar per capita increase in police spending leads to a roughly 6 cent decrease in per capita seizures, suggesting that police fundraising accounts for at most 7% of the estimated relationship between DOJ seizures and police budgets (0.06/0.82). Thus, while the offset estimates may capture a behavioral response on the part of police, they are driven primarily by county capture of police seizure resources.

While these approaches to disentangling causality are neither conclusive nor problem-free, the consistency of results across them is reassuring. All three suggest that counties react to seizures by reducing their allocations to police budgets, partially undermining the statutory incentive for police to seize more.

5.3. Case studies in state program seizures

In lieu of processing a seizure through the DOJ, agencies in many states can declare assets forfeit under state law. We use data we have gathered on state program seizures in five states to compare local reactions to these seizures. When the statutory sharing rule is less than the DOJ share of 80%, local agencies make 29% of their seizures through the state program; when the statutory sharing rule is greater than 80% they make 37% of their seizures through the state program. As shown in Table 4, however, seizures made through state programs, like those

²⁸ To the extent that most drug arrests are part of a “climb the organizational ladder” approach to drug enforcement, whereby many small dealers are arrested and then negotiated with in order to get to the kingpin, the windfall or unexpected seizure strategy may seem limited. But, because the data are at the county–year level rather than the seizure level, agencies (i.e. local police departments) within a county would have to coordinate on major seizures to make a large enough difference to count as one of our shocks. Furthermore, because the time frame between knowing a large bust will be made and executing it is likely to be short compared to the budgeting cycle, counties are unlikely to be able to plan for this in their budgets.

²⁹ As a further test of this possibility, we also estimate Eq. (1) solely on the set of counties with positive seizures, with and without controls for lagged police allocations. Results are consistent: for example, including lagged police budgets and using the restricted sample results in an offset measure of 59 cents.

³⁰ We limit the analysis for this section to 52 large U.S. cities. In several cases, these cities correspond uniquely to counties (e.g. New York, Philadelphia, and San Francisco). Levitt (1997) and McCrary (2002) consider 59 large cities. We cut the sample down slightly because some of the associated counties are not in our main analysis.

³¹ We found no effect on spending in other budget categories — consistent with previous literature. Similarly, mayoral elections significantly affect total arrests or the fraction comprised of drug arrests, suggesting (although not proving) that the election cycle affects seizures primarily through changes in police budgets.

Table 4
The effect of state seizures on police budgets in five large states

	Per capita police budgets			
	(1)	(2)	(3)	(4)
Lagged state seizures	-0.47 (0.89)	-0.65 (0.89)	-0.87 (0.88)	-1.09 (0.87)
County deficit			-0.013 (0.007)	-0.012* (0.007)
Lagged state seizures* county deficit			-0.005* (0.002)	-0.006** (0.002)
Observations	953	953	846	846
R-squared	0.98	0.98	0.99	0.99
State-specific time trends	No	Yes	No	Yes
Sample	5-state	5-state	5-state	5-state

Notes: The sample consists of counties from AZ, CA, FL, NY, and PA for various years spanning 1994–2001. See the Appendix (available upon request) for information on state seizure data. The dependent variable is a county's per capita police spending. The independent variables of interest are the per capita values of seizures processed through the state the previous year, the county deficit, defined as non-police spending minus revenue, and their interaction. All regressions also include county and year fixed effects, per capita index crime arrests, and county unemployment rate. Regressions reported in even columns also include linear, state-specific time trends. All regressions are weighted by population. Standard errors are clustered by state and given in parentheses. *Significant at the 10% level; **significant at the 5% level.

processed through the DOJ, appear to be partially offset by police budget reductions. As shown in columns (1) and (2), each dollar of seizures made by police through the state program results in a reduction of an (insignificant) 47 to 65 cents in county allocations to police. Offsets are higher when a county is running a budget deficit. A \$100 per capita increase in the deficit increases the offset rate of state program seizures by 50 to 60 cents. Counties engage in budgetary offsetting regardless of which forfeiture program processes the seizure.

5.4. Police responses to incentives

The results presented above imply that a simple consideration of the sharing rules laid out in forfeiture statutes will give a misleading view of the incentives police face and of the effect of seizure programs on the distribution of resources. By combining information on the sharing percentages established under state forfeiture law with a measure of the extent to which counties offset police budgets in response to DOJ seizures, we can distinguish the responsiveness of police to both de jure and de facto forfeiture policies.

What drives variation in de facto sharing? We cannot answer this question systematically with our data but anecdotal evidence suggests that idiosyncratic factors play an important role. For example, when John Ashcroft (a strong proponent of the war on drugs) was governor of Missouri, he allowed police to keep all net forfeited proceeds even though the Missouri constitution requires the funds be put towards public education ([The Post Standard, 2001](#)). To characterize local differences in de facto sharing, we estimate Eq. (1) but add an interaction between county dummies and lagged DOJ seizures. We then use these county-specific seizure offset coefficients to construct our measure of offsetting.

Specifically, we create a continuous county measure based on the county-specific offset coefficients (the coefficients on the interaction between lagged seizures and county dummies, rescaled to be strictly between 0 and 1). The measure is set equal to zero in years in which no

forfeiture statute is in effect.³² Note that this measure, which parameterizes the real net share that police can expect to keep through state seizure laws once county offsets are taken into account, is based on observed responses to the federal sharing law. While this choice is driven by data limitations, it seems reasonable to assume that a county's underlying propensity to offset state and federally-processed seizures is correlated (and to the extent that this assumption is incorrect it will bias us against finding an effect on behavior).³³ We then estimate Eqs. (3) and (4) using this measure of de facto sharing (both with and without our measure of statutory sharing rules) to see how anti-drug policing responds to both gross and net forfeiture incentives.³⁴

The mean de facto sharing rate (shown in Table 1) is less than a tenth of the statutory sharing rate (0.05 versus 0.61), but both measures of sharing are highly correlated. The raw correlation coefficient between the two measures is 0.92, and a population-weighted regression of the de facto sharing measure on the statutory sharing rate and county and year fixed effects from 1977 to 2001 yields a coefficient of 0.07 with a standard error of 0.004.

Table 5 evaluates the impact of these measures of statutory and de facto incentives on drug arrest rates. We consider all drug crime arrests, as well as arrests broken down by type (sales versus possession) and by drug (marijuana versus cocaine, heroin, and other opiates). Drug arrests increase with statutory sharing rules. Going from no sharing to complete sharing is associated with an increase in the drug arrest rate of 82 per 100,000 residents, or almost 20%. With a mean sharing rate of 38%, this implies that a 1% increase in the sharing rate increases the drug arrest rate by almost 0.1%. The increase is concentrated less in sales (column 4) and more in possession (column 7) arrests, although the distinction is somewhat artificial because it is based on the amount of the illicit substance found on offenders, not their (unmeasurable) intent.³⁵ When broken down by drug type (columns 10 and 13), the effect is only statistically significantly different from zero for cocaine and opiate arrests.

Looking at the statutory sharing rate alone, however, gives a misleading picture of police responses to incentives. When we consider the “real” net incentives faced by police, we see that arrests increase substantially when police get to keep more of the proceeds. Given the strong correlation between statutory and de facto sharing at the county–year level (along with the difference in mean values), it is difficult to evaluate which incentives police respond to more strongly from these separate regressions. When the statutory and the de facto sharing rates are included simultaneously in our models of arrest rates, the impact loads entirely on de facto sharing. In other words, real incentives matter more than the statutory ones, confirming the

³² We scale the coefficients based on the minimum and maximum coefficients, so that each represents (the actual coefficient minus the minimum) over (the maximum minus the minimum) — or the percent of the distance from the smallest to the largest. We set the sharing variable equal to zero in years in which there is no state sharing law to reflect the fact that the agencies do not have access to any de facto sharing through a state program.

³³ Data limitations also mean that our de facto sharing measure is based on a more limited time period (1991–1999) than our policing measures (1977–2001 in some specifications).

³⁴ We continue to “cluster” the standard errors in these models at the state level. In actuality, the standard errors should be adjusted to account for the use of a generated regressor (de facto sharing) but the correction in this context would be both computationally and analytically difficult. Moreover, the state-level clustering offers a more conservative estimate of the standard errors than simply adjusting at the level of aggregation (the county). For example, clustering at the county rather than the state level reduces the standard error for the estimate of de jure sharing on drug arrest rates in column 1 of Table 4 from 41 to 24.

³⁵ To the extent that the division is real, possession arrests may respond more to seizure laws than sales arrests if police increase their efforts to arrest and plea out low-level dealers in an attempt to get to the head of the organization. Alternatively, if police divert resources away from enforcement of possession offenses towards trafficking offenses, the relative increase may actually capture a shift out in the demand for illicit drugs.

Table 5
Effects of incentives on anti-drug policing

	Drug arrest rates (per 100,000 residents)														
	Total		Sales			Possession			Marijuana			Opiates/cocaine			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
Statutory share of proceeds agencies can keep	82** (41)		–333* (174)	28 (22)		–129 (84)	53** (23)		–206* (106)	8 (13)		25 (33)	68** (37)		–301** (150)
De facto share agencies keep		1592** (651)	6031** (2592)		558* (329)	2269* (1227)		1019*** (371)	3764** (1545)		81 (170)	–256 (417)		1350** (592)	5355** (2243)
Observations	14,537	14,537	14,537	14,537	14,537	14,537	14,537	14,537	14,537	14,537	14,537	14,537	14,537	14,537	14,537
R-squared	0.73	0.73	0.73	0.72	0.72	0.72	0.71	0.71	0.71	0.63	0.63	0.63	0.75	0.75	0.75

Notes: The sample consists of counties in Metropolitan Statistical Areas in the 48 contiguous states for the years 1977–2001. The dependent variable is a county's arrests of the indicated type (total drug, drug sales, drug possession, all marijuana offenses, all opiate and cocaine offenses) per 100,000 residents. The independent variables of interest are the statutory sharing rate (the share of forfeited assets that a seizing agency can keep based on state law, reported in Appendix Table 1 (available upon request)) and the de facto share rate (created based on county-specific offsets of police seizures, described in the text). All regressions also include county and year fixed effects, and linear, state-specific time trends. All regressions are weighted by population. Standard errors are clustered by state and given in parentheses. *Significant at the 10% level; **significant at the 5% level.

hypothesis that police are more responsive to incentives than a naïve examination of statutory rates alone would suggest. Evaluated at the mean of our de facto sharing variable, 0.05, the coefficients (in column 3) imply that a 1% increase in the de facto sharing rate increases the drug arrest rate by about 0.66%, or almost 7 times as much as the de jure sharing rate suggests. The implied elasticities for drug possession and opiate/cocaine arrests, evaluated at their means of 344 and 181 per 100,000, are 0.23 and 0.65 respectively.³⁶

This analysis of sharing exploits differences in the timing of the introduction of laws across states to identify the effect of sharing on police behavior. If states pass forfeiture laws or increase their sharing rules endogenously in response to increased drug activity, however, these estimates may not isolate the impact of forfeiture policies. To assess this possibility, we rerun our models of arrests including 3 years of leads and lags of our sharing variables. While the leads typically suggest that drug arrests may have been drifted up slightly in the years before a state adopted a new or more generous forfeiture law, the magnitudes of the lags are markedly and significantly higher. For example, the average effect of the three leads of the de facto police share on drug arrest rates is 1798 (s.e. 834), while the average effect of the three lags is 3978 (s.e. 1807), with the difference between these significant at the 4% level. These results suggest that forfeiture incentives had a real impact on anti-drug policing.

In Table 6 we assess how real forfeiture incentives influence the composition of arrests (between drugs and other crimes). For comparison with Table 5, we first consider the statutory sharing rule, then the de facto sharing rate, and finally both together. The dependent variables studied are the fraction of all arrests that are for: marijuana; opiates and cocaine; liquor or DUI; robbery, larceny, or burglary; and a miscellaneous category that includes arrests for gambling, loitering, vagrancy, bookmaking, embezzling, runaways, rape, forgery, motor vehicle theft, sex offenses, weapons charges, and arson. Although imprecise, these results imply that when police are allowed to keep a greater fraction of seizures, arrests for marijuana relative to all arrests decrease ($p < 0.11$) and arrests for cocaine and heroin relative to all arrests increase ($p < 0.08$). An increase in de facto sharing is also associated with a reduction in the intensity of arrests for offenses such as robbery, larceny and burglary, and for the miscellaneous composite arrests.

Evaluated at the population-weighted mean fraction of arrests for marijuana (0.038) and opiates/cocaine (0.033) and the weighted mean de facto sharing rate for this sample (0.033), a 10% increase in de facto sharing increases the relative intensity of heroin/cocaine policing by 1.2% and decreases the relative intensity of marijuana policing by a smaller 0.3%. This is suggestive of revenue-seeking police behavior since heroin is a small but lucrative market, while marijuana is a drug much less likely to generate large seizure revenues.³⁷ These results also suggest we are capturing a true effect of incentives on policing, rather than an increase in drug demand. If a general increase in drug demand were driving the adoption of forfeiture laws, we should see an increase in the intensity of policing across all drug categories.

³⁶ We tested the sensitivity of this analysis to a de facto sharing measure that accounts for money channeled to other criminal justice purposes. These results are as precisely estimated but, as expected, somewhat smaller in magnitude than the results in Table 4. For example, when we take into account spending on other criminal justice programs, the elasticity of the drug arrest rate with respect to the de facto sharing falls from about 0.66 to 0.23.

³⁷ In 1994, there were an estimated 120,000 past month users of heroin in contrast to 1.4 million regular users of cocaine and over 10 million regular users of marijuana, SAMSHA (1999). But, as one police sergeant characterized the market in the 1990s, "Heroin is a very-high-profit item ... The dealers can make a lot more money selling heroin than they can selling cocaine," (New York Times, 1993).

Table 6
Effects of incentives on composition of arrests

	Fraction of all arrests in category														
	Marijuana			Opiates/cocaine			Liquor and DUI			Robbery, larceny, and burglary			Miscellaneous other		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
Statutory share of proceeds agencies can keep	−0.002 (0.002)		0.002 (0.006)	0.006 (0.004)		−0.033* (0.017)	−0.001 (0.013)		0.022 (0.043)	−0.011* (0.006)		0.002 (0.021)	0.011 (0.009)		0.040** (0.019)
De facto share agencies keep		−0.038 (0.023)	−0.063 (0.081)		0.1198* (0.065)	0.560** (0.244)		−0.045 (0.153)	−0.337 (0.510)		−0.168* (0.088)	−0.198 (0.311)		0.110 (0.114)	−0.420* (0.221)
Observations	14,533	14,533	14,533	14,533	14,533	14,533	14,533	14,533	14,533	14,533	14,533	14,533	14,533	14,533	14,533
R-squared	0.60	0.60	0.60	0.80	0.80	0.80	0.79	0.79	0.79	0.75	0.75	0.75	0.74	0.74	0.74

Notes: The sample consists of counties in Metropolitan Statistical Areas in the 48 contiguous states for the years 1977–2001. The dependent variable is a county's fraction of total arrests of the indicated type (total drug, murder and manslaughter, liquor and DUI offenses, robbery, larceny and burglary, and a "miscellaneous other" category). "Miscellaneous other" includes arrests for gambling, loitering, vagrancy, bookmaking, embezzling, runaways, rape, forgery, motor vehicle theft, sex offenses, weapons charges, and arson. The independent variables of interest are the statutory sharing rate (the share of forfeited assets that a seizing agency can keep based on state law, reported in Appendix Table 1 (available upon request)) and the de facto share rate (created based on county-specific offsets of police seizures, described in the text). All regressions also include county and year fixed effects, and linear, state-specific time trends. All regressions are weighted by population. Standard errors are clustered by state and given in parentheses. *Significant at the 10% level; **significant at the 5% level.

Table 7
Effects of incentives on drug prices

	Natural log of prices					
	Heroin			Crack and cocaine		
	(1)	(2)	(3)	(4)	(5)	(6)
Statutory share of proceeds agencies can keep	0.24 (0.13)		-0.27 (0.42)	0.13* (0.07)		-0.17 (0.20)
De facto share agencies keep		4.14** (1.97)	7.41 (5.93)		-1.52 (1.00)	0.59 (2.59)
Observations	1993	1993	1993	5128	5128	5128
R-squared	0.64	0.64	0.64	0.65	0.65	0.65

Notes: Drug price data are from STRIDE (1979–1999). The sample consists of counties in the 48 contiguous states where Drug Enforcement Administration agents made undercover drug buys during the years 1979–1999. The dependent variable is the median price per gram of marijuana purchased in a given county and year or the median price per purity adjusted gram of heroin purchased in a given county and year. Purity data are not available for marijuana purchases. The independent variables of interest are the statutory sharing rate and the de facto share rate (see notes to Table 6 and discussion in the text for details of these measures). All regressions also include county and year fixed effects. All regressions are weighted by population. Standard errors are clustered by state and given in parentheses. *Significant at the 10% level; **significant at the 5% level.

The ultimate stated goal of forfeiture laws is to curtail illegal drug activity. If police respond to forfeiture laws, and if policing has an appreciable effect on drug markets, we should see a change in the price of illegal drugs. Although STRIDE data suffer from many problems, as discussed above and in more detail in Horowitz (2001), they are collected primarily by DEA agents and informants, making them very useful for our analysis. Because DEA agents (and any seizures they make) are governed by federal law, the buys that generate the STRIDE data should not be influenced by the state laws we are studying. In other words, it is unlikely (as confirmed by simple regressions of the number of buys and the share of STRIDE observations that are buys versus seizures) that these laws affect the composition of price observations in the STRIDE data.

Table 7 evaluates the effect of the forfeiture incentives on heroin and cocaine prices.³⁸ The effect of de facto sharing on log cocaine prices is small and imprecise. In contrast, forfeiture incentives are associated with a clear increase in the log price of heroin, the most commonly used illicit opiate: evaluated at the mean de facto sharing rate of 0.33, the coefficient of 4.14 implies an elasticity of heroin prices with respect to real sharing of 0.14. This is consistent with past work establishing an impact of prohibition policies on heroin prices (Miron, 2003), as well as the increase in cocaine and heroin arrest rates (unfortunately reported together in the UCR) shown in Table 5. Due to data limitations, however, these results are merely suggestive of an effect of forfeiture policies on illicit drug markets.

Together, Tables 5, 6 and 7 paint a consistent picture of police reactions to forfeiture policies. Police respond more strongly to real incentives than to statutory incentives. And they respond to real net financial incentives, in part by making more drug arrests overall and in part by making more lucrative ones. These changes may ultimately affect drug markets.

³⁸ These results include county and year dummies, but not state-specific time trends (given the limited number of observations). Results with state-specific time trends are consistent but much less precisely estimated.

6. Conclusion

Asset forfeiture policies, like performance bonuses for schools, hospitals, or managers, provide financial incentives to align local agents' interests with those of policy-makers. In a federal system, where different intervening levels of government may have competing interests and social goals, such incentive programs may not work as efficiently as intended.

Our analysis of asset seizure laws shows that these financial incentives change the behavior of not only police but also county governments. Both respond to the incentives created by seizures laws in a sophisticated way. Local governments capture a significant fraction of the seizures that police make by reducing their other allocations to policing, partially undermining the statutory incentive created by seizure laws. They are more likely to do so in times of fiscal distress. Police, in turn, respond to the real net incentives for seizures once local offsets are taken into account, not simply the incentives set out in statute. When police are really allowed to keep the assets they seize, they increase anti-drug policing. A simple analysis of the effects of asset forfeiture laws, as they appear on the books, would provide a distorted view of their effects.

These findings have strong implications for the effectiveness of using financial incentives to solve agency problems in the provision of public goods in a federal system more broadly, from education to health care to welfare. In particular, they suggest that the ability to influence public agents through federal and state laws is limited by the ability of local governments to divert funds to other uses. Ignoring this yields a misleading picture of the responsiveness of local agents to incentives and the effectiveness of federal and state policies. Understanding the financial incentives faced by each agency and each level of government involved in the budget process is a complicated but crucial component of designing policies to affect the provision of public goods. As in private markets, when incentives can be made to reach the agents in question, they can be very effective and powerful tools for influencing behavior. In the case of public goods provided in a federal system, however, optimal policies must account for the responses of intervening levels of governments to incentives as well.

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