

# Special interest groups and the allocation of public funds <sup>☆</sup>

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## Abstract

I propose a fully rational model of government contracting that explains differences in local government spending from grants and other income. In this model, violations of fungibility arise from dynamic interactions between politicians and interest groups with the ability to raise funds for the local government. The predictions of the model are tested by exploiting unique features of windfalls received by states under a settlement with the tobacco industry. Although windfalls are legally unrestricted, the median state increased spending on tobacco control programs from zero to \$2.30 per capita upon receipt of funds. The marginal propensity to spend on such programs is 0.20 from settlement revenue and zero from overall income. States which were not involved in the settlement lawsuits spend less. These results cannot be explained by existing models in the literature.

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## 1. Introduction

Standard models of fiscal federalism predict that unconstrained grants received by local governments should be considered equivalent to increases in the income of the local constituency. Perhaps the most commonly studied violation of this fungibility principle is the flypaper anomaly: the empirical observation that money “sticks where it hits.” Local governments spend more from intergovernmental grants than from equivalent increases in constituent income, and grants for particular programs tend to increase spending on those programs far more than standard theory suggests. Numerous studies have documented the existence of flypaper effects, with estimates of the increase in local spending arising from a dollar grant ranging from 25 cents to one dollar (Hines and Thaler, 1995).<sup>1</sup>

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<sup>1</sup> Models illustrating the standard revenue equivalence proposition include Bradford and Oates (1971a,b). See Gramlich (1977), Inman (1979), Fisher (1982) and Hamilton (1983) for a review of the earlier literature. More recent work includes Baicker (2001), Gordon (2004), Lutz (2006), and Evans and Owens (2007).

In this paper, I argue that targeted grants that appear unconstrained may in fact carry implicit contractual obligations. I propose a new, rational model of government spending decisions that focuses on the potential role of special interest groups in influencing the allocation of public funds. In this model, special interest groups have the ability to raise funds for local governments by undertaking costly effort. In a dynamic setting, it is optimal for rational politicians to take the preferences of these interest groups into account when making spending decisions to ensure that groups have incentives to undertake the effort costs of raising funds in the future. Resulting violations of fungibility are actually welfare improving for constituents.<sup>2</sup> I test the predictions of the model by examining the response of state spending on tobacco prevention and control programs to receipt of windfalls arising from state lawsuits against the tobacco industry.

There are few theories in the existing literature that can explain observed violations of fungibility. [Filimon, Romer and Rosenthal \(1982\)](#) propose a model in which agenda-setting bureaucrats are able to hide grants from voters. While this model predicts that money received by governments will remain at the government level, it does little to explain why categorical grants should systematically increase expenditure in particular spending categories. Models focusing on rent seeking by public officials ([Inman, 1979](#)), fiscal illusion ([Oates, 1979](#)), voter uncertainty ([Turnbull, 1992](#)), possible differences in the tax burden of the average versus median voter ([Fisher, 1982](#)) or the deadweight costs associated with raising tax revenue ([Hamilton, 1986](#)) similarly predict that grants should produce a greater increase in government spending than equal increases in constituent income but make no predictions about the allocation of spending across categories. [Hines and Thaler \(1995\)](#) argue that these effects can be explained by voter mental accounting; however, I present evidence that is difficult to reconcile with a simple mental accounting story.

Two studies explicitly consider the processes by which grants received by local governments are determined and the potential role of such processes in generating violations of fungibility across spending categories. [Chernick \(1979\)](#) argues that granting agencies allocate project grants to communities willing to commit more local funds to the project, creating implicit matching requirements even for lump sum grants. [Knight \(2002\)](#) proposes a legislative bargaining model in which a bargaining process at the federal level leads to endogenous grants that reflect local spending preferences. Applying this model to federal highway grants, he finds that instrumenting for grants with measures of political bargaining power eliminates apparent flypaper effects.

I focus instead on the interaction between special interest groups and government. A substantial literature exists addressing the role of special interest groups in influencing political decision-making. However, to the best of my knowledge, existing work has not considered the potential influence of special interest groups on local spending in the fiscal federalism context.<sup>3</sup>

I exploit unusual features of windfalls that states received as a result of a 1998 settlement with the tobacco industry to test the special interest group model against alternative hypotheses. The tobacco settlement agreement resolved multiple lawsuits filed by states against the tobacco industry during the 1990s. Under the terms of the settlement, tobacco companies must pay states large annual sums (on the order of \$7 billion per year) in perpetuity. I examine the response of state spending on tobacco prevention and control programs to receipt of settlement funds.

Two key features of the settlement windfalls are advantageous for testing violations of fungibility in the allocation of funds. First, settlement money is legally unrestricted and use of funds is left entirely to the discretion of states. Second, I demonstrate that the timing and magnitude of windfalls are plausibly exogenous to desired spending on tobacco control programs. Grants do not reflect underlying spending preferences and are truly lump-sum, without explicit or implicit matching provisions. The models proposed by Chernick and Knight are therefore not applicable in this case.<sup>4</sup>

I find clear evidence of violations of fungibility in government spending decisions. Average per capita spending on tobacco control programs increased more than six-fold from the fiscal year before settlement revenues were received to the fiscal year after receipt. The marginal propensity to spend on such programs is 0.20 from settlement revenues and zero from other income. I find that states that did not file lawsuits prior to the settlement, where anti-tobacco interest groups presumably exerted less effort, spend significantly less on tobacco control programs after the settlement.

This empirical setting differs in at least two important ways from traditional targeted grants. First, unlike many grants, settlement windfalls were not specifically labeled for tobacco prevention and control programs. Second, transfers in this case are from private industry to local government, rather than intergovernmental transfers. While these

<sup>2</sup> In contrast, violations of fungibility from truly (explicitly and implicitly) unconstrained grants would have negative welfare effects for constituents. I am grateful to an anonymous reviewer for pointing out this distinction and its implications.

<sup>3</sup> [Dougan and Kenyon \(1988\)](#) examine a potential role for pressure groups in generating flypaper effects. In their model, however, interest groups are not responsible for procuring grants and they assume limited crowd-out by local governments.

<sup>4</sup> Concerns that apparent violations of fungibility may be driven by econometric misspecifications, such as incorrect treatment of price effects arising from matching grants ([Moffitt, 1984](#)) or omitted variable bias ([Hamilton, 1983](#)) are also unlikely to be problematic.

features have advantages in distinguishing among alternative models, they also raise potential caveats in generalizing the findings to other settings. I consider these issues in the concluding section of the paper.

The remainder of the paper proceeds as follows. Section 2 presents the model. Section 3 provides background on the settlement agreement and payments. Section 4 describes the empirical methodology and data used, and Section 5 presents the results. Section 6 concludes.

## 2. Interest groups and the allocation of funds

*“It’s moral treason to me. We got all this money, then legislatures and governors who were not even in this fight act like the money fell out of heaven and spend it on the political whim of the day.”* — Mississippi Attorney General Michael Moore on states spending settlement funds on non-tobacco related programs (New York Times, 2001).

### 2.1. Motivation

Grants-in-aid from the federal government to states are of two main types: mandatory “entitlement” grants, for which spending is determined by existing law, and discretionary grants, for which funding is allocated on an annual basis. In fiscal year 2003, almost 60% of federal dollars given in grants-in-aid to states, excluding Medicaid, were discretionary.<sup>5</sup> In this system, interest groups have the ability to influence grants-in-aid through contributions and lobbying efforts. Interest groups are large contributors to federal legislators: during the 1997–1998 election cycle, over 4500 Political Action Committees spent almost \$500 million dollars and a variety of other organizations spent \$1.5 billion on lobbying Washington.<sup>6</sup> A substantial literature (Becker, 1983; Grossman, 1994; Grossman and Helpman, 2001) has shown that these groups do have the power to influence policy and the distribution of grants.

I develop a model of how interest groups procuring funds at the federal level can influence the allocation of public funds at the *local* level. An interest group is defined broadly as any agent who has the ability to influence grants to local governments and has specific spending preferences that may differ substantially from the local median voter.<sup>7</sup> I argue that local governments may not treat these grants as fungible once they are received as the result of a dynamic interaction between interest groups and local government. If an interest group raises funds for its preferred good and the local government does not increase spending on that good, interest groups have no incentive to undertake the costs of procuring grants. Local governments must trade off the social welfare benefit of treating funds as fungible against the cost of losing future grants.

My empirical analysis focuses on a particular case: the tobacco settlement. Similar interactions between interest groups and government, however, are common in a wide variety of settings. Discretionary grants-in-aid to states include allocations for local health programs, environmental projects, schools, law enforcement and workforce programs, and are lobbied for by interest groups ranging from medical associations to labor unions. Although the existing literature has typically not made a distinction between explicit and implicit constraints, numerous grants may carry implicit contractual obligations to the lobby groups involved in raising funds, and the model is therefore applicable to violations of fungibility in a number of contexts.<sup>8</sup>

### 2.2. Model

I begin with a simple stylized reputation model in which a long-run government player interacts with a number of short-run interest group players.<sup>9</sup>

<sup>5</sup> Source: Center on Budget and Policy Priorities. The primary mandatory grants-in-aid from the federal government to states are through Medicaid, food stamp, and welfare programs. Most other grants-in-aid are discretionary.

<sup>6</sup> Source: The Center for Responsive Politics. [www.opensecrets.org](http://www.opensecrets.org).

<sup>7</sup> An “interest group” could also be a group within the government, as long as these two conditions are met.

<sup>8</sup> For example, the types of interactions I describe provide one possible explanation for the flypaper effects observed by Evans and Owens (2007) in the COPS program.

<sup>9</sup> This model adapts standard models of reputation with a single long-run player; see Kreps and Wilson (1982), Milgrom and Roberts (1982), Fudenberg and Tirole (1991).

An interest group derives utility from spending on a particular preferred good: the “lobby good,”  $z$ . It cannot produce  $z$  directly but can raise amount  $L$  for the local government by exerting effort. The government chooses spending on  $z$  and other goods  $x$ , conditional on  $L$  and other income  $Y$ . I assume no political agency by the government to demonstrate that violations of fungibility are possible even in a framework equivalent to one in which decisions are made by a median voter. I make the strong assumptions that lobby groups are homogeneous and that all lobby goods enter the government utility function in the same way,<sup>10</sup> and I restrict the interest group effort choice to be binary, for simplicity.

The government makes its decision simultaneously with its interest group opponent in each period. If the government receives no interest group funds, it solves the following problem:

$$\max_{z,x} U_G(z, x) \text{ subject to } p_z z + p_x x \leq Y, \tag{1}$$

which yields the optimal choice of goods, denoted as:  $(z^0, x^0)$ . When an interest group chooses to raise funds, it does so with an implicit understanding that the government will provide “payback” by spending the funds on the interest group’s preferred good,  $z$ . Whether payback occurs depends on two factors: the type of the government (Strategic or Committed) and the action chosen by the government (*Reciprocate* or *Reneged*).

Under *Reneged*, the government breaks the implicit contract and treats  $L$  as it would other income, leading to a choice of goods along the government’s income expansion path:  $(z', x')$ . Under *Reciprocate*, the government spends  $L$  only on the lobby good, leading to the choice  $(\hat{z}, \hat{x})$ . The Strategic government chooses between *Reciprocate* or *Reneged*; the Committed government always chooses *Reciprocate*. The government would prefer to allocate  $L$  across all goods and would therefore be better off by renegeing. Interest groups prefer to undertake effort and provide  $L$  if and only if the government *Reciprocates*.

This framework implies the following payoff matrix:<sup>11</sup>

		Government	
		Reciprocate	Reneged
Interest group	Effort	$(a, c)$	$(-b, d)$
	No effort	$(0, 0)$	$(0, 0)$

Since  $c < d$ , the Nash equilibrium of the stage game is (*No effort, Reneged*) yielding payoffs of  $(0, 0)$  even though (*Effort, Reciprocate*) results in higher payoffs  $(a, c)$  for both players.

In a dynamic setting, the government interacts with an interest group in each period. An interest group observes the past actions of the government but not its type. An interest group will put forth effort if it has a sufficiently high belief that the government is Committed, and a Strategic government has incentives to build a reputation for being Committed by paying back interest groups.<sup>12</sup> The goal of the government is to maximize its discounted sum of payoffs. I assume no borrowing or savings; the government must balance the budget in each period.

Intuitively, Strategic governments face the following fundamental trade-off: spending more on the lobby good creates a social welfare loss relative to renegeing and allocating interest group funds across all goods; however, it generates a gain in the form of additional income from interest groups in the future if the government is perceived to be Committed as a result.<sup>13</sup>

In this model, the government faces an interest group, but not necessarily the same interest group, each period. The model is one of general reputation effects, so that by renegeing on one interest group, the government reveals its type to all interest groups. The model therefore predicts violations of fungibility even in cases in which the government may be unlikely to interact with a given interest group repeatedly. The tobacco settlement studied here may appear to be a “one-

<sup>10</sup> Allowing heterogeneity in lobby goods does not alter the basic intuition of the model.  
<sup>11</sup> The zero payoffs in the second row arise from normalizing  $U_G(z^0, x^0)$  and  $U_L(z^0, \text{no effort})$  to zero for simplicity. The payoffs in the first row are then as follows:  $a = U_L(\hat{z}, \text{effort})$ ,  $b = |U_L(z'; \text{effort})|$ ,  $c = U_G(\hat{z}, \hat{x})$ , and  $d = U_G(z', x')$ .  
<sup>12</sup> This setup corresponds to the standard reputation effects framework. Without multiple types, there is no uncertainty for the interest group. As Fudenberg and Tirole (1991) point out, an alternative approach is to model reputations in a repeated game of complete information with trigger strategies. For example, the interest group could provide funds as long as the government has not renegeed on a previous interest group and refuse to provide funds as soon as the government renegees. This approach does not change the set of equilibria and does not capture the idea that reputation corresponds to something the opponents have learned (Fudenberg and Tirole, 1991).  
<sup>13</sup> While altering the definition of the Committed type changes some of the specific empirical predictions of the model, the basic intuition and result remain the same. The key condition is that the Committed type spends more on the lobby good than the Strategic government would choose to spend in a static setting. As long as interest group effort is conditional on a sufficiently high belief that the government is Committed, there exists scope for reputation effects.

shot deal”; however, by renegeing on the interest group involved in raising funds, the government faces a loss of reputation and therefore the loss of funds from other interest groups in the future.

### 2.3. Solution

In the one period case, the Strategic government always *Reneges*, and the interest group provides effort if  $p^0 a - (1 - p^0)b > 0$  where  $p^0$  is the prior probability of a Committed government. This occurs when  $p^0$  exceeds a threshold value:  $p^0 > \frac{b}{a+b} \equiv \bar{p}$ .

In the two period case, the necessary condition for the Strategic government to *Reciprocate* is:

$$\frac{c}{d} > (1 - \delta) \quad (2)$$

where  $\delta$  is the discount factor.<sup>14</sup> The Strategic government is willing to *Reciprocate* in period 1 if doing so induces interest groups to provide funds in period 2. If the condition in Eq. (2) holds, the equilibrium depends on  $p^0$ . If  $p^0 > \bar{p}$ , the government *Reciprocates* in period 1 and interest groups provide effort in both periods. If  $p^0 < \bar{p}$ , the Strategic government *Reciprocates* in period 1 with probability  $\frac{p^0 a}{(1 - p^0 b)}$ . Interest groups are indifferent about providing effort in period 2 and provide effort in period 1 if  $p^0 > \left(\frac{b}{a+b}\right)^2 = \bar{p}^2$ . Solving by induction to the  $N$  period case, the prior probability of a Committed government ( $p_0$ ) required for the interest group to provide funds decreases in  $N$  geometrically at the rate  $\left(\frac{b}{a+b}\right)$ .<sup>15</sup>

This model implies a positive probability of the government reciprocating, thereby spending more on  $z$  when it receives a grant than if it followed the income expansion path, as long as the necessary condition given in Eq. (2) holds. The model thus predicts systematic violations of fungibility across spending categories ( $\frac{\partial z}{\partial L} > \frac{\partial z}{\partial Y}$  in expectation).<sup>16</sup> In this case, apparent flypaper effects represent governments honoring the implicit contract for the long term benefit of being perceived to be Committed. This option is only chosen if it is beneficial to constituents, and so violations of fungibility in this model are welfare improving for constituents.

### 2.4. Alternative models

One alternative theory is that governments spend lobby money on the lobby good because they fear voter punishment if they behave otherwise. Such a model, however, would require either behavioral preferences on the part of voters or a framework in which spending money on the lobby good provides a costly signal of some other characteristic voters care about. Another alternative is a bargaining model between interest groups and politicians. This type of model would need to explain why interest groups are more willing or able to punish the local government when the funds are for “their” good.

I now test some of the predictions of the special interest group model by examining state responses to funds received under a settlement agreement with the tobacco industry.

## 3. Background on the tobacco Master Settlement Agreement

### 3.1. History

The Master Settlement Agreement represents the culmination of lawsuits filed by states against the tobacco industry in the mid-1990s. More than 40 states brought suit against tobacco companies, alleging that these companies violated

<sup>14</sup> The Strategic government can *Reneg*e in period 1, revealing its type. The total payoff to the government is then  $d + 0$ . The government can also *Reciprocate* in period 1 to build a reputation for commitment. If doing so causes the interest group to provide effort in period 2, the government gets a total payoff of  $c + \delta d$ .

<sup>15</sup> Further details about this class of reputation models are available in section 9.2 of Fudenberg and Tirole (1991).

<sup>16</sup> Violations of fungibility are more likely when  $\delta$  is high (more weight is given to future periods), holding the other parameters fixed. Eq. (2) also shows that for a given  $\delta$ , the probability that the government *Reciprocates* is increasing in  $\frac{c}{d}$ , the ratio of social welfare when the government *Reciprocates* to social welfare when the government *Reneges*. This implies:  $\text{prob}\left(\frac{\partial z}{\partial L} > \frac{\partial z}{\partial Y}\right) = f\left(\frac{c}{d}\right)$ , where  $f' > 0$ . We should be more likely to observe violations of fungibility when there are low costs of misallocating toward the lobby good relative to pursuing the socially optimal spending path.



Table 1

Panel A: summary statistics: settlement revenues (FY 2002)

	Dollars (000s)	Dollars per capita	Dollars per 1998 smoker
Min	13,800	10.25	56.74
Median	68,300	21.55	89.90
Mean	101,000	21.97	98.26
Max	361,000	37.20	172.13
N	46	46	46

Panel B: summary statistics: tobacco prevention and control program spending

	FY 1998	FY 1999	FY 2001	FY 2002
Min	0	0	0	0
Median	0	0	2.30	2.78
Mean	0.52	0.49	3.34	3.21
Max	6.88	6.51	21.26	18.10
N	46	46	44	44

Notes: All figures in 2002 dollars. Settlement revenue figures include both annual and initial payments. Tobacco control spending data for fiscal years 1998 and 1999 compiled by author; data for fiscal years 2001 and 2002 come from CDC (2001, 2002).

consumer protection and antitrust laws, concealed information about their products, manipulated nicotine levels in cigarettes in order to increase their addictiveness, and conspired to keep less addictive products off the market. States sought reimbursement from the tobacco industry for expenditures on tobacco-related illness.

A settlement proposal was negotiated by state attorneys general and the tobacco industry in 1997. The settlement was then proposed as Congressional legislation that would have been binding for all states. However, the bill was voted down in June of 1998. During this period, Florida, Minnesota, Mississippi and Texas negotiated independent settlement agreements with the tobacco industry. After the failure of the bill, several states began negotiations with tobacco companies to reach a joint settlement agreement that would not require Congressional approval. These efforts were successful, and in November of 1998, the remaining 46 states settled jointly under the Multistate Master Settlement Agreement. The primary condition of the settlement agreement is the requirement that the tobacco industry transfer large amounts of money to the states annually in perpetuity.<sup>17</sup>

To be eligible to receive funds, each state was required to obtain approval of the settlement from its state court, a process known as achieving state-specific finality. The first payments to the states were disbursed when 80% of the states whose shares equaled 80% of total payments reached state-specific finality. This occurred in November 1999, and the first payments were released the following month. Settlement revenue is unrestricted and the allocation mechanism and use of funds are left entirely to the discretion of the states.

### 3.2. Payments

States receive three types of payments under the settlement: (1) initial payments, paid in five installments from 1999 to 2003; (2) annual payments, paid in perpetuity; and (3) Strategic Contribution Fund payments meant to compensate states for the costs incurred in state lawsuits, paid from 2008 to 2017. The two major adjustments made to annual settlement payments are an inflation adjustment and a volume adjustment. Annual payments increase by the CPI or 3%, whichever is higher. The volume adjustment is based on increases or decreases in the number of cigarettes shipped nationally relative to a base volume. The volume adjustment is not state-specific. Initial payments are subject to the volume adjustment but not the inflation adjustment. At the time of the settlement, total *unadjusted* payments made to settling states under the agreement through 2025 were projected to be almost \$206 billion, or \$120 billion in present value terms using a discount rate of 4%.

Table 1, Panel A provides a summary of settlement disbursements to states in fiscal year 2002. The average amount of revenue a state receives is \$100 million annually, which corresponds to \$22 per capita and \$100 per smoker.

<sup>17</sup> The settlement also placed strong restrictions on tobacco advertising practices. For the full text of the settlement agreement see: [http://naag.org/upload/1032468605\\_cigmsa.pdf](http://naag.org/upload/1032468605_cigmsa.pdf).

Initial and annual payments are distributed among the states according to fixed state allocation percentages. Base allocation percentages are calculated using a formula that equally weights two factors: the state's share of total direct medical costs related to smoking and the state's share of smoking-attributable Medicaid expenditures (Modisett, 1997).

Total direct medical costs related to smoking represents smoking-related health costs incurred by all payment sources in a state in 1990. Smoking-attributable Medicaid expenditures represents the amount of a state's Medicaid expenditures directly attributable to smoking and to illnesses associated with smokeless tobacco use for individuals over 18 in 1993.<sup>18</sup> Two adjustments were made to direct medical costs: figures were multiplied by 1.28 to reflect inflation in medical costs between 1990 and 1993 and Medicaid costs were then subtracted to prevent double counting of these expenditures. The percent of the total settlement amount allocated to state  $i$  is then given by the following formula:

$$\text{percent}_i = 0.5 * \left( \frac{\text{SMCD}_i}{\sum_i \text{SMCD}_i} \right)_{1993} + 0.5 * \left( \frac{\text{AdjDMC}_i}{\sum_i \text{AdjDMC}_i} \right)_{1993} \quad (3)$$

where  $\text{SMCD}_i$  and  $\text{AdjDMC}_i$  are the smoking-related Medicaid costs and the adjusted direct medical costs for state  $i$ .

Negotiations among states at the time of the settlement resulted in some small adjustments to these base percentages. Table 2 illustrates the allocation percentages as they would have been had the above formula been followed as well as the actual percentages under the settlement. Differences between the simulated and actual allocation percentages may not be completely random (it is unlikely to be a coincidence that California and New York receive exactly the same shares) but are generally very small. The coefficient of correlation between the two is 0.99, and proxying for actual settlement revenues using the simulated allocation percentages does not affect the results.

The size of a state's windfall in a given year is then the aggregate annual payment, determined under the terms of the settlement, multiplied by its allocation percentage. Allocation percentages were fixed at the time of the settlement agreement, so states' spending decisions do not affect future revenues.<sup>19</sup> Counties in New York and California receive a share of state settlement payments directly since counties in these states bear a share of Medicaid costs. New York and California state governments therefore receive 51% and 50% of their total state allocations, respectively.<sup>20</sup>

Smoking-attributable Medicaid and other health care costs in 1993 are the only systematic determining factors of state settlement revenue receipt. These two factors alone account for over 99% of the variation in settlement revenues if New York and California state revenues are not adjusted for direct payments to counties and over 90% if revenues are adjusted (Table 3).<sup>21</sup> Running the regression in per capita terms gives an  $R^2$  of 72% when New York and California revenues are not adjusted and 53% when revenues are adjusted. Per capita settlement revenues are orthogonal to a variety of other potentially relevant state characteristics (column 6). Controls for state income per capita, an indicator for whether the state had a large pre-existing tobacco control program, the share of the state population under 18 and the conservativeness of the state as measured by Republican vote share in the 2000 presidential election are all insignificant.

## 4. Empirical methodology and data

### 4.1. Testing the main prediction of the interest group model

The interest group model predicts that when interest groups are instrumental in procuring funds, governments will spend these funds disproportionately on the interest group's preferred goods. In the case of the tobacco

<sup>18</sup> The population of each state was categorized into non-smokers, current smokers, former smokers with less than 15 years exposure and former smokers with greater than 15 years exposure. The effect of type of exposure on each smoking-related medical condition and then the level of expenditure was estimated as a function of smoking, medical conditions and health status. The costs do not reflect lifetime medical care costs but rather medical care costs paid for by all sources per year. Models controlled for age, race/ethnicity, poverty status, marital status, education, medical insurance, region, seat-belt use and obesity. See Modisett (1997) for further details on calculations.

<sup>19</sup> State spending on tobacco control programs could affect future revenues in an extremely indirect way through the national volume adjustment. However, this effect would bias *against* spending on such programs.

<sup>20</sup> The direct payment of a share of settlement revenues to counties in these states raises an interesting set of questions about the response of state governments to increased county revenues. I do not address these issues here.

<sup>21</sup> Coefficients differ from 0.5 because the regressions are run on the levels of smoking-related health costs rather than the shares.

Table 2  
State allocation percentages

State	Actual (%)	Simulated (%)
Alabama	1.62	1.59
Alaska	0.34	0.25
Arizona	1.47	1.02
Arkansas	0.83	0.94
California	12.76	11.25
Colorado	1.37	1.39
Connecticut	1.86	2.03
Delaware	0.40	0.31
Georgia	2.45	2.62
Hawaii	0.60	0.45
Idaho	0.36	0.28
Illinois	4.65	5.23
Indiana	2.04	2.40
Iowa	0.87	0.95
Kansas	0.83	0.89
Kentucky	1.76	2.02
Louisiana	2.26	2.35
Maine	0.77	0.84
Maryland	2.26	2.37
Massachusetts	4.04	4.13
Michigan	4.35	4.43
Missouri	2.27	2.22
Montana	0.42	0.34
Nebraska	0.59	0.53
Nevada	0.61	0.51
New Hampshire	0.67	0.59
New Jersey	3.87	3.97
New Mexico	0.60	0.50
New York	12.76	13.91
North Carolina	2.33	2.50
North Dakota	0.37	0.30
Ohio	5.04	5.48
Oklahoma	1.04	1.15
Oregon	1.15	1.08
Pennsylvania	5.75	5.58
Rhode Island	0.72	0.66
South Carolina	1.18	1.18
South Dakota	0.35	0.27
Tennessee	2.44	2.50
Utah	0.44	0.37
Vermont	0.41	0.31
Virginia	2.04	2.25
Washington	2.05	2.10
West Virginia	0.89	1.01
Wisconsin	2.07	2.16
Wyoming	0.25	0.14

Notes: Simulated figures calculated from formula after removing shares of non-settlement states. Correlation: 0.99.

settlement, lawsuits were orchestrated largely by state attorneys general with substantial involvement by anti-tobacco and health organizations. A large body of anecdotal evidence indicates that these groups felt that settlement dollars should be spent on tobacco prevention and control programs. The following quote is typical:

*“A compassionate but naïve person would expect the states to use their \$246 billion [sic] windfall to try to prevent more people from suffering and dying from cancer, emphysema or other smoking-related illnesses. If this is blood money, why not try to stop the bleeding? Ah, but the greedy deal makers in our state capitals have*



Table 3  
Determinants of state settlement revenue (FY 2002)

	Settlement revenue		Settlement revenue per capita			
	(1)	(2)	(3)	(4)	(5)	(6)
Smoking-attributable Medicaid	0.534**(0.036)	0.154(0.079)				
Smoking-attributable other health	0.103**(0.005)	0.081**(0.010)				
Smoking attr. Medicaid per cap			0.625**(0.105)	0.404**(0.133)	0.387**(0.133)	0.391**(0.142)
Smoking attr. other health per cap			0.067**(0.022)	0.085**(0.027)	0.103**(0.033)	0.119**(0.040)
State income per cap					-2.01e-04 (1.92e-04)	-1.38e-04 (2.26e-04)
Pre-settlement program						-1.437(2.411)
Youth share (2000)						0.016(0.572)
Republican vote share (2000)						0.094(0.120)
Observations	46	46	46	46	46	46
R-squared	0.992	0.901	0.716	0.530	0.542	0.557

Notes: Smoking-attributable Medicaid and smoking-attributable other health correspond to  $SMCD_i$  and  $AdjDMC_i$  in the allocation formula. Columns 1 and 3 do not adjust New York and California state settlement revenues for payments made directly to counties. Youth share comes from the 2000 census and measures the percentage of the state's population under 18. \* significant at 5%; \*\* significant at 1%.

*other plans for the money... I'm talking about construction projects. Paying bills, new non-medical programs... Most of this spending would be fine if it came out of state tax revenue, but ... this money should not be poured into general funds. It should be used to help prevent and cure disease.* — Judy Jarvis, radio host and lung cancer victim (New York Times, 1999).

I therefore focus my analysis on state spending on tobacco prevention and control programs. Fig. 1 illustrates the number of states allocating substantial funds toward such programs over time. Although the settlement agreement was reached in 1998, states first received funds in the middle of the 2000 fiscal year. The number of states spending at least \$0.50 per capita on tobacco control programs increased almost six-fold from six states in fiscal year 1999 to thirty-four states in fiscal year 2001. The five states with substantial programs prior to the settlement<sup>22</sup> funded their programs primarily through increases in excise taxes on cigarettes. The remaining states allocated virtually no state funds toward such programs prior to the settlement (Fig. 2). Among these (non-prespending) states, mean per capita spending increased from only \$0.04 in 1999 to \$2.78 in the year after settlement funds were received. Despite displaying virtually no preference for spending on tobacco control programs through the mid-to-late 1990s (a period of substantial budget surpluses for most states), all but one of the non-prespending states had instituted such a program by fiscal year 2002.<sup>23</sup> States with pre-existing programs also responded to settlement revenues, increasing spending from an average of \$4.15 per capita in fiscal year 1999 to \$7.67 in fiscal year 2001.

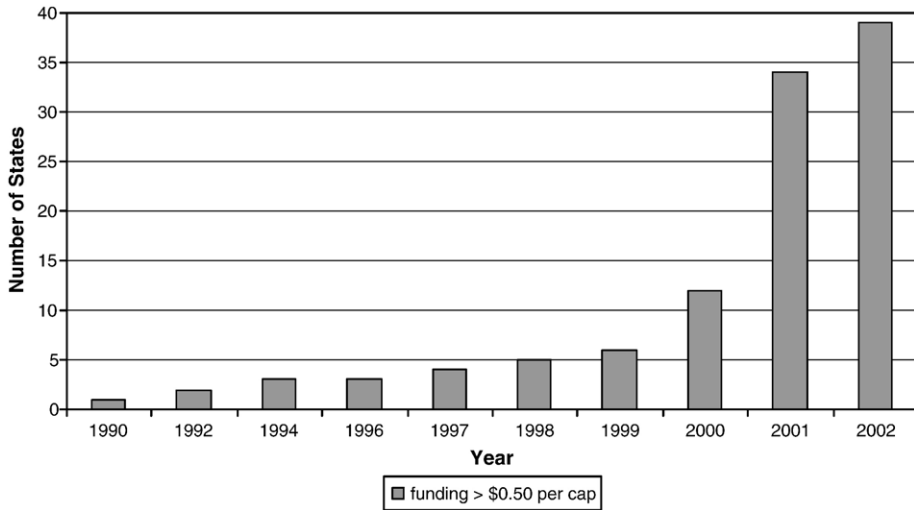
At this point, I pause to consider the role of the tobacco industry in the framework of the model. The tobacco industry lobby certainly exists and would likely have preferences over spending on tobacco prevention and control programs that directly oppose the preferences of public health groups. However, in the context of this model, governments reward only those groups that raise money for the state. Since the tobacco lobby did not provide funds (and, in fact, tried to reduce settlement payments), governments do not have incentives to take the preferences of this group into account when making spending decisions.<sup>24</sup>

In the next sections, I test the predictions of the interest group model more formally.

<sup>22</sup> Arizona, California, Maine, Massachusetts and Oregon.

<sup>23</sup> The only state not allocating state funds toward tobacco prevention and control by fiscal year 2002 was Tennessee.

<sup>24</sup> Some have argued that the settlement payments did not adequately compensate states and that the tobacco industry was the “winner” in the settlement. The actions of the tobacco industry may have resulted in settlement windfalls being lower than they otherwise would have been. However, this does not change the prediction that governments should still spend these funds on the preferred good of the public health groups. There is certainly an interesting set of issues surrounding the determination of  $L$ ; I focus here on understanding how  $L$  is spent once it is received.



Notes: This figure illustrates spending for the 46 states that were involved in the settlement. Data for 1990, 1992 and 1994 come from surveys conducted by the American State and Territorial Health Officials (ASTHO). Data for 1996-2000 were collected by author. Data for 2001-2002 come from the CDC.

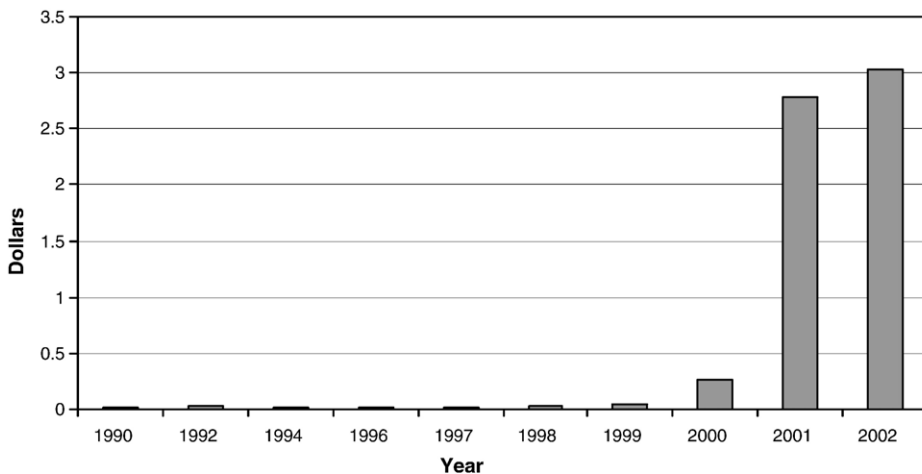
Fig. 1. Settlement states allocating funds for tobacco control.

4.2. Econometric specification

The empirical strategy allows me to test for violations of fungibility in two ways, exploiting both the time series and cross-sectional variation in settlement revenue receipt. Consider the following regression framework:

$$\text{Tobacco Control}_{it} = \alpha_0 + \alpha_1(\text{Srev}_{it} - \overline{\text{Srev}_t}) + \alpha_2(\text{Inc}_{it} - \overline{\text{Inc}_t}) + \gamma_t + \phi_i + \delta X_{it} + \varepsilon_{it} \tag{4}$$

where Srev is settlement revenue, Inc is state income,  $\gamma_t$  is a set of year dummies,  $\phi_i$  is a set of state dummies and  $X_{it}$  is a set of time-varying state controls. In this case, both  $\alpha_1$  and the  $\gamma_t$ 's have causal meaning and can be used to test for



Notes: All figures given in 2002 dollars. Excludes Arizona, California, Maine, Massachusetts and Oregon. Data for 1990, 1992 and 1994 come from surveys conducted by the American State and Territorial Health Officials (ASTHO). Data for 1996-2000 were collected by author. Data for 2001-2002 come from the CDC.

Fig. 2. Mean per capita spending on tobacco control programs (excluding states with large pre-existing programs).

violations of fungibility. The  $\gamma_t$  coefficients indicate whether states spent on the lobby good when they received lobby funds and  $\alpha_1$  indicates whether states that received more lobby money spent more on the lobby good.<sup>25</sup> Formally, coefficients on the time dummies pick up changes in tobacco control spending within a state over time; the first test is whether there exists a discontinuity in spending at the time of settlement revenue receipt. The second test is whether the marginal propensity to spend on tobacco control from settlement revenues is higher than the marginal propensity to spend from state income. The relevant test is  $\alpha_1 > \alpha_2$  (rather than  $\alpha_1 > 0$ ) to distinguish the income effect component of settlement revenue receipt from a true fungibility effect.

There are two primary identification assumptions. The identifying assumption for  $\gamma_t$  is that spending on tobacco control programs would not have changed from the fiscal year before funds were received to the fiscal year after in the absence of receipt of settlement funds. The identifying assumption for  $\alpha_1$  is that the size of a state's settlement windfall is orthogonal to other state characteristics that might influence spending on tobacco control programs. The allocation formula does reflect the historical costs of smoking in each state, raising the potential concern that settlement revenue is proxying for the need or desirability of spending money on such programs. I test both identifying assumptions in Section 5.2.

### 4.3. Data

The last comprehensive surveys of state tobacco control spending prior to the settlement were conducted by the Association for State and Territorial Health Officials (ASTHO) in 1994. Data on state spending for fiscal years 1996–2000 were collected by the author from individual states. As illustrated in Figs. 1 and 2, only five states had substantial tobacco control programs prior to the receipt of settlement funds; the remaining states spent virtually nothing.<sup>26</sup> I exclude fiscal year 2000 data from my analysis since states first received funds in the middle of this fiscal year.

Data on state tobacco control funding after settlement funds were received comes from two sources. The primary data source is a series of *State Highlights Reports* published by the Centers for Disease Control (CDC, 2001, 2002). Reported state allocations include funding specifically appropriated to any governmental agency, foundation, trust fund, board or university for tobacco control programs. They do not include funds directed toward tobacco research, health services, tobacco farmers or tobacco dependent communities (CDC, 2001). In a few cases, appropriations were made for multiple fiscal years at once or revenues were set aside in trust funds. The CDC includes the full appropriation amount in the year in which it was allocated.

A secondary data source is information on allocation of tobacco settlement revenue compiled by the National Conference of State Legislatures (NCSL, 2001, 2002, 2003). Reported state allocations include funds for community and school-based tobacco-use prevention programs, media campaigns, tobacco control measures and tobacco cessation treatment (NCSL, 2002). The major advantage of the NCSL data is that they contain allocations for fiscal years 2003 and 2004, whereas CDC data are currently limited to fiscal years 2001 and 2002. The main drawback is that NCSL data include only tobacco control spending from settlement revenues. Data from the two sources are close in most cases, but NCSL data underreport spending in states where settlement revenues were not the only funding source for tobacco control programs. In addition, NCSL data do not include money set aside in endowment funds. Both data sources reflect appropriations for spending related to tobacco control at the beginning of the fiscal year and may differ from actual expenditures. I use CDC data whenever possible and supplement the analysis with NCSL data as a specification check and also in cases in which adding additional years of data is especially useful. The two data sources produce almost identical results.

<sup>25</sup> Settlement revenue and income are demeaned only so that  $\alpha_1$  and the  $\gamma$ 's can be correctly estimated in the same regression. The  $\gamma$ 's are meant to measure the average increase in spending when settlement funds are received; this cannot be done when the level of settlement funds is held constant. This choice of specification does not affect the substantive results: The settlement revenue coefficient is almost identical to that obtained regressing spending on the levels of settlement revenue and income in the post-settlement period (the only period for which there is cross-sectional variation in settlement revenue) and to that obtained using a de-meaned left hand side variable. The  $\gamma$  coefficients are almost identical to those obtained regressing spending on time dummies without controlling for settlement revenue.

<sup>26</sup> Data for Alaska, Louisiana and Maryland were not obtainable for the 1996–2000 fiscal years. These states spent nothing in the 1990, 1992 and 1994 ASTHO surveys. I therefore assign them zero spending for 1996–1999. My finding that almost all states spent virtually nothing on tobacco prevention and control programs prior to the receipt of settlement funds is confirmed by numerous sources including CDC (1999) and Farrelly et al. (2001).

Table 4  
Per capita spending on tobacco control

	(1)	(2)	(3)	(4)
Settlement revenue per capita	<b>0.184** (0.042)</b>	<b>0.210** (0.045)</b>	<b>0.187** (0.043)</b>	<b>0.214** (0.046)</b>
State income per capita	2.87e-05 (5.02e-05)	-4.80e-04 (3.42e-04)	2.73e-05 (5.05e-04)	-4.93e-04 (3.45e-04)
After	<b>2.927** (0.409)</b>	<b>3.071** (0.318)</b>		
FY=1999			-0.031 (0.572)	-0.031 (0.442)
FY=2001			3.107** (0.583)	3.174** (0.456)
FY=2002			2.720** (0.579)	2.943** (0.449)
Constant	0.507 (0.285)		0.523 (0.405)	
State fixed effects	No	Yes	No	Yes
Observations	180	180	180	180
R-squared	0.274	0.447	0.276	0.448

Notes: Per capita settlement revenue and income are measured in real 2002 dollars. Settlement revenue per capita and income per capita are demeaned. Years: 1998, 1999, 2001 and 2002. Fiscal year 2000 excluded because settlement revenues were received in the middle of this fiscal year. \* significant at 5%; \*\* significant at 1%.

Settlement revenues received by states were tabulated by the National Association of Attorneys General (NAAG) and reflect the amount disbursed to each state in a given fiscal year.<sup>27</sup> The state population and income come from the Bureau of Economic Analysis. The calendar year population and income are counted for the following fiscal year (for example, the 1990 population is counted for the 1991 fiscal year).

## 5. Results

### 5.1. Main findings

Table 1, Panel B provides summary statistics on per capita tobacco control program allocations for fiscal years 1998, 1999, 2001 and 2002.<sup>28</sup> The mean amount allocated toward tobacco control after receipt of settlement funds is a little more than \$3.00 per capita. There is substantial variation in allocation amounts across states.

I test for violations of fungibility by estimating Eq. (4). In the baseline specification, I replace the time dummies with an indicator, *After*, which is equal to 1 for fiscal years 2001 and 2002. I find strong evidence that states violate fungibility in spending decisions as predicted by the interest group model (Table 4). Column 1 gives the results when state fixed effects are not included.  $\alpha_1$ , the propensity to spend from settlement revenue, is 0.18 and significant at the 1% level;  $\alpha_2$ , the propensity to spend out of income, is essentially zero and insignificant. The average increase in spending upon receipt of settlement funds,  $\gamma$ , is 2.93 and also significant at the 1% level. This represents an almost six-fold increase in spending. Adding state fixed effects does not affect the coefficient estimates, as shown in Column 2.  $\alpha_1$  increases to 0.21 and  $\gamma$  to 3.07. In columns 3 and 4, I replace the indicator for *After* with a full set of time dummies. It is clear that the effect is being driven by a discontinuity at the time of settlement fund receipt; there is almost no change in spending from fiscal year 1998 to 1999 or from fiscal year 2001 to 2002. Including earlier years or using earlier years as the base years does not change the results. An *F*-test rejects equality of  $\alpha_1$  and  $\alpha_2$ . The *F*-test statistic is close to 20 across all specifications in Table 4.

Both effects are large in magnitude. The increase in spending from fiscal year 1999 to 2001 at the mean income and mean per capita settlement revenue level (approximately \$24) is \$3.00. Taking a propensity to spend of 20 cents per dollar of settlement revenue, this implies that per capita spending at the minimum level of settlement revenue receipt (\$11) would be \$0.40 and spending at the maximum level of receipt (\$40) would be over \$6.00.

While the model in Section 2 defined reciprocating as dollar for dollar spending for simplicity, the only real necessary condition for the incentives in the model to hold is that governments spend “enough” on the lobby good for interest groups to feel that the government has reciprocated. Although it is difficult to define this amount, we can say that a government that has reneged should spend only according to the income expansion path, which in this case implies zero spending.

<sup>27</sup> Arkansas and Missouri did not immediately achieve state-specific finality. Their settlement disbursements for fiscal 2001 therefore reflect both fiscal 2000 and 2001 payments. I exclude 2001 data for these two states in all analyses. State specific allocation data is not available for 2003 and 2004. I therefore inflate the 2002 figures, which if anything should overstate allocation amounts.

<sup>28</sup> Data are not available for Arizona and Massachusetts for fiscal year 2002.

There is certainly no incentive for a government to allocate funds to these programs but in an amount insufficient to be perceived as Committed. We can therefore interpret the observed spending as governments reciprocating.

In addition, while a marginal effect of 20 cents on the dollar may first appear small relative to existing flypaper estimates, it is quite large given the context of this particular experiment. Settlement revenues received by states are generally much larger than the amount that could be feasibly spent on tobacco prevention and control programs. Dollar-for-dollar spending might not be reasonable in this case.<sup>29</sup>

Another possible theory relevant in this particular context is that voters may have learned about the costliness of smoking during the course of the lawsuits.<sup>30</sup> However, states did not increase spending on tobacco control programs during the lawsuits or even after the settlement agreement was reached; spending increased only after the receipt of settlement funds. This discontinuity is difficult to reconcile with a learning story. In addition, I show in the next section that factors which we might expect to influence spending if states truly learned about the costliness of smoking, such as smoking prevalence or youth smoking rates, have no effect on spending decisions.

While these baseline findings alone do not provide direct evidence in favor of the interest group model, the results are not consistent with existing alternative models. As discussed, the models of Chernick (1979) and Knight (2002) would not predict violations of fungibility in this case, since grants do not have matching incentives and are not endogenous to spending preferences. Models arguing that agenda-setting politicians are able to hide grants from voters would predict higher overall spending from these windfalls than from increases in constituent income but would not predict the observed disproportionate spending on tobacco prevention programs in particular. These findings cannot be explained simply by a difference in the overall propensity to spend.<sup>31</sup> Other existing models outlined in the introduction (Inman, 1979; Oates, 1979; Turnbull, 1992; Fisher, 1982; Hamilton, 1986) similarly predict higher spending from grants than from increases in constituent income, but would not predict that settlement revenues be spent specifically on tobacco prevention programs.

I have also examined the effects of settlement revenue receipt on other state spending categories (health, welfare, other cash assistance, primary and secondary education and total) and on personal and corporate income tax revenue. I find significant increases in health, education and total spending as well as a decline in corporate tax revenue between 1999 and 2001. However, I am not able to separate the effects of settlement windfalls from other time varying factors, such as overall state fiscal health. States exhibit positive propensities to spend on all spending categories, and increased settlement revenue is also associated with lower personal and corporate tax revenues. The only significant effect is on education spending, and this effect is implausibly large (coefficient estimate of 3.98). The lack of precision may reflect the fact that while variation in settlement windfalls is large, it is still small relative to variation in state budgets and large spending and revenue categories.

## 5.2. Testing the identifying assumptions

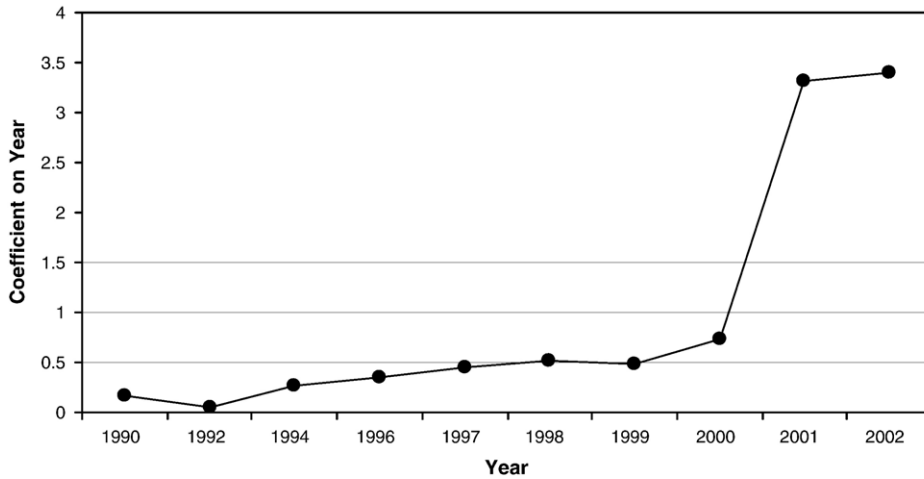
The identification assumption for the *After* coefficient is a constant underlying time trend. Fig. 3 illustrates a plot of the time coefficients obtained from regressing per capita tobacco control spending on year dummies with state fixed effects for years 1992–2002. It seems clear that the time trend prior to the settlement was flat and that receipt of settlement funds is the key driving factor behind the increase in spending from fiscal year 1999 to fiscal year 2001.

The identification assumption for the settlement revenue coefficient is that settlement revenues are not proxying for other state characteristics that might influence tobacco control spending. Such factors cannot explain the discontinuity

<sup>29</sup> Also, the marginal propensity to spend on tobacco control programs from state income or state government revenue prior to the settlement was essentially zero. The existing flypaper effect literature tends to report the propensity to spend out of grants for programs such as health or education without considering the magnitude of the effect relative to the marginal propensity to spend on those goods from income or state revenue. This raises difficulties when attempting to compare the magnitude of flypaper across different spending categories.

<sup>30</sup> This theory could generalize to other contexts if voters believe the timing or magnitude of categorical grants carry real information about the desirability of spending in that category.

<sup>31</sup> Suppose politicians are perfectly able to hide grants from voters and retain the full amount of the windfall in the government budget. We would then expect the propensity to spend on tobacco prevention from windfalls to be equal to the propensity to spend from government revenues. However, governments displayed essentially no propensity to spend on these programs prior to the settlement from government revenues or income, and we do not observe, for example, increased spending on such programs during the state budget surpluses of the 1990s. In addition, one might argue that this type of spending is particularly visible to voters, since funds were not previously allocated to such programs and since spending on these programs was tracked by a variety of organizations, which is difficult to reconcile with a “hiding funds” story.



Notes: All figures given in 2002 dollars. Data for 1990, 1992 and 1994 come from surveys conducted by the American State and Territorial Health Officials (ASTHO). Data for 1996-2000 were collected by author. Data for 2001-2002 come from the CDC.

Fig. 3. Test of *After* coefficient identification assumption regression of per cap tobacco control spending on year dummies with state fixed effects.

in spending over time, but must be addressed when using cross-sectional variation in settlement payments across states, particularly since the settlement revenue formula is a function of smoking-related factors.

I test this identification assumption by adding controls for measures of the need for tobacco control programs using data from the post-settlement period with an indicator for whether the state had a large pre-existing program (Table 5). I control for the percent of the state population that smoked in 1998, state-specific minimum spending guidelines

Table 5  
Test of settlement revenue identification assumption

	(1)	(2)	(3)	(4)
Settlement revenue per cap	<b>0.199** (0.070)</b>	<b>0.199* (0.090)</b>	<b>0.255+ (0.148)</b>	<b>0.206** (0.071)</b>
State income per cap	-5.30e-06 (9.48e-05)	3.56e-07 (1.04e-04)	-1.37e-04 (1.55e-04)	-1.12e-04 (1.31e-04)
Pre-settlement program	4.069** (1.411)	4.184** (1.402)	6.072** (2.083)	3.067 (1.966)
FY=2002	-0.218 (0.467)	-0.222 (0.522)	-0.605 (0.871)	-0.269 (0.490)
% pop smokes (1998)	-0.053 (0.176)			-0.090 (0.172)
CDC recommended min per cap		-0.013 (0.257)		
Youth smoking rate (1997)			-0.160 (0.170)	
Region=South				0.831 (1.510)
Region=Midwest				2.931 (2.146)
Region=West				2.510 (2.063)
Youth share (2000)				-0.133 (0.293)
Republican vote share (2000 press)				-0.142+ (0.077)
Constant	4.417 (4.138)	3.278 (2.548)	9.234 (6.620)	14.388 (10.622)
Observations	88	88	43	88
R-squared	0.221	0.219	0.270	0.321

Notes: Per capita settlement revenue and income are measured in real 2002 dollars. Settlement revenue per capita and income per capita are de-meaned. Years: 2001, 2002. The percent of population that smokes comes from the Centers for Disease Control; the youth smoking rate comes from the Youth Tobacco Surveys (YTS). The CDC recommended min per cap comes from a 1999 CDC report that put forth “best practices” state-specific spending guidelines. This variable measures the CDC’s estimate of the minimum spending required for each state to implement a comprehensive and effective tobacco control program. The full report is available at: <http://www.cdc.gov/tobacco/bestpract.htm>. Youth share comes from the 2000 census and measures the percentage of the state’s population under 18. All columns are pooled OLS with state-clustered standard errors. + significant at 10%, \* significant at 5%; \*\* significant at 1%.



recommended by the Centers for Disease Control, and the state youth smoking rate in 1997. These measures have no significant direct effects, no effect on the settlement revenue coefficient, and no additional explanatory power.<sup>32</sup> The results are virtually unchanged when controls for region, youth share and conservativeness of the state are added (column 4).<sup>33</sup>

### 5.3. Robustness checks and persistence over time

I perform two additional robustness checks: I substitute actual settlement revenues with simulated figures reflecting the recommended allocation formula and I use NCSL data on tobacco control spending rather than CDC data. Both strategies give results that are almost identical to the baseline results.<sup>34</sup>

I also test whether the effects are persistent or diminish over time by including NCSL data on fiscal year 2003 and 2004 allocations. Again, NCSL data tabulates payments on tobacco control programs only from settlement revenues and is therefore a biased measure of spending. The advantage is the availability of additional years of data, necessary for analyzing the dynamics of the effect. I find that average and marginal propensities to spend are strongly persistent, at least into the fifth year of fund receipt.<sup>35</sup>

In recent years, a number of states have securitized at least some of their settlement funds, issuing bonds backed by the future stream of revenues. The model does not predict that states should not securitize *per se*, but rather that states should spend on tobacco control programs if a lobby with preferences for such spending has successfully raised funds. Therefore, if securitization states maintain funding on these programs after securitization, they would not be considered to have reneged. Using NCSL data, I find no evidence of a decline in spending between fiscal year 2001 (a time when almost no states had securitized) and fiscal year 2004 for eventual securitization states.

### 5.4. Interest group effort

The model I have proposed argues that governments will spend on the lobby good in order to pay back interest groups that exerted effort to procure funds. In the case of the tobacco settlement, interest groups in some states were involved in lawsuits leading up to the settlement. Anti-tobacco and public health groups and activists provided expert testimony, produced “anti-smoking” reports, supported the development of lawsuit strategies, and engaged in grassroots campaigns against the tobacco industry. Other states simply signed on to the final settlement, receiving windfalls without effort by interest groups. Since these governments do not have interest groups to pay back, we should expect them to treat settlement funds as they would other state income. Ideally, this prediction would be tested by constructing state-specific measures of involvement or costs incurred by anti-tobacco groups prior to the settlement; unfortunately, the necessary data are not available. In the absence of direct measures of interest group effort, I use lawsuit filing as a proxy.

I test the prediction empirically by constructing an indicator equal to one if the state did not file a lawsuit prior to the settlement.<sup>36</sup> Interacting this indicator with settlement revenue and the *After* indicator, I find that states that did not file lawsuits spent less than states that filed (Table 6). States that filed lawsuits increased average spending by \$3.45 after

<sup>32</sup> The statistical significance of the settlement revenue coefficient drops to the 10% level when the youth smoking rate is added as a control as a result of reductions in sample size; comparable state-level data on youth smoking is only available for half of the settlement states.

<sup>33</sup> The possibility remains that states do care about these factors, but in some nonlinear function that is captured by the settlement revenue variable. I therefore examine the direct effect of a variety of indicators of the costliness of smoking without including settlement revenue in the regression. Smoking prevalence, state-specific minimum spending guidelines recommended by the Centers for Disease Control and youth smoking rates have no significant effects on tobacco control spending.

<sup>34</sup> Both checks result in only slight reductions in the marginal propensity to spend from settlement revenue: the coefficient on per capita settlement revenue is 0.18 when simulated settlement figures are used and 0.19 when NCSL data are used. Estimates are significant at the 1% level in both cases. The *After* coefficient drops to 2.05 when the NCSL data are used since these data underestimate spending in the post-settlement period, particularly for states with pre-existing programs in which substantial funding comes from non-settlement revenues. However, despite the downward bias, the coefficient is still large in magnitude and significant at the 1% level.

<sup>35</sup> Spending at the average levels of per capita settlement revenue and income in fiscal years 2001–2003 is almost identical. Spending falls somewhat in fiscal 2004, but the difference is not statistically significant. The propensity to spend from settlement revenues is higher in fiscal year 2002 relative to the other fiscal years, but there is no systematic decrease in spending propensity over time. In fiscal 2004, spending at the average levels of settlement revenue and income is \$1.37 higher than in fiscal 1999 in real terms, and the propensity to spend is 0.16. Both effects are significant at the 1% level.

<sup>36</sup> The following states did not file lawsuits: Alabama, Delaware, Kentucky, North Carolina, Tennessee, Virginia and Wyoming.

Table 6  
Interest group effort

	(1)	(2)
Settlement revenue per capita	0.227**(0.048)	0.228**(0.049)
(No lawsuit)*(Settlement revenue per cap)	<b>-0.083(0.135)</b>	<b>-0.070(0.157)</b>
(Tobacco state)*(Settle revenue per cap)		-0.205(0.221)
After	3.449**(0.347)	3.454**(0.353)
(No lawsuit)* After	<b>-2.019*(0.810)</b>	<b>-1.577(0.991)</b>
(Tobacco state)* After		-1.652(1.572)
State income per capita	-4.82e-04(3.37e-04)	-5.28e-04(3.40e-04)
State fixed effects	Yes	Yes
Observations	180	180
R-squared	0.473	0.478

Notes: No lawsuit states are Alabama, Delaware, Kentucky, North Carolina, Tennessee, Virginia and Wyoming. Per capita settlement revenue and income are measured in real 2002 dollars. Settlement revenue per capita and income per capita are de-meaned. Years: 1998, 1999, 2001 and 2002. The lawsuit indicator is taken from a summary of litigation documents available at the following website: <http://www.library.ucsf.edu/tobacco/litigation/summary.html>. Fiscal year 2000 excluded because settlement revenues were received in the middle of this fiscal year. Big tobacco producing states are Georgia, Kentucky, North Carolina, South Carolina, Tennessee and Virginia. \* significant at 5%; \*\* significant at 1%.

receipt of settlement funds compared with \$1.41 for states that did not file lawsuits, and the difference is statistically significant at the 5% level. States that did not file lawsuits also have a propensity to spend of 14 cents on the dollar compared to 23 cents for filing states, although this difference is not statistically significant. These results hold when interactions for being a large tobacco producing state are added (column 2) although the significance of the negative effect on average spending decreases from the 5% level to the 15% level.

We might expect states that filed lawsuits to have different underlying preferences for such spending. However, we should then see differences in spending between lawsuit and non-lawsuit states prior to the settlement as well as differences in the propensity to spend out of overall income. I do not observe any such differences. In addition, any fixed difference across states will be picked up by the state fixed effect. Interacting lawsuit filing with settlement revenue and the *After* indicator when state fixed effects are included captures whether or not these states react differently than other states to the receipt of settlement funds relative to other income.

Similar patterns are observed in Lutz's (2006) study of a New Hampshire court-mandated school finance reform. Although he finds little evidence of flypaper effects overall, he does find that "plaintiff towns" that filed the suits leading to the court mandate spend significantly more than other municipalities on education upon receipt of the resulting state grants.<sup>37</sup>

The model predicts that governments that weigh future periods more should spend more on tobacco prevention and control programs. I do not find significant interaction effects of years to the next election or governor's eligibility for re-election on spending. It may be that governors internalize their parties' payoffs or that these measures are imperfect proxies for the true discount factor for other reasons.

## 6. Conclusion

I find clear evidence that states systematically violate fungibility in spending decisions in response to windfalls received under the tobacco settlement. The observed spending patterns are not consistent with existing models of the political process. The interaction effects of lawsuit participation are also difficult to reconcile with a simple mental accounting story. I find support for my proposed model, which predicts violations of fungibility as a result of dynamic interactions between interest groups and local government.

As noted in the Introduction, there are at least two main caveats to generalizing these results. First, settlement revenues were not specifically labeled for tobacco prevention and control programs, whereas most grants-in-aid are labeled for particular projects. In this model, the relevant factor is not the label of the grant but rather the preference of the interest group that procured the grant. In practice, these are likely to be the same for most grants-in-aid. The model can thus provide an explanation for cases of classic flypaper effects. Second, the transfers in the settlement are from

<sup>37</sup> I thank Byron Lutz for helpful discussion on this point.

industry to local government. The same model, however, applies to grants from federal to local governments; if anything, we might expect the links between interest groups and grants to be stronger in the case of intergovernmental grants.

The interest group model would not be as applicable to situations in which there is no discretionary component to grants-in-aid and grants are truly unconstrained. This is rarely the case. As mentioned, a substantial share of intergovernmental grants are allocated on a discretionary basis. In addition, flypaper effects in entitlement programs are generally identified from program expansions. These expansions may be the result of interest group involvement, in which case the incentives outlined in the model would apply. Similar effects in other contexts, such as in spending by local governments in response to grants from international aid agencies and non-governmental organizations, could also be explained by a similar type of dynamic interaction between local governments and granting agencies. Spending targeted grants on targeted activities in this case is rational and optimal for constituents given long-run incentives.

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