SPECIAL INTEREST GROUPS AND THE
ALLOCATION OF PUBLIC FUNDS

Monica Singhal

ABSTRACT

A long-standing puzzle in the fiscal federalism literature is the empirical non-equivalence in government spending from grants and other income. I propose a fully rational model in which violations of fungibility arise from dynamic interactions between politicians and interest groups with the ability to raise funds for 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 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. The findings are consistent with the predictions of the model when political partisanship is introduced: Republican governors spend less and factors which should lead to political convergence increase spending for Republicans and decrease spending for Democrats. These results cannot be explained by existing models in the literature.

Monica Singhal
Harvard University
JFK School of Government
79 JFK Street
Cambridge, MA 02138
and NBER
monica_singhal@harvard.edu

1 Introduction

Standard models of fiscal federalism predict that 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).[1]

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. 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

---

[1]Models illustrating the standard revenue equivalence proposition include Bradford and Oates (1971a, 1971b). See Gramlich (1977), Inman (1979) Fisher (1982) and Hamilton (1983) for a review of the earlier flypaper literature. More recent work includes Baicker (2001), Gordon (2004), Lutz (2004), and Evans and Owens (2005).

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.[2]

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

---

[2]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.

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 unrestricted and use of funds is left entirely to the discretion of states. Settlement windfalls should therefore, in theory, be considered equivalent to increases in state income. 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.[3]

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. Finally, I show that spending patterns conform closely to the predictions of the model in a world with political partisanship. Republican governors spend less than Democrats, and factors which should lead to political convergence, namely eligibility for re-election and facing an opposition controlled senate, result in increased spending by Republicans and decreased spending by Democrats.

This empirical setting differs in at least two important ways from traditional empirical flypaper

---

[3]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.

studies. First, as mentioned, settlement revenues are unrestricted. Examining the response of spending on tobacco prevention and control programs to settlement windfalls therefore provides a test of fungibility but differs from classic flypaper since revenues were not specifically labeled for such programs. Second, transfers in this case are from private industry to local government, rather than intergovernmental transfers. While these 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 state decisions to spend 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.[4] In this system, interest groups have the ability to influence grants-in-aid through contributions and lobbying efforts. Interest

---

[4]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.

groups are large contributors to federal legislators: during the 1997-1998 election cycle, over 4,500 Political Action Committees spent almost $500 million dollars and a variety of other organizations spent $1.5 billion on lobbying Washington.[5] 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.

In this paper, 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. I define an interest group 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.[6] Why should local governments not treat these grants as fungible once they are received? I argue that this occurs 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. The model is therefore applicable to violations of fungibility in a number of contexts.[7]

---

[5] Source: The Center for Responsive Politics. www.opensecrets.org.

[6] An "interest group" could also be a group within the government, as long as these two conditions are met.

[7] For example, the types of interactions I describe provide one possible explanation for the flypaper effects observed by Evans and Owens (2005) in the COPS program.

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.[8]

An interest group derives utility from spending on a particular good that it cares about: 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 a variety of goods, including z, conditional on funds received from the interest group and other income Y. I do not assume political agency by the government in order 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 consider the implications of the model in a world with political agency and partisanship in Section 5.5.4. I make the strong assumption that lobby groups are homogeneous and that all lobby goods enter the government utility function in the same way.[9] For simplicity, I also restrict the interest group effort choice to be binary.

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

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

where z represents the lobby good (also a "good" in the government utility function) and x represents other government and private voter goods. Prices in this case represent the cost of production of the various goods. Solving this problem gives the optimal choice of goods, which I denote as: (z^0, x^0). When an interest group chooses to raise funds for the government, it does so with an

---

[8] 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 (1998).

[9] Allowing heterogeneity in lobby goods does not alter the basic intuition of the model.

implicit understanding that the government will provide "payback" by spending the funds on the good the interest group cares about, z. Whether payback occurs depends on two factors: the type of the government and the action chosen by the government.

This model assumes two types of governments: Committed and Strategic. The Committed government always chooses *Reciprocate*. The Strategic government can choose one of two strategies: *Reciprocate* or *Renege*. I define these in the following way. Under *Renege*, the government breaks the implicit contract and treats the interest group funds as it would other income, maximizing $U_G(z,x)$ subject to the constraint $p_z z + p_x x \leq Y + L$. Solving this problem leads to a choice of goods along the government's income expansion path: (z', x'). Under *Reciprocate*, the government spends all the interest group funds on the lobby good, leading to the consumption choices $(\widehat{z}, \widehat{x})$. The government would prefer to allocate L across all goods and would therefore be better off by reneging.

I assume that interest groups have utility functions such that $U_L(\widehat{z}, \textit{effort}) > U_L(z^0, \textit{no effort})$ and $U_L(z', \textit{effort}) < U_L(z^0, \textit{no effort})$; that is, interest groups prefer to undertake effort and provide L if and only if the government pays them back.

The above equations imply the following payoff matrix:[10]

<div align="center">

GOVERNMENT

		Reciprocate	*Renege*
INTEREST GROUP	*Effort*	(a, c)	$(-b, d)$
	No Effort	$(0, 0)$	$(0, 0)$

</div>

The Nash equilibrium of the stage game is then (*No Effort, Renege*) yielding payoffs of $(0,0)$ even though (*Effort, Reciprocate*) results in higher payoffs (a, c) for both players.

I now consider the implications of this model in a dynamic setting in which the government

---

[10]The zero payoffs in the second row arise from normalizing $U_G(z^0, x^0)$ and $U_L(z^0, \textit{no effort})$ to zero for simplicity. The payoffs in the first row are then as follows: $a = U_L(\widehat{z}, \textit{effort})$, $b = |U_L(z', \textit{effort})|$, $c = U_G(\widehat{z}, \widehat{x})$, and $d = U_G(z', x')$.

interacts with an interest group in each period. An interest group observes the past actions of the government with previous interest groups but not its type. In this setting, 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.[11] The goal of the government is to maximize its discounted sum of payoffs with a discount factor δ. 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 reneging 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.[12]

The model predicts violations of fungibility even in cases in which the government may be unlikely to interact with a particular interest group repeatedly. By reneging on any interest group, the government signals its type to all interest groups.

2.3 Solution and Comparative Statics

Suppose that the interest group's prior probability that the government is Committed is p^0. In the one period case, the Strategic government always reneges, and the interest group provides effort if $p^0 a - (1 - p^0)b > 0$. This occurs when the prior probability that the government is Committed

---

[11]This setup corresponds to the standard reputation effects framework. Without multiple types, there is no uncertainty for the interest group. As Fudenberg and Tirole (1998) 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 reneged on a previous interest group and refuse to provide funds as soon as the government reneges. 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 1998).

[12]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.

exceeds a threshold value: $p^0 > \frac{b}{a+b} \equiv \bar{p}$.

In the two period case, the Strategic government can *Renege* 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 $c + \delta d$. Solving yields the following necessary condition for the Strategic government to *Reciprocate*:

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

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 equation (2) holds, the equilibrium depends on p^0, the prior probability of a Committed government. 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 $\left(\frac{b}{a+b}\right)$. For details of the solution, please see *Appendix A.1*.

The standard revenue equivalence proposition states that government expenditure from grants and from other income should be the same: $\frac{\partial z}{\partial L} = \frac{\partial z}{\partial Y}$. Much of the empirical literature on categorical flypaper effects finds instead that $\frac{\partial z}{\partial L} > \frac{\partial z}{\partial Y}$. 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 equation (2) holds. The model thus predicts systematic violations of fungibility across spending categories consistent

9

with flypaper effects.

Violations of fungibility are more likely when δ is high (more weight is given to future periods), holding the other parameters fixed. Equation (2) also shows that for a given δ, 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:

$$prob\left(\frac{\partial z}{\partial L} > \frac{\partial z}{\partial Y}\right) = f\left(\frac{c}{d}\right), \quad \text{where } f' > 0 \tag{3}$$

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.

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 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.[13]

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

---

[13] 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.

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 (*Table 1*), or $120 billion in present value terms using a discount rate of 4%.

Table 2 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.

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

12

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.[14]   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:

$$percent_i = 0.5 * \left(\frac{SMCD_i}{\sum\limits_{i} SMCD_i} \right)_{1993} + 0.5 * \left(\frac{AdjDMC_i}{\sum\limits_{i} AdjDMC_i} \right)_{1993} \tag{4}$$

where $SMCD_i$ and $AdjDMC_i$ are the smoking-related Medicaid costs and the adjusted medical costs for state i.

Negotiations among states at the time of the settlement resulted in some small adjustments to these base percentages. *Table 3* 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

---

[14]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.

revenues.[15]   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.[16]

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 4*).[17] 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 settlement, lawsuits were orchestrated largely by state attorneys general with substantial involvement by anti-tobacco and health organizations.   A large body of anecdo-

---

[15]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.

[16]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.

[17]Coefficients differ from 0.5 because the regressions are run on the levels of smoking-related health costs rather than the shares.

tal 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 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. *Figure 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[18] 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 (*Figure 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.[19] 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.

---

[18] Arizona, California, Maine, Massachusetts and Oregon.

[19] The only state not allocating state funds toward tobacco prevention and control by fiscal year 2002 was Tennessee.

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

4.2   Econometric Specification

The empirical strategy I employ to test for violations of fungibility is a variation on a traditional fixed effects specification.   By exploiting both the time series and cross-sectional variation in settlement revenue receipt, I test for violations of fungibility in two ways.   Consider the following regression framework:

$$Tobacco\,Control_{it} = \alpha_0 + \alpha_1(Settlement\,revenue)_{it} + \alpha_2\,(Income)_{it} + \gamma_t + \phi_i + \delta X_{it} + \varepsilon_{it} \quad (5)$$

where γ_t is a set of year dummies, ϕ_i is a set of state dummies and X_{it} is a set of time-varying state controls.   In a standard fixed effects setting, the key parameter of interest is α_1, which would be interpreted as measuring the effect of settlement revenue receipt on tobacco control spending. γ_t would be included primarily as a control to pick up underlying trends in spending over time.

The tobacco control experiment is unusual in that the pre-trend in tobacco control spending is essentially flat and close to zero.   A large, discontinuous increase in spending occurs when settlement revenues are received (*Figure 1*).   Thus, *both* α_1 and the γ_t's have causal meaning and can be used to test for violations of fungibility.   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.   The γ_t coefficients indicate whether states spent on the lobby good when they received lobby funds and α_1 indicates whether states that received more lobby money spent more on the

16

lobby good.

There are two primary identification assumptions. The identifying assumption for γ_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 α_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 *Figure 1* and *Figure 2*, only five states had substantial tobacco control programs prior to the receipt of settlement funds; the remaining states spent virtually nothing.[20] 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). Reported state allocations include funding specifically appropriated to any governmental agency, foundation, trust fund, board or university for tobacco control programs.

---

[20]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).

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). 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.

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.[21] Sources on the remaining variables are given in *Appendix A.2*.

---

[21] 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.

5 Results

5.1 Main Findings

Table 5 provides summary statistics on per capita tobacco control program allocations for fiscal years 1998, 1999, 2001 and 2002.[22] 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 the following equation:

$$Tobacco\,Control_{it} = \alpha_0 + \alpha_1(Srev_{it} - \overline{Srev_t}) + \alpha_2(Inc_{it} - \overline{Inc_t}) + \gamma(After) + \phi_i \qquad (6)$$

$Srev_{it}$ and Inc_{it} are per capita settlement revenue and income for state i in year t, $After$ is an indicator which is equal to 1 for fiscal years 2001 and 2002 and ϕ_i is a state fixed effect. The two tests are: $\alpha_1 > \alpha_2$ and $\gamma > 0$. Settlement revenue and income are measured in deviations from the year mean so that γ can be interpreted as the change in spending at the average levels of these variables. Per capita settlement revenue and income are measured in real 2002 dollars.

I find strong evidence that states violate fungibility in spending decisions as predicted by the interest group model (*Table 6*). Column 1 gives the results when state fixed effects are not included. α_1, the propensity to spend from settlement revenue, is 0.18 and significant at the 1% level; α_2, the propensity to spend out of income, is essentially zero and insignificant. The average increase in spending upon receipt of settlement funds, γ, 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. α_1 increases to 0.21 and γ 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

---

[22]Data are not available for Arizona and Massachusetts for fiscal year 2002.

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.

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 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.

In addition, 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.

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.[23] 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

---

[23]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.

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.

5.2 Testing the Identifying Assumptions

The identification assumption for the $After$ coefficient is a constant underlying time trend. *Figure 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 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 7*). I control for the percent of the state population that smoked in 1998, state-specific minimum spending guidelines recommended by the Centers for Disease Control (for more details see *Appendix A.2*), 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.[24] The results are virtually unchanged when controls for

---

[24]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.

region, youth share and conservativeness of the state are added (column 4).

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 (*Appendix Table 1*). 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.

5.3   Robustness Checks

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. The results are given in *Table 8*. 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.

5.4   Persistence Over Time

I next 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.

Table 9 presents regression results and *Figure 4* and *Figure 5* plot the time dummies and settlement revenue coefficients with 95% confidence intervals. 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. Overall, the effects appear to be strongly persistent, at least into the fifth year of fund receipt.

5.5 Further Predictions of the Interest Group Model

5.5.1 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. Other states simply signed on to the final settlement, receiving windfalls without effort by interest groups.[25] 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.

I test this prediction empirically by constructing an indicator equal to one if the state did not file a lawsuit prior to the settlement. Interacting this indicator with settlement revenue and the

---

[25] 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. In the absence of direct measures of interest group effort, I use lawsuit filing as a proxy. The following states did not file lawsuits: Alabama, Delaware, Kentucky, North Carolina, Tennessee, Virginia and Wyoming.

After indicator, I find that states that did not file lawsuits spent less than states that filed (*Table 10*). States that filed lawsuits increased average spending by $3.45 after 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.[26]

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 (unreported). 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 (2004) 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.[27]

5.5.2 Discount Factor

The model predicts that governments that weigh future periods more should spend more on tobacco prevention and control programs. I do not find interaction effects of years to the next election or governor's eligibility for re-election on spending (unreported). It is likely that these measures

---

[26] No-lawsuit states still spend $2.45 less on average when major tobacco producing states (Georgia, Kentucky, North Carolina, South Carolina, Tennessee and Virginia) are excluded, although this result is no longer statistically significant (column 2).

[27] I thank Byron Lutz for helpful discussion on this point.

are imperfect proxies for the true discount factor. I argue in the next section that eligibility for re-election, in particular, appears to be picking up political constraints on government rather than the government discount factor.

5.5.3 Political Factors

The model in Section 2 does not consider political agency on the part of the government and is equivalent to a model in which decisions are made by a median voter. I now consider the implications of the model in a world with political partisanship. In particular, I relate the interest group model to models of the political process in which politicians have preferred policies and cannot credibly commit to enact more moderate policies if elected (Alesina 1988, Alesina and Rosenthal 1995). In these models, policy convergence is achieved through repeated elections (politicians are punished if they break their promises once in office) or through compromises between the executive and legislature if the government is divided.

We can think of politicians in this world making their decisions based on a utility function that is some weighted average of the utility function of the politician and the utility function of the median voter. We might imagine that different politicians have different utility costs of misallocating toward tobacco control programs, particularly from a corner solution in which no funds were being spent. The necessary condition for *Reciprocating* is then more likely to be satisfied when the costs of misallocation are low. If politicians from one political party face lower costs of misallocating on such programs than the median voter and politicians from the other party face higher costs, we should observe the party with low costs spending more. Suppose for now that Democrats face lower costs of misallocating toward tobacco control than the median voter and that Republicans face higher costs. The model then predicts that we should observe Democrats spending more from settlement funds than Republicans. It further generates the strong prediction

that factors that lead to greater political convergence, such as eligibility for re-election and facing an opposition controlled legislature, should then *decrease* spending for Democrats and *increase* spending for Republicans. (The pattern would be reversed if in fact Republicans are the low cost party.)

I test these predictions by analyzing the effects of political factors on spending in the post-settlement period.[28] I find strong evidence in support of the model (*Table 11*). Having a Republican governor at the time the budget for the fiscal year is passed decreases average spending by $1.90 from a base of $4.74 (significant at 10%). Having a Republican governor also decreases the propensity to spend to 5 cents on the dollar from a base of 37 cents on the dollar, and this difference is significant at the 1% level. Note that we do not see differing propensities to spend out of other income, indicating that these results are not being driven solely by differences in the propensity to spend on tobacco control.

I find that eligibility for re-election (which should move parties closer to the median voter) reduces both average spending and the propensity to spend for Democrats and increases both for Republicans (column 2). Similarly, facing an opposition controlled state senate decreases spending for Democrats and increases spending for Republicans, both on average and at the margin (column 3).

These results should be taken with some caution given the relatively small sample size. Nevertheless, the patterns are quite striking and consistent, lending support to the interest group model.

---

[28] I focus on the post-settlement period because the vast majority of states were not spending anything on such programs prior to the settlement. The direct effect of political factors in the pre-settlement period cannot be credibly estimated.

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 and political factors are also difficult to reconcile with a 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. Spending patterns also conform to the specific predictions of the model when we allow political partisanship and policy divergence.

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 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 truly no discretionary component to grants-in-aid. 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. Observed flypaper 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.

References

[1] Alesina, Alberto. "Credibility and Policy Convergence in a Two Party System with Rational Voters." *American Economic Review*, 78: 796-806, 1988.

[2] Alesina, Alberto and Howard Rosenthal. *Partisan Politics, Divided Government, and the Economy.* Cambridge: Cambridge University Press, 1995.

[3] Baicker, Katherine. "Government Decision-Making and the Incidence of Mandates." *Journal of Public Economics.* 82(2): 147-194, 2001.

[4] Becker, Gary S. "A Theory of Competition Among Pressure Groups for Political Influence." *Quarterly Jounal of Economics.* 98(3): 371-400, 1983.

[5] Bradford, David and Wallace Oates. "The Analysis of Revenue Sharing in a New Approach to Collective Fiscal Decisions." *Quarterly Journal of Economics.* 85(3): 416-439, 1971a.

[6] Bradford, David and Wallace Oates. "Toward a Predictive Theory of Intergovernmental Grants." *American Economic Review.* 61(2): 440-448, 1971b.

[7] Centers for Disease Control. "Investment in Tobacco Control: State Highlights 2001." U.S. Department of Health and Human Services, 2001.

[8] Centers for Disease Control. "Investment in Tobacco Control: State Highlights 2002." U.S. Department of Health and Human Services, 2002.

[9] Centers for Disease Control. "Tobacco Control Activities in the United States: 1994-1999." U.S. Department of Health and Human Services, 1999.

[10] Chernick, Howard A. "An Economic Model of the Distribution of Project Grants" in *Fiscal Federalism and Grants-in-Aid*, Peter Mieszkowski and William H. Oakland, eds. Washington D.C.: Urban Institute, 1979.

[11] Dougan, William R. and Daphne A. Kenyon. "Pressure Groups and Public Expenditures: The Flypaper Effect Reconsidered." *Economic Inquiry.* 26(1): 159-170, 1988.

[12] Evans, Bill and Emily Owens. "Flypaper COPS." Mimeo, University of Maryland, 2005.

[13] Farrelly, Matthew C., Terry F. Pechacek and Frank J. Chaloupka. "The Impact of Tobacco Control Program Expenditures on Aggregate Cigarette Sales: 1981-1998." NBER Working Paper 8691, December 2001.

[14] Fisher, Ronald C. "Income and Grant Effects on Local Expenditure: The Flypaper Effect and Other Difficulties." *Journal of Urban Economics*, 12: 324-345, 1982.

[15] Filimon, Radu, Thomas Romer and Howard Rosenthal. "Asymmetric Information and Agenda Control." *Journal of Public Economics*, 17: 51-70, 1982.

[16] Fudenberg, Drew and Jean Tirole. *Game Theory.* Cambrige: The MIT Press, 1991.

[17] Gordon, Nora. "Do Federal Grants Boost School Spending? Evidence from Title I." *Journal of Public Economics.* 88(9-10): 1771-92, 2004.

[18] Gramlich, Edward M. "Intergovernmental Grants: A Review of the Empirical Literature" in *The Political Economy of Fiscal Federalism*, Wallace E. Oates, ed. Lexington, Mass.: D.C. Heath, 1977.

[19] Grossman, P.J. "A Political Theory of Intergovernmental Grants." *Public Choice.* 78: 295-303, 1994.

[20] Grossman, Gene and Elhanan Helpman. *Special Interest Politics.* Cambridge: The MIT Press, 2001.

[21] Hamilton, Bruce W. "The Flypaper Effect and Other Anomalies." *Journal of Public Economics*, 22: 347-362, 1983.

[22] Hamilton, Jonathan. "The Flypaper Effect and the Deadweight Loss from Taxation." *Journal of Urban Economics,* 19: 148-155, 1986.

[23] Hines, James R. and Richard H. Thaler. "Anomalies: The Flypaper Effect." *Journal of Economic Perspectives*, 9(4): 217-226, 1995.

[24] Inman, Robert P. "The Fiscal Performance of Local Governments: An Interpretive Review" in *Current Issues in Urban Economics*, Peter Mieszkowski and Mahon Straszheim, eds. Baltimore: Johns Hopkins Press, 1979.

[25] Knight, Brian. "Endogenous Federal Grants and Crowd-out of State Government Spending: Theory and Evidence from the Federal Highway Aid Program." *American Economic Review*, 92(1): 71-92, 2002.

[26] Kreps, D. and R. Wilson. "Reputation and Imperfect Information." *Journal of Economic Theory*, 27: 253-279, 1982.

[27] Lutz, Byron. "Taxation with Representation: The Incidence of Intergovernmental Grants in a Plebiscite Democracy." Mimeo, MIT, 2004.

[28] Milgrom, P. and J. Roberts. "Predation, Reputation and Entry Deterrence." *Journal of Economic Theory*, 27: 280-312, 1982.

[29] Modisett, Jeffrey A. "An Analysis of Various Methods of Allocating Tobacco-Settlement Proceeds Among the States (Allocation Subcommittee Draft Version)." unpublished manuscript, August 8, 1997.

[30] Moffitt, Robert. "The Effects of Grants-in-Aid on State and Local Expenditures: The Case of AFDC." *Journal of Public Economics*, 23: 279-305, 1984.

[31] National Conference of State Legislatures. "State Management and Allocation of Tobacco Settlement Revenue 1999-2001." August 2001.

[32] National Conference of State Legislatures. "State Management and Allocation of Tobacco Settlement Revenue 2002." August 2002.

[33] National Conference of State Legislatures. "State Management and Allocation of Tobacco Settlement Revenue 2003." September 2003.

[34] Oates, Wallace E. "Lump-sum Intergovernmental Grants Have Price Effects" in *Fiscal Federalism and Grants-in-Aid*, Peter Mieszkowski and William H. Oakland, eds. Washington D.C.: Urban Institute, 1979.

[35] Turnbull, Geoffrey K. "Fiscal Illusion, Uncertainty and the Flypaper Effect." *Journal of Public Economics*, 48: 207-223, 1992.

[36] Williams, Evans and Emily Owens. "Flypaper COPS." mimeo, University of Maryland, September 2004.

Appendix

A.1 Solution to Interest Group Model

Since the Committed government always pays back, the probability of being Committed conditional on *Reciprocate* must be at least p^0. If $p^0 > \bar{p}$, interest groups always provide effort in period 1 and effort in period 2 if the government *Reciprocates* in period 1. If $p^0 < \bar{p}$, the equilibrium condition requires that the interest group in period 2 will randomize so that the government is indifferent in period 1; in other words, so that the the posterior probability of Committed conditional on *Reciprocate* is exactly the threshold value $\bar{p}$. If γ is the probability of *Reciprocate* by a Strategic government, Bayes' rule implies that the probability of being Committed conditional on *Reciprocate* is $\frac{p^0}{p^0 + \gamma(1-p^0)}$. Setting this equal to $\bar{p}$ and solving gives $\gamma = \frac{p^0 a}{(1-p^0) b}$. The total probability of *Reciprocate* in period 1 is therefore $p^0 + (1-p^0)\left(\frac{p^0 a}{(1-p^0) b}\right) = p^0\left(\frac{a+b}{b}\right)$. The interest group will provide effort in period 1 if $p^0\left(\frac{a+b}{b}\right)a - \left(1 - p^0\left(\frac{a+b}{b}\right)\right)b > 0$. This condition holds when $p^0 > \left(\frac{b}{a+b}\right)^2$. We can now solve by induction to the N period case. For example, when $N = 3$ we can see that if $p^0 > \bar{p}^2$, the government will *Reciprocate* and the interest group will provide funds. If p^0 is between $\bar{p}^2$ and $\bar{p}^3$, the government will randomize and the interest group will provide funds in period 1. If $p^0 < \bar{p}^3$, the government will randomize and the interest group will not provide funds in period 1. The prior probability of a Committed government (p_0) required for the interest group to provide funds decreases in N geometrically at the rate $\left(\frac{b}{a+b}\right)$.

A.2 Data Sources

Centers for Disease Control Minimum Spending Guidelines: The Centers for Disease Control put forth a 1999 report with "best practices" state-specific spending guidelines. The report included the CDC's estimate of the minimum spending required for each state to implement comprehensive and effective tobacco control programs. The full report is available at: http://www.cdc.gov/tobacco/bestprac.htm.

Lawsuit Participation: This variable indicates whether a state filed a lawsuit prior to the settlement. The indicator is taken from a summary of litigation documents available at the following website: http://www.library.ucsf.edu/tobacco/litigation/summary.html

Political Variables: Data on political parties come from various issues of *The Book of the States*, published by the Council of State Governments. Gubernatorial and state senate control reflect the party in power at the time budget allocations are made for the fiscal year. The independent governor of Maine was classified as a Democrat. Eligibility for re-election was computed taking into account state-specific term limit laws.

State Population and Income: Bureau of Economic Analysis. The calendar year population and income are counted for the next fiscal year (ex: 1990 population counted for the 1991 fiscal year).

Youth Share: 2000 census. The youth share is the percentage of the state population under 18.

Smoking Variables: Percent of population that smokes from Centers for Disease Control; youth smoking rate from the Youth Tobacco Surveys (YTS).

FIGURE 1
Settlement States Allocating Funds for Tobacco Control
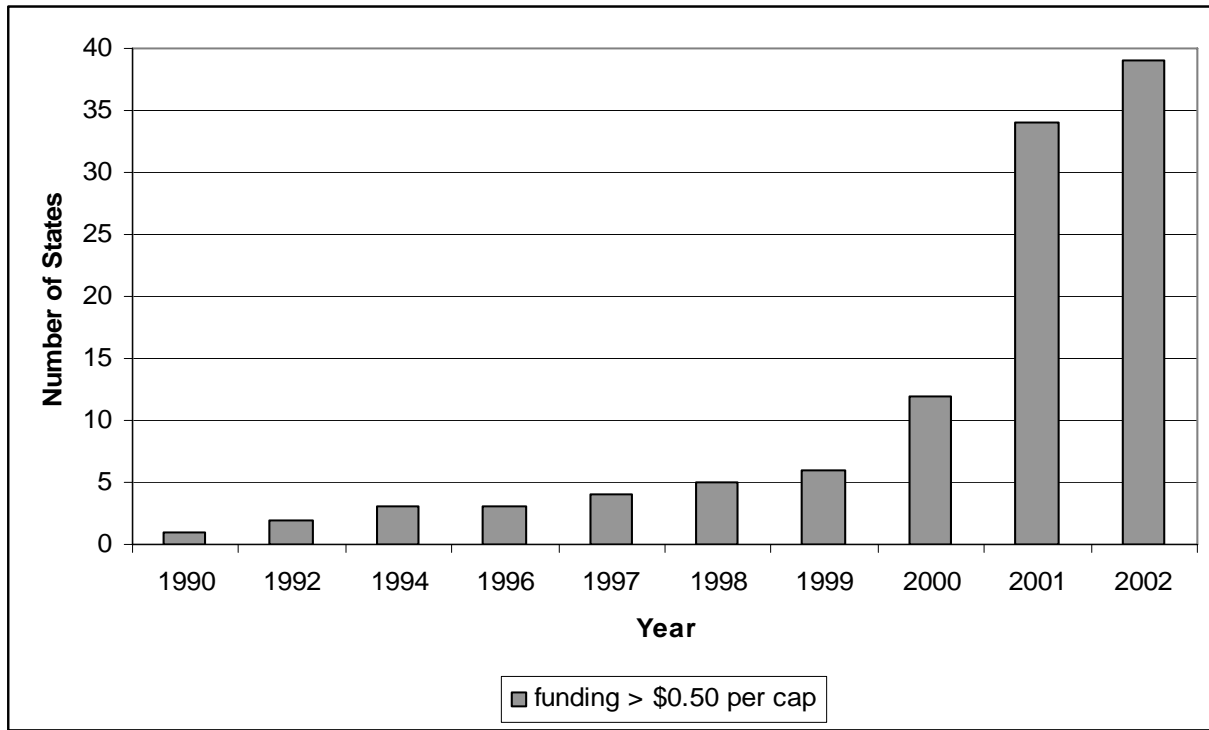
Notes: This figure illustrates spending for the 46 states that were involved in the settlement. Data for 1990, 1992 and 1994 come from surveys conducted by the American State and Territorial Health Officials (ASTHO). Data for 1996-2000 were collected by author. Data for 2001-2002 come from the CDC.

FIGURE 2
Mean Per Capita Spending on Tobacco Control Programs
(excluding states with large pre-existing programs)
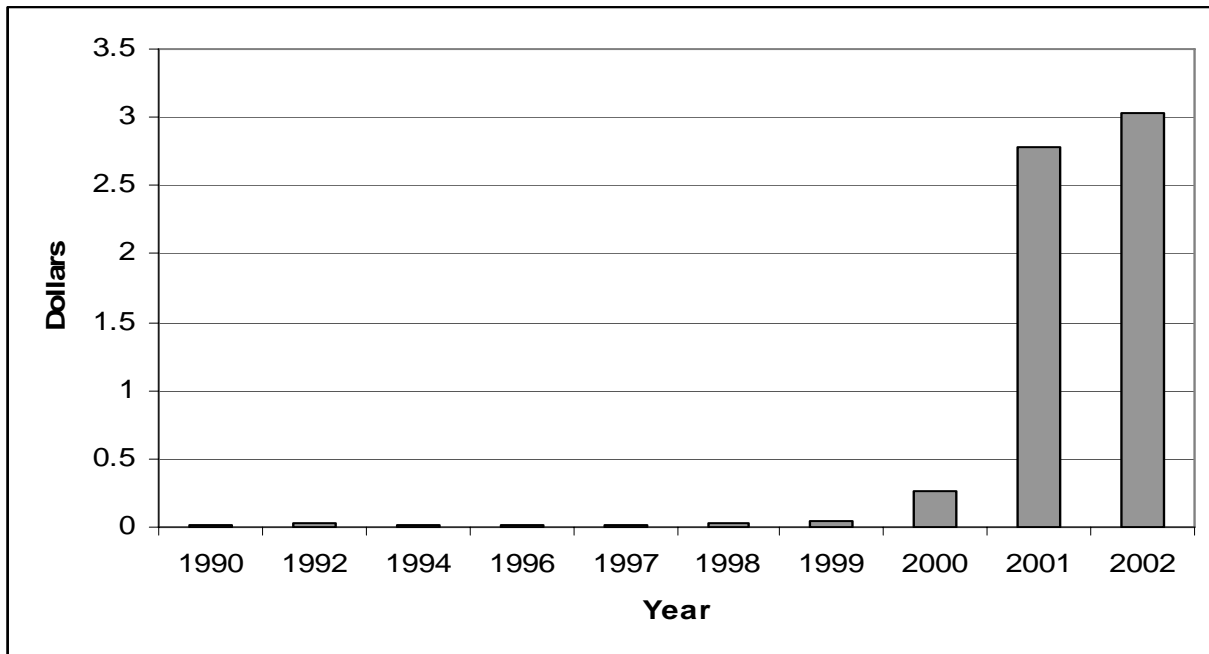
Notes: All figures given in 2002 dollars. Excludes Arizona, California, Maine, Massachusetts and Oregon. Data for 1990, 1992 and 1994 come from surveys conducted by the American State and Territorial Health Officials (ASTHO). Data for 1996-2000 were collected by author. Data for 2001-2002 come from the CDC.

Figure 3

Test of *After* Coefficient Identification Assumption

Regression of Per Cap Tobacco Control Spending on Year Dummies with State Fixed Effects
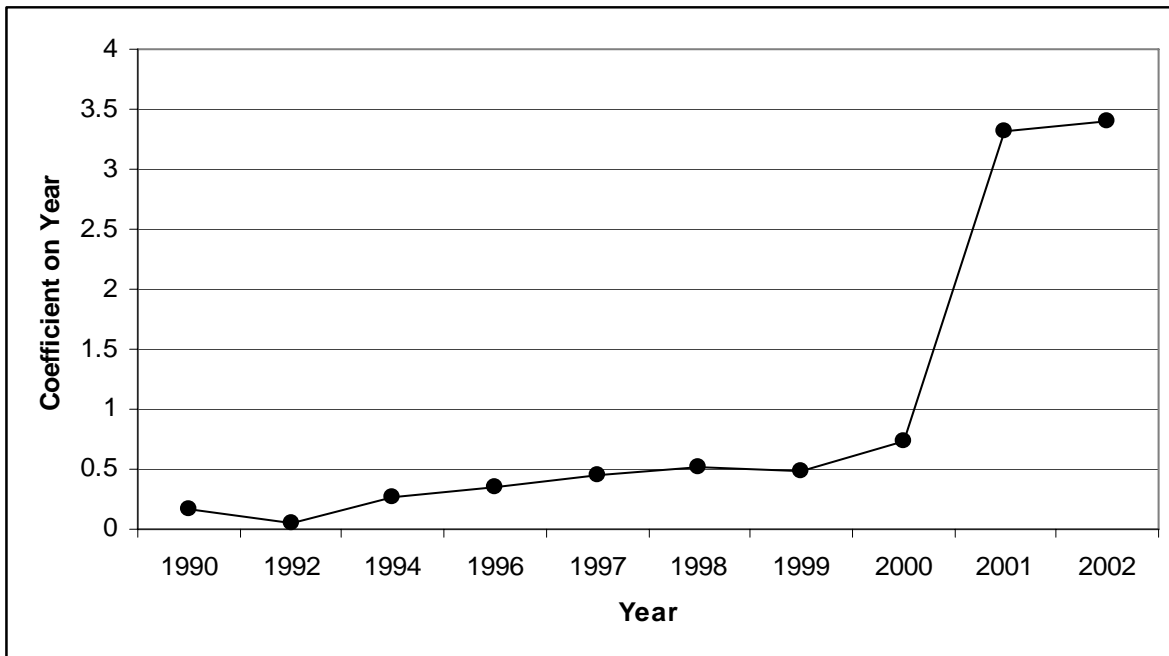
Notes: All figures given in 2002 dollars. Data for 1990, 1992 and 1994 come from surveys conducted by the American State and Territorial Health Officials (ASTHO). Data for 1996-2000 were collected by author. Data for 2001-2002 come from the CDC.

FIGURE 4
Persistence of Spending Over Time
Graph of Time Dummies with 95% Confidence Interval
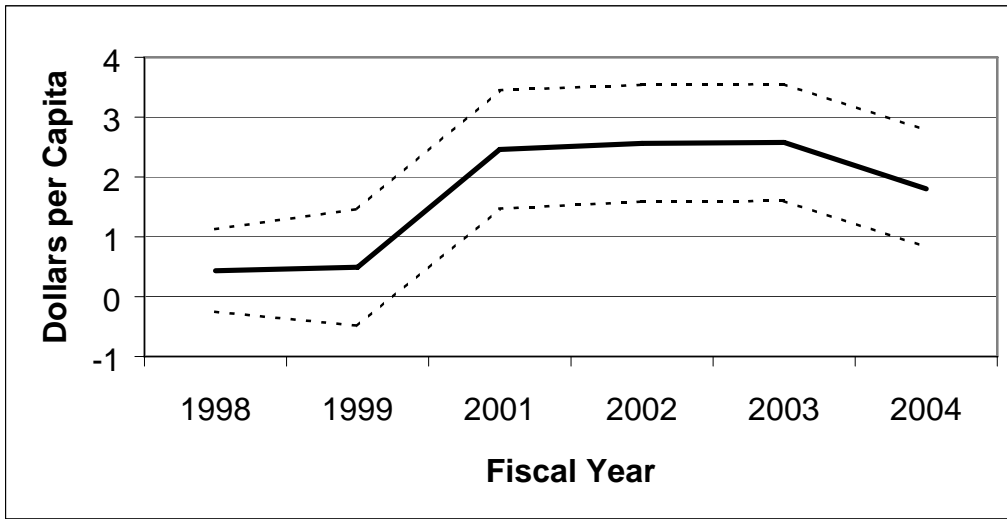
FIGURE 5
Propensity to Spend Over Time
Graph of Settlement Revenue Coefficient with 95% Confidence Interval
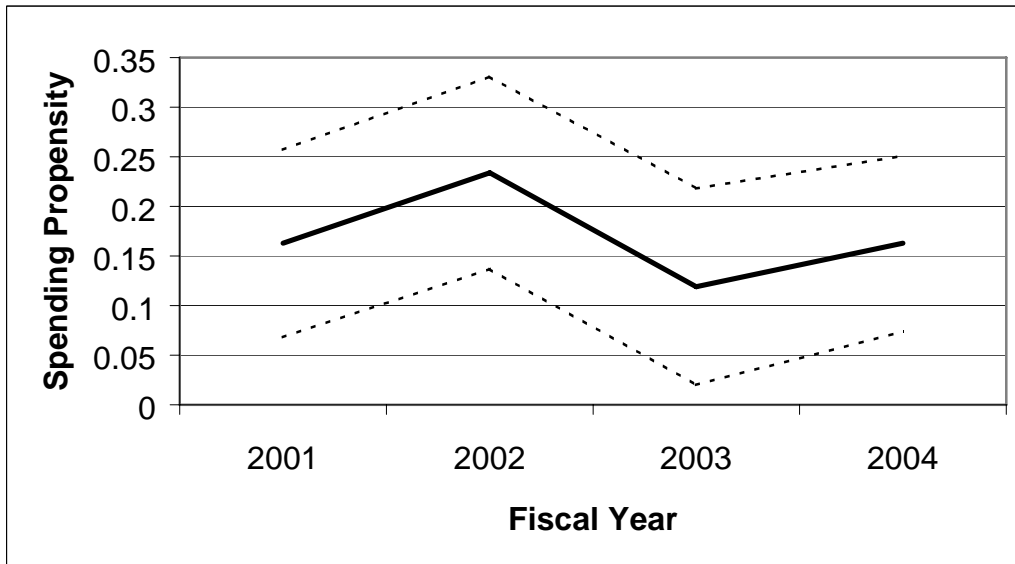
Notes: All figures given in 2002 dollars. Data for 1998-1999 were collected by author. Data for 2001-2004 come from the NCSL.

TABLE 1
Settlement Payments Through 2025

Type of Payment	Amount	Total
1998	$2,400,000,000	$2,400,000,000
2000		$6,411,750,000
Initial	$2,472,000,000	
Annual	$3,939,750,000	
2001		$6,923,660,000
Initial	$2,546,160,000	
Annual	$4,377,500,000	
2002		$8,313,294,800
Initial	$2,622,544,800	
Annual	$5,690,750,000	
2003		$8,391,971,144
Initial	$2,701,221,144	
Annual	$5,690,750,000	
2004-2007		$28,016,000,000
Annual	$7,004,000,000	
2008-2017		$80,040,000,000
Annual	$7,143,000,000	
SCF	$861,000,000	
2018-2025		$64,031,999,976
Annual	$8,003,999,997	
TOTAL		**$204,528,675,920**

Notes: Figures reported are without any adjustments other than the Previously Settled States reduction. Source: National Governors' Association. www.nga.org/cda/files/TOBDETAIL.pdf.

TABLE 2

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

Notes: These figures include both annual and initial payments.

TABLE 3: 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.

TABLE 4
Determinants of State Settlement Revenue (FY 2002)

	Settlement Revenue		Settlement Revenue per Capita			
	(1)	(2)	(3)	(4)	(5)	(6)
Smoking attributable Medicaid	0.534**	0.154				
	(0.036)	(0.079)				
Smoking attributable other health	0.103**	0.081**				
	(0.005)	(0.010)				
Smoking attr. Medicaid per cap			0.625**	0.404**	0.387**	0.391**
			(0.105)	(0.133)	(0.133)	(0.142)
Smoking attr. other health per cap			0.067**	0.085**	0.103**	0.119**
			(0.022)	(0.027)	(0.033)	(0.040)
State income per cap					-2.01e-04	-1.38e-04
					(1.92e-04)	(2.26e-04)
Pre-settlement program						-1.437
						(2.411)
Youth share (2000)						-0.016
						(0.572)
Repub 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. * significant at 5%; ** significant at 1%.

TABLE 5

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. Data for fiscal years 1998 and 1999 compiled by author. Data for fiscal years 2001 and 2002 come from CDC.

TABLE 6
Per Capita Spending on Tobacco Control

	(1)	(2)	(3)	(4)
Settlement rev per capita	**0.184****	**0.210****	**0.187****	**0.214****
	(0.042)	**(0.045)**	**(0.043)**	**(0.046)**
State income per capita	2.87e-05	-4.80e-04	2.73e-05	-4.93e-04
	(5.02e-05)	(3.42e-04)	(5.05e-04)	(3.45e-04)
After	**2.927****	**3.071****		
	(0.409)	**(0.318)**		
FY=1999			-0.031	-0.031
			(0.572)	(0.442)
FY=2001			3.107**	3.174**
			(0.583)	(0.456)
FY=2002			2.720**	2.943**
			(0.579)	(0.449)
Constant	0.507		0.523	
	(0.285)		(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: Settlement revenue per capita and income per capita are de-meaned. 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%.

TABLE 7
Test of Settlement Revenue Identification Assumption

	(1)	(2)	(3)	(4)
Settlement rev per cap	**0.199****	**0.199***	**0.255[+]**	**0.206****
	(0.070)	**(0.090)**	**(0.148)**	**(0.071)**
State inc per cap	-5.30e-06	3.56e-07	-1.37e-04	-1.12e-04
	(9.48e-05)	(1.04e-04)	(1.55e-04)	(1.31e-04)
Pre-settlement program	4.069**	4.184**	6.072**	3.067
	(1.411)	(1.402)	(2.083)	(1.966)
FY = 2002	-0.218	-0.222	-0.605	-0.269
	(0.467)	(0.522)	(0.871)	(0.490)
% pop smokes (1998)	-0.053			-0.090
	(0.176)			(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)
Repub vote share (2000 pres)				-0.142[+]
				(0.077)
Constant	4.417	3.278	9.234	14.388
	(4.138)	(2.548)	(6.620)	(10.622)
Observations	88	88	43	88
R-squared	0.221	0.219	0.270	0.321

Notes: Settlement revenue per capita and income per capita are de-meaned. Years: 2001, 2002. All columns are pooled OLS with state-clustered standard errors. [+] significant at 10%, * significant at 5%; ** significant at 1%.

TABLE 8
Robustness Checks

	(1) Base Case	(2) Simulated Settlement Rev	(3) NCSL tobacco control data
Settlement rev per capita	0.210**	0.177**	0.185**
	(0.045)	(0.041)	(0.039)
State income per capita	-4.80e-04	-1.65e-04	-2.24e-04
	(3.42e-04)	(4.78e-04)	(2.89e-04)
After	3.071**	2.911**	2.051**
	(0.318)	(0.321)	(0.273)
State Fixed Effects	Yes	Yes	Yes
Observations	180	180	182
R-squared	0.447	0.663	0.351

Notes: Settlement revenue per capita and income per capita are de-meaned. 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%.

TABLE 9
Persistence of the Effects over Time

	(1) FY 1998, 1999, 2001-2004	(2) FY 1998, 1999, 2001-2004
Settlement rev per cap	0.159**	0.184**
	(0.034)	(0.042)
Settlement rev per cap*(FY>2002)		-0.039
		(0.039)
FY = 1999	0.056	0.056
	(0.402)	(0.402)
FY = 2001	2.085**	2.123**
	(0.411)	(0.413)
FY = 2002	2.127**	2.127**
	(0.402)	(0.402)
FY = 2003	2.142**	2.142**
	(0.402)	(0.402)
FY = 2004	1.368**	1.368**
	(0.402)	(0.402)
State inc per cap	7.64e-06	6.78e-05
	(3.38e-04)	(3.43e-04)
State Fixed Effects	Yes	Yes
Observations	274	274
R-squared	0.585	0.587

Notes: Settlement revenue per capita and income per capita are de-meaned. Years: 1998, 1999, 2001-2004. Fiscal year 2000 excluded because settlement revenues were received in the middle of this fiscal year. * significant at 5%; ** significant at 1%. Tobacco control spending data from 2001-2004 from the NCSL.

TABLE 10
Tests of the Interest Group Model

	(1)	(2)
Settlement revenue per capita	0.227**	0.227**
	(0.048)	(0.052)
(No lawsuit)*(Settlement rev per cap)	**-0.083**	**0.083**
	(0.135)	**(0.273)**
After	3.449**	3.478**
	(0.347)	(0.378)
(No Lawsuit)*After	**-2.032***	**-2.450**
	(0.965)	**(1.753)**
State income per capita	-4.83e-04	-6.23e-04
	(3.38e-04)	(3.71e-04)
State fixed effects	Yes	Yes
Observations	180	156
R-squared	0.473	0.482

Notes: No lawsuit states are Alabama, Delaware, Kentucky, North Carolina, Tennessee, Virginia and Wyoming. Settlement revenue per capita and income per capita are de-meaned. Years: 1998, 1999, 2001 and 2002. Fiscal year 2000 excluded because settlement revenues were received in the middle of this fiscal year. Column 1 includes all states. Column 2 excludes big tobacco producing states: Georgia, Kentucky, North Carolina, South Carolina, Tennessee and Virginia. * significant at 5%; ** significant at 1%.

TABLE 11
Testing the Predictions of the Model with Political Partisanship

	(1)	(2)	(3)
Settlement revenue per capita	0.369**	0.595**	0.469**
	(0.091)	(0.168)	(0.098)
(Repub governor)*(Settle rev per cap)	**-0.318****	-0.579**	-0.375*
	(0.103)	(0.178)	(0.181)
(Elig for re-election)*(Settle rev pc)		-0.332[+]	
		(0.198)	
(Repub gov)*(Elig)*(Settle rev pc)		0.438[+]	
		(0.232)	
(Opposition senate)*(Settle rev pc)			-0.443**
			(0.116)
(Repub gov)*(Opp senate)*(Settle rev pc)			0.378*
			(0.181)
Constant (measures avg spending for Democrats)	4.742**	4.581**	5.463**
	(0.781)	(1.125)	(0.864)
Repub governor	-1.900[+]	-2.594*	-2.223
	(0.957)	(1.228)	(1.406)
Eligible for re-election		-0.178	
		(1.386)	
(Repub governor)*(Elig)		1.905	
		(1.884)	
Opposition senate			-2.612**
			(0.905)
(Repub governor)*(Opposition senate)			1.634
			(1.623)
State income per capia	-2.01e-05	5.81e-06	6.88e-05
	(2.32e-04)	(9.78e-04)	(8.00e-05)
(Repub governor)*(State income per capita)	8.29e-05		
	(2.46e-04)		
Observations	88	88	88
R-squared	0.157	0.303	0.334

Notes: Years: 2001, 2002. Controls for an indicator for fiscal year 2002. Settlement revenue per capita, income per capita are de-meaned.   All columns are pooled OLS with state-clustered standard errors. + significant at 10%; * significant at 5%; ** significant at 1%.

Effect of Smoking Related Variables on Per Capita Tobacco Control Spending

	(1)	(2)	(3)
% pop smokes (1998)	0.016		
	(0.185)		
CDC recommended min per cap		0.311	
		(0.216)	
Youth smoking rate (1997)			-0.037
			(0.163)
State inc per cap	6.64e-05	9.77e-05	-9.56e-06
	(8.31e-05)	(8.14e-05)	(1.42e-04)
Pre-settlement program	3.971	4.248*	8.113**
	(2.136)	(1.950)	(1.335)
FY = 2002	0.114	0.130	-0.299
	(0.442)	(0.442)	(0.852)
Constant	2.452	0.419	4.913
	(4.323)	(1.875)	(6.297)
Observations	88	88	43
R-squared	0.097	0.139	0.175

Notes: Years: 2001, 2002. Income per capita is de-meaned. All columns are pooled OLS with state-clustered standard errors. * significant at 5%; ** significant at 1%.