TAXATION WITH REPRESENTATION: INTERGOVERNMENTAL GRANTS IN A PLEBISCITE DEMOCRACY

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Abstract—Economic theory suggests that intergovernmental grants are equivalent to private income. A large empirical literature, however, contradicts this prediction. A school finance reform in New Hampshire, where local public goods decisions are made by a form of direct democracy, provides an unusually compelling test of the theory. The results, which suggest that approximately ninety cents per grant dollar are spent on tax reduction, provide support for equivalence. The paper’s findings have important policy implications for the financing of local public goods in general and for school finance reform in particular.

I. Introduction

INTERGOVERNMENTAL grants play a central role in the provision of local public goods. In 2005, grants accounted for approximately 40% of local government revenue in the United States. Grants are also one of the primary mechanisms used by the federal and state governments to enact redistribution. The seminal work of Bradford and Oates (1971a, 1971b) provides a strong prediction on how such grants should influence local government spending decisions. The theory emphasizes that voters perceive grant income and private income to be fungible. Under the assumption that government spending decisions reflect voter preferences, unconditional grants are spent on public goods at voters’ marginal propensity to spend on such goods out of private income. Grants are therefore mostly crowded out by reductions in local government spending, and there is little net increase in government expenditures in the area targeted by the grant. The theory can be summarized as predicting that intergovernmental grants are equivalent to a lump-sum tax reduction for the residents of the government receiving the grant.

A large empirical literature contradicts this prediction and finds that grants are systematically spent as intended by the sending government. This empirical tendency has been termed the “flypaper effect” because it suggests that grants stick where they are targeted. Hines and Thaler (1995) survey the empirical intergovernmental grants literature and document that virtually all previous research finds a substantial flypaper effect.1 This literature, however, is difficult to evaluate. Many of the grants studied may not be unconditional; price effects may explain the deviation from the theoretical prediction (Moffitt, 1984; Megdal, 1987). In other cases, grants examined may have a repeated game element. Governments receiving the grants may be constrained to spend them on the targeted area in order to ensure the sending government will continue the transfers in the future (Chernick, 1979). Finally, most of the studies consider situations in which there is considerable doubt whether the budgeting process reflects the preferences of voters (Oates, 1979; Filimon, Romer, & Rosenthal, 1982).

A more recent set of papers finds more support for the theory. Duggan (2000), Knight (2002), and Gordon (2004) document that grants are crowded out by reductions in spending by the governments that receive them. These papers use strong research designs and demonstrate that the empirical regularity known as the flypaper effect does not hold in all cases. In contrast, several recent papers, also using strong research designs, estimate a significant flypaper effect (Baicker, 2001; Card & Payne, 2002; Hoxby, 2001; Evans & Owens, 2005; Singhal, 2008). Viewed as a whole, the recent literature is inconclusive and suggests the flypaper effect holds in some instances but not in others.

This paper contributes to the intergovernmental grants literature by conducting an unusually direct test of the Bradford and Oates (1971a, 1971b; henceforth B&O) prediction that grants are equivalent to a tax reduction. The test is conducted using a 1999 court-mandated school finance reform in New Hampshire. The reform provides a direct and compelling test of the B&O hypothesis for at least three reasons. First, the New Hampshire reform provides a setting that coincides with the assumptions of the B&O model much more fully than previous studies; it could reasonably be argued that the reform provides an environment as close to the assumptions of the model as is likely to be found. The reform introduced pure unconditional lump-sum grants as assumed by B&O. More significant, there is a strong presumption that the grants were used in a manner consistent with the preferences of voters, again as assumed by B&O. This presumption arises from the fact that a majority of municipalities in New Hampshire determine the provision of public goods by a form of direct democracy: local

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1 Fisher and Papke (2000) survey the intergovernmental grant literature focused on education.
government budgets are set directly by voters, not by an elected representative. In addition, the reform itself was highly politicized; it was the central issue in New Hampshire politics over the period in question. Finally, New Hampshire is one of only five states with no state-imposed limitations on the taxing or spending behavior of local governments. Thus, localities have an unusual degree of latitude in setting local government spending levels. Given these facts, it is reasonable to presume that the voters were aware of the policy reform, could easily express their preferences through the political process, and were unconstrained from selecting their preferred bundle of local public goods. In contrast, the connection between voter preferences and government decisions is unclear in many previous tests of the theory.

Second, the data used in this paper allow an assessment of the effect of the grants on total local taxation and spending. This is important because it permits a more nuanced approach than merely testing for the presence or absence of the flypaper effect. It allows directly testing the theoretical prediction that grants are equivalent to a tax reduction. The absence of the flypaper effect—demonstrating that a grant failed to increase spending in the area targeted—is insufficient to confirm this prediction, because grants often produce changes in spending in areas other than the area targeted (Craig & Inman, 1982; Baicker & Gordon, 2004). If a significant portion of a grant is used to fund government spending in any area, targeted or not, the theoretical prediction is contradicted.2

Third, the reform produced an extremely large increase in intergovernmental grants: the typical school district experienced an approximate 200% increase in grants from the state. In addition, the reform was imposed by an outside body, the courts. The magnitude and exogenous nature of the reform allow a strong research design.

Understanding the fundamental validity of the B&O prediction is important. Attempts to understand why the flypaper effect appears to hold in many situations but does not hold universally (Duggan, 2000; Knight, 2002; Gordon, 2004), crucially depend on knowing whether the standard theory holds in an ideal setting or is fundamentally flawed. If the theory holds under ideal conditions, explanations for the flypaper effect focused on factors that distort voter preferences, such as voter misperception and bureaucratic capture, should be examined. If the theory is false, alternative explanations, such as the behavioral theory that grant...

2 Assessing the impact of grants on total government spending is also important from a policy perspective. The incidence of the crowded-out grants—the assessment of who receives the benefit of the transfer payments that did not stick to the area targeted—differs depending on how they are ultimately used. For example, Duggan (2000) shows that local governments in California reduce their funding to public hospitals by $1 for each $1 the hospital receives in grants. The incidence of these offset grants differs if they were used for tax reduction or to fund local health clinics unassociated with the targeted hospitals.

The empirical estimates produced by this paper range from 75 to 100 cents of crowd-out per grant dollar: governments spend only 0 to 25 cents per grant dollar on the targeted area (education) and use the remainder to fund tax reduction. A preferred set of estimates suggests crowd-out of around 90 cents per grant dollar. These results are supportive of the prediction that in a setting in which it is likely that government spending decisions reflect the preferences of voters, grants are equivalent to a tax reduction.

The results of this paper have important policy implications. Governments need to take great care in how grants are implemented. The estimates in this paper suggest that the manner in which the New Hampshire grants were issued rendered them ineffective at increasing funding for low-income students—a stated goal of the reform. In addition, I document that school finance reform has a significant impact on taxation equity—the distribution of tax burdens with respect to income. The recent literature on school finance reform has overlooked this potentially important aspect of tax equalization.

The paper proceeds as follows. Section II provides background information on New Hampshire and the 1999 reform. Section III presents a basic theoretical model. Section IV discusses the data and presents summary statistics. Section V presents the empirical model and discusses identification. Section VI presents the results of estimation, and section VII concludes.

II. Background

A. The 1999 New Hampshire School Finance Reform

Prior to the 1999 reform, New Hampshire education was funded primarily from local sources, with 87% of total...
primary and secondary education revenue raised locally—the highest in the nation. The state with the next highest percentage, Connecticut, attributed 57% of total revenues to local sources, and the median state, Wisconsin, attributed 41% (U.S. Bureau of the Census, 2000). New Hampshire's reliance on local financing created significant dispersion in per pupil funding and property tax burdens across school districts. For example, Sunapee, a town with lakefront property, spent $8,233 per pupil in current expenditures with a tax rate of 11.6 mils.4 Claremont, a property-poor town located near Sunapee, spent $4,223 per pupil with a tax rate of 23.57 mils. Local revenue was raised exclusively by property taxation.

In the Claremont II (Claremont v. Governor, 1997), ruling the New Hampshire Supreme Court declared the finance scheme used to fund K–12 education unconstitutional because it provided inadequate educational opportunity in property-poor towns and imposed inequitable property tax burdens. Following the decision, the legislature struggled to produce a new school finance system, because the Claremont mandate clashed with the state’s strong commitment to local government autonomy. Initially a constitutional amendment was proposed to reverse the decision, but it fell far short of the support required for passage. At any given point between the ruling and enactment, there were dozens of proposed reform plans being considered by the legislature. On three occasions, the state supreme court ruled that reforms proposed by the legislature were insufficient. The final such instance occurred after the start of the 1999 school year, when the new finance scheme was legally mandated to begin. The legislature passed a reform acceptable to the court only after facing the prospect of the state's school districts entering insolvency due to a legal prohibition on raising funds under the old financing scheme. As a result of the difficulty the legislature had in producing a new financing scheme, there was tremendous uncertainty concerning the magnitude of the reform and the details of how it would be enacted between the December 1997 Claremont ruling and the new scheme’s actual enactment in fall 1999.

The legislature ultimately resolved the financing dilemma through a reform in which towns receive intergovernmental grants administered by the state. These grants are primarily a function of the per pupil property wealth of the municipality, although adjustments are made for the number of high school students, special education students, low-income students, and district transportation costs. Although most communities receive positive grants, municipalities with unusually high per pupil property wealth are forced to remit payments to the state. In the first year of the reform, 20% of New Hampshire communities, containing 9% of the state’s population, made payments. These communities came to be known as donor towns. The payments of the donor towns, referred to as excess tax payments, can be viewed as negative grants. (See appendix B for more detailed information on the 1999 reform.)

Three elements of the reform are worth emphasizing. First, the reform has no price effects; the marginal cost of education is unchanged in all New Hampshire communities. The education grants are in essence lump-sum wealth transfers to the communities that receive them, because they are provided with no stipulation on their use; they are unconditional grants. Likewise, the excess tax payments are negative wealth shocks to the donor towns. The reform therefore left control of school finances completely under local control. This aspect of the reform was an explicit attempt by the legislature to preserve the state’s political traditions. Second, the reform is large in magnitude. As displayed in table 1, the $276 million in new funding is equal to 19% of total prereform education revenue in the state. In addition, another $131 million, primarily representing funds from former programs cancelled as part of the reform, is subject to redistribution. Third, the reform is not self-financing; the excess tax payments fund only 10% of the education grants. The remaining 90% is raised from a variety of statewide revenue sources. Examples of these

3 Nine percent of education revenue was provided by the state through an equalization program and several small categorical aid programs. For a summary of prereform education finance in New Hampshire, see Hall (1998) and Michener (2001).

4 A mil expresses the property tax rate per $1,000 of property value.

5 See appendix A for an account of the history of school finance in New Hampshire prior to the 1999 reform.

6 Although general equilibrium effects of the reform potentially increase the price of educational inputs in the state, the extent of grant crowd-out makes such effects unlikely.
TABLE 2.—SUMMARY STATISTICS

<table>
<thead>
<tr>
<th>Row</th>
<th>1998</th>
<th>2000</th>
</tr>
</thead>
<tbody>
<tr>
<td>A: Municipality</td>
<td></td>
<td></td>
</tr>
<tr>
<td>1</td>
<td>Household income</td>
<td>62,339</td>
</tr>
<tr>
<td>2</td>
<td>% donor towns</td>
<td>10</td>
</tr>
<tr>
<td>3</td>
<td>% representative government</td>
<td>6</td>
</tr>
<tr>
<td>4</td>
<td>% modified traditional meeting</td>
<td>25</td>
</tr>
<tr>
<td>5</td>
<td>Population</td>
<td>7,076</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(11,873)</td>
</tr>
<tr>
<td>6</td>
<td>Students in residence</td>
<td>1,170</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(1,779)</td>
</tr>
<tr>
<td>7</td>
<td>Local revenue for education per pupil</td>
<td>6,287</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(1,517)</td>
</tr>
<tr>
<td>8</td>
<td>Net reform grant per pupil</td>
<td>1,632</td>
</tr>
<tr>
<td>9</td>
<td>Excess tax payment to state per pupil</td>
<td>1,548</td>
</tr>
<tr>
<td>10</td>
<td>Market value of property*</td>
<td>395</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(574)</td>
</tr>
<tr>
<td>11</td>
<td>Tax rate per $1,000 of property (in mils)</td>
<td>27.2</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(6.6)</td>
</tr>
<tr>
<td>12</td>
<td>Log(90th percentile) − Log(10th percentile)</td>
<td>0.65</td>
</tr>
<tr>
<td>13</td>
<td>Coefficient of variation</td>
<td>0.24</td>
</tr>
<tr>
<td>14</td>
<td>Number of observations</td>
<td>160</td>
</tr>
<tr>
<td>B: School District</td>
<td></td>
<td></td>
</tr>
<tr>
<td>15</td>
<td>Students</td>
<td>1,568</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(2,136)</td>
</tr>
<tr>
<td>16</td>
<td>State grants per pupil</td>
<td>762</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(606)</td>
</tr>
<tr>
<td>17</td>
<td>Current expenditures per pupil¶</td>
<td>6,916</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(1,146)</td>
</tr>
<tr>
<td>18</td>
<td>Log(90th percentile) − Log(10th percentile)</td>
<td>0.421</td>
</tr>
<tr>
<td>19</td>
<td>Coefficient of variation</td>
<td>0.24</td>
</tr>
<tr>
<td>20</td>
<td>Current expenditures per pupil¶ (conditional on positive net change in grants)</td>
<td>6,758</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(1,053)</td>
</tr>
<tr>
<td>21</td>
<td>Current expenditures per pupil¶ (conditional on negative net change in grants)</td>
<td>8,169</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(1,109)</td>
</tr>
<tr>
<td>22</td>
<td>Number of observations</td>
<td>125</td>
</tr>
</tbody>
</table>

Note: The cells are municipality means in panel A and school district means in panel B. Standard deviations are in parentheses. All dollar values are expressed in constant 1999 dollars. The sample is restricted to municipalities and school districts with 200 or more students.

* The market value of property is expressed in millions of constant 1999 dollars.

| ¶ Current expenditures excludes several expenditure categories, including capital expenditures.

Table 2, discussed in more detail below, presents some additional information on the reform. Conditional on receiving a reform grant, the typical municipality receives approximately $1,600 per pupil (see row 8), and the average school district experiences an increase of approximately 200% in state funding (see row 16). The typical donor town remits approximately $1,550 per pupil to the state (see row 9).

B. Are Budgeting Decisions Likely to Express the Preferences of the Decisive Voter?

New Hampshire’s unique form of local government makes it likely that, as B&O assumed, local government spending decisions reflect the preferences of voters. The state has a long tradition of local autonomy, colorfully expressed in the state motto, “Live Free or Die.” Public goods are provided at an extremely localized level and are financed by property taxation. It is the only state never to have enacted a statewide income or sales tax and has the nation’s highest property taxes when measured against income. Although almost all states impose limits on the fiscal autonomy of local governments, New Hampshire does not. For example, many states limit the property tax rate or cap the annual growth in taxable assessments or property tax revenues. New Hampshire municipalities are not subject to such caps and limits (Sexton & Sheffrin, 1995; Michener, 2001; Anderson, 2006).7

The state is also unique in having preserved a form of direct democracy for setting the annual level of local public goods. There are two distinct forms of direct democracy. The first is the traditional open meeting form of government. Proposed budgets are presented at an annual meeting, and residents, acting as the legislature, vote to approve or amend the budget. The second form of direct democracy is the official ballot. Town residents meet and, acting as the legislature, approve or amend a proposed budget. The budget emerging from the meeting is then voted on by ballot at a later date. Finally, a small number of communities are no-meeting jurisdictions where a representative body, typically a city council, sets the annual budget.8

Municipal and school budgets are determined completely independently of each other: a municipal town meeting is held to set the municipal budget, and a school district meeting is held to set the school budget. Each municipality therefore holds its own town meeting annually. New Hampshire school districts take several organizational forms. A majority of towns have school districts that are coterminous with their municipal boundary, and a minority participate in cooperative school districts composed of two or more municipalities. School district budgets are set at meetings in which citizens from the towns comprising the school district act as the legislature. A small number of municipalities send their students to an outside school district and pay tuition to the district receiving the students. These municipalities pay a flat per student fee equal to the average cost of per pupil education in the receiving district. Citizens of the sending municipality cannot vote on the budget of the school district that receives the students.

In the context of the identification strategy used in this paper, which uses variation in state grants induced by the 1999 reform, the assertion that budgeting decisions express the preferences of the decisive voter as assumed by B&O

7 Anderson (2006) notes that the New Hampshire state government requires annual assessments of taxable property. This requirement, however, was put into place after the reform studied in this paper and does not constrain local governments from selecting their preferred level of spending and taxation.

8 For a discussion of New Hampshire political institutions, see Hall and Knapp (2000) and Minard and Gagnon (2002).
requires two assumptions: (a) the decisive voter is fully aware of the existence and structure of the 1999 reform and (b) the political process reflects the decisive voter’s preferences.\(^9\)

Assumption a likely holds for two reasons. First, the reform was the central fact in New Hampshire politics over the time period studied here.\(^10\) Second, to participate in the political process, most voters have to attend a town meeting where the issues are discussed and then voted on. Under these conditions, it is probable that the decisive voter has full information on the reform.

Assumption b likely holds for municipalities engaged in direct democracy. There are no intervening situations to distort the decisive voter’s preferences.\(^11\) Furthermore, the state contains a large number of small jurisdictions, and public goods are financed primarily out of local revenue. These facts suggest there will be a high degree of both Tiebout (1956) sorting (sorting across jurisdictional boundaries in a manner that allows each household to consume their preferred combination of public goods and taxes) and competition among local governments. This competitive environment increases the likelihood that budgeting decisions will reflect voters’ preferences (Brennan & Buchanan, 1980).\(^12\)

Although it is probable that assumptions a and b hold, it cannot be verified. What can be said with more certainty is that the decisive voter’s preferences are more likely to be expressed in budgeting decisions in New Hampshire than in the settings considered in most of the previous literature. I proceed with the unverifiable assumption that budgeting decisions reflect the preferences of the decisive voter and interpret the empirical analysis accordingly.

III. Theoretical Grant Offset Prediction

The following simple model provides a framework for interpreting the empirical analysis. The decisive voter \(i\) in municipality \(m\), assumed to have a single child enrolled in municipality \(m\)’s schools, has the following preferences and budget constraint,

\[
U(i, h, e_m) = c_i + y_i - \tau_m P_i, \quad \text{subject to}
\]

\[
F_m = L_m + G_m,
\]

where \(c_i\) denotes the personal consumption of individual \(i\) (all prices, other than property prices, are normalized to 1 for simplicity), \(h_i\) denotes the quantity of residential property owned by individual \(i\), \(e_m\) denotes the units of per pupil education provided by municipality \(m\), \(y_i\) denotes income, \(\tau_m\) denotes the property tax rate, and \(P_i\) denotes the value of the property owned by individual \(i\). Education is financed by a property tax and grants from the state. (For simplicity, education is assumed to be the only publicly provided good.)

The government budget constraint is given by

\[
F_m = L_m + G_m,
\]

where \(F_m\) is the province level of education; \(F_m = e_m \times S_m\), where \(S_m\) is the number of students in municipality \(m\), \(L_m\) is total local property tax revenue and is equal to the tax rate times the aggregate value of property in town \(m\), and \(G_m\) is the level of the state-provided lump-sum grant.

Define the percentage of total town property owned by the decisive voter as \(\omega_d\) where the subscript \(d\) denotes the decisive voter. Solving for \(\tau_m\) in equation (3) and inserting into equation (2) yields a redefined budget constraint for the decisive voter:

\[
y_d + \omega_d \times G_m = \omega_d \times F_m + c_d.
\]

Equation (4) demonstrates that the decisive voter’s budget constraint is expanded by the grant in proportion to the percentage of total municipal property owned. Define the decisive voter’s preferred level of per pupil education as

\[
e^*_d(\omega_d \times S_m, y_d + \omega_d \times G_m),
\]

and note that

\[
\frac{\partial e^*_d}{\partial G_m} = \frac{\partial e^*_d}{\partial y_d} \times \omega_d.
\]

Equation (6) expresses the fungibility of private and grant income—the essential insight of Bradford and Oates (1971a, 1971b). Inserting the decisive voter’s preferred level of per pupil education into equation (3), differentiating with respect to the grant, \(G_t\), and inserting equation (6) yields

\[
\frac{\partial L_m}{\partial G_m} = \frac{\partial e^*_d}{\partial y_d} \times (S_m \times \omega_d) - 1.
\]

Equation (7) shows the standard offset (or crowd-out) result under property tax financing. Per pupil education provision will increase by the decisive voter’s marginal propensity to spend on education, \(\frac{\partial e^*_d}{\partial y_d}\), times the adjustment factor \(\alpha_s = \frac{\partial y_d}{\partial x_d}\)
pensity to spend on public goods, \( \alpha \),. The adjustment factor is the percentage of property owned by the decisive voter weighted by the number of students residing in the town. Under the assumption that a town’s taxable property is distributed equally among voters and that each voter has a single child enrolled in the town’s school, the adjustment factor, \( \alpha \), equals 1, and the result collapses to the easily interpreted offset result:

\[
\frac{\partial L_m}{\partial G_m} = \frac{\partial e^*_d}{\partial y_d} - 1. \tag{8}
\]

According to estimates in the literature, the marginal propensity to spend on public goods, \( \frac{\partial e^*_d}{\partial y_d} \), is between 0.05 and 0.10 (Hines & Thaler, 1995), implying that \( \frac{\partial L_m}{\partial G_m} \) should equal between −0.90 and −0.95.\(^\text{13}\) Each dollar of grant income received by a municipality will result in a reduction in local funding of 90 to 95 cents, and this reduced spending will be used to finance property tax reduction. Testing this prediction is the primary focus of the empirical section that follows.

IV. Data and Summary Statistics

The data utilized in this paper come from multiple sources. The New Hampshire Department of Education provides data on the reform grants and enrollment by town and school district. The New Hampshire Department of Revenue Administration provides municipal-level tax data. The New Hampshire Office of State Planning provides annual population estimates by municipality. The National Public-Elementary-Secondary Finance Data, produced by the Census Bureau, provides school-district-level finance data. The 2000 Census provides demographic information. All dollar values are converted into constant 1999 dollars. Municipalities with fewer than 200 students in 1998, the year prior to the reform, are excluded from the sample. Several variables, including total state education aid, are provided at the school district level but are used in estimation at the municipality level (and vice versa). These variables are disaggregated to the municipality level using the procedure described in appendix C.

Table 2 presents summary statistics for 1998, the last year prior to the reform, and for 2000, the second year of the reform. Panel A contains municipality means. Local education revenue fell by approximately $830 per pupil between 1998 and 2000 (see row 7), strong evidence that local governments offset a large portion of the reform grants. The $830 decrease in funding likely understates the causal decrease in funding induced by the grants because education revenue was generally increasing during this period. Property tax rates fell approximately 20% over the two years, and tax rate dispersion decreased (see rows 11–13). The decrease in the tax rate must be interpreted cautiously, however. It combines the effects of the decrease in local revenue collection with the effects of a substantial increase in property values that occurred over this period (see row 10). An increase in property values mechanically drives the property tax rate down for a fixed bundle of public goods.

Panel B contains school district means. Both districts that had a net financial gain from the reform and districts that suffered a net loss increased per pupil spending by around $385 (see rows 20 and 21). The comparison suggests that the reform had little influence on educational expenditures. Similarly, there was little change in the dispersion of school district expenditures over this period (see rows 18 and 19).

V. Empirical Model and Identification

The response of local education funding to intergovernmental grants can be assessed with a standard fixed-effects model, estimated by differencing away the fixed effect,

\[
\Delta \frac{L_{mt}}{enroll_{mt}} = \beta \Delta \frac{G_{mt}}{enroll_{mt}} + \eta_t + \epsilon_{mt}. \tag{9}
\]

where \( L_{mt} \) is local funding for education, \( enroll_{mt} \) is the number of students residing in municipality \( m \) at time \( t \), and \( \eta_t \) is a year fixed effect. \( G_{mt} \) is the total amount of education aid provided by the state government. The unit of observation is municipality-year.\(^\text{14}\) The equation relates how grants per pupil, \( \frac{G_{mt}}{enroll_{mt}} \), influence local funding for education per pupil, \( \frac{L_{mt}}{enroll_{mt}} \). \( \beta \) can be interpreted as the extent to which local governments offset education funding from the state.

This estimation strategy suffers from several likely sources of bias. The most serious is time-varying correlation between determinants of local funding and state grants. Such correlation can arise for any number of reasons. Suppose a municipality suffers an economic shock (a plant closure, for example) that reduces its aggregate income. The income shock will cause it to decrease school funding. At the same time, grants given on the basis of community income will increase. Such a situation would generate a spurious negative correlation between grants and local revenue for education. Another source of bias arises from

\(^{13}\) Although the use of 0.05 to 0.1 as the marginal propensity to spend on public goods is nearly ubiquitous in the empirical intergovernmental grants literature, a caveat concerning its use should be noted. The estimate pertains to government services at the state level (Hines & Thaler, 1995). It is possible that the marginal propensity to spend at the local level differs from the propensity at the state level. Note also that the estimate refers to all public goods, not just education. Public goods other than education are examined empirically below.
endogenous response to the grants. Towns receiving a grant may experience in-migration, altering the preferences of the decisive voter, and school district officials may inflate the statistics on which grants are based in order to increase the size of the grant received. Finally, a small portion of state aid, \(G_{mt}\), is provided as matching grants. Equation (9) fails to properly control for the fact that such grants reduce the price of local public goods.

I use an instrumental variables strategy to overcome these problems. The per pupil reform grant, \(\Delta_{enrollmt}^{\text{reform}}\), is used as an instrument for per pupil state grants, \(\Delta_{enrollmt} G_{mt}\), the lump-sum reform grant, issued as part of the 1999 reform, net of state grants cancelled as part of the reform. It equals 0 in all years prior to the reform. \(enroll_{mp}\) is enrollment in year \(p\), the year prior to the reform. The instrument, \(\Delta_{enrollmt}^{\text{reform}}\), is strongly correlated with the change in state grants and therefore meets the first requirement of a valid instrument. The instrument must also be uncorrelated with \(\Delta_{mt}\) in order to be valid. The grants represent a large, sudden spike in state aid produced by the action of an outside body—the courts. They can therefore be viewed as an exogenous shock to the level of grants received by a given municipality. More intuitively, the grant instrument can be thought of as the amount of money won unexpectedly by a town in a lottery.

Despite the exogenous nature of the grants, their magnitude is a function of town characteristics, primarily per pupil property wealth, measured in the year prior to the reform, and these characteristics are likely correlated with unobservable preferences for education spending. These unobservable preferences are controlled for by the municipal fixed effect. Finally, because the grants, \(reform_{mt}\), are functions of town characteristics measured prior to the reform and the denominator of the instrument, \(enroll_{mp}\), is held at its prereform level (\(t = p\), where \(p\) denotes the year prior to the reform), endogenous response to the grants does not bias the instrument.\(^{13}\)

The structural equation is

\[
\Delta_{enrollmt} = \beta_2 \Delta_{enrollmt} + \pi_2 E_{mt} + \phi_2 R_{mt} + \eta_t + \Delta \varepsilon_{mt}. \tag{10}
\]

The structural equation contains several variables in addition to state grants. It contains the per pupil excess tax payment made by a district, \(E_{mt}/enroll_{mt}\). (The excess tax payments are reflected in neither \(G_{mt}\) nor \(L_{mt}\)—that is, the excess tax payments are not literally grants, although they can be thought of as negative grants, nor are they funding for local education.) The variable serves as an important control and is of interest in its own right. The per pupil excess tax payment made by a district, \(E_{mt}/enroll_{mt}\), is instrumented with \(E_{mt}/enroll_{mp}\), the per prereform pupil excess tax payment.

The model developed in section III suggests that \(\beta_2\) should equal \(-0.95\) to \(-0.90\)—each dollar of grant income should reduce the municipalities contribution to local education by 90 to 95 cents. The net impact of the grant introduction and the local funding reduction is to increase local education expenditures by 5 to 10 cents. In contrast, \(\pi_2\) should equal between \(-0.05\) and \(-0.10\). For every dollar that must be remitted to the state, the locality will reduce its funding of local education by 5 to 10 cents, and local education expenditures will fall by the same amount. Note that in addition to the individual predictions for \(\beta_2\) and \(\pi_2\), the theory predicts that \(\beta_2 + \pi_2 = 1\).

Baicker and Gordon (2004) document that school finance reform often leads states to reduce aid in areas other than education. Such a reduction occurred in New Hampshire. Revenue sharing to municipalities was reduced, and municipalities lost a small portion of their property tax base associated with utility properties. In addition, in 2000, the year after the reform was introduced, there was an increase

\[^{13}\] See Cullen (2003) for evidence on school district officials endogenously responding to grants. Endogenous migration is discussed further in section VIA. Another potential source of endogeneity bias is political endogeneity (Knight, 2002). A municipality desiring to make a large capital expenditure might lobby the state government for a grant. This would generate a spurious positive correlation between local revenue for education and grants. Political endogeneity is not a concern for the empirical strategy pursued in this paper because the reform grants are governed by formulas and are not granted on a discretionary basis.

\[^{10}\] The grants cancelled as part of the reform (see table 1) warrant discussion because the instrument is constructed by netting these funds out of the reform grants. These funds were lump sum in nature. A substantial majority of the canceled funds was from a foundation aid program that was explicitly general, untargeted aid (see table 1 of Hoxby, 2001, for summary information on the foundation aid program). Most of the remaining canceled grants came from a program that awarded funds based on the number of kindergarten students. The funds provided by this program would have been sufficient to cover only around a third of the typical per pupil cost of a half-day kindergarten program. An alternative approach to netting the canceled grants out of the reform grants would be to control for them as right-hand-side variables. Results using this approach (unreported) are extremely similar to those reported in the paper.

\[^{17}\] The statewide taxes used to fund the reform grants warrant discussion in the context of the validity of the instrument. The taxes increased, which did not include an income or broad-based sales tax, include the business profits tax, business enterprise tax, meals and rooms tax, tobacco tax, real estate transfer tax, and the rental car tax (Hall, 2001). This list contains only the largest tax increases. The wealth effect associated with these tax increases may reduce local education spending. Because the tax increases were statewide, their impact on local education revenue should be absorbed by the year effects, \(\eta_t\). However, if the incidence of the tax increases on voters differs by municipality, the year effects will not absorb their impact. \(\beta\) remains unbiased as long as the incidence is not correlated with the instrument. Formally, implicit in the statement that the instrument is uncorrelated with the error term is the assumption that the incidence of the statewide tax increases is not correlated with the magnitude of the reform grants. Determining the incidence of the numerous tax increases by municipality is well beyond the scope of this paper. The assumption is quite plausible, however. There is no obvious reason that the incidence of the statewide tax increases should be correlated with the reform grants.
in funding for the few, relatively small, state education grant programs not associated with the reform (for example, aid for districts with an unusually large number of special education students).

These changes are controlled for by including them as a vector of control variables, denoted as \( R_{\text{enroll}} \). Neither revenue sharing nor utility tax revenue is part of state education aid. They are not included in \( G \) and, hence, must be controlled for as right-hand-side variables. The rationale for controlling for the postreform grants, which are part of \( G \), is slightly different. If the postreform grants are correlated with the reform grants and are not controlled for, the first-stage coefficient on the instrument will be biased upward—that is, each dollar of reform grant aid will be associated with more than a dollar increase in total state aid.

Although \( R_{\text{enroll}} \) is potentially an important control, its exclusion from the structural equation has almost no impact on the estimates of \( \beta_2 \) and \( \gamma_2 \) presented below.

The grants were introduced in late 1999, after budgeting decisions for the school year had been made. The law that enacted the reform contained a provision allowing towns to call meetings in order to spend the new funds. Although some municipalities did utilize the grants for educational expenditures in the first year (Hall, 2001), it is possible that other towns were constrained from their optimal expenditures by the timing of the grant introduction. I address this possibility by employing two- and three-year differences of the data. The estimate of offset, \( \beta_2 \), is identified by the difference in local funding from 1998 to 2000 when two-year differences are used. These estimates of offset in the reform’s second year and are referred to as the second year of reform estimates. The three-year difference results identify the estimate of offset from the difference in local funding between 1998 and 2001 and are referred to as the third year of the reform estimates. In 2000 and 2001, grant amounts were known well in advance of the annual meetings that established spending levels. The estimates of offset will reflect the behavioral decision of the decisive voters, not a mechanical response arising from the timing of the grant introduction.

VI. Results

A. Local Revenues for Education and Other Public Goods

Table 3 reports OLS estimates of equation (10). Panel A presents the second year of the reform results, and panel B presents the third year of the reform results. This format is maintained for tables 4 and 5. The OLS estimates, equal to approximately \(-0.5\), suggest that municipalities reduce their own funding of education by 50 cents for every dollar of grant income provided by the state—that is, they offset or crowd out half of the grant funds and spend the other half on education. These estimates imply a considerable flypaper effect but are likely biased for the reasons discussed above.

Column 1 of table 4 reports the first-stage results of the IV estimation of equation (10). (The remaining columns display the first-stage estimates of robustness checks that are described in detail below.) The first stage regresses total state grants per pupil on reform grants per pupil. The coefficients on the reform grant instrument are quite precise and are equal to approximately 1, suggesting that, as expected, each reform grant dollar increases total state grants by $1.

The second-stage estimates are displayed in column 1 of table 5. The second year of the reform estimate is in panel A. This estimate can be interpreted as the causal response of local education revenue to the receipt of grant income. The estimate, \(-0.81\), suggests that the receipt of $1 of grant income induces municipalities to reduce local funding of education by 81 cents. Although the estimate is somewhat smaller than the \(-0.90\) to \(-0.95\) predicted by the standard theory, it does suggest that the basic presumption of the theory holds.

An alternative explanation for the result is that municipalities are initially constrained from spending the grants. Making capital improvements takes time, and there may be a limited supply of teachers available for hire. Panel B therefore presents the results for the third year of the reform. If the short-run constraint theory is valid, the level of offset should decrease over time. Instead, the estimate increases to \(-0.89\)—very close to the theoretical prediction.\(^{19}\)

The most likely violation of the assumption that the instrument is uncorrelated with the error term in equation (10) is correlation between the magnitude of the reform grant instrument and time-varying preferences for education spending. For example, property-wealthy districts may


\(^{19}\) The specifications displayed in table 5 are unweighted, producing estimates that reflect the mean preference of New Hampshire’s decisive voters. Specifications weighted by the number of students residing in a municipality produce estimates very similar to those in table 5.
lost revenue sharing, lost utility tax revenue, and the change in nonreform grants. In column 5, the lagged dependent variable is instrumented as described in the text.

as five years ago, average household income, and quadratics in lagged number of students in residence in the community and median household income. As described in the text, all columns include controls for

in levels: percentage nonwhite in the municipality, unemployment rate, percentage in poverty, percentage of adults with a high school diploma, percentage of adults with a college degree, percentage in same house

with the excess tax payment per prereform number of pupils. All columns, with the exception of column 3, include a vector of year effects. Column 3 utilizes a single difference and includes a vector of covariates

are in parentheses. Municipalities with fewer than 200 students in residence in 1998 are excluded. Panel A employs two-year differences and utilizes differences from 1994 through 2000. Panel B employs three-year

differences and utilizes differences from 1995 through 2001. All columns, with the exception of column 3, include a vector of year effects. Column 3 utilizes a single difference and includes a vector of covariates

Note: The table displays IV estimates. The dependent variable is

municipality per pupil state revenue for education. Municipality-year is the unit of observation. Standard errors clustered by school district are in parentheses. Municipalities with fewer than 200 students in residence in 1998 are excluded. Panel A employs two-year differences and utilizes differences from 1994 through 2000. Panel B employs three-year

differences and utilizes differences from 1995 through 2001. All columns, with the exception of column 3, include a vector of year effects. Column 3 utilizes a single difference and includes a vector of covariates in levels: percentage nonwhite in the municipality, unemployment rate, percentage in poverty, percentage of adults with a high school degree, percentage of adults with a college degree, percentage in same house as five years ago, average household income, and quadratics in lagged number of students in residence in the community and median household income. As described in the text, all columns include controls for lost revenue sharing, lost utility tax revenue, and the change in nonreform grants.

$^a$ Column 4 allows the year intercepts to vary by the quartile of prereform per pupil property wealth.

A: Second Year of Reform

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B: Third Year of Reform

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Note: The table displays IV estimates. The dependent variable is $\Delta$ municipal per pupil state revenue for education. Municipality-year is the unit of observation. Standard errors clustered by school district are in parentheses. Municipalities with fewer than 200 students in residence in 1998 are excluded. Panel A employs two-year differences and utilizes differences from 1994 through 2000. Panel B employs three-year differences and utilizes differences from 1995 through 2001. All columns, with the exception of column 3, include a vector of year effects. Column 3 utilizes a single difference and includes a vector of covariates in levels: percentage nonwhite in the municipality, unemployment rate, percentage in poverty, percentage of adults with a high school diploma, percentage of adults with a college degree, percentage in same house as five years ago, average household income, and quadratics in lagged number of students in residence in the community and median household income. As described in the text, all columns include controls for lost revenue sharing, lost utility tax revenue, and the change in nonreform grants.

$^a$ Column 4 allows the year intercepts to vary by the quartile of prereform per pupil property wealth.
choose to increase their education spending at a faster pace than property-poor districts. Such a scenario would induce correlation between the instrument and the error term because the instrument is largely a function of per pupil property wealth and the municipal fixed-effects will not control for the evolution of spending preferences over time.

The last four columns in table 5 present robustness checks designed to explore the possibility that the instrument is biased by correlation with time-varying factors that influence education spending. The first robustness check enters a set of municipal-specific linear trends into the model to control for municipal trends in education funding. The second robustness check is performed by including a vector of covariates from the 2000 Census in the model. These variables enter the model in levels and control for trends associated with the demographic characteristics of the community. For example, communities with high median household income may increase spending at a faster rate than lower-income municipalities. Inclusion of a quadratic in household income controls for such trends. The Census data are available only in 2000. In order to utilize these demographic characteristics, I remove most of the prereform data from the sample and estimate the model using a single difference (for example, the 2000 – 1998 difference for the second year of the reform estimates in panel A). (See table 5 for a complete list of demographic variables included in this model.) The third robustness check allows the year intercepts to vary by the quartile of per pupil property wealth in 1998, the year prior to the reform. Allowing the year intercepts to vary controls for the possibility that property-wealthy districts are increasing education funding at a faster rate than less wealthy districts. Finally, it is possible that local revenue levels are serially correlated, even after controlling for the fixed effect. The instrument, the per pupil reform grant, is (negatively) correlated with the lagged dependent variable and, under standard assumptions, is uncorrelated with the error term.

The offset result in column (1) is robust to these additional specifications (see columns 2–5). The estimates, which range from −0.75 to −0.98, provide suggestive, although not conclusive, evidence against the hypothesis that the instrument is biased by correlation with time-varying determinants of education funding.

Municipalities that make an excess tax payment to the state experience a negative wealth shock. The theoretical treatment above suggests these municipalities should reduce local education revenue by 5 to 10 cents per dollar of excess tax payment. Although the excess tax coefficients in table 5, which range from −0.04 to −0.11, are close to this prediction, most are imprecisely estimated. A response of the predicted magnitude is likely too small to estimate with precision given the sample size. Theoretically, the response to the excess tax payment plus the response to the state grants should equal −1 (as discussed in section V). The estimates in column 1 of panel A fail to reject this hypothesis with a p-value of 0.40. The remaining columns similarly fail to reject the hypothesis.

The structural equation does not control for time-varying covariates because of the possibility of bias arising from endogenous response to the grants. Perhaps the most significant omitted time-varying covariate is the number of students in a municipality. Table A1 presents specifications identical to those in table 5, but they include the log of the number of pupils as a time-varying covariate. The results are quite similar to those found in table 5. Table A2 and appendix D present and discuss the full regression results for table 5, panel A. Attempts to determine in which area of school expenditures the portion of the nonoffset grants were used, such as current school expenditures and capital expenditures, produce imprecise estimates (see the working paper version of this paper: Lutz, 2006, table 10). Finally, the interpretation of the empirical results in 5 implicitly assumes that the identity of a town’s decisive voter does not change endogenously as a result of the reform. This assumption is supported by a failure to find any evidence that the 1999 reform induced migration across town boundaries (see the working paper version of this paper: Lutz, 2006, table 11).

Table 6 presents two final robustness checks. The first is a falsification test. The data are restricted to the years prior to the reform, 1992 through 1998, and a “placebo” grant is generated by giving each municipality in 1998 the grant it

\[
\Delta \frac{L_{it}}{enroll_{it}} = \beta_2 \Delta \frac{G_{it}}{enroll_{it}} + \pi_2 \frac{E_{it}}{enroll_{it}} \\
+ \phi_2 \frac{R_{it}}{enroll_{it}} + \kappa_2 \Delta \frac{L_{it-1}}{enroll_{it-1}} + \eta_i + \Delta \varepsilon_{2,mt}.
\]

Equation (11) is not consistently estimated by OLS due to mechanical correlation between the error term and the lagged dependent variable: \(\text{corr}(\Delta \frac{L_{it}}{enroll_{it}}, \Delta \varepsilon_{2,mt}) > 0\).

The second lag of the dependent variable in levels, \(\frac{L_{it-2}}{enroll_{it-2}}\), is used as an instrument for the lagged dependent variable \(\Delta \frac{L_{it-1}}{enroll_{it-1}}\) and, under standard assumptions, is uncorrelated with the error term.

The fixed-effect lagged dependent variable model, equation (11), requires the assumption of sequential exogeneity conditional on the fixed effect. For simplicity, assume the following dynamic model with fixed effect: \(y_{it} = \beta y_{it-1} + \delta_i + \varepsilon_{it}\). Sequential exogeneity conditional on the fixed effect requires \(E(\varepsilon_{it}|y_{it-1}, y_{it-2}, \ldots, y_{it-3}, \delta_i) = 0; t = 1, 2, \ldots, T\). An important implication of the sequential exogeneity assumption is that the error terms are conditionally serially uncorrelated: \(E(\varepsilon_{it}\varepsilon_{s,t-1}, y_{it-3}, \ldots, y_{it-3}, \delta_i) = 0, \forall t \neq s\). Anderson and Hsiao (1981) propose the instrumenting strategy.
The 2002 grant variable is problematic. It is based on school and community characteristics in 1999—the first year of the reform. Use of the reform grants as an instrument in 2002 introduces the possibility of endogenous response bias. To address this possible bias, the instrument is lagged one year. Instead of using the 2002 reform grants as an instrument, the 2001 reform grants are used as a proxy for the 2002 grants. The reform grants prior to 2002 were based on community and student characteristics measured prior to the reform and do not suffer from endogenous response bias.

The estimated level of offset in 2002 is virtually identical to the level of offset in 2001. This suggests the grants were considered permanent prior to the formal announcement by the legislature and that the results based on 2001 (table 5, panel B) reflect the permanent response to the grant introduction.

Although the results in tables 5 and 6 confirm that the grants were largely offset and failed to increase education spending, they are insufficient to confirm the full B&O prediction that grants are equivalent to a tax reduction. The grants could have been used to fund noneducation local public goods. The wealth effect associated with the grant income would be expected to increase the provision of noneducation public goods by only a few cents on the dollar. Previous research, however, has shown that grants often significantly influence government spending in areas other than that targeted by the grant (Craig & Inman, 1982; Baicker & Gordon, 2004).

The impact of the grants on noneducation public goods is estimated by the following specification,

$$
\Delta \frac{O_{mt}}{pop_{mt}} = \beta_o \frac{reform_{mt}}{enroll_{mt}} + \pi_o \frac{E_{mt}}{pop_{mt}} + \phi_r \frac{R_{mt}}{enroll_{mt}} + \eta_t + \epsilon_{0,mt}.
$$

where $O_{mt}$ is the level of funding for local public goods other than education in municipality $m$ at time $t$ (which comes solely from property taxation) and $pop_{mt}$ is the population. $\frac{reform_{mt}}{pop_{mt}}$ is instrumented with $\frac{reform_{m,p}}{pop_{m,p}}$ (where $p$ is the year prior to the reform). The approach can be viewed as reduced form in that $\frac{reform_{mt}}{pop_{mt}}$ enters the second stage directly.

The estimates from equation (12) are presented in table 7. When interpreting these estimates, it is important to note that, as discussed more fully in section IIB, education and noneducation local public goods budgeting is completely segmented in New Hampshire. Two distinct town meetings

<table>
<thead>
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<th>Table 6.—Effect of Change in Education Revenue from the State on Local Education Revenue Robustness Checks</th>
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<tbody>
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<td>Reduced Form—Two-Year Differences</td>
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</tr>
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<td>$\Delta$ placebo per pupil net reform grant</td>
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<td>$\Delta$ per pupil net reform grant</td>
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Note: Municipality-year is the unit of observation. The dependent variable is a municipal per pupil local revenue for education. Standard errors clustered by school district are in parentheses. Municipalities with fewer than 200 students in residence in 1998 are excluded. Columns 1 and 2 are reduced-form-style estimates (see the text) and use two-year differences. Column 1 utilizes differences from 1994 through 1998, and column 2 utilizes differences from 1994 through 2000. Columns 3 and 4 are IV estimates and use four-year differences. Column 3 utilizes differences from 1998 and 2002. Column 4 utilizes a single difference. All columns, other than column 4, include a vector of year effects. In column 1 the per pupil net reform grant is a placebo generated by setting the variable equal to its 2000 value in 1998. In column 2, the per pupil net reform grant is instrumented with the net reform grant per pupil reform number of pupils. In columns 3 and 4, per pupil revenue from the state is instrumented with the one-year lag of the net reform grant per pupil reform number of pupils. The use of a lagged instrument accounts for the possibility of endogenous response bias in the unlagged instruments. See the text for additional information. As described in the text, all columns include controls for lost revenue sharing, lost utility tax revenue, the change in nonreform grants, and excess tax payments.
are held, and the budgets, once passed, are controlled by distinct, unconnected government bodies: the school district and the town government. There is no evidence that the reform grants influenced noneducation public goods. The table does, however, provide some evidence that the excess tax payments reduced the provision of other local public goods by 10 cents per dollar of excess tax payment in the second year of the reform (see panel A), within the range of the theoretical prediction. There is no evidence, however, of an effect in the third year of the reform.

The reform grants could have been used for only three purposes: education, other local public goods, and tax reduction. The results presented in tables 5 to 7 jointly suggest that only a small portion of the grants was used to fund public goods. It can therefore be concluded that the grants were mostly used to reduce property taxes.

From a policy perspective, one way to judge the reform is by assessing its success at addressing the two forms of fiscal equity: expenditure equity and taxation equity (Inman & Rubinfeld, 1979). Expenditure equity concerns the relationship between expenditures and income, while taxation equity concerns the relationship between tax burden and income. The school finance reform in New Hampshire was largely unsuccessful in addressing expenditure equity. Despite the stated goal of increasing education funding for low-income communities, only around 10 cents per grant dollar were used to increase education expenditures.

The reform was more successful at addressing taxation equity. The introduction of the grants substantially reduced dispersion in property tax rates. An offset estimate of $-0.9$, roughly the midpoint of the range of estimates presented above, implies the reform grants caused the property tax rate log 90-10 ratio to fall from 0.65 to 0.45 and the coefficient of variation to fall from 0.24 to 0.19. Although these are significant reductions in dispersion, the interpretation is unclear. Equity is typically considered in relation to income. A potentially more meaningful assessment of tax equity is the elasticity of property tax burden with respect to income (which should be interpreted as a descriptive statistics, not a behavioral parameter). An elasticity of 1 indicates a neutral tax—the tax burden increases proportionally with income. The reform increased the elasticity from 0.42 to 0.64, a significant reduction in the regressivity of the property tax. Judged by this metric, the grants were quite successful at reducing taxation inequality in New Hampshire. The recent literature on school finance reform has extensively considered expenditure equity but has overlooked taxation equity. The analysis presented here suggests this may be an important component of the cost-benefit analysis of school finance reform.

### B. Extensions: Heterogeneity by Institutions and Income

Many of the explanations for the flypaper effect concern the type of political institutions within which public goods provision decisions are made. New Hampshire provides a unique opportunity to assess these explanations. Columns 1 and 4 of table 8, which present the second and third years of the reform estimates, respectively, restrict the sample to the set of towns that operate their own single-municipality school districts (they are not part of a cooperative) and use direct democracy for determining the school budget.

Some of these towns do not actually operate a school; instead they send their students to a neighboring school district and pay tuition. Citizens of towns that pay tuition cannot vote on the school budget; they simply pay a flat per student fee equal to the average cost of per pupil education in the receiving district. Furthermore, tuitioning agreements are typically many years in length contractually. Towns that pay tuition therefore have no ability to adjust their education spending; they can neither adjust per pupil expenditures nor can they, in the short run, remove their students from the district to which they are being sent. These towns are constrained from spending the reform grants on education.

The specifications in columns 1 and 4 allow the level of offset to vary by whether a town pays tuition in order to capture the impact of this constraint.

The set of municipalities that operate their own districts, are engaged in direct democracy, and do not pay tuition provide what is perhaps the cleanest of the B&O theory. These towns

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21 The causal decrease in dispersion implied by the offset estimates is significantly larger than that suggested by the raw data in table 2. The difference is accounted for by the rapid increase in property values that caused an increase in dispersion over the same period that the grants were decreasing dispersion.

22 The elasticity of property tax burden with respect to income is estimated by the following cross-section regression: $\ln(p_{taxm}) = \alpha + \beta \ln(average_{income}m) + e_m$, where $p_{taxm}$ is the mean individual property tax payment made in municipality $m$, defined as total tax payments divided by municipal population, and $average_{income}m$ is average household income in municipality $m$.

---

**Note:** The table displays reduced-form style estimates. The dependent variable is $\Delta$ municipal per resident revenue for public goods other than education. Municipality-year is the unit of observation. Standard errors clustered by school district are in parentheses. Municipalities with fewer than 200 students in residence in 1998 are excluded. Panel A employs two-year differences and utilizes differences from 1994 through 2000. Panel B employs three-year differences and utilizes differences from 1995 through 2001. Per resident net reform grant is instrumented with the net reform grant per prereform number of residents. Excess tax payment per resident is instrumented with the excess tax payment per prereform number of residents. As described in the text, controls for lost revenue sharing, lost utility tax revenue, and the change in noneform grants are included, as are year effects.
have absolutely no institutional arrangements that might distort budgeting decisions away from the preferences of voters. In addition, the data for these municipalities do not suffer from possible measurement error associated with mapping some of the variables from school districts to municipalities (see appendix C). The offset estimate for these towns is $-0.93$ in the second year of the reform and $-0.88$ in the third year (see the first row of columns 1 and 4, respectively). As expected, the towns that tuition students offset an even larger portion of the grants.

Columns 2 and 5 of table 8 present estimates utilizing the full sample of municipalities. The extent of offset is allowed to vary by whether a town is a no-meeting jurisdiction for school budgeting decisions (direct democracy is not used to set the school budget). The results reveal that the no-meeting towns—those with representative democracy—respond to the grants in a manner consistent with voter preferences. The second relates to tax competition (Tannenwald, 2005). The magnitude of the grants, and the fact that they were mostly offset, led to significant decreases in tax burdens across the state. Had representative democracy municipalities not offset their grants, they would have experienced significant increases in their tax burdens relative to their neighbors. Although commercial property owners cannot vote on municipal or school budgets, they do choose where to locate their businesses. The towns engaged in representative democracy may have feared losing their commercial tax base if they failed to lower their tax burdens as other municipalities had done. Policymakers may also have feared losing future residential additions to their tax bases due to Tiebout-style competition for residents. Indeed, Lutz (2009) demonstrates that the change in relative tax burdens induced by the reform caused the building of new homes to shift toward those communities whose tax burden had been reduced.

The standard B&O offset prediction derived in section III implicitly assumes that all decisive voters have the same marginal propensity to spend on public goods. This assumption may not hold. In particular, marginal propensities may differ by income. Stated differently, the Engel curve for education—the relationship between demand for education and income—may be nonlinear. In order to assess this possibility, columns 3 and 6 allow the level of offset to vary with the median household income of a municipality. Both the second and third years of the reform estimates reveal that low-income communities have a higher marginal propensity to spend on education than do high-income communities (the income interaction term coefficients have t-stats of 2.11 and 1.82 for the second and third years of the reform, respectively). The third-year estimates imply grant offset of $-0.87$, $-0.91$, and $-0.98$ for the 25th, 50th, and 75th percentile of median income municipalities, respectively. A more flexible specification, which allows the level
of offset to vary nonlinearly with median household income, produces very similar conclusions (see figure 1). The specification utilizes an interaction between the reform grant per prereform number of pupils and a quartic in median household income. The main effect net reform grant per prereform number of pupils is also included. The figures display the estimates from the 5th percentile to the 95th percentile of median household income for the group of municipalities that received a positive reform grant.

These results suggest the Engel curve for education is concave. Lower-income municipalities offset grants to a lesser extent than wealthier municipalities, indicating that such communities have a higher marginal propensity to spend on education. Concave Engel curves exist under several different scenarios. For example, if all decisive voters in New Hampshire have identical preferences and there are decreasing marginal returns to education expenditures, the Engel curve will be concave.

Concave Engel curves have important implications for previous estimates of grant offset. The extent of grant offset in this paper is estimated using most of the New Hampshire income distribution. Over 90% of the New Hampshire population lives in a community that receives a reform grant, and the empirical model is unweighted. Communities at different points in the income distribution therefore receive equal weight in estimating the parameters. Many previous papers, however, have focused on grant programs targeted at low-income communities (e.g., Card & Payne, 2002). The Engel curve results from this paper suggest that such communities have a relatively high marginal propensity to spend on education. Estimates of grant offset from these communities may be lower than the expected −0.90 to −0.95 even if voters or policymakers view them as fully fungible with private income. Concave Engel curves may therefore be a partial explanation for the apparent failure of the theory in the previous literature.

VII. Conclusion

The estimates presented in this paper suggest that the response of New Hampshire’s decisive voters to the 1999 school finance reform was to offset from 75 cents to 100 cents per grant dollar, a range that encompasses the theoretical prediction of 90 to 95 cents. A relatively heavy weight should be placed on the estimates produced by restricting the sample to municipalities operating their own district, utilizing direct democracy, and not paying tuition (table 8). These municipalities provide an especially straightforward test of the theory and produce estimates ranging from 88 to 93 cents. In a setting in which voters are well informed and can easily express their preferences through the political process, the basic premise of Bradford and Oates (1971a, 1971b) that lump-sum grants are equivalent to a tax reduction is validated.

The results of this paper have implications for future work on intergovernmental grants. Although they suggest possible that lower-income communities face different prices for education—that the tax price systematically varies by income—which may have an impact on the level of grant offset. A theoretically and empirically important predictor of tax price is the percentage of nonresident property (Ladd, 1975; Anderson, 2005). The number of children per capita may also influence grant offset through tax price effects, as well as through other channels (Ladd, 1975; Bergstrom, Rubinfeld, & Shapiro, 1982). Unreported results, available from the author on request, find no evidence of heterogeneity in the extent of grant offset by the percentage of nonresident property (defined as both commercial property and nonresident personal property) in a municipality or the number of students per capita in a municipality.
that the flypaper effect fails or is small in a setting similar to that assumed by the standard theory, they should not be taken as indicating that the flypaper effect never operates. Indeed, the results have important implications for situations in which the flypaper effect does operate. They suggest that efforts to explain the flypaper effect should start with, or at minimum seriously consider, traditional explanations such as voter misperception or bureaucratic capture. These explanations focus on institutional factors that prevent the decisive voter’s preferences from being expressed. Strumpf (1998), which suggests that the degree of voter misperception produces heterogeneity in the flypaper effect, is a useful step in this direction. Similarly, work exploring reasons that decisive voters might rationally consider grant income to be nonfungible with private income would be productive. Singhal (2008), which suggests the flypaper effect arises out of the dynamic interaction between politicians and special interest groups, is an example. More work in the vein of these two papers is needed. Similarly, but more broadly, efforts to understand why governments fail to behave as predicted by standard neoclassical theories of individual behavior should consider explanations focused on factors that distort government decisions away from individual preferences, as opposed to behavioral theories that suggest individuals fundamentally treat public goods differently from private goods.

The results presented here suggest the Engel curve for local public expenditures may be concave. Concave Engel curves have important implications for estimates of grant offset. If grants are systematically targeted at low-income communities, grants may “stick” at a higher rate than the predicted 5 to 10 cents on the dollar because of a relatively high marginal propensity to spend on public goods in these communities. It is important to distinguish between this phenomenon and grants sticking because of a lack of fungibility between private and grant income.

Finally, the results of this paper have important policy implications. Grant programs need to be designed carefully if they are to meet their objectives. The New Hampshire reform contains no incentives to induce municipalities to increase their spending (such as a matching mechanism to reward additional local spending) and no elements to coerce municipalities to increase their spending (such as mandating that local funding for education not be decreased after the grant introduction). As a result, the grants failed to increase education funding for low-income communities, a primary goal of the New Hampshire reform and of school finance equalization in general. The reform did, however, significantly reduce taxation inequality in New Hampshire. If taxation equity is valued by policymakers, it is a significant piece of the cost-benefit analysis of school finance reform that has been overlooked by the recent literature on school finance reform.

REFERENCES


APPENDIX A

School Finance Reform in New Hampshire Prior to 1999

Prior to the 1999 reform there were several attempts at reforming school finance in New Hampshire through the state courts. The most recent was the 1982 *Jessen v. Board of Education*, suit which was settled out of court when the state legislature enacted a minor reform. It was the habitual failure of the legislature to fully fund this form of aid that led to the *Claremont* suit being filed in 1991. The *Claremont* reform is part of the national third wave education reform based on state constitutional clauses guaranteeing an adequate education.25 These reforms began with a 1989 ruling by the Kentucky Supreme Court. From 1990 through 1999, eleven states enacted these types of reforms. Of particular importance for New Hampshire was an early 1990s ruling in Massachusetts. New Hampshire’s constitution is copied in large part from that of Massachusetts, and the educational clauses in the two constitutions are virtually identical. As a result, when the *Claremont* case was being decided, the New Hampshire Supreme Court had a ruling on a very similar suit based on the exact same constitutional language. The Massachusetts decision was cited at length in the *Claremont* decision. The Kentucky decision is also quoted. It seems clear that it was the development of legal doctrine in other states that dictated the *Claremont* case’s success as compared with the failure of earlier suits.

**APPENDIX B**

Implementation of the 1999 New Hampshire School Finance Reform

Under the reform, a statewide property tax was introduced. The tax imposes a uniform rate on all property in the state that is collected and retained locally. A “cost of adequate education” is also calculated for each town. The adequate education amount is a function of the number of students in residence in the municipality, with adjustments for the number of high school students, special education students, low-income students, and the transportation costs of a district. If the amount raised by the statewide property tax in a municipality is less than the municipality’s adequate education amount, the state issues a grant to make up the difference. These grants are referred to as reform grants. Alternatively, if the amount collected by the tax exceeds the adequate education amount, the excess is remitted to the state. Twenty percent of New Hampshire communities, containing 9% of the state’s population, made these payments in the first year of reform. The most important determinant of the magnitude of the magnitude of the reform grants and excess tax payments is per pupil property wealth.

The statewide property tax can therefore be viewed as having two components: excess tax payments and the locally retained portion. The locally retained portion is simply a relabeled property tax. These funds were previously collected by the towns and continue to be collected by the towns, and they are retained locally (Hall, 2001). Furthermore, the locally retained portion of the statewide property tax is completely inframarginal: every community, both prior to and after the reform, chose to raise funds significantly in excess of the amount required by the statewide tax. Although the tax was important from a legal standpoint—it allowed the reform to meet the mandate of the *Claremont* decision—from an economic perspective, it was a meaningless relabeling. For these reasons, in the empirical work, locally retained portion of the statewide property tax is considered to be local revenue for education (it is included in the $L_d$, variable). The second component of the state tax, the excess tax

| TABLE A1.—IV: EFFECT OF CHANGE IN EDUCATION REVENUE FROM THE STATE ON LOCAL EDUCATION REVENUE CONTROLLING FOR CHANGES IN ENROLLMENT |
|---------------------------|-----------------|-----------------|-----------------|-----------------|-----------------|
|                                | (1)             | (2)             | (3)             | (4)             | (5)             |
| A: Second Year of Reform      |                 |                 |                 |                 |                 |
| $\Delta$ per pupil revenue from the state | $-0.85$  | $-0.79$  | $-0.80$  | $-0.79$  | $-0.86$  |
| $\Delta$ per pupil excess tax  | $-0.11$  | $-0.12$  | $-0.05$  | $-0.11$  | $-0.10$  |
| $L_d$ lagged dependent variable  | $0.09$  | $0.04$  | $0.03$  | $0.04$  | $0.08$  |
| Number of observations  | $640$  | $640$  | $640$  | $640$  | $640$  |
| B: Third Year of Reform       |                 |                 |                 |                 |                 |
| $\Delta$ per pupil revenue from the state | $-0.91$  | $-0.80$  | $-0.94$  | $-0.96$  | $-0.82$  |
| $\Delta$ per pupil excess tax  | $-0.10$  | $-0.10$  | $-0.03$  | $-0.11$  | $-0.11$  |
| $L_d$ lagged dependent variable  | $0.54$  | $0.54$  | $0.54$  | $0.54$  | $0.54$  |
| Number of observations  | $483$  | $483$  | $483$  | $483$  | $483$  |
| Linear trends  | $X$  | $X$  | $X$  | $X$  | $X$  |
| Vector of covariates  | $X$  | $X$  | $X$  | $X$  | $X$  |

Note: The table displays IV estimates. The dependent variable is $\Delta$ municipal per pupil local revenue for education. The table displays estimates from specifications identical to those displayed in Table 5 with the exception that all columns include the $\Delta \log$ of enrollment as a covariate. See the notes to Table 5 for additional information.

25 The first wave began with the *Brown v. Board of Education* desegregation case and occurred in the federal courts. The second wave was based on equal protection clauses of state constitutions and occurred in state courts.
payments, will influence the behavior of the decisive voter under the
theory developed in section III and is explicitly included as a right-hand-
side variable in the estimating equation.

APPENDIX C

Data Appendix

Several variables used in estimation at the municipal level are available
only at the school district level and vice versa. These municipal-level vari-
able are generated by mapping the variables from the district level to the
municipal level. In cases where a municipality is part of only a single school
district, the mapping is one-to-one. When a municipality is part of two or
more school districts, assumptions must be made. The New Hampshire
Department of Education divides the tax base of each municipality into
separate pieces corresponding to the school districts the municipality partic-
ips in. This is done on the basis of the percentage of total students in the
municipality attending each school district. I follow this convention. Variables
are mapped from the school district to the municipal level based on the
percentage of total municipal students enrolled in each relevant school
district. The empirical results are robust to alternative mappings—for in-
stance, mapping based on the percentage of total municipal education revenue
going to each relevant school district in the year prior to the reform.

The municipal population variable is an estimate produced by the New
Hampshire Office of State Planning, except in 2000, when the Census
population figure is used. Many municipalities have large jumps in the
population variable in 2000 due to error in the non-Census estimates. It is
likely that these errors were gradual and accumulated over the decade as time
passed from the 1990 Census. I therefore apportion the error in the population
estimate, revealed by the 2000 Census, equally over the years from 1991 to
1999. This produces a substantially smoother population variable.

The observation from the municipality of Seabrook is omitted. Seabrook
contains a nuclear power plant. The plant was successively devalued over the
course of the 1990s, and as a result, Seabrook lost close to $800 million in
property value, a situation that generates uncertainty concerning the data
quality of the variables pertaining to property wealth and property taxes
(specifically, there appears to be longitudinal inconsistency in how the
contribution of the power plant to Seabrook’s tax base and tax revenue is
handled). This is a unique situation unrelated to the school finance reform.
Two municipalities participating in interstate school districts (both are in
cooperatives with municipalities in Vermont) are omitted from the sample.
These two municipalities are dropped due to longitudinal inconsistency in the
way revenue from the Vermont municipalities is handled.

APPENDIX D

Full Regression Results of Table 5

Table A2 presents the full set of coefficient estimates for the specifications
displayed in column 1, table 5 (with the exception of the year effects). The
additional coefficient estimates displayed are lost municipal revenue sharing,
lost utility tax revenue, and the change in nonreform grants. See the notes for table 5 and
appendix D for additional information.

<table>
<thead>
<tr>
<th></th>
<th>Second Year of Reform</th>
<th>Third Year of Reform</th>
</tr>
</thead>
<tbody>
<tr>
<td>Δ per pupil revenue from the state</td>
<td>−0.81 (0.07)</td>
<td>−0.89 (0.09)</td>
</tr>
<tr>
<td>Δ per pupil excess tax</td>
<td>−0.08 (0.07)</td>
<td>−0.09 (0.07)</td>
</tr>
<tr>
<td>Δ lost revenue sharing</td>
<td>0.53 (0.53)</td>
<td>0.82 (0.72)</td>
</tr>
<tr>
<td>Δ lost utility tax revenue</td>
<td>0.86 (0.82)</td>
<td>0.85 (0.89)</td>
</tr>
<tr>
<td>Δ nonreform grants</td>
<td>1.31 (0.94)</td>
<td>1.32 (1.09)</td>
</tr>
</tbody>
</table>

Number of observations

The state reduced its level of direct financial support to municipalities
when it enacted the 1999 education finance reform. The lost revenue
sharing, which was lump sum in nature, is a negative wealth shock to the
community. Assuming a marginal propensity to spend on education of 5
cents per dollar, the expected point estimate on this variable is .95. The
municipality will replace the lost funds, with the exception that the
negative wealth shock will induce a 5 cents per lost revenue-sharing dollar
decrease in education expenditures.

The locally retained portion of the statewide property tax is simply a
relabeling of the existing local property tax. An exception to this pertains
to utility property. The state retains 100% of revenue collected by the
statewide property tax on utility property. This revenue previously was
collected by the municipalities. The expected sign on this variable is −.05.
The municipality will increase the tax rate to replace the lost tax revenue
but will decrease total education funding by 5 cents on the dollar in
response to the negative wealth shock.

Finally, in 2000, the state expanded several nonreform, lump-sum
education grant programs. Failure to control for these new grants might
bias the first stage. The expected coefficient on this variable is −.90 to
−.95.

Table A2 shows that the coefficients on the two lost revenue variables
are imprecisely estimated. There is no evidence of a response to these
negative wealth shocks. Their inclusion, however, ensures that the coef-
ficient estimates of interest, change in state aid and excess tax payment, do
not suffer from omitted variable bias. The coefficient on the nonreform
grant variable is also imprecise.