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How Elastic is Residential Capital Investment to Property Taxation? Evidence from Court Induced Tax Changes

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Abstract

The property tax is the most significant local tax in the U.S. This study assesses the impact of property taxation on residential capital investment decisions using a unique school finance reform in the state of New Hampshire which produced large changes in property tax rates across the state. The estimates suggest that the community receiving the typical fiscal shock, equal to fifteen percent of the community's total pre-reform tax revenue, experienced an increase in residential construction of approximately sixteen percent. A series of robustness checks and falsification tests provide assurance that the estimates reflect the causal response to the fiscal shock. There is significant geographic heterogeneity in the response. The housing market cleared the shock through a quantity (i.e. investment) adjustment in most of the state and a price (i.e. capitalization) adjustment in the suburban ring near Boston. The differing response is attributed to differing housing supply elasticities. The paper's findings have relevance for the analysis of policies, such as school finance reform, which induce changes in rates of property taxation across communities. The findings are also relevant to the theoretical debate over the incidence of the property tax and the large literature on the capitalization of local amenities.

*Board of Governors of the Federal Reserve, M.S. # 83, 20th & C Sts., NW, Washington DC 20551-0001; Byron.F.Lutz@frb.gov. **Preliminary - do not cite without permission.** The opinions expressed here are those of the author and not necessarily those of the Board of Governors of the Federal Reserve System or its staff. I thank Laurel Beck for research assistance. I thank Doug Hall of the New Hampshire Center for Public Policy for help in understanding New Hampshire's political institutions and the 1999 reform. Sallie Fellows and Ron Leclerc of the New Hampshire Department of Education provided assistance with data and background information on the New Hampshire educational system. I thank Cindy Currier, Jim Currier, Jeff Reed and Meagan Reed for the discussions which generated my initial interest in the project. The following individuals provided useful comments and suggestions: Josh Angrist, David Autor, Tom Davidoff, Michael Greenstone, Josh Gallin, Jon Gruber, Chris Hansen, Bill Kerr, Ashley Lester, Adam Looney, Raven Saks and Federal Reserve Board Lunchtime Seminar participants. All errors are my own. The National Science Foundation provided support.

I Introduction

The property tax is the dominant form of local taxation in the U.S. It accounts for seventy-five percent of local taxes and is equal to three and a half percent of personal income in the U.S. (Duncombe and Yinger 2000). Residential capital, at least in the long run, is mobile. It may respond to relatively high levels of taxation by moving to lower tax jurisdictions. Given the magnitude of the property tax, such locational distortions are likely of economic significance.

This paper asks the question : how does residential capital investment respond to differences in property tax rates across communities? While the primary focus of the paper is on the investment response to tax differentials, the housing market, like any market, potentially clears through both a quantity and a price response. The paper therefore also examines how housing prices respond to property taxation. Stated most broadly, the paper examines the way in which the housing market adjusts to property tax differentials.

Relative to the importance of the property tax in the U.S. system of fiscal federalism, the response of residential investment to property taxation has received very limited attention. Wasmmer (1993) assesses the connection between property taxation and residential housing capital intensity and Ladd and Bradbury (1988) examine the link between the property base, which includes both residential and business capital, and property tax rates. Although neither paper directly examines the impact of property taxes on investment, both papers do find a negative association between the capital stock and property taxes.

Unlike the investment, or quantity, response, the price response to tax rate differentials is the subject of a large body of research. Such research comprises a portion of the voluminous literature on the capitalization of local amenities into housing values. These studies attempt to estimate the value home-owners place on amenities and, as is the case with property taxes, disamenities by examining their impact on housing prices. The starting point of the literature is Oates (1969), who examines the relationship between housing values and local spending and property taxation. Recent examples estimate the capitalization of education spending (e.g. Black 1999, Barrow and Rouse 2004), environmental amenities (e.g. Chay and Greenstone 2005) and property tax rates[1] (e.g. Palmon and Smith 1998). These studies often assume either implicitly or explicitly that the stock of housing is fixed – i.e. that housing supply is perfectly inelastic.[2] With supply fixed, the

[1] See Yinger, Borsch-Supan, Bloom and Ladd (1988) for a review of the older literature on the capitalization of property taxes.

[2] For example, many of the studies which examine the capitalization of local government spending rely on the model developed in Brueckner (1979). While it is explicitly acknowledged in the paper that the housing stock will likely respond in the long-run to differences in spending and taxes across communities, the model explicitly assumes

housing market clears a shock to local amenities exclusively through a price adjustment.

There are several arguments made to justify the assumption of perfectly inelastic housing supply. Community boundaries are fixed (Epple and Romer 1989) and the housing supply cannot be elastic in a jurisdiction with fixed boundaries (Ross and Yinger 1999). While this argument may be true in communities which are fully developed, it is not necessarily true if developable land exists. Land will be bid away from an alternative use, such as agriculture, and into residential use when residential rents exceed agricultural rents – i.e. when residential rents exceed the opportunity cost of development (Capozza and Helsley 1989). A positive price shock will induce the least productive agricultural plots to covert. The price shock thereby increases housing supply. A recent example of housing supply elasticity arising from land conversion is provided by northern California. Timber companies are rapidly selling off land that has become more valuable for residential development than for logging (Eilperin 2006).

Zoning potentially makes the supply of housing in a community inelastic. Communities may zone to prevent newcomers from free-riding on the existing tax base – i.e. consuming more in local public goods than they pay in local taxes (Hamilton 1976a) – or they may vote to zone in a manner which maximizes the value of their residential property (Fischel 2001a). Mayer and Somerville (2002) document that land use regulation reduces the elasticity of supply. Similarly, Glaeser and Gyourko (2002) present evidence that zoning restricts housing supply. They find, however, that the extent of this restriction varies greatly across the U.S. and that zoning has minimal effect on supply in large parts of the country.

If housing supply is not perfectly inelastic, a shock to tax rates may induce both a price and quantity response. If land is earning a rent there is an incentive for landowners to shift land into the scarce activity (Hamilton 1976a), in this case residential housing with an advantageous tax burden. In the presence of a supply response, you cannot predict the price effect, the extent of capitalization, without knowing the supply elasticity (Hamilton 1976b). If housing is in elastic supply, the price response will be muted compared to the case in which supply is perfectly inelastic. If supply is very elastic, there may be little to no price response.

The impact of elastic housing supply on capitalization has received only limited attention. Glaeser, Gyourko and Saks (2006) discuss how variation in the elasticity of housing supply across metropolitan areas affects the degree to which demand shocks result in capitalization versus an increased quantity of housing. Hilber and Mayer (2004) use the amount of developable land as a measure of the elasticity of housing supply in a community. Their empirical work documents that

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a fixed supply of housing.

public school spending capitalizes at a higher rate in communities with less developable land.[3]

The empirical challenge in assessing the connection between residential capital investment and property taxation is the endogeneity of tax rates to determinants of investment. For instance, tax rates may be high in jurisdictions which provide positive amenities such as good schools. Capital will reallocate geographically in response to property tax differentials only if the differentials are not associated with differences in the level of services provided to capital (Nechyba 2000). Similarly, capitalization of tax differences will occur only if they reflect a difference in the fiscal surplus between communities – the difference between the value of local public goods consumed and the local tax burden (Hamilton 1976a). An empirical assessment of the effect of local taxes on residential investment and property values must hold all other factors constant. In particular the bundle of public goods provided in a community must be held constant.

This paper overcomes the econometric identification problem by exploiting an unusual school finance reform in New Hampshire. In 1999, the state government issued large lump-sum grants to most municipalities in the state in an effort to reduce disparities in both per-pupil education funding and tax burdens. Eighty to one hundred percent of the grants were used to fund property tax reduction and the remainder were used to fund increased education spending (Lutz 2006). Because most of the grant funds were used to enact tax reduction, they can be viewed as an exogenous shock to the fiscal surplus available in a community. The shock to a given community is transparent and can be summarized by a single variable, a fact which makes interpreting the empirical results straightforward.

Three elements of the New Hampshire setting increase the likelihood of successfully estimating an effect of property tax burdens on investment. First, New Hampshire contains numerous small jurisdictions. The costs associated with capital mobility are relatively small in this setting, increasing the probability of a tax rate shock inducing investment to relocate geographically.[4] Second, the magnitude of the property tax in New Hampshire, the state has the highest property tax rates in the U.S. when measured against income, also increases the likelihood a change in tax rates inducing a change in investment behavior. Third, residential investment was robust in New Hampshire over the time period examined. This also increases the likelihood of being able to estimate an effect.

This paper focuses on residential investment, the *flow* of capital. Previous studies which have examined the connection between property taxes and the location of residential capital have

[3] They also document that school spending influences residential investment decisions.

[4] For instance, many individuals are employed outside of their municipality of residence. Commuting entails transportation and time costs. With large jurisdictions, these costs may be significant and tend to inhibit residential capital mobility associated with differentials in property tax rates.

examined capital intensity, the *stock* of capital. Existing residential capital may flee a high tax jurisdiction only slowly, through depreciation. The stock of existing capital will therefore evolve slowly in response to a fiscal shock. New capital, however, may reallocate immediately. Focusing on investment allows for a sharper test of the response of capital to tax differences.

The results of this paper demonstrate that residential investment is highly sensitive to property tax rates. A community receiving the mean grant, equal to 15% of local property tax revenue, experiences an increase in residential investment of 16%, implying an elasticity of approximately one. A set of rigorous robustness checks and falsification tests provide strong support for the contention that the increase in building activity represents the causal impact of the fiscal shock.

There is significant heterogeneity in the building response associated with distance from the nearest major urban area, Boston. There is no evidence of an increase in residential investment in the New Hampshire communities within fifty miles of Boston. The response is most intense in the communities just outside this suburban ring. The response to the grants then gradually reduces with distance from Boston. While there is no evidence for a capitalization effect in the state as a whole, there is a large effect inside the suburban ring.

The housing market cleared the demand shock induced by the reform through a quantity adjustment in most of the state and a price adjustment in the suburban ring near Boston. The differing responses are interpreted as reflecting differing housing supply elasticities in the suburban ring and the rest of the state, which is a mix of moderately sized urban areas, exurban areas and rural areas.

The evidence provided by this paper on the manner in which housing markets clear property tax differentials is relevant to the long running theoretical debate over the incidence of the property tax. There are two theories of the incidence of the property tax, the benefit view and the new view. Under the benefit view the property tax is a user charge for local public services. It is as an extension of the Tiebout (1956) model of local public goods provision. The Tiebout model suggests that the mobility of consumers, the ability to "vote with their feet", creates competition between local governments and produces efficient provision of local public goods. The benefit view extends the Tiebout model by adding zoning regulations and capitalization of average tax price differences into housing values. Capitalization and zoning prevent individuals from engaging in free riding on public expenditures and ensure that the property tax is a payment for the use of local public goods.[5]

Under the new view the property tax is a distortionary tax on capital. The view has two

[5] Hamilton (1975) and Fischel (1975) originated the benefit view. Fischel (2001b) provides a recent and detailed discussion of the benefit view.

elements. First, the average level of property taxation in an economy, referred to as the profits tax, will be born by all capital owners. Second, differentials in the property tax across jurisdictions will exert an excise tax on capital in relatively high tax jurisdictions. The mobility of capital, however, means that capital bears none of the incidence of this element of the property tax. It is born by the non-mobile factors in high property tax jurisdictions (if all factors are perfectly mobile, property tax differentials will be born solely by land in the form of negative capitalization)[6].

Distinguishing between the relative validity of the two views is important. They have very different implications for the incidence of the property tax and for assessing the efficiency of local public goods provision (Nechyba 2000; Oates 2000; Oates 1994). Under the benefit view, the property tax is a user charge; it has no distributional incidence and is non-distortionary. The new view suggests that the incidence of the property tax is progressive because the profits tax is born by all capital and capital is disproportionately held by high income individuals. The property tax is distortionary under the new view. The profits tax reduces the level of capital in the economy below the efficient level. The excise tax distorts the allocation of capital across jurisdictions.[7] Existing empirical work largely fails to distinguish between the two views (Nechyba 2000). Given the magnitude of the property tax, the lack of empirical evidence on its incidence represents a significant gap in our understanding of tax incidence in the U.S.

A key, and empirically testable, difference between the views is that the new view predicts high property taxes will cause capital to exit a jurisdiction. The empirical work performed in this paper directly tests this key distinction. Specifically, the work tests the excise tax element of the new view which predicts property tax differentials will cause housing capital to flee high tax rate municipalities. This is precisely the type of evidence which a leading proponent of the new view has suggested would be useful in assessing the views validity.[8]

The evidence provided by this paper does not definitively prove either the benefit or new views of the property tax to be correct. It can be interpreted, however, as validating the excise tax component of the new view in certain environments. It also lends support to the hypothesis that the views are of differing relevance in different settings, specifically that the benefit view is most relevant in suburban settings (Ladd 1998a).

The paper proceeds as follows. Section II provides background on the property tax in New

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[6]The new view was proposed by Mieszkowski (1972). Zodrow (2000) contains a recent and thorough discussion of the different variants of the new view.

[7]For general background on the two theoretical views, Zodrow and Mieskowski (1989) survey the literature. Oates (2000) and Nechyba (2000) contain recent comparisons of the views.

[8]See Zodrow (2000) pg. 105. Specifically, Zodrow suggests testing for capital intensity effects in the suburbs. Some of New Hampshire, particularly the southern portion, is suburban in nature.

Hampshire and the 1999 education reform. Section III presents a simple theoretical model of the how the housing market clears fiscal shocks. Section IV discusses the data. Section V presents the empirical model and discusses identification issues. Section VI presents the results of estimation and leaves interpretation of the results for Section VII. Section VII includes discussion of the results relevance to school finance reform, the incidence of the property tax, and the literature on capitalization. Section VIII concludes.

II Background Information

This section is divided into three subsections. The first discusses the property tax in New Hampshire. The second discusses the 1999 school finance reform. The third discusses the municipal fiscal response to the reform.

II.1 The Property Tax in New Hampshire

In New Hampshire, own source local government revenue is derived almost entirely from the property tax. The tax is applied to the value of all land and structures, both residential and business, in a municipality. All property is taxed at a uniform rate within a municipality and the tax is administered and collected locally.

Tax rates are set annually. Each municipality passes a budget for local public goods. Property tax rates are calculated by dividing the passed budget by the total value of land and structures in the municipality.

II.2 The 1999 School Finance Reform

Prior to 1999, New Hampshire education was funded primarily from local sources. Eighty-seven percent of total primary and secondary education revenue came from the local level — the highest in the nation. The state with the next highest percent, Connecticut, attributed 57 percent of total revenues to local sources and the median state, Wisconsin, attributed 41 percent[9]. Nine percent of education revenue was provided by the state and the remainder was provided by Federal funding.

The reliance on local, property tax based financing created significant dispersion in per-pupil funding and property tax burdens across municipalities. In the *Claremont II* ruling the New Hampshire Supreme Court declared the local property tax used to fund K–12 education unconstitutional. The ruling found the existing school finance scheme provided inadequate educational

[9] U.S. Bureau of the Census (2000)

opportunity in property-poor towns and imposed inequitable tax burdens.

In response to the *Claremont* ruling, the state legislature enacted a major reform in November of 1999. Under the reform, each municipality is assigned a 'cost of adequate education'. The adequate education amount is a function of the number of students in residence in the municipality, with adjustments for factors such as poverty. Each town is also assigned an ability to pay measure, based on the property wealth of the municipality.

If the adequate education cost exceeds the ability to pay, the state issues the municipality a lump-sum grant equal to the difference in the two measures. These grants are referred to as reform grants. Alternatively, if the ability to pay exceeds the adequate education amount, the municipality must remit the difference to the state. These payments are referred to as excess tax payments. The primary determinant of the magnitude of a municipality's reform grant or excess tax payment is per-pupil property wealth.

Twenty percent of New Hampshire communities, referred to as 'donor towns', sent excess payments to the state in the first year of the reform. The remaining eighty percent of municipalities received a positive education grant. Several state programs which provided revenue to municipalities and school districts were cancelled as part of the reform and some municipalities lost a small portion of their property tax bases.

The excess tax payments of the donor towns funded only a small portion of the total cost of the reform. The remaining revenue was raised by increasing several state-wide taxes and the use of lottery revenue. None of taxes increased were property based, nor is there any obvious reason why the incidence of these taxes would differ by municipality or geographic region.

The reform was large in magnitude. The net new funding provided, \$276 million, is equal to 19% of total pre-reform education revenue in the state. In addition, another \$130 million, primarily representing funds from former programs cancelled as part of the reform, was subject to redistribution. Figure 1 displays the large shift from local to state financing produced by the reform.[10]

II.3 The Municipal Fiscal Response to the Reform

Economic theory predicts that a lump-sum grant to a locality, like the reform grants discussed above, will be spent on public goods at the communities marginal propensity to spend on public goods out of private income (Bradford and Oates 1971 a,b). As the marginal propensity to spend

[10] The treatment of the 1999 reform is a simplification which highlights the important elements. See Lutz (2006), and the references it contains, for more detailed information.

on public goods is estimated to be between 5 to 10 cents on the dollar, theory predicts that only 5 to 10 cents per dollar of grant income will be used for increased public goods provision, including education. The rest will be spent on private consumption. In New Hampshire, where virtually all own source revenue is derived from the property tax, this would occur via a reduction in the rate of property taxation.

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 documents that grants tend to stick where they are targeted (Hines and Thaler 1995). The empirical tendency creates the expectation that only a limited portion of the New Hampshire grants will be spent on property tax reduction.

Lutz (2006), however, demonstrates that the reform grants were subject to little to no flypaper effect. Estimates of the portion of the grants used to fund property tax reduction range from eighty to 100 hundred cents per grant dollar. Long-run estimates, based on the preferred set of specifications, are tightly clustered from ninety to ninety-five cents.

The lack of a flypaper effect may be attributable to New Hampshire's use of a form of direct democracy for determining the annual provision level of local public goods. The system, which involves citizens voting directly on budget items in a town meeting format, likely expresses the decisive voters preferred level of spending. In contrast, most studies which document a flypaper effect do so in environments in which it is less clear whose preference are determining budgeting decisions.

The reform is almost certainly permanent because it is based on a ruling by the state Supreme Court. The reform can be revoked only by an amendment to the state Constitution. Such an amendment was attempted and failed by a substantial margin. The long run nature of the reform is important because capital investment decisions are made on the basis of expected long run tax burdens. If the reform was short run in nature, capital investment would be less likely to respond. Similarly, the extent of capitalization would be reduced if the reform was viewed as impermanent (Ross and Yinger 1999; Yinger, Borsch-Supan, Bloom and Ladd 1988).

III A Simple Model of Housing and Local Taxes

This section presents a simple model of local taxes and the housing market. The model is narrowly focused. It provides a framework for interpreting the investment and price response to the 1999 reform. Specifically, it explores how the housing market clears a fiscal shock when supply is perfectly inelastic and when it is elastic. It also assess the effect of a fiscal shock combining both a tax reduction and an increase in public goods spending, as may have been the case in the New Hampshire episode examined by this paper. Readers interested in a more general treatment of the interaction of local public finance and the housing market should refer to the review of the literature in Ross and Yinger (1999).

Individuals purchasing new homes must choose between residing in community A or any one of a large number of identical alternative communities. These communities will be referred to as community B. The number of housing units is fixed in A. Community A is small relative to the surrounding communities so the rental rate for housing in community A, r_a, does not affect the rate in Community B, $\bar{r}_b$, which is considered fixed.

The communities provide a single public good, education, funded by a local property tax and grants from the state. The community budget constraint is therefore

$$e_m = g_m + T_m \tag{1}$$

where e_m is per-household education provision in municipality m, g_m is the per-household grant and T_m is the per-household tax. The communities have pre-existing residents who have, exogenous to this model, determined the level of local public goods and the tax burden (see Lutz 2006 for a model of this process relevant to the New England communities examined in this paper).

Individual i chooses his community of residence so as to maximize utility subject to a budget constraint. Utility is quasi-linear.

$$U_i(x, e_m, m) = \Phi(e_m) + \Psi_i(m) + x \tag{2}$$

$$y = x + r_m\bar{h} + T_m \tag{3}$$

where x is a numeraire consumption good with a price normalized to 1. $\Psi_i(m)$ is the individual specific premium to residing in community B arising from non-fiscal amenities. i indexes individuals with respect to their non-fiscal preference for residing in community B

$$\Psi_i(m) = \{^{0 \text{ if } m=a}_{\pi * i \text{ if } m=b} \tag{4}$$

where π quantifies how compact or disperse the benefits to residing in community B are among the population of homebuyers (the π parameter is discussed more fully below). h is a unit of housing. All homebuyers purchase a single unit of identical housing, $\bar{h} = 1$ (implying an equal property tax burden for all individuals within a given community). Housing does not directly enter the utility function – purchasing a house is equivalent to purchasing the local public goods provided by a municipality.

Equilibrium in the housing market requires that the marginal homebuyer, i^*, is indifferent between community A and B

$$\Phi(e_a) + y - r_a\bar{h} - T_a = \Phi(e_b) + \pi * i^* + y - \bar{r}_b\bar{h} - T_b \tag{5}$$

where $*$ denotes the marginal homebuyer. Define δ_{i^*} as the difference in rents between the communities which satisfies equation (5)

$$\delta_{i^*} = \bar{r}_b - r_a = \Phi(e_b) - \Phi(e_a) + T_a - T_b + \pi * i^* \tag{6}$$

$$\delta_{i^*} = S_b - S_a + \pi * i^* \tag{7}$$

where S_m is the fiscal surplus of community m – the difference between the benefit received from the local public good and the price paid for the local public good

$$S_m = \Phi(e_m) - T_m \tag{8}$$

The rent differential, δ_{i^*}, is equal to the the difference in the fiscal surplus in the two communities plus the value to the marginal homebuyer of the non-fiscal amenities in community B.

House values, p_m, are equal to the discounted stream of rental payments, r_m. r_a is equal to the rental payment in B minus δ_{i^*}, the rent deferential required to make the marginal homebuyer indifferent between A and B

$$p_b = \sum_{t=1}^{N} \frac{\bar{r}_b}{(1+k)^t} \approx \frac{\bar{r}_b}{k} \tag{9}$$

$$p_a \approx \frac{r_a}{k} = \frac{\bar{r}_b - \delta_{i^*}}{k} \tag{10}$$

where k is the discount rate.

Panel A of Figure 1 graphically displays equilibrium in the market for new housing. The downward slopping line, which can be viewed as an aggregate demand curve, is the price of housing at which each individual i would be indifferent between residing in community A or B. The market price of housing is set at the point at which the marginal homebuyer i^* is indifferent between the two communities. Note that the fixed supply of new housing in community A determines the identity of the marginal homebuyer. If there are y units of available housing, then individual $i = y$ is the marginal homebuyer. All individuals $i > y$ reside in community B.

III.1 Grant Increase with Fixed Housing Supply

An increase in the per-pupil grant received by community A will change the equilibrium price of housing

$$\frac{\partial p_a}{\partial g_a} = -\frac{\partial \delta_{i^*}}{\partial g_a} * \frac{1}{k} = [\frac{\partial \Phi(e_a)}{\partial e_a} * \frac{\partial e_a}{\partial g_a} - \frac{\partial T_a}{\partial g_a}] * \frac{1}{k} \tag{11}$$

Define the marginal propensity to spend out of grant income of community A's decisive voter as α: $\frac{\partial e_a}{\partial g_a} = \alpha$. Total differentiation of the community budget constraint, equation (1), yields $\frac{\partial T_a}{\partial g_a} = \alpha - 1$. Equation (11) can now be re-written as

$$\frac{\partial p_a}{\partial g_a} = [\frac{\partial \Phi(e_a)}{\partial e_a} * \alpha - (\alpha - 1)] * \frac{1}{k} \tag{12}$$

Equation (12) reveals under what circumstances the grants fully capitalize. Full capitalization implies that the increase in house prices equals the discounted stream of grant payments: $\frac{\partial p_a}{\partial g_a} = \frac{1}{k}$. The grants will fully capitalize if the marginal propensity to spend on education is 0 ($\alpha = 0$) or if the marginal homebuyer values additional education spending at its cost ($\frac{\partial \Phi_i(e_a)}{\partial e_a} = 1$)[11].

If both of these conditions fail to hold, the grants will capitalize at less than their full value. The extent of capitalization will be a function of α and $\frac{\partial \Phi_i(e_a)}{\partial e_a}$. Lutz (2006) suggests that in the New Hampshire sample considered by this paper, α is between 0 and .2. A lower bound capitalization prediction can be derived by setting $\alpha = .2$ and assuming the marginal benefit of additional education spending is 0. Under this scenario, $\frac{\partial p_a}{\partial g_a} = \frac{.8}{k}$. Eighty percent of the grant value capitalizes. Assuming $\alpha = .1$ and a marginal benefit of .5, $\frac{\partial p_a}{\partial g_a} = \frac{.95}{k}$. Ninety percent of the grant value capitalizes. These calculations suggest that the possibility that a portion of the grants were not used to fund tax reduction does not significantly alter the prediction that the full value of the grants will capitalize.

The change in property values can also be derived in terms of fiscal surplus. The change in fiscal surplus with the grant change is

$$\frac{\partial S_a}{\partial g_a} = \frac{\partial \Phi(e_a)}{\partial e_a} * \alpha - (\alpha - 1) \tag{13}$$

[11] The marginal homebuyer will fully value the education spending if he has preferences identical to the decisive voter. Note, however, that this will not occur in this model because the marginal homebuyer has quasi-linear preference. The quasi-linear preferences have the virtue of yielding transparent, tractable predictions. Specifying preferences in this form does have drawbacks, however. One of these is that the decisive voter, in order to have a positive propensity to spend out of grant income on education, must have preferences which differ from the marginal homebuyer. The quasi-linear preferences of the marginal homebuyer imply a marginal propensity of zero.

Substituting (13) into equation (12)

$$\frac{\partial p_a}{\partial g_a} = \frac{\partial S_a}{\partial g_a} * \frac{1}{k}$$

This result is displayed graphically in panel B of Figure 2 as an outward shift in the aggregate demand curve for new housing in community A equal to $\frac{\Delta S_a}{k}$. The fixed supply of housing units means the market clears the shock to the fiscal surplus solely through a price response. The identity of the marginal homebuyer is unchanged.

III.2 Grant Increase with Elastic Housing Supply

The above analysis assumes that the supply of new housing is fixed in each community. It is possible, however, that supply is not fixed and will respond to the price shock induced by the grants. If there is a supply of undeveloped land being used in an alternative activity, such as agriculture, the price increase induced by a fiscal shock may result in the least productive agricultural sites being sold for residential development. Specifically, sites for which the post-shock residential rent is in excess of the agricultural rent will convert to residential use (Cappoza and Helsley 1989), thereby increasing the housing supply.

Assume initially that the supply is perfectly elastic and that the grant increase is spent entirely on tax reduction so that the change in annual fiscal surplus is equal to the size of the grant. Panel C of Figure 2 displays this scenario. The market clears the fiscal surplus shock solely through an increase in the quantity of housing. Note that the increase in supply changes the identity of the marginal homebuyer. If supply is elastic, but not perfectly elastic, the market will clear the fiscal shock through both a price and quantity response. The resulting equilibrium will be on the line between points Z and Y.

The shaded area represents the complete set of possible market equilibriums after the grant increase. The demand curve shifts outward by $\frac{\Delta S_a}{k}$. The extent of the outward shift is therefore determined by the increase in fiscal surplus created by the reform. The determinants of the increase in fiscal surplus are exactly the same as the determinants of the extent of capitalization for the inelastic supply case discussed above. If the marginal propensity to spend on education is 0 or additional spending is fully valued at its cost, the increase in fiscal surplus will equal the size of the grant payment – see equation (13). If these conditions do not hold, the increase in fiscal surplus will be less than the size of the grant. As the amount of fiscal surplus generated decreases, the outward shift of the aggregate demand curve decreases. The location along the demand curve is determined by the supply curve.

If housing supply is elastic, the quantity and price response to a fiscal shock is partly a function of the distribution of the net benefits to living in community B arising from non-fiscal amenities, $\Psi_i(m)$. These benefits range over $[0, \pi * I]$, where I is the total number of individuals i. The π parameter determines how compact or diffuse the benefits are among the population of homebuyers. The smaller π is, the more compact the benefits. The more compact the benefits, the greater the quantity response.

The manner in which the distribution of benefits arising from non-fiscal amenities affects the quantity response is displayed graphically in panel D of Figure 1. Two aggregate demand curves are displayed, as well as an elastic supply curve. Demand curve D2 arises from a smaller π than D1, $\pi_1 > \pi_2$. The more compact distribution of benefits generated by π_2 implies a relatively less steeply sloped demand curve. The fiscal shock generates a larger quantity response when demand is characterized by D2. Intuitively, when communities are close substitutes, a large number of homebuyers will be induced to switch communities in response to a fiscal shock. If communities are not close substitutes, fewer homebuyers will be induced to switch communities and the price and quantity response will be muted. This aspect of the model is important in interpreting the empirical results and is discussed further in section VII.

An important aspect of the above model is the formulation of the grant introduction as a shock to a community's fiscal surplus. While this paper focuses on property tax differentials, the model suggests the empirical results have broader implications. This is important for two reasons. First, the empirical results of this paper can be interpreted as a response to a shift in the relative fiscal surplus of New Hampshire communities. The results are therefore interpretable regardless of the percent of the grants spend on tax reduction. Second, the results of the paper have implications beyond the property tax. The results have implications for any shock or policy which generates differentials in fiscal surplus between communities, whatever the source of the differential. Such differentials can arise for many reasons, including differences in spending levels, taxation levels, the efficiency of public goods provisions and the mix of public goods.

IV Data and Summary Statistics

The data used in this study cover the years 1996 to 2004 and were obtained from multiple sources. Building permit data for new single family homes measures investment in residential capital. The data is collected by the U.S. Census Bureau. The mean sales price of existing homes in a municipality measures property values. It was obtained from the New Hampshire Housing Finance Authority. Property tax data were obtained from the New Hampshire Department of Revenue Administration. The reform grant data were provided by the New Hampshire Departments of

Education and Revenue Administration. See the Data Appendix for additional information.

Table 1 displays municipality means in 1998 (the year prior to the reform), 2000 and 2002. The first row displays the measure of the fiscal shock induced by the reform, $\frac{netgrant_{m,99}}{ptax_{m,98}}$, where $netgrant_{m,99}$ is municipality m's net grant in 1999, the first year of the reform, and $ptax_{m,98}$ is total property tax payments in 1998, the year prior to the reform (both expressed in constant 1999 dollars). The net grant is equal to a municipalities reform grant net of funds lost as part of the reform.

The fiscal shock measure is easily interpreted. It is the percent reduction in each property owner's tax burden assuming all grant funds are used to fund tax reduction. The mean fiscal shock is .15, indicating the mean municipality would have been able to fund a 15% reduction in its tax burden. The 10th percentile municipality experiences a negative shock of -0.06. This community, which has high per-pupil property wealth, receives no grant and is forced to make an excess tax payment to the state. The shock at the 90th percentile is .30. This low per-pupil property wealth community receives aid equal to almost a third of total local tax revenue.

An alternative measure of the fiscal shock is $\frac{netgrant_{m,99}}{taxbase_{m,98}}$, where $taxbase_{m,98}$ is the property tax base in the year prior to the reform. The alternative fiscal shock measure can be interpreted as the reduction in the property tax rate induced by the reform and has a mean value is 4.76.

Conditional on receiving a positive net grant, both the municipal tax rate and total tax burden declined between 1998 and 2000. Conditional on receiving a negative net grant, both the tax rate and total tax burden increased. Tax burdens increased for both types of towns after 2000, primarily reflecting increased education spending. Neighboring New England states also experienced this increase. Despite the increased tax *burden*, tax *rates* fell substantially over this same period as the result of rapidly increasing property values.

Panel A of Figure 3 displays the mean values of the outcome variables, residential investment and house prices. Residential investment is measured by $\frac{permits_{m,t}}{hstock_m}$, where $permits_{m,t}$ is the number of single family home building permits at time t and $hstock_m$ is the stock of existing single family homes as measured in the first year of the sample, 1996.[12] House prices are measured as the mean sales value of existing homes in a municipality. The housing market was characterized by increasing investment and prices over the period considered by this study.

[12] See the Data Appendix for additional information. All of the empirical work presented employees single family home building permits as the metric for residential investment. The results of this paper are, however, robust to using total housing unit building permits as the measure of residential investment.

V Empirical Model and Identification

The most general empirical specification would estimate the effect of fiscal surplus, S_m, on residential investment

$$\frac{permits_{m,t}}{hstock_m} = \alpha + \beta S_m + \phi_t + \eta_m + \varepsilon_{mt} \qquad (14)$$

Equation (14) suffers from clear endogeneity bias – fiscal surplus is almost certainly correlated with determinants of investment. An instrumental variables strategy would be required. The reform grants are potential instruments. Municipal fiscal surplus, however, is inherently unobservable. As a result the following empirical model, which can be viewed as a reduced form version of equation (14), is used as the primary specification

$$\frac{permits_{m,t}}{hstock_m} = \alpha + \beta \frac{netgrant_{m,99}}{ptax_{m,98}} * postreform_t + \phi_t + \eta_m + \varepsilon_{mt} \qquad (15)$$

where $postreform_t$ is an indicator variable equaling one in years greater than or equal to 1999, the first year of the reform.

The reform grants are issued because an outside body, the state Supreme Court, forces the state government to issue them. They can be viewed as an exogenous shock to a municipality's budget constraint. The fact that the grants are an exogenous shock is insufficient, however, to ensure that the fiscal shock measure is uncorrelated with ε_{mt}.

The reform grants were recalculated annually after the second year of the reform. The recalculated reform grants will reflect endogenous adjustment to the reform. Of particular concern, they will reflect residential investment endogenous to the reform. The grant is therefore held fixed at its initial level in order to avoid endogenous response bias. The changes in the grants from year to year were small and the initial grant level can be considered a proxy for the grants received over the 2000 to 2004 period.[13]

The fiscal shock measure is therefore a function of time-invariant variables, specifically the arguments of the grant formula in the year prior to the reform and the municipal tax burden in the year prior to the reform. These time-invariant municipal characteristics may be correlated with determinants of capital investment decisions. Such a correlation would produce bias in β.

[13] An alternative approach would be to estimate $\frac{permits_{m,t}}{hstock_m} = \alpha + \beta_1 \frac{netgrant_{mt}}{ptax_{mt}} + \phi_t + \eta_m + \varepsilon_{mt}$ and instrument the time-varying fiscal shock measure with $\frac{netgrant_{m,99}}{ptax_{m,98}}$ interacted with a vector of year dummies for the post reform years 2000 - 2004 (the interactions are needed because the upward trend in the tax base, $ptax_{mt}$, causes the gradient of the instrument with respect to the time-varying shock measure to shift over time). The results produced with this specification are extremely similar to the results produced using equation (15). Equation (15) is preferred over the instrumenting procedure because of its greater transparency. This is particularly valuable in the estimates which utilize the fiscal shock measure interacted with other variables (see Tables 5 and 6).

This possibility is dealt with by the inclusion of the municipal fixed-effect terms, which control for any time-invariant determinants of investment. β is identified solely from variation in residential investment within municipalities.

The fixed effects do not, however, control for time-varying determinants of investment which may correlated with the fiscal shock-post reform interaction variable. For instance, the fiscal shock measure is largely a function of per-pupil property wealth. If property wealthy districts are experiencing an upward trend in the rate of investment relative to less wealthy districts, the estimate of β will be downwardly biased.

A two part approach is taken in response to the possibility that time-varying determinants of investment bias the estimates. First, three additional models are estimated as robustness checks. These models attempt to control for time-variant determinants of investment. Second, a falsification test is executed. This test is discussed below.

The first of these robustness checks is

$$\frac{permits_{m,t}}{hstock_m} = \alpha + \beta \frac{netgrant_{m,99}}{ptax_{m,98}} * postreform_t + \varphi_t * X_m + \phi_t + \eta_m + \varepsilon_{mt} \tag{16}$$

where X_m is a vector of municipal characteristics measured in the first year of the sample, 1996, and φ_t is a vector of time-varying coefficients. While the coefficients are time-varying, the X_m vector is time-invariant[14]. The characteristics in the X_m vector are percent of municipal property that is for seasonal or recreational use, percent of taxable property that is commercial, percent of taxable property that is owned by a utility and quadratics in distance from Boston, population and the aggregate market value of taxable property[15]. The model controls for changes over time in investment that are associated with these fixed characteristics. For example, the distance from Boston controls for rapid growth in southern New Hampshire.

The second robustness check is

$$\frac{permits_{m,t}}{hstock_m} = \alpha + \beta \frac{netgrant_{m,99}}{ptax_{m,98}} * postreform_t + \phi_t + \eta_m + \eta_m * t + \varepsilon_{mt} \tag{17}$$

While the year fixed-effect terms control for the statewide trend in investment, they do not control for municipal specific trends. Equation (17) controls for such trends through inclusion of the $\eta_m * t$

[14] None of the models include time-varying variables, such as demographic characteristics, as controls. Changes in such variables may be endogenous to the reform. Their inclusion would potentially bias the estimates.

[15] The percent of homes used for seasonal or recreational use, the percent of the tax base that is commercial and the percent of the tax base that is owned by a utility are only available as measured in 2000. These variables are considered to be time-invariant, but are not measured at the start of the sample as would be preferable.

term. The third robustness check utilizes data from neighboring New England states to directly control for the town characteristics which determine the magnitude of the grants. This model is discussed in detail below.

The falsification test is enacted as follows. A "placebo" fiscal shock is generated by assigning each New Hampshire municipality in 1998 and 1997 the shock it actually received in 1999

$$\frac{permits_{m,t}}{hstock_m} = \alpha + \beta \frac{netgrant_{m,99}}{ptax_{m,98}} * postreform_t + \beta_{placbeo} \frac{netgrant_{m,99}}{ptax_{m,98}} * prereform_t + \phi_t + \eta_m + \varepsilon_{mt} \tag{18}$$

where $prereform_t$ equals one in years 1997 and 1998. 1996 is the omitted year category for the vector of fiscal shock-time period interaction terms. There should be no response to the fiscal shock in the years prior to the reform. A large and precisely $\beta_{placbeo}$ would suggest that the estimates of β are spurious and do not represent the causal impact of the fiscal shock on building activity.

The models are estimated with data ranging from 1996 to 2004, with 1999 omitted from the sample. 1999 is omitted for two reasons. First, the reform was announced in November of 1999. Most of 1999 was prior to the reform and it is unlikely there was a significant response in investment in only two months (November and December). Second, when the reform was announced in late 1999, municipal budgeting decisions for the year had already been made. Many municipalities were constrained from reacting to the grants by the late announcement. It may have been unclear how a given municipality would respond to the grants in the long-run. In 2000 municipalities were unconstrained. Investment decisions are based on the long-run expected tax burden of a community, not the burden arising in a single year due to short term constraints. The estimates presented in the paper are not substantively changed if 1999 is included. Municipalities are excluded from the sample if they fail to have non-missing data for at least seven of the eight years. Finally, very small municipalities, those with less than 800 residents in year 2000, are dropped from the sample.

VI Results

This section focuses on reporting the results of estimating the empirical model. Detailed discussion and interpretation of the results is left for section VI. The section is divided into four subsections. The first section presents the results from the primary specification, equation (15). The second presents a falsification test and the robustness check which utilizes data from New England municipalities located outside of New Hampshire. The third presents results exploring geographic heterogeneity in the response to the fiscal shock. The final subsection presents results exploring the impact of the fiscal shock on property values.

VI.1 The Quantity Response: The Response of Residential Investment to the Fiscal Shock

Table 2, column (1), presents the β estimate from equation (15). The estimate is precise and large. Evaluated at the mean value of the fiscal shock measure, the point estimate implies a .26 increase in the rate of residential investment (the dependent variable has been multiplied by 100, so this is an increase of .0026 in the ratio of permits to housing stock). Using the mean rate of investment in 1998, 1.6, this implies the fiscal shock induced a 16 percent increase in the rate of residential investment. The mean value of the fiscal shock measure is .15 and this can be interpreted as a fifteen percent decrease in the property tax burden. The estimate can therefore be interpreted as implying that the elasticity of residential construction with respect to the property tax burden is approximately (negative) one.

The results can also be interpreted relative to the municipal capital stock, measured as the number of single family housing units in the first year of the sample, 1996. The estimates imply that ten years after the initiation of the reform, the capital stock will have increased by 2.5% in a town with the mean fiscal shock.

Column (2) adds the base year characteristic interaction terms, equation (16), and column (3) includes linear trend terms, equation (17). Inclusion of the base-year characteristics significantly increases the estimated effect. The point estimate implies a 27 percent increase in the rate of investment. Inclusion of the linear trend terms significantly reduces the magnitude and precision of the estimate. While the linear trend specification is an important robustness check, it is possible that the trend terms are capturing part of the effect of the fiscal shock on investment and thereby producing a downward bias. Estimates, presented below on Tables 4 and 5, which incorporate data from other New England states and allow heterogeneity in the estimates by geographic location, respectively, support the interpretation that the trend terms are inducing downward bias in column (3).

After the announcement of the reform, it may have taken time for investment to fully respond. Residential construction takes time to implement. Column (4) explores this possibility by allowing the coefficient on the fiscal shock measure to vary by year. The response to the reform was immediate and constant over time. While the point estimate for the final year of the sample, 2004, is somewhat larger than for the earlier years, the estimates are fairly stable across the years and cannot be statistically distinguished from each other. Column (5) employs the alternative fiscal shock measure, $\frac{netgrant_{m,99}}{taxbase_{m,98}}$, and produces results quite similar to the specifications using the standard measure.

VI.2 Falsification Test and New England States Robustness Check

The estimates on Table 2 suggest a strong investment response to the fiscal shock. The fixed-effect specification controls for any time-invariant determinants of investment that might be correlated with the fiscal shock. The model does not control for determinants of investment which are time-varying. If time-varying determinants of investment are correlated with the fiscal shock, β may be biased.

A particularly relevant concern arises from the fact that the reform grants are largely a function of per-pupil property wealth. Any correlation between per-pupil property wealth and investment decisions that is not constant over time may produce bias in the estimates. For example, communities with amenities that make them attractive for the construction of vacation homes may have seen a disproportionate increase in building as a result of the boom in the housing market over this period. Such communities typically have high levels of per-pupil property wealth. This set of circumstances would lead to biased estimates. The possibility of such bias motivates performing a falsification test and a robustness check which directly controls for determinants of grant magnitude, including per-pupil property wealth.

The falsification test, equation (18), is displayed in the final column of Table 2. A positively estimated placebo fiscal shock coefficient would suggest that the results in columns (1) - (5) of Table 2 are spurious. The results of the test are encouraging. The point estimate is extremely small, equal to $\frac{1}{2}$ a percent of the true fiscal shock coefficient, and is very imprecise. Furthermore, the true fiscal shock coefficient is invariant, to two decimal places, to the inclusion of the placebo shock. The falsification test provides no evidence against a causal interpretation of estimates in columns (1) - (5).

The robustness check assess whether or not the results are robust to controlling for the time-varying influence of town characteristics associated with the degree of fiscal shock. The grants are a function of several different town characteristics. The most significant determinant is per-pupil property wealth. Other determinants include the number of special education students, the poverty rate and transportation costs. They are directly controlled for in a manner which allows them to have a time-varying impact on investment with the following specification

$$\frac{permits_{m,t}}{hstock_m} = \alpha + \beta \frac{netgrant_{m,99}}{ptax_{m,98}} * postreform_t + \beta_d determinants_m * postreform_t + \phi_t + \eta_m + \varepsilon_{mt} \tag{19}$$

where $determinants_m$ is the vector of arguments appearing in the grant formula. The above specification is not viable, however, because the grants are primarily a linear function of $determinants_m$[16]. Inclusion of the $determinants_m$ vector yields an imprecisely estimated β.

The specification is viable, however, with the inclusion of data from other states. The specification uses the pattern of investment in communities outside of New Hampshire to control for the impact the grant determinants have on investment independent of the actual grants. Surrounding New England states provide the control municipalities and the fiscal shock variable is set equal to zero for these communities. The approach is similar in spirit to a triple difference-in-difference estimator. Under this formulation, the identifying variation comes from the interaction of three variables: the fiscal shock measure, a post reform indicator and a New Hampshire indicator.

The specification rests on the assumption that the other New England communities are a valid counterfactual for New Hampshire communities. The grant arguments, $determinants_m$, are assumed to influence investment in the same manner throughout New England. While the assumption that the other communities are a valid counterfactual is inherently untestable, the comparability can be assessed along observable dimensions. Table 3 displays demographic characteristics of New Hampshire communities and four neighboring New England state communities. Taken as a whole, the demographic characteristics suggest that Maine is the state most similar to New Hampshire. The two states have municipalities of similar size, measured both in terms of population and number of single family homes, have a similar racial composition, percent of houses that are used for recreational or seasonal use and a relatively similar level of per-child residential property wealth.

Panel B of Figure 4 plots the trends in residential construction for all five states. All have a similar level of investment in the three years prior to the reform, 1996 - 1998. New Hampshire and Maine have very similar upward trends over the entire sample period, while the remaining states experienced flat levels of investment.

Equation (19) will suffer from omitted variable bias if any of the control states enacted a policy change which is a function of $determinants_m$. As is clearly visible in Appendix Figure A1, Vermont enacted a school finance reform in 1998[17]. Vermont is not used as a control state because of this

[16] There are two points of non-linearity in the grant formula associated with the poverty rate. These discontinuities account for a very small portion of the overall variation in grant magnitude. Attempts to exploit the discontinuity to identify the effect of the grants on investment and property values yield extremely imprecise estimates.

[17] The 1998 Vermont school finance reform potentially has implications for the results displayed on Table 2. The reform, by altering the fiscal surplus available in Vermont municipalities, may have affected residential investment in surrounding states, including New Hampshire. To the extent that the reform increased or decreased the relative fiscal surplus of all New Hampshire municipalities, the year effects in equation (15) will control for the Vermont reform. There are circumstances under which the year effects will fail to control for the Vermont reform. For instance, property wealthy Vermont municipalities may be close substitutes for property wealthy New Hampshire municipalities. If the Vermont reform made property wealthy municipalities in Vermont less desirable by decreasing their fiscal surplus, investment may have increased in property wealthy New Hampshire municipalities. Similarly,

reform. The figure reveals that the remaining New England states had stable school finances over the period.

A significant problem with estimating equation (19) is that the $determinants_m$ vector is unobserved for the control states. A vector of demographic variables, taken from the 2000 Census, is used as a proxy for $determinants_m$. The vector includes a measure of per-child property wealth and five additional demographic measures (see Table 4 for a complete list). The census variables are strong predictors of the fiscal shock – a cross-sectional regression run on the New Hampshire communities produces an R-squared of .75.

Column (1) of Panel A, Table 4, estimates equation (19) with the sample restricted to New Hampshire municipalities. Including the vector of census covariates, which are strong predictors of the magnitude of a town's fiscal shock, yields an imprecisely estimated fiscal shock coefficient.

Column (2) estimates the identical specification, but includes data from Maine. The estimated effect of the shock is approximately 25 percent larger than that from the primary specification (displayed in column (1) of Table 2) and implies the mean municipality experienced approximately a 20 percent increase in building activity.

Column (4) adds municipal linear trends to the specification. The result is robust to this inclusion, although the point estimate is somewhat reduced and there is a loss of precision (the estimate is significant at the 10% level). The robustness to linear trend terms stands in contrast to the linear trend estimates on Table 2, which did not control for the determinants of grant magnitude.

The robustness check is invalid, or contaminated, if the New Hampshire reform induced changes in investment activity in the control state(s) associated with the $determinants_m$ vector. The impact of the reform outside of New Hampshire is theoretically ambiguous because of the increase in statewide, non-property, non-geographic specific taxes used to fund the grants. These tax increases make New Hampshire a less desirable place to locate residential investment. As a result, even in towns which receive a positive grant, it is unclear wether or not they should attract investment that, in the absence of the reform, would locate in another state. (The unambiguous theoretical prediction is that residential investment *within* New Hampshire reallocates geographically).

the reform may have made property poor Vermont towns more attractive and decreased investment in property poor New Hampshire municipalities. This scenario, and most other reasonable scenarios, implies a *downward* bias in the estimates on Table 2. Furthermore, the available evidence suggests the Vermont reform does not bias the estimates. The placebo falsification test in column (6) of Table 2 provides no evidence that the Vermont reform biases the estimates. Similar (unreported) estimates which limit the sample to 1996 to 1998 and have a placebo grant for only 1998, the year of the Vermont reform, provide no evidence of bias. Finally, the New England state robustness check, equation (19), also provides no evidence. Controlling for the determinants of the grant magnitude should capture any effect of the Vermont reform on investment in New England correlated with the grant magnitude.

An example of a situation that would invalidate the robustness check is the following. Investment in communities with amenities that make them attractive for vacation homes reallocates from Maine to New Hampshire in response to the reform. Furthermore, both Maine and New Hampshire communities with these amenities have high per-pupil property wealth. Under this scenario, the $determinants_m$ vector will be correlated with building activity in Maine as a result of the 1999 New Hampshire reform and the robustness check is invalid.

Panel B uses Connecticut and Rhode Island as the control states. While these states are less similar to New Hampshire along observable dimensions than Maine, they have the advantage of being more geographically distant. Connecticut and Rhode Island communities are less likely than Maine communities to be close substitutes for New Hampshire communities and are therefore less likely to suffer from contamination. The results using Connecticut and Rhode Island are very similar to those produced when Maine is the control state, although the specification including municipal linear trends is less precise. These results suggest, but do not confirm, that the robustness check does not suffer from contamination.

Panel C displays the estimates using all the New England states displayed on Table 3 as the control group. The results are quite similar to those produced using the other control groups. The municipal linear trend specification is significant at the 10% level. Viewed jointly, the results on Table 4 suggest the results on Table 2 represent the causal response of investment to the fiscal shock.

VI.3 Heterogeneity In Investment Response by Location

The investment response to the fiscal shock depends on the elasticity of housing supply. If there is heterogeneity in the elasticity of supply, then there will be heterogeneity in the response to the grants. Heterogeneity in the elasticity of supply could occur for any number of reasons. The return to the non-housing activity (i.e. agriculture or commercial) may differ by location, the extent of zoning may differ or the amount of land available for development may vary.

The southern portion of New Hampshire has experienced rapid development in recent years and the communities near the Massachusetts border have become part of suburban Boston. The suburban communities are denser, and hence have less developable land, than the rest of the state which is a mix of exurban, rural and mid to small sized cities. These communities may also have more stringent zoning requirements.

These facts motivate the following specification

$$\begin{aligned}\frac{permits_{m,t}}{hstock_m} &= \alpha + \beta\frac{netgrant_{m,99}}{ptax_{m,98}} * postreform_t + \\ & \beta_{idb}\frac{netgrant_{m,99}}{ptax_{m,98}} * postreform_t * distboston_m + \\ & \beta_{mdb} * distboston_m * postreform_t + \phi_t + \eta_m + \varepsilon_{mt}\end{aligned} \quad (20)$$

where $distboston_m$ is the distance from municipality m to Boston[18]. The coefficient estimates are presented in column (1) of Table 5[19]. Both the main grant coefficient and the interaction term coefficient are precisely estimated. The interaction term is negative, indicating that the farther from Boston a community is, the smaller the building response to the reform.

To aid in interpreting the results, the rows at the bottom of the table present the implied change in the rate of investment, evaluated at the mean fiscal shock, for communities at the 25th, 50th and 75th percentiles of distance from Boston. The estimated change in the rate of investment is .51, .35. and .18, respectively. These calculations reveal a significant decline in the response with distance from the nearest central city.

The results from equation (20) are counter-intuitive. The reasoning above suggests the response should increase with distance from Boston. It is possible, however, that the impact of location may operate in a non-linear fashion. This possibility is explored in column (2) by adding the interaction term $\frac{netgrant_{m,99}}{ptax_{m,98}} * postreform_t * subboston_m$, where $subboston_m$ is an indicator for the municipality being within 50 miles of Boston. The $subboston_m * postreform_t$ interaction term is also included in the specification, although not displayed on the table. Fifty miles is approximately the 10th percentile of distance from Boston.

The fiscal shock variable and the two fiscal shock interaction terms are all significant at the 1% level, indicating that all three belong in the model. The coefficients on the fiscal shock and fiscal shock-50 mile interaction term are of similar magnitudes and of opposite signs, suggesting there is no investment response in the communities close to Boston. This impression is confirmed by the hypothesis tests presented at the bottom of the table. The tests fail to reject the hypothesis that the effect of the reform equals zero for the communities nearest Boston (33 miles) and those farthest away, but still within the fifty mile band (50 miles). The distance variable coefficient again

[18] There is no relationship between the magnitude of the fiscal shock and distance from Boston. Cross-sectional regressions (unreported) which regresses the fiscal shock on either distance from Boston or a quadratic in distance from Boston produce imprecisely estimated coefficients. Similarly, there is no relationship between the fiscal shock and the indicator for being within 50 miles of Boston used below.

[19] Due to space constraints, the coefficients from the 'main effect' $distbost_m * postreform_t$ is not shown on Table 6 (full results available from the author upon request).

suggests a decline in the impact of the reform with distance from Boston.

The estimates indicate the response to the reform was subject to significant heterogeneity by municipal location. Those communities nearest Boston experienced no response. Those communities just outside the fifty mile suburban ring, which could be thought of as exurban communities, experienced the largest response. A community located sixty miles from Boston (equal to the 25th percentile of distance from Boston), which received the mean fiscal shock experienced a .76 increase in the rate of investment. This represents an increase of approximately thirty-five percent from the mean rate of investment in the year prior to the reform.[20] The impact of the reform then dissipates linearly with distance from Boston as you move away from the fifty-mile ring. A community located 108 miles from Boston (equal to the 75th percentile of distance from Boston) experiences an increase in investment of .22 – an increase of twenty percent.

Column (3) demonstrates that these results are robust to the simultaneous inclusion of the set of base-year interaction terms and the municipal linear trend terms. The specification is intensely saturated. The stability of the results suggest a significant degree of robustness. The result is important given the lack of robustness to linear trend terms of the primary estimating equation displayed on Table 2.

Column (4) demonstrates that it is only the communities closest to Boston which experience no investment response to the reform. The specification replaces the fifty mile indicator with an indicator for being in the Boston MSA.[21] The Boston MSA is a larger area, covering a greater portion of the state, than the area within fifty miles of Boston. The implied impact of the fiscal shock is approximately .7 for the community at the median distance from Boston within the MSA and the effect for this community can be distinguished from zero.

A potential explanation for the lack of a response in the fifty mile suburban ring is high housing density in this area. Density may be associated with an inelastic housing supply because it implies there is a limited supply of land to bid away from alternative activities. Column (5) demonstrates, however, that the lack of a response in the suburban ring is not solely due to the areas density. The specification is similar to that in column two, with the exception that a third interaction term is added, $\frac{netgrant_{m,99}}{ptax_{m,98}} * postreform_t * hdensity_m$, where $hdensity_m$ is the number of housing units

[20] Residential investment in the year prior to the reform is strongly correlated with distance from Boston. The implied percent change in residential investment is calculated using predicted residential investment from the following cross-sectional regression run over New Hampshire municipalities in 1998

$$\frac{permits_m}{hstock_m} = \rho_1 subboston_m + \rho_2 distboston_m + \rho_3 distboston_m^2 + \varepsilon$$

[21] The Boston MSA is the Boston-Cambridge-Quincy, MA-NH Metropolitan Statistical Area and is county based.

per square meter of land. The coefficient on this interaction term is imprecisely estimated. The fiscal shock and other two interaction terms remain precisely estimated. Replacing housing unit density with single family housing density or population density produce similar results. Section VI discusses other potential explanations for the geographic heterogeneity.

VI.4 The Price Response: The Response of Property Values to the Fiscal Shock

Under the assumption that the supply of housing is not perfectly elastic, the fiscal shock should produce a price response. Ideally, the price response would be measured using the price of land. Unfortunately data on the value of land is unavailable. Instead I use the mean sales value of existing homes in a municipality.

Column (1) of Table 6 presents the results of estimating equation (15) with the log of the sales price of existing homes as the dependent variable. The estimated response to the shock is small and imprecise. There is no evidence of capitalization.

Column (2) presents the results using the same specification presented in column (2) of Table 5 which includes the distance from Boston and suburban ring interaction terms. The response for the communities located within the suburban ring is large and precisely estimated. Unlike the quantity response, which displayed a linear decline in intensity with distance from Boston, there is no effect of distance from Boston on the price response, conditional on controlling for being located in the suburban ring.

Column (3) drops the distance interaction term and produces results similar to those in column (2). The result suggests that the suburban ring community receiving the typical fiscal shock experienced an increase in property values of five percent.

Column (4) includes the vector of base-year interaction terms. The results are unchanged. Column (5) adds municipal linear trend terms. The estimate of capitalization within the suburban ring becomes small and imprecise. It would be useful to have data on property values from surrounding states to produce estimates similar to those on Table 4 which control for determinants of grant magnitude. Unfortunately, comparable data on property values for other New England states is not available.

Column (6) presents a falsification test. The test is analogous to the one presented in column (6) of Table 2. A placebo shock is generated by assigning to each municipality in 1998 and 1997 the shock it actually received in 1999. The coefficient on the placebo shock-suburban ring interaction term is equal to less than 10 percent of the coefficient on the true shock-suburban ring interaction term. It is also very imprecise. The test provides no evidence against a causal interpretation of the estimated price response in columns (2) - (5).

The results on Tables 6 and 7 are complementary. They suggest that the housing market cleared the fiscal shock primarily through a quantity response in most of the state. There is evidence of a substantial investment response, but no capitalization. In the suburban ring, in contrast, the market cleared primarily through a price adjustment. There is no evidence of an investment response, but there is substantial capitalization.

The claim that market cleared the fiscal shock primarily through a price adjustment in the suburban ring implies the grants should have capitalized at close to their full discounted value in this area. The market should have moved from point X to point Z on Panel C of Figure 2. Full capitalization implies an increase in total municipal property of $\frac{\Delta G}{k}$.

The extent of capitalization in the suburban ring can be estimated using the coefficient values on Table 6, the mean values of the fiscal shock in the suburban ring, the mean value of municipal taxable property in the suburban ring and a discount rate. The discount rate is assumed to equal .07, the 30-year conventional mortgage rate in 2000. The coefficients in columns (2), (3) and (4) of Table 6 imply approximately eighty-five, ninety and one-hundred percent capitalization, respectively. These estimate provide further support for the claim that the housing market cleared the fiscal shock primarily through a price adjustment in the suburban ring area.

VII Interpretation

This section is divided into three subsections. The first discusses possible explanations for the heterogeneity in response to the fiscal reform by geographic location. The second discusses the implications of the paper's findings for policies which induce changes in property tax rates. It also discusses the implications for the theoretical debate over the incidence of the property tax. The third section discusses the implications for the literature on the capitalization of local amenities.

VII.1 Geographic Heterogeneity

The housing market appears to have cleared the fiscal shock through a price adjustment in the communities near Boston and through a quantity adjustment in the rest of the state. These contrasting responses are consistent with differing supply elasticities. Under this interpretation, the suburban communities are characterized by an inelastic supply of housing, while the communities outside the ring are characterized by an elastic supply of housing.

The question which naturally arises from this hypothesis is : what is responsible for the differing supply elasticities? There are two leading candidates: supply of developable land and the extent of zoning. Table 8 presents summary statistics by whether or not a community is within the suburban ring. The two types of communities receive identical fiscal shocks. They differ significantly, however, in the extent of residential investment in the period prior to the reform, population and population density. The suburban communities have considerably larger populations, are much more densely populated and had almost twice the rate of residential investment in 1998.

In the course of development, land with the least value in an alternative use is the first to be bid away for use in residential construction. As development continues, land with a higher value in the alternative use must be bid away, increasing the price of housing. At the same time, as land available for residential development becomes more scarce, the option value of the existing land for use in future development increases. The supply of developable land is therefore an important determinant of the elasticity of housing supply (Hilber and Mayer 2004).

Housing density is almost certainly correlated with the supply of land available to be bid away from other activities and the suburban communities have a much higher population density than the rest of the state. It is therefore reasonable to presume that a constrained supply of developable land is at least partially responsible for the inelastic supply in the suburban portion of the state. The hypothesis, however, is not supported by the results on Table 5 which control for housing density. The insignificance of the shock-density interaction term, and the invariance of the other coefficients to its inclusion, suggests that it is not housing density directly that generates an inelastic supply of housing in the suburban ring.

While the results from the specification including density do not rule out scarcity of land as an explanation for the differing supply elasticities – housing unit density is an imperfect measure of the supply of land – it does suggest that other factors are be important. The residual explanation is more intense zoning in the suburban area. Unfortunately no data that I am aware of exists on zoning in New Hampshire communities. There is anecdotal evidence, however, that as communities in the southern portion of the state have become more developed, zoning restrictions have become more restrictive. This evidence often comes in the form of complaints that "outsiders" have changes the political culture of the state and placed new, and onerous, restrictions on how property can be used (e.g. Hoyt 2003). These complaints can be seen as part of the more general tendency for increased use of zoning as areas become more developed (Ladd 1998b).

Under this hypothesis, the stringency of zoning has increased in New Hampshire communities as they have become more developed. As development has spread farther from Boston, so has the use of what I will term 'stringent zoning' – i.e. zoning capable of inhibiting a supply response to a shock in the housing market. The fifty-mile band represents the current boundary for stringent zoning.

There is anecdotal evidence that this boundary is slowly expanding outward. Dover, New

Hampshire is located 66 miles from Boston. It has experienced rapid growth in recent years. The use of zoning has increased significantly during this period of growth. The number of changes to the municipal zoning code increased by over 100% between 1999 and 2003 (City of Dover 2005). Many of these changes, such as minimum lot size requirements and increased environmental controls, will almost certainly reduce the supply of new housing.

Zoning may be used by the citizens of a community to maximize the value of their homes (Fischel 2001a). It is an open question why the communities outside the suburban ring allowed the potential increase in their home values to be undone by the large supply response. Why did these communities not zone out the new development and thereby capture the stream of future benefits provided by the reform?

One possibility is that zoning is a blunt instrument which cannot respond quickly to a shock. Zoning may have been (approximately) maximizing housing values in the pre-reform environment in which most communities funded the vast majority of local public expenditures with own source revenue. Such zoning may have been fiscally neutral and ensured that new development covered any additional public costs it generated (Hamilton 1975, White 1975). This type of zoning would not necessarily prevent a rapid increase in residential development in response to the 1999 fiscal shock.

An alternative hypothesis assumes that all New Hampshire communities have perfect control over zoning and engage in behavior slightly more complex than simply maximizing housing values. Assume, for simplicity, that the decisive voter can choose to engage in stringent zoning or fiscal squeeze zoning. Stringent zoning permits no supply response to the reform and ensures that the full discounted value of the grants capitalize. Fiscal squeeze zoning (White 1975) will generate an increase in the community fiscal surplus each year, f_t, equal to the additional tax revenue generated by new development minus the sum of additional public expenditures required by the development and the decrease in grant magnitude resulting from the new development[22]. f_t is sometimes referred to as fiscal profits (Ladd 1998b). The decision between zoning regimes is captured by the indicator variable z which equals one if stringent zoning is chosen and zero if fiscal squeeze zoning is chosen. The decisive voter's problem is

$$\max_z \frac{p_d}{p} * [z * \sum_{t=1}^{N} \frac{1}{(1+k)^t} * \Delta G_a + (1-z) * \sum_{t=1}^{N^*} * \frac{1}{(1+k)^t} * (\Delta G_a + f_t)]$$

[22] The grants can be viewed as being a function of the community fiscal surplus. If the new residential investment increases the fiscal surplus for the existing residents, it will mechanically decrease the amount of grant funds receieved from the state. The offset, however, is signifncantly below 100%.

where N^* is the expected time period at which the decisive voter will exit the community and ΔG_a is the direct change in fiscal surplus produced by the reform (i.e. it does not account for changes in fiscal surplus produced by development induced by the reform).

If stringent zoning is chosen, the decisive voter's life-time wealth is increased by the his share of the discounted stream of benefits accruing from the fiscal shock, ΔG_a. His share is equal to the percent of taxable property he owns, $\frac{p_d}{p}$, where p_d is the value of property owned by the decisive voter and p is the value of all taxable property in the municipality. While residing in the house, the decisive voter consumes the proceeds of the fiscal shock in the form of a lower tax burden. Upon selling the house, the remaining proceeds are obtained through an increased sales price – i.e. capitalization.

If fiscal squeeze zoning is chosen, then the decisive voter consumes the reduced tax burden arising from the reform itself and the additional tax reduction obtained from the new development. The zoning decision will depend upon the magnitude of the fiscal profits produced by new development, f_t, expected length of residence in the community, N^*, and the discount rate, k. Larger fiscal profits, longer expected residence and a higher discount rate will all increase the likelihood that fiscal squeeze zoning is the optimal choice.

The difference between the suburban ring and the remainder of the state is potentially explained by differences in these three variables. No information is available on differences in the discount rate between the two areas. Data from the 2000 Census indicates no difference in the probability that an individual resides in the same house as five years ago. This finding fails to support the contention that there is a systematic difference in N^* across the areas.

The only support provided by the data for optimal zoning explaining the different supply elasticities concerns the likely value of the fiscal profits, f_t, accruing to New Hampshire municipalities. Education expenditures are the largest component of local government spending in New Hampshire. f_t will primarily be a function of the number of school age children brought into a community by the new residential investment. There is no evidence that the reform increased the number of students in the towns which received positive grants (Lutz 2006). The implication is that the homes built in response to the reform were sold primarily to families without children or are being used as vacation homes. The vacation home theory is particularly relevant given the high proportion, equal to approximately 15 percent, of New Hampshire homes which are for recreational use. Additionally, new homes sold for an average of 20 percent more than existing homes in the post reform period in the area outside the suburban ring. These two pieces of information suggest, but certainly do not confirm, the possibility that fiscal profits produced by the reform were large.

The results of this paper suggest geographical heterogeneity within the non-suburban ring portion of the state. Specifically, the investment effect of the reform dissipates with distance from Boston. While it is possible that this represents a supply side response, it is unclear what the mechanism would be. Housing density decreases with distance from Boston. There is no obvious reason to expect more stringent zoning as you move away from the region's major urban area into less developed areas.

It is therefore likely that the decreasing investment response with distance from Boston is a demand-side phenomenon. As discussed in section II, the distribution of benefits from non-fiscal amenities will influence the response to a fiscal shock. The more compact the distribution of benefits, the less steeply sloped is the aggregate demand curve for new housing and the larger the supply and price response to a given shock (see Figure 1, Panel D).

Intuitively, the distribution of benefits can be seen as a measure of how substitutable communities are along the non-fiscal dimension. A compact distribution arises from communities being relatively substitutable. When communities are close substitutes, a given fiscal shock will induce more homebuyers to switch communities than if the communities are less substitutable. The larger number of individuals wishing to switch communities generates a larger quantity (and price) response.

It is possible that the distribution of non-fiscal benefits varies systematically with distance from Boston. Individuals interested in purchasing a home in the exurban area just outside the fifty-mile ring may consider the communities to be close substitutes. Individuals interested in purchasing homes in the relatively more rural portion of the state may consider the communities to be imperfect substitutes. For instance, homebuyers in the rural portion of the state may have strong personal attachments to given communities for reasons such as family ties. They are less easily induced to switch communities in response to a fiscal shock.[23]

VII.2 Policy Implications, School Finance Reform and the Incidence of the Property Tax

The findings of this paper have important implications for reforms and policy initiatives which induce changes in property tax burdens and, more generally, in fiscal surplus differentials across communities. The most significant example in recent years of this type of reform has been the

[23] An alternative possible explanation for the declining intensity with distance from Boston is that there is less building activity in the more remote portions of the state. With little activity, there would be little margin for a response to the fiscal shock. This may be a partial explanation. However, while investment does decline with distance from Boston, there is significant building activity throughout the state. The mean investment for the entire state in 1998 is 1.6. The mean rate of investment for the four quartiles of distance from Boston, in order, are 2.7, 1.5, 1.2 and 1. Even in the most distant municipalities there is significant investment and a margin for response to the fiscal shock.

wave of school finance reforms. Such reforms typically reduce the importance of local financing and increase the importance of state financing. To the extent that such actions produce changes in the relative fiscal surpluses of communities, they will generate distortion of capital investment decisions. Such distortions should be included in the cost-benefit analysis of school finance reform. While capitalization of reforms has been treated by the literature on school finance reform (e.g, Hoxby 2001, Hoxby and Kuziemko 2004), to the best of my knowledge, the impact of the reforms on the location of residential investment has not been empirically tested. This paper conducts such a test and confirms the distortionary potential of these reforms for residential investment.

The finding that capital flees high tax jurisdictions is a validation of the new view of the property tax. Specifically, the results document that the hypothesized reallocation of capital in response to the excise tax component of the property tax is operative. The result is an important finding given the lack of empirical evidence in support of the new view (Nechyba 2000). The new view appears to be an accurate characterization of the incidence of the property tax in most of New Hampshire.

The findings should not, however, be interpreted as invalidating the benefit view. It is unlikely that either view is strictly correct. Elements of both views may be simultaneously valid (Ladd 1998a, Oates 2000). A portion of the property tax may act as a user charge and a portion may act as a distortionary tax on capital (Kotlikoff and Summers 1987; Wildsain 1986) and the views may be of differing relevance in different settings.

It is likely that the benefit view is most relevant in a suburban setting (Ladd 1998a), a hypothesis confirmed by the results of this paper. The response to the reform in the suburban area near Boston confirms closely to that predicted by the benefit view. The shock capitalized into property values and there was no evidence of capital reallocation. Future research should further explore the conditions which determine which view is relatively more valid.

VIII Capitalization

As discussed in the introduction, a large body of past research examines the capitalization of local amenities into property values. These estimates are often used to infer the value the marginal homebuyer places on the amenity. Such an interpretation is problematic if the housing supply is not fixed.

As long as the amenity is reproducible, supply will expand until the capitalization, i.e. the rent being earned by homes which provide access to the amenity, is eliminated (Hamilton 1976b). For instance, if proximity to an urban center is a positive amenity, houses in communities near the center will earn a rent. If developable land exists, supply will expand until the rent is eliminated or the supply of land exhausted.

Failure to consider the supply side of the market may lead researchers to misinterpret capitalization results[24]. For instance, the finding of no capitalization in response to the fiscal shocks induced by the New Hampshire reform (see Table 7, column (1) and Lutz (2006)) might lead a researcher to conclude that either the tax reductions were not valued by the marginal homebuyer or that the housing market did not clear the fiscal shock. Particular care must be taken in interpreting heterogeneity in the extent of capitalization in different areas. Heterogeneity may reflect variation in the willingness to pay for an amenity or it may reflect differing conditions on the supply side of the housing market.

IX Conclusion

This paper demonstrates that residential capital investment decisions are responsive to property tax rates and, more generally, to differences in the fiscal surplus available in different communities. Using a school finance reform in the state of New Hampshire, it is demonstrated that a fiscal shock equal to fifteen percent of a communities total tax revenue produces an approximate sixteen percent increase in the rate of residential investment. A set of robustness checks suggests the estimate reflect the causal response of investment to the fiscal shock and may even be a downwardly biased, lower bound, estimate of the response.

There is significant heterogeneity in the response by geographic location. There is no investment response in the fifty mile suburban ring near Boston. The response is most intense just outside this ring and the intensity gradually fades with distance from Boston. While there is no capitalization effect in the state as a whole, there was significant capitalization within the suburban ring. The housing market cleared the fiscal shock through a price adjustment in the areas near Boston and a quantity adjustment in the rest of the state. The differing response by geographic location is likely a function of differing supply elasticities.

The results of the paper suggest that tax differences can generate significant distortion of residential capital investment decisions. Polices, such as school finance reforms, which induce property tax changes may entail significant distortionary costs. The response of capital to the reform can be seen as validation of the excise tax component of the new view of the property tax. Finally, the results suggest that when capitalization estimates are used to value local amenities, the supply side of the housing market needs to be considered.

[24] Hilber and Mayer (2004) make a similar point.

X Data Appendix

The Stock of Single Family Homes in 1996

The stock of single family homes in 1996 (the first year of the sample), $hstock_m$, is constructed as follows. The stock of single family homes in 1990 is obtained from the 1990 Census. The 1990 stock is then increased by the number of building permits issues between 1990 and 1995. This 1996 stock number is then adjusted as follows. The 1990 stock is grown out by the number of building permits issued between 1990 and 1999 to construct a 2000 stock measure. The difference between the 2000 constructed stock measure and the 2000 stock measured obtained from the 2000 Census is taken as the estimated error in the growth procedure. The 1996 stock measure is then adjusted using the estimated error under the assumption that the error is apportioned equally to each year between 1990 and 2000.

Omitted Observations

The observation from the municipality of Seabrook is omitted from the estimation sample. Seabrook contains a nuclear power plant. The plant was successively devalued over the course of the 1990s. As a result, Seabrook lost close to $800 million in property value, a situation which generates uncertainty concerning the data quality of the variables pertaining to property wealth and property taxes. This is a unique situation unrelated to the school finance reform. Two municipalities participating in inter-state school districts (both municipalities are in cooperatives with municipalities in Vermont) are omitted from the sample. These municipalities are dropped due to longitudinal inconsistency in the data.

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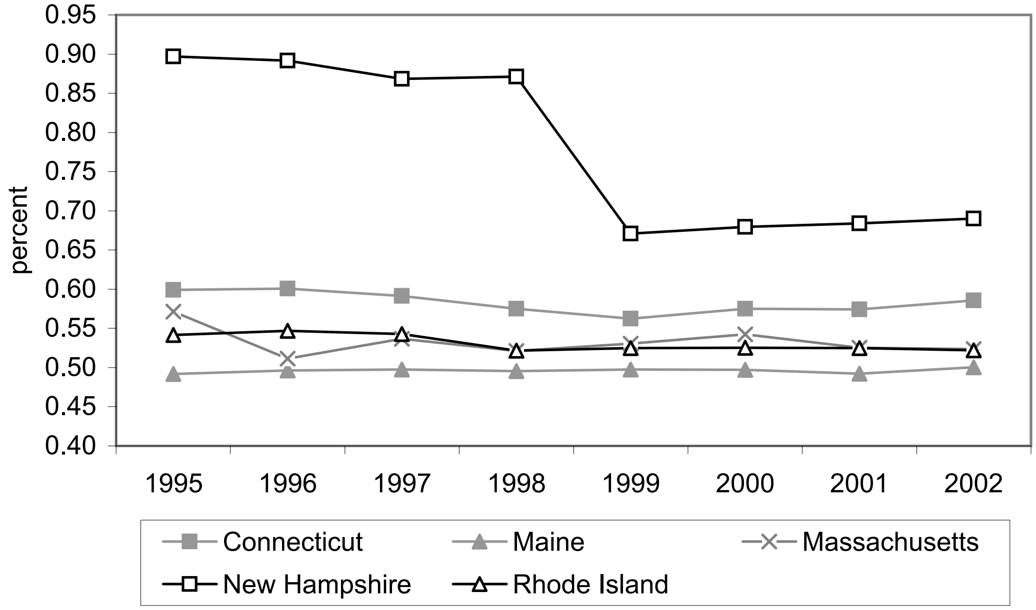
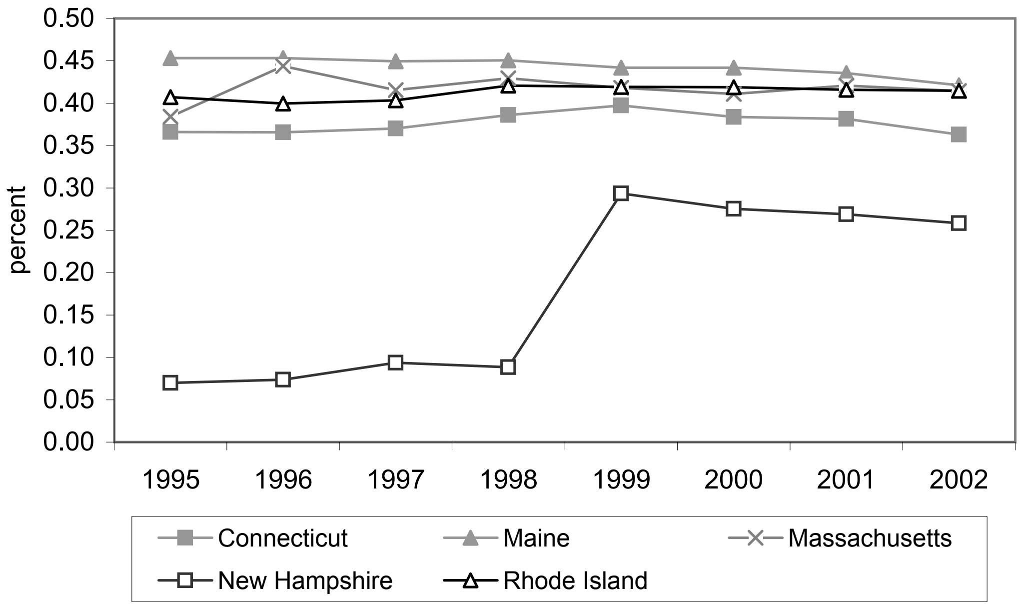
Figure 1: Education Funding by Level of Government

Panel A: Local Share of Education Funding



Panel B: State Share of Education Funding



Note. Data from Census Bureau School Finance data, New Hampshire Department of Education and New Hampshire Department of Revenue Administration.

Figure 2: Community A Housing Market

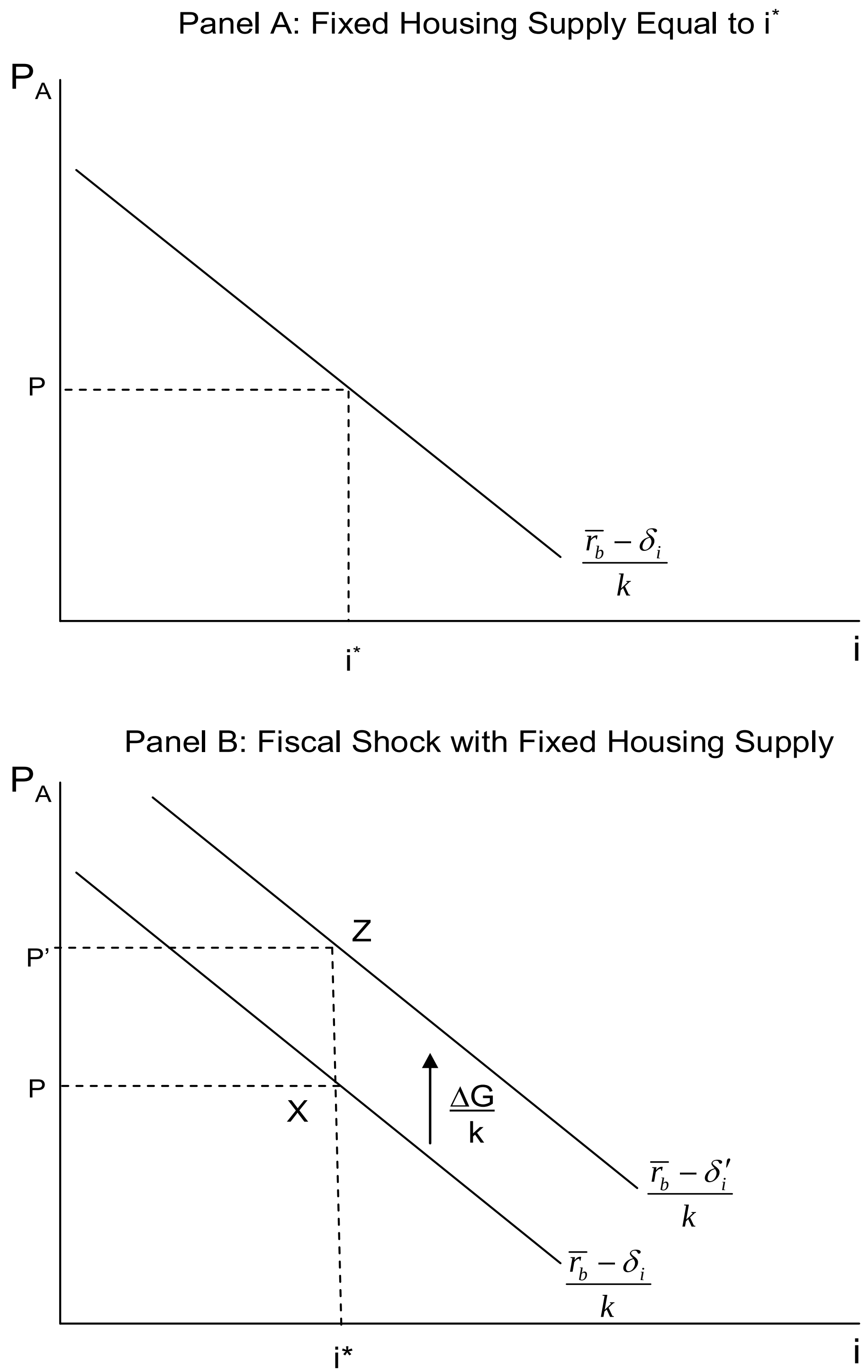
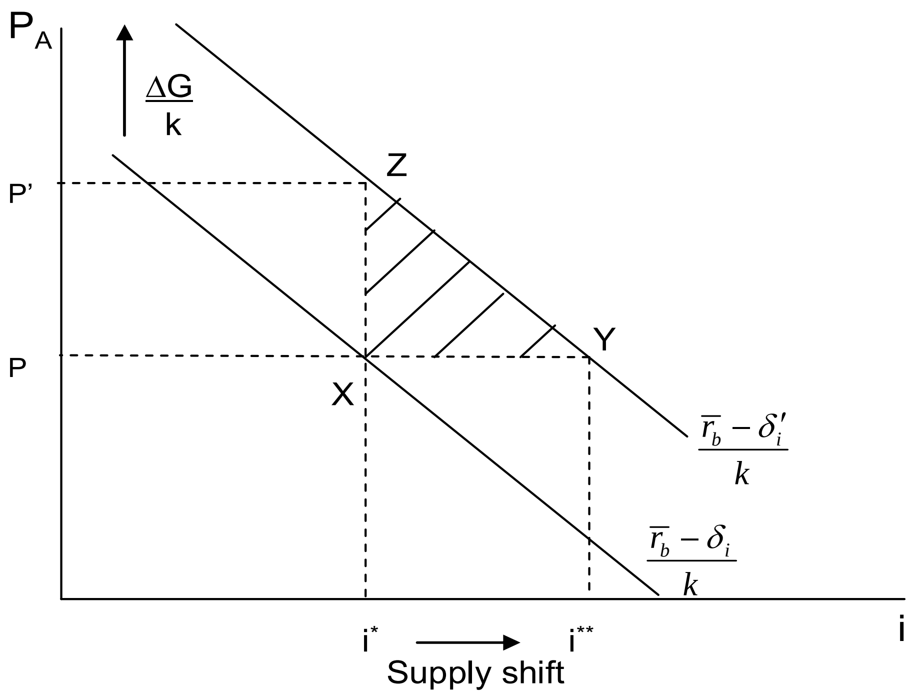


Figure 2 (cont.): Community A Housing Market

Panel C: Elastic Housing Supply



Panel D: Aggregate Demand Curves with Different Slopes

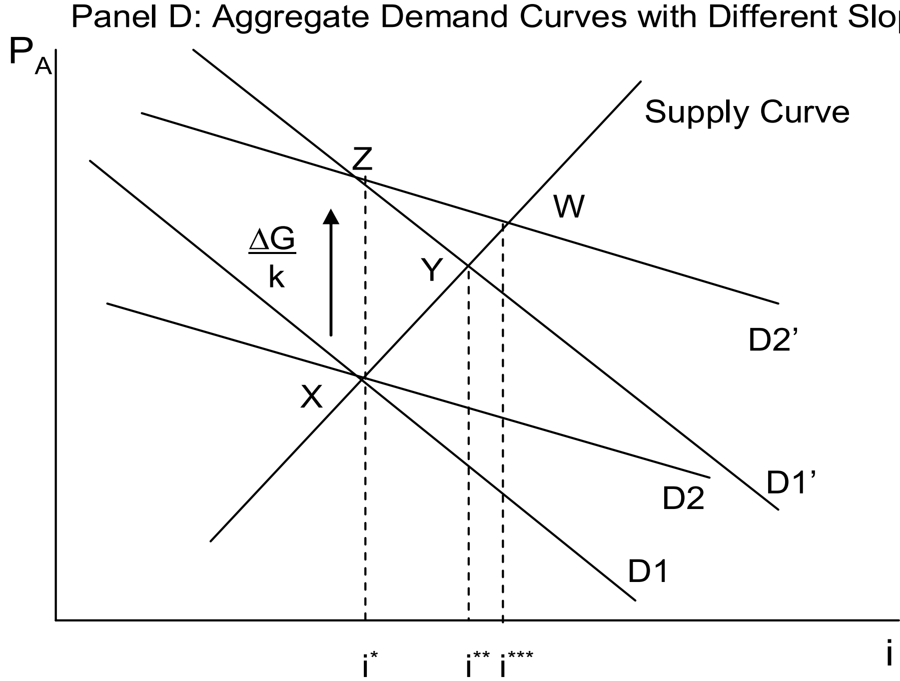
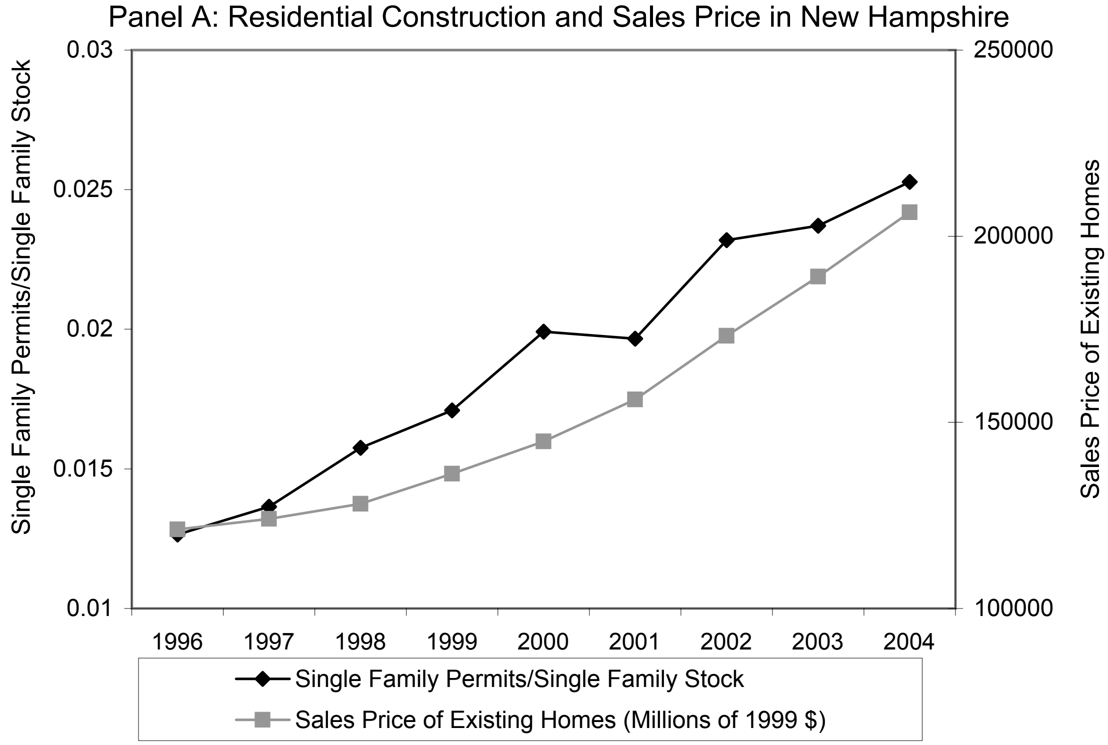
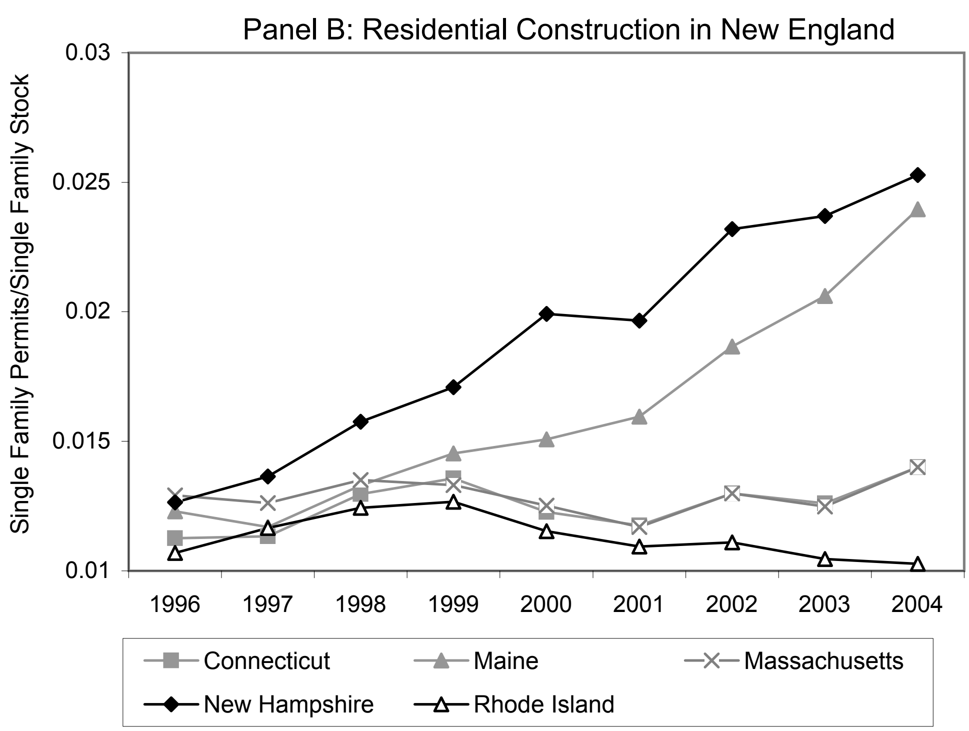


Figure 3: Residential Construction





Note. The figures display municipality means for the sample of municipalities with at least 800 residents in 2000 and which form a balanced panel over the period displayed. Building permit data from U.S. Census Bureau. Mean sales data are obtained from the New Hampshire Housing Finance Authority.

Table 1

Summary Statistics

	1998	2000	2002
1999 Net Education Grant / 1998 Local Property Tax Revenue	*	0.15	0.15
	*	(0.16)	(0.16)
10th Percentile	*	-0.06	-0.06
50th Percentile	*	0.18	0.18
90th Percentile	*	0.30	0.30
(1999 Net Education Grant / 1998 Taxable Property) * 1000	*	4.76	4.76
	*	(4.00)	(4.00)
Tax Rate Per $1000 of Property	28.2	22.3	20.4
(conditional on positive Net Refrom Grant)	(5.5)	(4.5)	(4.7)
Tax Rate Per $1000 of Property	14.8	15.1	13.3
(conditional on negative Net Reform Grant)	(4.3)	(4.3)	(3.6)
Total Tax Payment (millions of 1999 dollars)	9.53	8.65	9.47
(conditional on positive Net Refrom Grant)	(15.98)	(14.42)	(15.34)
Total Tax Payment (millions of 1999 dollars)	6.75	7.69	8.57
(conditional on negative Net Refrom Grant)	(8.27)	(8.67)	(9.83)
Population	6241	6409	6613
	(11314)	(11550)	(11727)
Distance from Boston		88	
		(34)	
Number of Observations		180	

Note. The cells are municipality means unless stated otherwise. Standard deviations are in parentheses. The sample used to calculate the means is restricted to th set of districts with greater than 800 residents in 2000 and that form a balanced panel for the three years displayed. All variables are calculated with dollar values converted to 1999 dollars.

Table 2
Effect of Change in Fiscal Surplus on Residential Investment

	(1)	(2)	(3)	(4)	(5)	(6)
	Single Family Building Permits per Single Family Homes * 100					
(1999 Grant / 1998 Tax Revenue) * year >= 2000	1.76	2.97	1.06			1.76
	(0.85)	(1.13)	(1.40)			(0.62)
(1999 Grant / 1998 Tax Revenue) * year = 2000				1.47		
				(0.92)		
(1999 Grant / 1998 Tax Revenue) * year = 2001				1.88		
				(0.93)		
(1999 Grant / 1998 Tax Revenue) * year = 2002				1.31		
				(0.95)		
(1999 Grant / 1998 Tax Revenue) * year = 2003				1.55		
				(0.75)		
(1999 Grant / 1998 Tax Revenue) * year = 2004				2.57		
				(1.04)		
(1999 Grant / 1998 Taxable Property) * year >= 2000					63.45	
					(25.81)	
(1999 Grant / 1998 Tax Revenue) * year = (1997 or 1998)						0.01
						(0.60)
Implied Change in Dep. Var*	.26	.44	.16		.30	
Implied Percent Change in Dep. Var*	.16	.27	.10		.19	
Number of Observations	1453	1453	1453	1453	1453	1453
Base Covariates * Year Indicators		X				
Municipal Linear Trends			X			

Note. The unit of observation is municipality-year. The dependent variable is the ratio of single family building permits to the number of single family homes in the 2000 Census. Standa errors clustered by municipality are in parentheses. The date range of the data is 1996 to 2004, with 1999 omitted from the sample (see text). The sample is restricted to the set of municipalities with greater than 800 residents in 2000 with at least six years of non-missing building permit data. All columns include municipal fixed-effects. Columns (2) - (3) include a time-invariant control variables interacted with a full set of year indicator variables. The variables are distance from Boston, distance from Boston squared, municipal population, municipa population squared, the aggregate market value of land and buildings in the municipality in 1996, the aggregate market value of land and buildings in the municipality in 1996 squared, the percent of municipal property that is commercial, the percent of property that is owned by a utility company and the percent of residential property that is for seasonal or recreation use. Se text for a discussion of the placebo grant variable used in column (6). Tables displaying the complete set of coefficients available from the author upon request. * The implied change in the dependent variable and the implied percent change in the dependent variable are calculated using the mean sample value of the dependent variable and independent variable of interest.

Table 3

Demographic Characteristics of New England States

	New Hampshire	Connecticut	Maine	Massachusetts	Rhode Island
Single Family Homes	1882	5205	1407	4439	6510
	(2333)	(4615)	(1409)	(4554)	(5467)
Population	6474	19981	4265	19098	26880
	(11522)	(24314)	(6108)	(38144)	(31208)
Per Child Residential Property Wealth	179197	257543	146881	262079	215704
	(102129)	(150424)	(90156)	(157132)	(145535)
Median Household Income	51165	63603	37857	59048	50313
	(12620)	(20249)	(8368)	(18716)	(11539)
Median House Value	130803	193302	95449	203585	157733
	(52045)	(110206)	(32803)	(27280)	(63347)
Percent Non-White	0.02	0.08	0.02	0.06	0.07
	(0.01)	(0.11)	(0.02)	(0.07)	(0.10)
Unemployment Rate	0.04	0.04	0.05	0.04	0.06
	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)
Percent of Houses for Recreation Use	0.14	0.04	0.16	0.06	0.06
	(0.16)	(0.06)	(0.15)	(0.13)	(0.13)
Number of Observations	183	171	259	332	39

Note. The cells display municipality means taken from the 2000 Census. Standard deviations in parentheses. The sample is restricted to the set of municipalities witl greater than 800 residents in 2000 with at least six years of non-missing building permit data. *See the text for a discussion of the predicted Fiscal Shock.

Table 4

Effect of Change in Fiscal Surplus on Residential Investment: Extensions using Data from other New England States

	(1)	(2)	(3)
	Single Family Building Permits per Single Family Homes * 100		
	A. Control Group: Maine		
(Grant / Tax Revenue) * year >= 2000	0.90	2.24	1.61
	(0.95)	(0.52)	(0.87)
Implied Change in Dep. Var*	.13	.33	.24
Implied Percent Change in Dep. Var*	.08	.21	.15
Number of Observations	1453	4195	4195
	B. Control Group: Connecticut and Rhode Island		
(Grant / Tax Revenue) * year >= 2000	*	2.33	1.30
	*	(0.57)	(0.95)
Implied Change in Dep. Var*	*	.34	.19
Implied Percent Change in Dep. Var*	*	.22	.12
Number of Observations	*	3142	3142
	C. Control Group: New England States		
(Grant / Tax Revenue) * year >= 2000	*	2.60	1.77
	*	(0.51)	(0.85)
Implied Change in Dep. Var*	*	.38	.26
Implied Percent Change in Dep. Var*	*	.24	.16
Number of Observations	*	8675	8675
Grant Predictors * year >= 2000	X	X	X
Data from Control Group included		X	X
Municipal Linear Trends			X

Note. Grant / Tax Revenue refers to the ratio of the net grant in 1999 to property tax revenue in 1998. The unit of observation is municipality-year. The dependent variable is the ratio of single family building permits to the number of single family homes in the 2000 Census. Standard errors clustered by municipality are in parentheses. The date range of the data is 1996 to 2004, with 1999 omitted from the sample (see text). The sample is restricted to the set of municipalities with greater than 800 residents in 2000 with at least six years of non-missing building permit data. All columns include municipal fixed-effects. The grant predictors, obtained from the 2000 Census, are : per child residential housing wealth, average household income, median household income, the unemployment rate, percent non-white and percent of adults with a college degree.

* The implied change in the dependent variable and the implied percent change in the dependent variable are calculated using the mean sample value of the dependent variable and independent variable of interest.

Table 5
Effect of Change in Fiscal Surplus on Residential Investment: Heterogeneity by Location

	(1)	(2)	(3)	(4)	(5)
	Single Family Building Permits per Single Family Homes * 100				
(Grant / Tax Revenue) * (year >= 2000)	7.09	9.71	12.34	3.27	6.88
	(2.31)	(1.99)	(4.79)	(2.57)	(2.56)
(Grant / Tax Revenue)*(distance from Boston)*(year >= 2000)	-0.05	-0.08	-0.12	-0.02	-0.05
	(0.02)	(0.02)	(0.04)	(0.02)	(0.02)
(Grant / Tax Revenue)*(<= 50 miles from Boston)*(year >= 2000)		-9.87	-13.52		-8.96
		(3.11)	(6.28)		(2.92)
(Grant / Tax Revenue)*(Boston MSA)*(year >= 2000)				2.57	
				(1.92)	
(Grant / Tax Revenue)*(housing unit density)*(year >= 2000)					10880.69
					(7849.07)
Implied Change in Dep. Var*					
25th Percentile of Distance from Boston: 60 miles	0.57	0.76	0.79		
50th Percentile of Distance from Boston: 83 miles	0.39	0.50	0.40		
75th Percentile of Distance from Boston: 108 miles	0.19	0.22	-0.03		
Median Distance for munic. <= 50 miles from Boston: 44 miles		-0.51	-0.92		
Median Distance for municaplities in Boston MSA: 55 miles				0.68	
P-value for test: Effect on Building 33 Miles from Boston = 0		.36	.39		
P-value for test: Effect on Building 50 Miles from Boston = 0		.17	.23		
P-value for test: Effect on Building 55 Miles from Boston = 0				.0008	
Number of Observations	1453	1453	1453	1453	1453
Base Covariates * Year Indicators			X		
Municipal Linear Trends			X		

Note. Grant / Tax Revenue refers to the ratio of the net grant in 1999 to property tax revenue in 1998. The unit of observation is municipality-year. The dependent variable is the ratio o single family building permits to the number of single family homes in the 2000 Census. Standard errors clustered by municipality are in parentheses. The date range of the data is 1996 to 2004, with 1999 omitted from the sample (see text). The sample is restricted to the set of municipalities with greater than 800 residents in 2000 with at least six years of non-missing building permit data. All columns include municipal fixed-effects. Column (3) include a set of time-invariant control variables interacted with a full set of year indicator variables. The variables are distance from Boston, distance from Boston squared, municipal population, municipal population squared, the aggregate market value of land and buildings in the municipality in 1996, the aggregate market value of land and buildings in the municipality in 1996 squared, the percent of municipal property that is commercial, the percent of property that is owned by a utility company and the percent of residential property that is for seasonal or recreation use. Tables displaying the complete set of coefficients available from the author upon request. All columns include the main interaction effect. For example, column (1) includes in the specification (distance from Boston) * (year >= 2000). The coefficient estimates for these main effects are not shown due to space limitations. * The implied changes in the dependent variable are calculated using the mean sample values of the relevant independent variables.

Table 6
Effect of Change in Fiscal Surplus on Property Values

	(1)	(2)	(3)	(4)	(5)	(6)
	Log(Mean Sales Value of Exisiting Homes)					
(Grant / Tax Revenue) * (year >= 2000)	0.06	-0.02	0.04	-0.01	-0.31	0.05
	(0.07)	(0.20)	(0.07)	(0.07)	(0.17)	(0.12)
(Grant / Tax Revenue)*(distance from Boston)*(year >= 2000)		0.00				
		(0.00)				
(Grant / Tax Revenue)*(<= 50 miles from Boston)*(year >= 2000)		0.36	0.32	0.43	0.04	0.34
		(0.17)	(0.15)	(0.17)	(0.25)	(0.17)
(Placebo Grant) * (year = 1998 or 1997)						0.02
						(0.12)
(Placebo Grant)*(<= 50 miles from Boston)*(year = 1998 or 1997)						0.03
						(0.14)
Implied Percent Change in Property Values: <= 50 Miles fr. Boston	*	*	0.05	0.06	-0.04	*
Implied Percent Change in Property Values: > 50 Miles fr. Boston	*	*	0.01	0.00	-0.05	*
Number of Observations	1461	1461	1461	1461	1461	1461
Base Covariates * Year Indicators				X	X	
Municipal Linear Trends					X	
Restricted to year <= 1998						X

Note. Grant / Tax Revenue refers to the ratio of the net grant in 1999 to property tax revenue in 1998. The unit of observation is municipality-year. The dependent variable is the log of th mean sales value of existing residential homes. Standard errors clustered by municipality are in parentheses. The date range of the data is 1996 to 2004, with 1999 omitted from the sample (se text). The sample is restricted to the set of municipalities with greater than 800 residents in 2000 with at least six years of non-missing building permit data. All columns include municipal fixed-effects. Column2 (4)-(5) include a set of time-invariant control variables interacted with a full set of year indicator variables. The variables are distance from Boston, distance from Bosto squared, municipal population, municipal population squared, the aggregate market value of land and buildings in the municipality in 1996, the aggregate market value of land and buildings in the municipality in 1996 squared, the percent of municipal property that is commercial, the percent of property that is owned by a utility company and the percent of residential property that is for seasonal or recreation use. See text for a discussion of the placebo grant variable used in column (6). Tables displaying the complete set of coefficients available from the author upon request. * The implied percent change in the dependent variable is calculated using the mean sample values of the independent variables.

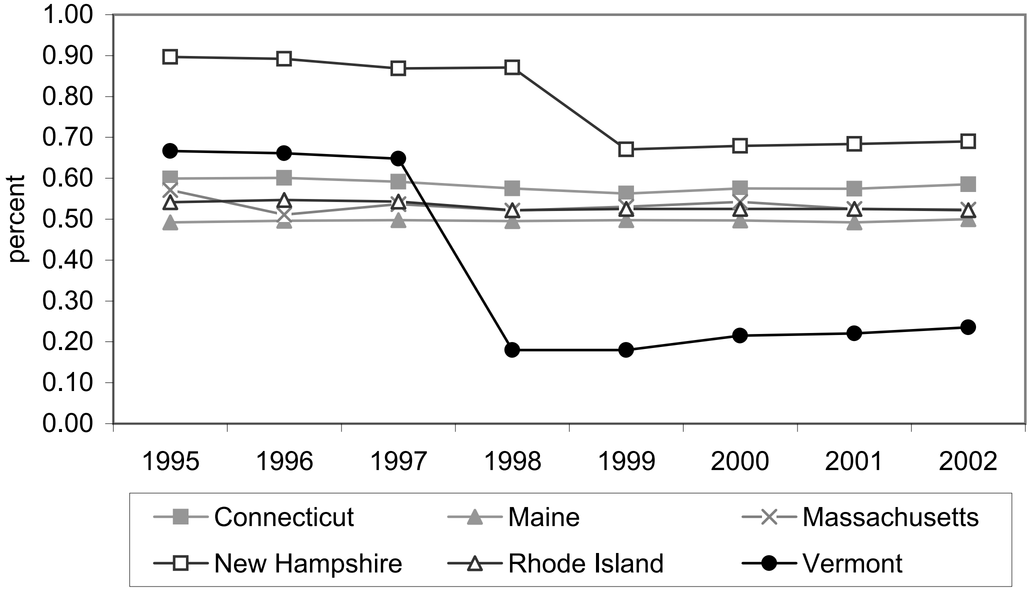
Table 7
Summary Statistics for Within and Outside 50 Mile Suburban Ring

	In Suburban Ring	Outside Suburban Ring
1999 Net Education Grant / 1998 Local Property Tax Revenue	0.147	0.147
	(0.101)	(0.171)
(1998 Building Permits / Housing Stock) * 100	0.024	0.013
	(0.014)	(0.009)
Population	12784	5521
	(18661)	(9951)
Total Housing Unit Density	0.008	0.003
	(0.010)	(0.006)
Population Density	0.215	0.076
	(0.227)	(0.129)
Number of Observations	22	158

Note. The cells are municipality means. Standard deviations are in parentheses. The sample is restricted to municipalities with greater than or equal to 800 residents in 2000. Total housing unit density is defined as the number of total housing units in 1996 per square meter of land multiplied by 1000. Population density is defined as the number of residents (as measured by the 2000 Census) per square meter of land multiplied by 1000.

Figure A1

Panel A: Local Share of Education Funding



Panel B: State Share of Education Funding