Quasi-Experimental Evidence on The Connection Between Property Taxes and Residential Capital Investment

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Abstract

Do low property taxes attract residential capital investment? This question is answered using an unusual school finance reform in the state of New Hampshire. The reform induced large shifts in property tax burdens and this shock is used to identify the empirical relationship between property taxes and new home construction. The estimates suggest that, in most of the state, communities with a reduced tax burden experience a substantial increase in residential construction. In the area of the state near the region's primary urban center (Boston), however, the shock clears through a price adjustment – i.e. by capitalizing into property values. The differing responses are attributed to differing housing supply elasticities. Moreover, communities which experience a decrease in property tax burdens, and witness a surge in building activity as a result, increase the stringency of their land use regulation – a response likely to slow the growth in housing supply.

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1 Introduction

Is the location of residential capital investment influenced by property tax burdens? Individuals choose their bundle of public services through their residential location decision and communities which provide public goods at a low tax cost would be expected to experience elevated housing demand. If housing supply is fixed, differences in fiscal amenities across localities will clear exclusively through price adjustments – i.e. through house price capitalization. A voluminous literature explores this possibility.[1] If housing supply is elastic, however, differences in fiscal amenities will induce both a price and a quantity response. This paper therefore focuses on how the quantity of homes responds to differences in fiscal amenities across communities. Specifically, the relationship between new home construction and property tax burdens – a fiscal disamentity – is examined.

The empirical challenge in assessing the connection between residential investment and property tax burdens is the endogeneity of taxes to determinants of investment. Tax burdens may be high in jurisdictions which provide positive amenities such as good schools. High quality schools will tend to attract capital investment and this complicates estimating the relationship between tax burdens and investment. This paper addresses the econometric identification problem using an unusual school finance reform in the state of New Hampshire. In 1999, the state began issuing large-scale grants to municipalities, the overwhelming majority of which were ultimately used to fund property tax reduction (Lutz 2010). This tax shock is used to identify the empirical relationship between tax burdens and building activity. An important aspect of this empirical design is that the grants are largely a function of per-pupil property wealth, with property-poor localities receiving the largest grants. Numerous robustness checks provide reassurance that the estimates are not a spurious result of correlation between municipal property wealth and housing construction arising from factors other than the grants.

The results suggest that the supply of new homes is quite sensitive to property tax liabilities: A community receiving the mean grant, equal to 15 percent of pre-reform local property tax revenue, experiences an 11 to 22 percent increase in residential investment, implying an elasticity of roughly (negative) one. The analysis also reveals significant heterogeneity in the response. There is no evidence of an increase in investment in the New Hampshire communities within fifty miles of the nearest major city, Boston, but a large response outside of this suburban ring. Mirroring this result, there is strong evidence of capitalization within the suburban ring, but only limited evidence

[1] The literature starts with Oates (1969). Relatively more recent studies include Palmon and Smith (1998), Black (1999), Barrow and Rouse (2004), Hoxby and Kuziemko (2004), Bayer, Ferreira and McMillan (2007) and numerous others.

of capitalization outside the ring. The differing responses appear to reflect differing housing supply elasticities in the suburban ring relative to the rest of the state. Finally, communities which saw their property tax burdens fall, and hence experienced a spike in building activity, were induced to increase the stringency of their land use policies. It thus appears that communities with a surge in building activity sought to dampen the pace of growth through increased regulation – an occurrence which can be viewed as an endogenous reduction in the elasticity of housing supply.

Given the importance of the property tax in the U.S. system of fiscal federalism, the response of residential capital to property taxation has received very little past empirical attention. Wassmer (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. Both papers find a negative association between the capital stock and property taxes. The current paper differs in two primary ways from these contributions. First, the "natural experiment" of the New Hampshire reform provides unusually credible exogenous variation in property tax burdens. Second, this paper explores several issues beyond the direct relationship between capital and the property tax. Most notably, housing supply elasticity and zoning are explored. Johnson and Walsh (2009) show that property taxes influence the location of vacation homes in Michigan. However, vacation properties form a relatively small share of residential capital in the U.S. Given that many of the considerations which play a crucial role in deciding where to locate primary residences—commuting times, school quality, etc.—are not in play for vacation homes, it is not clear that the Johnson and Walsh results can be extrapolated to the overall stock of residential capital.

The paper's findings have policy implications as they suggest that any policy which de-links expenditures and taxes at the local level may distort the location of residential capital. Relatedly, the results speak to the long running debate over the incidence of the property tax (e.g. Fischel 2001b, Oates 2001 and Zodrow 2001). In addition, the result that the reform sparked an increase in land use regulation contributes to our understanding of the determinants of such regulation—a subject where the existing empirical evidence is thin (Saks 2008, Hilber and Robert-Nicoud 2013).

The paper proceeds as follows. Section 2 presents background information on the New Hampshire reform. Section 3 discusses the connection between the housing market and fiscal amenities in general and specifically with respect to the New Hampshire reform. Section 4 presents the data. Section 5 presents the empirical model and discusses issues of econometric identification. Section 6 presents the results. Section 7 discusses the broader implications of the paper's finding, with a particular focus on the incidence of the property tax. Section 8 concludes.

2 The New Hampshire Reform

Prior to 1999, New Hampshire education was funded primarily by local property taxes. 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.

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 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, eighty percent of communities receive positive grants from the state. The remaining communities, referred to as 'donor towns', are forced to remit payments to the state (i.e. they receive negative grants). The primary determinant of a municipality's grant is per-pupil property wealth – the lower a town's per-pupil property wealth, the larger the grant. The grants are unconditional, however, in the sense that they are neither a function of taxes nor spending.

The 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[2] and the use of lottery revenue. None of the taxes increased were property based, nor is there any obvious reason why the incidence of these taxes by municipality would be correlated with the size of the grants.

The reform was large in magnitude. The 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. Appendix Figure A1 displays the large shift from local to state financing produced by the reform.[3]

Standard economic theory predicts that an unconditional grant to a locality will be spent on public goods at the community's marginal propensity to spend on public goods out of private income (Bradford and Oates 1971 a,b). As the marginal propensity to spend on public goods is estimated to be between 5 to 10 cents on the dollar, theory predicts that only 5 to 10 cents per

[2] Examples of these taxes, which do not include property taxes or broad based sales or income taxes, are the business profits tax, tobacco tax, real estate transfer tax and the car rental tax. See Hall (2002) and Lutz (2010) for more information.

[3] The treatment of the 1999 reform is a simplification which highlights the important elements. See Lutz (2010), and the references it contains, for more detailed information.

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. The "flypaper effect" literature, though, contradicts this prediction and documents that grants are typically spent as intended by the sending government (Hines and Thaler 1995). This empirical tendency creates the expectation that only a limited portion of the New Hampshire grants will be spent on property tax reduction.

Despite this expectation, Lutz (2010) documents that the New Hampshire grants were subject to little to no flypaper effect. Estimates of the portion of the grants used to fund property tax reduction range from 80 to 100 hundred cents per grant dollar. As a result, the grants can be viewed as being roughly equivalent to a downward shock to municipal property tax burdens.

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 voter's preferred level of spending. In contrast, most studies which document a flypaper effect do so in environments in which it is less clear whose preferences are determining budgeting decisions.

3 The Housing Market and Fiscal Amenities

3.1 Brief Sketch of the Theory

This subsection provides a brief overview of the theoretical connection between fiscal amenities and housing market outcomes. The working paper version of this work, Lutz (2009), contains a more formal treatment.

An increase in grants from a higher level of government, as occurred in New Hampshire, will increase a community's fiscal surplus – the difference between the benefit received from local public goods and taxes paid. The positive shock to fiscal surplus will, in turn, generate a positive demand shock for housing in the community as individuals can now consume more public goods per tax dollar paid. If housing supply is perfectly inelastic – i.e. the supply curve is vertical – the housing market will clear the fiscal shock solely thorough a price adjustment. Prices will rise until the benefit of the grants to the median homebuyer is perfectly offset by the increased price of housing in the community (i.e. full capitalization of the grants).

The above scenario assumes that the supply of housing is fixed in the community. Several arguments have been made to justify this assumption. Housing may be fixed because land – a required input into housing – is in perfectly inelastic supply. 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 alternative uses, such as agriculture, and into residential use when residential rents exceed agricultural rents (Capozza and Helsley 1989). Thus, a shock to fiscal amenities which raises residential rents will provide an incentive to shift land into residential use (Hamilton 1976). Zoning is another potential source of inelasticity. However, Glaeser and Gyourko (2002) present evidence that the impact of zoning varies greatly across the U.S. and has little effect in many locations. Ultimately the elasticity of housing supply is an empirical question and a recent literature suggests that supply is quite elastic in many part of the country.[4]

If housing supply is not perfectly inelastic, a fiscal shock will induce both a price and a quantity response. As in the inelastic supply case, a positive fiscal shock induces a positive demand shock for housing. Unlike in the inelastic case, though, the owners of the least productive agricultural land begin converting their land to residential use because residential use now provides a higher return than agricultural use. The conversions continue until the return on the least productive remaining piece of agricultural land equals the return to residential land. Relative to the pre-fiscal shock equilibrium, both the price and quantity of housing has increased.[5]

The price response to a fiscal shock will generally be larger in the inelastic supply case than in the elastic supply case. However, if demand is perfectly elastic—i.e. the demand curve is horizontal—then a fiscal shock will induce the same price response regardless of local supply conditions. In most models of jurisdiction choice, including the one in working paper version of this work (Lutz 2009), downward sloping demand curves require that both communities and homebuyers be heterogeneous. If communities are homogeneous commodities, demand for a *specific* community will be perfectly elastic at the market wide price. If homebuyers are homogeneous, then all achieve the same reservation level of utility and demand is perfectly elastic in each community at the price which, conditional on the jurisdiction's amenities, provides this reservation level of utility. The assumption that homebuyers are heterogeneous in terms of income and preferences is perhaps not controversial.

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[4] E.g. Glaeser and Gyourko (2002), Glaeser, Gyourko and Saks (2005,2006), and Saiz (2010). See Saks (2008) for a review of the literature.

[5] Many of the studies which examine the influence of public goods on the housing market rely on the model developed by Brueckner (1979, 1982, 1983), which explicitly assumes a fixed supply of housing. Three recent papers have, however, relaxed this assumption. All three are related to the work undertaken in this paper. Hilber and Mayer (2009) use the amount of developable land as a measure of the elasticity of housing supply in a community and document that public school spending capitalizes at a higher rate in communities with less developable land. They also document that the supply of developable land influences the impact of school spending on building activity. In contrast, Hoyt, Coomes and Biehl (2009) find that although property tax limitations capitalize, they do not influence housing supply. Greenstone and Gallagher (2008) examine the impact of Superfund-sponsored cleanups of hazardous waste sites on property values and housing supply. They find no evidence of either capitalization or a housing supply response.

The assumption that communities are heterogeneous is more open to debate (e.g. Glaeser and Ward 2009). However, as will be shown below, New Hampshire communities displayed stark differentiation in terms of public good and tax bundles prior to the 1999 reform. Moreover, the reform itself had sharply different effects across municipalities. Neither fact is consistent with these communities being undifferentiated commodities. Thus, it is quite plausible that the demand curves for individual New Hampshire localities are downward slopping. See Bayer, Ferreira and McMillan (2007) and Hilber and Robert-Nicoud (2013) for more in-depth and formal treatment of downward sloping local demand curves.

3.2 What Can Be Learned from the New Hampshire Reform?

Several aspects of the New Hampshire reform make it unusually well suited to assessing how the housing market adjusts to differences in fiscal amenities. First, and foremost, the reform provides unusually credible exogenous variation in property tax burdens with which to identify the relationship between housing quantities and tax burdens. Section 5 discusses this in significant detail.

Second, the reform is almost certainly permanent because it is based on a ruling by the state Supreme Court and can only be revoked 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.

Third, because the reform shifted property tax burdens with little change in the local public goods bundle, it clearly caused a shift in fiscal surplus—the difference between the value of public goods and the tax burden. This is important because capital will reallocate geographically in response to tax differentials only if the differentials are not associated with differences in the level of services relevant to that form of capital (Nechyba 2001). For instance, if a municipality has a higher tax rate than its neighbor, but the higher taxes are used to fund public goods fully valued by homeowners, there is no reason for housing capital to flow into the lower-tax neighboring jurisdiction. The New Hampshire reform therefore provides precisely the type of variation in tax rates appropriate to test the connection between property tax burdens and capital mobility: variation which induces a change in fiscal surplus.

Fourth, although developed land and structures are assessed at their market value in New Hampshire, undeveloped land—e.g. forested land—is assessed at only a small fraction of its market value.[6] The different tax treatments are relevant because a negative shock to the tax burden will increase the after-tax returns to agriculture and thereby shift inward the supply curve for residential housing (i.e. landowners must now be paid more to shift their agricultural land to residential use). This dynamic will mute the response of residential investment to a decrease in the property tax rate. However, with undeveloped land assessed at such low values, the inward shift in the supply curve will be very minor relative to the outward shift in the demand curve caused by the tax change. This makes it relatively more likely that the response of residential investment to a tax burden shock will be large enough to estimate empirically.

Finally, it is important to note that New Hampshire has undergone significant development in recent years and is no longer a rural state: It ranks 18th among the fifty states in terms of housing density and sixty percent of its population lives in an Urbanized Area (2000 Census).

4 Data and Summary Statistics

The data come from multiple sources. Building permit data for new single-family homes, collected by the U.S. Census Bureau, measures investment in residential capital.[7] Sales price data, collected by the New Hampshire Housing Finance Authority, measures property values. Property tax data come from the New Hampshire Department of Revenue Administration and the reform grant data come from the New Hampshire Departments of Education and Revenue Administration. Data on land use regulation come from the New Hampshire Office of Energy and Planning and a dataset compiled by Richard England. The Office of Energy and Planning data are supplemented by a survey of municipalities conducted by the author. Data on land use are from Hilber and Mayer (2009). Finally, demographic data are from the 2000 Census. 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 are expressed in constant 1999 dollars).

[6] A hypothetical 50 acre tract that is forested or in agricultural use would face an annual tax burden of around \$300. The same tract, in use for residential purposes, would face a \$7,000 tax bill exclusive of the tax on housing capital (Ruedig and Gartrell 2002). The lower tax on agricultural land in New Hampshire reflects typical practice in the U.S.: Land that is vacant or in use for agricultural purposes is almost always taxed at a fraction, typically a small fraction, of the land's market value (Vitaliano and Gravelle 2005).

[7] Although a measure of the dollar value of residential investment would be useful, an accurate measure of this type is not available.

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 for tax reduction. The mean fiscal shock is equal to 0.15 (row #1), indicating the mean municipality would have been able to achieve a 15% reduction in its tax burden. The 10th percentile municipality experiences a negative shock of -0.05 (row #2). 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 0.29 (row #4). This low per-pupil property wealth community receives aid equal to almost a third of total local tax revenue.

Conditional on receiving a positive net grant, both the municipal tax rate and total tax burden declined between 1998 and 2000 (rows #6 and #8). Conditional on receiving a negative net grant, both the tax rate and total tax burden increased (rows #7 and #9). Tax burdens increased for both types of towns after 2000, primarily reflecting increased education spending. (Education spending also rose in neighboring New England states over this period.) Despite the increased tax *burden*, tax *rates* fell substantially after 2000 as the result of rapidly increasing property values.

Panel A of Figure 1 displays the mean values of two of the outcome variables used in this study: 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.[8] This is a measure of the extensive margin of residential investment. Although intensive margin investment—the size and quality of both new and existing homes—may respond to differences in fiscal surplus, data on such investment are not available by municipality. House prices are measured as the mean sales value of existing homes in a municipality. Figure 1 suggests that demand for housing in New Hampshire was increasing over this period – both housing prices and the quantity of new homes rose rapidly. Rows 12 - 15 of Table 1 present additional summary statistics for residential investment.

5 Empirical Model and Identification

The effect of the 1999 New Hampshire fiscal shock on building activity is estimated with the following reduced-form specification

$$\frac{permits_{m,t}}{hstock_m} = \alpha + \beta \frac{netgrant_{m,99}}{ptax_{m,98}} * postreform_t + \phi_t + \eta_m + \varepsilon_{mt} \tag{1}$$

[8] See the Data Appendix for additional information. The analysis uses single-family home building permits as the metric for residential investment. However, the results are robust to using total housing unit building permits.

where $postreform_t$ is an indicator variable equaling one in years greater than or equal to 1999, the first year of the reform. β is the coefficient of interest and captures the relationship between the reform grants and residential investment.

The estimate of β will be biased if the grant variable is correlated with the unobserved determinants of building activity, ε_{mt}. Although the reform grants can be viewed as an exogenous shock, they may still be correlated with ε_{mt} for several reasons. First, the reform grants were recalculated annually after the second year of the reform. These recalculated reform grants may reflect adjustment to the reform. In particular, they may reflect residential investment endogenous to the reform — i.e. increased investment in response to the reform will increase aggregate property values and, all else equal, reduce the size of the grant. The grant is therefore held fixed at its initial level. 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. Second, the fiscal shock measure is a function of the time-invariant arguments of the grant formula (primarily per-pupil property wealth) and the municipal tax burden in the year prior to the reform. Any correlation between these factors and unobserved determinants of building activity could produce bias in $\hat{\beta}$. For instance, there may be permanently higher demand for new homes in wealthy communities. Alternatively, wealthy communities may have more stringent land use policies which effectively keep new construction at lower levels than in less wealthy localities. The inclusion of municipal fixed-effects, η_m, controls for any such time-invariant factors which might lead to spurious estimates of β. Third, any state-wide difference in building activity in the pre versus post reform period will be correlated with the $postreform_t$ indicator and could therefore introduce bias. Year fixed-effects, ϕ_t, are included to address this possibility.

With the inclusion of the municipal and time fixed-effects, the effect of the grants on residential investment is identified by the municipal-specific change in building activity over time. Thus, the identifying assumption required to interpret β in a causal sense is that $\frac{netgrant_{m,99}}{ptax_{m,98}} * postreform_t$ be uncorrelared with municipal-specific, time-varying determinants of investment activity other than the grants. It is important to scrutinize the likely validity of this assumption.

The grants are mostly a function of per-pupil property wealth and, as a result, the principal threat to the identifying assumption is the presence of trends in building activity associated with per-pupil property wealth for reasons other than the grants. Of particular concern is the possibility that the nationwide housing boom occurring during the sample period may have caused building activity to evolve differentially across communities. If such differential evolution is correlated with per-pupil property wealth, the identifying assumption will be violated. For instance, it is possible that demand for communities with elevated levels of per-pupil property wealth—e.g.

resort communities—rose at a particularly fast clip over this time. Such a scenario would produce downward bias in β. Alternatively, the rapid growth in subprime lending—the extending of mortgage credit to households with low credit scores—over this period may have caused a relative surge in home construction in low property wealth communities. Such a scenario would produce upward bias in β.

A two-part approach is taken to address such concerns. First, four additional models are estimated as robustness checks. These models attempt to control for time-variant, municipal-specific determinants of investment that may be correlated with per-pupil property wealth. Second, a falsification test is executed. While none of these specifications are definitive in isolation, jointly they provide a useful assessment of the likely validity of the identifying assumption.

The first of the robustness checks is

$$\frac{permits_{m,t}}{hstock_m} = \alpha + \beta \frac{netgrant_{m,99}}{ptax_{m,98}} * postreform_t + \varphi_t * X_m + \phi_t + \eta_m + \varepsilon_{mt} \tag{2}$$

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.[9] The model controls for changes over time in investment that are associated with these fixed municipal characteristics. For example, distance from Boston controls for rapid growth in southern New Hampshire over this period, while the percent of property for recreational use controls for a possible increase in building activity in resort communities. The second robustness check controls for differential evolution in home construction across the 10 counties of New Hampshire by allowing the year intercepts to vary by county c (i.e. ϕ_t in equation (1) becomes $\phi_{t,c}$). This specification controls for differential evolution in building activity across counties. For instance, it addresses the possibility that the housing boom was more pronounced in some counties than in others. The third robustness check adds municipal-specific linear trend terms, $\eta_m * t$, to the model. This specification controls for the municipal evolution in building activity over the sample period (conditional on this evolution being linear). The final robustness check directly controls for variation in building activity over time associated with per-pupil property wealth by including data from states which neighbor New Hampshire. This approach is described in detail below.

[9] The complete set of characteristics in the X_m vector are distance from Boston, distance from Boston squared, municipal population, municipal population squared, the percent of municipal property that is residential, the percent of municipal residential property that is for seasonal or recreational use, and municipal density (defined as the total number of housing units divided by land area). The percent of homes used for seasonal or recreational use and the percent of the tax base that is residential are only available as measured in 2000. None of the models include time-varying variables, such as demographic characteristics, as controls because changes in such variables may be endogenous to the reform.

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

$$\frac{permits_{m,t}}{hstock_m} = \alpha + \beta \frac{netgrant_{m,99}}{ptax_{m,98}} * postreform_t + \beta_{placbeo} \frac{netgrant_{m,99}}{ptax_{m,98}} * prereform_t + \phi_t + \eta_m + \varepsilon_{mt} \tag{3}$$

where $prereform_t$ equals one in 1998 (as discussed immediately below, 1999 is omitted from the sample and 1998 is therefore the last pre-reform year). 1996 and 1997 are the omitted year categories for the vector of fiscal shock–time period interaction terms. The rationale behind the test is straightforward – there should be no response to the fiscal shock in the year prior to the reform. In addition to serving as a general robustness check against preexising trends in building activity correlated with the fiscal shock, the test is well suited to addressing the possibility that the reform was anticipated. If housing market participants anticipated the fiscal shock, they may have responded before the reform's legal enactment. Such a scenario would likely bias the β coefficient downward.[10]

The above models are estimated with data ranging from 1996 to 2003, with 1999 omitted from the sample. 1999 is omitted for two reasons. First, the reform was announced in November of 1999 and it is unlikely that there was a significant investment response in the remainder of the year. 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 and it may have been unclear how a given municipality would respond in the long-run (i.e. if the grants would be used for tax reduction or increased government spending). 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. Very small municipalities, those with fewer than 1200 residents in year 2000, are dropped from the sample. Finally, municipalities are excluded if they have missing data for either the residential investment or fiscal shock variables in two or more years of the sample. (Missing data are due to the relevant government agency failing to report data for a given municipality in a given year; this exclusion drops $3\frac{1}{2}$ percent of the potential observations).

[10] As discussed in detail in Lutz (2010), although the court decision mandating school finance reform was initially issued in 1997, there was tremendous uncertainty over both the magnitude and specifics of the reform. The reform that was finally enacted did not take shape until late 1999, when the legislature was facing the prospect that the state's schools would be forced into insolvency. Thus, it seems unlikely that housing market participants would have been in a position to take anticipatory action.

6 Results

6.1 The Quantity Response: The Response of Residential Investment to the Fiscal Shock

Table 2, column (1), presents the results of estimating equation (1). The β estimate is precise[11] and economically large. Evaluated at the mean value of the fiscal shock measure, the estimate implies a 0.17 increase in residential investment (see the "Implied Change in Dep. Var." row). Using the mean rate of investment in 1998, 1.6, this implies the fiscal shock induced an 11 percent increase in the rate of residential investment (see the "Implied Percent Change in Dep. Var." row). The mean value of the fiscal shock measure is 0.15, interpretable as a 15 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 very roughly equal to (negative) one.

After the announcement of the reform, it may have taken time for investment to fully respond because construction takes time to implement. Column (2) explores this possibility by allowing the coefficient on the fiscal shock measure to vary by year. The results display no clear trend over time. Column (3) tests the sensitivity of the results to the decision to exclude communities with fewer than 1,200 residents. In this case only the smallest communities, those with fewer than 500 residents, are excluded. The estimate remains precise and is roughly a third larger than the estimate in column (1).[12]

The remaining columns present the robustness checks and the falsification check. These specifications shed light on the likely validity of the identifying assumption that the reform grant variable is uncorrelated with unobserved, time-varying determinants of municipal building activity. Column (4) displays the results of estimating equation (2). Inclusion of the municipal characteristic-year interaction terms more than doubles the magnitude of the estimate relative to column (1): The grants are estimated to increase investment by 22 percent. Inclusion of the county-specific year terms (column (5)) also yields a larger coefficient.

In contrast, inclusion of the municipal-specific linear trend terms (columns (6)) diminishes the size of the point estimate and reduces its precision. As is always a concern with specifications of this type, it is possible that the trend terms are absorbing some of the impact of the event being

[11] Throughout the text, the word “precise” is used as shorthand for “statistically distinguishable from 0 at the conventional 95 percent confidence level".

[12] Use of other possible sample size cutoffs, including dropping no municipalities, produces broadly similar conclusions (unreported).

studied – the grant introduction – and that the point estimate is therefore biased. One method for minimizing this possibility is the inclusion of a long spell of pre-reform data to more credibly identify the *preexisting* trends (Wolfers 2006). Thus, four additional years of data are added to the sample, extending the data back to 1992. First, the model without municipal-specific trends is estimated on the new sample (column ((7)). The estimate is essentially unchanged from column (1). Second, the model with trend terms is estimated over the new sample (column (8)). The magnitude of the estimate remains quite similar to column (1). However, if the municipal-specific trends in construction over this longer time period are non-linear, this specification may not be appropriate. Residential construction in New Hampshire was relatively constant in the early part of the 1990s and then began to accelerate around 1997. The year effects will absorb this market-wide, non-linear trend. It is possible, though, that the residual, municipal-specific construction also tends to display trend breaks around 1997. The municipal-specific linear trend terms are ill-suited to controlling for such non-linear dynamics. Additional specifications including municipal-specific linear trend terms are explored below.

Finally, column (9) presents the results of estimating the falsification check, equation (3). The placebo grant point estimate is negligible and the true fiscal shock coefficient is little changed. Thus, the falsification check fails to cast doubt on the validity of the empirical design. In particular, it suggests the results are not biased by correlation between the grants and trends in building activity which pre-date the introduction of the grants.

The final robustness check would ideally be executed as

$$\frac{permits_{m,t}}{hstock_m} = \alpha + \beta \frac{netgrant_{m,99}}{ptax_{m,98}} * postreform_t + \beta_d determinants_m * postreform_t + \phi_t + \eta_m + \varepsilon_{mt} \quad (4)$$

where $determinants_m$ is the vector of arguments appearing in the grant formula. The specification controls, in a time-varying manner, for the determinants of the magnitude of the fiscal shock. Thus, it addresses in a relatively direct fashion the concern that the β estimates are biased because the fiscal shock is correlated with municipal-specific, time-varying determinants of investment activity. Although equation (4) is not viable when estimated using data only from New Hampshire – the grants are primarily a linear function of $determinants_m$ – it becomes viable with the inclusion of data from other states. Surrounding New England states provide '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 with the identifying variation coming from the interaction of three variables: the fiscal shock measure, a post-reform indicator and a New Hampshire indicator.

A practical problem with estimating equation (4) is the unobservability of the $determinants_m$

vector for the control states. A measure of per-child residential property wealth, taken from the 2000 Census, is therefore used as a proxy for the $determinants_m$ vector. The census measure is a strong predictor of the fiscal shock: The two variables have a cross-sectional correlation of -0.76 in the New Hampshire sample.

The specification rests on the assumption that the other New England communities are a valid counterfactual for the New Hampshire communities. Although this assumption is inherently untestable, the control groups are constructed to be similar to New Hampshire along observable dimensions. Column (1) of Table 3 presents demographic characteristics for the New Hampshire sample. Column (2) presents the demographics for the 'Southern Maine' control group and Appendix Figure A2 maps its geographic boundaries (the precise geographic definitions of the control groups, as well as the rationale underlying their definitions, are provided in the Data Appendix). Other than somewhat lower median incomes and home values, the Southern Maine communities are demographically quite similar to their New Hampshire counterparts. Most importantly, the average per-child residential property wealth is extremely comparable. Furthermore, as displayed on Panel B of Figure 1, the two groups display a very similar upward trend in building activity over the sample period.

Table 4 presents the results of estimating equation (4). Column (1) displays the results when no control communities are included. As expected, given that per-child residential property wealth is a strong linear predictor of the fiscal shock, the β point estimate is small and imprecise. With the inclusion of the 'Southern Maine' control group in column (2), however, the estimate becomes precise and similar in magnitude to those in Table 2. Column (4) uses 'Western Massachusetts' as the control group and the estimate becomes somewhat larger, but remains within the range of those in Table 2.

The robustness check is invalid if the New Hampshire reform induced changes in investment activity in the control groups. In general, there is no clear prediction for how the reform should influence building activity outside of New Hampshire because of the increase in statewide taxes used to fund it: While the grants make some New Hampshire communities relatively more attractive compared to out-of-state communities, the statewide tax increase has the opposite effect. The net effect is ambiguous. It is possible, though, that the reform influenced investment decisions across state borders for certain communities. For instance, investment in property wealthy communities located near the Maine-New Hampshire border may have reallocated toward Maine because the New Hampshire communities received no benefit from the reform, but were required to pay higher statewide taxes. To address this concern, column (6) uses a control group comprised of 'Southeastern Maine'. The municipalities in this control group are a minimum of 40 miles from the border with New Hampshire, although the sample size is rather small. The results are again quite similar to those in Table 2. Column (8) adds a portion of 'Central New England' to increase the size of the control group and produces very similar results. Using all communities in Connecticut, Massachusetts, Maine and Rhode Island as a control group – column (10) – also produces similar results.[13]

As discussed below, the investment response appears to vary with distance from Boston and it would therefore be preferable for the control groups to be broadly similar to New Hampshire along this dimension. The Western Massachusetts control group is extremely comparable in this regard (see the bottom of Table 3). Some of the other control groups, most notably Southern Maine, are not. Column (12) therefore uses all communities in Connecticut, Massachusetts, Maine and Rhode Island which are located between 33 and 135 driving miles from Boston—a control group constructed to be extremely similar to New Hampshire in terms of distance from Boston. The estimate is nearly identical to the result produced using the control group comprised of the same states, but without the distance from Boston restriction (column (10)).

Finally, Table 4 also displays estimates from specifications which include municipal-specific trend terms. The estimates are robust to this specification check.

6.2 Heterogeneity in the Quantity Response

If there is heterogeneity in the elasticity of housing supply, there will be heterogeneity in the response of investment to the fiscal shock. Heterogeneity in the elasticity of supply can occur for a number of reasons, including the amount of land available for development and variation in the extent of land use regulation. The canonical monocentric land use model provides relevant predictions. Both the classic monocentric model (Alonso 1964, Mills 1967 and Muth 1969) and models of urban growth based upon it (e.g. Arnott and Lewis 1979, Capozza and Helsley 1989, Wheaton 1982) predict that the amount of land available for development will increase with distance from the urban center. Similarly, the monocentric model suggests that land use regulation will be most intense near the urban core as regulation tends to originate in central cities and then gradually spread outward to surrounding areas (Fischel 2004, Rudel 1989). Thus, the elasticity of housing supply is expected to be relatively low in and near the urban center.

This prediction motivates the division of New Hampshire into two regions – the portion of the state which is a part of suburban Boston, termed the "suburban ring", and the remainder of the state. The suburban ring is comprised of all communities within 50 driving miles of Boston

[13] Vermont is not used to provide control communities because it enacted a school finance reform in 1998. The remaining New England states had stable school finances over the period.

and is displayed graphically on Figure 2 and Appendix Figure A2 (see the Data Appendix for additional information). The 50-mile ring is a reasonable demarcation of the New Hampshire portion of suburban Boston as it is nearly congruent with the boundaries of the census-defined Boston Urbanized Area (which does not follow municipal boundaries).[14] Twenty-seven percent of the sample population resides within the ring.

Table 5 suggests that, as predicted by the monocentric model, the suburban ring differs significantly from the rest of New Hampshire. In terms of land availability, both housing unit density and the percent of land which is developed are much higher in the suburban ring. In terms of land regulation, the prevalence of growth management – a fairly stringent land use regulation which permits municipalities to set a binding limit on the number of new homes built each year – is substantially higher within the ring.

Table 6 presents the results of a specification which allows the response to the reform to differ inside, and outside, of the suburban ring. Within the ring there is no evidence of an investment response as the coefficient is extremely imprecise. Outside of the ring the investment response is precise and quite large, implying a 16 percent increase in home construction for the typical municipality. The inside and outside response can be distinguished from each other at the 10% level (see the "P-value for test" row). The inclusion of either the base demographic-year terms, column (2), or the county-year terms, column (3), yields coefficients which can be distinguished at the 5% level.

Column (4) includes municipal-specific trend terms. The results are little changed from column (1)—an important finding given the sensitivity of the results in Table 2 to the inclusion of such trends. With the investment response constrained to be constant across the state in Table 2, the trend terms may be biasing the estimates by absorbing part of the effect of the reform. Parameterizing the effect of the reform in a more geographically flexible manner may avoid such bias. This interpretation has strong parallels to Wolfers (2006) who demonstrates that an excessively constrained parametrization of the policy under study can cause unit-specific trend terms to bias the estimated treatment effect.

Importantly, the investment response outside the suburban ring is not purely a rural phenomenon. The area is moderately dense by U.S. standards: As a stand alone state, it would be the 22nd densest state and it contains four Urbanized Areas. The evidence in Table 6 and Figure 3 suggest there was an investment response in these urban areas.[15] Moreover, the geographic

[14] Within New Hampshire, 50 miles is approximately the 10th percentile of distance from Boston. The closest New Hampshire community to Boston is located 33 miles from the city.

[15] The largest of these Urbanized Areas, the Manchester Urbanized Area, has around 140,000 residents. In compar-

heterogeneity in the investment response does not reflect correlation between ring communities and the intensity of the fiscal shock: The average magnitude of the shock is almost perfectly equal inside and outside ring (first row of Table 5). Furthermore, Figure 2 reveals significant variation in the size of the shock both inside and outside the ring.[16]

The precise mile cutoff for the ring could be viewed as arbitrary. Figure 3 therefore presents results from a specification which allows the impact of the reform to vary with a quartic in distance from Boston. This specification is extremely flexible and permits the data to determine where the investment response occurred. The x-axis displays distance from Boston and the y-axis displays the marginal effect of the fiscal shock on residential investment, comparable to the β estimates in Table 2. The evidence again suggests that the response to the fiscal shock was concentrated outside the suburban ring. Furthermore, the figure suggests that the response outside the suburban ring was geographically broad.[17]

The geographic heterogeneity in the response to the reform is consistent with the hypothesis that housing supply elasticity is relatively higher outside of the suburban ring. Likely explanations are land availability and regulation. Although the suburban ring is much denser than the rest of the state, it is not dense in absolute terms. It is only roughly a third as dense as the Massachusetts communities within 33 miles of Boston (excluding Boston) and only 20 percent of land in this area has been developed (Table 5). These facts suggest that there is ample land available for development and regulation therefore seems a relatively more likely explanation. This conjecture, though, cannot be confirmed by the empirical evidence: Specifications which test for heterogeneity in the investment response associated with differences in land availability and land use regulation across communities are largely uninformative. See Appendix 1.2 for details.[18]

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ison, the Boston Urbanized Area has over 4 million residents. The Boston Urbanized Area therefore appears to be a much better fit for the "central city" of the monocentric land use model. Extensive efforts were undertaken to test for heterogeneity in the investment response in the urban versus rural areas outside the suburban ring (unreported). While the results are sensitive to specification choices, on net there is little evidence that the investment response differed in the urban areas as compared to the rural areas outside of the ring.

[16] It is possible that there is an investment response in the suburban ring, but that it is delayed. In particular, in supply inelastic locations with tight regulation, the development and permitting process may be slow. However, adding two additional years to the sample (unreported) produces no substantive change to the results displayed in Table 6.

[17] There is some variation in the response to the reform outside of the suburban ring as the marginal effect peaks around 70 miles from Boston and then declines. However, this result should be viewed with significant caution for two reasons. First, the specification is extremely flexible and the marginal effects outside of the suburban ring are not distinguishable from each other over most of the range of distance from Boston (e.g. you cannot distinguish the effect at 70 miles from the effect at 90 miles). Second, 90 percent of the sample population resides within 110 miles of Boston and there is only limited building activity as you move into the far northern region of the state. The low level of construction in this area may limit the scope for an investment response to the reform.

[18] Attempts to directly estimate the supply price elasticity using the instrumental variables approach of Hilber and

Finally, before turning to outcomes other than residential investment, it is useful to view the robustness and falsifications checks appearing in Tables 2, 4 and 6 jointly. Although it is not possible to verify an empirical model's identifying assumption with certainty, these specifications jointly rule out most plausible scenarios which would violate the identifying assumption that the grants are uncorrelated with other determinants of the municipal-specific evolution in building activity. Thus, it is likely, but not certain, that the results in this paper capture the causal response of home construction to the introduction of the grants.

6.3 The Price Response: The Capitalization of the Fiscal Shock

If the supply of housing is not perfectly elastic, the fiscal shock will produce a price response. Column (1) of Table 7 presents the results of estimating equation (1) with the log of the mean sales price of existing homes as the dependent variable. The estimated response to the shock is positive but only marginally significant. Column (2) includes the suburban ring interaction terms and demonstrates that the grants capitalize at a substantially higher rate in the area near Boston than in the rest of the state. Evaluated at the mean fiscal shock, the results imply that the typical suburban ring community experiences a 5% increase in the value of existing homes. There is also weak evidence for a smaller capitalization effect outside the ring and it is not possible to distinguish between the extent of capitalization inside and outside the ring (see the "P-value for test" row of Table 7). However, when the base demographic-year terms, column (3), or the county-year terms, column (4), are included, these effects can be distinguished. Columns (5) - (8) use a different measure of property values – the market value of all real estate, including commercial, as measured for tax purposes – and produce similar conclusions.

There is an important caveat to the interpretation of the price results. The intensive margin of residential investment—i.e. the size and quality of homes—may respond to the fiscal shock within the suburban ring. The factors such as zoning and land availability, which prevent an extensive margin investment response to the fiscal shock, may not prevent intensive margin investment from responding. For example, the reform may have increased the number of additions and renovations to existing homes. Intensive margin investment would likely increase the sales value of a home and therefore potentially explains a portion of the suburban ring "price" response documented in Table 8. Unfortunately data limitations preclude estimating the effect of the reform on intensive margin investment. However, the annual size of such investment is too small to explain more than a small

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Mayer (2009) find the hypothesized higher elasticity outside the suburban ring than within the ring. However, these results suffer from both a lack of precision and from a weak instruments problem and are therefore tentative in nature. They are available from the author upon request.

fraction of the estimated price response under a reasonable set of assumptions. See Appendix 1.3 for the calculations underlying this claim and additional discussion.[19]

The investment and capitalization results are complementary. It appears that the housing market cleared the fiscal shock primarily through a quantity response in most of the state. In the suburban ring, in contrast, the market cleared primarily through a price adjustment. These results are consistent with a substantially more inelastic supply of housing in the area near Boston.

The claim that the 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. The estimates broadly support this implication, as the extent of capitalization within the suburban ring in columns (2)-(4) and (6)-(8) ranges from 70 to 97 percent.[20]

6.4 The Regulatory Response

It is possible that the increase in residential construction sparked by the fiscal shock will cause voters to enact land use regulations aimed at reducing the pace of development or altering its form. Homeowners may wish to restrict new construction in order to mitigate costs associated with additional development and density, to prevent newcomers from free-riding on the existing tax base (Hamilton 1975, 1976), to extract rents from newcomers (White 1975) or to increase the value of their homes (Fischel 2001a).

Table 8 presents estimates of the impact of the fiscal shock on the probability of utilizing growth management and charging impact fees for development (these are the only regulatory measures available at multiple points in time – see the Data Appendix for more information, including summary statistics). A two-period panel with data from 1999 and 2008 is used, both because of data limitations and because a regulatory response would be expected to manifest itself only gradually, after the increased pace of development becomes apparent. The approach is otherwise the same as on the preceding tables.

Column (1) shows that the fiscal shock is associated with an increased probability of utilizing growth management. Evaluated at the mean value of the fiscal shock, the estimate suggests that municipalities were 0.06 percentage points more likely to adopt the policy as a result of the reform – a striking increase of almost 50 percent relative to the probability of having adopted the policy

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[19] England and Huang (2012) assess the impact of the property tax on residential density and show both theoretically and empirically that the tax is associated with less living space per newly developed acre.

[20] The full capitalization rate, $\frac{netgrant_{ring}}{k}$, is calculated with a discount rate k equal to 0.07, the 30-year conventional mortgage rate in 2000, and $netgrant_{ring}$ equal to the mean value of net grants in the suburban ring in the first-year of the reform. The actual capitalization rate is estimated using the value of all taxable municipal real estate in 1998, the year prior to the reform, and the estimates in columns (2)-(4) and (6)-(8) of Table 7.

as of 1999 (0.13). Specifications which allow the investment response to vary by the suburban ring find statistically precise evidence of an effect outside the ring, whereas the effect inside the ring is imprecise. The point estimates are similar in magnitude, though, and are not distinguishable (unreported). Column (4) presents the estimate for impact fees – payments required of developers to defray the municipal cost of new housing (e.g. new school construction). No evidence is provided that the reform increased regulation on this margin.[21]

The growth management result can be interpreted as suggesting there was a supply side response to the reform: The increased regulatory stringency will steepen the slope of a community's housing supply curve (or even cause it to be vertical at the quantity of houses permitted under the growth management ordinance). In turn, this supply side response will dampen the construction, or quantity, response caused by the reform induced outward shift in demand.[22]

7 Implications

7.1 Property Tax Incidence

The evidence in this paper speaks to the long running debate over the incidence of the property tax. There are two views: the 'benefit view' and the 'capital tax view'.[23] The benefit view extends the Tiebout (1956) model, in which individuals select their preferred bundle of local public goods through their choice of which community to reside in, by adding zoning regulations and capitalization (Hamilton 1975, 1976). Zoning fixes the supply of residential capital in a community. With housing supplied perfectly inelastically, any difference between the property tax burden associated with a given home and the corresponding bundle of local public goods will capitalize into the price of the home. Zoning and capitalization thereby convert the property tax, inclusive of the home

[21] Impact fees may be used to address short-term concerns such as newcomers failing to cover one-time development costs (e.g. new sewer lines). Growth management, on the other hand, may be motivated by longer-run concerns such as preserving home values and avoiding excess density. If the desire for additional regulation was motivated by the longer-term consequences of elevated building activity, this may explain why the growth management results differ from the impact fee results. Alternatively, impact fees may in some instances actually facilitate development by permitting side payments to communities thereby reducing opposition to new development. In this case, it is not clear that the fiscal shock, and corresponding surge in building activity, would be expected to trigger an increase in the use of impact fees.

[22] It is possible that the reform induced supply side responses beyond the one documented here. For instance, prior to the reform towns may have had an incentive to favor commercial development over residential development because commercial properties place fewer demands on public services (in particular they do not increase the number of enrolled students). The reform possibly weakened this incentive because new commercial property will now increase a community's per-pupil property wealth and thereby decrease the amount of reform grants received. Unfortunately, the existing data is insufficient to test such hypotheses.

[23] Fischel (2001b), Oates (2001), Nechyba (2001) and Zodrow (2001) provide an overview of the two views including detailed literature reviews.

price, into a user charge for local public goods. With costs and benefits aligned, the tax causes no distortion.

The ‘capital tax view’, often referred to as the ‘new view’, offers a sharply different assessment of the tax. Employing a Harberger-style general equilibrium framework, the tax is shown to have two distinct effects (Mieszkowski 1972, Zodrow and Mieszkowski 1986). First, the average level of taxation across communities acts as an economy-wide tax on capital. Assuming the capital stock of the economy is fixed, the incidence of this portion of the tax falls on the owners of capital. Second, differentials around the average rate of taxation exert an "excise tax" effect. Capital is assumed to be mobile and therefore escapes the incidence of this portion of the tax. Instead, the incidence falls on whatever factors are assumed immobile (which can include land, workers, consumers, renters, etc.). Thus, the property tax is distortionary: The profits tax effect lowers the economy-wide capital stock and the excise tax effect causes a misallocation of capital across jurisdictions.

Determining the relative validity of the two views is important as they have sharply different implications for the incidence of the property tax and for assessing the efficiency of local public goods provision (Nechyba 2001, Oates 1994, 2001). Existing empirical work, however, largely fails at this task (Nechyba 2001) and as a result our understanding of the incidence of the tax is "in a sad state" (Fischel, Oates and Youngman 2011).

The empirical evidence in this paper advances our understanding of property tax incidence in three ways. First and foremost, the evidence substantiates the operative mechanism of the "excise tax" component of the capital tax view – the fleeing of capital from relatively high property tax jurisdictions. As emphasized in Zodrow (2001):

> *The essential difference between the new view and the benefit view is that the new view implies that relatively high levels of property taxation should drive mobile capital out of a jurisdiction, resulting in lower capital intensity. By comparison, under the benefit view the property tax functions as a user charge for services received, and a relatively high property tax rate should not affect capital intensity.*

Thus, the evidence that the property tax distorts the location of housing capital in the non-suburban ring portion of New Hampshire is an important validation of the capital tax view.

Turning to the suburban ring, the lack of an investment response in this area, combined with the full capitalization of the fiscal shock, is consistent with the benefit view.[24] The incidence of the

[24]The capitalization result alone does not help distinguish between the two theories of the property tax because

reform itself falls fully on current homeowners as a capital gain (loss) in the form of the change in the value of their home. This capitalization ensures that the property tax, inclusive of the house price required to buy into a community, remains a user charge—a payment equal in value to the local public services received. There are two important caveats to interpreting the suburban ring results as supportive of the benefit view, however.

The first caveat is that the benefit view explicitly assumes that housing supply is inelastic due to binding zoning. Although it seems likely that zoning plays an important role in the inelasticity of housing supply documented in the suburban ring, the evidence on this point is inconclusive—see Appendix 1.2. The second caveat is that the benefit view is more expansive than the assertion that inelastic housing supply converts the property tax into a non-distortionary user charge. It further hypothesizes that the combination of household mobility and capitalization causes the bundle of local public goods and associated tax burden to be set at the level preferred by residents. For instance, in the version of the benefit view espoused by Fischel (2001a,b), local officials are incentivized to provide the bundle of public goods preferred by the marginal homebuyer—i.e. the bundle which maximizes house prices.[25] While the results of this paper verify that the capitalization and inelasticity of housing supply necessary for these more expansive claims of the benefit view are present in the suburban ring, they are insufficient to verify that these conditions succeed in producing the preferred bundle of public goods.

Combining the evidence from inside and outside of the suburban ring points toward a synthesis of the two views based on housing supply elasticity: In dense areas with stringent land use regulation, the benefit view is likely a *relatively* better characterization of the tax, while in less dense areas with less land use regulation, the capital tax view is likely the *relatively* better characterization.[26] This synthesis is the paper's second contribution to our understanding of property tax incidence. It is quite consistent with the conjecture of numerous authors that elements of both views may be simultaneously valid (e.g. Wildsain 1986, Kotlikoff and Summers 1987, Ladd 1998, Oates 2001, Fischel, Oates and Youngman 2011).

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capitalization can arise under both views (Zodrow 2001, Nechyba 2001). Capitalization arises under the new view because the return to immobile land is lowered by the tax-induced outflow of capital (Zodrow and Mieszkowski 1986). However, the suburban ring finding of capitalization *occurring in the absence of a capital intensity response* is only consistent with the benefit view.

[25] Brueckner (1979, 1982, 1983) demonstrates that, when housing supply is fixed, property values are maximized at the allocatively efficient level of local public goods.

[26] The discussion in this paragraph draws on Fischel, Oates and Youngman (2011) who also discuss the implications of the evidence in this paper for the incidence of the property tax. Using data on the density of U.S. communities in conjunction with the evidence in this paper, Fischel, Oates and Youngman calculate that roughly $\frac{3}{4}$ of the U.S. may be best characterized as subject to the benefit view.

The paper's third contribution to our understanding of property tax incidence is the evidence that local fiscal conditions *cause* changes in zoning. Specifically, communities which saw a surge in building activity due to the reform increased the stringency of their land use regulation. These jurisdictions may have feared that their public goods—schools, parks, roads, etc.—would become congested and hence less valuable. Under the benefit view, zoning allows communities to prevent their fiscal surplus from being encroached upon by those outside the community. Thus, the increase in regulatory stringency—an action which serves to prevent the erosion of fiscal surplus in the face of the surge in building activity—is extremely consistent with the benefit view. It also appears to confirm the suggestion of Kotlikoff and Summers (1987) that capital tax distortions may over time be at least partially offset by benefit view forces such as zoning changes.

7.2 Additional Implications

Beyond the incidence of the property tax, the results have implications for the Tiebout (1956) theory. Although this theory has been the subject of a vast amount of empirical research, this first-order contention of the original theory—that individuals "vote with their feet" to select their preferred bundle of local public goods and associated tax burden—has rarely been tested.[27] This paper can be interpreted as confirming that household location decisions are, as predicted, heavily influenced by fiscal amenities.

From a policy perspective, the analysis suggests that any policy, such as school finance reform, that de-links expenditures and taxes at the local level may cause housing capital to reallocate. Such distortions are likely to be inefficient. For instance, the geographic location of where land is being converted from non-residential use to residential use will be shifted and may produce inefficient land use patterns. Such distortions should be included in the cost-benefit analysis of these policies.

Finally, the finding that the fiscal shock caused a change in land use regulation adds to our understanding of the determinants of such regulation—an area where the empirical evidence is thin (Saks 2008, Hilber and Robert-Nicoud 2013). Specifically, it suggests that demand shocks can induce regulation. The result is therefore consistent with theoretical work suggesting relatively more dense and developed areas endogenously engage in more stringent land use regulation (Rudel 1989, Fischel 2001a, Hilber and Robert-Nicoud 2013 and Ortalo-Magné and Prat 2007). The results can also be seen as complementary to those in Saiz (2010) which document a similar causal

[27] Most tests of the Tiebout theory have been indirect – e.g. testing for the extent to which fiscal amenities capitalize, assessing the link between income stratification and public goods provision, testing the theory's predictions concerning community heterogeneity, etc. See Oates (2005) and Banzhaf and Walsh (2008) for a more extended discussion. In contrast to most of the literature, both Banzhaf and Walsh (2008) and Reschovsky (1979) directly examine how fiscal amenities influence residential location choice.

relationship running from development and density to regulation, but over a much longer time horizon than examined here.

8 Conclusion

This paper documents important interactions between the market for local public goods and the supply side of the housing market. Using an unusual school finance reform in the state of New Hampshire, it is shown that shocks to property tax burdens have a significant influence on new home construction in most of the state. In the portion of the state closest to Boston, however, the shock has no discernible effect on building activity. Instead the shock clears through home price adjustments. This geographic pattern is consistent with a highly elastic supply of housing outside of the suburban ring and an inelastic supply within the ring. Land availability and land use regulation are potential mechanisms behind this apparent difference in supply elasticities. Firmly pinning down the relative importance of these and other potential supply side factors is an important avenue for future research. Finally, the results suggest that communities which saw a surge in building activity due to the property tax shock responded by tightening the stringency of their land use policies. This can be viewed as an endogenous reduction in the elasticity of housing supply likely to temper the effect of the reform on residential construction over time.

From a policy perspective, the results suggest that policies which shift the attractiveness of public good bundles may distort the location of new home construction. The results also advance our understanding of the incidence of the property tax in three ways. First, they provide some of the first credible evidence that the primary mechanism of the capital tax view – the fleeing of capital from high tax jurisdictions – is operative. Second, they indicate that fiscal shocks can cause zoning – a finding quite supportive of the benefit view. Finally, they point toward a synthesis of the two views, with the benefit view most applicable in urban and dense suburban settings and the capital tax more relevant in less dense suburban and rural locations.

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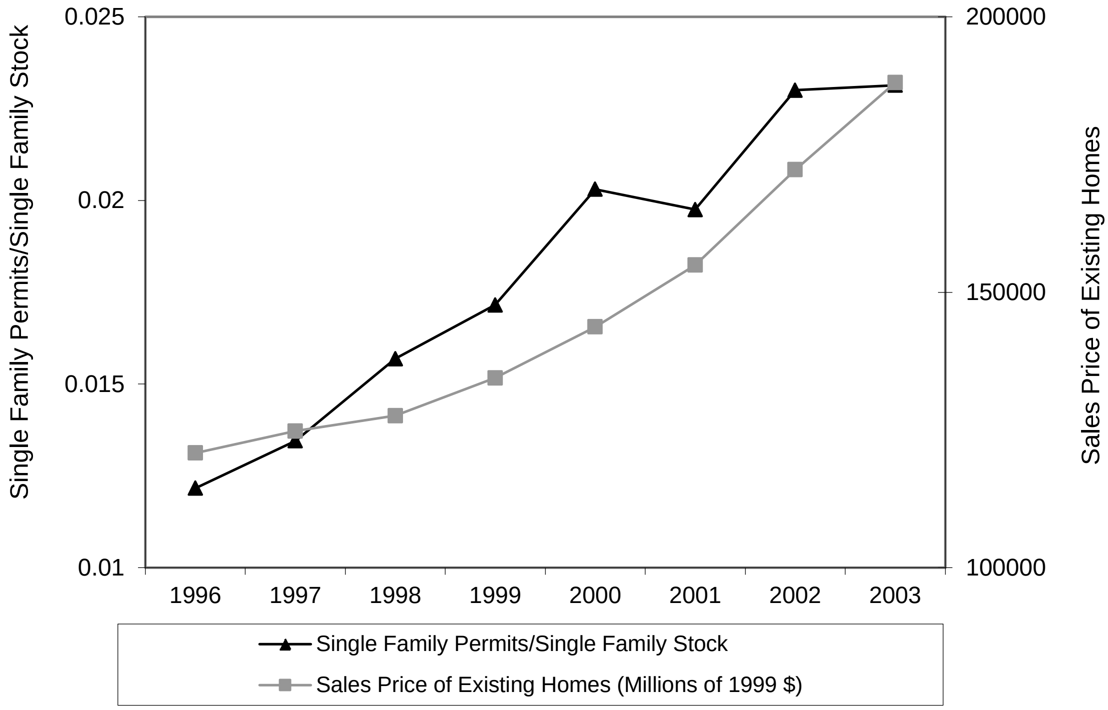
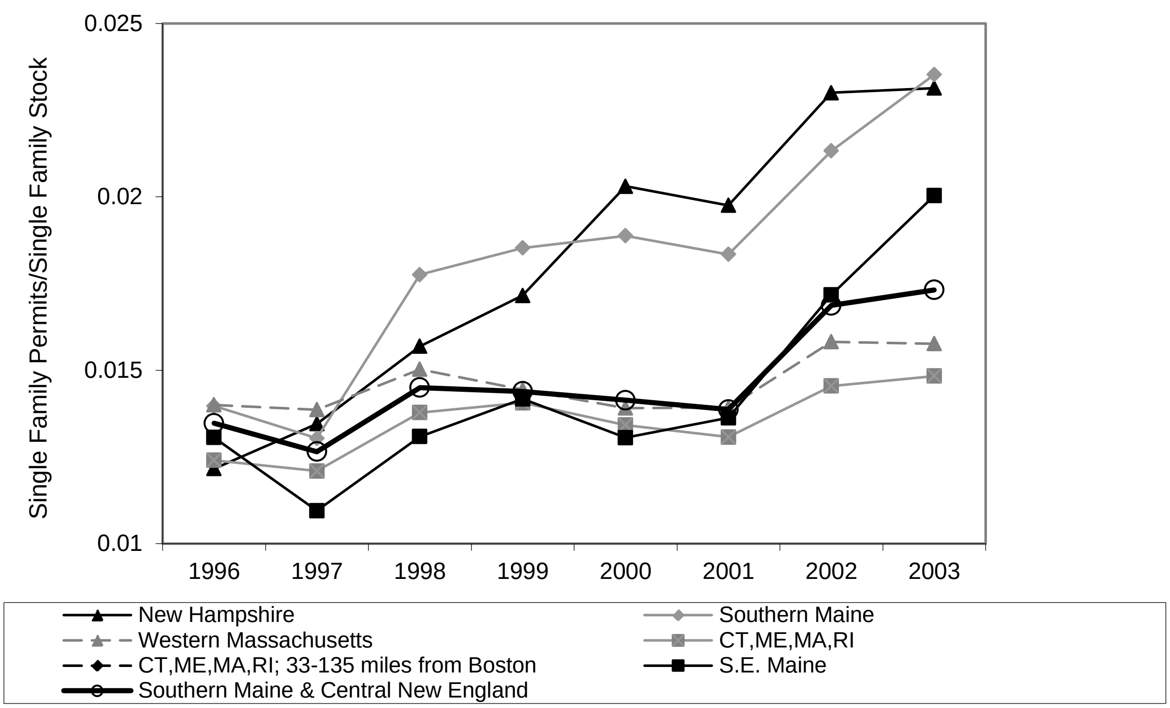
Figure 1: Residential Construction

Panel A: Residential Construction and Sales Price in New Hampshire



Panel B: Residential Construction in the New England Control Groups



Note. The figures display municipality means for the sample of municipalities with at least 1200 residents in 2000 and which form a balanced panel over the period displayed. Building permit data from U.S. Census Bureau. Mean sales data in panel A are obtained from the New Hampshire Housing Finance Authority.

Figure 2: Geographic Distribution of Fiscal Shock

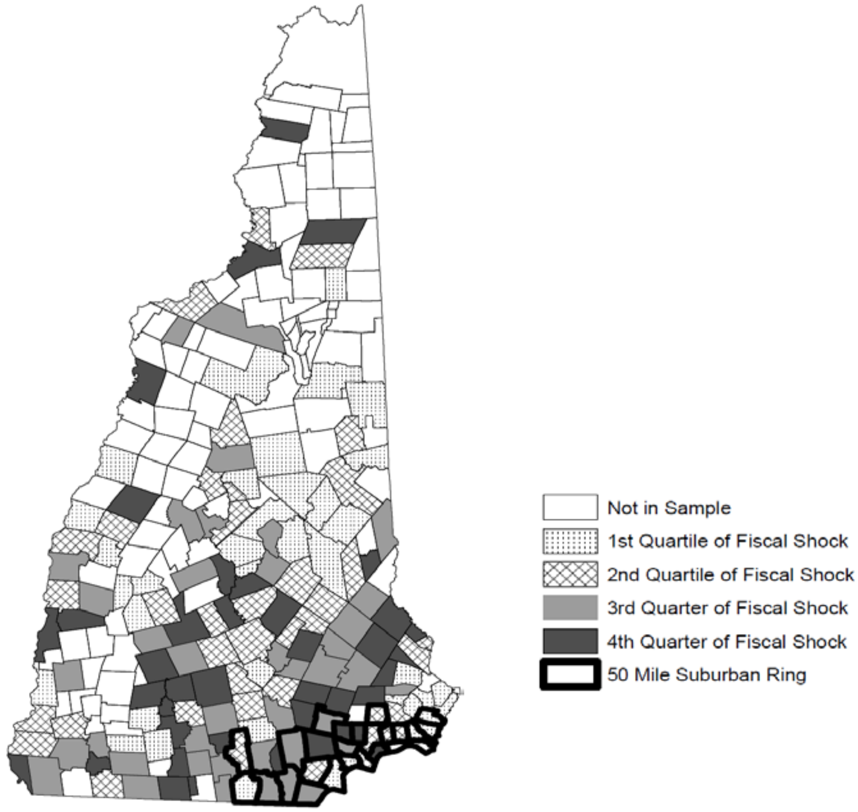
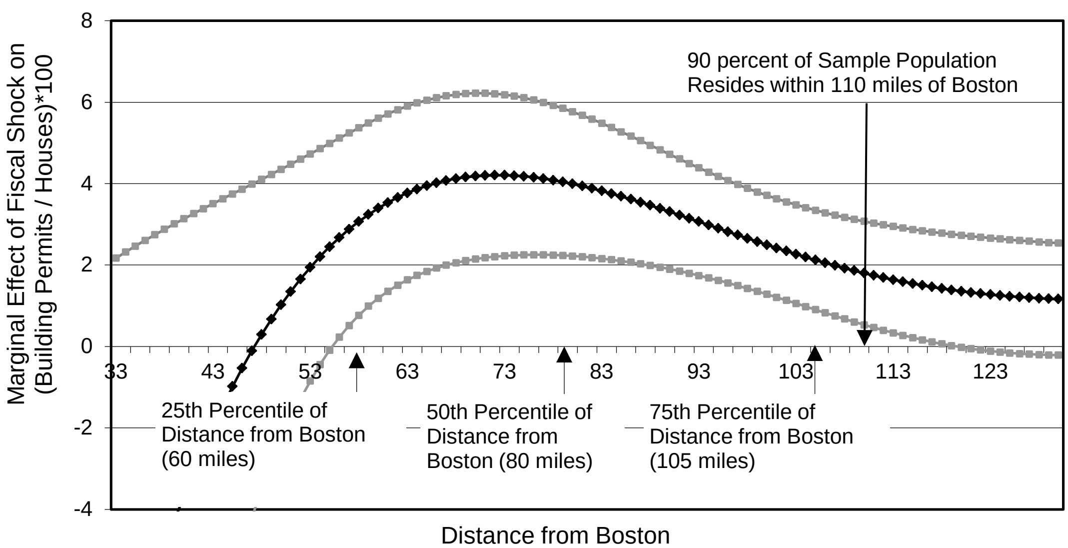


Figure 3: Fiscal Shock Interacted with Quartic in Distance from Boston



Note. The light lines are 95% confidence intervals. The specification includes the interaction of the fiscal shock variable with a quartic in distance from Boston and the interaction of a post-reform indicator variable with a quartic in distance, but is otherwise identical to the specification used in column (4) of Table 2. The figure displays the estimates up to 135 miles from Boston (95% of the sample population lives within this range).

Table 1
Summary Statistics

row #		1998	2000	2002
1	1999 Net Education Grant / 1998 Local Property Tax Revenue	*	0.15	0.15
		*	(0.14)	(0.14)
2	10th Percentile	*	-0.05	-0.05
3	50th Percentile	*	0.17	0.17
4	90th Percentile	*	0.29	0.29
6	Tax Rate Per $1000 of Property	28.7	22.5	20.5
	(conditional on positive Net Reform Grant)	(5.4)	(4.5)	(4.8)
7	Tax Rate Per $1000 of Property	15.1	15.6	13.5
	(conditional on negative Net Reform Grant)	(3.9)	(4.1)	(3.5)
8	Total Tax Payment (millions of 1999 dollars)	10.4	9.4	10.3
	(conditional on positive Net Reform Grant)	(16.6)	(15.0)	(15.9)
9	Total Tax Payment (millions of 1999 dollars)	8.5	9.7	10.7
	(conditional on negative Net Reform Grant)	(9.1)	(9.4)	(10.7)
10	Population	6977	7164	7392
		(11895)	(12141)	(12321)
11	Distance from Boston		86	
			(33)	
12	(Housing Permits / 1996 Housing Stock) * 100	1.6	2.0	2.3
		(1.2)	(1.4)	(1.3)
13	10th Percentile	0.5	0.6	0.7
14	50th Percentile	1.2	1.6	2.3
15	90th Percentile	3.1	3.8	4.0
16	Number of Observations		158	

Note. The cells are municipality means unless stated otherwise. Standard deviations are in parentheses. The sample used to calculate the means is restricted to the set of districts with greater than 1200 residents in 2000 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)	(7)	(8)	(9)
	(Building Permits / Homes) * 100								
(1999 Grant / 1998 Tax Revenue) * (year >= 2000)	1.15		1.58	2.41	1.77	0.44	1.16	1.35	1.19
	(0.56)		(0.72)	(0.59)	(0.59)	(0.84)	(0.60)	(0.79)	(0.55)
(1999 Grant / 1998 Tax Revenue) * (year = 2000)		0.91							
		(0.51)							
(1999 Grant / 1998 Tax Revenue) * (year = 2001)		1.34							
		(0.61)							
(1999 Grant / 1998 Tax Revenue) * (year = 2002)		0.82							
		(0.66)							
(1999 Grant / 1998 Tax Revenue) * (year = 2003)		1.55							
		(0.82)							
(1999 Grant / 1998 Tax Revenue) * (year = 1998)									0.10
									(0.30)
Implied Change in Dep. Var*	.17	*	.35	.35	.26	.06	.17	.20	*
Implied Percent Change in Dep. Var*	.11	*	.22	.22	.16	.04	.11	.12	*
Number of Observations	1109	1109	1374	1109	1109	1109	1768	1768	1109
Municipalities with >= 500 Pop. Included			X						
Base Covariates * Year Indicators				X					
County * Year Indicators					X				
Municipal Linear Trends						X		X	
Sample Date Range : 1992 - 2003							X	X	

Note. Standard errors clustered by municipality are in parentheses. The date range of the data is 1996 to 2003 unless otherwise noted. 1999 is omitted from the sample in all cases (see text). The unit of observation is the municipality-year. The sample is restricted to the set of municipalities with greater than 1200 residents in 2000 with at least six years of non-missing building permit data unless otherwise noted. The dependent variable is the ratio of single-family building permits to the number of single-family homes in 1996 multiplied by 100. All columns include municipal fixed-effects and year fixed-effects. Base covariates, interacted with year terms in column (4), are distance from Boston, distance from Boston squared, municipal population, municipal population squared, the percent of municipal property that is residential, the percent of municipal residential property that is for seasonal or recreational use, and municipal density, defined as the total number of housing units divided by land area. * The implied change in the dependent variable and the implied percent change in the dependent variable are calculated using the mean value of the dependent variable in 1998 (the last pre-reform year) and mean value of the fiscal shock (1999 grant/1998 tax revenue) in 2000 (the first post-reform year).

Table 3
Demographic Characteristics of New England States

	Treatment Group	Control Groups					
	New Hampshire	Southern Maine	Western Mass.	Southeastern Maine	Southeastern & Central New England	CT, ME, MA, RI	CT, ME, MA, RI; 33-135 miles from Boston
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Per Child Residential Property Wealth	170,964	167,042	190,577	163,221	175,265	221,929	213,539
	(83,297)	(111,768)	(75,392)	(70,087)	(66,220)	(137,583)	(127,342)
Median Household Income	51,637	42,763	53,395	41,538	49,771	53,860	54,026
	(12,795)	(8,208)	(13,990)	(06,528)	(12,120)	(19,425)	(13,069)
Percent Beneath Poverty Line	0.06	0.08	0.06	0.09	0.07	0.07	0.06
	(0.03)	(0.04)	(0.04)	(0.03)	(0.05)	(0.05)	(0.04)
Median House Value	129,518	111,410	153,393	107,748	133,462	167,035	158,021
	(42,527)	(31,952)	(48,176)	(25,553)	(35,904)	(94,782)	(53,097)
Percent Non-White	0.03	0.02	0.05	0.02	0.04	0.06	0.06
	(0.01)	(0.01)	(0.06)	(0.01)	(0.06)	(0.08)	(0.08)
Unemployment Rate	0.04	0.04	0.04	0.05	0.04	0.05	0.04
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)
Percent of Houses for Recreation Use	0.12	0.16	0.05	0.13	0.07	0.08	0.07
	(0.15)	(0.15)	(0.09)	(0.15)	(0.11)	(0.12)	(0.13)
Single-Family Homes	2,078	1,999	2,912	1,514	2,924	3,930	4,183
	(2,443)	(1,822)	(3,750)	(1,249)	(3,516)	(4,256)	(4,088)
Population	7,268	6,252	11,673	5,017	11,259	15,834	15,487
	(12,165)	(8,141)	(20,477)	(6,410)	(17,620)	(29,824)	(21,154)
Distance to Boston	85	128	84	148	107	113	85
	(34)	(32)	(34)	(27)	(44)	(53)	(29)
25th percentile	60	106	54	142	65	53	60
median	80	133	80	152	106	102	87
75th percentile	105	152	109	161	150	147	110
Number of Observations	159	111	146	42	109	745	396

Note. The cells display municipality means taken from the 2000 Census. Standard deviations in parentheses. The sample is restricted to the set of municipalities with greater than 1200 residents in 2000 with at least six years of non-missing building permit data. *See Appendix Figure A2 and the Data Appendix for precise definitions of the control groups.

Table 4

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

	No Control Group	Control Groups											
		Southern Maine		Western Massachusetts		Southeastern Maine		Southeastern Maine & Central New England		CT, ME, MA, RI		CT, ME, MA, RI; 33-135 miles from Boston	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
	(Building Permits / Homes) * 100												
(Grant / Tax Revenue) * (year >= 2000)	0.45	1.08	1.75	2.42	2.01	1.49	2.56	1.96	2.31	2.30	1.57	2.26	1.60
	(0.87)	(0.54)	(0.93)	(0.61)	(0.92)	(0.65)	(1.05)	(0.60)	(0.95)	(0.50)	(0.79)	(0.52)	(0.81)
Implied Change in Dep. Var*	.07	.16	.26	.35	.29	.22	.33	.29	.34	.34	.23	.33	.23
Implied Percent Change in Dep. Var*	.04	.10	.16	.23	.19	.14	.21	.18	.21	.21	.15	.21	.15
Number of Observations	1109	1884	1884	2129	2129	1402	1402	1871	1871	6309	6309	3874	3874
Grant Predictor * (year >= 2000)	X	X	X	X	X	X	X	X	X	X	X	X	X
Municipal Linear Trends			X		X		X		X		X		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 1996 multiplied by 100. Standard errors clustered by municipality are in parentheses. The date range of the data is 1996 to 2003, with 1999 omitted from the sample (see text). The sample is restricted to the set of municipalities with greater than 1200 residents in 2000 with at least six years of non-missing building permit data. All columns include municipal and year fixed-effects and a control for per-child residential housing wealth interacted with a post-reform indicator variable. See the Data Appendix and Appendix Figure A2 for precise definitions of the control groups. * 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 the fiscal shock (grant / tax revenue).

Table 5
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	0.144	0.148
	(0.099)	(0.144)
(1998 Building Permits / 1996 Housing Stock) * 100	2.7	1.4
	(1.9)	(1.0)
Population	13353	6215
	(18925)	(10520)
Any Land in Urbanized Area	0.95	0.15
	(0.22)	(0.35)
Total Housing Unit Density in 1996	84.6	34.8
	(99.3)	(56.0)
Percent of Land Developed	0.20	0.07
	(0.16)	(0.10)
Growth Management Ordinance in 1999	0.33	0.10
	(0.48)	(0.30)
Number of Observations	21	137

Note. The cells are municipality means. Standard deviations are in parentheses. The sample is restricted to municipalities with greater than or equal to 1200 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 1,000,000.

Table 6

Effect of Change in Fiscal Surplus on Residential Investment: Heterogeneity by Distance from Boston

	(1)	(2)	(3)	(4)
	(Building Permits / Homes) * 100			
(Grant / Tax Revenue) * (year >= 2000) * (> 50 miles from Boston)	1.48	2.98	2.29	1.31
	(0.54)	(0.58)	(0.57)	(0.74)
(Grant / Tax Revenue) * (year >= 2000) * (<= 50 miles from Boston)	-3.40	-3.82	-3.52	-10.06
	(2.75)	(2.89)	(2.79)	(6.10)
Implied Change in Dep. Var > 50 miles from Boston	.22	.44	.34	.19
Implied Percent Change in Dep. Var > 50 miles from Boston	.16	.31	.24	.14
P-value for test: Effect of Fiscal Surplus Equal In and Out of 50-mile Suburban Ring	.08	.02	.04	.07
Number of Observations	1109	1109	1109	1109
Base Covariates * Year Indicators		X		
County * 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 of single-family building permits to the number of single-family homes in 1996 multiplied by 100. Standard errors clustered by municipality are in parentheses. The date range of the data is 1996 to 2003, with 1999 omitted from the sample (see text). The sample is restricted to the set of municipalities with greater than 1200 residents in 2000 with at least six years of non-missing building permit data. All columns include municipal and year fixed-effects. Column (2) includes 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 percent of municipal property that is residential, the percent of municipal residential property that is for seasonal or recreation use, and municipal density, defined as the total number of housing units divided by land area. All columns include main interaction effects: For example, column (1) includes: (> 50 miles from Boston) * (year >= 2000) and (<= 50 miles from Boston)*(year >= 2000). The coefficient estimates for these main effects are not shown due to space limitations. Tables displaying the complete set of coefficients are available from the author upon request.

Table 7

Effect of Change in Fiscal Surplus on Property Values

	Log(Mean Sales Value of Existing Homes)				Log(Market Value of Taxable Property)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(Grant / Tax Revenue)	0.13				0.09			
	(0.07)				(0.07)			
(Grant / Tax Revenue) * (year >= 2000) * (<= 50 miles from Boston)		0.33	0.34	0.34		0.43	0.45	0.46
		(0.12)	(0.15)	(0.13)		(0.15)	(0.19)	(0.16)
(Grant / Tax Revenue) * (year >= 2000) * (> 50 miles from Boston)		0.12	-0.02	0.00		0.07	0.10	0.04
		(0.08)	(0.08)	(0.08)		(0.07)	(0.06)	(0.05)
Implied Percent Change in Property Values: <= 50 Miles from Boston	*	0.05	0.05	0.05	*	0.06	0.06	0.06
P-value for test: Effect of Fiscal Surplus Equal In and Outside of 50-mile Ring	*	0.15	0.03	0.03	*	0.03	0.07	0.01
Number of Observations	1113	1113	1113	1113	1113	1113	1113	1113
Base Covariates * Year Indicators			X				X	
County * Year Indicators				X				X

Note. See notes to Table 2 for columns (1) and (5) and the notes to Table 6 for columns (2)-(4) and (6)-(8). The only difference from Tables 2 and 6 is the dependent variable, which is displayed in the column headers.

Table 8

Effect of Change in Fiscal Surplus on Land Use Regulation

	Growth Management			Impact Fees		
	(1)	(2)	(3)	(4)	(5)	(6)
(Grant / Tax Revenue) * (year = 2008)	0.42	0.44	0.38	0.01	0.10	-0.14
	(0.14)	(0.19)	(0.15)	(0.26)	(0.32)	(0.27)
Implied Change in Dep. Var	0.06	0.06	0.06	0.00	0.02	-0.02
Number of Observations	330	330	330	330	330	330
Base Covariates * Year Indicators		X			X	
County * Year Indicators			X			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. All columns use a two-period panel. Columns (1) - (3) use data from years 1999 and 2008. Columns (4) - (6) use data from years 2000 and 2008. The dependent variable is an indicator variable for the form of land use regulation given in the column header. Standard errors clustered by municipality are in parentheses. The sample is restricted to the set of municipalities with greater than 1200 residents in 2000. All columns include municipal and year fixed-effects. See the note to Table 2 for information on the base covariates used in columns (2) and (5).

NOT FOR PUBLICATION:
Appendix to Quasi-Experimental Evidence on The Connection Between Property Taxes and Residential Capital Investment

Byron Lutz

January 13, 2014

1 Appendix

1.1 Data Appendix

Municipal and School District Variables

Total property tax payments, $ptax_{m,t}$, contain both school and municipal property tax collections. Municipal property tax rates are set by the individual towns. School tax rates are set by the citizens of the town(s) which comprise the school district. In most cases towns and school districts have coterminous boundaries, although some towns participate in cooperative school districts composed of two or more municipalities. For cooperative districts, the school property tax payments are mapped from the school district level to the municipal level following the convention used by the New Hampshire Department of Education: Each town's school district taxes are assumed equal to the percentage of district enrollment the town accounts for times total school district taxes.

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 issued 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 (specifically, there appears to be longitudinal inconsistency in how the contribution of the power plant to Seabrook's tax base and tax revenue is handled). This is a unique situation unrelated to the school finance reform. Two municipalities participating in 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.

New England Control Group Definitions

The Southern Maine control group is defined as all Maine communities within 175 driving miles of Boston – over 99 percent of the New Hampshire sample lives within this radius. The Western Massachusetts control group is defined as all Massachusetts communities greater than 33 miles from Boston – the New Hampshire community closest to Boston is located 33 miles from the city – and west of 71.3837 W. The Southeastern Maine control group is defined as communities east of 70.1970 W and within 175 miles of Boston. Central New England is defined as communities in Massachusetts south of 42.1497 N, west of 71.3837 W and located 33 miles or more from Boston and communities in Rhode Island and Connecticut north of 41.8645 N. The control group including all Connecticut, Massachusetts, Maine and Rhode Island communities between 33 and 135 miles from Boston was constructed to be as similar as possible to New Hampshire in terms of distance from Boston. The 33 mile cutoff reflects that fact that the New Hampshire town closest to Boston is 33 miles away. The 135 mile cutoff was chosen such that the mean distance in the control group exactly matches the mean distance in New Hampshire. This control group is also broadly similar to New Hampshire in terms of standard deviation and distribution.

Distance from Boston

The distance between a municipality and Boston is defined in terms of driving distance. For New Hampshire, the data is obtained from the New Hampshire Economic and Labor Market Information Bureau. For the New England control group states, the data is produced using the code developed by Ozimek and Miles (2011).[1] The code inputs two pairs of longitude and latitude coordinates and then queries Google Maps for the driving distance. Within New Hampshire, the Ozimek and Miles produced data is extremely similar to the data produced by the New Hampshire Economic and Labor Market Information Bureau. The measures have a raw cross-sectional correlation of 0.99 and the mean difference between the two measures is -1.6 miles, a small fraction of the 85 mile mean distance in the government produced data.

Measures of Land Availability

Two measures of land availability are used. The first measure is the number of houses per square meter of land from the 2000 Census. The second measure is the percent of land which has been developed. Land is considered developed if it is in use for residential, commercial, industrial or transportation purposes. Land is considered undevelopable, and therefore not included in the denominator of the measure, if it is classified as any of the following: open water, perennial ice or snow, barren, or wetlands. The data was produced by Hilber and Mayer (2009) and is based on

[1] Ozimek, A. and D. Miles, "Stata utilities for geocoding and generating travel time and travel distance information" *Stata Journal*, Volume 11, No. 1, 2011.

the National Land Cover Data 1992. In some cases, the data cannot be mapped uniquely into a single municipality. In these cases, the data are mapped into an area comprised of two or more municipalities and these communities are all assigned the same value.

Land Use Regulation

Only two forms of land use regulation are available at multiple points in time – growth management and impact fees. These variables are used in the regressions in Table 8. Impact fees are paid at the time of development and are intended to cover the cost of the public infrastructure associated with development. The impact fee data are available as of 2000 in a dataset on New Hampshire zoning collected by Richard England. The 2008 data come from the New Hampshire Office of Energy and Planning. Impact fees are the only aspect of zoning which can be linked across the two sources / time periods. The growth management data are available for 1999 and 2008. The data come partially from a survey conducted by the author and the remaining data are from the Office of Energy and Planning.

Appendix Table A1[2]

Percent of Municipalities with Land Use Regulation

	1999/2000	2008
Growth Management	0.13	0.26
Impact Fees	0.13	0.44

1.2 Heterogeneity in the Investment Response Due to Land Availability and Land Use Regulation

As discussed in section 6.2 of the text, the geographic heterogeneity in the investment response to the reform is consistent with the hypothesis that housing supply elasticity is relatively higher outside of the suburban ring. Likely explanations for this hypothesized difference in supply elasticity are land availability and regulation. These explanations are explored in Appendix Table A2. Column (1) contains results examining land availability, using an interaction between the grant variable and housing market density – a proxy for land availability. Columns (2) and (3) explore land use regulation. The specification in column (2) includes an interaction between the grant variable and an indicator variable for having a growth management ordinance – a type of land use regulation which sets a binding annual limit on the number of new homes constructed. The interaction

[2] Note. The sample is restricted to municipalities with 1200 or more residents in 2000. The first column contains data from 1999 for growth management and from 2000 for impact fees.

term in column (3) uses an indicator variable for impact fees – payments required of developers to defray the municipal cost of new housing (e.g. new school construction). See the note to Table A2 for further information on these specifications. In all three cases the interaction terms are imprecise and hence uninformative.[3] Unlike the availability of land, which can be measured relatively accurately, land use regulation is notoriously difficult to quantify (e.g. Nechyba 2001 and Glaeser, Gyourko and Saks 2005). It is therefore possible that the measures used in columns (2) and (3) fail to fully characterize the regulatory environment.

1.3 The Intensive Margin of Residential Capital Investment

The measure of the residential capital investment used in this paper—the number of new housing units—captures only the *extensive* margin of residential investment. A shock to fiscal surplus may also influence the *intensive* margin of residential investment—the size and quality of both new and existing homes. For instance, with the annual tax cost of renovations and additions falling due to a decline in the property tax rate, homeowners would be expected to increase expenditures on renovations and additions.

Despite the evidence that there is no extensive margin response within the suburban ring, there may very well have been an intensive margin response in this area. The factors which make housing supply inelastic—e.g. zoning, the availability of land, etc.—and which block extensive margin residential investment from responding to a fiscal shock, may not block intensive margin investments such as improvements or additions to existing homes. Intensive margin investment potentially increases the sales value of a property. As a result, the suburban ring "price" response documented in Table 7 may partially reflect a "quantity" response, specifically an intensive margin investment response.

Unfortunately, data on intensive margin residential investment for existing homes are not available for New Hampshire's municipalities and it is therefore not possible to directly estimate the effect of the fiscal reform on this margin. It is possible, however, to use data at a coarser geographic level to gain an understanding of the likely magnitude of the intensive margin response. The Census Bureau's Survey of Residential Alterations and Repairs (SORAR) shows that nearly $30 billion (1999 dollars) was spent on residential home improvements in the Northeast census region in 2000. The definition of home improvements is broad and includes all construction activity intended to maintain or improve residential property (e.g. additions, alterations, repairs, etc.). The 2000 decennial census counts around 22.6 million residential housing units in the Northeast region. Thus,

[3] Specifications using other measures of land availability, such as the percent of land developed, and other measures of land use regulation, such as elements of municipal zoning codes, also produce uninformative results (unreported).

average intensive margin residential investment equaled roughly $1,325 in the region in 2000.

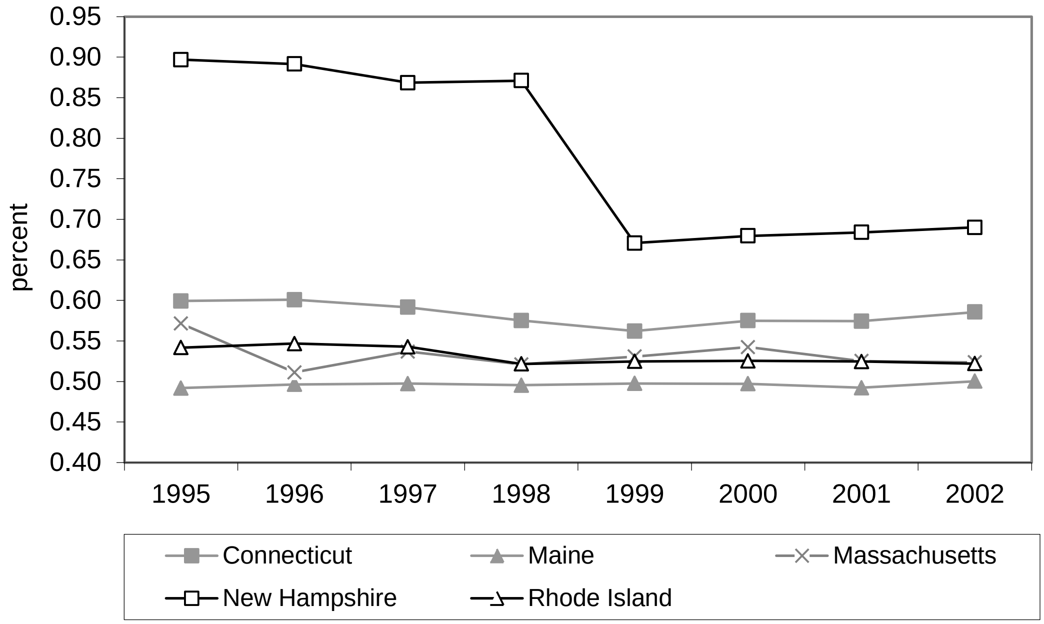
Assume that the fiscal shock increased intensive margin residential investment by 16 percent — equal to the extensive margin investment response outside of the suburban ring (see section 3.4.3). Under this assumption, the fiscal shock increased intensive margin investment by around $212 per year in the suburban ring ($1,325*0.16). Over the four post-shock years used in the sample, this would be expected to increase the value of a home by an average of $530.[4] Within the suburban ring, the fiscal shock increased home values by 5% (see section 3.4.4). As the mean sales price of existing homes in the suburban ring over the four post-shock years is around $237,000, the typical suburban ring home experienced an increase in value of roughly $11,850 ($237,000*0.05). Thus, these calculations suggest that the intensive margin investment response accounts for roughly $4\frac{1}{2}$ percent ($530/$11,850) of the overall suburban ring price response documented in Table 7.

The intensive margin response calculation is extremely rough and subject to any number of possible critiques. Nonetheless, the calculation suggests that, under plausible assumptions, the intensive margin response is unlikely to account for a significant portion of the suburban ring price response. For instance, even if the intensive margin response was assumed to be double the magnitude of the extensive margin response (32 percent instead of 16 percent), the intensive margin response would still account for only 9 percent of the price response documented in Table 7.

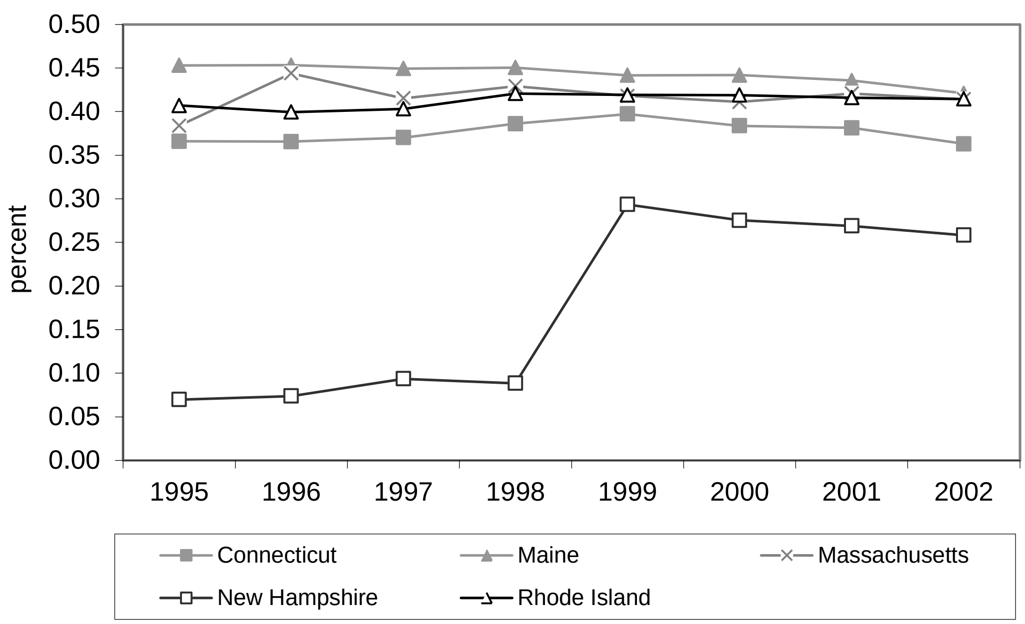
[4] The first year increase in value is $212; the second year cumulative increase in value is $424; the third year cumulative increase in value is $636; the fourth year cumulative increase in value is $848. The mean of these incremental increases in value is $530.

Figure A1: 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 A2: Monocentric Map of New England

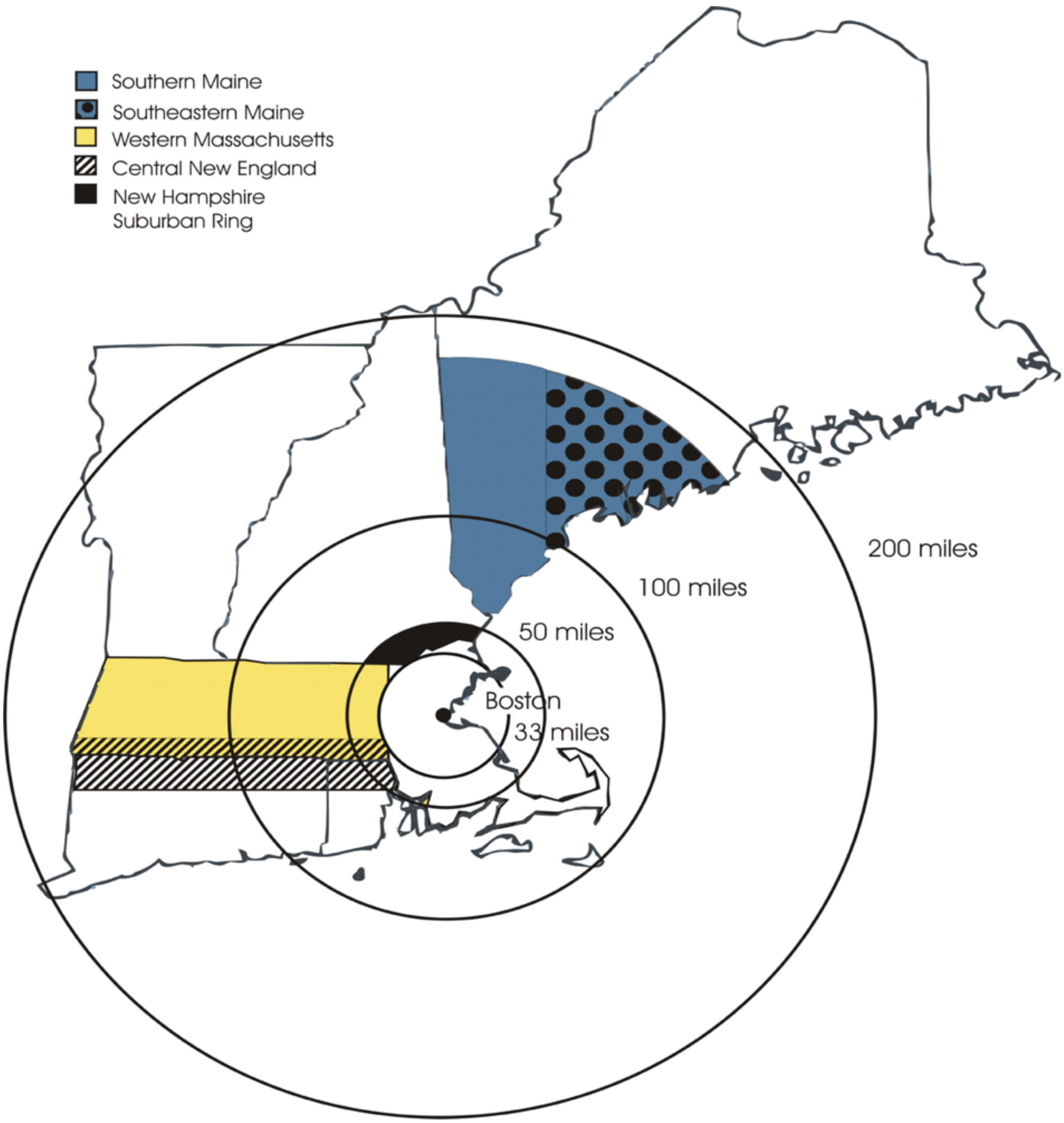


Table A2

Effect of Change in Fiscal Surplus on Residential Investment: Land Availability and Land Regulation

	Land Availability	Land Use Regulation	
	(1)	(2)	(3)
	(Building Permits / Homes) * 100		
(Grant / Tax Revenue) * (year >= 2000)	1.41	1.17	1.09
	(1.57)	(0.56)	(0.53)
(Grant / Tax Revenue) * (Log of Housing Density in 1996) * (year >= 2000)	-0.12		
	(0.49)		
(Grant / Tax Revenue) * (Growth Management in 1999) * (year >= 2000)		-0.50	
		(1.85)	
(Grant / Tax Revenue) * (Impact Fees in 1999) * (year >= 2000)			1.55
			(3.32)
Number of Observations	1109	1109	1109

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 1996 multiplied by 100. Standard errors clustered by municipality are in parentheses. The date range of the data is 1996 to 2003, with 1999 omitted from the sample (see text). The sample is restricted to the set of municipalities with greater than 1200 residents in 2000 with at least six years of non-missing building permit data. All columns include municipal and year fixed-effects. All columns include main interaction effects: For example, column (1) includes in the specification (log of housing density) * (year >= 2000). The coefficient estimates for these main effects are not displayed. Full set of results available from author upon request.