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THREE ESSAYS ON THE LINKS BETWEEN LOCAL GOVERNMENT STRUCTURAL CHANGES AND PUBLIC FINANCE

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Abstract

This dissertation, comprised of three essays, explains and evaluates local government structural changes from a public finance perspective. The first essay examines the determinants of the rapid growth of special districts, while the next two essays estimate the effects on property values of school district consolidation and village dissolution in New York State, respectively. Together, the three essays contribute to our understanding of the causes and consequences of local government structural changes in the United States.

By bring together two central trends in state and local public finance, namely, the expansion of state-imposed tax and expenditure limitations (TELs) and the rapid growth of special districts, the first essay looks into the hypothesis that TELs are partly responsible for the increase of special districts over the last several decades. To eliminate the possible omitted variable bias, I employ a combination of fixed effects, regional time trends and approximate measures of fiscal conservativeness. Based on a national data set of counties over the period 1972-2007, I find TELs, on average, increase the use of special districts (circumvention effects), whereas TELs don't force local governments to cut their intergovernmental fiscal transfer to special districts in the same county area (deterrent effects). The estimation results are robust to multiple tests of common trends assumptions, five alternative measures of TELs, alternative model specifications and different empirical strategies. This results confirm the theory that special districts have been extensively created by local general-purpose governments as an institutional strategy to circumvent the fiscal constraints imposed by TELs.

The second essay explores the impacts of school district consolidation on property values in upstate New York from 2000 to 2012. This research, conducted in collaboration with Professors

William Duncombe and John Yinger, adds a time dimension to research on the property-value impacts of consolidation. By combining propensity score matching and double-sales data to compare house value changes in consolidating and comparable school districts, we find that it takes time either for the advantages of consolidation to be apparent to homebuyers or for the people who prefer consolidated districts to move in. In addition, the long-run impacts of consolidation on house values are positive in low-income census tracts but negative in high-income census tracts. This result suggests that high-income households are particularly attached to the benefits, such as close contact with teachers, of small districts.

Streams of institutional, economic and fiscal factors recently have been converging and substantially changing the landscape of local government in the United States. Dissolution, an old and new approach, has increasingly been used and therefore drawn much public attention nowadays. The third essay provides the first study investigating whether village dissolution, as a form of general-purpose government reorganization, affects the attractiveness of local communities. In New York, voters in several villages voted to dissolve the village and hence to shift all government services to the town government in which the village is located. I show that village dissolution does not alter the amount people are willing to pay inside the (eliminated) village boundaries, but that the price of housing declines in areas of the town outside the village (TOV). Presumably, residents in the TOV areas are upset with the negative externalities of village dissolution.

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Introduction

American fiscal federalism has dramatically changed at local level, and the change is clearly reflected through three stylized facts. First, the number of special districts has increased more than 100 percent over the last several decades, and the combined expenditures of single-function jurisdictions, which include 35,000 special districts and 13,500 school districts, exceeded the combined expenditures of all municipalities (Berry, 2009). Second, the number of school districts has been cut by almost 90 percent over the past century (National Center for Education Statistics (NCES) 2006, 2011), and many states still contain several school districts with enrollment below 500 pupils, which are good candidates for consolidation. Third, more than 50 villages have studied the feasibility of dissolution in New York State and 12 of them succeeded in voting for dissolution.

When a new government emerges or an existing government ceases, local residents' tax burden will be directly influenced. The quantity and quality of public services will also be affected. The extent to which local residents' interests could be well represented, local identity, the bargaining power of residents, and the link between public services and their payments will accordingly change as well. These factors are associated with two key concerns in fiscal federalism studies, one is the trade-off between allocative efficiency and productive efficiency, and the other is political accountability. Take school district consolidation as an example. The principal justification for consolidation is economies of scale; however, as shown in the literature and also in my dissertation, allocative efficiency may be sacrificed for high-income parents, as they put much more value on parental access to teachers in small school districts. Another example is special districts, which have extensively issued debt and borrowed money in a

"backdoor" approach, putting into question the political accountability of special districts (New York State Office of the State Comptroller, 2005).

Thus, a fundamental research task is to identify the real determinants and to assess the effects of those local government structural changes. Based on the three above-mentioned stylized facts, this dissertation seeks to examine the cause and consequence of those changes from a public finance perspective. Basically, the first essay investigates whether TELs incentivize local governments to create more special districts to provide public services. The other two essays, based on property sales data, use the change of property values to assess the overall impact of school district consolidation and village dissolution, respectively. The logic behind this capitalization approach is property values comprehensively, although indirectly, capture the impact of any improvements affected by government reorganization. It is noteworthy that the focus of these two studies is not to estimate the impact of government reorganization on people's average willingness to pay. Instead, the purpose is simply to look for a sign whether people care about government reorganization and how much home buyers value those government reorganizations. So, in this narrow sense, using capitalization approach to assess the overall impact of government reorganization is reasonably appealing.

The rapid increase of special districts and the quick expansion of state-imposed TELs are the two central trends in state and local public finance since the 1970s. However, there is a big legal loophole in the TEL laws, namely, special districts are commonly exempted from TELs. The temporal coincidence of those two trends and the existence of the loophole lead to a testable hypothesis whether TELs could partly explain the rapid growth of special districts. The existing literature, however, fails to achieve a consensus on this topic. To fill the gap, I adopt four TEL

indicators and a TEL index to measure the fiscal stringency of TELs. In addition, because every state or region has a distinctive political and fiscal culture, and because fiscal conservativeness may simultaneously affect the adoption of TELs and local governments' fiscal behaviors (Rueben, 1997), I employ a combination of fixed effects, regional time trends, and two approximate measures of fiscal conservativeness to minimize the potential bias in the estimate of the impact of TELs on the number of special districts. To avoid the possibility that the results are driven by other time-varying factors, I add a vector of state-specific linear time trends to the model but find no substantial change of the coefficient of TELs. I also examine the use of special districts not only after the adoption of TELs but also before the adoption of TEL laws. Significant changes in special districts are not found in the years before the adoption of TELs, either. These two tests justify the common trend assumption and warrant the validity of the main model. The final estimation results demonstrate that TELs significantly stimulate local governments to create more special districts. In other words, the results lends solid support to politics of circumvention theory, suggesting that local governments seek to circumvent fiscal constraints caused by TELs through creating more special districts to finance public services. In addition, combining the empirical findings in my dissertation with those in existing studies, we should be aware that the "entrepreneurship" of local government may help to strike a balance between the demand for public services and the political and fiscal pressure from the public, on the one hand; it may complicate the local governance system and blur, or even increase the true cost of public services, on the other hand.

Seven school districts were consolidated into three between 2000 and 2012 in New York State. Based on a sample of repeat sales, the second essay, which is conducted in collaboration with Professors Duncombe and Yinger, responds to two questions. First, what is the impact of

school district consolidation on property values? Second, are there heterogeneities in property value impacts of consolidation across different income groups and pre-consolidation enrollment size? Considering that the impact of consolidation may depend on many observable school districts characteristics and house traits, we use Propensity Score Matching to reduce covariate bias between the treated and control groups. Then, we use traditional regression methods with difference-in-difference (D-D) design to adjust for residual bias caused by unobserved variables and to increase estimation efficiency. Basically, we find, except in one relatively large district in the Albany area, consolidation has a negative impact on house values during the years right after it occurs and that this effect then fades away and is eventually reversed. This pattern suggests that it takes time either for the advantages of consolidation to be apparent or for the people who prefer consolidated districts to move in. In addition, we find the long-run impacts of consolidation on house values are positive in census tracts that initially have low incomes, but negative in high-income census tracts, where parents may have a relatively large willingness to retain the non-budgetary advantages of small districts. We also estimate models that include interactions with both the dummy for the large school district and initial census-tract income. As the large school district has a relatively high income, however, we were unable to separate the impact on this pattern of initial district enrollment and of initial neighborhood income. These are topics for future research. Finally, since relatively few districts in New York consolidate, districts that do consolidate are not likely to be typical and representative of all other big or small districts in the state. Thus, this study should be regarded as a case study about the impact on house values of three specific consolidations, not as a study about the impact of consolidation in general.

The last chapter targets on village dissolution. Currently, village governments are confronted with a variety of political, economic, and fiscal challenges in New York State, including the outdated municipal class designation, contentious inter-governmental fiscal relationships, increasing budget constraints, and the fragmented property tax assessing system. Consequently, dissolution has been widely adopted by village residents as a way to reduce fiscal burden and eliminate duplicative services. New York State government has even provided a Local Government Efficiency Grant to facilitate village dissolution. So, the first question I ask in Chapter 3 is whether village dissolutions affect property values in former village areas. Most villages are located in a town area and the town government will automatically be responsible for providing public services after the village is dissolved. That means, town residents may have to face negative/possible externalities resulting from village dissolution, as their public service quality and cost could be subsequently changed. Town residents, however, are excluded from the decision-making process of village dissolution. This unique institutional arrangement raises another interesting question: do village dissolutions affect property values in town outside village (TOV) areas? The lack of sufficient, straddling, and representative property sales make it impossible for me to employ a combination of propensity score matching and difference-in-difference regression, as I do in Chapter 2. However, a sample of repeat sales still allows me to get unbiased estimation results when controlling for parcel-level fixed effects and village-specific time trends and excluding non-comparable observations from dissimilar geographical regions. Basically, I find dissolution events do not bother property values in former village areas, presumably special districts are designed to replace former village governments in providing basic public services. In the future, more information is needed about special districts and the change of public service cost in village areas to confirm and enrich my empirical findings.

Dissolution events, however, do decrease property values in TOV areas, owing to unavoidable cost shifts and possibly other negative spillover effects to TOV areas.

Overall, my dissertation sheds light on the fiscal determinants of special districts and provides a comprehensive, though indirect, evaluation of village dissolution and school district consolidation. It also makes direct and indirect contributions to perennial debates on local government size, revenue and expenditure assignments, and intergovernmental fiscal relations in a federal system.

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Chapter 1:

The Unintended Impact of Tax and Expenditure Limitations on the Use of Special Districts: The Politics of Circumvention

1.1. Introduction

The past half century in the United States has witnessed a dramatic change in local governance and local fiscal resource distribution. Specifically, state and local governments are increasingly choosing to spin-off special districts to finance and provide public services (Bourdeaux, 2005). According to the Census Bureau, the number of special districts has increased more than 100 percent from 18,323 in 1962 to 37,203 in 2012[1]; and, the combined expenditures of single-function jurisdictions, which include special districts and school districts, exceeded the combined expenditures of all municipalities (Berry, 2009). Special districts have a variety of government functions, including financing public infrastructure[2], natural resources, utilities, sewage, water supply, housing, and soil conservation among others, most of which are crucially important to maintaining the quality of local public service and the attractiveness of the local business investment environment. For example, every county in New York State has a state-designated "Industrial Development Agency" (IDA) that issues non-guaranteed debt, typically for economic development purposes.

[1] School districts are not special districts, although they are also defined as one type of special-purpose governments. The historical change in the number of school districts in the U.S. mainly reflects economies of scale and other considerations, which are fundamentally different from the rationale behind the growth of special districts. So, following the traditional definition in the Census of Government Dataset, the analysis in this paper excludes school districts.

[2] It is a common misconception that special districts exist primarily to finance the construction of infrastructure and public works. As clearly shown in Berry (2009, P35, Table 2.3), most special districts are direct or indirect service providers.

Although special districts are important in providing public services, stimulating community development, and enhancing allocative efficiency[3], they are denounced as a source of diseconomies of scale and wasteful duplication of services (ACIR, 1964). In addition to these problems discussed in the literature on the horizontal dimension of fiscal federalism, recent studies on the vertical dimension of fiscal federalism suggest that the vertical layering of fragmented special districts causes aggregate public budgets to be larger than when the same services are provided by a single general-purpose government. Stated differently, they find that concurrent taxation causes significant increases in local taxes and spending, as predicted by the fiscal common-pool theory (Berry, 2009).

Special districts, often labeled "shadow governments" or "ad-hoc governments," are also found to be politically invisible. Particularly, the governing boards of many special districts are either elected with extremely low turnout rates or appointed by local parent government officers. Although there is no national data on the turnout rate in all special districts' elections, scattered data suggests that the turnout rates in special districts are extremely low in Nassau County in New York, California, Michigan, and Cook County in Illinois (Suozzi, 2007; Weimer, 2001; Little Hoover Commission, 2000; Berry, 2009). In addition, the timing of elections for special districts are often not the same day as general elections, increasing the participation cost and significantly discouraging voters who are least concerned with the special districts' policy issues (Dunne et al., 1997; and Berry, 2009). The political invisibility of special districts leads to the concern that they mainly serve particular interest groups and/or appointing authorities and calls

[3] As argued by scholars of public choice schools and proponents of "polycentricity" (Tullock, 1969; Ostrom et al., 1988), special districts are likely to increase inter-jurisdictional competition.

their political and fiscal accountability into question (Bollens, 1957; Axelrod, 1992; Foster, 1997; and Radford, 2013).

Given that special districts raise a series of concerns on the efficiency and accountability of the fragmented local governance system in the U.S., it is important to investigate what accounts for the growth of special districts over the past several decades. One of the largest bodies of scholarship on special districts explores their association with state-imposed TELs precisely because of the following facts: (1) special districts are commonly exempted from Tax and Expenditure Limitations (TELs), which are imposed by a state government on its local governments, and (2) both special districts and TELs have expanded rapidly since the 1970s. Theoretically, TELs could either negatively or positively affect the use of special districts. On the one hand, TELs perhaps undermine the fiscal capacity of local governments, who may in turn, as a way to address the imposed fiscal pressure, cut their fiscal aid to cooperated special districts in corresponding jurisdictions. In other words, local general-purpose governments could pass the fiscal constraints imposed by TELs over to special districts, preventing the growth of special districts (i.e., deterrent effect). On the other hand, local general-purpose governments[4] may use special districts as an institutional strategy to circumvent the fiscal constraints of state-imposed TELs, leading to a positive relationship (i.e., circumvention effect). The existing literature, however, fails to conceptually differentiate the two channels and empirically achieve a consensus on the estimation sign and the magnitude of the effects that TELs have on special districts.

[4] Local general-purpose governments and local governments are interchangeably used in this paper.

To fill the gap in the literature, I built a longitudinal government dataset at the county level from 1972 to 2007, and I exploit a wide degree in variation of TEL laws and the number of special districts across states and counties. I adopt four TEL indicators and a TEL index to measure the fiscal stringency of TELs and employ a combination of fixed effects, regional time trends, and approximate measures of fiscal conservativeness to eliminate the bias in the estimate of the impact of TELs on special districts. The empirical results lend strong support to circumvention effects and refute the possible deterrent effects. Specifically, I find TELs, on average, increase the use of special districts, whereas TELs don't force local governments to cut their intergovernmental fiscal transfer to special districts in the same county area. The estimation results are robust to alternative model specifications and empirical strategies.

In a broad view, the empirical findings indicate that creating new special districts, as a circumvention tool, is comparatively appealing relative to traditional options. Stated differently, when facing fiscal pressures caused by TELs, local government not only use traditional approaches such as outsourcing, cutting expenditures, diversifying revenue sources, etc, but they also turn to off-budgetary approaches such as building special districts to "invisibly" finance public services. Given that the primary motivation of TELs is to reduce tax and increase the efficiency of government operation, the unintended proliferation of off-budgetary special districts represents a backlash, as the extensive use of fragmented special districts would probably undermine the local accountability and complicate, even increase, the true cost of local public services.

This paper is organized as follows. Section Two briefly describes historical trends of special districts in the existing literature. Next, in Sections Three to Five, I introduce the

theoretical framework, sample building, and empirical strategies. Empirical results and explanations will be presented in Section Six. Section Seven checks the sensitivity of my results, followed by conclusions and discussions on the limitations of the study.

1.2. The Growth of Special Districts and Literature Review

According to the definition by the Census of Governments, special districts are independent special-purpose government units, which "exist as separate entities with substantial administrative and fiscal independence from general-purpose local governments." Specifically, fiscal independence implies that an entity can determine its budget, set tax rates, collect fees and charges, and issue debt without review by other local governments; whereas administrative independence indicates that an entity "has a popularly elected governing body representing two or more local governments or even in the event its governing body is appointed, it performs functions that are essentially different from those of, and are not subject to specification by, its creating government."[5] Thus, some local government agencies with considerable fiscal autonomy are not considered to be governments, but are classified as dependent agencies of another government if they are controlled by a board composed wholly or mainly of parent government officials, or fiscally, their revenue largely depends on appropriations or allocations at the discretion of another state, county, municipality, township, school district or special district.[6] In addition to the administrative and fiscal independence, special districts are also

[5] More information about the definition of special districts can be found in *Individual State Descriptions: 2012*, issued by the Census Bureau.

[6] It is noteworthy that homeowners' associations (HOAs) and other types of institutions, which are often labeled as "private government," are not special districts, either. According to Helsley and Strange (1998), private governments have five distinctive essential characteristics: they are voluntary, exclusive, supplementary, self-financing, and strategic. Their analysis indicates that private government causes the public sector to reduce its provisions of the collective good, and the welfare effects of private government are ambiguous and complex. Cheung (2008) finds that TELs stimulated the growth of HOAs in the era of Proposition 13 in California, but his empirical results cannot be simply applied to the case in this paper due to the fundamental difference between special districts and private governments.

characterized by being exempt from civil service regulations, rules for competitive bidding and procurement, local building codes, and some fiscal constraints (Walsh, 1978).

Compared with the stable trend of all types of local general-purpose governments and the declining trend in the number of school districts in the past one hundred years, it is well-known that special districts have grown rapidly in the U.S., as shown in Figure 1. Special districts could be classified and understood in many ways. One approach is to divide them into two categories by the number of functions they assume, including single-function districts and multiple-function ones. Figure 2 (below) indicates that single-function, multiple-function, and the total number of special districts increased from 1972 to 2007 by 44%, 372%, and 56.5%, respectively. Another approach among scholars to differentiate special districts is to divide them into two types of independent local institutions: one type is taxing districts (TD), which are governed by elected boards and have the power to tax, and the other type are public authorities (PA), which are governed by appointed boards and have no taxing power (Foster, 1997).[7]A third approach differentiates all special districts into 37 categories by their specific functions, including fire protection, school building authority, mortgage credit, libraries, irrigation, drainage, water supply utilities, and the like.

State enabling legislation fundamentally provides the legal basis for the creation of special districts at the local level. State general-enabling legislation sets forth the rules for all districts of a particular type in a state, while state special-enabling legislation pertains to a single special district. Although a few special districts are established through special-enabling legislation, most special districts are mainly created through two common ways pursuant to state

[7] An example of public authorities is the Recreation Authorities in Georgia. More details about it can be found at http://www2.census.gov/govs/cog/gc0212ga.pdf

general-enabling legislation. First, one or more local general-purpose governments may create a special district by resolution; second, a citizen or group of citizens may initiate taxing districts by petition (Foster, 1997). In a nutshell, the state statutes basically govern the number of types or the functional breadth of local special districts, which in turn determines the real number of special districts.

The existing literature on special districts has extensively discussed the following research questions. First, what are the determinants of the increase of special districts (Foster, 1997)? Second, does the decentralized public service provision via more special districts affect residential property values (Billings and Thibodeau, 2010)? Third, do special districts affect local public sector size, measured by the tax or all own-source revenues as a share of total income in particular jurisdictions (Zax, 1989)? Fourth, given that multiple territorially overlapping special districts share the authority to provide services and levy taxes in a common geographic area, is there a common-pool problem, particularly, whether increasing the number of overlapping special districts results in overfishing from the shared tax base (Berry, 2009)? While the literature on special districts explores all of these questions, for this literature review, I will focus on what other scholars have found that pertains to the question this paper seeks to address; that is, the possible causes for the rapid growth of special districts.

Several competing theories have been proposed to explain the determinants of the increase of special districts: (1) some studies find special districts are used to buffer policymaking from political pressure or to establish credible commitment with particular constituency and interest groups. Burns (1994), for example, claims real estate developers want the government to provide infrastructure investment to improve their property value, and this is

most easily achieved by creating a special district; (2) special districts assist in resolving problems of economy of scale in an inter-jurisdictional context[8]; (3) special districts are created to better match service provisions for appropriate areas of effect, and thereby improve allocative efficiency (Ostrom et al., 1988); (4) local governments use special districts to circumvent state-imposed fiscal constraints such as debt limitations (Smith, 1964; Walsh, 1978; Bollens, 1986; Bunch, 1991; Sbragia, 1996; Wallis and Weingast, 2008; Radford, 2013) and tax and expenditure limitations (ACIR, 1962, 1977; MacManus, 1981; Bennett and DiLorenzo, 1982; Nelson, 1990; Bowler and Donvan, 2004; Carr, 2006; Goodman and Leland, 2014; Slivinski, 2014)[9]; or, put differently, special districts are the direct products of the "politics of circumvention."

This paper is relevant to the fourth theory and aims to examine whether and how the use of special districts could be explained by the fiscal stringency of TELs. By TELs, I mean the TELs imposed on local governments by state governments (i.e., local-level TELs) instead of TELs imposed on state governments by themselves (i.e., state-level TELs). Although several TELs were passed before Proposition 13 in California, it is generally acknowledged that Proposition 13 began the modern anti-tax movement (Kioko and Martell, 2012). TEL measures continue to be popular to this day. Indiana and New York, for example, enacted TEL measures in 2010 and 2011, respectively. Broadly, TELs have little effect on the overall size of the state and local public sector (Shadbegian, 1999; Sun, 2012; Mullins and Joyce, 1996). They do, however, have a significant impact on local revenue composition. Specifically, TELs lead to declining

[8] However, it is a misconception that special districts are mainly established to deal with regional or cross-county problems. Statistics provided by the census of government clearly show that most special districts are within a specific county's boundaries.
[9] In her seminal book, Foster (1997) argues that the first theory is a structuralist perspective, the third is a public choice viewpoint, and the fourth reflects spirits embedded in a metropolitan economy view.

reliance on broad-based taxes like the property tax (Bradbury et al., 2001; Dye et al., 2005) and increasing reliance on pro-cyclical revenues, including sales tax, income tax, user charges, and fees (Shadbegian, 1999; Hoene, 2004; Jung and Bae, 2011; Sun, 2012; McCubbins and Moule, 2010; Skidmore, 1999). These changes lead to a significant loss in local fiscal autonomy and an expanded role for state governments (Sokolow, 2000; Saxton, Hoene, and Erie, 2001; Skidmore, 1999; Thompson and Green, 2004). Because the property tax is used to fund K-12 education, TELs are also found to diminish the quality of education (Downes and Figlio, 1999), including the quality of the teaching workforce (Figlio and Rueben, 2001). In addition, TELs place an additional burden on governments through increased borrowing costs and lower ratings (Poterba and Rueben, 2001; Johnson and Kriz, 2005; Wagner 2004), where even the threat of a TEL is perceived negatively in the market (Benson and Marks, 2010).

As a general rule, special districts are exempt from TELs imposed on local governments (Berry, 2009). This legal loophole makes it possible for new special districts to be formed by local governments to evade the fiscal pressure caused by TELs. The temporal coincidence between the modern anti-tax movement, which began in the 1970s, and the expansion of special districts also suggests a strong correlation between the two. The possible impacts of TELs on the use of special districts has long been discussed but not rigorously tested with updated datasets. Indeed, extant literature provides mixed evidence about the possible effects of TELs, both in case studies and in regression analysis.[10]

Case studies conducted in several states find contradicting results. MacManus (1981) finds both property and tax restrictions are positively associated with increases in the number of

[10] In contrast to the mixed findings on circumvention theory with attention to TELs, empirical results on the circumvention theory with respect to debt limits are basically consistent in the literature.

special districts in ten southern states. Bennett and Dilorenzo (1982) argue that local governments have responded to local-level TELs by dramatically increasing the number of special districts or off-budget enterprises (OBE), issuing more nonguaranteed debt and placing more expenditures off-budget in Pennsylvania, Indiana, Kansas, Minnesota, Montana, and Wisconsin, since the 1970s. Lewis (2000), however, indicates that Proposition 13 did not encourage reliance on special districts in California because local governments are more malleable than is commonly supposed, and they are often able to adapt to external fiscal constraints without resorting to the creation of new local governments.

Regression analyses also fail to provide a consensus on this debate. Nelson (1990) claims fiscal limits imposed on municipalities are positively associated with the number of special districts. Similarly, McCabe (2000) finds positive connections between TELs and special districts by analyzing data from 1977 to 1992. Based on a sample from 1992 to 2002, Carr (2006) argues that when municipal- and county- TELs are jointly examined, larger numbers of special districts are seen in those states with the most restrictive TELs. Bowler and Donvan (2004) use data from 1977 to 1997 and find that state-level TEL adoption has a significant effect on the growth of both municipalities and special districts in states where ballot initiatives are easier to use. The most recent research, conducted by Goodman and Leland (2014)[11], also supports the theory of the politics of circumvention. They use county-level data from 1972 to 2007 and fixed-effects negative binomial regression[12] to argue that circumvention appears to be largely a county phenomenon, rather than a strategy adopted by municipalities. Some of the recent evidence, however, refutes the belief in the use of special districts increasing in response

[11] https://dl.dropboxusercontent.com/u/2338036/Goodman_Leland_ABFM_2014_Presentation.pdf
[12] Interestingly, the conditional negative binomial model for panel data is not a true fixed–effects method. This method does not in fact control for all stable covariates (Allison and Waterman, 2002).

to the imposition of TELs over time. Heikkila and Ely (2003), for example, construct a sample from 1992 to 2002 and argue that property tax limits impose negative effects on the proliferation of special districts[13]. In her seminal work, Foster (1997) argues that heavy property tax limits on municipal governments reduced metropolitan reliance on special districts, as local officials realized that tax limits are reflective of an anti-tax sentiment for all types of local governments.[14]

At least four reasons explain why previous investigations have achieved mixed findings. First, some empirical studies fail to clarify why they choose to use state-level TELs instead of local-level TELs. Although both of them are imposed by state governments, it is more appropriate to lock onto local-level TELs, as most special districts are initiated by local governments or local residents to address local service concerns. Second, the modern anti-tax ethos gained much more popularity in the 1970s and 1980s, and as a result, studies might produce different empirical findings when they are only focused on the 1990s and afterwards. Third, either positive evidence or negative associations are very likely to be statistical artifacts when substantial heterogeneities across regions and states are not adequately considered in empirical models. Fourth, related to the third point, both TEL laws and the use of special districts are probably jointly influenced by underlying fiscal conservativeness of citizens, which may vary over time and is difficult to measure and control. An estimation without considering the influence of underlying fiscal conservativeness may just capture a spurious correlation between TELs and the growth of special districts. The possible omitted variable bias, however, has not been seriously discussed in the existing literature.

[13] It is noteworthy that they find that expenditure limits positively influence the use of special districts across states.
[14] A related study conducted by Berry (2009) shows that TELs couldn't explain the aggregate own-source revenue across all governments in a county. But, his findings are not equivalent to saying that TELs have no impact on the growth of the number of special districts.

Before turning to my theoretical model, I would like to emphasize the heterogeneities in the function of special districts and the differences in TEL laws across states, which in turn require one to make careful assumptions regarding the unit of analysis, the measure of TELs, and the definition of growth of special districts. For example, because no two TELs are the same across states, it is unpersuasive to conduct empirical analysis using a single indicator of TELs. Thus, a convincing empirical analysis should first clarify the basic assumptions they made; then, check whether their assumptions can hold on the basis of theory and data; and finally, test whether estimation results are sensitive to alternative assumptions. More discussions on the assumptions I made in this paper will be presented in following sections.

1.3. Analytical Framework

Theoretically, TELs could affect the use of special districts in two conflicting ways through the same mediator—local governments. As described earlier, most special districts are initiated and created by local governments or citizens, pursuant state general-enabling legislations. So, to address fiscal pressures caused by TELs, local governments may pass on parts of the fiscal constraints by cutting their intergovernmental aids to special districts in their jurisdictions, slowing down the growth of special districts. The validity of this deterrent mechanism implicitly relies on a controversial assumption that, to some extent, cutting public services is acceptable to local residents. To approve this possible channel, one has to locate evidence that TELs result in a significant decrease of the aggregate fiscal aid from local governments to special districts.

On the contrary, as suggested in circumvention theory, state-imposed TELs could also impose a positive impact on the use of special districts: namely, local governments may create

more special districts in order to circumvent the fiscal constraints caused by TELs. In doing so, local governments can maintain a satisfying quality of public services without incurring high tax burdens. Broadly, circumvention theory implies that TELs not only fail to reduce local on-budget revenue and expenditure size, but they also force local governments to seek new off-budget opportunities to survive, probably resulting in a higher true cost of public service. Different from the first possible channel, which views local general-purpose governments and special districts separately and independently, this second channel treats all types of local governments as an integrate local governance system in which special districts are complementary to counties and municipalities in public service provision. The validity of circumvention theory fundamentally relies on the prerequisite that creating more special districts is a necessary and relatively appealing circumvention tool compared with other circumvention options. Considering the fact that TELs result in a significant loss of local fiscal autonomy and an expanded role for state governments (Saxton et al., 2001; Sokolow, 2000; Skidmore, 1999; Thompson and Green, 2004), the second channel requires controlling for intergovernmental aids in my empirical model.

Based on the aforementioned assumptions and extant literature, the number of special districts within a county is conceptually a function of four categories of variables. The first strand of variables includes the degree of fiscal stringency imposed by a series of fiscal institutions, including a variety of debt limits and TEL laws. Debt limits (i.e., dollar limits) and voter approval requirements (i.e., procedural limits) should be controlled for, as public authorities have been extensively involved in the so-called "backdoor borrowing" approach.[15]

[15] For example, backdoor borrowing is the most egregious method that New York State uses to accumulate debt. Public authorities frequently borrow money but those actions are often hidden from the public eye, as they are neither approved by voters nor authorized by the state constitution. More information on the dramatic increase of public authority debt in New York can be found in the *New York State's Debt Policy: A Need for Reform*, by the New York State Office of the State Comptroller in Feb. 2005.

Based on the literature, the relationship between debt-related limits and the use of special districts is expected to be positive. The state-imposed TELs, however, could either discourage or intensify the use of special districts, as discussed earlier.

The second category is determinants of the cost and demand for public services. This includes population, population density, personal income per capita, employment level, the fraction of senior people more than 65 years old, the fraction of people below 19 years old, the proportion of black people, and the proportion of other racial groups. A larger population size may yield more special districts to provide public services, whereas a county with a larger population density is expected to have fewer special districts due to the possible economics of scale in the provision of public services. On the basis of Wagner's "law," both personal income and employment level per capita are expected to increase the demand for public services and thereby stimulate the use of special districts. The next four variables of the demographic composition are used to capture the various preference for public services. Adding racial heterogeneity also allows for a direct test of the conflicting viewpoints regarding the effects of racial heterogeneity on the formation of special districts between Burns (1994) and Alesina et al. (2004). Burns (1994) finds that counties with more African Americans had more special district formations, whereas Alesina et al. (2004) claim no significant relationship between racial heterogeneity and the number of special districts.[16] [17]

[16] It is worth noting that, instead of using a racial heterogeneity index, this paper directly puts the population fraction of each racial and age group in the regression, because a heterogeneity index cannot precisely distinguish the situation where a county has 70% white and 30% black from the "reverse" situation where a county has 30% white black and 70% black. A similar strategy is used in Berry (2009).

[17] Some structuralists may argue for the importance of county business patterns and developer interests (Burns, 1994; McCabe, 2000). Foster (1997), however, tests this argument and fails to find any significant explanatory power of real estate developers in the formation of special districts. As she suggests, "property developers need not form special districts to achieve pro-growth goals" and "the availability of alternative pro-development institutional choices means special districts may, but need not necessarily, be a familiar institution in areas where pro-development sentiment is high." Given the theoretical ambiguity of that argument in the literature, the lower reliability of simply using the number of real estate developers as a measure of real estate's political and

Based on the existing studies on TELs, the third cluster includes local reliance on intergovernmental fiscal transfers, which could be explored by local governments to mitigate fiscal pressure when facing TELs. Logically, local governments don't need to increase the use of special districts at all when there is sufficient state aids to offset the fiscal constraints of TELs and those aids are not subject to local-level TELs. In other words, inter-governmental aids should be controlled for, as they are likely to be one of the mediators between the fiscal stringency of TELs and the local incentive to use special districts.

The fourth group encompass statewide legal determinants of the creation of special districts, including the aforementioned state general-enabling legislations, home rule policies, and the laws governing municipal annexation and incorporation. Specifically, states will have more special districts if they permit more kinds of special districts to be created. Without comprehensive and accurate information about those general-enabling laws, I use the functional breadth of special districts as a proxy measure and expect a positive connection between functional breadth and the number of special districts. In addition, home rules and laws on municipal annexation and incorporation are also expected to influence the feasibility and necessity of using special districts in the provision of public services. For instance, when municipalities could easily annex surrounding unincorporated areas or local residents could easily incorporate a city or a village to secure the provision of public services, new special districts are less likely to be formed.

marketing strategies, and the weak connection between local real estate developers and state-wide anti-tax movements, my model does not directly take into account the structuralist perspectives. Nevertheless, I control for the total number of jobs per county and personal income per capita in my model.

1.4. Data and Measures

The number of independent special districts and their financial information is consistently available from the Census of Government dataset from 1972 to 2012. The government data is comprehensively collected every five years ending in a 2 or 7. Mullins and Wallin (2004) provide a summary of local-level TELs across states.[18] Cost and demand variables can be obtained from the Bureau of Economic Analysis. The rest of this section will discuss data coding, measurement issues, and the final sample size, followed by a brief descriptive analysis.

Three data and measurement issues possibly affect the validity of my estimation. First, an optimal approach to measure the degree of fiscal stringency of TELs is to calculate the fiscal gap between the actual revenue (or expenditure) size and the legal ceiling of revenue (or expenditure) size mandated by TELs. But this approach requires comprehensive and accurate legal information about the TELs' fiscal coverage, and unfortunately, not every state and local government consistently provides it. Due to the infeasibility of calculating the exact degree of fiscal stringency imposed by TELs, I draw on the summary and classification of TELs by Mullins and Wallin (2004), and then develop four composite measures of TEL stringency to conduct the estimation. Basically, there are six important types of TEL laws constraining local revenue-collection capacity or expenditure size, including: 1) overall property tax rate limits applying to all local governments, 2) specific property tax rate limits applying to specific types of local governments or specific functions, 3) property tax levy (revenue) limits, 4) limits on property assessment increase, 5) general revenue increase limits, and 6) general expenditure

[18] There is an exception. Based on publicly available data provided by the Illinois Department of Revenue, I have updated the local-level TEL codes in Illinois. *(http://tax.illinois.gov/localgovernment/PropertyTax/PTELLcounties.pdf)*

increase limits.[19] The first composite measure I will use is a simple TEL indicator, which equals 1 as long as local governments are imposed to any of the six types of TEL limits. Based on the common understanding that TELs will be binding when they are imposed on property tax rate and property assessment together, I develop the second composite indicator, which seeks to measure whether binding property tax TELs have different effects than a simple TEL indicator. The third and fourth indicators equal 1 when local governments face general revenue increase limits and general expenditure limits, respectively. Supposing the politics of circumvention exist, at least one of the four above measures is expected to have positive and significant effects on the growth of special districts, both statistically and financially. One advantage of this approach, compared with just using a single TEL indicator, is that four measures are more likely to capture multiple facets of TEL laws across states, thus helping us to better understand the real circumvention mechanisms, if any. In addition to the four composite indicators, I also adopt the TEL index developed by Deller et al. (2012). The index takes into account the type and scope of TELs, exemption provisions, and methods of override, so it will approximately measure the degree of fiscal stringency of TELs. Nevertheless, the estimation results of using the TEL index should be consistent with those of using four composite measures, as they are all formulated out of the same law provisions.

Second, the unit of analysis is county area, but usually there are two layers of general-purpose governments in a county area: a county government and some municipality governments.[20] In other words, the unit of analysis is not equal to the behavioral unit. So, one challenging task is to identify the specific layer of general-purpose governments that possibly

[19] Full disclosure is not included in this study as it simply requires some type of public discussion and specific legislative vote prior to the enactment of tax rate or levy increase. It neither directly targets fiscal limits nor has binding effects on local fiscal power.
[20] The majority of American states don't have towns or townships.

adopt circumvention strategies. More often than not, state governments impose TELs on counties and municipalities at the same time, and the correlation between the TELs on the two layers of governments is quite high.[21] In addition, as far as I am aware, no theory sheds light on the behavioral assumption that county and municipality behavior substantially differ in response to a similar, if not the same, set of state fiscal policies within a state. Based on the assumption that counties and cities behave in a consistent way, I systematically synthesize the county-level TELs and municipality-level TELs for all composite measures. That is to say, I impose further restrictions on the way to code composite indicators. For example, the first composite TEL indicator will be assigned to be one only when both the county-level TEL indicator and municipality-level TEL indicator equal one. By doing so, I treat all local general-purpose governments the same and thus, my estimation explores how TELs affect the dynamics between all general-purpose governments as a whole and all special districts.

Third, I want to clarify how "missing" values are handled in my analysis and briefly introduce the final sample size. In the Census of Government data set, some counties do not report any special districts information even in census years, and consequently, they are presented as "missing" values in my sample. Given the fact that some counties do not have any special districts during the early years in my sample and many special districts are established as "ad-hoc" institutions or have a very short time span, I assign 0 to the number of special districts for the counties with "missing" values.[22] The final sample excludes counties or county-equivalent entities in Alaska, DC, and Hawaii, as they are usually considered to be very different

[21] Based on my own calculation, the correlation coefficient between the TELs on counties and those on municipalities is almost 0.9.
[22] My final results are not substantially affected by the way I deal with missing values. Sensitivity tests were conducted and they are available from the author upon request.

from those in other states. Independent cities are not taken into account either, because geographically and administratively, independent cities like those in Virginia are not comparable to typical counties. By the census definition and statistics from 2010 [23], there are 3,044 active counties and legally-defined inactive counties in total[24] and 18 county-city consolidated entities, like New York City and San Francisco City[25], which are also considered to be good candidates in need of special districts in the 48 Continental United States. After merging the Census of Government dataset with economic statistics provided by the Bureau of Economic Analysis, I lose 24 counties in Virginia as the economic and demographic statistics (e.g., personal income and employment data) for those counties are bundled with their neighboring independent cities and I cannot separate them. Ultimately, I get 3,038 counties in 2007, among which five counties were established after 1972, and the remaining 3,033 counties have complete panels over all 8 periods (i.e., 1972, 1977, 1982, 1987, 1992, 1997, 2002, and 2007) in my sample.

As shown in Table 2, on average, each county has around 10 special districts from 1972 - 2007, almost 60% of which are taxing districts and the remaining 40% are public authorities. Harris County, within the Houston–The Woodlands–Sugar Land metropolitan area of Texas, uses special districts most frequently, with 465 special districts in 1992. Special districts basically achieve budget balance since their average revenue and average expenditure are roughly the same. It is interesting to find that 49% of the revenues of special districts come from general charges and miscellaneous sources, with less than 10% from local property tax levies. This is consistent with a common notion that even though they are generally exempted from

[23]More information can be found at https://www.census.gov/geo/reference/codes/cou.html
[24] Legally-defined inactive or nonfunctioning counties include those in Connecticut, Rhode Island, and some in Massachusetts. Those counties do not have real county-level governments, and they mainly exist for geographical and statistical purpose. But given that my unit of analysis is county area rather county government, I include them in my analysis.
[25] My final results are also very robust to the exclusion of city-county consolidated entities like New York City and San Francisco City. Empirical results based on the sample without those entities are available from the author upon request.

TELs imposed on municipalities, special districts prefer to use other financial tools rather than property taxes, which are traditionally tapped by other local governments. Special districts use debt extensively and their average long-term outstanding debt is almost twice as large as their expenditure size. TELs have been widely adopted, yet very few states impose fiscal constraints on the local general revenue and general expenditure size, largely because they are much more binding than simply imposing a limit on property tax rate.

1.5. Empirical Strategies

This paper aims to test the impacts of TELs on special districts by exploiting the variation of the number of special districts across counties in the U.S. Towards that end, there are two methodological challenges facing this research.

First, a positive (negative) association between TELs and the number of special districts would hardly be conclusive evidence of the circumvention (deterrent) effects we hypothesized. Existing studies also question the assumption that TELs are randomly assigned across states in empirical work on TELs (Rueben, 1997; Shabegian, 1999; Poterba and Rubin, 2001; Wagner, 2004; and Sun, 2014). Any omitted variable at the state/county level associated with both the propensity to adopt TEL laws and their preference in using special districts would lead to biased coefficients. Thus, my basic empirical strategy is to look within counties across time and measure how their number of special districts changed as a function of the adoption of TEL laws. The estimation could be represented by the following equation, with county as the unit of analysis:

$$(1) \quad Y_{isrt} = \alpha + \beta L_{isrt} + \gamma X_{isrt} + \delta A_{isrt} + \rho S_{st} + W_{rt} + \mu_i + \theta_t + \varepsilon_{isrt}$$

Where i indexes counties, s indexes states, r indexes regions, t indexes periods, Y indicates the number of special districts in a county, L is a dummy variable coded as one if county i faces TEL laws in year t, X is a vector of cost and demand for public services in each county, A is intergovernmental aid received by local governments aggregated at the county level, S is a series of legal determinants of the creation of special districts at the state level, W is region-year dummies designed to capture linear and non-linear regional time trends, μ is a vector of county fixed effects, θ is a vector of year fixed effects, and ϵ is error term.

One advantage of this model is that all time-invariant factors that affect the growth of special districts will be subsumed into county-level fixed effects (i.e., μ_i). For instance, as explained earlier, revenue bonds could be issued by special districts without being restricted by voters' approval requirements or dollar limits. This institutional loophole, established prior to the sample period in the analysis, has stimulated local governments to create special districts to accumulate debt (Sbragia, 1996; Foster, 1997; Weingast and Wallis, 2008). The existing incorporation power of local residents, annexation ability of local governments, and home rule policies are also relevant to the feasibility and necessity of forming new special districts. Geographical features impose another influence on the use of special districts in water supply, drainage, and irrigation. By using a fixed effects model, all of those time-invariant institutional, legal, and geographical factors will be implicitly controlled.

Accounting for time-invariant unobservable factors with a fixed effects model is a step in the right direction but is unlikely to eliminate all bias in the estimate of β. As discussed in the literature review section, underlying fiscal conservativeness varies over time, and it may affect both the propensity of adopting TELs and the use of special districts in a state/county. A further

challenge is that the direction of possible bias caused by the omitted fiscal conservativeness is theoretically unclear. On the one hand, voter support for TEL measures, like Proposition 13, has been largely driven by a desire for lower taxes and more efficiency in government[26] (Levy, 1975; Ladd and Wilson, 1982). Stated differently, fiscally conservative voters are more likely to vote for TELs or more-stringent TELs. Meanwhile, they might be against the growth of special districts as special districts finance public services via taxation or collecting user fees, neither of which will decrease public service costs. In that case, voters' fiscal conservativeness will positively affect the adoption of TELs and negatively affect the growth of special districts, thereby introducing negative bias into my OLS estimation. On the other hand, fiscally conservative voters may think "businesslike" special districts are more efficient than traditional general-purpose governments, so that they may prefer to use more special districts in many service aspects. The second possibility, then, leads to a positive bias in my OLS estimation.

To address the concern related to the possible omitted variable bias caused by time-varying fiscal conservativeness, I have improved the estimation model by adding two political taste variables as approximate measures for voters' fiscal conservativeness. As suggested in Gilligan and Matsusaka (1995), political taste and climate may affect fiscal behaviors and institutional choice in a state. Specifically, I incorporate a state government ideology index (Berry et al., 2010) and a citizen ideology index (Berry et al., 1998) in Equation (1), both of which are operationalized by interest-group ratings compiled by the Americans for Democratic Action (ADA) and the AFL-CIO Committee on Political Education (COPE). The state government ideology index measures the average location of the elected officials in each state on a liberal-conservative continuum, with 100 being the most liberal value and 0 being the most

26 But voters by no means want reduced public services, as clearly suggested in the literature.

conservative value. The citizen ideology index measures the average location of the active electorate in each state on the same continuum (Berry et al., 2010).[27] As a result, the final empirical model is below:

$$(2) \quad Y_{isrt} = \alpha + \beta L_{isrt} + \gamma X_{isrt} + \delta A_{isrt} + \rho S_{st} + C_{st} + W_{rt} + \mu_i + \theta_t + \varepsilon_{isrt}$$

Where the added C indexes fiscal conservativeness, approximately measured by the state government ideology index and citizen ideology index.

In addition to employing fixed effects, controlling for two approximate measures of fiscal conservativeness, and incorporating regional time trends[28] to eliminate bias in the estimate of β caused by time-invariant factors and time-varying fiscal conservativeness, I perform two fundamental checks to further address the concern that other time-varying factors, like unobserved state-specific time trends in both a state's probability to adopt TELs and its propensity of building special districts, may drive the positive/negative association between TELs and the number of special districts. First, I examine the change of β after adding state-specific time trends to Equation (2). Namely, I alternatively estimate the following model:

$$(3) \quad Y_{isrt} = \alpha + \beta L_{isrt} + \gamma X_{isrt} + \delta A_{isrt} + \rho S_{st} + C_{st} + W_{rt} + \mu_i + \theta_t + TR_s + \varepsilon_{isrt}$$

[27] The state government ideology index assumes that "state officials mirror their federal counterparts, i.e., that the average ideological position of state elected officials from each party matches the mean ideological position of that party's congressional delegation, as measured by ADA and COPE scores." Assuming that "voters choose the candidate they perceive as having an ideology closest to their own," the citizen ideology index "infers the ideological position of the electorate from the distribution of votes in congressional races and ADA/COPE scores for members of Congress." (Berry et al., 2010)

[28] The region-year dummies could be helpful in further controlling for the fact that states in west regions are usually more conservative than states in other regions and many other heterogeneities such as economic structure, climate change, and migration across regions and across time. However, because TELs are more stringent and more widely adopted in western states, adding region-year dummies will absorb the variation of TELs and thereby reduce the explanatory power of TELs, driving the estimate of β smaller. In this sense, the estimate obtained in Equation (2) could be understood as a conservative estimate of the true effect of TELs. Indeed, I alternatively run Equation (2) without controlling for regional trends, finding stronger results of TELs, and these results are available from the author upon request.

Where TR_sindicates state-specific time trends. If β in Equation (3) became statistically and economically insignificant, the circumvention/deterrent effect obtained in Equation (2) would be less credible. Second, I examine the pattern of the use of special districts not only during the years right after adopting TEL laws but also during the years immediately before the adoption. This "even-time specification" could be described by the following equation:

$$(4) \quad Y_{isrt} = \alpha + \beta_{-4}T-4_{isrt} + \beta_{-2}T-2_{isrt} + \beta_0 T0_{isrt} + \beta_2 T2_{isrt} + \beta_4 T4_{isrt} + \beta_6 T6_{isrt} + \beta_8 T8_{isrt} + \beta_{10} T10_{isrt} + \gamma X_{isrt} + \delta A_{isrt} + \rho S_{st} + C_{st} + W_{rt} + \mu_i + \theta_t + TR_s + \varepsilon_{isrt}$$

Where T-2 is a dummy variable indicating the two years before a county is confronted with TEL laws, T0 correspond to the year of adoption of TELs, T2 correspond to the two years immediately following the adoption of TELs. Because most TELs were enacted in the 1970s and 1980s, the average time length before TELs is shorter than the average time length after TELs. Thus, Equation (4) captures more post-treatment periods.[29] In the years before a county imposed TELs, any significant increase in the number of special districts would undermine the circumvention hypothesis.

Second, direct control for contemporary inter-governmental aid variables will lead to endogeneity problems as both fiscal transfers from upper-level governments to local governments and those to special districts could be simultaneously influenced by state governments' financial management strategies and political pressure from interest groups and other behaviors. To avoid the endogeneity problem, I adopt 5-year lagged inter-governmental aid

[29] I also tried alternative model specifications. For example, I designed a model that captures the annual use of special districts from five years prior to TELs through fifteen years after TELs. Basically, the results are consistent with Equation 4.

variables in my model. By the same token, to address the possible concern that the function, the number, and the size of special districts may be simultaneously planned and determined, I use functional breadth lagged by five years as an approximate measure of state general-enabling laws in my model. While using lags could relieve endogeneity concerns, it is important to note that my results do not substantially change in any sense if I simply use contemporaneous values of intergovernmental aids or functional breadth. The results with a contemporaneous value of those variables will be presented in the robustness check section.

1.6. Empirical Results

1.6.1. TELs and the Number of Special Districts

Table 3 shows the estimation results for five models, all of which are based on Equation (2), but each has a different measure of TEL. For Model 1, the number of special districts in a county area is regressed on the first composite TEL indicators. Namely, when both counties and municipalities in a county area are confronted with any state-imposed TELs, local governments and/or residents will establish 0.567 more special districts to circumvent the fiscal pressure of TELs, holding all other factors constant. Model 2 captures the impact of binding property tax TELs on the use of special districts; as expected, the treatment effect is stronger because the second composite indicator is more binding than the first one. Model 3 and Model 4 aim to test the impact that the two most binding TEL laws have on the growth of special districts; one is the TELs that cap the general revenue size, and the other one is the TELs that restrict the general expenditure size. Although only a couple of states enacted such binding limitations and the use of regional time trends may undermine the explanatory power of those measures, it is still clear that TELs stimulate the creation of special districts. The last column investigates whether the effects of TELs on special districts is consistent with a numeric measure of TELs – TEL Index.

Again, a positive effect is found, although it is hard to give an intuitive explanation of the estimate.

Most of the other variables controlled for in Equation (2) also get a meaningful estimate. As expected, population size and population density are significantly and positively associated with the use of special districts. Personal income is also significantly and positively connected to the number of special districts, whereas the number of jobs doesn't play an important role in the models. Regarding racial heterogeneity, my results are consistent with Burns (1994). Specifically, I find counties with larger shares of black people are much more likely to build new special districts, which contrasts with the argument that "arrangements for special districts are idiosyncratic" with respect to racial heterogeneity (Alesina et al., 2004). Different age groups seem to compete for limited financial resources, leading to a fact reflected in the estimation results: a larger proportion of young students reduces the use of special districts. Because there are not many special districts for educational purposes, it makes sense that school districts and special districts compete with each other for securing financial resources. In addition, after receiving more intergovernmental transfer from state governments, it seems local governments are relieved from fiscal pressures to the extent that they are discouraged from creating additional special districts. In addition, state general-enabling laws, approximately measured by the functional breadth in my model, positively affect the number of special districts. The two approximate measures of fiscal conservativeness achieve different results, which demonstrates the uncertainty of estimation bias if we had not controlled for fiscal conservativeness.

As discussed before, the estimation looks within counties across time and measures how their number of special districts changed as a function of the adoption of TEL laws. To make

sure that states with TELs and states without TELs have parallel trends in creating special districts before the adoption of TELs, I estimate Equation (3), which adds state-specific time trends to Equation (2). As shown in the first two columns in Table 4, the two models are consistent, as the significant effects of TELs on the number of special districts are very comparable, both statistically and economically. That is to say, the positive and significant empirical findings are driven by TELs rather than by any state-specific time trends in both a state's probability to adopt TELs and its propensity towards building special districts. To further exclude other explanations of the increasing trends of special districts, I estimate Equation (4), which captures the number of special districts not only during the post-treatment periods but also during the years before the events occur. Figure 3 provides a graphical representation of the estimation results of Equation (4). The y-axis has been scaled so that the number of special districts is normalized to approximately zero. The notable increase in the number of special districts comes after the adoption of TELs[30].

1.6.2. TELs and the Fiscal Aid from Local Governments to Special Districts

As analyzed in the literature review section, the flip side of the circumvention effect is the deterrent effect. If the deterrent effect were true, we are supposed to see that local governments would substantially reduce fiscal support of the special districts they established. To test the deterrent effect, I directly estimate how TELs affect the aggregate fiscal aid that all special districts have received from local general-purpose governments in a county area. As shown in the third column of Table 4, I didn't find any empirical evidence for the deterrent story.

[30] The big jump of the number of special districts after the adoption of TELs is robust to several alternative model specifications. Regression results are available upon request from the author.

In a nutshell, all estimation results from Equation (2) to Equation (4) clearly support the circumvention story. Compared with other strategies, such as changing revenue composition, contracting services out to private entities, and relying more on state aids, which have been well documented in extant studies, creating new special districts is still comparatively appealing to local governments after the 1970s.

1.7. Robustness Check

This section further examines whether my empirical findings are sensitive to the use of lagged control variables and a possible Instrumental Variable (IV) approach.[31]

First, as discussed earlier, endogeneity problems may arise when contemporaneous values of functional breadth are employed in the estimation model, as state governments may simultaneously determine the type, which is one of the right-hand side variables, and the number of special districts, which is the left-hand side variable. By the same token, the inclusion of contemporaneous values of intergovernmental aid may be problematic, especially in Model 6, as state governments may strategically strike a balance between two fiscal-relief approaches to help local governments, one is fiscal support via intergovernmental aid, and the other one is institutional support via the permission to create special districts. In that case, the estimated treatment effect of TELs on the number of special districts will be biased. So, I use one-period lagged functional breadth and intergovernmental aid in Equations (2) – (4). However, it is reasonable to check that my estimation results are not purely or largely driven by this empirical strategy. My first robustness check is to examine the empirical results of using contemporaneous

[31] In addition, I test whether my results are robust to alternative sets of IVs and using the log form of the number of special districts as the dependent variable. Basically the results are consistent and they are available upon request from the author.

values of those variables in my estimation. As shown in the first column of Table 5, the point estimate is very consistent with what I received previously, namely, when local governments face TELs, on average 0.732 more special districts will be established in a county area.[32]

Second, I use a combination of fixed effects, regional time trends, and approximate measures for fiscal conservativeness to eliminate bias in the estimate of β in the main analysis. Another way to tease out the effects of TELs on the number of special districts from those of unobserved factors that influence both TELs and the number of special districts is to employ an instrumental variable (IV) approach. In principle, good IVs are supposed to theoretically explain the endogenous regressor but have no direct effects on the dependent variable except through the endogenous regressor. Given my use of panel data and county fixed effects, the IV candidates also need to have sufficient time series variation. These restrictions determine that many variables, which have been used as instruments for TELs in existing cross-sectional studies but basically are constant over time after the 1970s, are not appropriate to be used as IVs anymore in my study; these include: (1) an indicator that equals unity if voters can initiate an amendment to the state's constitution directly, (2) an indicator that equals unity if voters must submit ballot measures for legislative review, and (3) an indicator that equals unity if citizens must satisfy a geographic signature requirement to propose initiatives, etc. Furthermore, because the initiative process is only allowed in 24 states, the number of voter initiatives and the passage rate of voter initiatives are not convincing IVs, either; even though they have variation over time and are employed in the extant literature.

[32] Consistent results are also found for Model 2 – Model 5. All TEL measures have a positive and significant impact on the number of special districts. Due to space limitation, these outcomes are not reported here, but they are available upon request from the author.

A combination of two historical observations provides the first potential IV. First, the U.S. has witnessed two waves of tax revolt movements in history, both of which resulted in TEL laws that were imposed by states on local governments. The first wave mainly spanned from late in the 19th century to the start of World War II, whereas the second wave (i.e., the modern one) has been put in place after the 1970s. For example, Arizona State imposed the first wave of TELs on counties and municipalities in 1913, and then enacted modern TEL laws in 1980. Because most of the former TELs occurred before the 1940s, it is reasonable to believe that they might be positively related to the second wave of TELs, but had little to do with the special districts which were created approximately 40 years later. Second, voters are more likely to push TEL laws when they are much more conservative than before. That's to say, a relative change of voter conservativeness over time within a state, which can be measured by the subtraction of the current value of citizen ideology index from the corresponding value five years ago, could predict the probability of adopting TELs. The relative change in conservativeness in a five-year period could be understood as part of the idiosyncratic deviations from the general trend of conservativeness. Based on the logic that states with a first-wave of TELs are more responsive to the tax-revolt movement when the citizens become more conservative than before, the first instrumental variable I chose is an interaction term between the indicator of the first-wave of TELs and the relative conservativeness within a five-year period.

Based on the logic that TELs are proposed because voters want lower taxes and higher efficiency of government, the second IV I pick up is the tension between citizens and the state government, which is measured by the difference between the citizen ideology index and state government ideology index. The empirical results of the IV approach is exhibited in the second column of Table 5. It turns out that the two IVs predict the probability of adopting the modern

TELs very well, and as expected, both are significantly negative in the first stage. Basically, all of these find consistent empirical evidence to buttress the theory of the politics of circumvention.

1.8. Conclusion

The local landscape of American fiscal federalism has been characterized by the rapid growth of special districts over the past several decades. Several fundamental issues about the origins, institutional design, political accountability, and growth of special districts, however, remain unclear. This paper investigates whether the growth of special districts could be partly explained by the expansion of tax and expenditure limitations. The existing literature doesn't conceptually sort out different channels through which TELs may affect the increase of special districts, nor empirically discuss and address the omitted variable bias. This paper aims to fill in the intellectual gap by employing 35-year long panel data to test the impacts of TELs on special districts.

To eliminate possible bias in the estimate of β caused by time-invariant factors and time-varying fiscal conservativeness, my empirical models adopt fixed effects, control for two approximate measures of fiscal conservativeness, and incorporate regional time trends. I also perform two fundamental checks to ensure that common trend assumption is met. Basically, I find that TELs increase the number of special districts in a county, and the results are consistent across four indicators of TEL laws and a TEL index. The empirical findings lend solid support to the politics of circumvention theory, suggesting that local governments circumvent fiscal constraints caused by TELs by creating more special districts to finance public services. In addition, this paper directly examines the concern that local governments, when facing TELs, may reduce their fiscal transfer to special districts in their jurisdictions. I find no empirical

evidence for this possible deterrent effect. My estimation results are robust to alternative model specifications and empirical strategies.

Together with the consensus achieved in the literature that that many special districts were designed to circumvent debt limits (dollar limits of debt) and voter approval requirements (procedural limits of debt), the circumvention effects of TELs provide additional evidence that intergovernmental fiscal rules have substantially shaped the formation and growth of special districts in the U.S. The unintended proliferation of special districts also reveals that local governments have never been passive creatures and they are always full of "entrepreneurship" in addressing external pressure. The "entrepreneurship" may help local governments to strike a balance between the demand for public services and the political pressure from the public, but it may also complicate the local governance system and further blur, or even enhance, the true cost of public services. The subsequent problems worthy of further investigation include: 1) whether TELs are appropriate to the extent that they effectively lower the cost and improve government efficiency, and 2), whether special districts are desirable to local residents as byproducts of TELs.

It is important to note that all of the special districts in my sample are independent ones. Actually, many dependent special districts that are either affiliated with any governments or any governmental departments have also been widely used within governments. Due to the substantial difference between these two types of special districts, the estimation results in this paper could not be generalized to dependent special districts. Actually, New York State has begun to clear up the real number of total special districts and improve the accountability issues

raised by so many invisible governments.[33] But, as I am aware, not every state has started to work on this, and therefore, it is impossible to expand my research to a sample with comprehensive information on dependent special districts across states. In addition, due to the lack of annual data and the lumpy nature of capital investment, this paper cannot investigate how TELs affect local governments' strategies in accumulating debt via "backdoor" borrowing or issuing non-guaranteed debt. These important topics belong to a future research agenda.

[33] More information about special districts in NY could be found in the following link: http://www.osc.state.ny.us/localgov/datanstat/findata/index_choice.htm

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Figure 1.1: The Number of Local Governments, 1952 - 2007

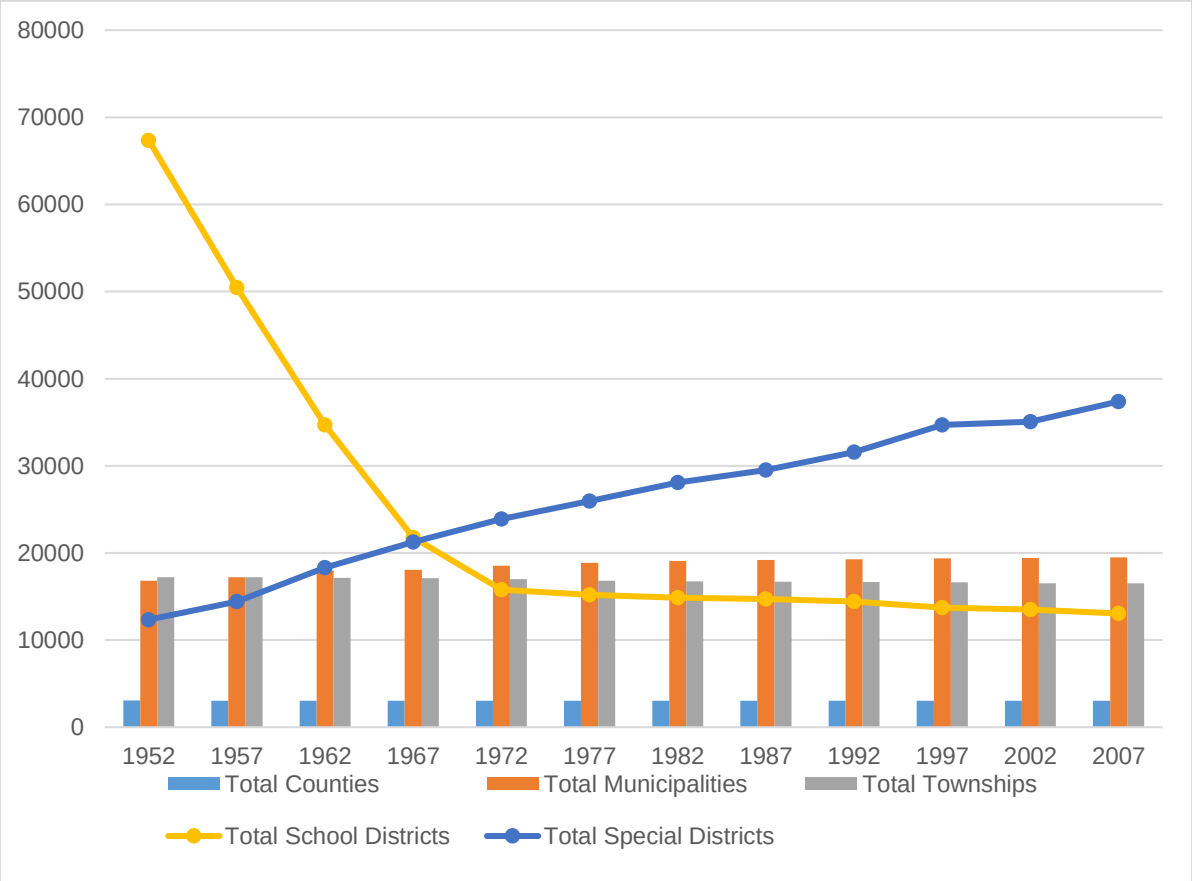

Figure 1.2: The Growth of Special Districts, 1972-2007

Figure 1.3: The Creation of Special Districts in Event Time

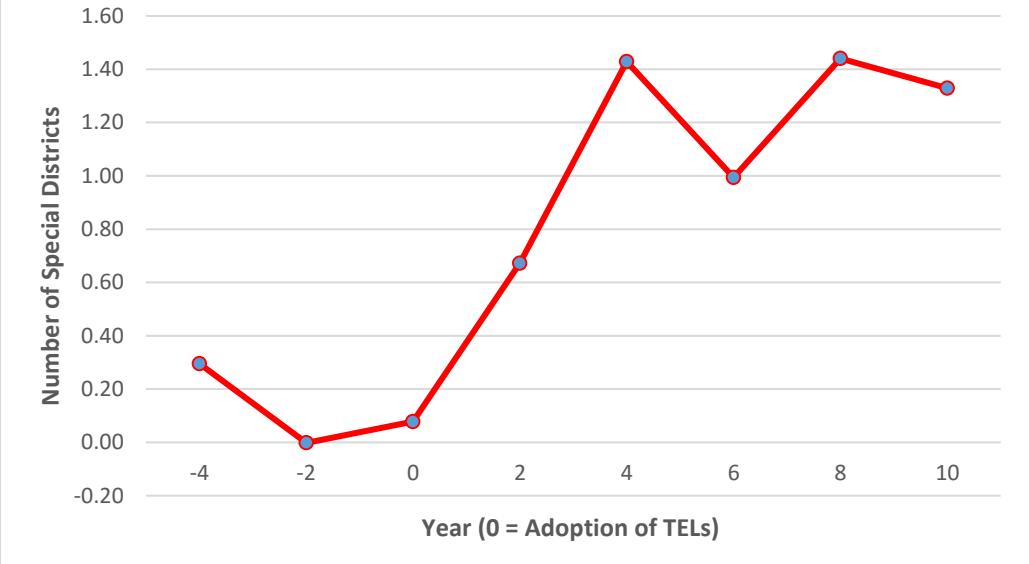

Table 1.1: The Structure of Four TEL Indicators

	1 Overall Property Tax Rate Limits	2 Specific Property Tax Rate Limits	3 Property Tax Levy Limits	4 Property Assessment Limits	5 General Revenue Limits	6 General Expenditure Limits
TEL Indicator	1 or 2 or 3 or 4 or 5 or 6					
Binding Property Tax TEL Indicator	1&4 or 2&4 or 3					
General Revenue TEL Indicator	5					
General Expenditure TEL Indicator	6					

Note: This table is heavily based on discussion in Mullins and Wallin (2004)

Table 1.2: Descriptive Statistics

VARIABLES	N	Mean	SD	Min	Max
Aggregate Data of Special Districts at County Level					
Total Number of Special districts	24,285	10.05	15.85	0	465
Total Number of Public Authorities	24,285	4.114	6.746	0	138
Total Number of Taxing districts	24,285	5.936	12.30	0	359
Total Revenue Per Capita	24,285	131.1	373.0	0	14,833
Total Rev. Own Sources Per Capita	24,285	109.3	359.3	0	14,830
Total Property Tax Per Capita	24,285	12.80	35.77	0	889.3
Total Charge and Misc. Rev. Per Capita	24,285	64.15	162.0	0	4,739
Total Intergovt. Aids Per Capita	24,285	21.84	56.19	0	2,803
Total Intergovt. Aids From Local Govt. Per Capita	24,285	4.314	18.86	0	670.4
Total Expenditure Per Capita	24,285	131.9	436.7	0	22,669
Total Capital Outlay Per Capita	24,285	26.47	166.0	0	14,538
Total L.T. Debt Issued Per Capita	24,285	26.43	310.7	0	20,333
Total L.T. Debt Outstanding Per Capita	24,285	248.1	1,820	0	75,052
A Proxy Measure of State General-Enabling Legislations					
Functional Breadth of Special Districts	24,285	16.62	4.939	4	28
The Reliance of Local General-Purpose Governments on Intergovernmental Fiscal Transfers					
Total Intergovt. Rev. From State Per Capita	24,285	169.4	176.4	0	3,088
Total Intergovt. Rev. From Federal Per Capita	24,285	31.00	72.31	0	7,526
Cost And Demand Variables for Public Services					
County Size	24,285	1,022	1,336	25.96	20,105
Total Population	24,285	80,701	295,203	75	9.706e+06
Population Density	24,285	131.0	529.2	0.111	17,215
Employment: Number of Jobs Per Capita	24,285	0.469	0.140	0.114	5.022
Personal Income Per Capita	24,285	11,780	3,345	3,716	59,859
Population Share: White	24,285	89.14	15.09	3.155	100
Population Share: Black	24,285	8.555	14.32	0	86.83
Population Share: Other Racial Groups	24,285	2.306	6.162	0	96.81
Population Share: Age 0_19	24,285	30.92	4.840	11.52	54.77
Population Share: Age 20_64	24,285	54.88	4.181	38.83	77.85
Population Share: Age 65_more	24,285	14.19	4.256	0.886	38.69
Population Share: Male	24,285	49.34	1.750	42.78	88.69
Approximate Measures of Fiscal Conservativeness					
State Government Ideology Index	24,285	44.69	14.32	6.533	95.97
Citizen Ideology Index	24,285	54.08	19.58	6.514	95.58
Four Indicators and An Index of Modern TELs					
TEL Indicator (C&M)	24,285	0.536	0.499	0	1
Binding Property Tax TEL Indicator (C&M)	24,285	0.429	0.495	0	1
General Revenue TEL Indicator (C&M)	24,285	0.0290	0.168	0	1
General Expenditure TEL Indicator (C&M)	24,285	0.0399	0.196	0	1
TEL Index (Statewide)	24,285	13.97	8.715	0	38
First Wave of TELs					
TEL Indicator (C&M) First Wave	24,285	0.593	0.491	0	1

*All variables of the first cluster are aggregate data of special districts at the county level. For example, "Total Revenue Per Capita" means the total revenue per capita for all special districts in a particular county. "Functional Breadth of Special Districts" calculates the total functional categories of special districts in a state, as a proxy measure of state general enabling legislation. The two variables of the third cluster are the aggregate intergovernmental revenues received by local general purpose governments from upper-level governments. "TEL Index" is derived from Deller et al. (2012).
**C&M, as explained in the text, means the corresponding TEL laws are imposed to county and municipality together.
***All financial and economic statistics are deflated by CPI index with 1983 as the base year.

Table 1.3: Regression Results (All Models Are Based on Equation 2)

VARIABLES	Model 1	Model 2	Model 3	Model 4	Model 5
TEL Indicator	0.567***				
	(0.167)				
Binding Property Tax TEL Indicator		0.770***			
		(0.148)			
General Revenue TEL Indicator			1.805***		
			(0.627)		
General Expenditure TEL Indicator				0.777	
				(0.676)	
TEL Index					0.0366**
					(0.0164)
Population (log)	3.475***	3.458***	3.387***	3.387***	3.362***
	(0.916)	(0.910)	(0.905)	(0.912)	(0.905)
Population Density	0.008*	0.008*	0.008*	0.008*	0.008*
	(0.0045)	(0.0045)	(0.0045)	(0.0045)	(0.0045)
Personal Income Per Capita	0.0002***	0.0002***	0.0002**	0.0002**	0.0002**
	(6.93e-05)	(6.89e-05)	(6.94e-05)	(6.94e-05)	(6.95e-05)
Number of Jobs Per Capita	0.343	0.338	0.375	0.412	0.398
	(0.672)	(0.673)	(0.655)	(0.667)	(0.664)
Black Share of Population	0.137***	0.136***	0.138***	0.140***	0.137***
	(0.0462)	(0.0459)	(0.0461)	(0.0462)	(0.0460)
Other Share of Population	0.0299	0.0226	0.0441	0.0363	0.0337
	(0.0585)	(0.0586)	(0.0587)	(0.0588)	(0.0585)
Population Share with Age 0_19	-0.0775*	-0.0753*	-0.0750*	-0.0754*	-0.0722
	(0.0435)	(0.0436)	(0.0439)	(0.0433)	(0.0439)
Population Share with Age 65_more	0.0282	0.0278	0.0295	0.0278	0.0311
	(0.0478)	(0.0476)	(0.0480)	(0.0477)	(0.0479)
Federal Intgov. Revenue Per Capita	-0.0006	-0.0006	-0.0006	-0.0006	-0.0006
	(0.0005)	(0.0005)	(0.0005)	(0.0005)	(0.0005)
State Intergov. Revenue Per Capita	-0.0014**	-0.0015**	-0.0013**	-0.0014**	-0.0014**
	(0.0006)	(0.0006)	(0.0006)	(0.0006)	(0.0006)
Functional Breadth of Special Districts	0.122***	0.139***	0.0908***	0.102***	0.113***
	(0.0258)	(0.0262)	(0.0236)	(0.0239)	(0.0257)
Citizen Ideology Index	0.0224***	0.0183**	0.0226***	0.0232***	0.0213**
	(0.0087)	(0.0085)	(0.0087)	(0.0086)	(0.0088)
State Government Ideology Index	-0.0094***	-0.0089***	-0.0096***	-0.0089***	-0.0091***
	(0.002)	(0.002)	(0.0020)	(0.0020)	(0.0020)
Constant	-30.91***	-31.14***	-29.03***	-29.21***	-29.69***
	(10.05)	(9.916)	(9.882)	(9.990)	(10.02)
Observations	21,247	21,247	21,247	21,247	21,247
R-squared	0.949	0.949	0.949	0.949	0.949
County FE & Year FE	Yes	Yes	Yes	Yes	Yes
Regional Time Trends	Yes	Yes	Yes	Yes	Yes
Intergov. Aid & Functional Breadth Lag	Yes	Yes	Yes	Yes	Yes

Note: The dependent variables are the total number of special districts per county. Clustered standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 1.4: Regression Results

VARIABLES	Model 1 (Based on Equation 2)	Model 1 (Based on Equation 3)	Model 6 (Base on Equation 2)
TEL Indicator	0.567***	0.327***	-0.495
	(0.167)	(0.121)	(0.482)
Population (log)	3.475***	3.768***	-1.057
	(0.916)	(1.050)	(2.138)
Population Density	0.0080*	0.0088*	-0.0021
	(0.0045)	(0.0047)	(0.0062)
Personal Income Per Capita	0.0002***	0.0002***	0.0002
	(6.93e-05)	(5.63e-05)	(0.0003)
Number of Jobs Per Capita	0.343	0.354	0.890
	(0.672)	(0.550)	(6.282)
Black Share of Population	0.137***	0.129***	-0.0468
	(0.0462)	(0.0461)	(0.139)
Other Share of Population	0.0299	0.0790	0.308
	(0.0585)	(0.0691)	(0.311)
Population Share with Age 0_19	-0.0775*	-0.0044	-0.116
	(0.0435)	(0.0436)	(0.181)
Population Share with Age 65_more	0.0282	0.129**	-0.157
	(0.0478)	(0.0505)	(0.153)
Federal Intgov. Revenue Per Capita	-0.0006	-0.0004	-0.0011
	(0.0005)	(0.0004)	(0.0013)
State Intergov. Revenue Per Capita	-0.0014**	-0.0008	0.0057
	(0.0006)	(0.0007)	(0.0038)
Functional Breadth of Special Districts	0.122***	-0.0146	0.0513
	(0.0258)	(0.0156)	(0.0706)
Citizen Ideology Index	0.0224***	-4.99e-05	-0.0068
	(0.0087)	(0.0048)	(0.0242)
State Government Ideology Index	-0.0094***	-0.0034**	0.0278***
	(0.002)	(0.0013)	(0.0103)
Constant	-30.91***	-56.88*	15.90
	(10.05)	(30.34)	(23.37)
Observations	21,247	21,247	21,247
R-squared	0.949	0.954	0.407
County FE & Year FE	Yes	Yes	Yes
Regional Time Trends	Yes	Yes	Yes
Intergov.Aid & Functional Breadth Lag	Yes	Yes	Yes
State-Specific Time Trends		Yes	

Note: The dependent variable of Model 1 is the total number of special districts per county, both in column 1 which is based on Equation 2 and column 2 which is based on Equation 3. The dependent variable in Model 6 is the total amount of intergovernmental aid all special districts have received from local governments in a county area. Clustered standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 1.5: Robustness Check

VARIABLES	Model 1 (Without Lags)	Model 1 (IV Approach)
TEL Indicator	0.732***	4.343***
	(0.180)	(1.489)
Population (log)	3.135***	4.189***
	(0.983)	(0.895)
Population Density	0.0104	0.0084**
	(0.0068)	(0.0042)
Personal Income Per Capita	0.0002***	0.0002***
	(6.83e-05)	(6.75e-05)
Number of Jobs Per Capita	0.0389	-0.235
	(0.689)	(0.705)
Black Share of Population	0.126**	0.120***
	(0.0502)	(0.0429)
Other Share of Population	0.0762	8.50e-06
	(0.0599)	(0.0559)
Population Share with Age 0_19	-0.0483	-0.112**
	(0.0376)	(0.0475)
Population Share with Age 65_more	0.0362	0.0315
	(0.0472)	(0.0463)
Federal Intgov. Revenue Per Capita	-0.0010*	-0.0004
	(0.0005)	(0.0005)
State Intergov. Revenue Per Capita	-0.0013**	-0.0006
	(0.0006)	(0.0006)
Functional Breadth of Special Districts	0.269***	0.295***
	(0.0292)	(0.0701)
Citizen Ideology Index	0.0072	0.0154**
	(0.0081)	(0.0072)
State Government Ideology Index	-0.0120***	-0.0127***
	(0.0024)	(0.0025)
P-value for Hansen's J		0.4746
Kleibergen-Paap rk Wald F Stat		111.99
Results of IVs In The First Stage		
1st IV: Interaction Between 1st Wave of TELs and		-0.0032***
Relative Change of Citizen Conservativeness		(0.0002)
2nd IV: Tension Between Citizen and State Government		-0.0003***
		(0.0001)
Observations	24,285	21,246
R-squared	0.932	0.09
County FE & Year FE & Regional Time Trends	Yes	Yes
Intergov.Aid & Functional Breadth Lag	No	Yes

The dependent variable is the number of special districts per county. Clustered standard errors are in parentheses: *** p<0.01, ** p<0.05, * p<0.1

Chapter 2:

How Does School District Consolidation Affect Property Values?

A Case Study of New York[1]

2.1 Introduction

Over the past century, many small school districts that were established as this nation was settled have gradually been consolidated, and the number of districts has been cut by almost 90 percent (National Center for Education Statistics (NCES) 2006, 2011). Despite this dramatic change, many states still contain several school districts with enrollment below 500 pupils, which are good candidates for consolidation. The principal justification for consolidation is economies of scale in the provision of public education, which have been documented by many studies (Andrews, Duncombe, and Yinger 2002; Duncombe and Yinger 2007). School district consolidation, however, may also have negative impacts on education quality by affecting student motivation, parental involvement in schools, or parental access to teachers. Moreover, the cost and quality of public schools may influence the attraction of a community to potential residents and thereby affect local property values (Nguyen-Hoang and Yinger 2011). This paper combines these two issues by examining the impact of consolidation on housing prices, that is, by determining whether the market value of single family houses goes up or down when consolidation occurs. Building on previous research by Brasington (2004) and Hu and Yinger (2008), we examine the impact of recent consolidations on housing prices in New York State.

An investigation into the property value impact of consolidation provides information

[1] This paper was conducted in collaboration with Professors William Duncombe and John Yinger and published in *Public Finance Review*. It is cited in the third chapter as Duncombe et al (2016).

about the way home buyers view consolidation and therefore contributes to the debate about state policies that affect school district size. Despite extensive consolidation in the past, some states still provide incentives to consolidate. Several states, including New York, have aid programs to encourage district "reorganization," which usually means consolidation (NCES 2001). Transportation or building aid formulas also encourage consolidation in some cases (Haller and Monk 1988). Surprisingly, however, these pro-consolidation policies may be accompanied by policies that discourage consolidation, such as operating aid formulas that compensate school districts for small enrollment or for small enrollment per square mile (Huang 2004).

This paper reviews the literature on consolidation, with a focus on the impact of consolidation on property values, describes the data and methods we employ, and presents the results from our estimation. A final section presents our conclusions.

2.2 Literature Review

In principle, consolidation enables school districts to benefit from economies of scale, obtain new aid from state government, and provide students with more course options. According to the New York State Department of Education (NYSED, 2013), more than 30 school district consolidations have taken place in New York State since 1980. Regardless of potential cost savings, however, school district consolidation is sometimes difficult to achieve. Consolidation may increase travel time for students and therefore impose additional transportation costs on households (Kenny 1982). Residents may fear a loss of local identity or worry that their own community is not compatible with potential partner districts (NYSED 2013). Moreover, high-income households are less likely to support consolidation, perhaps because they place a high value on personal contact with teachers or good access to schools and

school activities (Duncombe and Yinger 2007; Hu and Yinger 2008). In addition, students in larger school districts may have weaker connections to the school community, a less positive attitude toward school, and a lower motivation to learn (Cotton 1996; Barker and Gump 1964; Duncombe and Yinger 2007). Finally, consolidation may raise concerns about representation on the Board of Education or about whether the new district will operate as expected (NYSED 2013). These possibilities help to explain why some consolidation propositions failed to pass in New York, even when the districts involved are relatively comparable.

Several scholars have written about the determinants of consolidation (Brasington 1999, 2003; Gordon and Knight 2006) and about the impacts of consolidation on the cost of education (Andrews, Duncombe, and Yinger 2002; Duncombe and Yinger 2007). This literature indicates that districts with widely disparate characteristics, such as property values, are unlikely to choose to consolidate. Brasington (2003, p. 687) also finds that "greater differences in income and racial composition discourage consolidation." When two small districts do consolidate, however, they are likely to experience some adjustment costs but to save money overall because of economies of scale. Duncombe and Yinger find that these economies are considerably smaller in percentage terms for two consolidating districts with 1,500 pupils each than for two 500-pupil districts.

Recent articles by Brasington (2004) and Hu and Yinger (2008) explore the property-value impact of consolidation. Property value impacts reflect cost savings, but also raise additional issues because they may reflect parental concerns that do not appear in school districts' budgets, such as parental access to teachers or the time parents and students spend getting to school. Indeed, Brasington finds that once one controls for changes in student test scores and property tax rates, consolidation has a negative impact on property values.

Hu and Yinger provide an accounting of the channels through which consolidation affects house values. The first broad channel reflects changes in the quality of local public services. Consolidation leads to increases in state aid to education and, due to economies of scale, it leads to a decline in the marginal cost of public services. Both of these impacts give voters an incentive to demand higher school quality. The impact of each additional unit of school quality on house value is weighted by the difference between the marginal benefits it provides and the taxes it requires. The second broad channel reflects changes in the cost of providing existing school services. This channel has four parts. First, consolidation may raise the cost of off-budget services, such as parental access to teachers or the time it takes students to get to school. Second, consolidation may lead to economies of scale, which means that it may result in a lower cost per pupil to provide the same level of school services. Third, consolidation may involve adjustment costs, such as re-organizing staff or re-designing bus routes. Fourth, consolidation brings in state aid. The first and third parts are likely to have a negative impact on house values, whereas the second and fourth are likely to have a positive impact.

The empirical work by Hu and Yinger, which is based on census tracts in New York State, finds that the impact of consolidation on house values depends on the enrollment change associated with consolidation. As expected from the literature on economies of scale, the property value impacts are largest for the smallest consolidating districts, and positive impacts fade out once a district size of about 1,000 pupils is reached. Hu and Yinger also show that all the impacts of consolidation on property values are weighted by house value. This theoretical prediction is upheld in their empirical work, which finds that consolidation has a strong positive impact on house values in census tracts with low average house values and a strong negative impact where average house value is high.

One final lesson from previous studies is that one must be careful to recognize that changes in property values over time may reflect both changes in the value people place on the school services they receive in each district and changes in household sorting across school districts (Figlio and Lucas 2004, Bogin 2011, Nguyen-Hoang and Yinger 2011). Households that strongly prefer the access to teachers they receive in a small district, for example, may leave a school district when it consolidates. As a result, changes in house values when a district consolidates reflect some combination of changes in the willingness to pay of people with the same income and preferences as those originally in the community and changes in the types of people who decide to move there. One way to estimate the impact of consolidation on housing prices is to use a sample of house sales over time to estimate a hedonic regression with house value as the dependent variable and with an indicator variable equal to one in districts where consolidation has occurred. The most obvious problem with this approach is that the decision to consolidate may be influenced by unobserved factors that also influence house value. Brasington (2004) addresses this problem using spatial statistics; Hu and Yinger (2008) use an instrumental variables procedure. An alternative approach to this endogeneity problem, which is used in this paper, is to focus on the change in price for houses that sell twice. This difference-in-difference approach compares the change in housing prices in districts that consolidate between sales to the change in housing prices in districts that did not consolidate. As a result, all time-invariant factors that influence the decision to consolidate are differenced out. This approach does not account for time-varying factors that might influence consolidation, but, as discussed in detail by Duncombe and Yinger (2007), consolidation is a long process in New York State and short-run changes are unlikely to influence the consolidation decision.

A limitation of these studies is that the districts that consolidate and the houses they

contain may be systematically different from other districts (and their houses). Under these circumstances, a comparison of house sales in treatment (i.e. consolidating) and control (non-consolidating) districts may yield biased results whenever the impact of consolidation depends on district or house traits. This problem can be addressed using a propensity score matching (PSM) estimator, which accounts for the possibility that the impact of consolidation depends on district and house characteristics. With this approach, the first step is to estimate a series of propensity score regressions, which predict the probability that a house sale with certain housing and district traits will be in a district that consolidates. When combined with a weighting scheme, each regression can then be used to select a comparison sample, that is, a set of observations in districts that did not consolidate that are otherwise equivalent to the observations in consolidating districts. Each comparison sample can then be examined with a series of balancing tests to determine whether the distributions of the explanatory variables are similar in "treatment" and "comparison" samples. If they are, then the possibility of bias from an interaction between "treatment" and explanatory variables is minimal. Details of our differencing and matching procedures are presented below.

2.3 Data and Measures

The data used in this analysis come from two sources. Property sales information and housing characteristics were provided by the New York Office of Real Property Services (ORPS) in the New York State Department of Taxation and Finance (NYDTF). Data from 2000 to 2012 are available in the "Sales" database, which includes 10 years of parcel-level property sales information for the state.[2] Property transfer reports are filed with the County Clerk and

[2] Information on the Sales database is available from NYDTF [Online] http://www.tax.ny.gov/research/property/assess/sales/localoff.htm (acc. December 10, 2013).

forwarded to NYDTF. The Sales database includes information on property location, class, sales date, and sales price. We include in this analysis only arms-length transactions of 1-to-3 unit family homes, constructed for year-round residence with a sales price greater than $10,000.

Information from the Sales database was merged with the detailed parcel-level data included in the Real Property System (RPS) database. The Real Property System collects information from local assessors on a number of parcel characteristics. For this analysis, we use information on the characteristics of residential owner-occupied parcels in 2003. We selected 2003 for two reasons. First, two pairs of districts consolidated in 2004, so this base year gives us the pre-treatment information we need to implement PSM. Second, we have complete parcel-level data for 2003. These data include measures of quality/condition, size, and the availability of special features for each house. Quality measures include an assessment of the overall physical condition of the residence (including interior and exterior walls, foundation, kitchen, baths, heating, plumbing and electrical systems).[3] A second quality measure is an assessment of the construction grade of the house, which refers to the quality of the material and workmanship used to construct the house. Additional variables include the number of square feet of living area in a house, the number of bedrooms, and the number of full bathrooms. Special housing features in the data set include whether the house has a full basement, central air conditioning, or a fireplace. Finally, the data set contains an estimate of the house age at the time of sale.[4]

We also collected information about the demographics and socio-economic status of residents in the 2000 census tract of each house sale. Specifically, our data set includes measures

3. More information on the house characteristics used in this analysis can be found in the Assessor's Manual produced by NYDTFe [Online] http://www.tax.ny.gov/research/property/assess/manuals/assersmanual.htm (acc. Dec. 10, 2013).
4. Information on the RPS data comes from NYDTF [Online] http://www.tax.ny.gov/research/property/assess/rps/index.htm (accessed December 10, 2013).

of age, race and ethnicity, gender, poverty, income distribution, distribution of house values, educational attainment, enrollment in public schools, employment by industry and occupation, and unemployment. In short, we have housing sales information from 2000 to April 2012, along with housing characteristics data and census-tract information before 2004 when the first consolidation in our data set occurred. We merge the three datasets together by assigning house characteristics in 2003 and census tract information in 2000 to the dual-sale houses we observe. We assume that the houses and neighborhoods in our sample have not experienced much change during the sample period—except for, a common time trend and, in some cases, consolidation.

2.4 Methodology

Compared with traditional regression analysis, matching methods have two key advantages for our study. First, matching methods introduce some of the advantages of a randomized experiment into a study based on observational data. Second, matching methods reduce the sensitivity of results to model-based and inherently untestable assumptions (Stuart and Rubin 2007). Matching methods and regression-based model adjustments are not mutually exclusive, however. In fact, many scholars argue that the best approach is often to combine the two methods by conducting regression adjustment on balanced samples (Heckman et al. 1997, Abadie and Imbens 2006, Ho et al. 2007). The logic behind this combined empirical strategy is that the matching methods make it possible to reduce large covariate bias between the treated and control groups, and the traditional regression methods can be used to adjust for any residual bias and to increase efficiency (Stuart and Rubin 2007).

2.4.1 Selection Problems and Sample Selection

Between 2000 and 2012, three sets of districts consolidated in New York State.[5] The three sets of districts include Canesteo Central School District (CSD) (921) annexing Greenwood CSD (429 students) in 2004, Eastport Unified School District (UFSD) (1,042 students) annexing South Manor UFSD (936 students) and Eastport-South Manor Central High School District (1,159 students) in 2004, and the North Colonie CSD (5,646 students) annexing the Maplewood Common School District (147 students) in 2008. These districts are geographically dispersed across the state (Albany County, Steuben County, and Suffolk County). The combined districts ranged in size (at the time of consolidation) from 1,250 to 5,793 students.

For three reasons, the sample of parcels in the consolidating districts included in this analysis is not likely to be representative of the rest of the state. First, since relatively few districts in New York consolidate, despite generous financial incentives from the state, districts that do consolidate are not likely to be typical even of small districts in the state. Brasington (1999) found that differences in the student population increased the probability of consolidation, while differences in property values per pupil decreased this probability. Gordon and Knight (2009) found that increasing heterogeneity in the share of adults with a college degree and in district spending are negatively associated with the decision to merge. Second, only a small subset of houses experienced two sales from 2000 to 2012, which implies that these houses may not be representative of houses in the state. Third, we limit our sample in consolidating districts to only those with a sale before and after consolidation. As a result this study should be regarded as a case study about the impact on house values of three specific consolidations, not as a study

5. In all three cases one district annexed the other district(s), but voters in all districts approved. For more information on consolidation, see the NYSED (2013).

about the impact of consolidation in general.

Another methodological challenge is that districts with certain unobserved housing or neighborhood traits may be more likely than others to consolidate. These types of unobservable factors might lead to biased estimates of the effects of consolidation on housing prices. To address this problem, we use dual sales data to estimate a difference–in-difference regression. This approach accounts for time-invariant unobservable factors, but does not rule out the possibility of bias from changes in unobserved traits that are correlated with consolidation.

In addition, we used PSM to select a comparison group that is as similar as possible to the treatment group in terms of observable characteristics. Several steps are involved in finding a comparison group with PSM.[6] First, a set of "confounding" variables need to be identified; these variables should be related to both the treatment (the decision to buy/sell a house in a consolidating district) and the dependent variable (property value change). We selected a set of housing characteristics and neighborhood characteristics that are likely to be related to housing prices. These variables include income, property values, racial heterogeneity among enrolled students, and parental educational attainment, which have been found to be related to district consolidation decisions.[7] Model selection was based on both improving the fit of the model and finding a sample that meets the common support criteria.[8] As part of the process of finding a balanced sample, we explored the use of squared and interaction terms. Table 1 describes the

6. For more on PSM see Guo and Fraser (2010), Steiner and Cook (forthcoming), Heinrich, Maffioli and Vazquez (2010), Caliendo and Kopeinig (2005), and Becker and Ichino (2002).
7. We also estimate a propensity score model that includes local revenue effort (= local revenues divided by property value), since it may be related to the impacts of consolidation. This approach leads to a sample of 482 observations and to a significant impact estimate (-7.25 percent, significant at the 1 percent level), but this sample fails one key balancing test.
8. See Heinrich, Maffioli and Vazquez (2010) and Caliendo and Kopeinig (2005) for more on strategies for variable selection. Models with variables measuring differences between districts and their neighbors in demographics, fiscal capacity, and student need explained over 90 percent of the probability that a parcel was in a consolidating district, but led to little overlap in propensity scores between treatment and comparison groups.

variables used in the final PSM regression model.

The results of the PSM logistic regression are reported in Table 2. Parcels in consolidating districts are more apt to be younger, in bad condition, have a higher grade of construction material, larger in terms of square feet and the number of bedrooms and bathrooms and to have central air conditioning. These homes are less apt to have a full basement and a fireplace, on average. Among census tract variables, the percent of adults with a BA or higher, the share of the population less than 20 years-old, and the share of owner occupied houses have a nonlinear relationship with the probability of being in a consolidating district (an inverted U-shape). The child poverty rate and percent of adults in profession or managerial occupations are negatively related to consolidation, while the share of low value houses (below $50k) and the share of employment in government are negatively related.

The PSM model was then used to develop propensity scores for a parcel being in a consolidating district. We first limit the sample to the area of common support by removing observations outside the region where the propensity scores of treatment and control observations overlap. This step reduced the number of dual sales by 10 percent in consolidating districts and by 5.8 percent in non-consolidating districts. With this trimmed sample, we identified comparison groups using several matching strategies.[9] We focus on the results from a 1-to-1 nearest neighbor match without replacement because best meets on the balancing tests[10]. The matches are limited to a caliper around the propensity score for the treatment group

9. We conduct the matching using the STATA program"psmatch2" developed by Leuven and Sianesi (2003) and explained by Guo and Fraser (2010).
10. We also tried 1-to-1 nearest neighbor matching with replacement, which resulted in a reduction of 40 observations. The new sample passed the standardized bias test but not the t-test; results based on it indicate that consolidation, on average, lowered housing price by 2.98 percent (significant at 6.3 percent level)—a result that is similar to the one in Table 4.

member.[11] The result is a sample of 271 treatment and 271 comparison parcels[12]. Interestingly, more than half of the houses in this sample represent repeat sales in the consolidating districts in Albany or their matching partners in other areas. The unequal distribution of the sample reinforce the conclusion that our study cannot be interpreted as a general treatment of the impact of consolidation. We return to this issue when discussing our results.

The final step in developing a matching sample is checking the balance between the treatment and comparison parcels. The simplest approach is to compare mean values for each variable in the two groups. If the matched sample is balanced we should not see any statistically significant differences in the variables in the propensity score model or in the propensity score. As indicated in Table 3, this is the case for this matched sample. It is also important to check whether the difference between treatment and comparison group values (bias) is below some acceptable level. A simple test is to compare the standardized bias (difference in means divided by standard deviation of the respective X variable). One standard that is used is that this bias should be below 0.25 (a quarter of a standard deviation).[13] As indicated in Table 3, none of the confounding variables or the propensity score have a standardized bias above 0.25. So our matched sample passed this test, as well[14].

11. Guo and Fraser (2010) recommend using a caliper of 0.25 standard deviations for the log of the odds ratio from the logistic regression (log(P/(1-P), where P is the propensity score). We set a tougher standard by limiting matches to within 0.15 standard deviations.

12. We deleted a few (13 observations) outliers because they experienced large changes in housing traits. For example, some of them installed central air conditioning system during the time between the first sale and the second sale; some expanded the square feet of living area; and some "unreasonably "experienced more than a 50 percent price change within one month.

13. See Ho et al. (2007) and Stuart and Rubin (2008) for a discussion of this approach. Ho et al. (2007) are critical of using hypothesis testing to check balance and prefer quantile-quantile (QQ) plots. This alternative test could be attempted in future research.

14. Another test suggested by Sianesi (2004) and Caliendo and Kopeinig (2005) is to determine whether the pseudo-R^2 and likelihood ratio chi-square statistic for the propensity score regression are significantly smaller for the matched sample than for the sample as a whole. We implemented this test using the Stata "pstest" program of Leuven and Sianesi (2003). Compared to the full sample, the pseudo-$R^2$for the matched sample decreases from 0.419 to 0.068 and the likelihood ratio chi-square declines from 1466.9 to 51 but is still significant. The matched sample also passes this test with 1:5 nearest-neighbor matching and kernel

2.4.2 Regression Models

To estimate the impact of consolidation on housing prices, we must first decide on the best way to set the date of a particular consolidation. For most districts, three official dates are associated with consolidation: 1) the date of the advisory referendum or filing of petitions; 2) the date of the official referendum on reorganization in each consolidating district; and 3) the actual date of consolidation. While advisory votes are common, they do not take place in all cases of consolidation and their results are non-binding. Instead, we think that the date of the official referendum or the start of the first school year in the consolidated districts are likely to frame the time when home owners of the district become aware that reorganization is taking place. However, for the three consolidating districts in this paper, we find big time difference between these two dates ranging from six to eight months. Due to the time gap, we decided to use the date of the official referendum on reorganization as the start of consolidation[15].

With a well matched sample it is possible to determine the average effect of the treatment on the treated (ATT) by comparing the means of the treatment and comparison groups. Since our impact measure is the change in house sale price before and after consolidation, this would be equivalent to a difference-in-difference method (Guo and Fraser 2010; Heinrich, Maffioli, and Vazquez, 2010). While this approach has the potential to control for time-invariant unobservable differences across treatment and comparison, this comparison has some limitations in our case.

matching but not with Mahalanobis matching. Because these results are ambiguous and this test has no obvious advantages over others, we rely on the balancing on the tests presented in the text.

15. We prefer the date of the official referendum as the consolidation date for two reasons. First, residents are likely to know about the reorganization plan by the date of the referendum. Second, if we use the start of the first school year after consolidation, house sales during the time gap will become first sales instead of second sales, thereby reducing our sample size and potentially introducing measurement error. Nevertheless, changing the start date to the beginning of the first academic year for the consolidation (July 1) serves as a robustness check. With this change our sample has 47 fewer observations but the results are similar; for example, consolidation decreases housing prices by 3.6 percent, on average (significant at the 4.5 percent level).

Under the classic difference-in-difference comparison the treatment is applied at the same time to all treatment group members and the pre- and post-observations are made at the same time. Neither of these conditions is met in this study. Not all of the districts consolidated at the same time and very few of the house sales in our sample occurred on the same day.

To account for these timing differences, we developed a regression model for the matched sample. The dependent variable is the change in the log of house value. We calculated the months between the first sale and the starting date in our sample (January 1, 2000), and between the second sale and the starting date. One control variable is the difference in months between these two measures (D) and a second is the difference in the squares of these two measures (D^2). If t_2 represents the time for the second sale, t_1 the time for the first sale, and t_0 represents the starting date (January 1, 2000), then these measures can be represented as:

$$D = (t_2 - t_0) - (t_1 - t_0) = (t_2 - t_1)$$
$$D^2 = (t_2 - t_0)^2 - (t_1 - t_0)^2 \quad (1)$$

The second measure captures not only the time difference between the first and second sales but also where these sales fall relative to the beginning of the decade. Given recent changes in the housing market, it is important to control for the point in the decade when the sales occur.

Because the effects of consolidation on property values may take time to emerge, we also need to account for where the second sale occurred relative to date of consolidation. We devised measures similar to those in Equation (1) to capture the difference in months between the second sale and the date of consolidation (M) and the square of this measure (M^2). If t_c represents the date of consolidation, then these measures can be represented as

$$M = (t_2 - t_0) - (t_c - t_0) = (t_2 - t_c)$$
$$M^2 = (t_2 - t_c)^2 \quad (2)$$

As discussed earlier, Hu and Yinger (2008) found that the impact of consolidation varies across districts and census tracts. Specifically, they found that the positive effects of consolidation on property values declines with enrollment and that the impact is the highest in low-property-value tracts. Given that the North Colonie CSD in Albany has substantially greater pre-consolidation enrollment than all other six consolidating school districts, we hypothesize that the impact of consolidation on property values is smaller in this district than elsewhere. To test this hypothesis, we interact the dichotomous variable for whether the district has consolidated (*C*) with a dummy variable for the large North Colonie CSD (*L*). We also test the Hu/Yinger finding that the property-value impact of consolidation is smaller in wealthier census tracts by interacting C with the average income in the census tract (*I*). In both cases, interactions with *M* and M^2 are included in the specification, as well. The two major models for the change in the log of property values (DV) can be represented as:

$$DV = \alpha_0 + \alpha_1 D + \alpha_2 D^2 + \alpha_3 C + \alpha_4 M + \alpha_5 M^2 + \alpha_6 (CL) + \alpha_7 (ML) + \alpha_8 (M^2 L)$$
$$DV = \beta_0 + \beta_1 D + \beta_2 D^2 + \beta_3 C + \beta_4 M + \beta_5 M^2 + \beta_6 (CI) + \beta_7 (MI) + \beta_8 (M^2 I) \quad (3)$$

We also estimate models with both interaction sets to see if these two effects can be separated.

2.5 Impact Estimates

Table 4 presents impact estimates with several different specifications. Model 1 just includes the consolidation dummy (*C*) and the measures capturing the months between first and second sale (*D* and D^2). The coefficient of this dummy is negative and significant at the 9.5 percent level. It indicates that, on average, consolidation lowers house values by about 2.6

percent during our sample period. (The change in the log of house value is approximately equal to a percentage change.) The results for Model 2 indicate that this change is not constant over time. Although the coefficients of the two timing variables, M and M^2 are not statistically significant, their magnitudes suggest that house values decline in the years right after consolidation, but then start to increase after about two years and a half. By around five years (61 months) after consolidation, house values have returned to their initial values and then increase beyond that point. These results are illustrated by the solid line in Panel A of Figure 1.

Model 3 adds interactions between C, D, and D^2 and a dummy for the North Colonie CSD. [16] Although the first interaction term is only significant at the 9.5 percent level, the other two terms (and the three terms taken together) are highly significant. Moreover, the introduction of these interaction terms leads to significant coefficients for two of the three original variables and for the set (the first three rows). Indeed, as shown by the dotted line in Panel A of Figure 1, consolidation has a striking effect on house values in the six districts other than North Colonie. The initial impact of consolidation is positive (12.1 percent) but not statistically significant. From that point on, however, the pattern in Model 2 arises, but in much stronger form. The estimated net effect of consolidation on house values in the six districts equals zero after about six months and then becomes negative and significant, reaching -26.8 percent after 31 months. At this point the impact turns around, gets back to zero in 57 months, and then becomes positive. At the largest post-consolidation time period observed in the data, 65 months, the impact of consolidation is positive, as in Hu and Yinger (2008), but this impact, 18.7 percent, is not quite significant. In short, consolidation leads to a significant decline in house values for the first 2 ½

16. A model containing interactions with district enrollment leads to similar results.

years, but this decline is then reversed and disappears by 5 years after consolidation takes place.

We believe that these results probably reflect two effects. First, people may be skeptical about consolidation, but then come to appreciate its cost savings and the broader sets of high school courses that tend to accompany it. Second, people who dislike consolidation may leave consolidating districts and be replaced by people who prefer it or who are willing to pay more to get it. We look for signs of sorting in the form of changes in the composition of local residents in consolidating districts, but substantial differences in demographic and economic characteristics across consolidating and non-consolidating areas have not appeared recently. One plausible explanation is that it may take a while for sorting to be noticeable.[17] These effects do not arise in North Colonie because the consolidation we observe there represents a small change in district enrollment and because North Colonie was already so large before consolidation that further cost savings from economies of scale probably were not possible.

Model 4 in Table 4 adds interactions between initial tract average household income and the three consolidation variables in Model 2. Two of these interaction terms (and the three together) are statistically significant. The resulting pattern, illustrated in Panel B of Figure 1, shows that the impact of consolidation over time is heavily influenced by tract income. Indeed, the solid line in Panel A of Figure 1 is magnified in low-income tracts, moderated in middle-income tracts, and reversed in high-income tracts. In the case of low-income tracts, the decline in house values at 30 months for low-income tracts, -26.9 percent, and the increase in house values

17. We test for sorting by examining whether the composition of residents in consolidating districts has changed, compared with both non-consolidating districts during the same time and consolidating districts in earlier periods. We find more adults with a bachelor degree or higher, fewer families with income below $20k, a lower poverty rate for persons under 19, and a bigger percent of owner-occupied houses in consolidating areas in 2006-2010 American Community Survey (ACS) data than in 2000 census data. But significant differences in census traits are not found across consolidating and non-consolidating districts in the 2006-2010 ACS dataset.

at 65 months, 27.5 percent, are both statistically significant. Moreover, the decline in house values at 65 months in high-income tracts, 26.4 percent, is significant, as well. Changes in house values over time are not significant in middle-income tracts.

These income results are consistent with the findings of earlier studies but add a time path. As in Hu and Yinger, the ultimate impact of consolidation is to raise house values in low-income tracts and to lower house values in high-income tracts. We also find, however, that the impact of consolidation on house values in low-income tracts is negative at first and only becomes positive after about five years. Moreover, the impact in high-income tracts is minimal at first but eventually becomes significantly negative. As discussed earlier, these patterns reflect some combination of changing views about consolidation as it is experienced and changes in the type of households living in consolidated districts. One possibility is that people in low-income tracts are particularly skeptical about consolidation at first but then come to appreciate its costs savings, whereas high-income people do not expect much from consolidation but eventually become discouraged by the loss of contact with teachers or by some other change that accompanies it. These finding also could reflect changes in the types of people who move into a district once it has consolidated.

We also estimated models that included interactions with both the dummy for North Colonie and initial tract income. Because North Colonie has a relatively high income, however, we were unable to separate these two sets of interactions. In other words, none of the interaction terms is significant in this specification. Thus, we cannot determine whether our results reflect variation in enrollment, variation in initial income, or some combination of the two. Additional research with larger data sets is needed to separate the impacts of these two factors.

2.6 Robustness Checks

We also examine the sensitivity of our results to differences in the sample, estimation method, and matching algorithm.[18,19] Tables 5 and 6 compare estimation results based on our main strategy and three alternatives.[20] We first check whether the results change significantly if we confine the comparison group to the county in which the consolidation occurred or to that county's neighbors. The comparison group for the two consolidated school districts in Albany, North Colonie CSD and Maplewood CSD, for example, consists of houses that have sold twice between 2000 and 2012 in Albany, Columbia, Greene, Montgomery, Rensselaer, Saratoga, Schenectady and Schoharie Counties. By restricting the sample to neighboring regions, we eliminate potential biases from unobserved factors that vary across regions of the state.

The second alternative is to include propensity scores as weights in a weighted regression model (Steiner and Cook forthcoming; Guo and Fraser 2010). The advantage of this approach is that we are able to use all treatment and comparison observations in the area of common support. The disadvantage is that results can be sensitive to outliers (i.e., propensity scores close to zero). Finally, because impact estimates with PSM can also be sensitive to the matching algorithm, we also looked at several other matching strategies, and some of them

18. A reviewer suggested that a weighted repeat sales index might be inappropriate when it is based on a small sample size. Given that we just have 529 observations in the balanced sample, we conduct a robustness check based on housing price per square foot. The average house area is 1669 sq. ft. and school district consolidation decreases housing price per sq. ft. by $1.68 or by $2,803 (=(1.68)(1669) overall. This is consistent with results based on price alone.

19. A reviewer also suggested that our results might be biased by the inclusion of real-estate owned (REO) properties and foreclosure sales. A decline in price for these houses cannot be simply attributed to a change in the demographics of their community or to residents' perceptions about consolidation. We find that only 6 houses (1.13% of our sample) are in the category "conditional government sale" in which distressed sales are likely to fall. After eliminating these 6 cases, the consolidation coefficient becomes -0.029 (p-value = 0.056), suggesting that houses in this category have little impact on our results.

20. Following Dinardo (2002), we also calculate both average treatment effects (ATE) and average treatment effects on the treated (ATT) on the full sample; the estimated effects of consolidation are -4.1 percent and -6.5 percent, respectively. Since this approach is sensitive to extreme low or high values of the propensity score (PS), we also estimate our model after deleting observations with a PS below 0.1 or above 0.9. The estimated effects are -7.9 percent and -6.5 percent. These results are consistent with those in the text, but assume, incorrectly in our case, that all consolidations happen at the same time.

passed balancing tests. In the sample selected by 1-to-5 nearest neighbor matching, for example, none of the variables is significantly different between the treatment group and control groups.[21]

Panel A of Table 5 indicates that Model 1, with just one consolidation variable, yields similar results for all four methods. The range in estimated coefficients is -0.0174 to -0.0342 and two of the estimates are significant at the 5 percent level. Panel B makes the same comparisons based on Model 4 in Table 4. The magnitudes and significance of the variables are similar across the 4 columns of this panel.

As a final check, we looked at the distribution of predicted sales prices changes from Model 4 using these different methodologies. As shown in Table 6, the results are quite similar. The median predicted house value change, for example, ranges from -2.83 percent (the fourth column) to -4.53 percent (the second column).

2.7 Conclusions

This paper explores the impact of school district consolidation on house values using a sample of house sales in New York State from 2000 to 2012. During this period, three sets of districts consolidated. We use propensity score matching to identify house sales that are comparable to the sales in these districts and then use double-sales data to compare house value changes in consolidating and comparable districts.

We find that, on average, consolidation has a small negative impact on house values, at

21. The five closest comparison group observations within the caliper (0.15 standard deviations) are matched with each treatment group member. The final sample was 764 comparison group members and 271 treatment group members. This matching was done with replacement using the "psmatch2" Stata program developed by Leuven and Sianesi (2003).

least outside of districts that are relatively large before consolidation. This average impact reflects a downward trend in house values right after consolidation combined with a positive trend starting two years and a half later. After five years, house values have returned to their pre-consolidation level and then begin to rise above house values in comparable districts. These results suggest it takes a while either for the advantages of consolidation to be apparent or for the people who prefer consolidated districts to move in. Finally, the long-run impacts of consolidation on house values are positive in low-income tracts, but negative in high-income census tracts, where parents may have a relatively large willingness to pay for the access to teachers and other non-budgetary advantages of small districts.

Our results reveal a clear time pattern of consolidation-induced changes in house values. This pattern appears to reflect some combination of learning about the consequences of consolidation and differences in the preference of households who move into districts before and after they consolidate. With the data available to us, however, we cannot test these hypotheses about the pattern of house value effects or separate the impact on this pattern of initial district enrollment and of initial neighborhood income. These are topics for future research.

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Table 2.1. Summary Statistics for Sales Price Difference and Variables Used in Propensity Score Model

Variables	Mean	Standard Deviation	Minimum	Maximum
Difference in sales price adjusted for inflation	18960.65	63963.01	-7820259	2858065
2003 House characteristics:				
Overall condition is good (yes-1)	0.004	0.062	0.000	1.000
Overall condition is poor (yes-1)	0.105	0.306	0.000	1.000
Grade A (yes-1)	0.004	0.060	0.000	1.000
Grade B (yes-1)	0.087	0.281	0.000	1.000
Full basement (yes-1)	0.727	0.445	0.000	1.000
Central air conditioning (yes-1)	0.187	0.391	0.000	1.000
Number of full bathrooms	1.403	0.590	0.000	11.000
Number of bedrooms	3.035	0.808	0.000	16.000
No fireplace (yes-1)	0.579	0.494	0.000	1.000
Age	48.721	36.103	0.000	312.000
Square feet	1,600.492	603.702	0.000	14,460.0
2000 Census tract variables:				
Percent of adults with a BA or higher	28.615	14.380	4.600	91.600
Poverty rate for persons under 19	9.212	8.213	0.000	65.800
Percent of families with income below $20k	9.960	6.913	0.000	61.300
Percent of houses with value below $50k	6.224	10.580	0.000	82.500
Percent owner occupied house	74.745	14.730	3.200	98.300
Percent of employment in government	6.206	4.047	0.000	26.344
Percent of professional occupations	38.055	11.046	9.648	75.544
Percent of population under 20	13.026	2.329	0.100	36.700
Percent of white in enrolled students	93.132	7.311	15.043	100.000
Number of observations	64483			

Source: New York State Department of Taxation and Finance, *Online Sales Report* and *Real Property System* database; U.S. Bureau of the Census, *2000 Census of Population and Housing.* Data on house characteristics from the RPS was available for 2003.

Table 2.2. Logistic Regression Results for Propensity Score Model

Variables	Coefficient	Standard Error	p-value
House characteristics:			
Overall condition is good (yes-1)	-2.0129	0.4917	0.0000
Overall condition is poor (yes-1)	0.6523	1.0486	0.5340
Grade A (yes-1)	0.3793	1.3389	0.7770
Grade B (yes-1)	0.9324	0.2641	0.0000
Full basement (yes-1)	-0.3046	0.1463	0.0370
Central air conditioning (yes-1)	0.4895	0.1561	0.0020
Number of full bathrooms	0.0896	0.1366	0.5120
Number of bedrooms	0.0875	0.1019	0.3900
No fireplace (yes-1)	0.4351	0.1505	0.0040
Age	-0.0065	0.0027	0.0160
Square feet	0.0007	0.0003	0.0170
Square feet squared	0.0000	0.0000	0.1420
2000 Census tract variables:			
Percent of adults with a BA or higher	0.7135	0.0498	0.0000
Percent of adults with a BA or higher squared	-0.0066	0.0006	0.0000
Poverty rate for persons under 19	-0.2349	0.0287	0.0000
Percent of families with income below $20k	0.0020	0.0305	0.9470
Percent of houses with value below $50k	0.1403	0.0109	0.0000
Percent owner occupied house	0.3990	0.0581	0.0000
Percent owner occupied house squared	-0.0031	0.0004	0.0000
Percent of employment in government	0.2899	0.0144	0.0000
Percent professional occupations	-0.3091	0.0212	0.0000
Percent of population under 20	2.5835	0.4417	0.0000
Percent of population under 20 squared	-0.1220	0.0193	0.0000
Percent of white in enrolled students	-0.0505	0.0216	0.0190
Constant	-31.9117	3.3604	0.0000
Number of observations	64483		
Pseudo R-squared	0.4373		
Likelihood ratio (chi-square)	1637.75		

Note: Dependent variable is whether district consolidated or not between 2000 and 2012.

Table 2.3. Test for Balance of Sample 1 (1-to-1 Nearest Neighbor Match)

	Full Sample				Sample 1			
	Mean in Districts				Mean in Districts			
2003 House characteristics:	Consol.	No Consol.	t-test (p-val)	Standard Mean Diff.	Consol.	No Consol.	t-test (p-val)	Standard Mean Diff.
Overall condition good (yes-1)	0.018	0.093	0.000	-0.2580	0.018	0.044	0.085	-0.1486
Overall condition poor (yes-1)	0.004	0.004	0.953	-0.0036	0.004	0.000	0.318	0.0861
Grade A (yes-1)	0.004	0.003	0.915	0.0065	0.004	0.000	0.318	0.0861
Grade B (yes-1)	0.062	0.085	0.167	-0.0834	0.063	0.077	0.502	-0.0578
Full basement (yes-1)	0.703	0.722	0.471	-0.0435	0.712	0.779	0.076	-0.1529
Central air conditioning (yes-1)	0.355	0.191	0.000	0.4161	0.362	0.354	0.858	0.0154
Number of full bathrooms	1.471	1.402	0.052	0.1174	1.480	1.483	0.951	-0.0053
Number of bedrooms	3.083	3.043	0.393	0.0515	3.092	3.037	0.408	0.0713
No fireplace (yes-1)	0.601	0.576	0.385	0.0524	0.598	0.550	0.260	0.0971
Age	43.562	48.173	0.032	-0.1294	43.288	44.487	0.609	-0.0440
Square feet	1657.300	1600.000	0.113	0.0955	1659.200	1703.300	0.438	-0.0668
Square feet squared (000)	3200	2900	0.156	0.0856	3200	3300	0.520	-0.0555
2000 Census tract variables:								
% adults with BA or higher	36.256	28.547	0.000	0.5877	36.646	36.028	0.464	0.0631
% adults with BA or higher sq.	1412.800	987.060	0.000	0.4767	1434.600	1398.700	0.547	0.0518
Pov. rate for persons under 19	4.144	8.292	0.000	-0.6239	3.912	4.103	0.653	-0.0387
% families with income < $20k	6.631	9.314	0.000	-0.4629	6.390	6.300	0.821	0.0195
% houses with value < $50k	4.557	5.991	0.021	-0.1391	3.743	5.699	0.074	-0.1543
% owner occupied houses	71.612	75.707	0.000	-0.3202	71.515	72.968	0.201	-0.1102
% owner occupied houses sq.	5281.200	5895.200	0.000	-0.3389	5269.600	5516.800	0.128	-0.1312
% of employment in gov't.	14.934	6.332	0.000	2.1088	15.098	15.834	0.102	-0.1408
% professional occupations	43.395	38.120	0.000	0.5145	43.668	43.441	0.762	0.0261
% population under 20	11.468	12.830	0.000	-0.6741	11.435	11.555	0.302	-0.0889
% population under 20 sq.	133.430	168.680	0.000	-0.6984	132.630	135.300	0.324	-0.0850
% white in enrolled students	92.245	93.858	0.000	-0.2918	92.124	92.426	0.358	-0.0791
Propensity Score	0.164	0.004	0.000	5.9403	0.1667	0.16727	0.9600	-0.0043
Number of observations	276	57768			271	271		

Note: The full sample was limited to the area of common support before the matching was done. Sample 1 is based on 1:1 nearest neighbor matching within a caliper set at 0.15 standard deviation of the log of the odds ratio. Sampling is done without replacement.

Table 2.4. Regression for Percent Change in Sales Price after Consolidation

(Based on Comparison Group Selected with a 1-to-1 Nearest Neighbor Match)

Policy-related Variable	**Model 1**	**Model 2**	**Model 3***	**Model 4****
Consolidation	-0.0256	0.0049	0.1209	-0.0106
	(0.095)	(0.875)	(0.152)	(0.704)
Months since consolidation		-0.0028	-0.0249	-0.0011
		(0.359)	(0.003)	(0.627)
Months since consolidation squared		4.59E-05	0.0004	5.49E-06
		(0.465)	(0.003)	(0.895)
Large school district-consolidation interaction			-0.1464	
			(0.095)	
Large school district-months since consolidation interaction			0.0265	
			(0.002)	
Large school district-months since consolidation interaction sq.			-0.0004	
			(0.002)	
Income-consolidation interaction				-0.1378
				(0.236)
Income-months since consolidation interaction				0.0254
				(0.013)
Income-months since consolidation interaction squared				-0.0005
				(0.004)
R-squared	0.4318	0.4333	0.4586	0.4489

Note: Dependent variable is change in log of sales price. Sample size is 529. Estimated with OLS regression with robust standard errors. Significance levels are in parentheses. All regression models include as control variables the propensity score, the difference in months between first and second sale and the difference in months between first and second sales squared.

*The large school district indicates the North Colonie CSD in Albany which has 5,464 students before consolidation, much larger than 772 students, the average pre-consolidation enrollment level of all other school districts in our sample. The coefficients of three interaction terms for that large school district are -0.0256, 0.0016 and -0.0000, respectively; none of them is significant.

** The income variable is de-meaned in order to make the results of Model 4 comparable to those of other models.

Table 2.5. Estimated Impacts of Consolidation on Sales Prices with Different Methodologies

	1:1 NN Matching	1:1 NN Matching (Neighboring Counties)	Propensity Score Weighted Regression on Full Sample	1:5 NN Matching
Panel A: Model 1				
Policy-related Variables	**Model 1**	**Model 1**	**Model 1**	**Model 1**
Consolidation dummy variable	-0.0256	-0.0342	-0.0300	-0.0174
	(0.095)	(0.025)	(0.022)	(0.193)
R-squared	0.4318	0.4622	0.3075	0.3503
Panel B: Model 4				
Policy-related Variables	**Model 4**	**Model 4**	**Model 4**	**Model 4**
Consolidation dummy variable	-0.0106	-0.0192	-0.0065	0.0034
	(0.704)	(0.488)	(0.839)	(0.905)
Months since consolidation	-0.0011	-0.0007	-0.0021	-0.0023
	(0.627)	(0.757)	(0.404)	(0.309)
Months since consolidation squared	5.49E-06	-5.05E-06	2.10E-05	3.14E-05
	(0.895)	(0.903)	(0.627)	(0.444)
Income-consolidation interaction	-0.1378	-0.1485	-0.2017	-0.1566
	(0.236)	(0.204)	(0.100)	(0.163)
Income-months since consolidation interaction	0.0254	0.0257	0.0284	0.0283
	(0.013)	(0.012)	(0.004)	(0.003)
Income-months since consolidation interaction squared	-0.0005	-0.0005	-0.0006	-0.0006
	(0.004)	(0.004)	(0.000)	(0.000)
R-squared	0.4489	0.4817	0.3255	0.3637

Note: Significance levels are in parentheses. NN stands for “nearest neighbor.”

Table 2.6. Distribution of Predicted Change in Sales Price after Consolidation

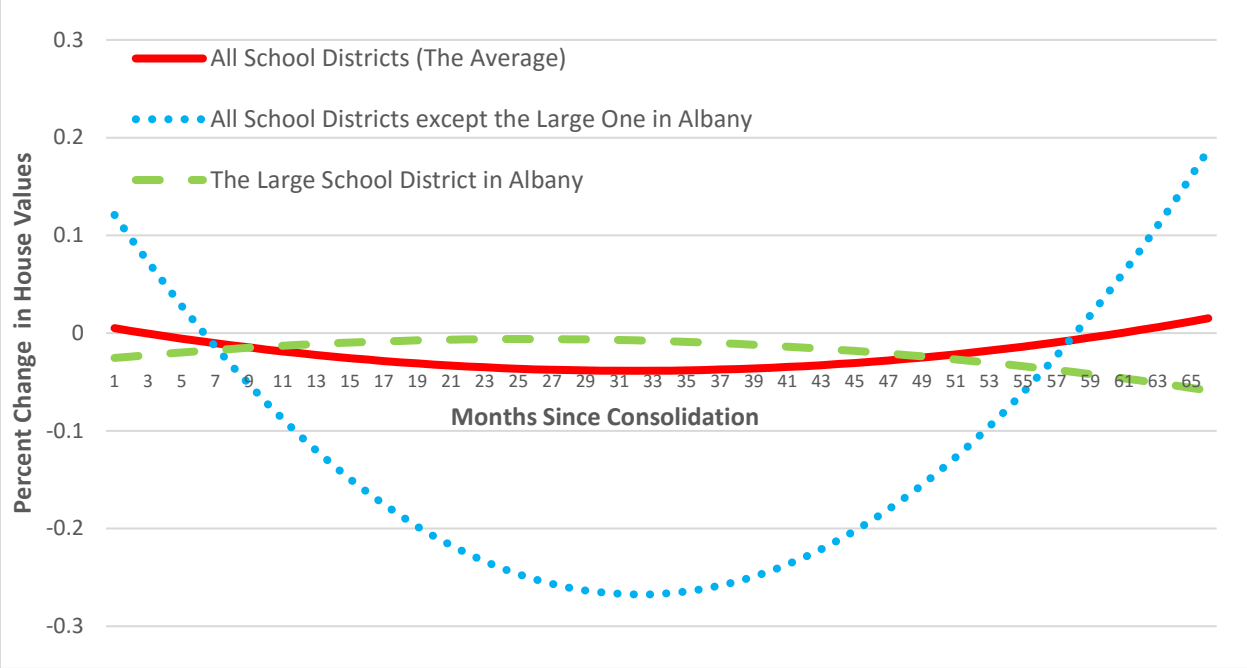
(Results for Model 4 with a Different Sample, Weighting Method and Matching Algorithm)

	1:1 NN Matching	1:1 NN Matching (Neighboring Counties)	Propensity Score Weighted Regression on Full Sample	1:5 NN Matching
Distribution	**Model 4**	**Model 4**	**Model 4**	**Model 4**
Mean	**-3.11%**	**-4.18%**	**-3.80%**	**-2.04%**
5th	-7.34%	-8.82%	-9.67%	-6.91%
10th	-6.40%	-7.85%	-7.52%	-5.40%
25th	-5.12%	-6.28%	-5.83%	-4.28%
Median	**-3.53%**	**-4.53%**	**-4.08%**	**-2.83%**
75th	-1.92%	-2.82%	-2.65%	-1.17%
90th	1.35%	0.16%	-0.83%	-2.94%
95th	3.73%	2.39%	1.78%	6.32%

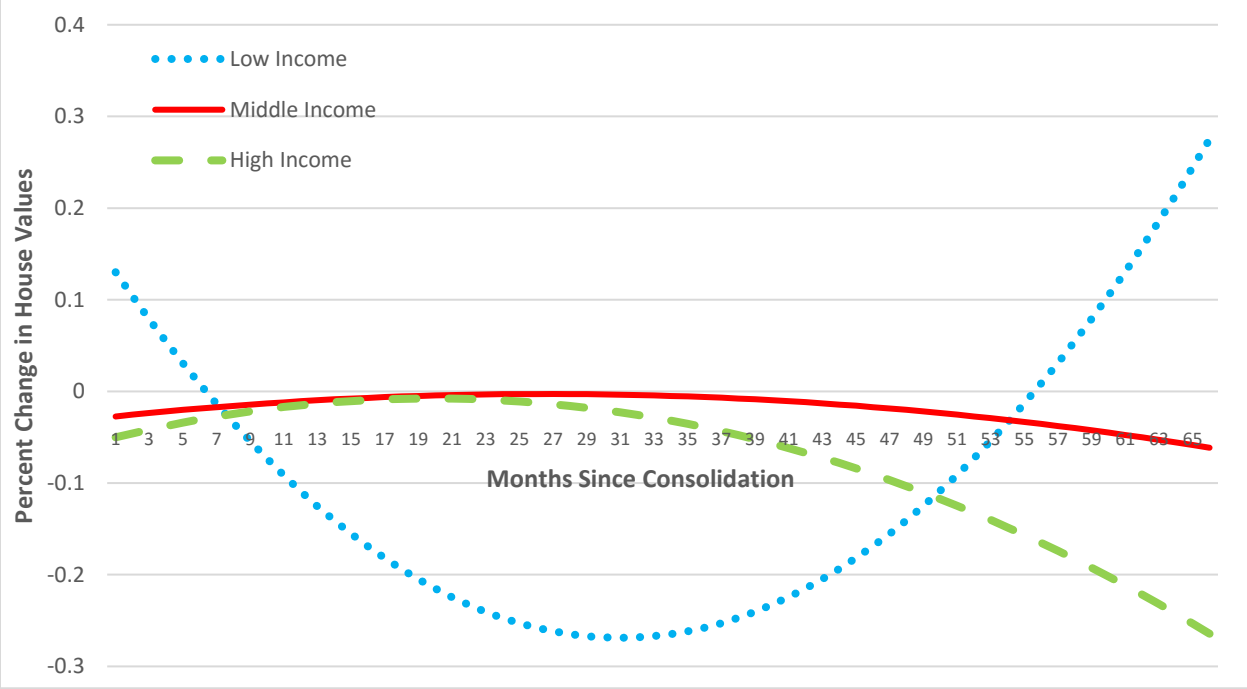
Note: Predicted percent change in the sales price between the second and first sale in consolidating districts minus what it would predicted without consolidation. This accounts for time between the second sale and date of consolidation.

Figure 2.1. Change in Property Values After Consolidation

Panel A: By School District Size



Panel B: By Census Tract Income (Adjusted Model 4)



Chapter 3:
How Does the Change of Local Government Structure Affect Property Values? A Case Study of Village Dissolution in Upstate New York

3.1 Introduction

In the federal system of the U.S., villages and townships typically embody the spirit of local autonomy as they are small enough for people to know each other, to easily assemble to debate and to conveniently exercise democracy. However, streams of institutional, economic and fiscal factors recently have been converging together and substantially changing the landscape of local government in the U.S., including New York (NY) State. First and foremost, a locality's municipal designation has many implications for governance, service provision and inter-governmental aid. But most of the cities, towns and villages were established prior to 1920 in NY State; since then many changes have taken place in the social-cultural environment, transportation, demographics and economy that encourage local residents to find more efficient ways of public service provision[1]. In particular, the financial crisis of 2008 and its aftermath have imposed a variety of fiscal pressures on NY local governments (i.e. counties, cities, towns and villages), including "declining or static tax base, stagnant level of state aid, escalating healthcare and employee benefit costs, the lack of mandate relief and the need to support new security measures"[2]. In addition, complicated fiscal relations continue to be a source of contention between towns and villages in NY. According to *Local Government Handbook of NY State* (2009), village residents are liable for payment of taxes to the village in which they reside, as

[1] More discussion on the outdated municipal class system in NY State can be found in *Outdated Municipal Structures: Cities, Towns and Villages-18th Century Designations for 21st Century Communities*, Alan G. Hevesi, Office of The New York State Comptroller. http://www.yurgartis.net/munistructures.pdf

[2] Office of New York State Comptroller. *Intermunicipal Cooperation and Consolidation* http://www.osc.state.ny.us/localgov/pubs/research/cooperation1.pdf

well as to the town where their villages are located. When residents are taxed for town services they do not receive or when they do not want to pay the village governments for local services, they begin to doubt the legitimacy of overlapping tax systems and the corresponding government structure. For example, in many towns, village residents are required by town boards to bear the costs of town highway equipment and snow removal on town roads, although the State Highway Law exempts them from paying the cost of repair and improvement of town highways. Many village residents are not satisfied with the duplicative fiscal burdens on their shoulders. Moreover, the redundant and fragmented property tax assessing system may also disappoint residents in villages. According to the NY State Department of Taxation and Finance[3], there are 1,116 property tax assessing units in NY, including 2 counties, 61 cities, 920 towns and 133 villages. Moreover, village assessors are probably duplicative; village residents receive two different assessments for taxes levied by different units of government[4] (Yinger 2012). As a result, outdated municipal class designation, contentious inter-governmental fiscal relationship, increasing budget constraints and fragmented property tax assessing system force local governing bodies and residents to rethink the existing mode of governance and to seek new ways of delivering service. Apart from restructuring, privatization, government entrepreneurship, and some other strategies recommended by experts and professionals, consolidation-an old and new approach-has increasingly drawn public attention nowadays.

The principal argument in favor of consolidation or dissolution is economies of scale, through which production cost is expected to decrease holding service quality constant. However, centralizing local autonomy by eliminating "redundant" layers of government goes directly

[3] http://www.tax.ny.gov/pit/property/assess/reform/index.htm
[4] Four Flaws in New York State's Property Taxes and How to Fix Them: Small Assessment Units (Yinger 2012) http://cpr.maxwell.syr.edu/efap/about_efap/ie/Aug12.pdf

against the traditional wisdom of the Decentralization Theorem. This theorem, formalized by Oates, states that decentralization will increase economic efficiency because local provision of public service is usually tailored to the demand of residents in respective jurisdictions, resulting in higher levels of social welfare than central determination of uniform levels of outputs across jurisdictions (Oates 1972; 1993)[5]. The trade-off between harvesting producer efficiency through centralization and enhancing consumer efficiency through decentralization naturally leads to a basic concern that, for dissolved villages in NY State, whether they have obtained "net" efficiency gains after dissolution[6], or put it differently, whether they have reduced public service cost without sacrificing service quality.

This paper is the first study to provide an overall evaluation of village dissolution by exploring how much home buyers value dissolution as measured by how much they are willing to pay to live in a village that has recently dissolved. It is noteworthy that the focus of this study is not to estimate the average price changes of all sold and unsold houses caused by village dissolution[7], but to look for a sign whether people care about dissolution by only analyzing property sales data. In this narrow sense, using changes of sold-property values to assess the overall impact of village dissolution is reasonably appealing, as sold-property values comprehensively, although indirectly, capture the impact of any improvements affected by village dissolution. In addition, as the responsibility of providing public service to former village areas will be assumed by town

[5] Both Oates (1972) and Besley & Coate (2003) argue that, for heterogeneous districts, decentralization is welfare superior to centralization in the absence of spillovers, although they differ in justification and implications of centralization.

[6] Many village residents may deny dissolution decision because they do not believe the town is able to maintain the same level of public service quality in the village areas after dissolution, while some other residents may approve dissolution either because they believe they will receive the same public service or because they prefer financial bonus brought by productive efficiency. One example exhibiting the tension between these two attitudes is http://blog.syracuse.com/opinion/2013/05/votes_have_consequences_and_ea.html#incart_river.

7 Actually, like many studies on capitalization topics in Urban Economics, it is also very hard to precisely capture the average impact on people's willingness to pay in a dissolved village, owing to the potential differences of property owners between sold houses and unsold houses, and the lack of information about unsold-house prices and corresponding owners.

governments, this paper also investigates whether the change of village government structure has impact on the local housing market in the town outside village (TOV) areas. In the next session, I will briefly discuss the concept of village dissolution and how it is relevant to property values in Upstate NY, and then review literature in Section III, which is followed by an analysis of methodological challenges and data issues in Section IV. The paper ends with discussions of empirical results and policy recommendations.

3.2 Village Dissolution and Its Impacts on Housing Prices in New York

Per the definition given by New York State Department of State, Consolidation of Village means "either (a) the combination of two or more local government entities resulting in the termination of the existence of each of the entities to be consolidated and the creation of a new entity which assumes jurisdiction over all of the terminated entities, or (b) the combination of two or more local government entities resulting in the termination of the existence of all but one of the entities which shall absorb the terminated entity or entities." Dissolution of Village is defined as "a process whereby a village ceases to exist as a governmental entity." It is noteworthy that the process of village dissolution "does not require the consent of the town in which the village is located."[8] More often than not, both dissolution and consolidation are put under the same conceptual umbrella of consolidation as village dissolution can also be interpreted as a process that a village consolidates with the town where the village is located. Nevertheless, this paper notes subtle differences between the two terms and only picks up cases of village dissolution in upstate NY.

[8] The New York Government Reorganization and Citizen Empowerment Act, Effective March 21, 2010. http://www.dos.ny.gov/lg/publications/ConsolidationDissolutionLaw.pdf

In NY State, the necessity of village dissolution has been seriously recognized and substantially encouraged by NY State Department of State (DSD), which provides both financial and technical support to help local governments to shrink government size. There are at least two explicit types of grants from the DSD in NY, one is competitive Local Government Efficiency Grant (LGEG), and the other one is noncompetitive The Local Government Citizens Reorganization Empowerment Grant (LGCREG). In the past a couple of years, around 50 villages and towns have applied for LGEG to study possible consolidation of public service with other jurisdictions or dissolution of their village governments.[9] From 2000 to date, 12 villages have successfully approved their dissolution plans and they are scattered among 10 counties in upstate NY [10] (see Table-1 and Figure-1), though at least 24 villages voted against dissolution after 2008[11].

In NY State, a village can be either formed or dissolved at the full discretion of local residents. Since villages are usually formed within towns, the underlying towns would become fully responsible for governing the territory of the former village after dissolution. The dissolution process can be initiated either by the village board of trustees on its own motion or through an appropriate voter petition to the board of trustees. In either case, the dissolution decision should be finally determined by the voters of the village at an election. Also, in either case, the village board of trustees is responsible for formulating a dissolution plan, which must address several important issues, including: (1), the disposition of the property of the village; (2), the payment of outstanding obligations and the levy and collections of the necessary taxes and assessments; (3), the transfer or elimination of public employees; (4), the continuation of village functions or

[9] Here is the website of Local Government Efficiency Grant http://www.dos.ny.gov/lg/lge/projects.html#GovConsDiss
[10] http://www.dos.ny.gov/lg/village-inc-diss.html
[11] I'm trying to build a comprehensive inventory of villages which failed to dissolve after 2000. The current 24 fail-to-dissolve villages all held their voting after 2008. More data before 2008 will soon be collected.

services by the town; (5), a fiscal analysis of the effect of dissolution on the village and the area of the town or towns outside the village, and any other matters which are necessary to carry out dissolution[12]. The village board of trustees must appoint a study committee to prepare a report on the village dissolution plan and the committee must hold at least one public hearing upon 20 days' notice published in the official village and town newspapers. In addition, the village board of trustees must also hold at least one public hearing, after which a dissolution proposition will be presented to the village voters at the next regular or special village election of officers held not less than 30 days after the board of trustees hearing. The proposition is used to educate and inform the resident village electors as to the consequence of their vote. If the proposition is approved by a majority of those voting on the question, the village would then be dissolved as of the thirty-first day of December in the year following the year in which the election took place. [13]

Based on dissolution study plans published by both successfully-dissolved and non-successfully-dissolved villages, many village taxpayers in NY believe village taxes are too high, especially in conjunction with county, town and school taxes. By dissolving village governments, village residents are able to cancel at least some administrative cost of village governments through eliminating administrative positions like village mayor, village court, clerks and corresponding salaries and benefits, and possibly shift part of their service cost to other town residents in TOV areas. So, logically, the primary benefits of village dissolution for former village residents is lower property tax burden. For example, the dissolution study plan by the Village of Seneca Falls

[12] The difference between board-initiated process and the voter-initiated procedure lies in that the later one requires an initial public referendum prior to development of a dissolution plan under the Article 17-A of the General Municipal Law. If the initial referendum passes, the village will then be required to draft a formal plan and the drafted plan can either automatically take effect upon approval of the village board, or subsequently determined by a permissive referendum. A similar discussion can be found in http://www.empirecenter.org/publications/streamline-options-for-localities/.

[13] *Local Government Handbook, 2009* http://www.dos.ny.gov/lg/publications/Local_Government_Handbook.pdf

(2009) claimed that dissolution will decrease property tax by approximately 48%[14]. The literature on property tax capitalization clearly demonstrates that lower property taxes, to some degree, increase property values, so it is reasonable to believe dissolution will be strongly associated with the increase of housing prices[15]. Practically, however, the degree to which residents' fiscal burden will be really reduced depends on the specific institutional tools that surrounding towns use to continue public service provision to the former village areas. If surrounding towns simply spread the former village cost to all town citizens, village residents will benefit more than if surrounding towns carefully use special districts to maintain public service in former village areas because special districts will still confine the public service costs within former village areas. In addition to property tax relief, dissolution may increase efficiencies in town management and town-wide resource distribution. Also, eliminating an invisible boundary that divides the townwide community may help economic development and engage more village residents in town affairs.

Despite above-mentioned merits, village dissolution may also bring potential detriments to village residents and their property values. If a village dissolves, it would no longer be a separate corporate entity. As described earlier, most of villages were established before 1920s and losing their status represent a significant change to the culture of the community. For many village residents, especially for property owners, the loss of identity means not only culture changes, but also loss of policy agenda control. Also, it is full of uncertainties about whether the future public service quality will remain the same in terms of sufficient code enforcement, sidewalk snow plowing, brush pickup, and the like. Nobody can guarantee that townwide representation will

[14] https://www.dos.ny.gov/lg/publications/LGEProjectReports/2008/SenecaFallsDissPlan.pdf
[15] Technically, of course, property tax savings for village property owners will not be realized until implementation.

effectively account for the interests of village residents who formed and maintained village governments in order to effectively obtain special services and easily access policemen, fire fighters and social workers. Many residents may think the projected property tax savings are not a fair compensation for the great uncertainty and potential loss that dissolution will bring. Under this circumstance, property values may decrease, even though some village residents are very active in voting for dissolution.

More interestingly, because village residents are the only electorate who have voting rights to make a dissolution decision, it is highly possible that village dissolution imposes an exogenous shock to TOV residents and their properties. The decision-making process of village dissolution implicitly determines that dissolution may be used by village residents to shift some village wide-service costs to other town residents living outside of village areas. Although town officers are usually engaged in developing dissolution plan and special districts are often used to maintain fire/streetlight service quality in former village areas, as acknowledged in many dissolution study plans, some costs shifts are unavoidable even if policy makers endeavor to minimize this cost-shifting impact. Indeed, as mathematical calculation in many village dissolution study plan demonstrates, property tax burdens usually decrease in potentially dissolved-village areas, but increase in TOV areas. The unique decision-making process and substantial difference in property tax burden impacts of dissolution between village and TOV areas, imply that both the estimation strategy and analytical results of housing price impacts of village dissolution will be different, which will be described and analyzed in Section IV.

3.3 Literature Review

Whether village dissolution will be captured by the change in local housing market is a capitalization research topic. Following Oates (1969), scholars have extensively explored capitalization issues in local public finance. According to subjects which have been studied to be capitalized into property values, literature on capitalization can be roughly classified into three streams of thoughts. The first strand of literature focuses on how property tax is capitalized into housing price (Oates 1969; Yinger et al 1988). The second category studies the extent to which the quality of public service and environmental amenities are capitalized into property values and specific topics range from crime rate, water quality (Leggett & Bockstael 20000), proximity to open space (Anderson & West 2006), access to public parks (Lall & Lundberg 2007) to quality of education (Ross & Yinger 1999; Nguyen-Hoang & Yinger 2011). These two streams of research confirm the existence of capitalization and are helpful in revealing the composition of housing price in particular regions. The current study belongs to the third stream of capitalization research which aims to unveil the mystery of the impacts of changing government structure on housing market from a political-economy perspective. I will go through the literature in the third stream and discuss how those previous studies differ from this paper, as below.

Different local government structures imply different governance channels through which the governed are managed, public affairs are handled and public interests are secured in particular jurisdictions. So, logically, changing government structures will affect residents' perception of tax burden, mode of service delivery, control of local agenda and their own interests and thereby is related with housing demand. Most of existing research on capitalization of government structure concentrates on special-purpose governments in the federal context of the U.S.

Since 1980, more than 30 pairs of school districts choose to consolidate to save costs in NY State. Regarding the property-value impacts of school district consolidation, Brasington (2004) finds that once one control for changes in student test scores and property tax rates, consolidation has a negative impact on property values in urban school districts. It is noteworthy that the estimate cannot be interpreted as a comprehensive evaluation of the value that presents place on all aspects of consolidation, as the service –tax package is controlled. In fact, what he estimates is how much people are willing to pay for consolidation-induced factors that fall outside the school district budget.

By using first differencing and 2SLS strategy to deal with endogeneity problems, Hu and Yinger (2008) identify several causal channels including economics of population scale, state aid, tax share, budgetary and non-budgetary factors affecting school quality, adjustment cost and consolidation-induced changes in tax shares, and propose that consolidation, on average, has a positive effect on housing values in rural school districts, but such an effect is moderated by enrollment size and income groups. Specifically, the property value impacts are largest for the smallest consolidating districts, and positive impacts fade out once a district size of about 1,000 pupils is reached. They also find consolidation has a strong positive impact on house values in census tracts with low average house values and a strong negative impact where average house value is high. In addition, they demonstrate that state aid accompanying consolidation generates approximately one third of the impact of consolidation on house values in small districts. Although they do not test the impact of each causal channel, their specification of causal mechanism illustrates that property value impacts of consolidation are driven by factors that are linked to both productive efficiency and allocative efficiency.

Duncombe et al. (2016) use double sales data in the housing market in upstate NY State and adopt propensity score matching to get rid of selection bias. They find that, except in one large school district, consolidation has a small negative impact on house values. But this average impact reflects a downward trend in house value during the years right after consolidation combined with a positive trend subsequently. In addition, the long-run impacts of consolidation on house values are negative in high-income census tracts. In short, findings on the capitalization of school district consolidation are basically consistent with literature in economies of scale; moreover, both Hu & Yinger (2008) and Duncombe et al. (2016) emphasize the importance of two moderators in the causal path diagrams: one is enrollment size and the other one is income level.

In addition to school district, there are many other types of special-purpose governments in the U.S., such as fire-service districts, water districts, and library districts. Because many special-purpose governments also have power to tax, collect user charges and fees, and deliver public service, the existence of them may affect people's decision on house purchase. Billings and Thibodeau (2010) use data in Denver Metropolitan area to test whether decentralizing public goods providers affects residential property appreciation rates. Their empirical findings show that institutional decentralization has no influence on single-family property appreciation rates. Specifically, although residential property values for homes located in jurisdictions that added security special districts[16] experienced rates of appreciation, recreation, fire, water, sewer and other special districts had no measurable influence on appreciation rates. In addition, their results indicate that more overlap among local governments reduces appreciation rates.

[16] Security districts provide service such as security gate, security guard, neighborhood watch, lighting and so forth.

To sum up, previous research carefully treats methodological challenges and reaches illuminating findings on the capitalization of special-purpose governments. Special-purpose governments, however, are quite different from general-purpose governments primarily in that the latter provide a bundle of public service instead of a single type of service. The mix of public service provision compound difficulties in controlling for public service efficiency when estimating cost function, implying that it is not feasible to directly apply cost function to specify impacts of economies of scale at village level as what Duncombe &Yinger (2007) and Hu & Yinger (2008) do at census tract level. In addition, changes in households across village-township boundaries and tax price suggest difficulties in accurately calculating consumer surplus changes which are crucial to estimate changes in allocative efficiency caused by dissolution. Besides, the total outlay of special-purpose governments (except school district) is not comparable to that of general-purpose governments. Many special districts are even not well perceived by local residents as they are too small and nearly voluntary-based. So, it is entirely possible that empirical findings and theoretical predictions based on special-purpose governments are not applicable to general-purpose governments. Thus, it is important to collect empirical evidence on whether and how village dissolution is capitalized into residential property values.

3.4 Methodological Challenges and Possible Solutions

This section first discusses key methodological challenges, and then analyzes and compares possible identification strategies to estimate housing price impacts of village dissolution in former village areas, based on both methodological legitimacy of each approach and practical

quality of housing sales data. After that, it will briefly describe empirical strategies for estimating how village dissolution affect housing price in TOV areas.

In non-experimental or observational studies, as exemplified by the village dissolution case in this paper, the houses and corresponding villages between dissolved villages and non-dissolved villages are likely to be different in both observed and unobserved ways. The first methodological challenge is that a simple comparison of house sales in dissolved villages and non-dissolved villages may yield bias whenever the housing price impacts of dissolution depends on observable village or housing traits. For instance, population trends and property tax burdens assumed by village residents may be quite different across villages and they probably influence both housing purchase and village dissolution decisions, as described in many village dissolution study plans. Under these circumstances, covariate bias or sample imbalance should be minimized in order to make house sales comparable between dissolved villages and non-dissolved villages. The second challenge is that village dissolution decision may be influenced by some unobserved factors which also affect house values. As previously described, in any village dissolution election, a dissolution proposition is required to inform citizens of the consequences of government reorganization in terms of possible changes in public service quality, property tax burden, public employee positions, disposition of village assets and liabilities, and the payment of outstanding obligations. So, when residents forecast that village dissolution will (or will not) achieve substantial production cost savings or that the additional gains in producer efficiency will (or will not) substantially exceed the additional loss in consumer efficiency, they will vote for or against dissolution decisions. In either case, dissolution decisions are determined by village residents' perception of the history, the status quo, and the future trade-off between public service quality and production cost in their communities. These decisions are also

determined by the relationship between their towns and their villages. All these concerns logically and invisibly influence both village dissolution decisions and local real estate market but are hard to explicitly control for in empirical models. So, a comparison between two study groups is susceptible to an omitted variable bias. Last but not least, most village dissolution referendums were held during or after the 2008 financial crisis. Consequently, the unstable underlying trends in housing market further complicates the estimation. It is noteworthy that it usually took a couple of years from dissolution petition/imitation to final dissolution vote, hence, random shocks or short-run changes (i.e. heavy snowstorm in a village in a particular year) would not be expected to drive or dominate the final voting results on village dissolution.

In evaluating housing impacts of educational services and programs, a couple of methods have been raised to deal with the covariate bias and omitted variable bias in the existing literature. When representative and sufficient straddling repeat sales[17] are available, an ideal approach is to combine propensity score matching (PSM) with difference-in-difference (D-D) to make an estimation. PSM, unlike regression, does not try to model the relationship between the X's and outcome but attempts to model the association between X's and treatment status in order to avoid using outcome data at the stage of sample selection. It replicates the spirit of randomization by creating balanced groups which look only randomly different from one another on the basis of observed variables (Stuart & Rubin 2007; Stuart 2011). The key idea is treatment group and control group are comparable only when the distributions of covariates are very similar between two groups[18]. As the second step, after obtaining a balanced sample in which each house has been

[17] Straddling repeat sales are those repeat sales for which the first sale occur before treatment and the second sale occur after treatment.

[18] A series of balance test (e.g. simple T-test, standardized bias test and so on) should be adopted and passed before one claim to obtain a balanced sample.

sold twice (or more), one can estimate a regression with price change of each house as the dependent variable and with an indicator equal to one in villages where dissolution has occurred. The setup of this regression functions like a D-D approach and controls for all time-invariant factors that influence both the house value and dissolution decision[19]. Again, the validity of this methodology depends on whether two conditions can be satisfied. First, whether one can obtain sufficient straddling repeat sales in both treatment group and control group before matching[20]. Second, whether the housing and neighboring characteristics of repeat sales are representative of those of all housing sales[21]. In this paper, it is obvious that I will not have a sufficient amount of straddling repeat house sales in treatment group for matching as housing market in rural areas are not so active as that in urban areas. Therefore, it is predictable that a large sample of straddling repeat sales will not be easily established.[22]

Although it is not feasible to adopt the above mentioned optimal identification strategy, a repeat sales sample, when coupled with hedonic model, parcel-level fixed effects and village-level time trends, is still very helpful in estimating impacts of dissolution on housing price in former village areas and TOV areas in the past 15 years in upstate NY.[23] Conceptually, treatment group observations include all repeat housing transactions in dissolved villages[24] from 2000 to 2014; while the counterfactual group involves all repeat housing transactions in comparable villages in

[19] Time-varying factors could be controlled for by adding a series of timing difference variables into regression on the basis of a balanced sample. More details can be found in Duncombe et al (forthcoming).
[20] When adopting PSM, both the step of limiting sample to a common-support area and the procedure of matching will substantially filter and reduce the size of repeat sales. So, the original size of repeat sales should be large enough.
[21] The external validity of research findings will be threatened without a set of representative repeat sales. That is why both Duncombe et al (2016) and this paper acknowledge the limitation of their sample and claim their research as case studies.
[22] It is expected that this approach will be more appropriate after updating the housing sales data next year.
[23] Similar identification strategies (i.e. combine repeat sales with a series of fixed effects) have been used in estimating housing price impacts of educational reform or school report cards. See Il Hwan Chung, William Duncombe & John Yinger (2013 under review), and Figlio & Lucas (2004).
[24] 12 dissolved villages include village of Andes (2003), village of Pike (2009), village of Limestone (2010), village of East Randolph (2011), village of Randolph (2011), village of Perrysburg (2011), village of Seneca Falls (2011), village of Edwards (2012) and village of Altmar (2013).

the same time period in the upstate NY[25]. By comparable group, this papers means villages in the same geographic divisions within NY State. Specifically, NY City, NYC northern suburbs, Nassau-Suffolk areas, Buffalo Region, Mid-Hudson Region, and Upper Hudson Region will be excluded as no village dissolution took place in those areas in the past 30 years and these areas present significantly different economic features from the remaining regions in upstate NY. In other words, only repeat house sales from regions 2,3,4,6 will be included in my research, as indicated in Figure-2.

Consider the following hedonic model, with log of housing price as the dependent variable and parcel as the unit of analysis (subscript i, n, v and t mean parcel, census tract, village and year, respectively),

$$log(P)_{invt} = \alpha X_{invt} + \beta N_{nvt} + \gamma V_{tv} + \delta V * (t - t_0) + \theta R_t + \rho R_t * (t_2 - t^*) + \mu_i + \varepsilon_{intv}$$

(Equation 1)

Let X stand for individual housing characteristics, N include neighborhood and social-demographic variables[26], V mean villages characteristics, $V * (t - t_0)$ stand for village-specific time trends with t_0 equal the starting date of my sample, R_t be a binary indicator that equals 1 in a dissolved village after dissolution date and 0 otherwise. Suppose dissolution occurs in year t^*, (t_2-t^*) captures the timing difference between dissolution date and second-sale date. Moreover, let μ_i stand for parcel-level time-invariant heterogeneity, and ε_{intv} be an error term.

[25] As mentioned earlier, there are 11 villages that tried but failed to dissolve their villages during the same time period. These villages are expected to be more comparable with dissolved villages than other villages who never consider the necessity of dissolution. This provides another opportunity for robustness check.

[26] As demonstrated in Yinger (2014), Ns should take quadratic forms based on constant elasticity demand functions for public services and housing. For simplicity, Ns shows up in linear form in Equation 1.

The key methodological challenges of estimating the above hedonic model include, a) it is hard to get sufficient, time-varying and/or time-invariant Xs, Ns and Vs, and b) the existence of unobserved heterogeneity will bias my estimation. However, repeat sales enable me to exploit parcel-level within variations by using fixed effects approach and other tactics to handle those challenges.

First, to avoid the possibility that dissolved villages and non-dissolved villages have systematically different unobserved heterogeneities that also affect housing prices, this paper adopts parcel-specific fixed effects. By doing so, all time-invariant omitted variables, whether at parcel level, census tract level or village level, will be fixed out from my model. Moreover, by getting rid of all houses which have experienced significant changes in inventory, condition, fire, an addition to properties, and all other substantial physical changes over time, my estimation could be largely protected against bias from time-varying parcel-level characteristics.

Second, because some neighborhood and village factors may affect both property values and village dissolution over time, because my sample period partially coincides with the Great Recession, and because dissolution events did not occur at the same time, it is necessary to account for changing macro-economic and social factors by controlling for village-specific time trends. The parcel-specific fixed effects and village-specific time trends together are supposed to account for all unobservable factors that vary linearly over time and therefore eliminate possibility of bias from these factors, even if they are strongly correlated with the treatment events, i.e. village dissolution[27].

[27] Bloom (1984) provides an example involving both fixed effects and time trends.

The above methods, however, fail to protect estimation against possible bias caused by unobserved factors that influence both housing prices and village dissolution decisions and that vary in a nonlinear way over time. Fortunately, as I explained earlier, the existing laws on local government reorganization basically guarantee that village residents could be intensively involved in the long dissolution debate and process so that random shocks or short-run changes are not able to drive the final voting results on village dissolution in NY. Procedurally, dissolution plans and public hearing are mandatory; institutionally, permissive referendums give residents second chance to change the initial voting results under the Article 17-A of the General Municipal Law; and economically and politically, rural areas are relatively stable in NY.

Third, the possibility that the effects of dissolution on property values may take time to emerge or that the impact is not constant over time, requires to account for when the post-dissolution sale occur relative to voting date of dissolution. By controlling for the time length between those two points in time, this paper is also able to analyze the possible time pattern of the housing price impacts of village dissolution.

Fourth, when setting the exact date of a particular dissolution event, I prefer the date of referendum to the effective date, because village residents could be immediately aware that dissolution is taking place on voting date[28]. Another motivation to do so is that many effective dates of dissolution events are so close to the end of my sample period that I would lose many post-treatment observations if I employ the effective date as the dissolution date. The choice of using voting date as the treatment date implies that, my estimation results will comprehensively

[28] It is rare that permissive referendum change the initial voting results, so I use initial voting date as the dissolution date for those successfully dissolved villages.

pick up non-budgetary effects of dissolution on housing prices between voting date and the effective date of dissolution.[29].

In short, this analysis builds on a comparison between repeat housing sales in dissolved villages and those in non-dissolved villages, uses parcel-specific fixed effect and village-specific time trends to deal with heterogeneity and endogeneity issues, and controls for timing difference between the second sale and the voting date of dissolution. By designing so, R_t enables to capture the shift in intercept while $[R_t * (t_2 - t^*)]$ capture the shift in post-dissolution time trend caused by village dissolution. This equation can also be directly applied to estimate housing price impacts of dissolution in TOV areas. The application of this model to TOV areas make perfect sense as village dissolution can reasonably be treated as an exogenous shock to residents and their properties in TOV areas, as explained earlier.

3.5 Data

Property sales information and housing characteristics were provided by the New York Office of Real Property Services (ORPS), which has recently been merged into the New York State Department of Taxation and Finance. Property sales data from 2000 to 2014 are available in the "Sales" database, which includes 15 years of parcel-level property sales information for the state. The Sales database includes information on property location, class, sales date, and sales price. This analysis only includes arms-length transactions of single family homes, which are constructed for year-round residence with a sales price greater than $10,000 and which have not

[29] When accumulating more data in future, I would like to use the effective date of a dissolution to see how my estimation results would be changed and thereby study time line effect, namely focusing on how reactions to village dissolution change over time. Different reactions at different points in time may point us to one policy implication that when policy makers should compensate people. But, so far, I do not have many observations during post-effective-date period, and this interesting topic will be explored in future. One example with focus on the time line effect is "House Price during a Siting Stages: The Case of an Incinerator from Rumor through Operation" (Kiel and McClain 1995).

experienced significant changes in physical conditions between the taxable status date and the sale date. The Real Property System also collects information from local assessors on a number of parcel characteristics. Quality measures include an assessment of the overall physical condition of the residence (including interior and exterior walls, foundation, kitchen, baths, heating, plumbing and electrical systems). A second quality measure is an assessment of the construction grade of the house, which refers to the quality of the material and workmanship used to construct the house. Additional variables include the number of square feet of living area in a house, the number of bedrooms, and the number of full bathrooms. Special housing features in the data set include whether the house has a full basement, central air conditioning, or a fireplace. Finally, the data set contains an estimate of the house age at the time of sale.

This paper will also use information about the demographics and socio-economic status of residents in the 2000 census tract in the section of robustness check. Specifically, the data set includes measures of age, race and ethnicity, gender, poverty, income distribution, distribution of house values, educational attainment, enrollment in public schools, employment by industry and occupation, and unemployment. In the section of robustness check, the selection of neighborhood amenity variables and model specification issues will be further discussed.

3.6 Empirical Results

As I explained earlier, the effects of village dissolution on housing prices in village areas could be positive or negative, conditional on many institutional, cultural, financial and historical factors. Based on Equation 1, the first column in Table-2 captures possible intercept shift in property values caused by dissolution events, while the second column shows us whether the property value effects of dissolution will change during the post-treatment period. Interestingly,

the empirical results did not find any significant shift in either intercept or post-treatment time trends in village areas. One possible, abstract, explanation is that the potential benefits of economies of size are offset by loss of allocative efficiency (e.g. loss of power in local agenda setting, easy access to local police, etc.) Another, more practical, explanation is that the specific institutions through which public services responsibilities are assumed by town governments may prevent financial costs of within-village public service from spreading to surrounding TOV residents. Since special districts are widely designed and used to replace the former village governments in providing fire, police, and sewer and water services, the existing housing demand curve will not substantially change. Actually, it is not common for town governments to unconditionally accommodate village service into their current system in upstate NY, meaning that financial costs of former village residents are not very likely to decrease, and thereby bidding and sorting functions will not be substantially affected[30].

Based on the same models, columns 3 and 4 reveal different stories in the TOV areas. First, dissolution decline property values by almost 16% in the TOV areas, and the declining effects become stronger as time goes by[31].The magnitude of the negative effect is very large considering the relatively stable economic environment in upstate NY. One possible reason for the negative effects is that TOV residents are usually not engaged in the village dissolution debate but they perceive to unfairly shoulder additional financial cost previously assumed by village residents[32]. The contrasting empirical effects of dissolution on housing prices between former village areas

[30] It is noteworthy that this paper is not designed to differentiate bidding and sorting effects of village dissolution.
[31] Due to a relatively small amount of repeat sales in my treatment group, it is not quite meaningful, either logically or statistically, to further explore non-linear patterns of post-treatment time trends.
[32] See "Surrounding Towns Not Happy with Village of Medina Dissolution Plan" http://www.twcnews.com/archives/nys/rochester/2014/04/18/surrounding-towns-not-happy-with-village-of-medina-dissolution-plan-NY_725160.old.html

and the TOV areas, demonstrate that TOV residents believe it is not fair to be excluded from village dissolution, and they should be effectively involved into policy-making process.

Although the combination of repeat sales, parcel-specific fixed effects and village-specific time trends could significantly improve internal validity of my estimation, it is not helpful in enhancing external validity of my empirical results. Specifically, neither straddling repeat sales nor non-straddling repeat sales could be simply treated as representative of single sales, and therefore, my results cannot be generalized without caution.

3.7 Robustness Checks

My previous estimation results are completely based upon all repeat sales in similar regions within upstate NY. In this section, I will check whether my results are robust against a different sample selection and the inclusion of single sales.

First, as described earlier, twenty-four villages initiated dissolution process but finally failed to pass the voting referendum after 2008. It is therefore interesting for both state policy makers and those willing-to-dissolve villages to understand how property values are affected by dissolution decision between successfully-dissolved villages/TOVs and unsuccessfully-dissolved villages/TOVs. So, my first sensitivity analysis is to confine my control group to repeat sales in villages where residents voted against dissolution and then make an estimation. Not surprisingly, the magnitudes and the significance levels of both dissolution and post-dissolution time trends are quite consistent with my previous results. Dissolution do not have any real and significant effects on housing prices within villages; while it does negatively affect real estate market in corresponding TOVs.

Second, the relatively small sample size of treatment group[33], and potentially unobserved difference between houses which have been sold at least twice and those which have been sold only once(Clapp et al 1991), present big challenges to the external validity of my previous estimation results. So, I should check the sensitivity of my results to the inclusion of single sales.

The previous way to estimate Equation 1, however, cannot be directly applied to a full sample with both repeat sales and single sales included. First, it is not appropriate to employ parcel-level fixed effects when single sales are included; second, without parcel-level fixed effects, I have to seek methods to deal with the existence of Xs, Ns, and Vs; third, the presence of Xs, Ns and Vs immediately lead to concerns about model specification issues. Fortunately, the existence of housing characteristics, neighborhood amenities and village factors and the concerns about model specification can be dealt with by direct control methods, village-level fixed effects, village-specific time trend, and referring to previous discussions on hedonic model specifications (Yinger 2015). Consider the following hedonic model, which is the same with Equation 1 except the new term, φ_v, the village-specific fixed effects,

$$log(P)_{invt} = \alpha X_{invt} + \beta N_{nvt} + \gamma V_{tv} + \delta V * (t - t_0) + \theta R_t + \rho R_t * (t_2 - t^*) + \mu_i + \varphi_v + \varepsilon_{intv}$$

(Equation 2)

Specifically, house age, area, house construction grade, quality measures of basement, central air conditioning, number of bathrooms and bedrooms, fireplaces are all explicitly controlled for as Xs. At the neighborhood level, I use Geo-coding techniques in GIS to find the matched census tract where a house is located and then attach the census tract information to that house. After merging the 2000 census tract data with the property sales data, I can directly control for the

[33] Deng et al (2011) argue that research methods based on repeat sales may face a large reduction in sample size.

following variables at census tract level (i.e. Ns) in my estimation: percent of adults with a bachelor degree or higher, percent of families with income below $20 thousands, percent of properties with values below $50 thousands, percent of properties with values more than $ 300 thousands, percent of owner occupied house, poverty rate, percent of people with professional occupations, percent of employment in government, percent of unemployment, percent of population older than 65 years old, percent of population under 20, percent of white people and so on. Village-specific fixed effects (φ_v) and time trends ($\delta V * (t - t_0)$) will be jointly used to control for time-invariant and time-varying village-level features which influence both dissolution decisions and property values. Again, the sample exclude all houses experiencing significant changes to physical conditions. Moreover, drawing on the existing research results that quadratic case respond to the assumption that the price elasticity of demand for public services is infinite and that a linear specification for the hedonic is inconsistent with the standard sorting theorem (Yinger 2015), my model adopts quadratic forms for Ns. Hopefully, we control for sufficient Xs and Ns so that we do not need to worry about the presence of μ_i.

To make the results of two datasets comparable, all model specifications follow Equation 2 in Table-4. Then, any difference in estimation results between full sample and repeat sales sample will be simply attributed to sample distinction rather than difference in model specifications. Table-4 demonstrate that the results based on full sample are partially consistent with those based on repeat sales sample, especially when it comes to the declining post-dissolution time trends in TOV areas. The main inconsistent finding is that, statistically, full sample supports a significant declining post-treatment trend. The magnitude, 0.5%, however, is not quite significant in economic sense.

The two robustness check basically provide consistent evidence with my main findings that village dissolutions, although do not significantly affect house prices in former village areas, do impose negative influences on property values in TOV areas where residents are very upset with negative externalities, i.e., possible increase of financial burdens brought by village dissolutions.

3.8 Discussion

The background for this study is provided by a broad concern among practitioners with improving the design of revenue assignment and expenditure assignment, especially when local governments face tight budget constraints, in the context of American fiscal federalism. In addition, whether government reorganization has economic impacts is a very important academic topic both in political economy and in local public finance. This paper is the first study focusing on whether village dissolution, as a form of general-purpose government structural change, will affect the attractiveness of local communities. Towards that end, a repeat sales sample, coupled with parcel-level fixed effects and village-specific time trends, is built to estimate whether local housing demand has been affected by village dissolution, both in former village areas and TOV areas.

Basically, dissolution events do not bother housing prices in former village areas, probably due to the institutional design of using special districts to replace former village governments in providing basic public service in NY State. However, TOV residents do not feel fair to assume externalities of a possible increase of financial burdens caused by local government reorganization. Because they are not really engaged in the decision-making process, the decline of housing demand reflects their attitudes towards possible negative spillover effects of village dissolution. Even worse, the decrease of property values may backfire in future, as town

governments will continue to face a shrinking property tax base. So, an important policy implication of this research is, even though TOV residents are not geographically affected by dissolution, it is important to take account of their benefits and interests in policy design.

Future research agenda includes, a) collecting more political and fiscal information on the operation of special districts which have been used in eliminated village areas, b) estimating net cost of village dissolution in NY, and c) comparing public service cost under different institutions, namely whether special districts are more efficient in providing fire service, water service, and police service than village governments.

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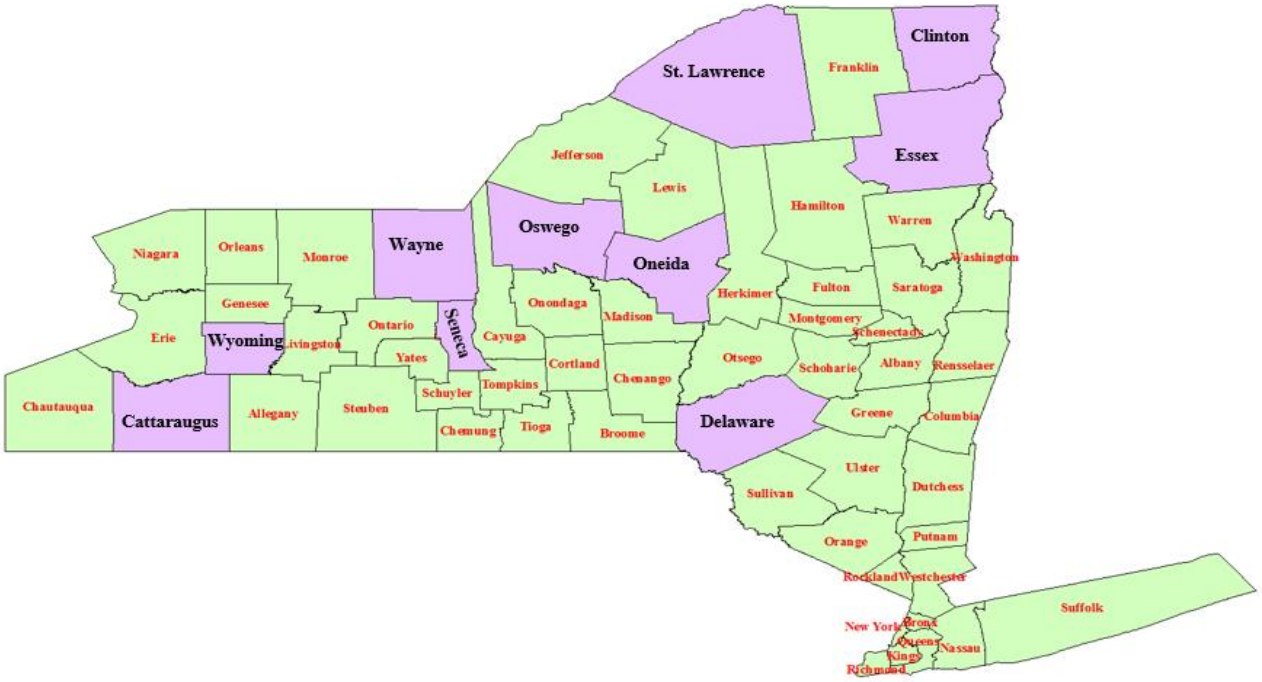
Figure3.1.

Geographical Distribution of Village Dissolution History in Upstate NY (2000-Present)



Note: Four villages passed the voting for dissolution in Cattaraugus County. In addition, the dissolved Keeseville was a village in Clinton and Essex Counties.

Figure3.2.

Geographical Regions within New York State

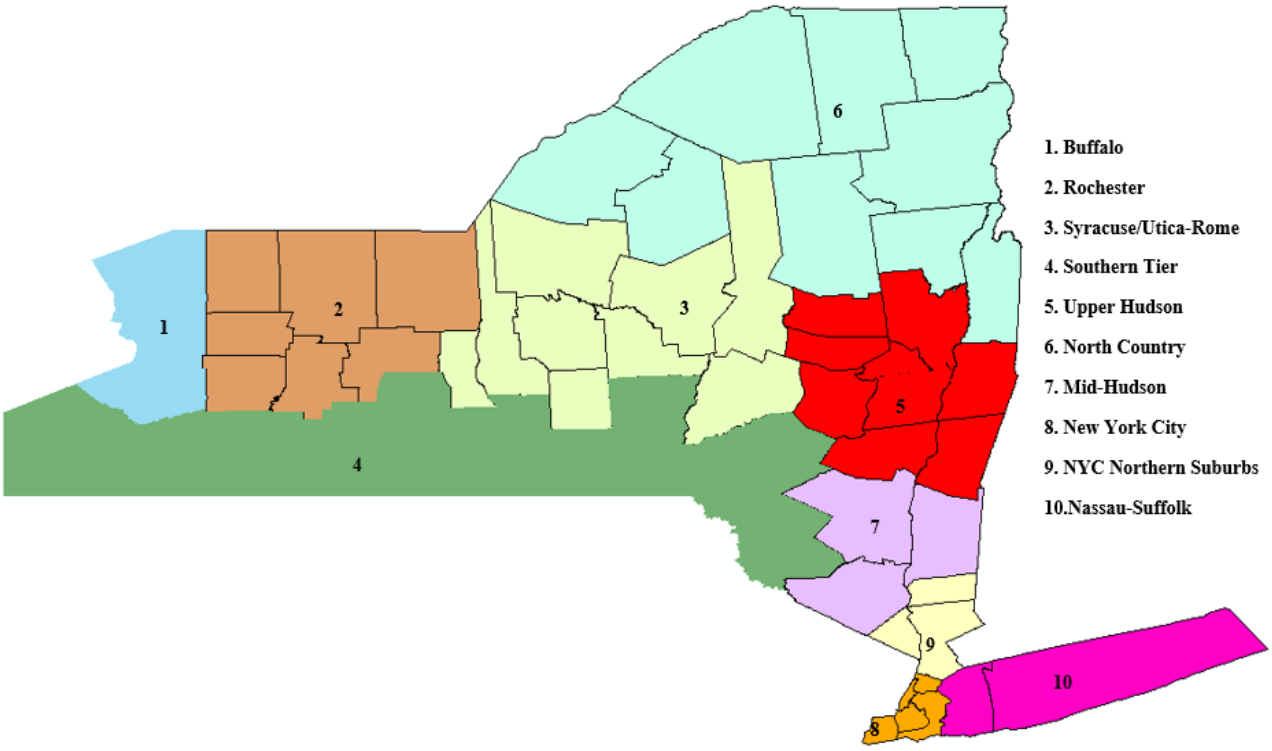


Table 3.1. New York State Village Dissolution between 2000 and 2014

Village Name	Town Name	County Name	Date of Referendum	Effective Date of Dissolution
Village of Andes	Town of Andes	Delaware County	2002, June 3	2003, December 31
Village of Pike	Town of Pike	Wyoming County	2008, March 18	2009, December 31
Village of Limestone	Town of Carrollton	Cattaraugus County	2009, September 28	2010, December 31
Village of Randolph	Town of Randolph	Cattaraugus County	2010, March 16	2011, December 31
Village of East Randolph	Town of Randolph & Town of Conewango	Cattaraugus County	2010, March 16	2011, December 31
Village of Seneca Falls	Town of Seneca Falls	Seneca County	2010, March 16	2011, December 31
Village of Perrysburg	Town of Perrysburg	Cattaraugus County	2010, March 16	2011, December 31
Village of Edwards	Town of Edwards	St.Lawrence County	2011, March 15	2012, December 31
Village of Altmar	Town of Albion	Oswego County	2011, December 6	2013, May 1
Village of Keeseville	Town of Ausable & Town of Chesterfield	Clinton County & Essex County	2013, October 22	2014, December31
Village of Bridgewater	Town of Bridgewater	Oneida County	2014, March 18	2014, December31
Village of Lyons	Town of Lyons	Wayne County	2014, March 18	2015, December31

Main Data Source: Village Dissolutions since 1900 in NY State
http://www.dos.ny.gov/lg/village-inc-diss.html

Table 3.2. The Effects of Village Dissolution on Property Values in Upstate NY, 2000-2014 (Models are based on Equation 1)*

VARIABLES	(1) Village Model 1	(2) Village Model 2	(3) TOV Model 1	(4) TOV Model 2
Dissolution	-0.0382	0.0547	-0.159*	0.0525
	(0.121)	(0.0733)	(0.0853)	(0.148)
Months Since Dissolve		-0.00662		-0.0115**
		(0.00542)		(0.00555)
Constant	11.16***	11.16***	11.53***	11.53***
	(0.000414)	(0.000362)	(0.000151)	(0.000174)
R-squared	0.911	0.911	0.936	0.936
Parcel-Level FE	Yes	Yes	Yes	Yes
Village-Specific Time Trends	Yes	Yes		
Town-Specific Time Trends			Yes	Yes
Observations**	18,612	18,612	41,272	41,272

Note: The dependent variable is housing price in log form. As explained in text, the variable "month since dissolve" equals the time length between the date of dissolution and the date of second sale. Clustered standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

*Model 1s capture the shift in intercept, and Model 2s additionally capture shifts in post-dissolution time trends.

**There are 268 observations in finally dissolved villages (Village Model 1 and Model 2) and 260 observations in corresponding TOV areas (TOV Model 1 and Model 2).

Table 3.3. The Effects of Village Dissolution on Property Values in Upstate NY, 2000-2014
Robustness Check: Comparison with Unsuccessfully-Dissolved Villages/TOVs
(Models are based on Equation 1)*

VARIABLES	(1) Village Model 1	(2) Village Model 2	(3) TOV Model 1	(4) TOV Model 2
Dissolution	-0.0382	0.0547	-0.182**	0.0248
	(0.123)	(0.0742)	(0.0863)	(0.128)
Month Since Dissolve		-0.00662		-0.0121**
		(0.00549)		(0.00588)
Constant	11.04***	11.04***	11.54***	11.54***
	(0.00318)	(0.00278)	(0.000428)	(0.000478)
R-squared	0.893	0.894	0.937	0.937
Parcel-Level FE	Yes	Yes	Yes	Yes
Village-Specific Time Trends	Yes	Yes		
Town-Specific Time Trends			Yes	Yes
Observations	2,456	2,456	14,272	14,272

Note: The dependent variable is housing price in log form. As explained in text, the variable "month since dissolve" equals the time length between the date of dissolution and the date of second sale. Clustered standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
*Model 1s capture the shift in intercept, and Model 2s additionally capture shifts in post-dissolution time trends.
**There are 268 observations in finally dissolved villages (Village Model 1 and Model 2) and 260 observations in corresponding TOV areas (TOV Model 1 and Model 2).

Table 3.4. The Effects of Village Dissolution on Property Values in Upstate NY, 2000-2014
Robustness Check: Comparison between a Repeat Sales Sample and a Full Sample
(All Models Are Based on Equation 2)*

	Full Sample***				Repeat Sales Sample****			
VARIABLES	(1) Village Model 1	(2) Village Model 2	(3) TOV Model 1	(4) TOV Model 2	(5) Village Model 1	(6) Village Model 2	(7) TOV Model 1	(8) TOV Model 2
Dissolution	-0.0268	0.0372	-0.0524	0.0964	-0.0890	-0.134	-0.0362	0.233***
	(0.0471)	(0.0577)	(0.0743)	(0.0937)	(0.0603)	(0.107)	(0.0603)	(0.0853)
Month Since Dissolve		-0.00570***		-0.00880***		0.00336		-0.0145**
		(0.00149)		(0.00270)		(0.00394)		(0.00677)
Constant	11.03***	11.02***	12.18***	12.18***	13.48***	13.48***	11.41***	11.41***
	(1.185)	(1.185)	(0.733)	(0.733)	(1.508)	(1.508)	(0.986)	(0.986)
R-squared	0.625	0.625	0.627	0.627	0.655	0.655	0.666	0.666
Direct Control**	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Village FE	Yes	Yes			Yes	Yes		
Village-Specific Time Trends	Yes	Yes			Yes	Yes		
Town FE			Yes	Yes			Yes	Yes
Town-Specific Time Trends			Yes	Yes			Yes	Yes
Observations	48,352	48,352	108,784	108,784	18,608	18,608	41,268	41,268

Note: The dependent variable is housing price in log form. As explained in text, the variable "month since dissolve" equals the time length between the date of dissolution and the date of second sale. My full sample only picks up house sales from regions 2,3,4,6 in Figure-2 as well. Clustered standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

*Model 1s capture the shift in intercept, and Model 2s additionally capture shifts in post-dissolution time trends.

**There are 10 housing-characteristic variables and 13 neighborhood-feature variables directly controlled in all models.

***When using a full sample (Column 1-4), there are 1,015 observations in finally dissolved villages (Village Model 1 and Model 2) and 800 observations in corresponding TOV areas (TOV Model 1 and Model 2).

****4 observations are lost due to the missing values of control variables when I estimate Equation 2 on the basis of repeat sales sample (Column 5-8).

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