

THE EFFECT OF THE SCHOOL FINANCE REFORMS IN VERMONT
AND NEW HAMPSHIRE ON PER PUPIL SPENDING

A Dissertation
presented to
the Faculty of the Graduate School
at the University of Missouri

In Partial Fulfillment
of the Requirements for the Degree
Doctor of Philosophy

by
Yangyong Ye
Dr. Bradley Curs, Dissertation Advisor
Dr. James Sebastian, Dissertation Co-Advisor

December 2014

Copyright by Yangyong Ye 2014

All Rights Reserved

The undersigned, appointed by the dean of the Graduate School, have examined the dissertation entitled

THE EFFECT OF THE SCHOOL FINANCE REFORMS IN VERMONT
AND NEW HAMPSHIRE ON PER PUPIL SPENDING

presented by Yangyong Ye,

a candidate for the degree of doctor of philosophy,

and hereby certify that, in their opinion, it is worthy of acceptance.

Professor Bradley Curs

Professor James Sebastian

Professor Sarah Diem

Professor Michael Podgursky

ACKNOWLEDGEMENTS

It is a long journey to complete the dissertation. I would like to thank all who give me support, encouragement, and advices during the process. First of all, I would like to express my sincere gratitude to my advisors Dr. Bradley Curs and Dr. James Sebastian. Their guide, comments, suggestions, and criticisms are extremely important to the writing of the dissertation. Without them, I was not able to enormously improve the dissertation. Thank Dr. Sarah Diem and Dr. Michael Podgursky for their willingness to be my committee members and constructive advices during my proposal and dissertation defense. I also would like to thank Jude Sommerjones and Betty Kissane for giving me many helps during the process of my coursework, comprehensive examination, assistantship, and graduation in the Department of Educational Leadership and Policy Analysis. Also, it is my pleasure to work with Dr. Noelle Arnold and Dr. Jennifer Fellabaum as their assistant in the four years. I appreciate the consideration and the freedom they give to me during the assistantship. Thank Haigen Huang, my colleague and friend, for his encouragement and help during the writing process. Finally, I would like to express my deepest gratitude to my parents. I would not have the opportunity to arrive here on the way to academic world without their support and love.

TABLE OF CONTENTS

ACKNOWLEDGEMENTS	ii
LIST OF FIGURES	vii
LIST OF TABLES	ix
ABSTRACT	xii
CHAPTER 1 INTRODUCTION	1
Introduction.....	1
Statement of the Problem.....	5
Vermont.....	6
New Hampshire	7
Purpose of the Study and Research Questions.....	8
Theoretical Framework.....	9
Research Design.....	12
The Significance of the Study.....	13
Definitions of Terms	15
Summary.....	16
CHAPTER 2 LITERATURE REVIEW	18
Introduction.....	18
School Finance in the United States	18
School Finance Systems	18
Public School Spending.....	24
School Finance Litigations	26
School Finance Reforms	29

School Finance in Vermont and New Hampshire.....	31
Vermont.....	31
New Hampshire.....	36
Literature on School Finance Reforms.....	40
Studies on the Effect of School Finance Reforms.....	40
Studies on Different School Finance Systems.....	47
Theoretical Framework.....	50
Theories on Local Public Spending.....	50
Hypotheses.....	58
Summary.....	60
CHAPTER 3 METHOD.....	62
Introduction.....	62
Estimation Strategy.....	63
Ideal Strategy.....	63
Difference in Difference.....	64
Data.....	70
Data Sources.....	70
Data Preparation.....	71
Definition of Variables.....	72
Statistical Model.....	74
Difference in Difference Model.....	74
Model Validation.....	76
Summary.....	78
CHAPTER 4 RESULTS.....	79

Introduction.....	79
Vermont	80
Descriptive Results.....	80
Difference in Difference Estimations.....	90
Robustness Check.....	99
New Hampshire	106
Descriptive Results.....	106
Difference in Difference Estimations.....	114
Robustness Check.....	122
CHAPTER 5	128
Introduction.....	128
Overview of This Study.....	128
Discussion of Research Findings.....	130
Research Question One	130
Research Question Two.....	132
Research Question Three.....	135
Implications for Studies on School Finance and Reform	138
Contribution to Studies on School Finance.....	138
Policy Implications.....	141
Limitation of This Study and Implication for Further Research.....	144
Conclusions.....	145
REFERENCES	148
APPENDIX.....	156
Appendix One: Robustness Check of the DID Estimations for Vermont	156

Appendix Two: Robustness Check of the DID Estimations for New Hampshire	171
VITA	180

LIST OF FIGURES

Figure 1. Percentage of Education Revenue From Federal, State, and Local Source From 1920-2011.....	20
Figure 2. State Share of Total Educational Revenue of 50 States in 2009.....	20
Figure 3. Current Expenditure per Pupil in Public Elementary and Secondary Schools: 1970-2010.....	25
Figure 4. The Trend of per Pupil Spending in Reform and Comparison States: 1996-2001	81
Figure 5. The Histogram of per Pupil Spending in Vermont in 1998 and 2001.....	83
Figure 6. The Histogram of Logarithm of per Pupil Spending in Vermont in 1998 and 2001.....	83
Figure 7. The Trend of Enrollment of School Districts in Reform and Comparison States: 1996-2001.....	85
Figure 8. The Histogram of Enrollment of School Districts in Reform and Comparison States in 1998 and 2001.....	86
Figure 9. The Histogram of Logarithm of Enrollment of School Districts in Reform and Comparison States in 1998 and 2001.....	86
Figure 10. The Trend of Percentage of Students in Free Lunch Program in the Reform and Comparison State: 1996 to 2001.....	89
Figure 11. The Trend of per Pupil Spending in Reform and Comparison States: 1997- 2002.....	107
Figure 12. The Histogram of per Pupil Spending in New Hampshire in 1999 and 2002	108

Figure 13. The Histogram of Logarithm of per Pupil Spending in New Hampshire in 1999 and 2002	108
Figure 14. The Trend of Enrollment of School Districts in Reform and Comparison States: 1997-2002.....	110
Figure 15. The Histogram of Enrollment of School Districts in New Hampshire in 1999 and 2002	110
Figure 16. The Histogram of Logarithm of Enrollment of School Districts in New Hampshire in 1999 and 2002	111
Figure 17. The Trend of Percentage of Students in Free Lunch Program in Reform and Comparison States: 1997-2002	113

LIST OF TABLES

Table 1: Hypotheses on the Effect of School Finance Reforms in Vermont and New Hampshire	59
Table 2: Descriptive Characteristics of per Pupil Spending in Reform and Comparison States: 1996-2001	81
Table 3: Descriptive Characteristics of Enrollment of School Districts in Reform and Comparison States: 1996-2001	84
Table 4: Descriptive Characteristics of Percentage of Students in Free Lunch Program in Reform and Comparison States: 1996-2001 (Unbalanced).....	87
Table 5: Descriptive Characteristics of Percentage of Students in Free Lunch Program in Reform and Comparison States: 1996-2001 (Balanced).....	88
Table 6: Correlation of All Variables for Vermont	90
Table 7: DID Estimation of the Effect of the School Finance Reform in Vermont on per Pupil Spending in All School Districts with Comparison State Maine and Rhode Island	92
Table 8: DID Estimation of the Effect of the School Finance Reform in Vermont on per Pupil Spending in Poor School Districts with Comparison State Maine and Rhode Island.....	95
Table 9: DID Estimation of the Effect of the School Finance Reform in Vermont on per Pupil Spending in Rich School Districts with Comparison State Maine and Rhode Island.....	98
Table 10: The Results of Robustness Check of DID Models for All School Districts in Vermont.....	101

Table 11: The Results of Robustness Check of DID Models for Poor School Districts in Vermont.....	102
Table 12: The Results of Robustness Check of DID Models for Rich School Districts in Vermont.....	104
Table 13: Descriptive Characteristics of per Pupil Spending in Reform and Comparison States: 1997-2002.....	106
Table 14: Descriptive Characteristics of Enrollment of School Districts in Reform and Comparison States: 1997-2002	109
Table 15: Descriptive Characteristics of Percentage of Students in Free Lunch Program in Reform and Comparison States: 1997-2002 (Unbalanced).....	112
Table 16: Descriptive Characteristics of Percentage of Students in Free Lunch Program in Reform and Comparison States: 1997-2002 (Balanced).....	112
Table 17: Correlation of All Variables for New Hampshire.....	114
Table 18: DID Estimation of the Effect of the School Finance Reform in New Hampshire on per Pupil Spending in All School Districts with Comparison State Maine and Rhode Island.....	116
Table 19: DID Estimation of the Effect of the School Finance Reform in New Hampshire on per Pupil Spending in Poor School Districts with Comparison State Maine and Rhode Island.....	119
Table 20: DID Estimation of the Effect of the School Finance Reform in New Hampshire on per Pupil Spending in Rich School Districts with Comparison State Maine and Rhode Island.....	121

Table 21: The Results of Robustness Check of DID Models for All School Districts in New Hampshire.....	123
Table 22: The Results of Robustness Check of DID Models for Poor School Districts in New Hampshire.....	125
Table 23: The Results of Robustness Check of DID Models for Rich School Districts in New Hampshire.....	126

ABSTRACT

At the end of the 20th century, the constitutionality of school finance systems in Vermont and New Hampshire was challenged in courts for the first time. Both states initiated dramatic school finance reforms after their school finance systems were ruled as unconstitutional. This study aims to evaluate the effect of these school finance reforms on per pupil spending. By utilizing a quasi-experimental research design and using school districts in Maine and Rhode Island as comparison groups, this study found that the school finance reform in Vermont decreased per pupil spending in rich school districts by 4-10% and increased per pupil spending in poor school districts by 2-5%. The school finance reform in New Hampshire decreased per pupil spending in rich school districts by 5-7% and had no significant effect on per pupil spending in poor school districts. The results from this study suggest that restrictions on the use of state aid are needed and should be considered in the process of school finance policy design. This study also indicates that directly transferring property tax revenue from rich school districts to poor school districts is probably not a sustainable way to achieve equity in education spending because it stimulates strong opposition from school districts whose tax revenue is recaptured.

CHAPTER 1 INTRODUCTION

Introduction

The system of public education in the United States was founded by local financing and local control (Card & Payne, 2002). Local communities historically provided most of the funding for the establishment and maintenance of public schools through property tax. In 1920, local revenue accounted for 83% of the total revenue for education at the national level. Since the 1920s, increase in the variation of local wealth resulted in many state governments beginning to assume more responsibility to fund public schools (Reschovsky, 1994). The average proportion of state funding in total school funding rose from 30% in 1940 to 40% in 1970 (Card & Payne, 2002). In the 1970s, local revenue was still the largest source of educational revenue. For example, local revenue accounted for 52% of total revenue in education in 1970. Since the 1970s, due to variation of property tax base among school districts, heavy reliance on local revenue led to the fact that property poor school districts could not provide adequate education resources to students, and this produced huge inequity in educational spending among school districts (Reschovsky, 1994). At the same time, property tax rate in poor school districts would be much higher if they wanted to keep the same level of educational spending as in rich school districts (Reschovsky, 1994).

The heavy reliance on local revenue in funding education has resulted in 125 school finance litigations in 45 states since the late 1960s (Springer, Liu, & Guthrie, 2009) . Plaintiffs in a majority of school finance litigations were taxpayers, students, or/and school districts in property poor areas. What they demanded was that state governments

should take the responsibility to provide adequate and equitable education to every student and reform state education finance systems (Thompson & Crampton, 2002).

Specifically, these school finance litigations could be divided into three waves based on the arguments of plaintiffs (Thro, 1990). The first wave began at the end of the 1960s. Plaintiffs resorted to the equal protection clause of the United States Constitution. The legal argument behind these litigations was equity in education expenditure. They argued that education expenditure should not be a function of property wealth of school districts. This was also called “fiscal neutrality” (Reschovsky, 1994). This wave of finance litigations ended with the *San Antonio Independent School District v. Rodriguez* (1973) in which the United States Supreme Courts ruled that the state finance system in Texas did not violate the United State Constitution. Plaintiffs in the second wave of school finance litigations resorted to education and equal protection clause in state constitutions. They argued that inequity of educational expenditure among school districts violated state constitutions. The third wave began with the *Rose v. Council for Better Education* (1989). In this case, the Kentucky Supreme Court ruled that the whole public school finance system in Kentucky was unconstitutional because students in property poor school districts received inadequate and inferior educational opportunity. The legal argument behind these litigations was adequacy of educational spending. Here, adequacy means that each student should be provided adequate educational resources to meet minimum standard of proficiency (Springer et al., 2009).

Since the 1970s, many states reformed their school finance systems either in response to court orders as a result of school finance litigations or by the action of legislators. For example, from 1970 to 1997, 16 states had court-order reforms, and 21

states initiated school finance reforms without court order (Evans, Murray, & Schwab, 1997). Corresponding to the demands of plaintiffs in school finance litigations, equity in educational spending was the main goal in early school finance reforms. For example, the school finance reform in California after *Serrano v. Priest* (1971) increased the foundation grant, which is the amount of funding that state governments decide to spend to make sure that each student has at least minimum quality education, and imposed limits on expenditure per pupil in rich school districts (Downes, 1992). These measures tried to reduce the correlation between educational expenditure and property wealth in school districts. Since 1989, school finance reforms started to focus on adequacy of educational spending. For example, the school finance reform in Kentucky after the *Rose v. Council for Better Education* (1989) increased the state's foundation grant and adjusted property assessments so that poorer school districts received a larger share of state aid (Murray, Evans, & Schwab, 1998).

Another feature of school finance reforms occurring since the 1970s was that they were characterized by the increased role of state governments in controlling and funding public education (Loeb, 2001). For example, the state share of school spending increased by more than 10% from 1971 to 1983 in five states (California, Wyoming, Washington, Connecticut, and New Jersey) (Theobald & Picus, 1991). Evidence from the Common Core of Data (CCD) of the U.S. Department of Education's National Center for Education Statistics (NCES) shows that six states increased state share of school spending by more than 10% after school finance reforms in the 1990s (Kansas, Oregon, Wisconsin, Michigan, Vermont, and New Hampshire). After these dramatic school finance reforms

in the 1990s, no state experienced this type of dramatic change in the ratio of state revenue in total educational revenue.

Did these school finance reforms affect the level and distribution of education expenditure in school districts? This question earned much attention from researchers. However, two problems exist in the extant literature. First, studies with national sample data treated school finances of states as homogeneous (e.g. David Card & Payne, 2002; Murray, Evans, & Schwab, 1998). Considering the vast difference in school finance system of states, this is not appropriate. School finance systems are extremely diverse in the United States. The diversity comes from three aspects. First, there are 50 distinctive school finance systems because the authority to govern and fund public education lies with state governments. Second, revenue for education is from federal, state, and local governments. In 2009, at the national level, 47% of revenue for public schools was from states, 44% was from local governments, and the remaining 10% was from the federal government. However, the composition of the three revenue sources varies a lot from state to state, for example, the state share was 92% in Vermont while it was 27% in Rhode Island in 2009, and other states were in between. Third, school finance systems in the United States are interwoven with a variety of tax systems because school revenue comes from many kinds of taxes such as income, sale, and property tax.

Second, the literature on school finance reform of individual states does not provide a conclusive answer to the effect of school finance reforms on per pupil spending. For instance, Downes (1992) found that the school finance reform in California after the *Serrano v. Priest* (1976) decreased the dependence of education expenditure on local wealth. However, total funding of public education fell between 10 and 15% compared to

the rest of the United States after the reform because it limited the supplementary funding of wealthier school districts (Fernandez & Rogerson, 1999). Michigan took a revolutionary school finance reform called *Proposal A* in 1994. By using a Difference in Difference model Chaudhary (2009) found that *Proposal A* increased operating expenditures of school districts. He also found that the increase in expenditures led to higher teacher salaries and smaller class sizes in Michigan. In Massachusetts, after the decision of the *McDuffy v. Secretary of the Executive Office of Education* (1993), in which plaintiffs prevailed, the state passed the *Massachusetts Education Reform Act* in 1993. Dee and Levine (2004) found that the reform increased state aid to school districts. Total educational revenue increased although local revenue for education decreased. This indicated the existence of flypaper effect. In sum, the effect of school finance reforms varied among states. One reason for this variation is that the approach that each school finance reform took were various (Hoxby, 2001). Thus, it is important to evaluate the impact of school finance reforms state by state.

Statement of the Problem

Vermont and New Hampshire were the last two states which took dramatic school finance reforms in the trend of school finance reforms occurring since the 1970s. School finance reforms in Vermont and New Hampshire are worthwhile to study for three reasons. First, the state share of education spending dramatically increased after the school finance reforms in both these states. This dramatic change in the state share provides a good opportunity to study the effect of the increase of state involvement on educational spending. Second, budget decisions in both states were made in a town meeting in the form of direct democracy, and there was no limit on local education

budget in both states. These two features provide a good opportunity to see how local residents respond to school finance reforms. The third reason is related to methodology. There is sufficient annual data before and after reform to evaluate the effect of school finance reforms in these two states. In the following sections, I will introduce the school finance reforms in Vermont and New Hampshire and describe the objectives of this study. Throughout the dissertation, when the school finance reforms in Vermont and New Hampshire are mentioned, I mean these two specific school finance reforms that I will introduce in the following section, except indicated otherwise.

Vermont

In February of 1997, the first year after the Democratic Party controlled both houses and governorship, the Vermont state legislature passed the *Equal Educational Opportunity Act* of 1997, also called as *Act 60*. This Act was a response to the Vermont Supreme Court decision in the case of *Brigham v. State* (1997) that held the state's education finance system unconstitutional. *Act 60* established a statewide property tax and a two-tier guaranteed yield school finance formula. The first tier was a power equalization program in which the state government decided a statewide property tax rate and base block grant that each school district received on the condition that they levied the statewide property tax. For the second tier, the state government guaranteed that school districts yielded the same tax revenue at the same additional property tax rate above the statewide property tax rate. Since the revenue source only came from the property tax revenue of school districts that were willing to spend more than the base block grant, this proposition means that poor school districts got more funding from the state government and rich school districts lost a portion of local property revenue which

was redistributed to poor school districts. The reform increased the state share of public school funding from 28% to 70% in the first year. It dramatically changed the school finance system in Vermont.

The responses of voters to the reform were mixed and sometimes confrontational. It was welcomed by poor school districts because they obtained more state aid. It was aggressively opposed by rich school districts through protests, lawsuits, and parents sending children to private schools and setting up private foundations to support schools that their children attended (Rebell & Metzler, 2002).

New Hampshire

In April of 1999, in response to the *Claremont v. Governor* (1997) decision in which the New Hampshire Supreme Court invalidated the state property tax system and declared that the New Hampshire school finance system was not adequate, its state legislature passed the House Bill 999 (Olabisi, 2006). The Bill established a statewide property tax at \$6.60 per \$1,000 of property value and defined an adequate education grant. This grant was calculated based on the cost of schools in which 40 to 60% of third and sixth grade students passed statewide-standardized tests. The state government made up the difference of the adequate education grant and the statewide property tax revenue that each school district produced and held. The portion of the statewide property tax revenue more than the adequate education grant in school districts was recaptured into a state education fund. This was called the recapture proposition. School districts were allowed to levy additional property tax other than the statewide property tax. The school finance reform pushed the state share of school spending from 9% up to 55% in the first

year. The recapture proposition induced much opposition from rich school districts (Olabisi, 2006).

The school finance reforms in Vermont and New Hampshire were extremely controversial because they dramatically changed the incentives and constraints that different school districts in these two states faced. To increase education spending in poor school districts and decrease inequity in educational spending among school districts were the aims of the school finance reforms. There are two potential reasons that these aims might not have been achieved. First, recapture provisions can discourage rich school districts from spending more money on schools because they lose a portion of local property revenue for education to state governments (Hoxby, 2001; Schmidt & Scott, 2006). Second, the increase of state aid to poor school districts does not necessarily increase education spending in these school districts because they might use the increased revenue to reduce tax burden of local residents (Card & Payne, 2002; Lutz, 2010). Therefore, it is not clear whether the school finance reforms in Vermont and New Hampshire increased per pupil spending for school districts. The impact of school finance reforms in Vermont and New Hampshire on per pupil spending has not been empirically explored.

Purpose of the Study and Research Questions

This study aims to evaluate the impact of school finance reforms in Vermont and New Hampshire on per pupil spending. Educational spending represents the level of educational resource available to students. More educational spending means that school districts could reduce class size and teacher-student ratio, and hire higher qualified teachers, and improve school facilities (Chaudhary, 2009; Figlio, 1998). The increase of

educational spending could also increase student performance (Card & Payne, 2002; Chaudhary, 2009). Therefore, it is important to know whether the school finance reforms in Vermont and New Hampshire changed educational spending in school districts. Further, since the demands of the plaintiffs in the school finance litigations in Vermont and New Hampshire were to increase per pupil spending in property poor school districts and decrease inequity in education spending, it is worthwhile to know whether the school finance reforms in Vermont and New Hampshire, as a result of the school finance litigations in both states, achieved the aims of these plaintiffs.

The school finance reforms in both states produced different incentives and constraints, for example, the power equalization program and recapture proposition introduced above, to rich and poor school districts. Besides the effect of the school finance reform on all school districts, this study also evaluates their effect on per pupil spending in poor and rich school districts. Specifically, the research questions are:

1. Did the school finance reforms in Vermont and New Hampshire change per pupil spending for all school districts?
2. Did the school finance reforms in Vermont and New Hampshire change per pupil spending for poor school districts?
3. Did the school finance reforms in Vermont and New Hampshire change per pupil spending for rich school districts?

Theoretical Framework

Since school budgets were decided by local school districts in Vermont and New Hampshire, evaluating the impact of school finance reforms in the two states on per pupil spending essentially entails examining how local school districts determined their

budgets in response to the new school finance systems. The literature on determinants of local public spending and the literature on the effect of intergovernmental grants are relevant to this study. This is because education spending is one important part of local public spending, and the school finance reforms in Vermont and New Hampshire involved the change in intergovernmental grant.

The prior research on determinants of local public spending tried to find out what determines the level of local public spending such as school spending. They derived these determinants from the economic theory on consumer behaviors in private consumption (Bergstrom & Goodman, 1973). Since price and income are important factors determining the consumption of private good and service of individuals, they assumed that the distribution of income of local residents and the distribution of tax price that local residents pay are important determinant of local public spending, which could be understood as the consumption of public service (Bergstrom & Goodman, 1973). Tax price refers to how much local school districts need to pay if they want to spend one more dollar on public service such as education.

In order to understand how the distribution of price and income among local residents determine the level of public spending in local governments such as school districts, median vote theory should be introduced. Median voter theory aims to explain and predict the result of public choice. It argues that public choice represents and adopts the preference of a median voter if the choice is picked by a simple majority rule and the median voter has a single-peaked preference point (D. Black, 1948; Downs, 1957). This theory applies to a variety of fields including elections and public policies. Local government expenditure is one of these. In considering both median voter theory and the

implication from the determinants of individual consumption of private good and service, it could be concluded that the tax price and income of median voter could be determinants of local public spending. In the area of local education spending, it means that the tax price and income that a median voter in school districts has would determine the level of education spending. Empirical studies using this theory proved that median voter income and tax price in a school district were significantly correlated with school spending (Denzau & Grier, 1984; Hoxby, 2001).

The school finance reform in Vermont changed tax price for school districts. Since this change applied to entire school districts, all voters including the median voter experienced the change in tax price. Thus, studies on the effect of tax price on local public spending are useful for analyzing the effect of the school finance reform in Vermont. Tax price was one for all school districts in Vermont before the school finance reform. After reform, tax price changed into less than one for poor school districts, and more than one for rich school districts. Thus, only considering the change in tax price, the school finance reform in Vermont should have a negative effect on per pupil spending in rich school districts and a positive effect on per pupil spending in poor school districts.

Another relevant theory is the equivalence theory on the effect of intergovernmental grants. An intergovernmental grant is the grant that high level governments give to low level governments to use it for the provision of public service, for example, federal grant given to states for public transportation and state grant given to school districts. It is argued that the effect of intergovernmental grant on local public spending is equivalent to the effect of income increase of local residents if local budget decision reflects the preference of local residents (Bradford & Oates, 1971). The

equivalence theory is relevant to this study because both school finance reforms in Vermont and New Hampshire involved changes in intergovernmental aid. Further, several characteristics of the school finance systems in Vermont and New Hampshire provide conditions in which school budget could reflect the preference of local residents. First, budget decisions are voted on by local residents instead of representatives in a form of direct democracy in a town meeting in a majority of school districts in both states. Only a few city school districts pass their education budgets by city councils. Second, no limit on educational expenditure or property tax rate in Vermont and New Hampshire exists while it exists in some states such as California and Massachusetts. Third, residents were highly aware of the school finance reforms as evidenced by fierce debates on school finance reform bill in court, legislature, and media before they were passed (Lutz, 2010; Schmidt & Scott, 2006). It is highly possible that the budget decision could represent the preference of local residents in Vermont and New Hampshire. Therefore, the equivalence theory is applicable to the analysis of school finance reforms in Vermont and New Hampshire.

Research Design

This study utilized a quasi-experimental research design by using school district expenditure data derived from the Common Core of Data (CCD) from the National Center for Education Statistics (NCES). Specifically, I conducted a Difference in Difference (DID) design because it could control for the influence of unknown variables on the estimation of the effect of school finance reforms on per pupil spending of school districts. This design utilizes a control state in which school districts did not experience school finance reforms and is as similar as possible to school districts in a reform state.

DID design also uses pre-reform and post-reform data. The difference of pre-reform and post-reform per pupil spending of school districts in the reform state is subtracted from the difference of pre-reform and post-reform per pupil spending of school districts in the comparison state. The difference in difference result could be interpreted as the estimation of the effect of school finance reforms (Angrist & Pischke, 2008; Meyer, 1995). This design can largely control for bias of factors that influenced outcome variable of interest in both reform and comparison states at the same time. School district fixed effect is also included in the model to control for the influence of time-invariant factors among school districts on the estimation of the effect of school finance reforms (Angrist & Pischke, 2008).

Also, because the measures that Vermont and New Hampshire took were different, I evaluated the school finance reforms in these two states separately, and examined each state as the independent treatment group in the separate DID analyses. Maine and Rhode Island were used as their comparison states. The reason for this choice is that the school finance systems in Maine and Rhode Island did not change during the same period when Vermont and New Hampshire reformed their school finance systems. Also, because these two states are neighbor states of Vermont and New Hampshire they share many features in their economic status and political systems. For example, school budgets are voted by residents in a town meeting in most of school districts in the four states, and there is no expenditure and tax rate limit on local education budgets.

The Significance of the Study

The results of prior studies on the impact of school finance reforms on per pupil spending in individual states are inconsistent (Dee & Levine, 2004; Manwaring &

Sheffrin, 1997; Silva & Sonstelie, 1995). This is due to the complexity of school finance reforms and the particularity of political structures in each state. The results from this study will contribute to the existing literature regarding school finance reforms in two aspects. First, there is no research on the effect of school finance reforms including recapture propositions in both Vermont and New Hampshire. This study will provide suggestive evidence on how these recapture propositions influenced per pupil spending in school districts whose revenue was recaptured.

Second, prior literature has supported the existence of the flypaper effect of intergovernmental aid on local public spending (Card & Payne, 2002; Fisher & Papke, 2000; Hines & Thalar, 1995). The flypaper effect indicates that local residents in recipient governments such as school districts would spend more out of these intergovernmental grants on a targeted public service such as public education than they spend from an equivalent personal income increase. However, recent studies shows that the flypaper effect might not exist in some cases (Duggan, 2000; Gordon, 2004; Lutz, 2010). This study will examine whether the distinctive procedure of budget decision and the structure of school finance system played a role in the existence of flypaper effect of intergovernmental grants.

This study also provides valuable implications on school finance policies. First, if the increased state aid without restriction of its use did not increase per pupil spending in poor school districts in Vermont and New Hampshire, this introduces doubt on the efficiency of increasing state aid to poor school districts for increasing educational spending in those districts. Thus, legislators might need to take additional measures to guarantee that the increased state aid be spent on education if an increase in educational

spending is the aim of the policy. Second, if this study shows that the recapture proposition, which directly redistributed property revenue from rich school districts to poor school districts, did decrease per pupil spending in these recaptured school districts, it will provide empirical evidence that explain why the recapture proposition was repealed in both Vermont and New Hampshire. This could remind policy makers that the recapture proposition might not be a sustainable way to take.

Definitions of Terms

Tax price refers to how much local residents need to pay if they want to spend one more dollar on public service (Bergstrom & Goodman, 1973).

Horizontal equity means that equally situated students are treated equally in per pupil spending (Springer et al., 2009).

Vertical equity means that differently situated students are treated differently (Springer et al., 2009). For example, the spending for students in special education should be more than regular students because it costs more than educating regular students.

Adequacy means that each student should be provided adequate educational resource to meet minimum standards of proficiency (Springer et al., 2009). It is difficult to find criterions to decide the amount of adequate educational resource. In practice, some states calculate it based on the cost of schools whose students have good performance in standardized test. Some states sum up the costs of individual services such as teacher salary and transportation. The cost of individual services is usually estimated by average cost in one state or estimated by education experts (Reschovsky & Imazeki, 2001).

School finance litigations refer to litigations in which plaintiffs challenge the constitutionality of state school finance formulas. The plaintiffs usually include school districts, students, and their parents. The defendants are state governors, state education agencies, or/and the state legislature. School finance litigations started since the late 1960s. Equal and adequate school spending are the core demands from these plaintiffs.

The flypaper effect refers to the phenomenon that recipient governments would spend more intergovernmental grant on public services than they spend their equivalent income increase (Hines & Thalar, 1995).

Foundation program is a school finance system in which local districts contribute to school funding usually through a uniform property tax rate while states make up the difference up to the specified amount of foundation grant.

Power equalization systems are school finance systems in which state governments assure that school districts get the same revenue at the same property tax rate regardless of their property valuation. School districts determine spending and tax rate within state-determined limits, and then state governments match the differences between actually raised amount by the tax rate that school districts choose and the guaranteed amount at the tax rate that state governments decide. It is also called guaranteed yield program.

Summary

This chapter provides an introduction of this study. First, it presents the history of school finance reforms in the United States. What do school finance systems in the United States look like? Why are school finance systems challenged? How are they reformed? Then I introduce the school finance reforms in Vermont and New Hampshire.

After that, the research questions are presented on the effect of school finance reforms in Vermont and New Hampshire. The theoretical framework that this study uses is derived from the literature on the determinants of local public spending and the effect of intergovernmental grants. A Difference in Difference research design is used in this study because it could effectively control for the influence of many confounding variables in the estimation of the effect of the school finance system in the Vermont and New Hampshire. This study provides empirical evidence on the effect of recapture propositions in school finance reforms and shows how institutional characteristics influence the effect of intergovernmental grants.

CHAPTER 2 LITERATURE REVIEW

Introduction

Before evaluating school finance reforms in Vermont and New Hampshire, I review literature on what is already known about school finance, school finance reforms, and the theoretical and empirical evaluation of school finance reforms. This chapter is divided into four sections. First, I introduce the school finance system in the United States and its history. This provides a big picture of school finance systems in the United States, and is helpful to understand the effect of school finance reforms in Vermont and New Hampshire. Second, I introduce the history of school finance systems in Vermont and New Hampshire and the school finance reforms in these two states that this study focuses on. This provides background information to understand the effect of the school finance reforms in Vermont and New Hampshire. Third, I summarize empirical studies on school finance reforms. Lastly, I outline the theoretical framework that this study uses and present the main hypothesis of this study.

School Finance in the United States

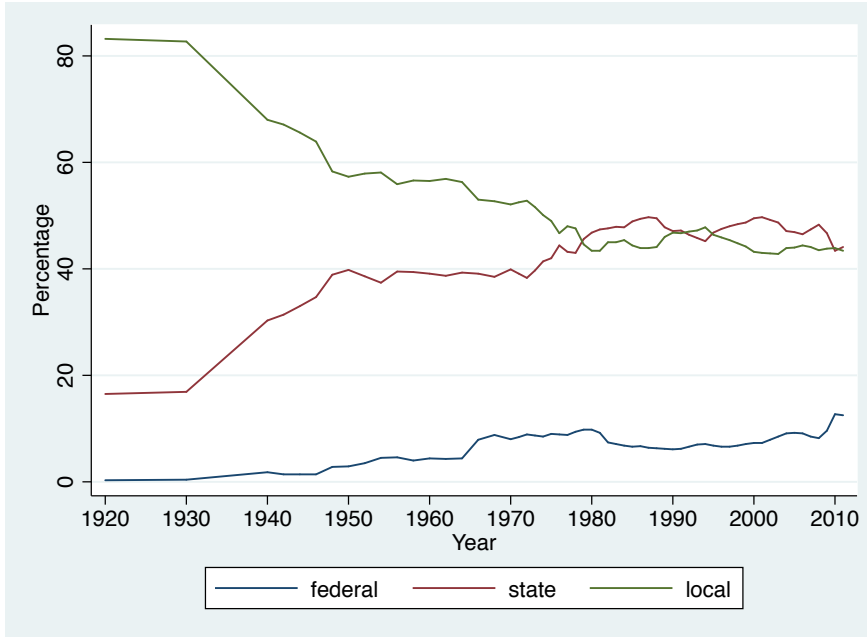
School Finance Systems

In the United States, education is not endowed to the federal government in the Constitution. Therefore, the federal government has limited authority to manage education and bears limited responsibility of funding public education. On the contrary, there are education clauses in every state constitution (Figlio, Husted, & Kenny, 2004). For example, the *California Constitution* regulates that “the Legislature shall encourage by all suitable means the promotion of intellectual, scientific, moral, and agricultural improvement”(Article 9, Section 1). The *Constitution of the Commonwealth of Kentucky*

regulates that “the General Assembly shall, by appropriate legislation, provide for an efficient system of common schools throughout the State”(Section 183). The *Constitution of the State of New Jersey* regulates that “the Legislature shall provide for the maintenance and support of a thorough and efficient system of free public schools for the instruction of all the children in the State between the ages of five and eighteen years” (Article VIII, § IV). As shown in these three examples, the wordings of educational clauses are different. This difference plays an important role in the differences of school finance system in each state and the school finance litigations that I will introduce later. However, one common theme is that funding and administrating public education is a state responsibility in the United States.

Historically, the system of public education was founded by local financing and local control in the United States (Card & Payne, 2002). As Figure 1 shows, local revenue accounted more than 80% in total educational revenue in the 1920s. From 1930 to 1950, state share of total revenue for education increased from 17% to 40%. State share stayed stable from 1950 to 1970. From 1970 to 1990, state share increased from 40% to 47% since many states reformed their school finance reforms since the 1970s. After that, it experienced a small decrease or increase each year. This figure only shows the change in the distribution of revenue among federal, states, and local governments at national level. Since school finance system in each state experiences a distinctive trajectory of development, the composition of the three revenue sources varies a lot from state to state (as shown in Figure 2), for example, the state share was 92% in Vermont while the state share was 28% in Rhode Island in 2009, and other states were in between.

Figure 1. Percentage of Education Revenue From Federal, State, and Local Source From 1920-2011

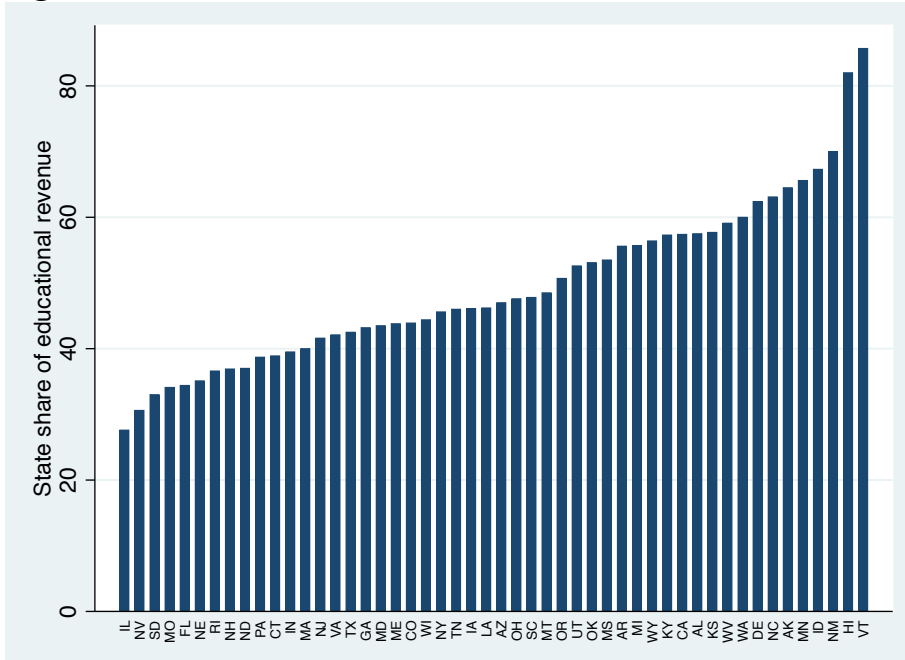


Note. From 1920 to 40, data is available by decades, from 1940 to 1970, data is available every two year, afterwards, data is available annually; Beginning in 1980-81, revenue for state education agencies are excluded. Beginning in 1988-89, data reflect new survey collection procedures and may not be entirely comparable with figures for earlier years.

Source: U.S. Department of Education, National Center for Education Statistics, *Digest of Education Statistics*, 2000, Table 158.

Revenues for public elementary and secondary schools, by source of funds: 1919-20 to 1997-98. *Digest of Education Statistics*, 2013, Table 235.10. Revenues for public elementary and secondary schools, by source of funds: Selected years, 1919-20 through 2010-11.

Figure 2. State Share of Total Educational Revenue of 50 States in 2009



Source: U.S. Department of Education, National Center for Education Statistics, Common Core of Data (CCD), "National Public Education Financial Survey (NPEFS)," fiscal year 2009, Version 1a.

Since state governments and local school districts account for most of funding for schools, school finance systems in each state mainly focus on two issues: how the responsibility of funding education is distributed between state and local governments and how state aid for schools is distributed among school districts. Verstegen and Jordan (2009) synthesized a comprehensive survey of school finance programs in 50 states conducted in 2007. It provided a snapshot of current state school finance systems. They found that state finance systems could be categorized into one of four finance formulas: (a) foundation program, (b) power equalization system, (c) full-state funding, and (d) flat grant. These are prototypical school finance formulas. The actual formula in each state is more complicated than these introduced here.

Foundation program. In a foundation program, state governments guarantee that each student attains a basic foundation grant. A foundation grant is the amount of funding that state governments decide to spend in order to make sure that each student has at least minimum quality education (Verstegen & Jordan, 2009). State governments and local school districts both contribute to the grant. There is a minimum property tax rate requirement for school districts to get state aid. The amount of state aid is the difference between the basic foundation grant and the property tax revenue at the required minimum property tax rate. Normally, the foundation grant is less than the actual educational expenditure of school districts (Verstegen & Jordan, 2009). School districts are allowed to levy property tax other than the required minimum property tax rate. The supplementation by extra property tax revenue is unlimited in some states, and capped in other states. As of 2009, the foundation program was being used by 40 states and five other states combined it with other school finance formulas (Verstegen & Jordan, 2009).

In terms of the effect of the foundation program on equity and adequacy in educational spending, it depends on many variables in the formula, for example, the amount of foundation grant, the required minimum local property tax rate, and whether the supplementation of local revenue is capped. If the amount of foundation grant is set at a higher level, that could increase the adequacy in educational expenditure in poor school districts. Equity in educational expenditure would be increased if the supplementation of property tax revenue by school districts is capped because this limits the increase in educational spending in rich school districts.

Power equalization program. In a power equalization school finance program, state governments decide a guaranteed property tax base, and then local school districts determine local property tax rate within state-determined limits. The state aid is a product of locally decided property tax rate and the difference between the guaranteed property tax base and the actual property tax base in school districts (Fernandez & Rogerson, 2003; Schmidt & Scott, 2006). There are two important features about this school finance formula. One is that it can guarantee that the same property tax rate produces the same property tax revenue for school districts. The other is that property poor school districts attain more grants from states than rich school districts. In order to make up the difference between guaranteed property tax base and actual property tax base in school districts, state governments either use general state revenues or recapture excess revenue beyond the revenue produced from the amount of the guaranteed property tax base in rich school districts. Therefore, power equalization systems could achieve tax equality and might improve the inequity in educational expenditure among school districts (Reschovsky, 1994). It was reported in 1994 that a third of states in the United State

adopted this type of school finance formula (Reschovsky, 1994). A more recent study showed that in 2007, three states were using power equalizing systems and five states incorporated this formula with other school finance formulas (Verstegen & Jordan, 2009).

Regarding effectiveness in promoting equity in educational spending of power equalization, theoretically, the extent to which power equalization programs could decrease the inequity in educational spending depends on the level of guaranteed tax base (Reschovsky, 1994). If the guaranteed tax base is higher, it means that state governments provide more state aid to property poor school districts. This could decrease the inequity in educational expenditure among school districts. However, in this case, state governments need to increase revenue by raising certain kinds of state tax rate, which is not politically easy to do. Also, the recapture of property revenue from rich school districts will also produce strong opposition from voters in rich school districts. Reschovsky (1994) argued that the increase in educational expenditure in property poor school districts with the power equalization formula depends on the tax rate that local school districts choose. Since the tax effort is inelastic with respect to local wealth, the tax rate would not change much if local wealth does not change (Reschovsky, 1994).

Full-state funding. Full-state funding is a fully state centralized school finance system. In this system, state governments are responsible for funding schools without funding from school districts. This school finance system is only used in Hawaii (Verstegen & Jordan, 2009).

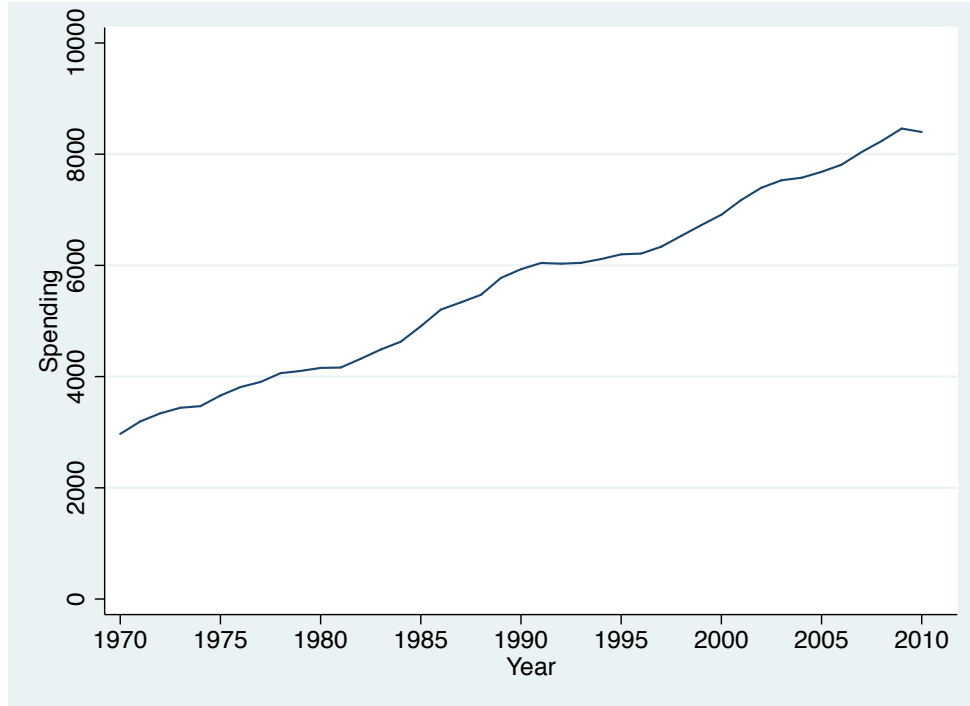
Flat grant system. In a flat grant program, local school districts are responsible for funding schools, and state governments distribute state aid to school districts based only on the number of students. Flat grant was used in some states in the early stage of

20th century in order to decrease the inequity in pure local school finance systems (Reschovsky, 1994). In 1976, six states exclusively used flat grant programs to district state aid (Card & Payne, 2002). But flat grant system did not effectively solve the inequity in education spending (Reschovsky, 1994). At present, it is only used by North Carolina (Verstegen & Jordan, 2009).

Public School Spending

Public school spending increased rapidly since 1970s in the United States. As Figure 3 shows, average per pupil spending increased from \$5,019 in 1970 to \$8,399 (2000 equivalent dollar) in 2010. It also shows that average per pupil spending gradually increased from 1970 to 2010 except the period of 1977-1981 and the period of 1991-1996 during which average spending either experienced a small decrease or stayed stable. Further, average education spending in the United States varies from state to state. For example, in 2009, average per-pupil spending for elementary and secondary education was less than \$8,000 in Utah, Idaho, Arizona, Oklahoma, and Tennessee while it was more than \$16,000 in New Jersey, New York, and Washington DC.

Figure 3. Current Expenditure per Pupil in Public Elementary and Secondary Schools: 1970-2010



Note: Data are converted into 2000 dollar

Source: U.S. Department of Education, National Center for Education Statistics, *Digest of Education Statistics*, 1989, Table 145. Total and current expenditure per pupil in public elementary and secondary schools: 1919-1920 to 1988-89; 2002, *Digest of Education Statistics*, 2002, Table 169. Current expenditure per pupil in in fall enrollment in public elementary and secondary schools, by state: 1959-60 to 1999-2000; *Digest of Education Statistics*, 2013, Table 236.65. Current expenditure per pupil in fall enrollment in public elementary and secondary schools, by state or jurisdiction: Selected years, 1969-70 through 2010-11.

Regarding the inequity in education spending, Murray et al., (1998) used every five years data on education spending from 1972 to 1992 to measure it. At national level, inequality in spending, expressed by coefficient of variation, decreased from 1972 to 1982, yet increased from 1982 to 1992. They also found that between-state inequality was much larger than within-state inequality during the period. For example, variation across states represented 65% of the total variance in per pupil spending at the national level.

Baird (2008) used a dataset from 1990 to 2000 and calculated the coefficient of variation and range ratio to describe the level of inequity in educational revenue among

school districts within and across states. The coefficient of variation is the standard deviation of total revenue of school districts divided by average level of total revenue of school districts. The range ratio is the ratio of per-pupil resources for the 95th and 5th percentile school districts. She found that funding disparities within states declined by about 8-15%, whereas disparities across states declined by 22-25%. Funding disparities at the national level decreased by 19-22%. She also found that federal dollars played virtually no role in reducing disparities in education spending.

Another way to measure the inequity in education resources is to look at the relationship between total revenue on education and median household income in school districts. Baker and Welner (2010) used this method to study the inequality between school districts. The dataset that they used consisted of 10,189 unified public school districts from 1990 to 2005. At the national level, the relationship between revenue on education and median household income became less positive from 1990 to 2005. This indicated that total revenue on education became less dependent on median household income. They also found that Illinois actually became more regressive over time because educational revenue became more dependent on median household income, while states like New Jersey, Massachusetts and Ohio made genuine progress in the equality of school funding. They concluded that problems with between-district revenue disparities were still not resolved.

School Finance Litigations

Local communities historically provided most of the funding for the establishment and maintenance of public schools through property tax in the United States. For example, local revenue accounted for 83% of total revenue for education at national level in 1920.

Since the 1920s, many state governments began to assume more responsibility to fund public schools (Reschovsky, 1994). The state share of total school funding rose from 30% in 1940 to 40% in 1970 (Card & Payne, 2002). However, local revenue was still the largest source of educational revenue in 1970. In 1970, local revenue accounted for 52% of total revenue on education. Due to the variation of property base among school districts, heavy reliance on local revenue leads to the fact that property poor school districts cannot provide adequate education resources to students, and this produces huge inequity in educational spending among school districts (Reschovsky, 1994). At the same time, property tax rate would be much higher in property poor school districts if they wanted to keep the same level of educational spending as in rich school districts (Reschovsky, 1994).

Motivated by lower education spending and higher property tax burden, school districts, students and their parents resorted to courts to secure adequate and equitable education spending at the end of 1960s. The plaintiffs in these school finance litigations challenged the constitutionality of state school finance formulas. The defendants were usually state governors, state education agencies, or/and state legislatures. Since the responsibility of providing education was dedicated to state government in all states of the United States (Figlio et al., 2004; Thro, 1990), these plaintiffs alleged that state governments failed to fulfill their responsibility. According to the Education Finance Statistics Center, there were 125 school finance litigations from 1970 to 2009. School finance systems of 45 states were challenged and 25 were overturned during this period (Springer et al., 2009).

Based on the characteristics of litigations, Thro (1990) viewed that there were three waves of finance litigations. In the first wave of school finance litigations, plaintiffs hoped to use equal protection clause in the United States Constitution to secure children equal rights to education. The legal argument of these litigations was based on the fiscal neutrality theory. This theory argued that the level of education expenditure should not be related to the level of local wealth. In the *Serrano v. Priest* (1971) case, the California Supreme Courts accepted the argument based on the equal protection clause in both the United States Constitution and the California State Constitution. However, the first wave of litigations ended with the *San Antonio Independent School District v. Rodriguez* (1973). In this case the United States Supreme Court denied the request of the plaintiffs and held that education was not a fundamental right according to the United States Constitution, and that they could not decide the constitutionality of school funding disparity between school districts in states.

In the second wave, school finance reform advocates started to resort to the state supreme courts by using education clause and equal protection in state constitutions (Thro, 1990). For example, in the case of *Robinson v. Cahill* (1973) the New Jersey Supreme Court argued that the state constitution regulated that the state should provide “a thorough and efficient system of free public schools”, and the inequality in education spending violated the constitutional mandate. What plaintiffs complained about in this wave of litigations was the inequality of educational spending in school districts. However, the use of the education clause and equal protection clause of state constitutions in school finance litigations was not very successful for plaintiffs. There

were six cases where plaintiffs prevailed, 15 cases where plaintiffs were defeated (Thro, 1990).

The third wave of litigations started with the *Rose v. Council for Better Education* (1989). In this case, the Kentucky Supreme Court ignored the equal protection clause in its state constitution, and exclusively relied on the education clause that “the General Assembly shall, by appropriate legislation, provide for an efficient system”. By reviewing the data on the expenditure of local school districts, the Kentucky Supreme Court ruled that the whole public school finance system in Kentucky was unconstitutional because students in property poor school districts received inadequate and inferior educational opportunity. The argument in this case was called adequacy argument. The argument maintained that state governments should provide sufficient educational resources to support students to achieve a minimum standard of proficiency. This argument was used in many later school finance litigations. For example, in the *McDuffy v. Secretary of the Executive Office of Education* (1993), the Massachusetts Supreme Judicial Court ordered that the school finance system in Massachusetts was unconstitutional because it failed to provide adequate education to students. There were twenty states whose school finance systems were declared as unconstitutional based on the adequacy argument from 1989 to 2005 (Springer, Liu, and Guthrie, 2009).

School Finance Reforms

Since the 1970s, many states have reformed their school finance systems either in response to court orders as a result of school finance litigations or by the action of legislators. This began with the school finance reform in California after the *Serrano v. Priest* (1971) decision in which the California Supreme Court ruled that the state school

finance system was unconstitutional. Further, *Proposition 13*, which was passed in 1978, limited maximum property tax rate to one percent. This reform transferred much authority to levy educational revenue and decide educational budget from school districts to state governments (Downes, 1992). From 1970 to 1997, 16 states had court-order reforms, and 21 states initiated school finance reforms without court order (Evans et al., 1997).

Since the motivation for these school finance reforms was the demand of adequate and equitable educational resources of property poor school districts, the trend of school finance reforms was characterized by the increase of the involvement of state governments. State share of school spending in five states increased by more than 10% from 1971 to 1983 (California, Wyoming, Washington, Connecticut, and New Jersey) (Theobald & Picus, 1991). Evidence from the Common Core of Data (CCD) of the U.S. Department of Education's National Center for Education Statistics shows that six states increased the state share of school spending by more than 10% after school finance reforms in the 1990s (Kansas, Oregon, Wisconsin, Michigan, Vermont, and New Hampshire).

Another feature of school finance reforms is that they were interwoven with property tax system reforms of states. This is because part of school finance reforms is related to how to produce new tax revenue and how to share the tax burden. One way to improve inequality in educational spending is to limit the spending level in rich school districts (Figlio, 1998). *Proposition 13* in California mentioned above is this type of reform. Other examples are *Proposition 2 and ½* in Massachusetts that allowed each city and town in Massachusetts to raise property tax for local public services by no more than

2 and ½ percent every year (Dee & Levine, 2004) and *Measure 5* in Oregon that capped property tax rate for all purposes to a specific percentage of assessed values (Figlio, 1998). This kind of tax reform was also called “local property tax revolt”(Figlio, 1998). These reforms limited the ability of local school districts to produce educational revenue. Another type of tax reform that was related to school finance reform was the establishment of new tax or the increase of certain tax rate. For example, the school finance reforms in Vermont and New Hampshire involved the creation of a new statewide property tax. The school finance reform in Michigan in 1993 included the increase of income tax (Courant & Loeb, 1997).

School Finance in Vermont and New Hampshire

Vermont

In 1998, Vermont had 290 school districts including 252 town, city and incorporated school districts, 32 union school districts, four unified union school districts and two interstate school districts (Mathis, 1999). The enrollment in public elementary and secondary schools was 99,967 in 1998 while the national enrollment was 42,854,612 (Snyder & Hoffman, 2001). The expenditure per student was \$7,075 in 1998 while the national average was \$6,189 (Snyder & Hoffman, 2001). In almost all towns and municipalities, education budgets must be approved by voters at the annual town meeting based on a majority rule. The education budgets in 12 cities and large towns did not have to be voted as long as it was below the threshold specified in their special charters.

School finance system before 1997. Vermont’s school finance system went through three changes before the 1990s (Mathis, 1999). From 1964 to 1997, the state share of school spending fluctuated between 20% and 37% (Mathis, 1999). In 1969, the

state adopted a percentage equalizing formula based on the property wealth of school districts. In 1982, a new percentage equalizing formula based on property and income wealth measure was created because the state share of education spending decreased (Mathis, 1999). After that the state share deteriorated to 20%, and a foundation formula was adopted in 1987. As introduced above, in a foundation formula, the state aid is the difference of a foundation grant and a product of local property value and state-decided minimum property tax rate. Because of the poor economy of the early 1990s in Vermont, the state government increased minimum property tax rate. This decreased state aid by 7% from 1991 to 1995, and exacerbated the tax burden of local school districts (Mathis, 1998).

Because of the variation of property tax base among school districts, property poor school districts produced less revenue at higher property tax rate than property rich school districts at a lower property tax rate. For example, in December of 1994, the top 5% of school districts spent from \$5,812 to \$7,803 per student while the bottom 5% spent from \$2720 to \$3608 (*Brigham v. State*, 1997). At the same time, there was a huge disparity in property tax rates. For example, on an \$85,000 home, the tax in Sherburne School District, which was a rich school district, was \$247 while it was \$2,040 in Stannard School District (*Brigham v. State*, 1997). This created a strong dissatisfaction among poor school districts. In March of 1995, a group of students, taxpayers, and two school districts filed a lawsuit against the state of Vermont. Two students claimed that the Vermont school finance system deprived them of their right under the Vermont and federal constitutions to the same educational opportunities as students who reside in wealthier school districts (*Brigham v. State*, 1997). Several property owners from

property poor school districts claimed that the Vermont school finance system compelled them to contribute more than their just proportion of money to fund education (*Brigham v. State*, 1997). Two school districts claimed that the Vermont school finance system deprived them of the ability to raise sufficient money to provide their students with educational opportunities equal to those afforded students in wealthier school districts, and compelled them to impose disproportionate tax rates in violation of the United States and Vermont Constitutions (*Brigham v. State*, 1997).

The *Brigham* case was first filed at the Lamoille Superior Court. The trial court refused to rule that the school finance system was unconstitutional. They argued that education was not a fundamental right based on both the United States and Vermont Constitutions, and that inter-district funding disparities were rationally related to the legitimate state purpose of fostering local control over education funding and programs. Both the plaintiffs and the state appealed to the Vermont Supreme Courts. The Supreme Courts first confirmed that wide disparities in student expenditures existed among school districts and that these disparities were correlated with taxable property wealth within the districts. And then it maintained that students had a right to equal educational opportunity based on the Vermont Constitution. Therefore, it ruled that the Vermont school finance system was constitutionally deficient because it produced vast funding disparities among school districts.

Four months later, the Vermont state legislature passed the *Equal Educational Opportunity Act*. Although the Act was a response to the court order, it was not the only reason for the passage of the Act. In 1994, a property tax reform bill was passed in the Democrat-controlled House of Vermont, but failed in the Republican-controlled Senate.

The same thing occurred in 1996 again. After the 1996 election, Vermont Democrats controlled both the House and the Senate. Although it was a short period between the *Brigham* decision and the passage of *The Equal Educational Opportunity Act*, the approach to solve the problem of inequity in educational spending and tax burden was debated in the Vermont Legislature for a long time.

School finance reform in 1997. The new school finance system that Act 60 set up in Vermont had a four-year phase-in period so that property rich school districts whose tax would increase had time to adjust to the new school finance system (Mathis, 1998). In the first year Vermont still implemented the old foundation finance system with additional general and special education aids. In 1999, the new school finance system created by Act 60 was implemented.

The main part of the school finance system was a two-tier system which combined a foundation program and a power equalization program. The first tier was a foundation program, the state government provided a block grant of \$5,010 per student to every school district. The revenue for this was the pre-reform state general aid and the newly created statewide property tax. The statewide property tax was levied in 1999. Because of the variation of property tax base, property rich school districts produced more statewide property tax than property poor school districts. And all the statewide property tax was used to fund the first tier block grant. For rich school districts, this meant that they lost a portion of property tax revenue. For poor school districts, if the statewide property revenue was less than the block grant, this meant that they attained state aid.

For the second tier, the state government used a power equalization formula. The state government guaranteed the same revenue yield for school districts if they were willing to levy the same property tax rate above the statewide property tax rate. What was unique about the second part of the system was that it was funded only by the property tax revenue from school districts which decided to spend more than the block grant of the first tier without the input of state revenue. That meant that some school districts lost a portion of property revenue while other school districts attained some external revenue because property bases were different among school districts. 239 towns voted a budget above the block grant amount of \$5,010 per pupil in 1999 (Mathis, 1999). In 1999, the recaptured revenue was 88.1 million (Mathis, 1999).

There was a circuit breaker proposition for households with less than \$75,000 income. The property tax was limited at 2% of income for households with less than \$75,000 income (Mathis, 1999). This proposition was designed to protect households with lower income from the higher property tax burden.

School finance after 1997 reform. The recapture proposition faced strong opposition from the property rich school districts. They sued the state government and set up protest (Buzuvis, 2011). In February of 2004, Vermont passed a school finance reform bill, also called Act 68. Act 68 reformed the school finance policy in Act 60. It maintained the two-tier system adopted in Act 60 but eliminated the recapture proposition. Act 68 used an increase in state sale taxes to cover the funding that was recaptured from rich school districts before 2004 (Schmidt & Scott, 2006). Act 68 also divided property tax into residential and non-residential property (Picus, Odden, Glenn, Griffith, & Wolkoff, 2012). The tax rate for residential property was calculated based on the level of

budget that each school district chooses. The tax rate for non residential was fixed regardless of the level of spending of school districts. Since then, the school finance system in Vermont operates under Act 60 and Act 68 (Picus et al., 2012). In order to exclude the confounding effect of Act 68, this study focus on school finance system from 1995 to 2002 in Vermont.

New Hampshire

In New Hampshire, 234 cities and towns were organized into 177 school districts in 1999 (Michener, 1999). 132 were single town or city school districts, 31 were regional school districts which served two or more towns or cities. 14 small school district did not operate schools and sent children to nearby districts and paid a per pupil tuition fee. In 1999, there were 194,250 elementary and secondary students (Snyder & Hoffman, 2002). Average expenditure per pupil was \$6,433 while the national average was \$6,508 in 1999 (Snyder & Hoffman, 2002). Each year, school boards presented a school budget in a town meeting that had the authority to approve, decrease or increase the budget. This process was followed by all but 10 city school districts. Budgets in nine city school districts were approved by the city council. One city had a fiscally independent school board which had the authority to adopt a final school budget.

School finance system before 1999. Before the school finance reform in 1999, no state in the United States relied on more local revenue to support public schools than New Hampshire. Overall, local revenue accounted for about 90% of total school spending, 7% of spending was from the state government, the remaining 3% was from the federal government (Michener, 1999). State aid aimed to cover 8% of total cost of public schools.

It was a foundation program which distributed the state aid based on the wealth level of school districts (Michener, 1999).

Heavy reliance on local revenue created huge inequality in the property tax rate among school districts. In 1991, five property poor school districts, including the town of Claremont, brought a lawsuit to the Merrimack Superior Court and demanded for a declaratory judgment. They argued that the school finance system in New Hampshire violated the New Hampshire Constitution for the following reason. First, the state failed to spread educational opportunities equitably among its students and adequately fund education. Second, the foundation aid statutes, which restrained state aid to public education by capping state assistance at 8%, was unconstitutional. Third, the heavy reliance on property taxes to finance New Hampshire public schools resulted in an unreasonable, disproportionate, and burdensome tax. The Superior Courts ruled that the New Hampshire Constitution did not impose a duty on the state to support the public schools and dismissed the plaintiffs' demand. The plaintiffs appealed to the New Hampshire Supreme Court.

In 1993, the New Hampshire Supreme Courts decided that the state had a duty to provide a constitutionally adequate public education and to guarantee adequate funding, and remanded the case to the Merrimack Superior Court. The Merrimack Superior Court argued that the school finance system in New Hampshire guaranteed adequate education to students in plaintiffs' school districts, and dismissed plaintiffs' demand again. The plaintiffs appealed to the New Hampshire Supreme Courts again. In 1997, the New Hampshire Supreme Court ruled that the existing educational finance system was unconstitutional because it created inequality of tax burden. For example, the equalized

tax rate for the 1994-1995 school year in Pittsfield School District was \$25.26 per thousand of property value while the rate in Moultonborough School District was \$5.56 per thousand of property value (*Claremont v. Governor*, 1997). The courts argued that the property tax was levied to fulfill the responsibility of state government to fund public education. Therefore, property tax for education should be a state tax. According to the New Hampshire Constitution, state tax should be uniform. However, the school finance system produced huge difference in property tax rate among school districts. The Supreme Court gave the governor and state legislature one year to design a school finance system to fulfill their responsibility.

At the beginning, a constitutional amendment was proposed to repeal the decision, but it fell far short of support for its passage (Lutz, 2010). Dozens of approaches to reform the school finance system in New Hampshire were proposed (Olabisi, 2006). In 1999, the New Hampshire state legislature passed the House Bill 999.

School finance reform in 1999. The new school finance reform laid out by the House Bill 999 was a foundation program. First, the state government established a definition of an adequate education cost. The adequate education cost was calculated by the following method (Olabisi, 2006). Schools in which 40 to 60 percent of third and sixth grade students passed statewide-standardized tests were regarded as schools providing adequate education. Dividing these schools' cost by their number of students was identified as the statewide base cost of adequate education.

In order to guarantee that each student received funding at the amount of the adequate cost, New Hampshire established a statewide property tax at \$ 6.60 per \$1,000 of property value which produced about half of state revenue for education (Michener,

1999). Although the statewide property tax was a state tax, it was collected and retained by school districts. However, school districts needed to give the excess property revenue to state government if their statewide property tax revenue was more than the adequate education cost. In the first year of the new school finance system, 20% of school districts, which accounted for 9% of the state population, paid the excess property tax revenue to the state (Lutz, 2010). The State government would make up the difference if the statewide property tax revenue in property for poor school districts was less than the adequate education grant. The revenue for state aid came from two sources. One was the former state grant that used to aid school districts. In 2000, this was 131 million. The other was the recaptured statewide property tax revenue and increase in business, real estate, and car rental tax, and other sources. This was 276 million. Regarding the statewide property tax, one feature that should be emphasized is that the locally retained portion of property tax revenue was collected and held by school districts. Thus, it was essentially a relabeled property tax (Lutz, 2010). However, it became a state tax instead of local tax.

In the new school finance system, school districts were allowed to spending more than the adequate education grant by levying additional local property tax. Since all school districts spent more than the adequate grants (Hall, 2003), that means that tax price for all school districts was still one after the school finance reform. Tax price for all school districts before the school finance reform was also one in New Hampshire. Therefore, the school finance reform did not change tax price for school districts. In other words, tax price is a marginal price instead of average price. It means that how much do voters pay in order to spend one dollar more on education. Although recapture

proposition led property rich school districts to lose a portion of property tax revenue, this only change average prices instead of marginal price.

Change after 1999 reform. In 2001, the statewide property tax rate was reduced to \$5.80 per \$1,000 of property value. In June of 2003, the New Hampshire legislature adopted HB 608 which reduced the statewide property tax rate from \$5.80 to \$4.92 for fiscal year 2004 and further down to \$3.24 for FY 2005. Since 2005, the tax revenue from statewide property tax would be fixed at \$363 million. Tax rate was calculated by dividing the revenue by total property value. In 2005, HB 616 was passed that allowed all towns to keep the excess statewide property tax revenue except two towns on the condition that the revenue was spent on education. The bill also eliminated the definition of adequate education cost. It distributed state aid based on the property value per pupil in school districts. In 2006, the new school finance system was ruled as unconstitutional by the New Hampshire Supreme Court. However, this study only focuses on school finance system from 1996 to 2003 in New Hampshire in order to exclude the influence of the HB 616.

Literature on School Finance Reforms

Studies on the Effect of School Finance Reforms

Effects estimated on nationwide data. Decreasing inequality in school spending is one of main goals of school finance reforms. Therefore, one criterion to evaluate effect of school finance reforms, which occurred since the 1970s, is to see how they affect inequity in school spending. Murray, Evans, and Schwab (1998) used school district level data on per student expenditure of over 10,000 districts from 46 states. They found that court-ordered finance reforms reduced within-state inequality in spending by 19 to 34%.

This was achieved by raising spending in the poorest districts while leaving spending in the richest districts unchanged. They also found that finance reforms led states to increase spending for education and leave spending in other areas, such as health care and highways, unchanged, indicating that states funded the additional spending on education through higher taxes. Springer et al. (2009) used a similar model as Murray et al., but used two new panel data, one from 1972 to 2002 every 5 years, and one from 1990 to 2000 in each year. Their results conformed to Murray et al.'s conclusion that the court decision of unconstitutionality of school funding system decreased the level of inequality between school districts at a similar magnitude as the estimation of Murray et al.

Another criterion to evaluate effect of school finance reforms is to see whether they decrease the correlation of school spending and local wealth level. The increase of state aid to poor schools is a way to achieve that. Card and Payne (2002) studied the effect of school finance reforms on the correlation coefficient between revenue from state and median family income of local communities. They estimated that the gap in state aid between a poor district and a rich district widened by about \$300 per student in states where the financing system was found unconstitutional than in other states. This indicated that states gave more aid to poor school districts than rich school districts. Using school district spending data, they also found that a one-dollar increase in state aid increased district education spending by 50-65 cents because school districts may reduce local taxes in response to an increase in the revenue from states.

One measure for states to achieve equality of school funding is to limit local spending of school districts with high property tax base by setting a local revenue cap or the requirement of supermajority when passing a request of increase in local property tax.

In 2004, there were 14 states that had statutory provisions that required a supermajority (SMRs) to adopt new taxes. Of the 14 supermajority states, 12 states also had tax and expenditure limitation (TEs) (Jordan, Jordan, & Crawford, 2005). Jordan et al. found that states with SMRs had lower rate of increase in average per-pupil expenditure (APPE), and that the greatest increases in APPE, average teacher salary, and percentage of funds from state sources were in the states with neither TEs nor SMRs. Positive changes in funding occurred less in states with constraints than in states without taxation or expenditure constraints. Blankenau and Skidmore (2004) studied the effect of court-ordered reform on educational spending in the presence or absence of TEs. They found that court-ordered reform had a negative effect on school funding from local source only in the presence of TEs. TEs decreased spending from local source in the absence of court-ordered reform, but the effect was less pronounced than when TEs and reforms were both present.

Manwaring and Sheffrin (1997) tried to identify the mechanisms by which school finance litigations and reforms affect educational spending. They identified four mechanisms that could influence the effect of finance litigations and reforms. They found that the increase in prominence of public school finance issues, as a result of school finance litigations or reforms, had a positive effect on educational spending. The increase of state share of educational spending and income per capital of state had a negative effect on the impact of school finance litigations and reforms on educational spending. The stability of state budget had a positive influence on educational spending after litigations or reforms. At the national level, school finance litigations and reforms had a positive effect on educational spending considering all these four factors. For individual

states, they found that school finance reforms had a negative effect in five states and a positive effect in sixteen states. Downes and Shah (2006) further found that the variation of the effect of school finance litigations or reforms depended on the stringency of constraints on local discretion, the characteristics of a state's population and of that state's schools.

These studies using national sampling and time-series data are problematic. First, the specification of education litigations or reforms in their analytical model is problematic. All the studies that I reviewed above used either the same or a modified method of operationalizing the variable of school finance litigations or reforms as that adopted by Murray et al. (1998). Murray et al. invented a dummy variable for finance reforms for each state. A state was assigned to be one in all years after its finance system was ruled unconstitutional, 0 for all year before the ruling. All states with no finance litigation and no ruling in favor of plaintiffs were coded as 0. This specification is problematic for the following reason.

The court ruling of unconstitutionality of state finance systems does not necessarily equate to finance reforms. Some state legislatures did not, or did not fully, or did not immediately follow state court rulings. Only five states explicitly changed the structure of their financing plans among the 12 states in which the school finance system were ruled as unconstitutional between 1976 and 1991 (Card & Payne, 2002). For example, the Ohio education finance system was ruled unconstitutional four times in 1997, 2000, 2001, and 2002. Its school finance reform was passed in 2009. Based on the analysis of finance litigations and reforms of 20 states with a court ruling of unconstitutionality, I found that Texas and New Hampshire initiated finance reforms six

years after court orders. New York and Arizona took four years. Wyoming took three years. North Carolina, New Jersey, Idaho, Connecticut, and California took two years. In Washington and Tennessee, state legislatures passed education finance reforms before the rulings of their supreme courts were made. At the same time, some states without court orders also initiated education finance reforms. Among the 21 states with no court rulings between 1970 and 1992, 12 changed the structure of their financing system(Card & Payne, 2002). For example, Michigan initiated a comprehensive and radical school finance reform without pressure from courts. This means that the operationalization of the reform variable as a dummy variable did not measure what researchers in these studies wanted to measure. Further, treating school finance reform as a dummy variable in these studies including many states is not appropriate because school finance reforms are more variable than these researchers thought. Hoxby (2001) clearly showed that the measures that court-ordered states took were various and even contradictory by characterizing these reform with four different variables. However, it is appropriate to treat school finance reforms as a dummy variable if researchers are able to show that school finance reforms in different states are comparable.

Since equating successful school finance litigations for plaintiffs with school finance reforms and treating school finance reform as homogenous are problematic, I think that the studies on the effect of school finance litigations or reforms of individual states is more appropriate and meaningful than the studies using national data.

Effects estimated on the effect of school finance reform in one state. There has been a group of studies that focused on school finance reforms in individual states. For instance, In California, after the *Serrano v. Priest* (1971), the case went into trial court.

The state legislature enacted a school finance reforms in 1974 which was claimed unconstitutional by the trial court and confirmed by the California Supreme Court in the *Serrano v. Priest* (1976) decision. The state legislature initiated another school finance reform in response to the decision. Also, in 1978, California passed *Proposition 13* which reduced local property tax rate to one percent and forbade any statewide property taxes (Fischel, 1989). The state share of total education revenue increased from 37% in 1978 to 60% in 1979 (Downes, 1992). Downes (1992) found that the school finance reform in California after the *Serrano v. Priest* (1976) decreased the dependence of education expenditure on local wealth by comparing the correlation between total expenditure per pupil and assessed valuation before and after reform. However, total funding of public education fell between 10 and 15% compared to the rest of the United States after the reform because it limited the supplementary funding of wealthier school districts (Fernandez & Rogerson, 1999).

School finance reform in Michigan also earned much attention from researchers. Without the pressure of court-ordered reform, Michigan took a revolutionary school finance reform called *Proposal A* in 1994. *Proposal A* replaced the former local property tax system with a centralized state system in Michigan. By using a Difference in Difference model Chaudhary (2009) found that *Proposal A* increased operating expenditures of school districts. He also found that the increase in expenditures led to higher teacher salaries and smaller class sizes in Michigan. Comparing per student spending pre-reform and post-reform, Courant and Loeb (1997) found that the beneficiary of *Proposal A* was small and rural districts because per student spending in these districts significantly increased. Zimmer and Jones (2005) found that Michigan's

high-spending school districts had a greater probability of issuing bonds after implementation of *Proposal A* which restricted operational expenditure from local source. This indicated that debt financing of capital expenditures might have become a mechanism to allow these school districts to circumvent the policy's intent for greater spending equity.

In Massachusetts, after the decision of the *McDuffy v. Secretary of the Executive Office of Education* (1993), in which plaintiffs prevailed, the state passed the *Massachusetts Education Reform Act* in 1993. Dee and Levine (2004) studied the school finance reform in Massachusetts and found that the reform increased state aid to school districts. Total educational revenue increased although local revenue for education decreased. This indicated the existence of flypaper effect. They also found that poor school districts spent more increased state aid on education than rich school districts that used more state aid to relieve their tax burden. Dee and Levine (2004) used a DID design and used Connecticut and Maine as control states of the treatment state Massachusetts.

In sum, the existing empirical studies reviewed above indicate that the effect of school finance reforms varies from state to state. There is no consistent evidence on the effect of school finance reforms on per pupil spending. This emphasizes the importance of the idiosyncratic features of each state and complicated characteristics of approaches to school finance reforms that states took when evaluating school finance reforms. School finance reforms in Vermont and New Hampshire presents several new measures such as the establishment of statewide property tax and the recapture proposition. Also, school budgets are decided in the form of direct democracy. These new features in school

finance reform and the specialty of budget decision procedure might produce different effect from the one that occurred in California, Michigan, and Massachusetts.

Studies on Different School Finance Systems

School finance reforms usually involved a change in type of school finance programs. Prototypical school finance systems include local school finance system, centralized state school finance system, foundation finance system, and power equalizations system. As describe above, there are variations for foundation finance program and power equalization program based on the different specification of these programs. Foundation finance systems include ones with limited local supplementation and ones with unlimited local supplementation. Power equalizations include ones with recapture and ones with recapture. One group of studies in school finance is to evaluate what school finance system is optimal in term of the level of per pupil spending and the equity in per pupil spending. This group of studies is helpful to this study because the school finance reform in Vermont and New Hampshire involved change in the type of school finance program. The literature on optimal school finance programs tried to characterize several ideal types of school finance reform, and evaluate them. In the following part, I introduce several comparison studies of this type (Fernandez & Rogerson, 2003; Hoxby, 2001; Loeb, 2001; Manwaring & Sheffrin, 1997).

Hoxby (1996) argued that foundation and power equalization school finance system were inferior to a mandatory spending level combined with categorical aid because both finance systems punished school districts that have high property bases. Categorical aid is the aid which state governments distribute to local school districts based on the characteristics of students such as students in special education and students

with limited English proficiency. To a certain extent, high property valuation was the result of the capitalization of effective schooling and high demand of education (Hoxby, 1996). This means that school finance equalization systems could discourage the effectiveness and high demand for education. On the contrary, categorical aid can solve the limits of ability to produce tax revenue of poor school districts, and do not discourage effective school districts and parents' high demand on education.

School finance reforms are characterized by an increase in the role of state governments in funding education. Loeb (2001) evaluated three types of school finance centralization systems by using economic simulation methods based on school finance data of Michigan. She found that the system of state grant plus unlimited local supplementation provided little cross-district equalization effect. A pure state funding system discouraged higher spending of rich school districts although it provided higher equalization effect. She argued that a system of state grant with capped local supplementation may be a good school finance system which could produce higher state grant than a pure state funding system and have more equalization effect than a system of state grant plus unlimited supplementation. These findings showed that it was how state centralization reform was implemented rather than the increase of state's role that influenced the spending of school districts.

Fernandez and Rogerson (2003) used a model calibration method to evaluate five different prototypical school finance systems: local system, state system, foundation, power equalizing with recapture, and power equalizing without recapture. In terms of total resources that each school finance system produced, they found that foundation system and power equalizing without recapture stimulated more spending on education

than other systems although this was achieved at the expense of equity. In terms of spending inequity, pure state school finance system was on the top to decrease inequity in educational spending. Based on the overall expected utilities, they argued that power equalizing with recapture ranked highest because it brought more resources to education than state finance systems and also decreased inequity.

The studies from Fernandez and Rogerson (2003) and Loeb (2001) revealed that the type of school finance systems could influence the level and distribution of school spending among school districts. However, as both studies were built on some simple assumptions and arbitrary choice of model parameters, their results should be interpreted with caution. Also, they did not reveal the mechanism of how various school finance systems function.

Hoxby (2001) took a more realistic approach to evaluate different school finance systems. She characterized all school finance systems with four variables and evaluated the effect of these features on school spending. These four variables were foundation tax rate, school-related income rate, foundation grant, and tax prices. Foundation tax rate was the property tax rate to produce state funding that was used on education. School-related income tax rate was the calculated income tax rate that was needed to produce state education funding minus revenue from foundation tax. Foundation grant referred to the amount of grant that school districts received from states. She found that foundation tax rate and tax price had negative effects on per pupil spending. The amount of state grant had a positive effect on per pupil spending. School-related income tax rate had no significant effect on per pupil spending.

Theoretical Framework

Since education budgets were decided by local school districts in Vermont and New Hampshire, to evaluate the impact of school finance reforms in both states essentially entails examining how local school districts determined its budget under the new school finance systems. The theoretical literature on determinants of local government spending and the literature on the effect of intergovernmental grants are relevant to this study. In the following section I review the two areas and explain how they could be used in this study. And then I use the concepts and conclusion in the literature to analyze the effect of the school finance reform in Vermont and New Hampshire on per pupil spending.

Theories on Local Public Spending

The determinants of local public spending. School spending has attained much attention in public finance literature as an important public service of local government (Bergstrom, Rubinfeld, & Shapiro, 1982; Romer, Rosenthal, & Munley, 1992; Romer & Rosenthal, 1982). Researchers have tried to find out what determines the level of local public spending such as school spending. They derived these determinants from the economic theory on consumer behaviors in private consumption (Bergstrom & Goodman, 1973). Since price and income are important factors determining the consumption of private good and service of individuals, they assume that the distribution of price and income among local residents are important determinant of local public spending which could be understood as the consumption of public service (Bergstrom & Goodman, 1973).

In order to understand how the distribution of price and income among local residents determine the level of public spending in local governments such as school

districts, median vote theory should be introduced. Median voter theory originated in the field of public choice (D. Black, 1948; Downs, 1957). It tries to explain and predict the result of public choice. It argues that public choice represents and adopts the preference of a median voter if the choice is picked by a simple majority rule and the median voter has a single-peaked preference point. This theory applies to a variety of fields including elections and public policies. Local government expenditure is one of these. In considering both median voter theory and the implication from the determinants of individual consumption of private good and service, it could be concluded that the tax price and income of median voter could be determinants of local public spending. In the area of local education spending, it means that the tax price and income that a median voter has would determine the level of education spending. Empirical studies using this theory proved that median voter income and tax price in a school district were significantly correlated with school spending (Denzau & Grier, 1984; Hoxby, 2001).

However, Romer and Rosenthal (1979) argued that empirical studies did not provide strong evidence for the median voter theory because they failed to show the superiority of the model specification using median income and tax share over alternatives using the measurement of a portion of median income and tax share. Considering this failure, Romer and Rosenthal (1982) provided another theory about local school spending. They thought that political structure might play an important role in deciding the level of local spending. Regardless of whether the budget plan is voted by a referendum or a representative body, voters are often offered one proposal. In the process, administrators can take advantage of the process and offer more than the median voter's desired level of school spending.

Romer and Rosenthal (1982) empirically showed that local school district boards made use of the reversion proposition in Oregon school finance system in the process of local school budget decision. The reversion proposition regulated that school districts would spend base budget, which was much lower than current budget, if voters failed to pass the proposed budget. School district boards could place the threat of lower spending on voters by offering more than the median voter desired budget level. They proved that the reversion proposition in Oregon significantly increased local school spending level by 15%. The result from Romer and Rosenthal (1982) is meaningful to this study because it highlights the importance of the procedure of budget decision when evaluating the effect of school finance reforms on school spending.

The most debated point in the determinants of local public spending is how to identify the median voter. The way of treating a voter with median income as median voter is questioned (Romer & Rosenthal, 1982). However, this problem did not influence its application to the evaluation of the effect of the school finance reform in Vermont because the school finance reform changed the tax price that different school districts, instead of only a median voter, had to bear. In order to understand this, I explain how the school finance reform in Vermont changed tax price in the following paragraph.

There are two important features about the power equalization part of the school finance reform in Vermont. First, all funding for this part came from property tax revenue that school districts decide to spend more than the foundation grant on education. Second, it guaranteed that equal property tax rate produced the same revenue. Thus, if school districts with lower property base set up the same property tax rate as school districts with higher property base, they would get the same increase in educational spending.

This means that the school district with lower property tax rate would get a portion of property revenue from the school district with higher property base since the source of funding of this part is only from statewide property tax revenue. Therefore, with the same increase in education spending, the tax price for school district with lower property tax base was less than one because it got a portion of revenue from school districts with higher property base, while the tax price for school district with higher property base was more than one because it lost a portion of revenue to school districts with lower property base.

Since the transfer of property revenue occurred between school districts instead of individuals, all voters including median voter in one school district experienced the change in tax price. Thus it is not necessary in this study to pinpoint what tax price a median voter faced in school districts in order to predict the effect of the school finance reform on per pupil spending. I will show how I utilize the theory in the hypothesis section.

Another often-discussed issue about local public spending is the Tiebout model. This model assumes that voters choose a resident location where public spending and tax rate meet their preference under several assumptions (Tiebout, 1956). These assumptions include voter's freedom to move, full knowledge of difference in revenue and expenditure patterns among communities, and a large number of communities that could be chosen (Tiebout, 1956). The result of the Tiebout process is that the preference for public spending of local residents in one community becomes so homogeneous that residents with the same preference live in the same communities. Several studies showed the existence of the Tiebout process (Aaronson, 1999; Hoxby, 2000; Urquiola, 2005). For

example, Urquiola (2005) presented that children's district-and-school level peer groups become more homogenous in terms of race and parental education when the availability of districts within a metropolitan area increased. Since school finance reforms change the revenue-expenditure pattern, it could change the population homogeneity within school districts (Aaronson, 1999).

The Tiebout process is relevant to this study because the change in the way that state governments distribute the state aid could change the property value. Hoxby (2001) argued that the part of property value was positively related to the demand on education of local residents. Residents with lower property value have lower demand on education. For instance, Black (1999) showed that parents were willing to pay 2.5% more on houses for a 5% percent increase in test spending. If state governments distribute state aid based on property value, and give larger portion of state aid to lower property school districts, that means that the state aid is given to those who have lower demand on education. Thus, school finance reforms based on the property value might decrease average educational spending per pupil (Hoxby, 2001).

The effect of intergovernmental grant. Intergovernmental grant includes two categories. One is called lump-sum grant. Recipient governments spend it on certain targeted public service such as education without requirement of contribution of local contribution. The other one is matching grant. Recipient governments need to contribute a certain percentage of increase in spending on targeted areas in order to receive the grant. In this study, I only use the conclusion of studies on lump-sum grant. Thus, I only review literature on the effect of lump-sum grant. Regarding the effect of lump-sum intergovernmental grant on the spending behavior of recipient government, Bradford and

Oates (1971) argued that the use of the grant applies to the economic theory of behaviors of individuals even though the grant is given to a group of individuals and the spending of it is a collective decision. This is true if the structure of political decision-making institutions reflects the preference of voters. This assumption indicates that the effect of lump-sum intergovernmental grant on public spending is equal to the effect of private income increase of local residents in recipient jurisdiction. Bradford and Oates (1971) stipulated several conditions on which this assumption holds. These conditions include singled value preference, majority rule, and competitive choice (Bradford & Oates, 1971). A singled value preference means that every resident has a single preference of spending level. The level of public spending should be voted in a majority rule. Competitive choice indicates that voters could offer different level of spending to be selected in the voting process. This means that budget proposal could be changed by voters in the process of voting. For the remainder of the study, I will call this theory as “equivalence theory”.

The equivalence theory is contradictory with empirical results on the flypaper effect of lump-sum state grant on public school. The flypaper effect refers to the fact that recipient governments would spend more intergovernmental grant on public services than they spend their equivalent income increase (Hines & Thalar, 1995). There is no consensus on the reason for the occurrence of flypaper effect. Because of numerous studies showing the existence of flypaper effects, the studies on it mainly focused on why it occurs (Hines & Thalar, 1995; Wyckoff, 1991). Wyckoff (1991) summarized four often-debated reasons: econometric misspecification, omitted variables, voters’ misperception of average price and marginal price, and voters’ unawareness of the existence of intergovernmental grant. The econometric misspecification refers to the error

that certain researchers treated a mix of lump-sum grant and matching grant as lump-sum grant. Because many studies used multiple regression analysis, omitted variables could produce the bias on the estimation of the effect of lump sum grant. Also lump sum grant reduced average price of public service, not marginal price, if voters thought marginal price was changed, they tended to spend more (Courant, Gramlich, & Rubinfeld, 1978). Last, if voters were unaware of the existence of intergovernmental grant, bureaucrats could spend this more on public service (Gramlich & Rubinfeld, 1982). Wyckoff (1991) used data on state aid to school districts in Michigan and tested the four explanations and found that they could not explain away the flypaper effect. Hines, James R. and Thaler (1995) maintained that voters might not see intergovernmental grant equivalent to private income because of loss aversion.

However, there are several recent studies indicating that flypaper effect might not exist (Duggan, 2000; Gordon, 2004; Lutz, 2010). Gordon (2004) showed that the increase in Title I grant did not increase total revenue for education after three years by using an instrumental variable which solved the endogeneity of the variation in Title I grant. Lutz (2010) showed that state grant increase as a result of school finance reform in New Hampshire did not disproportionately increase local revenue on education because the budget decision was made by direct democracy in New Hampshire and voters were very familiar with school finance reform.

School finance reforms in Vermont and New Hampshire both involved the change of intergovernmental grant to different school districts. This study holds that school districts in Vermont and New Hampshire spent the same amount out of intergovernmental grant as they spent out of residents' income increase as the

equivalence theory predicts. This means that the effect of school finance reform has a small positive effect on per pupil spending in school districts which receive more state aid than before the reform. The reason for this is that the school finance governance and finance system meet the assumptions that the equivalence theory requires (Bradford & Oates, 1971).

First, the school finance reforms in both Vermont and New Hampshire were fiercely debated in the court and legislatures. For example, public polls showed property tax reform as the number one issue on peoples' minds before the school finance reform in Vermont (Mathis, 1998). The reform in New Hampshire was also highly politicized (Lutz, 2010). It is reasonable to assume that voters were aware of the existence of intergovernmental grant. Second, the budget decision in Vermont and New Hampshire were decided by residents in school districts, and they could accept or amend the budget as they wanted (Lutz, 2010; Mathis, 1999). Third, there was no limitation on the level of educational spending. These features ensured that the school finance budget reflected the preference of local residents. Therefore, in the following section analyzing the effect of school finance reform, I assume that school districts spend the same portion of state grants on education as they spend out of their income increase rather than all the intergovernmental grants. Since it is predicted that 5 to 10% of state grant will be spent on education (Bradford & Oates, 1971; Hines & Thalar, 1995), that means that that the increase of intergovernmental state aid has a small positive effect on educational spending.

Hypotheses

Vermont. Post-reform school finance system in Vermont was a combination of a foundation program and a power equalization program. For the foundation part, it produced a change in state aid to school districts. School districts, whose statewide property tax revenue was higher than the first-tier block grant, lost a portion of property tax revenue. For school districts whose statewide property tax revenue was lower than the first tier block grant, they could attain state aid to make up the balance. It was expected that the state grant for these districts was more than the grant that they received before the reform because of the input of statewide property tax. Therefore, based on the equivalence theory (Bradford & Oates, 1971), per pupil spending would increase by 5 to 10% in property poor school districts, and per pupil spending would decrease a little in property rich school districts because these school districts lost a portion of property tax revenue.

The power equalization part of the new system changed the tax price that voters bear. Before the reform, the school finance system in Vermont was a foundation system. Almost every school district spent more than the state foundation level. When school districts wanted to spend one more dollar on education they needed to levy one dollar of local tax revenue. Therefore, tax price for local residents was one. However, the new system made tax price vary from district to district. Tax price was less than one if the second tier property revenue in school districts was less than the state guaranteed level because these school districts got revenue recaptured from other school districts. Tax price was more than one for school districts in which the second tier property revenue was more than the state guaranteed level because the extra property tax revenue was

recaptured. Since tax price is negatively related to school spending (Bergstrom & Goodman, 1973; Chandler, 2005; Denzau & Grier, 1984; Hoxby, 2001), school finance reform in Vermont would increase per pupil spending for poor school districts, and decrease per pupil spending for rich school districts depending on their tax price change, other things equal.

In sum, as Table 2 shows, considering the effects of both foundation part and power equalization part of the new school finance system in Vermont, the school finance reform should have a negative effect on per pupil spending in property rich school districts because of the loss of property tax revenue and the increase of tax price. On the contrary, it should have a positive effect for property poor school districts because of the increase of state aid and the decrease in tax price. Finally, because of the contradictory effects of the school finance reform in Vermont for property rich and poor school districts, it is difficult to theoretically predict the overall effect of school finance reform on per pupil spending. This is an empirical question that depends on which effect surpasses the other effect of opposite direction.

Table 1: Hypotheses on the Effect of School Finance Reforms in Vermont and New Hampshire

State	School Districts	Tax Price		Intergovernmental Aid		Hypotheses
		Pre-reform	Post-reform	Pre-reform	Post-reform	
Vermont	Poor	1	<1		↑	+
	Rich	1	>1		↓	-
New Hampshire	Poor	1	1		↑	+
	Rich	1	1		↓	-

New Hampshire. The school finance reform in New Hampshire did not change tax price after the reform because school districts were allowed to levy extra revenue

above adequate education cost without constraint. Since all school districts spent more than the adequate grants (Hall, 2003), that means that tax price for all school districts was still one after the school finance reform. But it changed the percentage of state funding of total educational spending from 9% to 55% in New Hampshire. About half of the increase in state share of educational revenue came from the newly established statewide property tax (Olabisi, 2006). The revenue of the other half was from pre-existing education aid, and increase in business, real estate, and car rental tax, and other sources (Olabisi, 2006).

The recaptured proposition in the new school finance system of New Hampshire increased state aid for property poor school districts and decreased property revenue of rich school districts. Since school budget decision was made through a procedure of direct vote of local residents in town meeting. I assume that the equivalence theory holds in New Hampshire. Thus I hypothesize that the school finance reform in New Hampshire had small a positive effect on per pupil spending in poor school districts. On the contrary, I hypothesize that the school finance reform had a negative effect on per pupil spending in rich school districts because of the loss of local property revenue. Similarly to the school finance reform in Vermont, it is hard to predict the overall effect of school finance reform on per pupil spending in all school districts. It depends on the balance of the negative and positive effect of the school finance reform.

Summary

This chapter first introduces the school finance system in the United States. This includes the funding source of public education, typical school finance formula, and the challenge on the school finance system, and corresponding school finance reforms. The

first part of this chapter provides a big picture to understand the school finance reforms in Vermont and New Hampshire which this study specifically focuses on. Then school finance systems in Vermont and New Hampshire are presented. This includes the history of school finance system in both states, the motive and content of the school finance reforms, and the reasons why they are worthwhile being studied. After that, the chapter presents empirical studies on the effect of school finance reforms. In the final part of theoretical framework, I review the theories on the determinant of local budgets and the effect of intergovernmental grants, and then provide the hypotheses on the effect of the school finance reforms in Vermont and New Hampshire based on these theories.

CHAPTER 3 METHOD

Introduction

The purpose of this study was to evaluate the impact of the school finance reforms in Vermont and New Hampshire on per pupil spending. Specifically, the research questions are:

1. Did the school finance reforms in Vermont and New Hampshire change per pupil spending for all school districts?
2. Did the school finance reforms in Vermont and New Hampshire change per pupil spending for poor school districts?
3. Did the school finance reforms in Vermont and New Hampshire change per pupil spending for rich school districts?

In order to answer these questions, this study utilized a quantitative research design by using school district expenditure data derived from the Common Core of Data (CCD) of the National Center for Education Statistics (NCES). The reason for the choice of a quantitative method instead of a qualitative method is that the aim of the study is to evaluate the effect of the school finance reforms in Vermont and New Hampshire on per pupil spending. The increase of per pupil spending was not only the aim of plaintiffs in the *Brigham v. State* (1997) and *Claremont v. Governor* (1997), but also the aim of the school finance reforms in Vermont and New Hampshire in a response to the two school finance litigations. Thus, a quantitative method could provide a comprehensive approach to evaluate the effect of the school finance reform on per pupil spending in all school districts in Vermont and New Hampshire. Further, a quantitative design provides more

generality of the conclusions that this study provides and stronger implications for predicting the effect of school finance reform in other similar states.

Estimation Strategy

Ideal Strategy

The naive method to evaluate the effect of the school finance reforms in Vermont and New Hampshire on per pupil spending is to compare pre-reform and post-reform per pupil spending of school districts. However, this design could suffer from omitted variable bias because it is possible that the variation in outcome variable might be mistakenly attributed to school finance reforms but is actually due to other unknown factors (Meyer, 1995). For example, declining enrollment or change in per capita income in the period might cause change in per pupil spending. Another method to evaluate the effect of the school finance reforms in Vermont and New Hampshire on per pupil spending is to compare per pupil spending in Vermont and New Hampshire and per pupil spending in some states which did not have school finance reforms after the school finance reform. However, this simple comparison could suffer from the bias of sample selection (Angrist & Pischke, 2008). For example, per pupil spending in the comparison state was probably higher than in Vermont and New Hampshire before school finance reforms.

The ideal method to solve the omitted variable bias and selection bias is to use an experimental design (Angrist & Pischke, 2008; Heckman, 2000). In this design, a group of school districts would be randomly selected from all school districts to participate in a new school finance system. After the school finance reform takes effect, the difference in per pupil spending before and after reform in these school districts is compared to the

difference in per pupil spending before and after reform in non-participant school districts (Angrist & Pischke, 2009). The result is the estimation of the effect of the school finance reform. The random choice of participants could make sure that the estimation of the impact of school finance reform is free of selection bias and omitted variable bias. This is because random choice of participant school districts assures that nonparticipant group and participant group are comparable (Angrist & Pischke, 2008). However, in the field of school finance reforms, new school finance reforms usually apply to all school districts in one whole state. The situation in which a school finance policy randomly applies to a sample of school district is not available to researchers. For example, in the school finance reforms of Vermont and New Hampshire, this was not the case. All school districts in Vermont and New Hampshire were influenced by the school finance reforms.

Difference in Difference

A Difference in Difference (DID) design is a good way to approximate the experiment approach to evaluate the impact of school finance reforms. A DID design needs a treatment group and a comparison group with data before and after treatment. The difference in the outcome variable of pre-treatment and post-treatment for participants in the treatment group is subtracted by the same difference for non-participants in the comparison group (Angrist & Pischke, 2008; Ashenfelter & Card, 1984; Card & Krueger, 1993). The final result is the estimation of the effect of the treatment if the choice of treatment and comparison group meet several assumptions. This method largely rules out bias from factors that occur during the targeted period and influence outcome variable in both the treatment and comparison group. The requirement for DID design is to make sure that treatment and comparison group are comparable.

That is, the characteristics that influence the outcome variable of interest are balanced between treatment group and comparison group. Therefore, it is critical to select a good comparison group in order to use a DID design (Atthey & Imbens, 2006; Meyer, 1995).

In order to utilize the DID design, three assumptions are needed. First, there is a well-defined and exogenous treatment that occurs (Besley & Case, 2000). Second, there is a comparison group with no treatment. Third, the trend of outcome variable for participants in treatment group would have the same trajectory as those in comparison group if they did not experience the treatment (Heckman & Vytlačil, 2005). That means that the assignment of treatment is independent on the characteristic of participants. What is needed to meet this assumption is that the characteristics of participants in treatment and comparison group are balanced. In the following sections, I show how the research design of this study meets these assumptions.

Assumption of exogenous treatment. The school finance reforms in Vermont and New Hampshire meet the first assumption because they dramatically changed the state share of educational spending and tax price that school districts bore. For example, the Vermont school finance reform increased the state share of public school funding from 28% to 69% in the first year. The school finance reform in New Hampshire pushed the state share of school spending from 9% up to 55% in the first year. Further, the school finance reforms in both Vermont and New Hampshire were a response to their state supreme court decisions. These court-ordered school finance reforms were regarded as exogenous to school finance systems in the school finance literature (Baicker & Gordon, 2006; Card & Payne, 2002; Murray et al., 1998). For example, Baicker and Gordon (2006) regressed the occurrence of a court-ordered school finance reforms on a serial of state-

year characteristics including constitutional language on education, population, income, unemployment, and racial composition. The adjusted R^2 of this regression was 0.037. This provided strong evidence that the occurrence of court-ordered school finance reform was exogenous.

Comparison state without treatment. The DID design needs a comparison group which did not experience treatment, such as a school finance reform in this study, in the same period as in treatment states. Vermont implemented its new finance system in 1999 and New Hampshire implemented its new finance system in 2000. Since I use 6 years of pre-and post-reform data for the reasons that I will explain later, I need to make sure that there was no dramatic finance system reform from 1996 to 2001 for selecting a comparison state to Vermont, and from 1997 to 2002 for a comparison to New Hampshire. Rhode Island and Maine are good choices as comparison states for Vermont as well as New Hampshire for the reasons that I show below.

Rhode Island. There were no dramatic school finance reforms in Rhode Island during the targeted period. Since 1996 the amount of state aid in Rhode Island that each school received was based on former year state aid adjusted by Consumer Price Index (CPI) (Wong, 2013). The original amount of base state aid that each school district received was calculated by the wealth of a school district versus the wealth per student of the state as a whole. There was no change in the way of distribution of general state aid from 1996 to 2003 although it changed its way of distributing categorical grants¹ to school districts. It is reasonable to assume that there was no strong effect of this change

¹ In the school finance system of each state, there is a portion of state aid that is distributed to school districts based on particular characteristics of the school districts or students rather than local wealth. These characteristics could be transportation, density of population, special education students, and so on. This portion of state aid is called as categorical aid.

because categorical grants only accounted for about four percent of total revenue for schools (Bilotti, 1999).

Maine. The school finance system in Maine during this period was a foundation system (Dow, Watkins, Leighton, & Cameron, 1999). The state guaranteed foundation cost was divided into operating cost, program cost, debt cost and adjustment. Operating cost and program cost was calculated by the actual cost of previous year for current year school budget. These two costs were funded by both the state government and school districts. The share of school districts of these costs depended on local fiscal ability. School districts with high property value shared more of the costs than school districts with low property value. Other than the guaranteed foundation amount, school districts were allowed to levy additional property tax. In 1997, Maine made a change in measuring local fiscal ability, for operating costs only, 85% of local fiscal capacity depended upon relative property values, and 15% depended upon relative median household income and cost of living. Before 1997, 100% of local fiscal ability depended on relative property values. This change did not alter the state share of public school funding and tax price of school districts. In 2004, Maine changed its method to calculate the guaranteed foundation amount into a program called Essential Program and Service. The state government identified essential programs and services that were needed to ensure that all students had equitable opportunities to achieve proficiency in state learning standards. Overall, there was no big change in the school finance system in Maine before 2004.

In sum, in terms of the absence of treatment, school districts in Maine and Rhode Island meet the second assumption for being comparison groups to school districts in Vermont and New Hampshire during targeted period.

Assumption of comparability. A DID design requires that the factors affecting outcome variable are controlled and balanced in reform states and comparison states. I use the following ways to meet this assumption. At first, there are some characteristics of school governance and finance system in reform states and control states that could influence the effect of school finance reforms. But they are difficult to quantify. To control these factors, I limit my choice of comparison states to New England states because Vermont and New Hampshire are New England states. Because of historical and geographical reasons these states have three similar characteristics in term of education governance and finance. First, school districts are coterminous with town or city governments. School district finance is part of and dependent on town or city budgets. Second, school district budgets are voted by a city council or town meeting. Third, there is no expenditure or tax rate limit for school districts in these four states. Massachusetts was excluded because the state passed the *Massachusetts Education Reform Act* in 1993 after the decision of the *McDuffy v. Secretary of the Executive Office of Education* (1993). Connecticut was excluded also because its school finance system was reformed in 1997. Finally, Rhode Island and Maine were chosen as treatment states.

However, these similarities do not reduce all the biases from some variables such as locality of school districts. In order to reduce the influence of these difference among states on the DID estimation, fixed effect of school districts is included in the DID model. This could reduce the bias from time-invariant factors of school districts on the estimation by including a constant fixed effect of these time-invariant factors for each school district on outcome variable (Angrist & Pischke, 2008).

Third, I include two important control variables which influence per pupil spending. Enrollment of school districts and percentage of students eligible for free lunch program were reported to be related to per pupil spending (Denzau & Grier, 1984; Manwaring & Sheffrin, 1997). Since the control variables have substantial explanatory power on per pupil spending, including them in regression reduces residual variance. Thus, this improves the precision of the DID estimation of the effect of school finance reforms (Angrist & Pischke, 2008).

Finally, I combine Maine and Rhode Island together as one comparison group. This could help cancel out the particularity of only one comparison state. However, caution should be advised that it is possible that the DID estimation of the school finance reforms in Vermont and New Hampshire could be biased by the choice of comparison state. I will introduce a way to test the liability of using combined comparison states in the model validation section.

Although the school finance reforms in Vermont and New Hampshire shared many similarities such as the increase of state funding and the establishment of statewide property tax, the methods of distributing state fund in the two states were totally different. Vermont used a combination of foundation program and power equalization program while New Hampshire used a foundation program. Therefore, I estimate their impacts separately.

In sum, I combine Maine and Rhode Island into one group and use it as a comparison group for Vermont and New Hampshire separately.

Data

Data Sources

The unit of analysis of this study is school district. The finance data could be accessed from the Common Core of Data (CCD) of the U.S. Department of Education's National Center for Education Statistics (NCES). CCD data is a comprehensive annual dataset on school districts from 1990 to 2012. The dataset used in this study for Vermont and its comparison states is from 1996 to 2001. The dataset used for New Hampshire and its comparison states is from 1997 to 2002.

The choice of a period of six years of data for Vermont and New Hampshire in this study is mainly based on the status of school finance systems in reform and comparison states although econometrics literature shows that it is better to include more years of data in order to reduce the bias from group error and serial correlation in the DID design (Donald & Lang, 2007). The Vermont school finance reform that I study was implemented in 1999. In February of 2004, Vermont initiated another school finance reform. Thus maximum number of years of post-reform data is six. The New Hampshire school finance reform that I study was implemented in 2000. In June of 2003, the New Hampshire legislature reformed its school finance again by reducing the statewide property tax rate from \$5.80 to \$4.92 for fiscal year 2004 and further down to \$3.24 for FY 2005. Therefore, it is better to only use data from 2000 to 2003 as post-reform data. Thus, the maximum number of year of post-reform data is four for New Hampshire.

However, for pre-reform data, the appropriate data for comparison state Rhode Island began in 1996. Since the school finance reform in Vermont was implemented in 1999, the maximum year of pre-reform data for Vermont is three. In order to make sure

that the number of years of post-reform data at least is balanced with the pre-reform data and make the studies on Vermont and New Hampshire comparable, I choose three years pre-reform data and three years post-reform data for Vermont and New Hampshire.

This approach is also consistent with the existing studies on the school finance reforms in Oregon (Figlio, 1998), Massachusetts (Dee & Levine, 2004), and Michigan (Chaudhary, 2009). These studies also used the DID research design and included six years of data in their models. In sum, the dataset for Vermont is from 1996 to 2001. The dataset For New Hampshire is from 1997 to 2002.

Data Preparation

The data that I use in this study was downloaded from the Elementary/Secondary Information System (ELSi) on the NCES website. The first step in creating the dataset for this study was to keep information on school districts. This step excluded the institutions like supervisory union administer office, regional educational service agency, state or federal agency charged with instruction service, and charter school agency. There were 280 school districts in Maine, 177 school districts in New Hampshire, 36 school districts in Rhode Island, and 284 school districts in Vermont in the dataset.

The second step in creating dataset for this study was to keep school districts which operate elementary and/or secondary school districts. This means that non-operating school districts, which sent their children to other school districts, were excluded. This type of school districts was excluded because expenditure and revenue structure was different between school districts operating schools and not. Another reason was that the NCES dataset did not provide information on expenditure and enrollment on this type of school districts. In Maine, there were 57 non-operating school districts and

one school district without information on whether it operated schools. In New Hampshire, there were 14 non-operating school districts and two school districts without this information. In Rhode Island, there were no non-operating school districts. In Vermont, there were 42 non-operate school districts and seven school districts without this information. Further, one school district in New Hampshire without information on per pupil spending was also excluded. Altogether, there were 222 school districts in Maine, 160 school districts in New Hampshire, 36 school districts in Rhode Island, 235 school districts in Vermont in the dataset. Information on per pupil spending, enrollment, and number of students in free lunch program of school districts was included in the dataset.

Definition of Variables

Dependent variable. The dependent variable used in this study is “total current expenditures for elementary and secondary education per student” (Cornman, Keaton, & Glander, 2013, p. A-2). Total current expenditures for elementary and secondary education per student are the district's total current expenditures for elementary and secondary education divided by the fall membership as reported in the district finance survey. The total current expenditures for public elementary and secondary education are the expenditures “associated with the day-to-day operations of the school district” (Cornman et al., 2013, p. A-2). They exclude long term expenditures (like capital outlays), debt service, and expenditures beyond the scope of public, elementary and secondary education (Cornman et al., 2013). The total current expenditures are also reduced by current expenditures made on behalf of students not included in the fall membership, which includes the expenditures for public charter schools and private schools (Cornman

et al., 2013). These data were taken from the CCD LEA Finance (F-33) survey. In this dissertation, for simplification, I use per pupil spending to indicate total current expenditures for elementary and secondary education per student.

Because per pupil spending represents the level of educational resources available to students, it was commonly used in the studies on school finance reforms (Card & Payne, 2002; Murray et al., 1998; Springer et al., 2009). Also, this variable was chosen as dependent variable in this study because the increase of spending in poor schools was the aim of plaintiffs in school finance litigations and school finance reforms in both Vermont (*Brigham v. State*, 1997) and New Hampshire (*Claremont v. Governor*, 1997). Data on per pupil spending was converted into 2000 dollar using Consumer Price Index (CPI).

Key independent variable. School finance reform is the variable of interest in this study. As indicated above, school finance reform is treated as a dummy variable. For school districts in treatment state after the reform, the value of the variable is one while the value of it is zero otherwise. School finance reform is a broad concept. It only has specific meaning when it refers to a specific school finance reform. In the case of Vermont, the school finance reform refers to the change in school finance system which was included in the *Act 60* of 1997. In New Hampshire, the school finance reform refers to the school finance reforms that occurred in 1998. Throughout the dissertation, when the school finance reforms in Vermont and New Hampshire are mentioned, I mean these two specific school finance reforms, except indicated otherwise.

Control variables: enrollment. I control for the effect of known factors on per pupil spending in analytical models. According to an existing study (Denzau & Grier, 1984), per pupil spending of each school district was significantly related to total

enrollment. Total enrollment of school districts is treated as controlling variables. The CCD dataset provides the number of fall membership of students of each school district. I convert it into its logarithm because the distribution of enrollment is extremely right skewed.

Control variable: percentage of students in free lunch program. Local wealth level is related to per pupil spending (Hoxby, 1998). The percentage of students eligible for free lunch program is an indicator of local wealth level because families whose children are in free lunch program earn less money (Chaudhary, 2009). Therefore, percentage of students in free lunch program is included in my statistical models as a control variable. It is calculated by dividing the number of students in free lunch program by fall enrollment of school districts.

Statistical Model

Difference in Difference Model

A DID model analyzes variation in the dependent variable by utilizing a control group and a comparison with pre-reform and post-reform data. The normal specification of DID is a multiple regression model shown as Model 1 (Abadie, 2005; Donald & Lang, 2007; Meyer, 1995).

Model 1:

$$Y_{jst} = \alpha_0 + \alpha_1 R + \alpha_2 T + \beta_j + \varepsilon_{jst}$$

In this study, Y_{jst} is per pupil spending for school district j , in state s and year t . R is a dummy variable for school districts in reform states after school finance reforms. The value of the variable is one for school districts in reform states after the reforms while the value of is zero otherwise. The coefficient α_1 of R is the DID estimator of the effect of

school finance reform on per pupil spending. It indicates the result of the difference in per pupil spending before and after reforms in reform states subtracted by the difference in per pupil spending before and after reforms in comparison states. T is a series of dummy variable for each year. The coefficient α_2 accounts for the fixed effect for each year. Fixed effect β_j for each school district is included in order to control the effect of time-invariant variables on the variation of per pupil spending. Standard error ε_{jst} is clustered at school district level.

Model 2:

$$Y_{jst} = \alpha_0 + \alpha_1 R + \alpha_2 T + \gamma X_{jst} + \beta_j + \varepsilon_{jst}$$

In model 2, a series of control variable X are included in this study in order to control the influence of other variables on the estimation of coefficient α_1 . These include student enrollment of school districts and percentage of students in free lunch program in this study.

In order to know the effect of school finance reforms on per pupil spending for rich and poor school districts, I divide school districts in each state into three groups based on per pupil student spending before school finance reforms. The first step is to average per pupil spending of three years pre-reform data. Then I categorize school districts into three quantiles based on the averaged per pupil spending before reforms within each state. The school districts whose average per pupil spending was in the lowest quantile are regarded as poor school districts. The school districts whose average per pupil spending was in the highest quantile are regarded as rich school districts. Because per pupil spending of school districts is close to normal distribution, school districts at the top third and lower third in per pupil spending includes school districts

with higher spending and lower spending. Also, the method was used in the study of the effect of school finance reform in Massachusetts (Dee & Levine, 2004). Dee and Levine effectively showed that the school finance reform produced different effect on these three groups of school districts. In the following analysis, I first use data on all school districts, and then use data on poor and rich school districts to run the statistical model presented above.

Model Validation

Although I try to meet the requirements of a DID design, because of data availability and the complexity of school finance reforms it is possible that some biases in estimation might occur. In this section, I identify the potential biases and propose ways to check them.

Combined comparison groups. Whether the treatment and comparison groups are comparable is important for DID research design (Angrist & Pischke, 2008; Heckman, 1997). In this study, school finance system in each state is a complicated system. Each year, states might tinker their school finance system. It is difficult to find a perfect state whose school finance system did not change at all. A combination of two similar comparison groups could reduce the bias from the minor changes that occurred in comparison groups (Meyer, 1995). In order to check the effectiveness of this method, I use Maine and Rhode Island as comparison state separately in order to check the robustness of the estimations derived from the model using a combination of the two states.

Multiple periods of data. For DID design, one choice to make is how many years of data after reform the analysis use. If only one year of post-reform data is

included, there is not enough time for school districts to fully respond to new reforms (Hoxby, 2001). If a chosen time period is too long, it is difficult to control for the effect of changes occurring during the time on the estimation of targeted school finance reforms (Dee & Levine, 2004). Therefore, I conduct separate analyses for two and three years of pre and post reform data in order to check whether the choice of period influences the estimation of the impact of school finance reforms. When I use four years of data, the categorization of poor and rich school districts is based on average per pupil spending of two years of pre-reform data.

Serial correlation correction. The DID design could suffer from serial correlation problems because it uses several time point pre-and post-reform data (Angrist & Pischke, 2008; Bertrand, Duflo, & Mullainathan, 2002; Donald & Lang, 2007). One assumption of Ordinary Least Square (OLS) estimation of error term is that the error term is not correlated among observations. However, for time-serial data, it is possible that the value of outcome variable for an individual at one time point is correlated the value for the same unit at later time point. This is called serial correlation. The violation of the assumption for the OLS estimation of standard error leads to the underestimation of the standard error of DID estimators. This could mistakenly lead to reject null hypotheses when there is no significant difference in outcome variable of interest before and after treatment.

Clustering standard errors within school districts is a common way to correct the issue of serial correlation (Donald & Lang, 2007). Donald and Lang (2007) showed that the cluster approach might be unreliable when the number of treatment and comparison groups was small. However, Brewer, Crossley, and Joyce (2013) showed that an adjusted

method of clustering standard error, which is what Cluster command in statistical software STATA does, could provide an efficient estimation even when the number of group is small. However, there is no consensus on the way to solve serial correlation problems in DID estimation (Angrist & Pischke, 2008). In this study, there are only two comparison states and one treatment state. In my analysis, the standard error is estimated by clustering standard error at school district level. I use the adjusted way of clustering the standard error suggested by Brewer et al. (2013). Bertrand et al. (2004) showed that averaging time series data into one time point pre-reform and one time point post-reform data could effectively solve the serial correlation problem even when the number of groups is small. Following Bertrand et al. (2014), I also average time series data into pre- and post-reform data to run the analytical model that I present above.

Summary

This chapter introduces the methodology that this study uses. This study uses a DID research design. First, I provide the rationales behind this design. In this section, I provide evidences that this study meets the three assumptions that a DID research design requires. Second, I describe data that this study uses. This includes data source, data preparation, and the definition of variables. Third, I present the DID statistical model. Finally, I provide several ways to check the robustness of the DID estimation of the effect of the school finance reforms in Vermont and New Hampshire.

CHAPTER 4 RESULTS

Introduction

The purpose of this study was to evaluate the effect of the school finance reforms in Vermont and New Hampshire on per pupil spending of school districts. Specifically, this study answers the following three questions:

1. Did the school finance reforms in Vermont and New Hampshire change per pupil spending in all school districts?
2. Did the school finance reforms in Vermont and New Hampshire change per pupil spending in poor school districts?
3. Did the school finance reforms in Vermont and New Hampshire change per pupil spending in rich school districts?

This chapter presents descriptive results and inferential results to address these questions. As mentioned in Chapter 3, the school finance reforms in Vermont and New Hampshire is evaluated separately. Thus, this chapter includes two sections. In the first sections, I present the descriptive results and the DID estimation of the effect of the school finance reform in Vermont on per pupil spending. I also include the results of robustness checks of the models. In the second section, the results for the school finance reform in New Hampshire are presented. This section also includes the descriptive and inferential results, and robustness checks for examining the school finance reforms in New Hampshire.

Vermont

Descriptive Results

Depending variable: per pupil spending. Regarding the data on per pupil spending of school districts in Vermont, per pupil spending in eight school districts in Vermont in 1998 was lower than \$3,000 or even \$2,000. This was abnormal because per pupil spending in these school districts in other years was more than \$4,000. Also, per pupil spending of each school district in Rhode Island, and Maine in 1998 was more than \$4,000. The total revenue, local revenue, state revenue, and enrollment in these school districts did not have this kind of abrupt change in 1998. Further, 6 out of the 8 school districts were in the same supervisory union in which they shared one superintendent. Based on these facts, it is high chance that the data for these eight school districts in Vermont in 1998 was not correctly reported or input. Thus, I excluded these eight school districts in my analysis. However, because I was not sure about this conclusion and unable to pinpoint the exact reason for this I also run my analysis by including these eight school districts in the robustness check section in order to see the influence of the exclusion of these eight school districts.

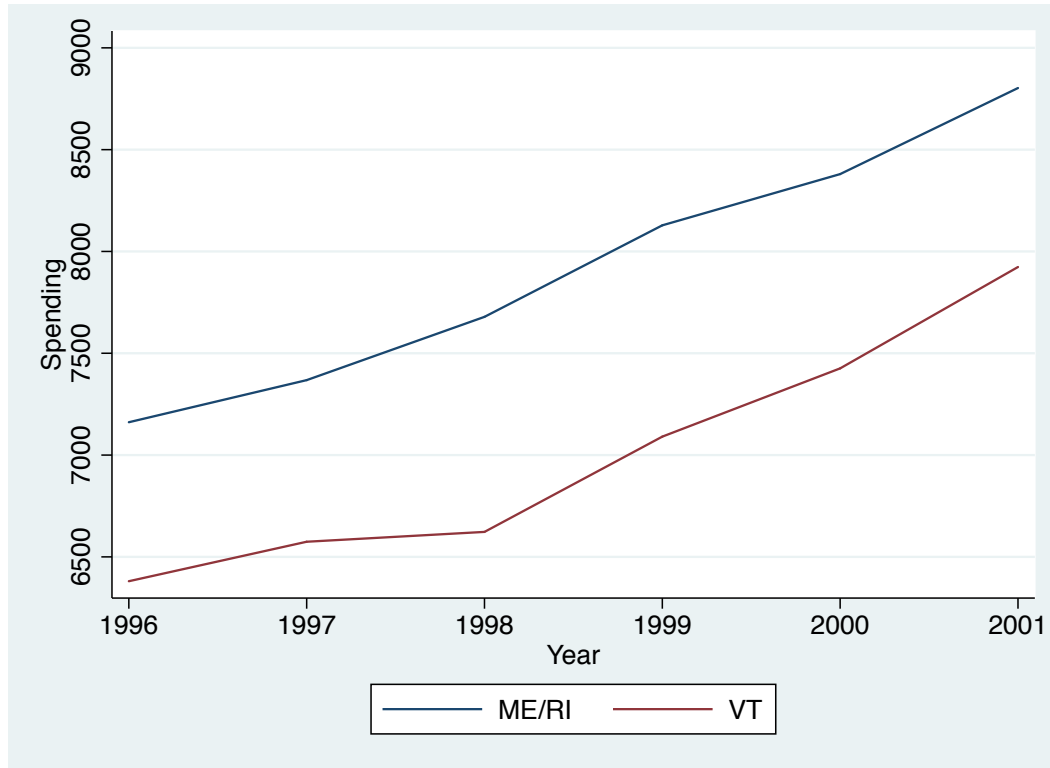
Table 2 shows the descriptive results of dependent variable per pupil spending in the reform state Vermont and the comparison states Maine and Rhode Island. In Vermont, 227 school districts were included in this study. Average per pupil spending of school districts was \$6,623 in 1998, the last year before the implementation of the school finance reform. Average per pupil spending of school districts increased by \$468 in 1999, by \$335 in 2000, and by \$498 in 2001. As shown in Figure 4, the change rate of per pupil spending, as indicated by the slope of the trend line, increased after the reform. All of

these results indicate that the average per pupil spending of school districts increased after the school finance reform in Vermont.

Table 2: Descriptive Characteristics of per Pupil Spending in Reform and Comparison States: 1996-2001

year	Comparison State: ME/RI			Reform State: VT		
	Mean	SD	Freq	Mean	SD	Freq
1996	7161	1645	258	6380	1186	227
1997	7368	1657	258	6574	1099	227
1998	7679	1860	258	6623	1195	227
1999	8128	2346	258	7091	1383	226
2000	8380	2310	258	7426	1260	227
2001	8803	2306	258	7924	1410	226
Total	7920	2120	1548	7002	1369	1360

Figure 4. The Trend of per Pupil Spending in Reform and Comparison States: 1996-2001



The comparison group included 222 school districts in Maine and 36 school districts in Rhode Island. Average per pupil spending was \$7,679 in 1998. As shown in Figure 4, it also experienced a gradual increase from 1996 to 2001. However, Maine and Rhode Island did not reform their school finance systems as Vermont did in this period.

Figure 5 shows the change in the distribution of per pupil spending in Vermont. The green bar shows the distribution of per pupil spending in 1998 before the school finance reform. There existed much variation in per pupil spending among school districts in 1998. The lowest per pupil spending was \$3,582 in Woodford School District while the highest per pupil spending was \$10,816 in Guildhall School District. The white bar with black border shows that the distribution of per pupil spending in 2001, the third year after the school finance reform. As Figure 5 indicates that the variation in per pupil spending among school districts did not change much while the mean of per pupil spending among school districts increased. Also, Figure 5 shows that the distribution of per pupil spending was slightly right skewed. That means that the number of school districts spending higher was less than those with lower spending. This was one reason that I converted the variable into its logarithm, which was closer to normal distribution as shown in Figure 6, in the following regression analysis.

Figure 5. The Histogram of per Pupil Spending in Vermont in 1998 and 2001

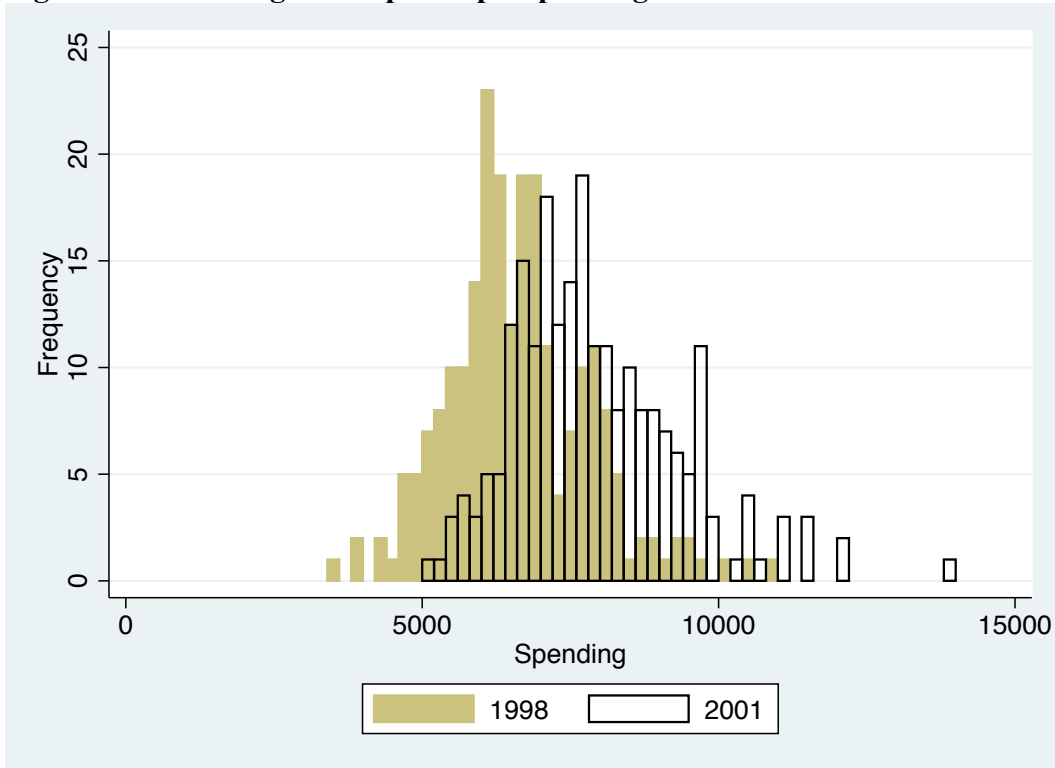
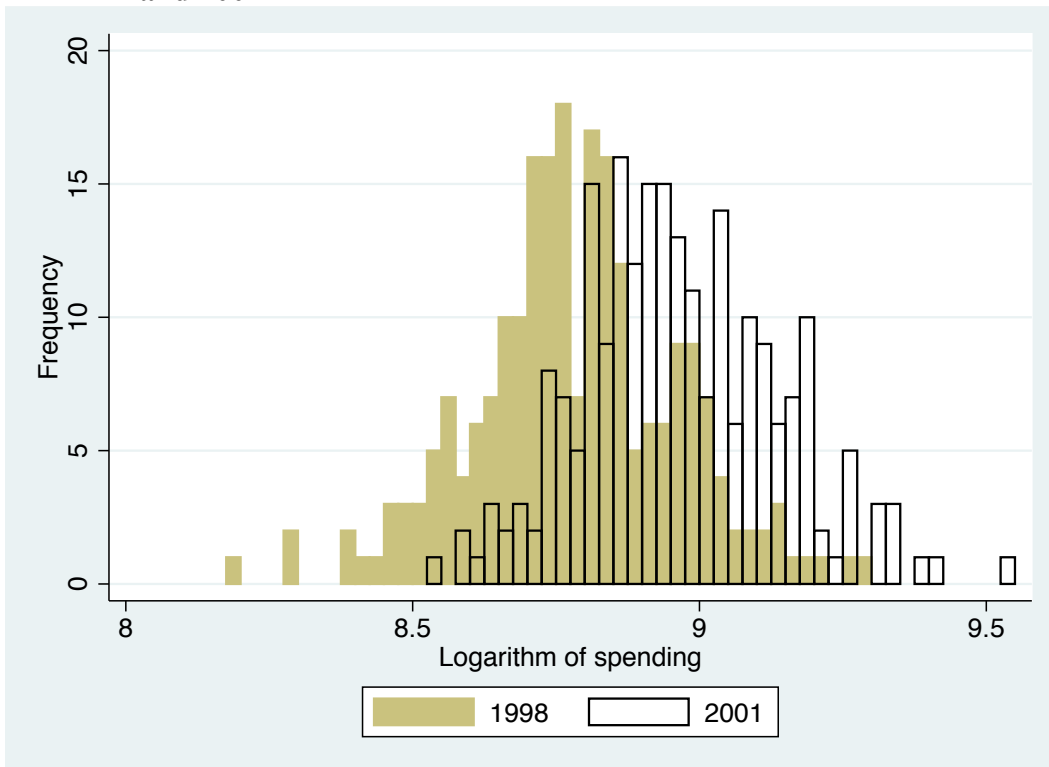


Figure 6. The Histogram of Logarithm of per Pupil Spending in Vermont in 1998 and 2001



Independent variable: student enrollment of school districts. Table 3 shows the trend of enrollment of school districts in the reform state Vermont and the comparison states Maine and Rhode Island. The average enrollment of school districts stayed stable from 1996 to 2001 in Maine and Rhode Island as shown in Figure 7. For example, the average enrollment was 1,408 in 1997, and the average enrollment was 1,412 in 2001. In Vermont, the average enrollment experienced a gradual and small decrease by 2-7 students in each year from 1996 to 2001. The average enrollment was 428 in 1996 while it was 409 in 2001.

Table 3: Descriptive Characteristics of Enrollment of School Districts in Reform and Comparison States: 1996-2001

Year	Comparison State: ME/RI			Reform State: VT		
	Mean	SD	Frequency	Mean	SD	Frequency
1996	1403	2216	258	428	520	227
1997	1408	2242	258	425	510	227
1998	1411	2293	258	423	510	227
1999	1409	2319	258	421	515	226
2000	1416	2327	258	416	513	227
2001	1412	2352	258	409	513	226
Total	1410	2288	1548	420	513	1407

Figure 7. The Trend of Enrollment of School Districts in Reform and Comparison States: 1996-2001

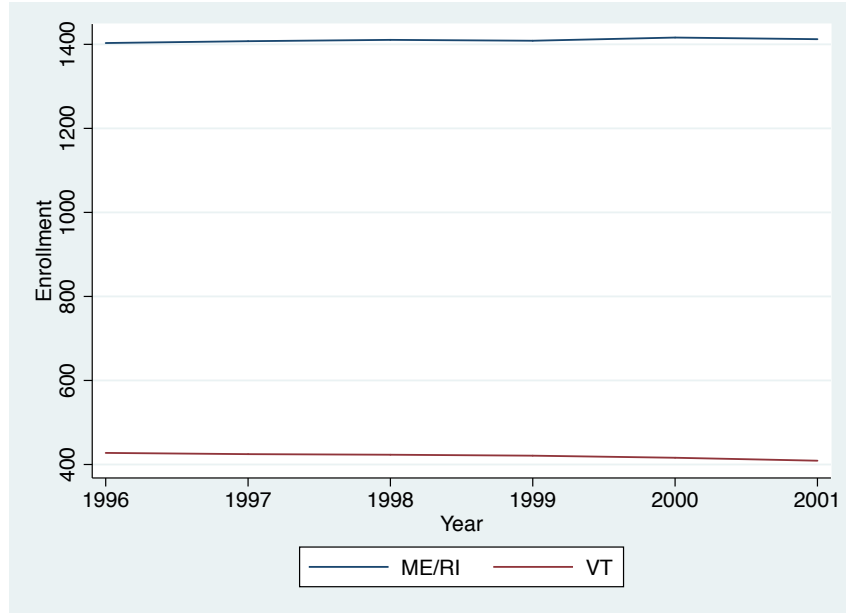


Figure 8 shows the distribution of enrollment of school districts in Vermont. The green bar shows the distribution of enrollment in 1998. The white bar with black border shows the distribution of enrollment in 2001. It presents that there was huge variation in enrollment among school districts. For example, the enrollment in Granby School District was 11 while the enrollment was 3,751 in Burlington School District. Also, the distribution was extremely right skewed. This means that larger number of school districts had smaller enrollment. The distribution of student enrollment of school districts did not change much from 1998 to 2001 because the distributions of the two years overlapped in Figure 8. Since the distribution of enrollment was right skewed, I included its logarithm in my analytical models. The logarithm of enrollment was closer to normal distribution as shown in Figure 9.

Figure 8. The Histogram of Enrollment of School Districts in Reform and Comparison States in 1998 and 2001

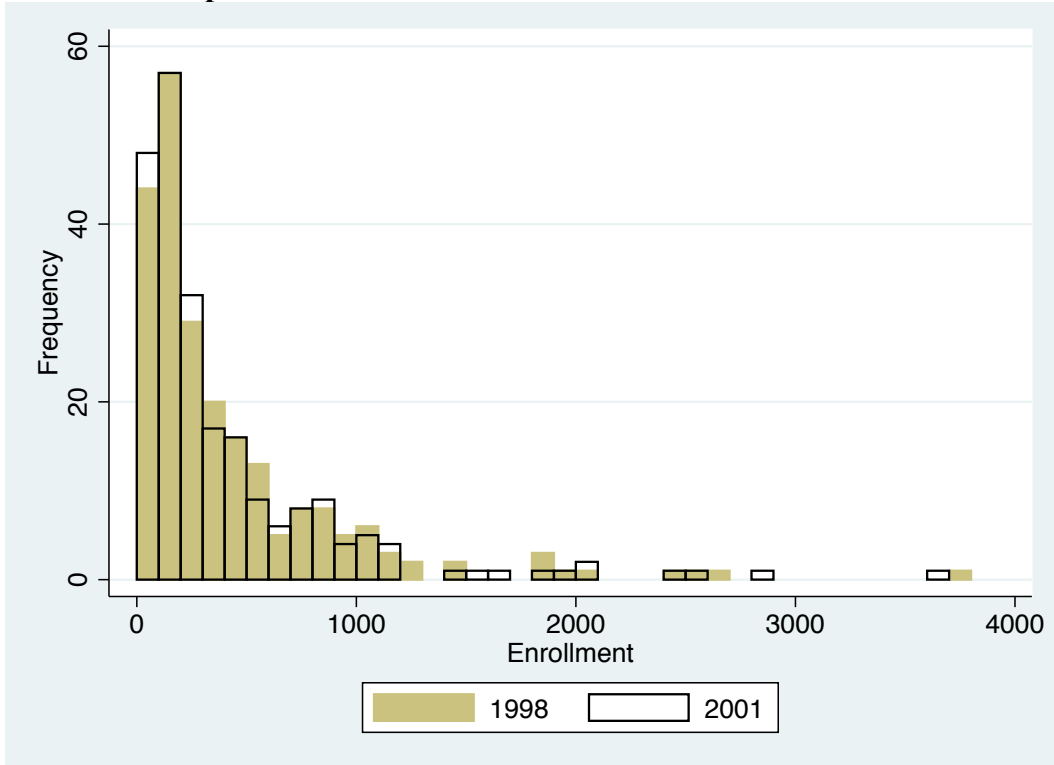
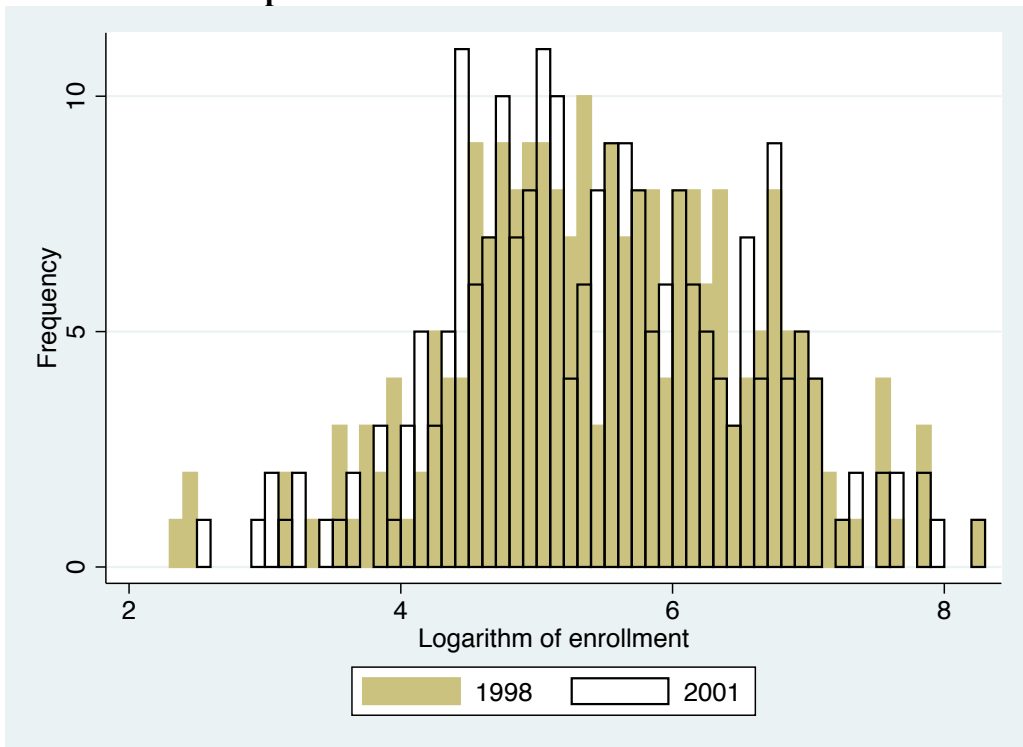


Figure 9. The Histogram of Logarithm of Enrollment of School Districts in Reform and Comparison States in 1998 and 2001



Independent variable: percentage of students in free lunch program. Table 4 shows the descriptive characteristics of percentage of students in free lunch program in the reform state Vermont and the comparison states Maine and Rhode Island. It shows that data on percentage of students in free lunch program was not balanced from 1996 to 2001. The Total number of school districts included in this study was 227 in Vermont and 258 in Maine and Rhode Island. Thus, in each year, the number of school districts was supposed to 227 for Vermont and 258 for Maine and Rhode Island. As shown in Table 4, there were some missing values for the variable in both reform state and comparison states. For example, there were 23 missing values in 2000 for the comparison states and 78 missing value for the reform state in 1998. This indicates that average percentage of students in free lunch programs of different year was not comparable because they included different size of samples in different years.

Table 4: Descriptive Characteristics of Percentage of Students in Free Lunch Program in Reform and Comparison States: 1996-2001 (Unbalanced)

Year	Comparison State: ME/RI			Reform State: VT		
	Mean	SD	Frequency	Mean	SD	Frequency
1996	0.255	0.152	245	0.190	0.146	227
1997	0.256	0.151	244	0.228	0.124	200
1998	0.256	0.140	241	0.342	0.176	149
1999	0.248	0.138	245	0.223	0.154	172
2000	0.231	0.157	235	0.207	0.124	206
2001	0.215	0.137	236	0.186	0.123	226
Total	0.244	0.147	1446	0.223	0.148	1180

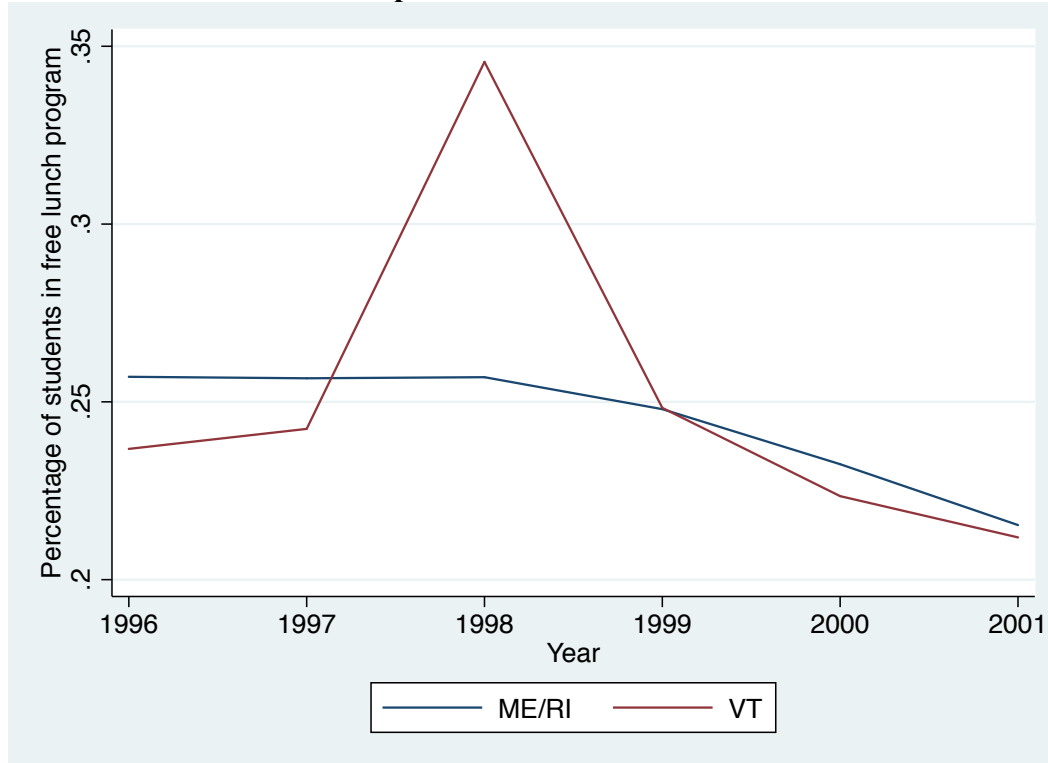
Table 5 shows that the descriptive characteristics of percentage of students in free lunch program in the reform state Vermont and the comparison states Maine and Rhode Island when school districts with missing value were excluded. In Vermont, the percentage of students in free lunch program was stable from 1996 to 1997. However,

there was a dramatic increase in the percentage of students in free lunch program in 1998. It increased from 0.228 in 1997 to 0.342 in 1998 (shown in Figure 10). In 1999, it backed to the value level in 1997, and experienced a small decrease in 2000 and 2001. Looking at the percentage of students in free lunch program in other states, in 1998, Georgia and South Dakota experienced the same abrupt up-and-down trend. Wyoming had this up-and-down trend in percentage of students in free lunch program in 2001. I was not able to figure out why this dramatic increase happened. Therefore, I ran separate analytical models both with and without the variable to check the influence of the dramatic increase in percentage of students in free lunch program in Vermont on the estimation of the effect of its school finance reform. In the comparison states, the percentage of students in free lunch program did not change much from 1996 to 1998. It decreased by 0.9% in 1999, 1.6% in 2000, and decreased by 1.7% in 2001.

Table 5: Descriptive Characteristics of Percentage of Students in Free Lunch Program in Reform and Comparison States: 1996-2001 (Balanced)

Year	Comparison State: ME/RI			Reform State: NH		
	Mean	SD	Frequency	Mean	SD	Frequency
1996	0.257	0.155	228	0.239	0.137	147
1997	0.257	0.152	228	0.245	0.133	147
1998	0.257	0.142	228	0.347	0.175	147
1999	0.248	0.141	228	0.249	0.143	146
2000	0.232	0.158	228	0.226	0.131	147
2001	0.215	0.137	228	0.214	0.121	147
Total	0.244	0.148	1368	0.254	0.147	881

Figure 10. The Trend of Percentage of Students in Free Lunch Program in the Reform and Comparison State: 1996 to 2001



Correlations of all variables. Table 6 shows the correlations of all variables for the analysis of the school finance reform in Vermont. Data included school districts in Vermont, Maine, and Rhode Island from 1996 to 2001. The correlation coefficient between reform and per pupil spending was -0.007 , indicating that the relationship was much weak. The correlation coefficient between per pupil spending and percentage of students in free lunch program was $-.045$. This was also a weak relationship. The correlation coefficient between per pupil spending and enrollment was -0.143 . This was a modest negative correlation. It indicates that school districts with larger enrollment spent less on education. However, all these correlations were calculated without controlling other variables and cannot reveal the true relations among these variables although it provided some suggestive relations among these variables. More advanced models were needed to understand the relation between school finance reforms and per pupil spending.

Table 6: Correlation of All Variables for Vermont

	Spending ^a	Freelunch ^b	Enrollment ^c	Reform ^d	Year
Spending	1.000				
Freelunch	-0.045	1.000			
Enrollment	-0.143	-0.146	1.000		
Reform	0.007	-0.114	-0.233	1.000	
Year	0.319	-0.078	-0.005	0.499	1.000

Note. Data include school districts in Vermont, Maine, and Rhode Island from 1996 to 2001.

^a logarithm of per pupil spending. ^b percentage of students with free lunch.

^c logarithm of enrollment. ^d dummy for reform state after reform

Difference in Difference Estimations

In order to know the causal effect of the school finance reform in Vermont on per pupil spending, a DID estimation was used in this study. It utilized a comparison group and pre-and post-data to reduce the bias from unknown variables. Year and school district fixed effect were also included in models. To account for the possibility of serial correlation in the error term, standard error was clustered at school district level. Also, in order to see the influence of control variables on the DID estimation, I ran the models with and without control variables. Finally, it should be mentioned that the coefficients of independent variables could not be interpreted directly because of the use of logarithm of per pupil spending and enrollment in models. I interpret the coefficients by converting them into percentage of increase or decrease in per pupil spending.

The purpose of this study is to know the effect of the school finance reform in Vermont on per pupil spending in all school districts, poor, and rich school districts. Thus, I present results for all school districts, poor school districts, and rich school districts separately.

All school districts. Table 7 shows the results of the DID estimation of the school finance reform in Vermont on per pupil spending for all school districts. Column 1 and 2

show the results from four years of data without and with control variables. The coefficients of reform variable were 0.008 without control variable and -0.017 with control variables. Both coefficients were not statistically significant at the 10% significance level. Column 3 and 4 show the results from six years of data without and with control variables. The coefficients of reform variable were 0.011 without control variable and -0.010 with control variables. Both coefficients were not statistically significant at 10% significance level. In sum, the DID estimation indicates that the school finance reform in Vermont had no significant effect on average per pupil spending in all school districts.

Table 7: DID Estimation of the Effect of the School Finance Reform in Vermont on per Pupil Spending in All School Districts with Comparison State Maine and Rhode Island

	Four Years of Data		Six Years of Data	
	No Control (1)	Control (2)	No control (3)	Control (4)
Reform	0.008 (0.009)	-0.017 (0.011)	0.011 (0.010)	-0.010 (0.010)
1997			0.030*** (0.004)	0.030*** (0.003)
1998	0.023*** (0.005)	0.033*** (0.005)	0.054*** (0.006)	0.063*** (0.006)
1999	0.078*** (0.007)	0.080*** (0.006)	0.107*** (0.008)	0.109*** (0.007)
2000	0.118*** (0.007)	0.129*** (0.007)	0.147*** (0.008)	0.157*** (0.008)
2001			0.204*** (0.008)	0.208*** (0.009)
Logarithm of enrollment		-0.231*** (0.050)		-0.236*** (0.041)
Percentage of free lunch students		-0.143** (0.071)		-0.110** (0.052)
Constant	8.834*** (0.003)	10.254*** (0.308)	8.803*** (0.004)	10.240*** (0.248)
Observations	1,939	1,692	2,908	2,626
R ²	0.314	0.403	0.451	0.533
Number of id	485	471	485	475

Note. School district level fixed effect is included in each model above. Robust standard errors in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

The coefficients of the logarithm of enrollment were -0.231 with four years of data and -0.236 with six years of data. Both were significant at 1%. This negative relation

indicates that per pupil spending decreased by 0.23%² or 0.24% if enrollment increased by 1%. The average per pupil spending was \$6,623, and the average enrollment was 423 in 1999 in Vermont. This means that about 4 students increase reduced per pupil spending by \$15 or \$16.

The coefficients of percentage of students in free lunch program were -0.143 with four years of data and -0.110 with six years of data. Both were significant at 5%. This negative relation indicates that per pupil spending decreased 0.1%³ if percentage of students in free lunch program increased by 1%. Average per pupil spending was \$6,623 in 1999. This means that one percent increase of students in free lunch program students reduced per pupil spending by \$7.

Poor school districts. Table 8 shows the results of the DID estimation of the school finance reform in Vermont on per pupil spending for poor school districts whose average per pupil spending before the reform belonged to the lowest third. Column 1 and 2 show the results from four years of data without and with control variables. The coefficients of the variable reform were 0.032 without control variables and 0.021 with control variables. Both coefficients were statistically significant at 5% and 10% significance level respectively. The coefficients 0.032 and 0.021 means that the school finance reform increased per pupil spending in poor school districts in Vermont by 3.1%⁴

² Since the dependent variable is logarithm of per pupil spending, the independent variable enrollment is also in its logarithm. The coefficient of the logarithm of enrollment could be understood as the percentage of increase or decrease in per pupil spending if the enrollment increases 1%.

³ Since the dependent variable was the logarithm of per pupil spending, this number was calculated by a function $(e^{0.01\gamma} - 1)$ where e was Euler's number, γ was the coefficient of the percentage of students in free lunch program. For the remaining portion of the dissertation, the coefficient of the variable percentage of students in free lunch program was converted in the same way as this.

⁴ The dependent variable was the natural logarithm of per pupil spending in this model, the coefficient of the variable reform could be converted into the percentage of increase or decrease in per pupil spending by a function of $e^{\alpha_1} - 1$ (e is Euler's number, α_1 is the coefficient of the variable reform). For the remaining portion of the dissertation, the coefficient of the variable reform was converted in the same way as this.

and 2.1%. Since the inclusion of control variables could improve the precision of the DID estimation, the model with control variable was more trustworthy. Thus, considering that per pupil spending in Vermont in 1998 before the school finance reform was \$6,233, that means that the school finance reform in Vermont increased per pupil spending in poor school districts by \$132.

Table 8: DID Estimation of the Effect of the School Finance Reform in Vermont on per Pupil Spending in Poor School Districts with Comparison State Maine and Rhode Island

	Four Years of Data		Six Years of Data	
	No Control	Control	No Control	Control
	(1)	(2)	(3)	(4)
Reform	0.032** (0.013)	0.021* (0.011)	0.041*** (0.014)	0.026** (0.012)
1997			0.032*** (0.006)	0.036*** (0.006)
1998	0.044*** (0.005)	0.047*** (0.005)	0.072*** (0.008)	0.078*** (0.007)
1999	0.089*** (0.009)	0.084*** (0.007)	0.115*** (0.009)	0.114*** (0.009)
2000	0.137*** (0.009)	0.132*** (0.008)	0.164*** (0.010)	0.164*** (0.011)
2001			0.224*** (0.011)	0.215*** (0.011)
Logarithm of enrollment		-0.208*** (0.051)		-0.278*** (0.048)
Percentage of free lunch students		-0.077 (0.053)		-0.119** (0.054)
Constant	8.659*** (0.004)	9.954*** (0.309)	8.630*** (0.006)	10.368*** (0.299)
Observations	644	588	966	892
R ²	0.546	0.613	0.652	0.721
Number of id	161	159	161	159

Note. School district level fixed effect is included in each model above. Robust standard errors in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

Column 3 and 4 show the results from six years of data without and with control variables. The coefficients of the variable reform were 0.041 without control variable, and 0.026 with control variables. Both coefficients were statistically significant at 1% and 5% significance level respectively. The coefficients 0.041 and 0.026 mean that the school

finance reform increased per pupil spending in Vermont by 4.2% and 2.6%. Since the inclusion of control variables could improve the precision of the DID estimation, the model with control variable was more trustworthy. Thus, given that per pupil spending in Vermont in 1998 before the school finance reform was \$6,233, this means that the school finance reform increased per pupil spending for poor school districts by \$164. This result indicates that the DID estimation of the effect of the reform from four and six years of data did not show much difference.

The coefficients of the logarithm of enrollment were -0.208 with four years of data and -0.278 with six years of data. Both were significant at 1%. This negative relation indicates that per pupil spending decreased 0.21% or 0.27% if enrollment increases by 1%. Based on per pupil spending \$6,233 in Vermont and the average enrollment 423 in 1998, this means that about four students increase reduced per pupil spending in poor school districts by \$14 or \$18.

The coefficients of percentage of students in free lunch program were -0.007 with four years of data and -0.119 with six years of data. Only the one with six years of data was significant at 5%. Considering the result from the six years of data, it indicates that per pupil spending decreased 0.1% if percentage of students in free lunch program increased by 1%. Based on per pupil spending \$6,233 in Vermont in 1998 before the school finance reform, this means that one percent increase of students in free lunch program students reduced per pupil spending in poor school districts by \$7.

Rich school districts. Table 9 shows the results of the DID estimation of the school finance reform in Vermont on per pupil spending for rich school districts whose average per pupil spending before the reform belonged to the highest third quantile.

Column 1 and 2 show the results from four years of data without and with control variables. The coefficients of variable reform were -0.015 without control variables and -0.056 with control variables. Only the latter one was significant at 5%. The coefficient -0.056 means that the school finance reform in Vermont decreased per pupil spending in rich school districts by 5.4%. Based on per pupil spending \$6,233 in Vermont in 1998 before the school finance reform, that means that the school finance reform decreased per pupil spending by \$339.

Table 9: DID Estimation of the Effect of the School Finance Reform in Vermont on per Pupil Spending in Rich School Districts with Comparison State Maine and Rhode Island

	Four Years of Data		Six Years of Data	
	No Control	Control	No Control	Control
	(1)	(2)	(3)	(4)
Reform	-0.015 (0.021)	-0.056** (0.025)	-0.011 (0.021)	-0.045* (0.025)
1997			0.032*** (0.008)	0.026*** (0.007)
1998	0.008 (0.011)	0.028** (0.012)	0.039*** (0.012)	0.050*** (0.012)
1999	0.076*** (0.016)	0.083*** (0.013)	0.103*** (0.018)	0.102*** (0.015)
2000	0.095*** (0.016)	0.127*** (0.019)	0.121*** (0.017)	0.143*** (0.020)
2001			0.178*** (0.018)	0.197*** (0.021)
Logarithm of enrollment		-0.247*** (0.086)		-0.218*** (0.076)
Percentage of free lunch students		-0.206 (0.126)		-0.121 (0.101)
Constant	9.034*** (0.007)	10.506*** (0.513)	9.000*** (0.009)	10.287*** (0.453)
Observations	640	541	960	839
R ²	0.139	0.248	0.250	0.334
Number of id	160	152	160	153

Note. School district level fixed effect is included in each model above. Robust standard errors in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

Column 3 and 4 show the results from six years of data without and with control variables. The coefficients of reform variable were -0.011 without control variable and -0.045 with control variables. The one without control variables was not significant while the one with control variables was only significant at 10%. The coefficient -0.045 means that the school finance reform decreased per pupil spending in Vermont by 4.4%. Based

on per pupil spending \$6,233 in Vermont in 1998 before the school finance reform, that means that the school finance reform decreased per pupil spending by \$274. The DID estimation of the effect of the reform from six years of data was lower than the one from four years of data.

The coefficients of the logarithm of enrollment were -0.247 with four years of data and -0.218 with six years of data. Both were significant at 1%. This negative relation indicates that per pupil spending decreased 0.24% or 0.22% if the enrollment increased by 1%. Based on per pupil spending \$6,233 in Vermont and the average enrollment 423 in 1998, this means that about 4 students increase reduced per pupil spending by \$16 or \$15.

The coefficients of percentage of students in free lunch programs are -0.206 with four years of data and -0.121 with six year of data. Both were not significant at even 10%.

Robustness Check

In this section I present the results for robustness check of the DID estimations of the effect of the school finance reform in Vermont presented above. There were four robustness checks of these estimations. The first was to average four year and six years into two time points of data: pre and post reform data. The second was to use data on school districts in Maine and Rhode Island individually instead of combining them. Third, I included eight school districts with abnormal per pupil spending in 1998 which were excluded in the models that I present in above section. Finally, I show the results without control variable percentage of students in free lunch program in order to check the influence of its dramatic increase in 1998 on the DID estimation. I present these results for all school districts, poor school districts, and rich school districts sequentially. I only

interpret the DID estimation of the effect of the school finance reform because it is the focus of this study. The coefficient of control variables and the information on sample size and *R* square are shown in Table 1-15 in Appendix 1. Also, I only compare the results with control variables because the model with control variables improved the precision of the DID estimation.

All school districts. Table 10 shows the results for robustness check of the DID estimation of the effect of the school finance reforms for all school districts in Vermont. In order to make comparison, the DID estimations of the effect from the models using multiple years data and a combination of comparison states that I show above were included in Row 1. Row 2 shows the results from models using averaged data. Row 3 shows the results when only school districts in Maine were used as a comparison group. Row 4 shows the results when only school districts in Rhode Island were used as comparison group. Row 5 shows the results when eight school districts with abnormal value of per pupil spending in 1998 in Vermont were included. Row 6 shows the results when the control variable percentage of students in free lunch program was excluded.

First, for the results from four years of data (Column 1), the estimated effect of the school finance reforms from the models with combined comparison states and multiple years data was negative and not significant. The models including eight school districts with abnormal data and the models without variable percentage of students in free lunch program also show this non-significant and negative effect. However, the model with averaged data and the model with Maine as comparison state show that the negative effect was significant at 5% confidence level. The coefficients were -0.034 and -0.025. This means that the school finance reform decreased average per pupil spending of

all school districts in Vermont by 3.3% and 2.5%. In sum, the DID estimations from four years of data shows some inconsistent results on the effect of the school finance reform in Vermont on per pupil spending in all school districts.

For the models with six year data, the DID estimation from the models with combined comparison states and multiple years data show that the school finance reform in Vermont had no effect on per pupil spending of school districts. The estimation from the models with averaged data, the model including eight school districts with problematic value in per pupil spending, and the model without variable of the percentage of students in free lunch program were consistent to this result. However, the models with individual comparison state Maine and Rhode Island both show that the negative effect was significant only at 10%. In sum, the DID estimations from six years of data shows that the school finance reform in Vermont had no significant effect on per pupil spending in all school districts at the significance level 5%.

Table 10: The Results of Robustness Check of DID Models for All School Districts in Vermont

Model		Four Years of Data (1)	Six Years of Data (2)
Multiple year data	(1)	-0.017 (0.011)	-0.010 (0.010)
Averaged year data	(2)	-0.034** (0.015)	-0.020 (0.013)
With Maine	(3)	-0.025** (0.011)	-0.019* (0.011)
With Rhode Island	(4)	0.018 (0.013)	0.025* (0.014)
Including eight school districts	(5)	-0.003 (0.012)	-0.001 (0.011)
Without free lunch variable	(6)	-0.003 (0.009)	-0.005 (0.010)

Note. Robust standard errors in parentheses
 *** p<0.01, ** p<0.05, * p<0.1

Poor school districts. Table 11 shows the results for robustness check of the DID estimation of the effect of the school finance reforms for poor school districts in Vermont. For the models using four years of data and with control variables (Column 1), the DID estimation of the effect of the school finance reform in Vermont from the model with averaged data was 0.033 and significant at 5%. The DID estimation from the models without eight school districts with problematic value of per pupil spending was 0.044 and significant at 1%. The DID estimation from the model without free lunch variable was 0.025 and significant at 5%. The DID estimation from the model with individual comparison state Rhode Island also shows the significant positive effect (0.049). The result from the model with comparison state Maine was also positive, but not significant.

Table 11: The Results of Robustness Check of DID Models for Poor School Districts in Vermont

Model		Six Years of Data (1)	Six Years of Data (2)
Multiple year data	(1)	0.021* (0.011)	0.026** (0.012)
Averaged year data	(2)	0.033** (0.016)	0.034** (0.015)
With Maine	(3)	0.014 (0.012)	0.020 (0.013)
With Rhode Island	(4)	0.049*** (0.014)	0.033 (0.025)
Including eight school districts	(5)	0.044*** (0.016)	0.037*** (0.014)
Without free lunch variable	(6)	0.025** (0.012)	0.028** (0.013)

Note. Robust standard errors in parentheses
 *** p<0.01, ** p<0.05, * p<0.1

For the models using six years of data and without control variable (Column 2), the DID estimation from the models with averaged data, the models without eight school districts with problematic value of per pupil spending, and the model without free lunch

variable were 0.034, 0.037, and 0.028. All of the three are significant at either 5% or 1%. The DID estimations from the model with individual state Maine or Rhode Island were positive but not significant.

Overall, based on the results from four years of data and six year, it could be said that the positive effect of the school finance reform in Vermont on per pupil spending in poor school districts was not sensitive to the problematic data on per pupil spending in eight school districts in 1998 and the dramatic increase in percentage of students in free lunch program. The consistency of results from multiple years of data and averaged data also shows that the estimation of standard error faces less doubt caused by serial correlation problem. The range of the DID estimation was from 0.021 to 0.049. That means that the school finance reform increased per pupil spending in poor school districts by from 2.1% to 4.7%. However, these results were sensitive to the choice of comparative states. When using either Maine or Rhode Island as a comparison state, the significance of positive effects disappeared.

Rich school districts. Table 12 shows the results for robustness check of the estimation of the effect of the school finance reforms for rich school districts in Vermont. Row 2 shows the results from models using averaged data. The DID estimations of the effect of the school finance reform in Vermont were -0.109 for the model with four years of data and -0.080 for the model with six years of data. The former coefficient was statistically significant at 1% significance level, and the latter coefficient was significant at 5% significance level.

Table 12: The Results of Robustness Check of DID Models for Rich School Districts in Vermont

Model		Six Years of Data (1)	Six Years of Data (2)
Multiple year data	(1)	-0.056** (0.025)	-0.045* (0.025)
Averaged year data	(2)	-0.109*** (0.034)	-0.080** (0.034)
With Maine	(3)	-0.063** (0.026)	-0.056** (0.027)
With Rhode Island	(4)	-0.014 (0.032)	0.009 (0.028)
Including eight school districts	(5)	-0.056** (0.024)	-0.046* (0.024)
Without free lunch variable	(6)	-0.034 (0.021)	-0.040* (0.022)

Note. Robust standard errors in parentheses
 *** p<0.01, ** p<0.05, * p<0.1

Row 3 shows the results when only school districts in Maine were used as a comparison group. The direction and significance were the same as the model using a combination of school districts in Maine and Rhode Island. The effect size was bigger (-0.063 for four years of data, -0.056 for six years of data) from the models with one comparison state Maine than the ones from the models with combined comparison states (-0.056 for four years of data, -0.045 for six years of data). Row 4 shows the results when only school districts in Rhode Island were used as comparison group. These results from both four and six years of data were not significant. The possible reason for the non-significance is that the sample size of comparison state Rhode was small. There were 12 rich school districts in Rhode Island while there were 76 rich school districts in Vermont.

Row 5 shows the results from the model including eight school districts with abnormal value in per pupil spending in 1998 was almost identical to the results from the model without these school districts. This indicates that the eight school districts with

abnormal value in per pupil spending in 1998 did not pose a problem on the DID estimation of the effect of the school finance reform on per pupil spending in rich school districts.

Row 6 shows the results from the model without control variable percentage of free lunch students. The model with six years of data provided similar results (-0.040) with the model including the control variable. The model with four years of data changes the negative effect from significance in the model with the variable to non-significance. This highlights the importance of the inclusion of the variable of percentage of students in free lunch program.

Overall, it could be said that the school finance reform in Vermont decreased per pupil spending in rich school districts by 4.4% to 10.3%. This result was not sensitive to the use of multiple year data or averaged data. However, there are two challenges to this conclusion. When the number of school districts in comparison group was small, the negative effect became not significant, for example, in the case of using Rhode Island, where there were only 12 rich school districts, as only comparison state. Second, the result was sensitive to the inclusion of the variable of the percentage of free lunch students. The potential challenge to it is that the exclusion of the variable percentage of students in free lunch program might influence the significance of the negative effect. Further evidence is needed to pinpoint the reason for the dramatic increase in percentage of students in free lunch program in 1998. At this point, I take this data as valid. The result from the model using this variable shows that the school finance reform in Vermont had significant and negative effect on per pupil spending in rich school districts.

New Hampshire

Descriptive Results

Dependent variable: per pupil spending. Table 13 shows the descriptive results of per pupil spending in the reform state New Hampshire and the comparison states Maine and Rhode Island. In New Hampshire, 160 school districts were included in this study. Average per pupil spending of school districts was \$7,207 in 1999, the last year before the implementation of the school finance reform. Average per pupil spending of school districts increased by \$213 in 2000, by \$266 in 2001, and by \$544 in 2002. As shown in Figure 11, the change rate of per pupil spending, as indicated by the slope of the trend line, increased after the school finance reform. All these results indicate that average per pupil spending of school districts increased after the school finance reform in New Hampshire.

Table 13: Descriptive Characteristics of per Pupil Spending in Reform and Comparison States: 1997-2002

Year	Comparison State: ME/RI			Reform State: NH		
	Mean	SD	Frequency	Mean	SD	Frequency
1997	7368	1657	258	6911	1780	160
1998	7679	1860	258	7031	1507	160
1999	8128	2346	258	7207	1444	160
2000	8380	2310	258	7420	1500	160
2001	8803	2306	258	7686	1712	160
2002	8924	2200	258	8230	1968	160
Total	8214	2199	1548	7414	1716	960

The comparison group included 222 school districts in Maine and 36 school districts in Rhode Island. Average per pupil spending was \$8,128 in 1999. As shown in Figure 11, it also experienced a gradual increase from 1997 to 2002. However, Maine and Rhode Island did not reform their school finance systems as New Hampshire did during the period.

Figure 11. The Trend of per Pupil Spending in Reform and Comparison States: 1997-2002

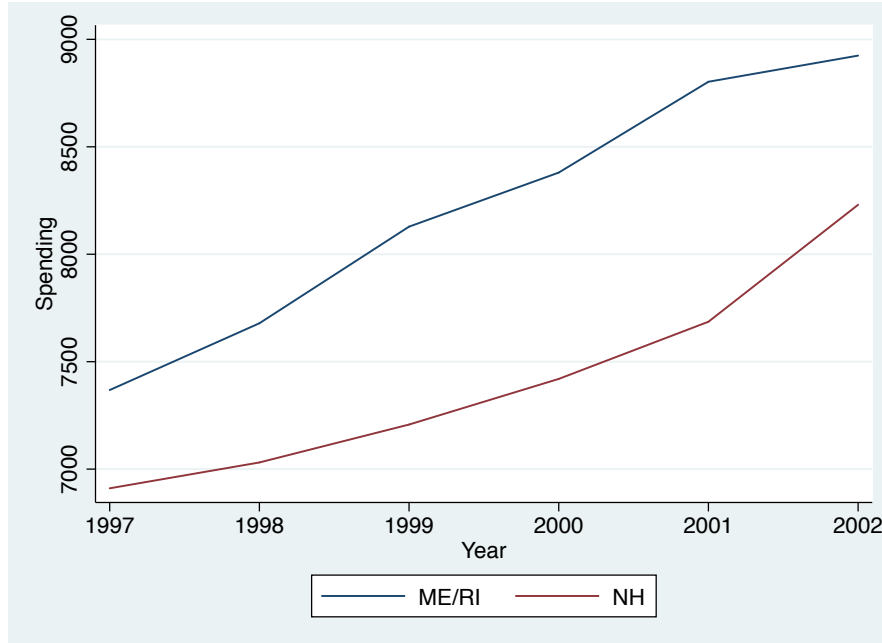


Figure 12 shows the change in the distribution of per pupil spending in New Hampshire. The green bar shows the distribution of per pupil spending in 1999 before the school finance reform. There existed much variation in per pupil spending among school districts. The lowest per pupil spending was \$4,613 in Milan School District while the highest per pupil spending was \$14,242 in Waterville Valley School District. The white bar shows that the distribution of per pupil spending in 2002, the third year after the school finance reform. As Figure 12 indicates, the variation in per pupil spending among school districts did not change much while the mean of per pupil spending among school districts increased. Also, Figure 12 shows that the distribution of per pupil spending was slightly right skewed. That means that the number of school districts spending higher was less than those with lower spending. This was the reason that I converted the variable into its logarithm in the following regression analysis, which was closer to normal distribution as shown in Figure 13.

Figure 12. The Histogram of per Pupil Spending in New Hampshire in 1999 and 2002

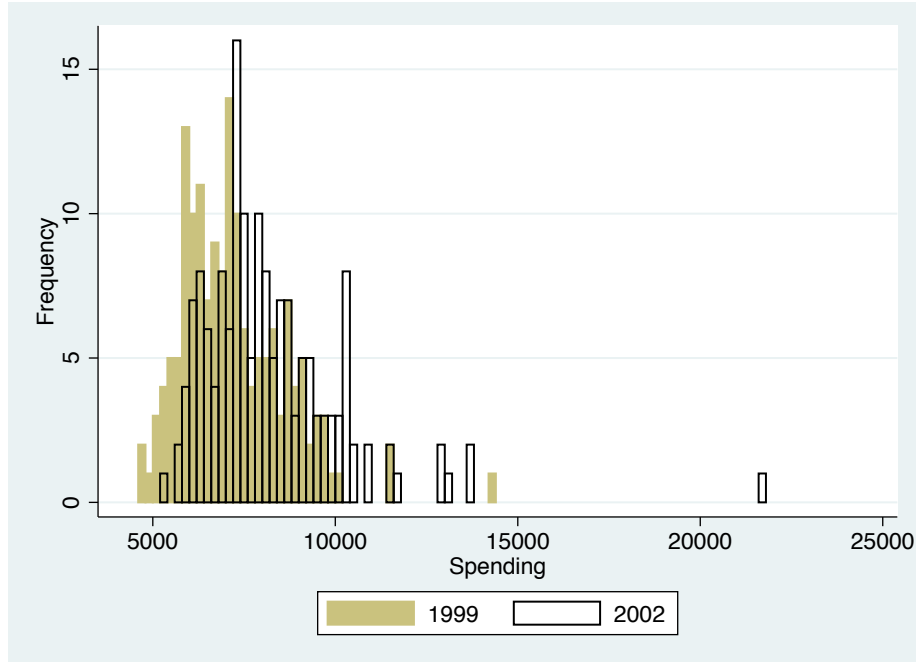
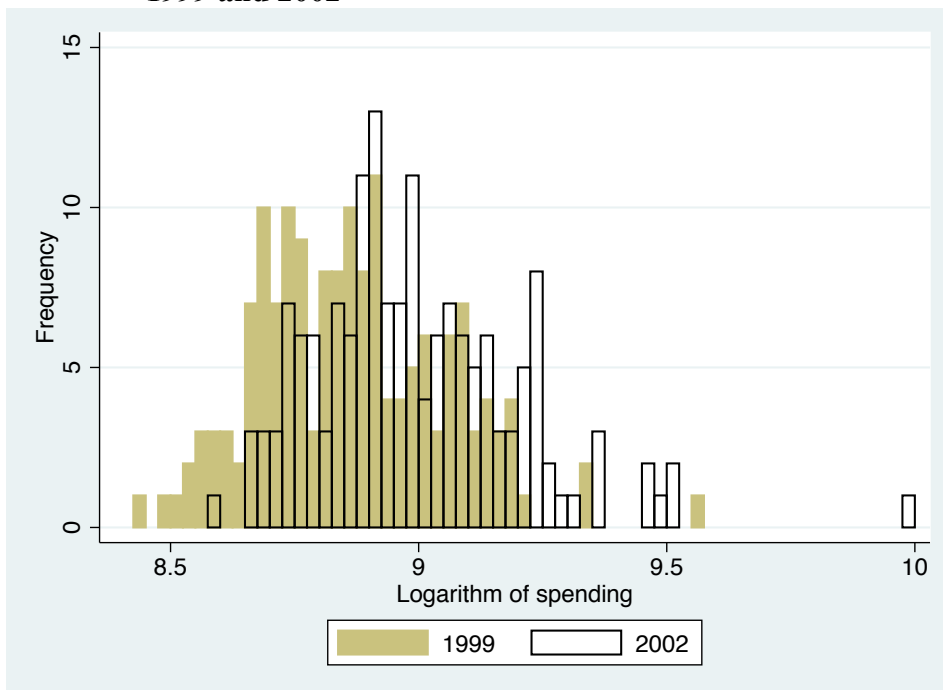


Figure 13. The Histogram of Logarithm of per Pupil Spending in New Hampshire in 1999 and 2002



Independent variable: enrollment of school districts. Table 14 shows the enrollment of school districts in the reform state of New Hampshire and the comparison

states of Maine and Rhode Island from 1997 to 2002. The Average enrollment of school districts stayed stable from 1997 to 2002 in Maine and Rhode Island as shown in Figure 14. For example, the average enrollment was 1,408 in 1997, and the average enrollment was 1,410 in 2002. In New Hampshire, the average enrollment increased by 48 in 1999, 21 in 2000, 20 in 2001, and decreased 5 in 2002.

Table 14: Descriptive Characteristics of Enrollment of School Districts in Reform and Comparison States: 1997-2002

Year	Comparison State: ME/RI			Reform State: NH		
	Mean	SD	Frequency	Mean	SD	Frequency
1997	1408	2242	258	1199	1910	160
1998	1411	2293	258	1204	1932	160
1999	1409	2319	258	1231	1980	160
2000	1416	2327	258	1242	2001	160
2001	1412	2352	258	1253	2021	160
2002	1407	2362	258	1242	2016	160
Total	1410	2312	1548	1229	1972	960

Figure 15 shows the distribution of enrollment of school districts in New Hampshire. The green bar shows the distribution of enrollment in 1999. The white bar with black border shows the distribution of enrollment in 2002. It shows that there was huge variation in enrollment among school districts. For example, the enrollment in Landaff School District was 20 while it was 16,932 in Manchester School District. Also, the distribution was extremely right skewed. This means that a larger number of school districts had smaller enrollment. The distribution of enrollment of school districts did not change much from 1999 to 2002 because the distributions of the two years overlap in Figure 15. Since the distribution of enrollment was right skewed, I included its logarithm in my analytical models. The logarithm of enrollment was close to normally distributed as shown in Figure 16.

Figure 14. The Trend of Enrollment of School Districts in Reform and Comparison States: 1997-2002

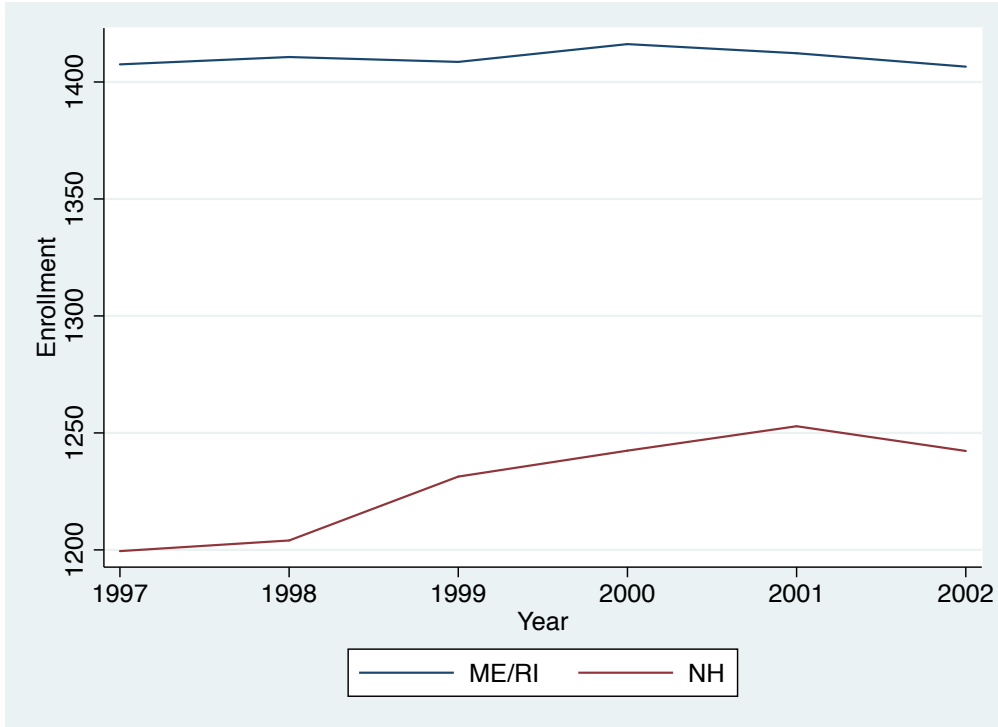


Figure 15. The Histogram of Enrollment of School Districts in New Hampshire in 1999 and 2002

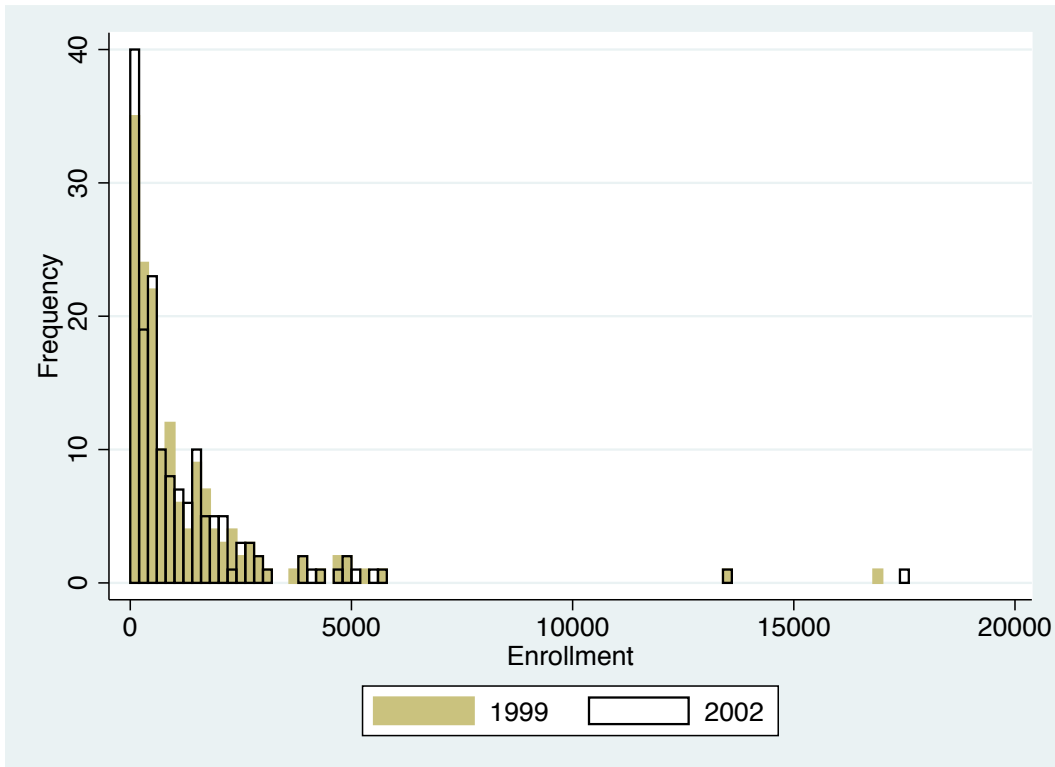
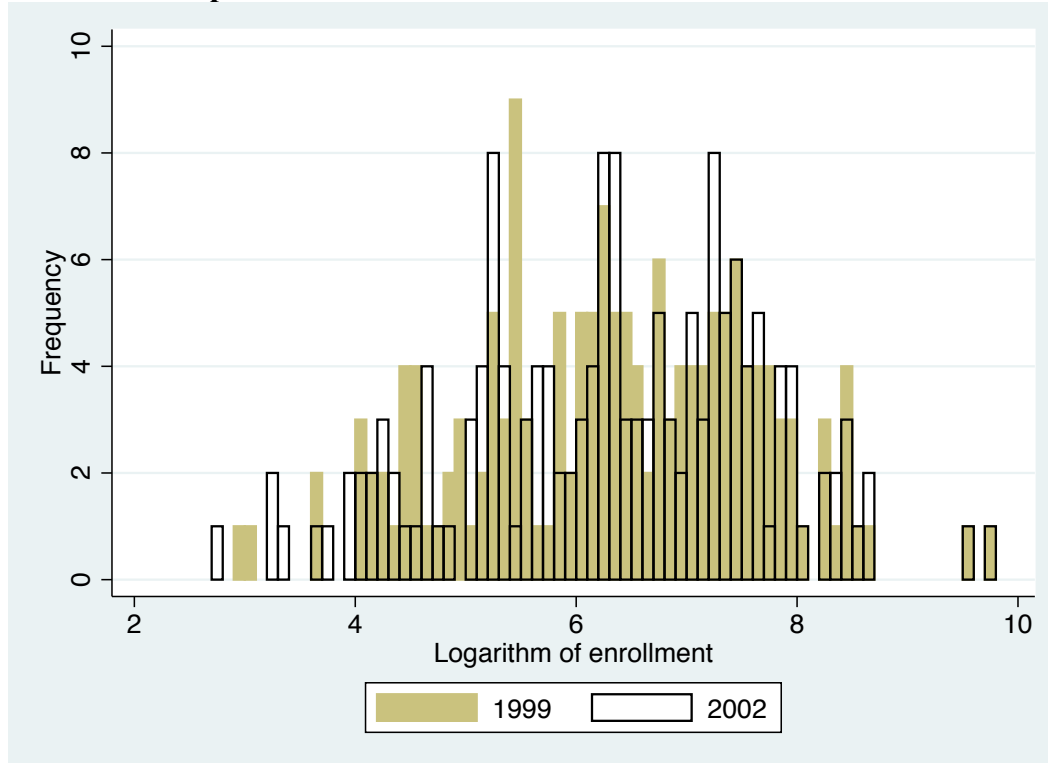


Figure 16. The Histogram of Logarithm of Enrollment of School Districts in New Hampshire in 1999 and 2002



Independent variable: percentage of students in free lunch program. Table 15 shows the descriptive characteristics of percentage of students in free lunch program in the reform state of New Hampshire and the comparison states of Maine and Rhode Island from 1997 to 2002. Table 4 shows that data on percentage of students in free lunch program was not balanced among years. The total number of school districts was 160 in New Hampshire, 258 in Maine and Rhode Island. There were some missing values for the variable in both reform state and comparison states. For example, there were 23 missing values for comparison states in 2000, and 45 missing values for reform state in 1999. This indicates that average percentage of students in free lunch program was not comparable across years, because they included different samples in different years.

Table 15: Descriptive Characteristics of Percentage of Students in Free Lunch Program in Reform and Comparison States: 1997-2002 (Unbalanced)

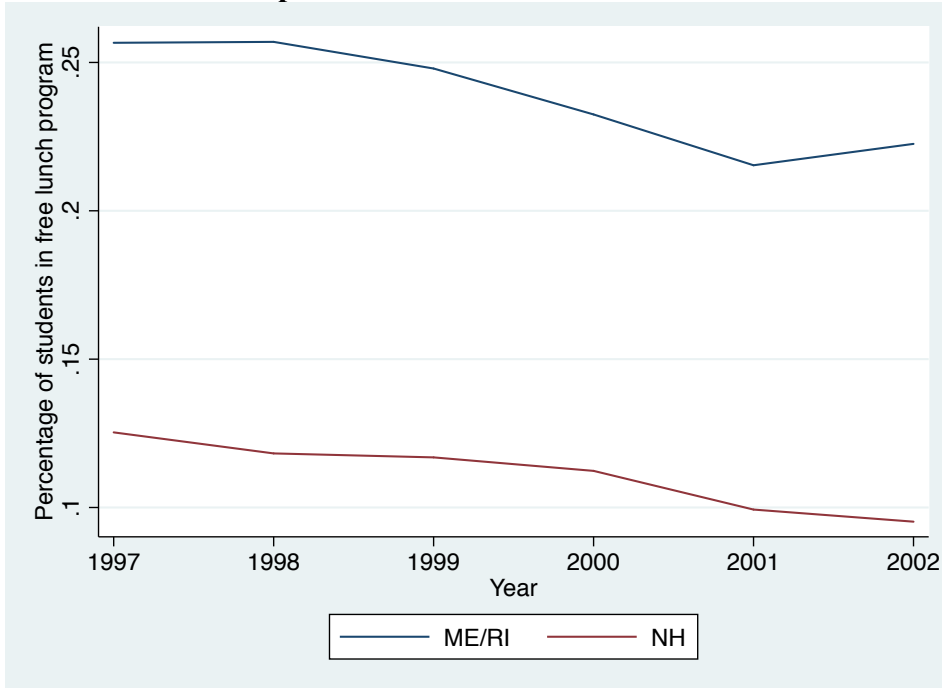
Year	Comparison State: ME/RI			Reform State: NH		
	Mean	SD	Frequency	Mean	SD	Frequency
1997	0.256	0.151	244	0.119	0.090	141
1998	0.256	0.140	241	0.118	0.096	147
1999	0.248	0.138	245	0.118	0.100	115
2000	0.231	0.157	235	0.115	0.082	160
2001	0.215	0.137	236	0.103	0.076	134
2002	0.224	0.148	251	0.100	0.078	157
Total	0.238	0.146	1452	0.112	0.087	854

Table 16 shows the descriptive characteristics of percentage of students in free lunch program in reform state New Hampshire and comparison states Maine and Rhode Island when school districts with missing value were excluded. In New Hampshire, the percentage of students in free lunch program was stable from 1997 to 1999 as shown in Figure 17. It began to decrease in 2000. It decreased by 0.8% from 2000 to 2001. In comparison states, percentage of students in free lunch program also did not change much from 1997 to 1999. It decreased by 1.7% in 2000, 1.6% in 2001, yet increased by 0.9% in 2002.

Table 16: Descriptive Characteristics of Percentage of Students in Free Lunch Program in Reform and Comparison States: 1997-2002 (Balanced)

Year	Comparison State: ME/RI			Reform State: NH		
	Mean	SD	Frequency	Mean	SD	Frequency
1997	0.257	0.152	228	0.125	0.092	104
1998	0.257	0.142	228	0.118	0.096	104
1999	0.248	0.141	228	0.117	0.096	104
2000	0.232	0.158	228	0.112	0.076	104
2001	0.215	0.137	228	0.099	0.071	104
2002	0.223	0.146	228	0.095	0.074	104
Total	0.239	0.147	1368	0.111	0.085	624

Figure 17. The Trend of Percentage of Students in Free Lunch Program in Reform and Comparison States: 1997-2002



Correlation of all variables. Table 17 shows the correlations of all variables for the analysis of the New Hampshire school finance reform. Data included school districts in New Hampshire, Maine, and Rhode Island from 1997 to 2002. The correlation coefficient between reform and per pupil spending was -0.024, and the relationship was weak. The correlation coefficient between per pupil spending and percentage of students in free lunch program was -.005. This was also a weak relationship. The correlation coefficient between per pupil spending and enrollment was -0.347. This was a modest negative correlation. It indicates that school districts with larger enrollments spent less on education. However, all these correlations were calculated without controlling for other variables, it cannot reveal true relations among these variable while it provided some suggestive relations among these variables. More advanced modeling was needed to understand the relation between school finance reforms and per pupil spending.

Table 17: Correlation of All Variables for New Hampshire

	Spending ^a	Freelunch ^b	Enrollment ^c	Reform ^d	Year
Spending	1.000				
Freelunch	-0.005	1.000			
Enrollment	-0.347	-0.114	1.000		
Reform	-0.024	-0.298	-0.024	1.000	
Year	0.260	-0.090	0.013	0.421	1.000

Note. Data included school districts in New Hampshire, Maine, and Rhode Island from 1997 to 2002.

^a logarithm of per pupil spending. ^b percentage of students with free lunch.

^c logarithm of enrollment. ^d dummy for reform state after reform

Difference in Difference Estimations

In order to know the causal effect of the school finance reform in New Hampshire on per pupil spending, a DID estimation was used in this study. It utilized a comparison group and pre-and post-data to reduce the bias from unknown variables. Year and school district fixed effect were also included in this model. To account for the possibility of serial correlation in the error term, standard errors were clustered at school district level. Also, in order to examine the effect of control variables, I ran separate models without and with control variables. Finally, it should be mentioned that the coefficient of independent variables in the following tables could not be interpreted directly because of the use of logarithm of per pupil spending, I interpret the coefficients by converting them into the percentage of increase or decrease in per pupil spending in following explanation.

The research questions of this study is to know the effect of the school finance reform in New Hampshire on per pupil spending in all school districts, poor, and rich school districts. Thus, I present results for all school districts, poor school districts, and rich school districts separately.

All school districts. Table 18 shows the DID estimations of the effect of the school finance reform in New Hampshire on per pupil spending for all school districts. Column 1 and 2 show the results from four years of data without and with control variables. The coefficients of the variable reform were -0.024 without control variables and -0.034 with control variables. Both coefficients were statistically significant at 1% significance level. The coefficient -0.024 and -0.034 means that the school finance reform decreased per pupil spending in New Hampshire by 2.4%⁵ and 3.3%. Since the inclusion of control variables could improve the precision of the DID estimation, the model with control variable was more trustworthy. Thus, considering that per pupil spending in New Hampshire in 1999 before the reform was \$7,207, the school finance reform in New Hampshire decreased per pupil spending by \$241 based on the estimation from four years of data.

⁵ The dependent variable was the natural logarithm of per pupil spending in this model, the coefficient of the variable reform could be converted into the percentage of increase or decrease in per pupil spending by a function of $e^{\alpha_1} - 1$ (e is Euler's number, α_1 is the coefficient of the variable reform). For the remaining portion of the dissertation, the coefficient of the variable reform was converted in the same way as this.

Table 18: DID Estimation of the Effect of the School Finance Reform in New Hampshire on per Pupil Spending in All School Districts with Comparison State Maine and Rhode Island

	Four Years of Data		Six Years of Data	
	No Control	Control	No Control	Control
	(1)	(2)	(3)	(4)
Reform	-0.024*** (0.009)	-0.034*** (0.009)	-0.021** (0.010)	-0.032*** (0.010)
1998.year			0.032*** (0.003)	0.031*** (0.003)
1999.year	0.041*** (0.004)	0.039*** (0.004)	0.074*** (0.005)	0.069*** (0.004)
2000.year	0.081*** (0.006)	0.091*** (0.007)	0.112*** (0.007)	0.124*** (0.008)
2001.year	0.125*** (0.007)	0.130*** (0.007)	0.156*** (0.007)	0.161*** (0.008)
2002.year			0.192*** (0.009)	0.192*** (0.008)
Logarithm of enrollment		-0.174*** (0.044)		-0.247*** (0.037)
Percentage of free lunch students		-0.174** (0.081)		-0.237*** (0.078)
Constant	8.890*** (0.003)	10.032*** (0.283)	8.858*** (0.004)	10.482*** (0.243)
Observations	1,672	1,513	2,508	2,306
R ²	0.309	0.381	0.395	0.481
Number of id	418	407	418	411

Note. School district level fixed effect is included in each model above. Robust standard errors in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

Column 3 and 4 show the DID estimation from six years of data without and with control variables. The coefficients of variable reform were -0.021 without control variables and -0.032 with control variables. Both coefficients were statistically significant at 5% and 1% significance level respectively. The coefficient -0.021 and -0.032 mean

that the school finance reform decreased per pupil spending in New Hampshire by 2.1% and 3.2%. Also, since the inclusion of control variables could improve the precision of the DID estimations, the model with control variables was more trustworthy. Thus, considering that per pupil spending in New Hampshire in 1999 before the reform was \$7,207, the school finance reform in New Hampshire decreased per pupil spending by \$227 based on the estimation from six years of data. This result also indicates that the DID estimations of the effect of the reform from four and six years of data did not show much difference.

The coefficients of the logarithm of enrollment were -0.174 with four years of data and -0.247 with six years of data. Both were significant at 1%. This negative relation indicates that per pupil spending decreased 0.17% or 0.25% if enrollment increased by 1%. Average per pupil spending was \$7,207 in 1999, and average enrollment was 1,231 in 1999. This means that about 12 students increase reduced per pupil spending by \$12 or \$18.

The coefficients of the percentage of students in free lunch program were -0.174 with four years of data and -0.237 with six years of data. Both were significant at 5% and 1%, respectively. This negative relation indicates that per pupil spending decreased 0.17%⁶ or 0.23% if percentage of students in free lunch program increased by 1%. Average per pupil spending was \$7,207 in 1999 in New Hampshire. This means that one percent increase of students in free lunch program students reduced per pupil spending by \$13 or \$17.

⁶ Since the dependent variable is logarithm of per pupil spending, this number is calculated by a function $(e^{0.01*\gamma} - 1)$ where e is Euler's number, γ is the coefficient of the percentage of students in free lunch program. For the remaining portion of the dissertation, the coefficient of the variable percentage of students in free lunch program was converted in the same way as this.

Poor school districts. Table 19 shows the DID estimation of the school finance reform in New Hampshire on per pupil spending for poor school districts whose average per pupil spending before the school finance reform belonged to the lowest third quantile. Column 1 and 2 show the results from four years of data without and with control variables. The coefficients of the variable reform were -0.014 without control variables and -0.012 with control variables. However, both coefficients were not statistically significant at even 10% significance level. Column 3 and 4 shows the results from six years of data without and with control variables. The coefficients of reform variable were -0.012 without control variable and -0.004 with control variables. Both coefficients were also not statistically significant at 10% significance level. This indicates that the school finance reform did not have a significant effect on per pupil spending in poor school districts.

Table 19: DID Estimation of the Effect of the School Finance Reform in New Hampshire on per Pupil Spending in Poor School Districts with Comparison State Maine and Rhode Island

	Four Years of Data		Six Years of Data	
	No Control	Control	No Control	Control
	(1)	(2)	(3)	(4)
Reform	-0.014 (0.015)	-0.012 (0.015)	-0.012 (0.017)	-0.004 (0.016)
1998			0.031*** (0.005)	0.032*** (0.005)
1999	0.053*** (0.006)	0.050*** (0.006)	0.081*** (0.007)	0.077*** (0.007)
2000	0.095*** (0.009)	0.092*** (0.009)	0.123*** (0.010)	0.118*** (0.010)
2001	0.150*** (0.010)	0.144*** (0.010)	0.175*** (0.011)	0.165*** (0.011)
2002			0.241*** (0.014)	0.223*** (0.011)
Logarithm of enrollment		-0.142** (0.057)		-0.213*** (0.053)
Percentage of free lunch students		-0.110 (0.066)		-0.202** (0.079)
Constant	8.708*** (0.006)	9.659*** (0.373)	8.677*** (0.006)	10.130*** (0.351)
Observations	560	502	840	766
R ²	0.509	0.550	0.596	0.677
Number of id	140	138	140	139

Note. School district level fixed effect is included in each model above. Robust standard errors in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

The coefficients of the logarithm of enrollment were -0.142 with four years of data and -0.213 with six years of data. Both were significant at 5% and 1%, respectively. This negative relation indicates that per pupil spending decreased 0.14% or 0.21% if enrollment increased by 1%. Average per pupil spending was \$7,207, and average

enrollment was 1,231 in 1999 in New Hampshire. This means that about 12 students increase reduced per pupil spending by \$10 or \$15.

The coefficients of percentage of students in free lunch program were -0.110 with four years of data and -0.202 with six years of data. Only the one with six years of data was significant at 5%. Considering the result from six years of data, it indicates that per pupil spending decreased by 0.2% if percentage of students in free lunch program increased by 1%. Since the average per pupil spending was \$7,207 in 1999, this means that one percent increase of students in free lunch program students reduced per pupil spending by \$14.

Rich School Districts. Table 20 shows the results of the DID estimation of the school finance reform in New Hampshire on per pupil spending for rich school districts whose average per pupil spending before the school finance reform belonged to the highest one third. Column 1 and 2 show the results from four years of data without and with control variables. The coefficients of the variable reform were -0.034 without control variable and -0.063 with control variables. The former coefficient was only significant at 10 % significance level while the latter one was significant at 1%. The coefficient -0.034 and -0.063 means that the school finance reform decreased per pupil spending in rich school districts in New Hampshire by 3.3% and 6.1%. Since the inclusion of control variables could improve the precision of the DID estimation, the model with control variable was more trustworthy. Thus, considering that per pupil spending in New Hampshire in 1999 before the reform was \$7,207, the school finance reform in New Hampshire decreased per pupil spending by \$440 based on the estimation from four years of data.

Table 20: DID Estimation of the Effect of the School Finance Reform in New Hampshire on per Pupil Spending in Rich School Districts with Comparison State Maine and Rhode Island

	Four Years of Data		Six Years of Data	
	No Control	Control	No Control	Control
	(1)	(2)	(3)	(4)
Reform	-0.034*	-0.063***	-0.024	-0.066***
	(0.017)	(0.019)	(0.020)	(0.024)
1998			0.033***	0.030***
			(0.008)	(0.007)
1999	0.033***	0.028***	0.069***	0.062***
	(0.010)	(0.007)	(0.013)	(0.010)
2000	0.068***	0.099***	0.100***	0.139***
	(0.015)	(0.019)	(0.016)	(0.022)
2001	0.108***	0.129***	0.133***	0.162***
	(0.014)	(0.020)	(0.016)	(0.023)
2002			0.124***	0.148***
			(0.017)	(0.023)
Logarithm of enrollment		-0.235***		-0.279***
		(0.071)		(0.067)
Percentage of free lunch students		-0.346**		-0.272*
		(0.167)		(0.147)
Constant	9.103***	10.572***	9.075***	10.766***
	(0.007)	(0.435)	(0.009)	(0.404)
Observations	556	510	834	765
R ²	0.160	0.296	0.166	0.281
Number of id	139	134	139	134

Note. School district level fixed effect is included in each model above. Robust standard errors in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

Column 3 and 4 show the results from six years of data without and with control variables. The coefficients of variable reform were -0.024 without control variables and -0.066 with control variables. The former coefficient was not significant while the latter one was significant at 1%. The coefficient -0.066 means that the school finance reform decreased per pupil spending in New Hampshire by 6.4%. Based on per pupil spending

\$7,207 in New Hampshire in 1999 before the reform, that means that the school finance reform decreased per pupil spending in rich school districts by \$460. This result indicates that the DID estimation of the effect of the reform from four and six years of data did not show much difference.

The coefficients of the logarithm of enrollment were -0.235 with four years of data and -0.279 with six years of data. Both were significant at significance level 1%. This negative relation indicates that per pupil spending decreased 0.24% or 0.28% if enrollment increased by 1%. Average per pupil spending was \$7,207, and average enrollment was 1,231 in 1999 in New Hampshire. This means that about 12 students increase reduced per pupil spending in rich school districts by \$17 or \$20.

The coefficients of percentage of students in free lunch program were -0.346 with four years of data and -0.272 with six years of data. The coefficient with four years of data was significant at 5%. The one with six years of data was significant at 10%. Considering the result from the four years of data, it indicates that per pupil spending decreased 0.3% if percentage of students in free lunch program increased by 1%. Average per pupil spending was \$7,207 in 1999. This means that one percent increase of students in free lunch program students reduced per pupil spending in rich school districts by \$22.

Robustness Check

In this section I present the results for robustness checks of the DID estimations of the effect of the school finance reform in New Hampshire. There were two robustness checks. The first was to average four year and six years into two time point data: pre- and post-reform data. The second was to use data on school districts in Maine and Rhode

Island individually instead of combining them. I show these results in the sequence of all school districts, poor school districts, and rich school districts. I only interpret the DID estimation of the effect of the school finance reform. The coefficient of control variables and information on R square and sample size are shown in Table 1-9 in Appendix 2.

All school districts. Table 21 shows the results for robustness check of the estimation of the effect of the school finance reforms on per pupil spending for all school districts in New Hampshire. In order to make comparison, the DID estimations of the effect of the school finance from the model using multiple years data and a combination of comparison states that I show above are included in Row 1.

Table 21: The Results of Robustness Check of DID Models for All School Districts in New Hampshire

Model		Four Years of Data (1)	Six Years of Data (2)
Multiple year data	(1)	-0.032*** (0.010)	-0.034*** (0.009)
Averaged year data	(2)	-0.028*** (0.010)	-0.020* (0.011)
With Maine	(3)	-0.039*** (0.009)	-0.039*** (0.011)
With Rhode Island	(4)	-0.005 (0.010)	0.011 (0.012)

Note. Robust standard errors in parentheses
 *** p<0.01, ** p<0.05, * p<0.1

Row 2 shows the results from the models using averaged four-year data. The DID estimations of the effect of the school finance reform in New Hampshire were -0.024 without control variables and - 0.0208 with control variables. Both coefficients were statistically significant at 1%. For the models with six years of data, the DID estimations of the effect of the school finance reform in New Hampshire were -0.020 without and with control variables. The one without control variables was statistically significant at 1%

significance level while the one with control variables was only significant at 10% significance level. Since the inclusion of control variables could improve the precision of the DID estimation, the model with control variable was more trustworthy. Comparing to the results from the model with multiple years data, the result from averaged four years of data (-0.028) was a little smaller than the effect estimated by using multiple four years (-0.032). For six years of data, the effect size estimated by averaged data (-0.020) was smaller than the one from multiple year data (-0.034). The significance level was also weaker for the model with averaged data (10%) than the model with multiple year data (1%).

Row 3 shows the results when only school districts in Maine were used as a comparison group. The direction and significance of the DID estimation were the same as the model using a combination of school districts in Maine and Rhode Island. The effect size was a little bigger (-0.039, for four years of data, -0.039 for six years of data) for the models with one comparison state Maine than the ones for the models with combined comparison states (-0.034, for four years of data, -0.032 for six years of data). Row 4 shows the results when only school districts in Rhode Island were used as a comparison group. These results from both four and six years of data were not statistically significant. The possible reason for the non-significance was that the sample size of the comparison state Rhode Island was small. There were 36 school districts in Rhode Island while there were 160 school districts in New Hampshire.

In sum, the results of the models with multiple years data and a combination of two comparison states were robust to the averaged data models and the models with one comparison state in which the number of school districts was not small. With different

models, The DID estimation was from 0.020 to 0.039. That means that the school finance reform in New Hampshire decreased per pupil spending in all school districts by 2.0% to 3.8%.

Poor school districts. Table 22 shows the results for robustness check of the DID estimation of the effect of the school finance reform in New Hampshire on per pupil spending for poor school districts. With control variables, both the models with averaged data and the models with individual comparison state reveal that the school finance reform in New Hampshire had no significant effect on per pupil spending in poor school districts. These results were consistent with the results from the models with multiple years of data and a combination of two comparison states.

Table 22: The Results of Robustness Check of DID Models for Poor School Districts in New Hampshire

Model		Four Years of Data (1)	Six Years of Data (2)
Multiple year data	(1)	-0.012 (0.015)	-0.004 (0.016)
Averaged year data	(2)	-0.017 (0.015)	-0.005 (0.019)
With Maine	(3)	-0.018 (0.016)	-0.012 (0.016)
With Rhode Island	(4)	0.016 (0.018)	0.034 (0.021)

Note. Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Rich school districts. Table 23 shows the results for robustness check of the estimation of the effect of the school finance reforms for rich school districts in New Hampshire. Row 2 shows the results from the models using averaged data. For the models with four years of data, the DID estimations of the effect of the school finance reform in New Hampshire were -0.031 without control variables and - 0.063 with control

variables. The former coefficient was statistically significant at 10% level, and the latter coefficient was only significant at 1% significance level. For the models with six years of data, the DID estimations of the effect of the school finance reform in New Hampshire were -0.018 without control variables and - 0.047 with control variables. The former coefficient was not statistically significant at 10% level, and the latter coefficient was only significant at 1% significance level. Since the inclusion of control variables could improve the precision of the DID estimation, the model with control variable was more trustworthy. Comparing to the results from the model with multiple years data, the results from averaged four years of data (-0.063) was the same as the effect estimated by using multiple four years (-0.063). For six years of data, the effect size estimated by averaged data (-0.047) was smaller than the one from multiple years data (-0.066). The significance was weaker for the model with averaged data (10%) than the model with multiple year data (1%).

Table 23: The Results of Robustness Check of DID Models for Rich School Districts in New Hampshire

Model		Four Years of Data (1)	Six Years of Data (2)
Multiple year data	(1)	-0.063*** (0.019)	-0.066*** (0.024)
Averaged year data	(2)	-0.063*** (0.022)	-0.047* (0.026)
With Maine	(3)	-0.068*** (0.023)	-0.073*** (0.027)
With Rhode Island	(4)	-0.024 (0.020)	-0.011 (0.024)

Note. Robust standard errors in parentheses
 *** p<0.01, ** p<0.05, * p<0.1

Row 3 shows the results when only school districts in Maine were used as a comparison group. The direction and significance of the estimation of the effect of the

reform were the same as the model using a combination of school districts in Maine and Rhode Island. The effect size was a little bigger (-0.068 for four years of data, -0.073 for six years of data) from the models with one comparison state Maine than those from the models with combined comparison states (-0.063 for four years of data, -0.066 for six years of data). Row 4 shows the results when only school districts in Rhode Island were used as a comparison group. These results from both four and six years of data were not statistically significant. The possible reason for the non-significance was that the sample size of comparison state Rhode was small. There were 12 school districts in Rhode Island while there were 53 school districts in New Hampshire.

Overall, the results of the models with multiple years of data and a combination of two comparison states for rich school districts were robust to the models with averaged data and the models with one comparison state in which the number of school districts was not small. The DID coefficient was from -0.047 to -0.073. This indicates that the school finance reform in New Hampshire decreased per pupil spending by 4.6% to 7.0%.

CHAPTER 5

Introduction

In this chapter I first overview the research questions, objectives, theoretical framework, and research design of this study. Second, I answer the research questions by interpreting the empirical results reported in Chapter 4. Third, I describe contributions of this study to school finance studies and policy. Forth, the limitations of this study and implications for further studies are presented. Lastly, I summarize conclusions of this study.

Overview of This Study

The purpose of this study was to evaluate the effect of the school finance reforms in Vermont and New Hampshire on per pupil spending, addressing the following three research questions:

1. Did the school finance reforms in Vermont and New Hampshire change per pupil spending in all school districts?
2. Did the school finance reforms in Vermont and New Hampshire change per pupil spending in poor school districts?
3. Did the school finance reforms in Vermont and New Hampshire change per pupil spending in rich school districts?

Two reasons motivated me to conduct this study. First, the school finance reforms in both Vermont and New Hampshire involved state governments recapturing local property revenue from rich school districts and distributing it to poor school districts in order to achieve equity in educational spending. This measure was politically unpopular because it induced strong opposition from rich school districts (Reschovsky, 1994). It is

unclear on whether rich school districts decreased spending on education or increased tax effort to substitute the loss. Second, the school finance reforms in Vermont and New Hampshire included the measure of increasing state aid to poor school districts. The form of direct democracy in budget decision procedure in Vermont and New Hampshire and the lack of the restraint on the use of state aid might not guarantee the increase in per pupil spending of poor school districts since they could use this aid for other purposes such as relief of property tax burden (Bradford & Oates, 1971; Card & Payne, 2002; Fisher & Papke, 2000).

By utilizing the concept of tax price in public finance literature and the economic theory on the effect of intergovernmental grant (Bergstrom & Goodman, 1973; Bradford & Oates, 1971; Denzau & Grier, 1984; Hoxby, 2001), this study hypothesizes that the school finance reform in Vermont had a negative effect on per pupil spending in rich school districts because of the increase of tax price and the loss of local property tax revenue. On the contrary, it had a positive effect on per pupil spending in poor school districts because of the decrease in tax price for them. Similarly, the school finance reform in New Hampshire would also have a negative effect on per pupil spending in rich school districts because of the loss of local property revenue while it had no significant effect on per pupil spending in poor school districts because there was no restraint on the use of the state aid although state aid to poor school districts increased.

Methodologically, this study utilizes a Difference in Difference (DID) model to analyze school district level expenditure data derived from the Common Core of Data (CCD) from the National Center for Education Statistics (NCES). By using comparison state Maine and Rhode Island and pre-and-post reform data, the DID design could reduce

the bias from factors that occurred during the targeted period and influenced per pupil spending in both treatment and comparison group. In order to control the influence of time invariant factors of school districts on the estimation of the effect of school finance reforms, fixed effect of school districts was included in the statistical models that this study uses. The dependent variable was per pupil spending of school districts. The key variable of this study was a dummy variable that indicates whether school districts went through the reformed school finance systems. The enrollment and percentage of students in free lunch program were included in the models as control variables.

Although the school finance reforms of both Vermont and New Hampshire shared many commonalities such as the establishment of statewide property tax and the recapture proposition, the finance formula that they adopted were different. School finance system in Vermont was a combination of foundation system and power equalizing system. School finance system in New Hampshire was a foundation system. Therefore, this study evaluated the effect of school finance reform in both states separately. Finally, alternative specifications of statistical models were tested in order to check the robustness of the statistical strategies that this study uses.

Discussion of Research Findings

Research Question One

The first research question of this study is whether the school finance reforms in Vermont and New Hampshire changed average per pupil spending in all school districts. The results from this study show that the school finance reform in Vermont did not increase average per pupil spending of all school districts. The DID estimation shows that the school finance reform in New Hampshire decreased average per pupil spending in all

school districts by 2% to 4%. Why did the school finance reform in Vermont not increase per pupil spending while the school finance reform in New Hampshire decreased per pupil spending? The results of the estimation of the effect of the school finance reforms in Vermont and New Hampshire on per pupil spending in school districts with different spending level provides clues to answer this question. This study shows that the school finance reform decreased per pupil spending in rich school districts and increased per pupil spending in poor school districts. It is reasonable that the school finance reform in Vermont did not increase average per pupil spending in all school districts. This study also shows that the school finance reform in New Hampshire had no effect on per pupil spending in poor school districts and decreased per pupil spending in rich school districts. This was the reason that the school finance reform in New Hampshire decreased average per pupil spending in all school districts.

These results add up further evidence to the literature that school finance reforms could either level down or level up per pupil spending depending on the different measures that the reforms took. Since 1970s, many states initiated school finance reforms characterized by the increased role of state government in deciding school budgets (Loeb, 2001; Murray et al., 1998), the school finance reforms in Vermont and New Hampshire also increased state share of total education revenue. There is no consensus regarding the effect of these reforms. For example, the school finance reform in California in 1978 and the finance reform in Washington in 1977 decreased average per pupil spending while the school finance reform in Michigan in 1994 and the reform in Massachusetts in 1994 increased average per pupil spending (Chaudhary, 2009; Dee & Levine, 2004; Downes, 1992; Manwaring & Sheffrin, 1997). However, since the motivation of this trend of

school finance reforms was to increase per pupil spending in poor school district and decrease the inequity in per pupil spending among school districts, instead of focusing on the average per pupil spending of all school districts, it is more important to know the effect of school finance reforms on per pupil spending for school districts with different spending levels. These are discussed in the following section.

Research Question Two

The second research question is whether the school finance reforms in Vermont and New Hampshire changed per pupil spending in poor school districts. In comparison to school districts of Maine and Rhode where there was no school finance reform in the same period, the results from this study show that the school finance reform in Vermont increased per pupil spending in poor school districts by from 2% to 5%, and the school finance reform in New Hampshire had no effect on per pupil spending in poor school districts. It is important to analyze the important measures in these two school finance reforms into order to explain the results.

The school finance reform in New Hampshire increased state aid to poor school districts because of the input of new state general revenue and recaptured property tax revenue from rich school districts. Why did this increased state aid not increase per pupil spending? Two potential mechanisms could explain the results. First, poor school districts spent the increased aid on education, yet reduced their property tax efforts as they should if there were no school finance reform. Second, poor school districts kept the same level of property tax effort, but they spent a portion of the increased aid on other public services such as transportation or police if they did not spend it on education and tax relief.

In Lutz's (2010) study of the relationship between state aid and local revenue in New Hampshire after the school finance reform, she found that approximately ninety cents per grant dollar were spent on tax reduction in New Hampshire after the school finance reform. This empirical evidence also confirms the equivalence assumption in economic theory. The equivalence assumption indicates that local residents treat state aid the same as their personal income increase if local budget decision could represent the preference of local residents (Bradford & Oates, 1971). Thus local residents spend only a small portion of the grant aid on education as they spend their personal income increase on education. This equivalence assumption is applicable to the school finance reform in New Hampshire because the budget decision procedure represented the preference of local residents in New Hampshire. Budget proposal was annually decided in a town meeting in most of school districts in New Hampshire. Also, there was no limit on tax rate or spending limit on education in New Hampshire. School districts were free to use state grant. Therefore, the school finance in New Hampshire did not increase per pupil spending in poor school districts because local residents used the increased state aid to relieve their property tax burden instead of increasing educational expenditure.

The school finance reform in Vermont increased the state share of public school funding from 28% to 69%. Since the part of the increase was due to the renaming of previous local property tax as the statewide property tax, the increased ratio of state aid did not certainly indicate the increase of state aid. However, it did increase the state aid to poor school districts for the following reason. The new school finance system in Vermont was a combination of foundation system and power equalizing system. Each student received the same amount of foundation grants on the condition that school districts levy

a state property tax at the uniform rate. Since property tax base in rich school districts was higher than in poor school districts, the foundation part was actually a way of the distribution of a portion of property tax revenue from rich school districts to poor school districts. Compared to state aid before the reform, total state aid for poor school districts increased because the external funding from rich school districts was input and total pro-reform state aid did not decrease.

For power equalizing part of Vermont school finance system, if school districts were willing to spend more than the amount of foundation block grant they had to levy local property tax for its extra spending. The power equalizing system guaranteed that equal tax rate produced equal revenue. The guaranteed revenue was exclusively from the excess property revenue of all school districts which were willing to spend more than the amount of foundation grant. This means that property poor school districts could get external funding from property rich school districts if they decided to spend more on education than the foundation grant. In term of tax price, this means that the tax price for poor school districts decreased into less than one. The decrease of tax price could provide incentive for poor school districts to spend more on education. Therefore, either the increase of state aid or the decrease in tax price could be a reason for the increase in per pupil spending in poor school districts.

Which one of the changes led to the increase in per pupil spending in poor school districts in Vermont? The specification of this study cannot answer this question. However, the result on the effect of the school the finance reform in New Hampshire might provide a clue to answer this question, the reform in New Hampshire also increased state aid to poor school districts. But the increased state aid did not increase per

pupil spending in poor school districts. School districts used the increased state aid to relieve local property tax burden because the budget decision procedure could represent the preference of local residents in New Hampshire (Lutz, 2010). The budget decision procedure in Vermont was similar to the one in New Hampshire. Both states did not have tax rate or expenditure limit. They were free to use the increased aid. Therefore, it is possible that the increased state aid in Vermont also did not increase per pupil spending. Thus, the increase in per pupil spending in poor school district in Vermont is probably due to the decrease in tax price. Also, many studies show that tax price was negatively related to local public spending (Denzau & Grier, 1984), or specifically, education spending (Hoxby, 2001). However, this is one hypothesis that needs further empirical evidence.

Research Question Three

The third question of this study is whether the school finance reform in Vermont and New Hampshire changed per pupil spending in rich school districts. The DID estimation shows that the school finance reform in Vermont decreased per pupil spending in rich school districts by 4% to 10%, and the school finance reform in New Hampshire decreased per pupil spending in rich school districts by 5% to 7%. What caused the decrease in per pupil spending in rich school districts in both Vermont and New Hampshire? I provide some explanations in the following section by analyzing the change in the school finance system in both states.

The school finance reform in Vermont produced two changes for rich school districts. First, because of the establishment of statewide property tax they lost a portion of property tax revenue, the loss of property tax revenue could be one reason for the

decrease in per pupil spending in rich school districts. Second, tax price for rich school districts changed from one into more than one after the school finance reform. They need to levy more than one dollar if they want to spend one more dollar on education than the foundation block grant. Schmidt and Scott (2006) analyzed tax price for each school district after the school finance reform in Vermont and found that 125 out of 253 towns paid more than \$1 to increase local education spending by \$1. Tax price in 37 towns was more than \$2. Tax price in nine towns exceeded \$5. School districts with high tax price indicate that they were also school districts with high spending. 76 school districts whose spending was at the top third of all school districts in Vermont were included as rich school districts in this study. Therefore, the tax price in these school districts was more than one.

The economic theory predicts that the increase in tax price could discourage the demand of public service such as education (Bergstrom & Goodman, 1973; Hoxby, 2001; Olmsted, Denzau, & Roberts, 1993). Thus, it is probable that both the loss of the property tax revenue and the increase of tax price for rich school districts made contribution to the decrease in per pupil spending in rich school districts. No wonder that Vermont changed the power equalization part of its finance system, instead of recapturing the property tax revenue from rich school districts, the state used an increase in sales tax to guarantee that equal property tax rate produce equal revenue in 2004.

The reason for the decrease in per pupil spending in New Hampshire is straightforward because of the recapture proposition in the school finance reform. This proposition recaptured the difference of the statewide property tax revenue and the amount of adequate grant determined by the state. This result explains the motivation of

the strong opposition to the new school finance system, especially the recapture proposition, and the later development of the school finance system in New Hampshire. In 2001, the second year after the 1999 school finance reform, the statewide property tax rate was reduced to \$5.80 per \$1,000 from \$6.60 per \$1,000 of property value. In 2003, the New Hampshire legislature adopted HB 608 which reduced the statewide property tax rate from \$5.80 to \$4.92 for fiscal year 2004 and further down to \$3.24 for FY 2005.

The reason for the gradual decrease in statewide property tax rate is that the statewide property tax rate was related to the amount of recaptured local property revenue. The amount of recaptured property revenue was the difference of a production of statewide property tax rate and local property value and the state decided adequate grant. The less the statewide property tax rate was, the less the amount of recaptured property revenue was. Finally, in 2005, the HB 616 was passed that allowed all towns to keep the excess statewide property tax revenue on the condition that the revenue was spent on education. There were only two towns whose excess statewide property tax revenue was still recaptured.

The reformed school finance system in Vermont and New Hampshire were different. For Vermont, rich school districts not only lost a portion of property tax revenue if the property revenue at statewide property tax rate was more than a state decided foundation grant, but also lost a portion of property tax revenue if they want to spend more than the foundation grant. The first portion was decided by the state government because the amount of the foundation grant and the statewide property tax rate was decided by state government. The second part depended on whether they were willing to spending more than the foundation grant. On the contrary, for New Hampshire,

the school finance reform was a foundation program. Rich school districts only gave a difference of property tax revenue at statewide property tax rate and the adequate grant. They would not lose more if they want to spend more than the adequate grant.

However, the school finance reforms in both Vermont and New Hampshire produce the similar negative effect on per pupil spending in rich school districts. This indicates that the difference in measures of school finance reform in Vermont and New Hampshire did not have different effect on per pupil spending in rich school districts. This is probably because rich school districts in both state lost a portion of property tax revenue. However, there is a difference in the way how this policy worked between Vermont and New Hampshire. Rich school districts in Vermont spent less not only because they lost a portion of property tax revenue but also if they spent more they would lose more. Rich school districts in New Hampshire spent less only because they lost a portion of property tax revenue that would be used on education.

Implications for Studies on School Finance and Reform

Contribution to Studies on School Finance

First, this study highlights the importance of the conclusion that school finance reforms had different effects on per pupil spending in school districts with different spending levels. The school finance reform in Vermont did not increase average per pupil spending of all school districts, but it increased per pupil spending in school districts with lower pre-reform spending. Also, the school finance reform in New Hampshire decreased average per pupil spending of all school districts while it did not change per pupil spending in school districts with low pre-reform spending.

The heterogeneity of the effect of school finance reform on different school districts was noticed by some existing studies. For example, by using state level data, Murray et al., (1998) showed that the school finance reform increase spending of poor school districts with spending at 5 percentile, and did not change spending of rich school district with spending at 95 percentile. By including an interaction term of the variable school finance reform and median income of school districts, Manwaring and Sheffrin (1997) showed that school finance reforms increased per pupil spending less for school district with higher income than school districts with lower income. Dee and Levine (2004) also showed that the school finance reform in Massachusetts increased per pupil spending more for poor school districts and rich school districts. Thus, this study with other studies placed some questions on the studies which assumed that school finance reform have a homogenous effect on all school districts (e.g. Chaudhary, 2009). The heterogeneity of the effect of school finance reform on different school districts contributed to our understanding school finance litigations and reforms. On surface, the school finance litigations are the conflicts between school districts and state governments, and conflicts between judicial branch and legislature branch in state government. However, the school finance reform is always about the inequity of income of residents. The nature of school finance reforms is to adjust the influence of income inequity on education spending among school districts.

Second, regarding the effect of lump sum intergovernmental grant on local public spending such as education spending, there is an inconsistency between theoretical prediction and empirical evidence. In theory, the effect of lump sum intergovernmental grant on local public spending is equivalent to income increase of voters in local

jurisdiction (Bradford & Oates, 1971). Since voters usually spend 5 to 10% of their income increase on education (Hines & Thalar, 1995; Romer & Rosenthal, 1982), it was expected that lump sum grant would produce the same magnitude of increase in local public service such as education spending. However, many empirical studies showed that school districts spent more out of intergovernmental grant on education than the theory predicted (Fisher & Papke, 2000; Hines & Thalar, 1995). This was called flypaper effect. The explanations for this included voter's misperception and the bureaucratic manipulation (Fisher, 1982). Hines and Thalar (1995) argued for the behavioral explanation, for example, voter might treat intergovernmental grant not the same as private income increase because of loss aversion. However, several recent study showed that the flypaper effect of the intergovernmental grant did not exist when considering the procedure of budget decision and using more rigorous research methods (Duggan, 2000; Gordon, 2004; Lutz, 2010).

This study also shows that the increased aid did not increase per pupil spending in poor school districts in New Hampshire. This echoes Lutz's conclusion that local residents spent more than 90 cents out of one dollar increase in state aid on property tax reduction. The main reason for this was that the budget procedure in New Hampshire met the assumption that Bradford and Oates (1971) laid out. Voters were aware of the reform and could freely amended budget proposals. There was no regulation such as reversion proposition in Oregon which provides chance to bureaucrats to manipulate the budget procedure (Romer & Rosenthal, 1982).

One aim of school finance reform since the 1970s was to decrease inequity in per pupil spending among school districts. Normally, state government either increases

education spending in poor school districts or limits the spending level in rich school districts. For example, in 2004, 12 states had tax or/and expenditure limitation (Jordan et al., 2005). Blankenau and Skidmore (2004) showed that court-ordered school finance reforms had a negative effect on education revenue from local source only in the presence of the tax or/and expenditure limitation. What was radical about the school finance reforms in Vermont and New Hampshire was that state government directly took away the property tax revenue from rich school districts although they did not impose the limitation on the spending and property tax rate. The recapture proposition, which directly transfer property tax revenue from rich school districts to poor school districts, was extremely unpopular in the approach to achieve the educational equity because it meant the loss of property tax revenue for these rich school districts (Reschovsky, 1994). Are these school districts willing to increase their tax effort to substitute the loss or do they just decrease expenditure on education? There is no empirical study on this question before this study. This study suggests that at least in Vermont and New Hampshire that the school districts losing property revenue chose to decrease their educational spending.

Policy Implications

This study provides three implications for school finance policy. First, if the increase of per pupil spending in poor school districts is the aim of school finance reforms, the increase of state aid without restriction of its use is not likely to achieve its goal because local residents are likely to use this aid to relieve their tax burden. This study shows that the increased state aid did not increase per pupil spending in poor school districts in New Hampshire. This is probably because budget procedure could represent the preference of local voters in New Hampshire. Thus, this study indicates that the

restrictions on the use of state aid are needed and should be considered in the process of policy design. For example, the power equalization part of the school finance system in Vermont, which changed the tax price for poor school districts, stimulated them spending more on education. Also, Hoxby (1996) argued that foundation and power equalization school finance system were inferior to a mandatory spending level combined with categorical aid. The increase of categorical aid instead of general aid to poor school districts might prevent them from using it to relieve their tax burden.

Second, the recapture proposition aimed to decrease the inequality in education spending among school districts by redistributing local property tax revenue in rich school districts to poor school districts. This study shows that the recapture decreased per pupil spending in rich school district, but did not increase per pupil spending in poor school districts in New Hampshire. This indicates that the school finance reform decreased inequity in spending between rich and poor school districts. Also, the school finance reform in Vermont, which included a recapture proposition, increased per pupil spending in poor school districts and decreased per pupil spending in rich school districts. This also indicated that the school finance reform decreased inequity in per pupil spending, and the capture might play an important role in it.

However, is this measure a sustainable way to achieve equity in education spending? The direct recapture of property tax revenue from rich school districts and distributing to poor school districts resulted in strong opposition from rich school districts. They attempted to invent measures, such as the increase of private fundraising organizations after the school finance reform in Vermont, to circumvent the policy (Steinman, 2005). Also, residents in these school districts tried to repeal this kind of

dramatic school finance reform measure in state legislature. For example, in February of 2004, Vermont passed Act 68 that reformed the school finance policy in Act 60. Act 68 maintained the two-tier system adopted in the Act 60, but eliminated the recapture proposition. In 2005, the New Hampshire legislature adopted HB 616 that allowed all towns to keep the excess statewide property tax revenue except two towns on the condition that the revenue was spent on education. Therefore, the approach to achieve equality in educational expenditure by direct redistribution of property tax revenue from rich school districts to poor school districts was not sustainable at least in Vermont and New Hampshire.

What measure could achieve equity in education spending? In 2004, although Vermont eliminated recapture proposition by Act 68, it maintained the two-tier system. It used an increase in state sale taxes to cover the funding that was recaptured from rich school districts before 2004 (Schmidt & Scott, 2006). This indicates that it is better to use general state revenue such as sale taxes or income tax to balance the inequity of education spending among school districts than direct transfer of property tax revenue from rich school districts to poor school districts.

Third, this study provided implication to understand true motivations of plaintiffs in school finance litigations. The plaintiffs in the case of *Brigham v. State (1997)* demanded the same educational opportunities as students who resided in wealthier school districts and complained about disproportionate tax rate burden they bore. The plaintiffs in the *Claremont v. Governor (1997)* case alleged that New Hampshire state government failed to spread educational opportunities equitably among its students and adequately fund education, and that the heavy reliance on property taxes to finance New Hampshire

public schools resulted in an unreasonable, disproportionate, and burdensome tax. The lower spending level and higher property tax were the two complaints that these plaintiffs had. Both school finance reforms in Vermont and New Hampshire were promoted by successful school finance litigations for plaintiffs. The corresponding school finance reforms did increase the state aid to these school districts with lower spending. However, this study shows that they did not spend the increased state aid on education at least in New Hampshire (Lutz, 2010). This indicates that lower educational spending was more tolerable than higher property tax rate for poor school districts.

Limitation of This Study and Implication for Further Research

Although several attempts to increase the robustness of the analytical models in this study were made, data availability places some limitations on this study. First, the control variables included in the study were limited. For example, the variables representing the wealth level of school districts such as median house value or per capita income were not available. To some extent, percentage of students in free lunch program, which was included in this study, could be a proxy of wealth level of school districts, but it is not accurate because poor family may not apply for free lunch program, or rich families have higher level of wealth although the percentage of students in free lunch program is high. This could produce bias on the estimation of the school finance reform on per pupil spending (Denzau & Grier, 1984). Temporal variation in wealth level of school districts could dilute the effect of school finance reforms although the fixed effect analysis could control for the difference in wealth level among school districts. Therefore, future studies should take advantage of more recent data to increase the robustness of models in estimating the effect of school finance reforms.

Second, as this study shows, the choice of comparison state plays an important role in the DID design. In this study, models with comparison states such as Rhode Island, in which the number of school districts was small, produced unstable results for the estimation of school finance reforms on per pupil spending. Caution should be advised when interpreting the results from the study.

Third, the advantage of DID estimation is that it could control the influence of unobservable variables that influences outcome variable in both reform and comparison states. The disadvantage of this strategy is that it only estimates the effect of whole package of the school finance reform. It could not identify specific measure that produced the effect. For example, the school finance reform in Vermont decreased per pupil spending in rich school districts. It reduced the property tax revenue and increased tax price for rich school districts. It is not possible for this study to conclude that which one or both are the reason for the decreased per pupil spending in rich school districts.

Conclusions

At the end of the 20th century, the constitutionality of school finance systems in both Vermont and New Hampshire was challenged in courts for the first time. Both states initiated dramatic school finance reforms after their school finance systems were ruled as unconstitutional. This included the establishment of new statewide property tax and the controversial recapture proposition. This study aimed to evaluate the effect of these school finance reforms on per pupil spending.

By utilizing the DID research design and using school districts in Maine and Rhode Island as comparison group, this study found that the school finance reform in Vermont had a negative effect on per pupil spending in rich school districts and a positive

effect on poor school districts. By analyzing the measures of the reformed school finance system, it could be said that the change in the tax price were significantly related to how school districts decide school budget. School finance reform in New Hampshire had a negative effect on per pupil spending in rich school districts and no significant effect on per pupil spending in poor school districts. This indicates that increasing state aid to poor school districts without restraint on its use might not increase per pupil spending. It also indicates that the recapture of property revenue from rich school districts did decrease the spending level in these school districts.

This study makes three important contributions to the literature on school finance. First, it shows that it is important to evaluate the different effect of school finance reforms on school districts in different spending level. This study provides strong evidence that school finance reform could produce contradictory effect on educational expenditure in poor and rich school districts. Second, this study shows that the recapture proposition used in school finance reforms did decrease per pupil spending in those school districts whose local revenue was recaptured. No wonder that the recapture propositions were repealed in both Vermont and New Hampshire after they took effect for several years. This places a question on the sustainability of this approach to increase educational equity. Third, this study shows that the increased state aid to school districts did not increase per pupil spending in these school districts if local residents were free to decide the use of these grants. This result is consistent to several recent studies which shows that the flypaper of intergovernmental grant might not exist(Duggan, 2000; Gordon, 2004; Lutz, 2010). This is important to policy makers because it indicates that

the increase of state grant with no restraint on its use might not be an effective way to increase education spending in recipient school districts.

Data availability places some limits on this study. The lack of annual property value of school districts might influence the estimation of the effect of the school finance reforms in Vermont and New Hampshire. Future studies need try to use new data to improve the estimation strategy. Also, the estimation could produce some inconsistency results when the comparison state just includes one state, especially when the state has a small number of school districts. Future studies should try to use more comparison states in order to utilize the DID research design. Third, the case of estimation the effect of school finance reform in Vermont, the approach to use school finance reform as a dummy variable could not answer what specific measure in the reform produced the effect of the reform. It is not possible to decide whether the increase in per pupil spending in poor school districts in Vermont was due to the increased state aid or the decrease of tax price. More empirical studies are needed to answer this question.

REFERENCES

- Aaronson, D. (1999). The effect of school finance reform on population heterogeneity. *National Tax Journal*, 52(1), 5–29. Retrieved from <http://www.jstor.org/stable/41789373>
- Abadie, A. (2005). Semiparametric difference-in-differences estimators. *The Review of Economic Studies*, 72(1), 1–19. Retrieved from <http://restud.oxfordjournals.org/content/72/1/1.short>
- Angrist, J. D., & Pischke, J.-S. (2008). *Mostly harmless econometrics: An empiricist's companion*. Princeton and Oxford: Princeton University Press.
- Ashenfelter, O., & Card, D. (1984). Using the longitudinal structure of earnings to estimate the effect of training programs. *The Review of Economics and Statistics*, 67(4), 648–660. Retrieved from <http://www.jstor.org/stable/10.2307/1924810>
- Athey, S., & Imbens, G. (2006). Identification and inference in nonlinear difference-in-differences models. *Econometrica*, 74(2), 431–497. Retrieved from <http://www.nber.org/papers/t0280>
- Baicker, K., & Gordon, N. (2006). The effect of state education finance reform on total local resources. *Journal of Public Economics*, 90(8-9), 1519–1535. doi:10.1016/j.jpubeco.2006.01.003
- Baird, K. E. (2008). Federal direct expenditures and school funding disparities, 1990–2000. *Journal of Education Finance*, 33(3), 297–310. Retrieved from <http://eric.ed.gov/ERICWebPortal/recordDetail?accno=EJ781684>
- Baker, B. D., & Welner, K. G. (2010). Premature celebrations: The persistence of inter-district funding disparities. *Education Policy Analysis Archives*, 18(9), 1–30. Retrieved from <http://epaa.asu.edu/ojs/article/view/718>
- Bergstrom, T. C., & Goodman, R. P. (1973). Private demands for public goods. *The American Economic Review*, 63(3), 280–296. Retrieved from <http://www.jstor.org/stable/10.2307/1914361>
- Bergstrom, T. C., Rubinfeld, D. L., & Shapiro, P. (1982). Micro-based estimates of demand functions for local school expenditures. *Econometrica*, 50(5), 1183–1205. Retrieved from <http://www.jstor.org/stable/10.2307/1911869>
- Bertrand, M., Duflo, E., & Mullainathan, S. (2002). How much should we trust differences-in-differences estimates? *The Quarterly Journal of Economics*, 119(1), 249–275. Retrieved from <http://www.nber.org/papers/w8841>

- Besley, T., & Case, A. (2000). Unnatural experiments? Estimating the incidence of endogenous policies. *The Economic Journal*, 110, 672–694. Retrieved from <http://onlinelibrary.wiley.com/doi/10.1111/1468-0297.00578/abstract>
- Bilotti, C. P. (1999). Public school finance programs of the United States and Canada: 1998-99 Rhode Island, 1–14. Retrieved from http://nces.ed.gov/edfin/state_financing.asp
- Black, D. (1948). On the rationale of group decision-making. *The Journal of Political Economy*, 56(1), 23–34. Retrieved from <http://www.jstor.org/stable/10.2307/1825026>
- Black, S. E. (1999). Do better schools matter? Parental valuation of elementary education. *Quarterly Journal of Economics*, 114(2), 577–599. Retrieved from <http://qje.oxfordjournals.org/content/114/2/577.short>
- Blankenau, W. F., & Skidmore, M. L. (2004). School finance litigation, tax and expenditure limitations, and education spending. *Contemporary Economic Policy*, 22(1), 127–143. doi:10.1093/cep/byh010
- Bradford, D. F., & Oates, W. E. (1971). The analysis of revenue sharing in a new approach to collective fiscal decisions. *The Quarterly Journal of Economics*, 85(3), 416–439. Retrieved from <http://www.jstor.org/stable/1885931>
- Brewer, M., Crossley, T. F., & Joyce, R. (2013). *Inference with difference-in-differences revisited* (pp. 1–25). Retrieved from <http://eprints.ncrm.ac.uk/3211/>
- Brigham v. State, 692 A.2d 384 (1997).
- Buzuvis, E. (2011). “A” for effort: Evaluating recent state education reform in response to judicial demands for equity and adequacy. *Cornell Law Review*, 86, 646–689. Retrieved from http://papers.ssrn.com/sol3/papers.cfm?abstract_id=1974942
- Card, D., & Krueger, A. (1993). Minimum wages and employment: A case study of the fast food industry in New Jersey and Pennsylvania. *American Economic Review*, 84(4), 772–793. Retrieved from <http://www.nber.org/papers/w4509>
- Card, D., & Payne, A. A. (2002). School finance reform, the distribution of school spending, and the distribution of student test scores. *Journal of Public Economics*, 83(1), 49–82. Retrieved from <http://www.sciencedirect.com/science/article/pii/S0047272700001778>
- Chandler, M. (2005). The median voter model versus the bureaucracy model of school finance equalization aid. *Public Finance Review*, 33(1), 62–83. doi:10.1177/1091142104270909

- Chaudhary, L. (2009). Education inputs, student performance and school finance reform in Michigan. *Economics of Education Review*, 28(1), 90–98.
doi:10.1016/j.econedurev.2007.11.004
- Claremont v. Governor, 703 A.2d 1353 (1997).
- Cornman, S. Q., Keaton, P., & Glander, M. (2013). *Revenues and Expenditures for Public Elementary and Secondary School Districts : School Year 2010–11 (Fiscal Year 2011)* (Vol. 11). Washington, DC. Retrieved from <http://nces.ed.gov/pubsearch/>
- Courant, P., Gramlich, E., & Rubinfeld, D. (1978). The stimulative effects of intergovernmental grants: Or why money sticks where it hits. In P. Mieszkowski & W. H. Oakland (Eds.), *Fiscal federalism and grants-in-aid* (pp. 5–21). Retrieved from http://www.warreninstitute.berkeley.edu/faculty/rubinfeldd/Profile/publications/stimulative_effects.pdf
- Courant, P., & Loeb, S. (1997). Centralization of school finance in Michigan. *Journal of Policy Analysis and Management*, 16(1), 114–136. Retrieved from <http://deepblue.lib.umich.edu/handle/2027.42/34833>
- Dee, T., & Levine, J. (2004). The fate of new funding: Evidence from Massachusetts' education finance reforms. *Educational Evaluation and Policy Analysis*, 26(3), 199–215. Retrieved from <http://epa.sagepub.com/content/26/3/199.short>
- Denzau, A., & Grier, K. (1984). Determinants of local school spending: Some consistent estimates. *Public Choice*, 44(2), 375–383. Retrieved from <http://link.springer.com/article/10.1007/BF00118770>
- Donald, S. G., & Lang, K. (2007). Inference with difference-in-differences and other panel data. *Review of Economics and Statistics*, 89(2), 221–233.
doi:10.1162/rest.89.2.221
- Dow, P., Watkins, J., Leighton, G., & Cameron, S. (1999). Public school finance programs of the United States and Canada: 1998-99 Maine, 1–28. Retrieved from http://nces.ed.gov/edfin/state_financing.asp
- Downes, T. A. (1992). Evaluating the impact of school finance reform on the provision of public education: The California case. *National Tax Journal*, 45(4), 405–419.
- Downes, T. A., & Shah, M. P. (2006). The effect of school finance reforms on the level and growth of per-pupil expenditures. *Peabody Journal of Education*, 81(3), 1–38. Retrieved from http://www.tandfonline.com/doi/abs/10.1207/s15327930pje8103_1

- Downs, A. (1957). An economic theory of political action in a democracy. *The Journal of Political Economy*, 65(2), 135–150. Retrieved from <http://www.jstor.org/stable/10.2307/1827369>
- Duggan, M. (2000). Hospital ownership and public medical spending. *The Quarterly Journal of Economics*, 115(4), 1343–1373. Retrieved from <http://qje.oxfordjournals.org/content/115/4/1343.short>
- Evans, W. N., Murray, S. E., & Schwab, R. M. (1997). Schoolhouses, courthouses, and statehouses after Serrano. *Journal of Policy Analysis and Management*, 16(1), 10–31.
- Fernandez, R., & Rogerson, R. (1999). Education finance reform and investment in human capital: Lessons from California. *Journal of Public Economics*, 74, 327–350. Retrieved from <http://www.sciencedirect.com/science/article/pii/S0047272799000468>
- Fernandez, R., & Rogerson, R. (2003). Equity and resources: An analysis of education finance systems. *Journal of Political Economy*, 111(4), 858–897. Retrieved from <http://www.jstor.org/stable/10.1086/375381>
- Figlio, D. N. (1998). Short-term effects of a 1990s-era property tax limit: Panel evidence on oregon's measure. *National Tax Journal*, (1981), 55–70.
- Figlio, D. N., Husted, T. a., & Kenny, L. W. (2004). Political economy of the inequality in school spending. *Journal of Urban Economics*, 55(2), 338–349. doi:10.1016/j.jue.2003.10.006
- Fischel, W. A. (1989). Did Serrano cause proposition 13? *National Tax Journal*, 42(4), 465–473.
- Fisher, R. (1982). Income and grant effect on local public expenditure the flypaper effect and other difficulties. *Journal of Urban Economics*, 12, 324–345. Retrieved from <http://www.sciencedirect.com/science/article/pii/0094119082900213>
- Fisher, R., & Papke, L. (2000). Local government responses to education grants. *National Tax Journal*, 53(1), 153–168. Retrieved from <http://www.jstor.org/stable/41789446>
- Gordon, N. (2004). Do federal grants boost school spending? Evidence from Title I. *Journal of Public Economics*, 88(9-10), 1771–1792. doi:10.1016/j.jpubeco.2003.09.002
- Gramlich, E. M., & Rubinfeld, D. L. (1982). Micro estimates of public spending demand functions and tests of the tiebout and median-voter hypotheses. *Journal of Political Economy*, 90(3), 536–560. doi:10.1086/261073
- Hall, D. (2003). *School finance reform: Trends and unintended consequences*.

- Heckman, J. (1997). Matching as an econometric evaluation estimator: Evidence from evaluating a job training programme. *The Review of Economic Studies*, 64(4), 605–654. Retrieved from <http://restud.oxfordjournals.org/content/64/4/605.short>
- Heckman, J. (2000). Causal parameters and policy analysis in economics: A twentieth century retrospective. *The Quarterly Journal of Economics*, 115(1), 45–97. Retrieved from <http://qje.oxfordjournals.org/content/115/1/45.short>
- Heckman, J., & Vytlacil, E. (2005). Structural equations, treatment effects and econometric policy evaluation. *Econometrica*, 73(3), 669–738. Retrieved from <http://www.nber.org/papers/w11259>
- Hines, J. R. J., & Thalar, R. H. (1995). Anomalies: The flypaper effect. *The Journal of Economic Perspectives*, 9(4), 217–226. Retrieved from <http://www.nber.org/papers/w14579>
- Hoxby, C. M. (1996). Are efficiency and equity in school finance substitutes or complements? *The Journal of Economic Perspectives*, 10(4), 51–72. Retrieved from <http://www.jstor.org/stable/10.2307/2138554>
- Hoxby, C. M. (1998). How much does school spending depend on family income? The historical origins of the current school finance dilemma. *The American Economic Review*, 88(2), 309–315. Retrieved from <http://www.jstor.org/stable/10.2307/116939>
- Hoxby, C. M. (2000). Does competition among public schools benefit students and taxpayers? *The American Economic Review*, 90(5), 1209–1238. Retrieved from <http://www.nber.org/papers/w4979>
- Hoxby, C. M. (2001). All school finance equalizations are not created equal. *The Quarterly Journal of Economics*, 116(4), 1189–1231. Retrieved from <http://qje.oxfordjournals.org/content/116/4/1189.short>
- Jordan, T. S., Jordan, K. F., & Crawford, J. (2005). The interaction between tax and expenditure limitations, supermajority requirements, and school finance litigation. *Journal of Education Finance*, 31(2), 125–145. Retrieved from <http://www.jstor.org/stable/10.2307/40704256>
- Loeb, S. (2001). Estimating the effects of school finance reform: a framework for a federalist system. *Journal of Public Economics*, 80(2), 225–247. doi:10.1016/S0047-2727(00)00083-9
- Lutz, B. (2010). Taxation with representation: Intergovernmental grants in a plebiscite democracy. *The Review of Economics and Statistics*, 92(2), 316–332. Retrieved from http://papers.ssrn.com/sol3/papers.cfm?abstract_id=899536

- Manwaring, R., & Sheffrin, S. M. (1997). Litigation, school finance reform, and aggregate educational spending. *International Tax and Public Finance*, 4(2), 107–127. Retrieved from <http://www.springerlink.com/index/m72577600g8r3746.pdf>
- Mathis, W. J. (1998). *Finance reform in Vermont: The legislature responds to the Brigham Supreme Court decision*. Retrieved from <http://files.eric.ed.gov/fulltext/ED425505.pdf>
- Mathis, W. J. (1999). Public school finance programs of the United States and Canada: 1998-99 Vermont, 1–24. Retrieved from http://nces.ed.gov/edfin/state_financing.asp
- McDuffy v. Secretary of the Executive Office of Education, 415 Mass. 545, 615 N.E.2d 516 (1993).
- Meyer, B. (1995). Natural and quasi-experiments in economics. *Journal of Business & Economic Statistics*, 13(2), 151–161. doi:10.2307/1392369
- Michener, R. D. (1999). Public School Finance Programs of the United States and Canada: 1998-99 New Hampshire, 1–13. Retrieved from http://nces.ed.gov/edfin/state_financing.asp
- Murray, S., Evans, W., & Schwab, R. M. (1998). Education-finance reform and the distribution of education resources. *American Economic Review*, 88(4), 789–812. Retrieved from <http://www.jstor.org/stable/10.2307/117006>
- Olabisi, O. (2006). *New Hampshire's quest for a constitutionally adequate education* (pp. 1–14).
- Olmsted, G., Denzau, A., & Roberts, J. (1993). We voted for this Institutions and educational spending. *Journal of Public Economics*, 52, 363–376. Retrieved from <http://www.sciencedirect.com/science/article/pii/004727279390040Z>
- Picus, L., Odden, A., Glenn, W., Griffith, M., & Wolkoff, M. (2012). *An evaluation of Vermont's education finance system*. Retrieved from <http://www.leg.state.vt.us/JFO/Education RFP Page/Picus and Assoc VT Finance Study 1-18-2012.pdf>
- Rebell, M., & Metzler, J. (2002). Rapid response, radical reform: The story of school finance litigation in Vermont. *Journal of Law & Education*, 31(3), 167–190. Retrieved from http://heinonlinebackup.com/hol-cgi-bin/get_pdf.cgi?handle=hein.journals/jle31§ion=21
- Reschovsky, A. (1994). Fiscal equalization and school finance. *National Tax Journal*, 47(1), 185–197. Retrieved from <http://www.jstor.org/stable/41789060>

- Reschovsky, A., & Imazeki, J. (2001). Achieving educational adequacy through school finance reform. *Journal of Education Finance*, (October). Retrieved from http://heinonlinebackup.com/hol-cgi-bin/get_pdf.cgi?handle=hein.journals/urban12§ion=49
- Robinson v. Cahill, 62 N.J. 473, 303 A.2d 273 (1973).
- Romer, T., & Rosenthal, H. (1979a). Bureaucrats versus voters: On the political economy of resource allocation by direct democracy. *The Quarterly Journal of Economics*, 93(4), 563–587. Retrieved from <http://qje.oxfordjournals.org/content/93/4/563.short>
- Romer, T., & Rosenthal, H. (1979b). The elusive median voter. *Journal of Public Economics*, 12, 143–170. Retrieved from <http://www.sciencedirect.com/science/article/pii/0047272779900100>
- Romer, T., & Rosenthal, H. (1982). Median voters or budget maximizers: Evidence from school expenditure referenda. *Economic Inquiry*, 20, 556–578. Retrieved from <http://onlinelibrary.wiley.com/doi/10.1111/j.1465-7295.1982.tb00366.x/abstract>
- Romer, T., Rosenthal, H., & Munley, V. G. (1992). Economic incentives and political institutions: Spending and voting in school budget referenda. *Journal of Public Economics*, 49(1), 1–33. doi:10.1016/0047-2727(92)90061-J
- Rose v. Council for Better Education, 790 S.W.2d 186, (1989).
- San Antonio Independent School District v. Rodriguez, 411 U.S. 1 (1973).
- Schmidt, S. J., & Scott, K. (2006). Reforming reforms: Changing incentives in education finance in Vermont. *Education Finance and Policy*, 1(4), 441–464. doi:10.1162/edfp.2006.1.4.441
- Serrano v. Priest, 18 Cal.3d 728 (1976).
- Serrano v. Priest, 5 Cal.3d 584 (1971).
- Silva, F., & Sonstelie, J. (1995). Did Serrano cause a decline in school spending? *National Tax Journal*, 48(2), 199–215. Retrieved from <http://www.freepatentsonline.com/article/National-Tax-Journal/17227920.html>
- Snyder, T. D., & Hoffman, C. M. (2001). *Digest of Education Statistics 2000*. Washington, DC.
- Snyder, T. D., & Hoffman, C. M. (2002). *Digest of Education Statistics 2001*. Washington, DC.

- Springer, M. G., Liu, K., & Guthrie, J. W. (2009). The impact of school finance litigation on resource distribution: A comparison of court-mandated equity and adequacy reforms. *Education Economics*, 17(4), 421–444. doi:10.1080/09645290802069269
- Steinman, J. (2005). *Alternative Revenue Generation in Vermont Public Schools* (pp. 1–76). Retrieved from <http://ase.tufts.edu/econ/research/documents/2005/thesisSteinman.pdf>
- Theobald, N., & Picus, L. (1991). Living with equal amounts of less: Experiences of states with primarily state-funded school systems. *Journal of Education Finance*, 17(1), 1–6. Retrieved from <http://www.jstor.org/stable/10.2307/41575622>
- Thompson, D., & Crampton, F. (2002). The impact of school finance litigation: A long view. *Journal of Education Finance*, 27, 783–816. Retrieved from <http://www.jstor.org/stable/10.2307/40704161>
- Thro, W. E. (1990). Third wave: The impact of the Montana, Kentucky, and Texas decisions on the future of public school finance reform litigation. *Journal Law and Education*, 19, 219–250. Retrieved from http://heinonlinebackup.com/hol-cgi-bin/get_pdf.cgi?handle=hein.journals/jle19§ion=19
- Tiebout, C. (1956). A pure theory of local expenditures. *The Journal of Political Economy*, 64(5), 416–424. Retrieved from <http://www.jstor.org/stable/10.2307/1826343>
- Urquiola, M. (2005). Does school choice lead to sorting? Evidence from Tiebout variation. *American Economic Review*, 95(4), 1310–1326. Retrieved from <http://www.jstor.org/stable/10.2307/4132718>
- Verstegen, D., & Jordan, T. S. (2009). A fifty-state survey of school finance policies and programs: An overview. *Journal of Education Finance*, 34(3), 213–230. Retrieved from <http://eric.ed.gov/ERICWebPortal/recordDetail?accno=EJ865038>
- Wong, K. (2013). The design of the Rhode Island school funding formula: Developing new strategies on equity and accountability. *Peabody Journal of Education*, 88(1), 37–47. doi:10.1080/0161956X.2013.752638
- Wyckoff, P. (1991). The elusive flypaper effect. *Journal of Urban Economics*, 30, 310–328. Retrieved from <http://www.sciencedirect.com/science/article/pii/0094119091900529>
- Zimmer, R., & Jones, J. T. (2005). Unintended consequence of centralized public school funding in Michigan education. *Southern Economic Journal*, 71(3), 534. doi:10.2307/20062058

APPENDIX

Appendix One: Robustness Check of the DID Estimations for Vermont

Table 1: DID Estimation of the School Finance Reform in Vermont with Comparison State Maine and Rhode Island on per Pupil Spending in All School Districts (Averaged Data)

	Four Years of Data		Six Years of Data	
	No Control (1)	Control (2)	No Control (3)	Control (4)
Reform	-0.006 (0.010)	-0.034** (0.015)	0.002 (0.010)	-0.020 (0.013)
Time	0.087*** (0.006)	0.086*** (0.005)	0.126*** (0.006)	0.122*** (0.006)
Logarithm of enrollment (6years)				-0.134*** (0.051)
Percentage of free lunch students (6years)				-0.257** (0.104)
Logarithm of enrollment (4years)		-0.193*** (0.057)		
Percentage of free lunch students (4years)		-0.300** (0.122)		
Constant	8.849*** (0.003)	10.087*** (0.354)	8.834*** (0.002)	9.701*** (0.314)
Observations	985	786	985	784
R-squared	0.359	0.418	0.571	0.586
Number of id	493	410	493	409

Note. School district level fixed effect is included in each model above. Robust standard errors in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

Table 2: DID Estimation of the School Finance Reform in Vermont with Comparison State Maine on per Pupil Spending in All School Districts

	Four Years of Data		Six Years of Data	
	No Control (1)	Control (2)	No Control (3)	Control (4)
Reform	0.001 (0.010)	-0.025** (0.011)	0.002 (0.010)	-0.019* (0.011)
1997.year			0.030*** (0.004)	0.030*** (0.004)
1998.year	0.024*** (0.005)	0.035*** (0.006)	0.055*** (0.006)	0.064*** (0.006)
1999.year	0.086*** (0.007)	0.088*** (0.006)	0.116*** (0.008)	0.117*** (0.007)
2000.year	0.126*** (0.008)	0.137*** (0.008)	0.155*** (0.008)	0.167*** (0.009)
2001.year			0.214*** (0.008)	0.219*** (0.009)
Logarithm of enrollment		-0.230*** (0.050)		-0.234*** (0.041)
Percentage of free lunch students		-0.146** (0.070)		-0.104** (0.052)
Constant	8.823*** (0.003)	10.196*** (0.299)	8.792*** (0.004)	10.175*** (0.241)
Observations	1,795	1,548	2,692	2,410
R-squared	0.320	0.413	0.459	0.545
Number of id	449	435	449	439

Note. School district level fixed effect is included in each model above. Robust standard errors in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

Table 3: DID Estimation of the School Finance Reform in Vermont with Comparison State Rhode Island on per Pupil Spending in All School Districts

	Four Years of Data		Six Years of Data	
	No Control	Control	No Control	Control
	(1)	(2)	(3)	(4)
Reform	0.052*** (0.012)	0.018 (0.013)	0.065*** (0.014)	0.025* (0.014)
1997.year			0.032*** (0.005)	0.031*** (0.005)
1998.year	0.006 (0.007)	0.007 (0.008)	0.038*** (0.008)	0.038*** (0.009)
1999.year	0.021** (0.010)	0.036*** (0.010)	0.042*** (0.014)	0.065*** (0.013)
2000.year	0.070*** (0.011)	0.075*** (0.011)	0.090*** (0.014)	0.105*** (0.015)
2001.year			0.149*** (0.014)	0.150*** (0.014)
Logarithm of enrollment		-0.738*** (0.054)		-0.667*** (0.075)
Percentage of free lunch students		0.035 (0.067)		0.066 (0.051)
Constant	8.804*** (0.005)	13.132*** (0.320)	8.772*** (0.006)	12.667*** (0.439)
Observations	1,051	871	1,576	1,396
R-squared	0.294	0.524	0.438	0.657
Number of id	263	259	263	263

Note. School district level fixed effect is included in each model above. Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 4: DID Estimation of the School Finance Reform in Vermont with Comparison State Maine and Rhode Island on per Pupil Spending in All School Districts (Without Free Lunch Variable)

	Four Years of Data		Six Years of Data	
	No Control	Control	No Control	Control
	(1)	(2)	(3)	(4)
Reform	0.008 (0.009)	-0.003 (0.009)	0.011 (0.010)	-0.005 (0.010)
1997.year			0.030*** (0.004)	0.029*** (0.003)
1998.year	0.023*** (0.005)	0.022*** (0.004)	0.054*** (0.006)	0.050*** (0.005)
1999.year	0.078*** (0.007)	0.077*** (0.006)	0.107*** (0.008)	0.107*** (0.007)
2000.year	0.118*** (0.007)	0.127*** (0.007)	0.147*** (0.008)	0.157*** (0.008)
2001.year			0.204*** (0.008)	0.209*** (0.008)
Logarithm of enrollment		-0.259*** (0.041)		-0.258*** (0.037)
Constant	8.834*** (0.003)	10.375*** (0.245)	8.803*** (0.004)	10.342*** (0.219)
Observations	1,939	1,939	2,908	2,908
R-squared	0.314	0.404	0.451	0.521
Number of id	485	485	485	485

Note. School district level fixed effect is included in each model above. Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 5: DID Estimation of the School Finance Reform in Vermont with Comparison State Maine and Rhode Island on per Pupil Spending in All School Districts (Including Eight School Districts)

	Four Years of Data		Six Years of Data	
	No Control (1)	Control (2)	No Control (3)	Control (4)
Reform	0.025** (0.011)	-0.003 (0.012)	0.023** (0.010)	-0.001 (0.011)
1997.year			0.030*** (0.004)	0.030*** (0.003)
1998.year	0.007 (0.007)	0.019** (0.008)	0.037*** (0.008)	0.049*** (0.008)
1999.year	0.070*** (0.007)	0.073*** (0.007)	0.101*** (0.008)	0.104*** (0.007)
2000.year	0.110*** (0.007)	0.122*** (0.008)	0.141*** (0.008)	0.153*** (0.008)
2001.year			0.199*** (0.008)	0.204*** (0.009)
Logarithm of enrollment		-0.236*** (0.049)		-0.242*** (0.040)
Percentage of free lunch students		-0.146** (0.070)		-0.116** (0.050)
Constant	8.830*** (0.004)	10.280*** (0.307)	8.800*** (0.004)	10.274*** (0.246)
Observations	1,970	1,718	2,955	2,668
R-squared	0.246	0.311	0.388	0.471
Number of id	493	479	493	483

Note. School district level fixed effect is included in each model above. Robust standard errors in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

Table 6: DID Estimation of the School Finance Reform in Vermont with Comparison State Maine and Rhode Island on per Pupil Spending in Poor School Districts (Averaged Data)

	Four Years of Data		Six Years of Data	
	No Control (1)	Control (2)	No Control (3)	Control (4)
Reform	0.035*** (0.013)	0.033** (0.016)	0.045*** (0.014)	0.034** (0.015)
Time	0.093*** (0.008)	0.087*** (0.007)	0.129*** (0.008)	0.122*** (0.009)
Logarithm of enrollment (6years)				-0.192** (0.075)
Percentage of free lunch students (6years)				-0.097 (0.147)
Logarithm of enrollment (4years)		-0.188** (0.092)		
Percentage of free lunch students (4years)		-0.019 (0.147)		
Constant	8.671*** (0.003)	9.852*** (0.576)	8.662*** (0.003)	9.888*** (0.479)
Observations	329	274	330	271
R-squared	0.637	0.687	0.761	0.798
Number of id	165	143	165	142

Note. School district level fixed effect is included in each model above. Robust standard errors in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

Table 7: DID Estimation of the School Finance Reform in Vermont with Comparison State Maine on per Pupil Spending in Poor School Districts

	Four Years of Data		Six Years of Data	
	No Control	Control	No Control	Control
	(1)	(2)	(3)	(4)
Reform	0.024*	0.014	0.034**	0.020
	(0.013)	(0.012)	(0.014)	(0.013)
1997.year			0.029***	0.032***
			(0.006)	(0.005)
1998.year	0.046***	0.049***	0.071***	0.077***
	(0.006)	(0.006)	(0.008)	(0.006)
1999.year	0.097***	0.092***	0.119***	0.117***
	(0.009)	(0.007)	(0.009)	(0.008)
2000.year	0.147***	0.142***	0.169***	0.168***
	(0.010)	(0.009)	(0.009)	(0.010)
2001.year			0.231***	0.221***
			(0.010)	(0.010)
Logarithm of enrollment		-0.201***		-0.269***
		(0.050)		(0.048)
Percentage of free lunch students		-0.078		-0.111**
		(0.053)		(0.053)
Constant	8.642***	9.866***	8.617***	10.259***
	(0.005)	(0.299)	(0.006)	(0.289)
Observations	596	540	894	820
R-squared	0.560	0.626	0.672	0.744
Number of id	149	147	149	147

Note. School district level fixed effect is included in each model above. Robust standard errors in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

Table 8: DID Estimation of the School Finance Reform in Vermont with Comparison State Rhode Island on per Pupil Spending in Poor School Districts

	Four Years of Data		Six Years of Data	
	No Control (1)	Control (2)	No Control (3)	Control (4)
Reform	0.083*** (0.016)	0.049*** (0.014)	0.081*** (0.028)	0.033 (0.025)
1997.year			0.039*** (0.009)	0.043*** (0.009)
1998.year	0.040*** (0.008)	0.027*** (0.008)	0.073*** (0.012)	0.069*** (0.015)
1999.year	0.030** (0.013)	0.041*** (0.011)	0.069** (0.029)	0.092*** (0.027)
2000.year	0.089*** (0.014)	0.088*** (0.012)	0.132*** (0.030)	0.142*** (0.028)
2001.year			0.189*** (0.030)	0.181*** (0.028)
Logarithm of enrollment		-0.658*** (0.058)		-0.692*** (0.080)
Percentage of free lunch students		0.073 (0.070)		-0.009 (0.076)
Constant	8.640*** (0.006)	12.464*** (0.336)	8.605*** (0.010)	12.670*** (0.471)
Observations	348	310	522	479
R-squared	0.549	0.705	0.637	0.772
Number of id	87	87	87	87

Note. School district level fixed effect is included in each model above. Robust standard errors in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

Table 9: DID Estimation of the School Finance Reform in Vermont with Comparison State Maine and Rhode Island on per Pupil Spending in Poor School Districts (Without Free Lunch Variable)

	Four Years of Data		Six Years of Data	
	No Control	Control	No Control	Control
	(1)	(2)	(3)	(4)
Reform	0.032** (0.013)	0.025** (0.012)	0.041*** (0.014)	0.028** (0.013)
1997.year			0.032*** (0.006)	0.031*** (0.006)
1998.year	0.044*** (0.005)	0.042*** (0.005)	0.072*** (0.008)	0.069*** (0.007)
1999.year	0.089*** (0.009)	0.086*** (0.007)	0.115*** (0.009)	0.111*** (0.008)
2000.year	0.137*** (0.009)	0.136*** (0.009)	0.164*** (0.010)	0.163*** (0.010)
2001.year			0.224*** (0.011)	0.217*** (0.010)
Logarithm of enrollment		-0.226*** (0.058)		-0.271*** (0.047)
Constant	8.659*** (0.004)	10.035*** (0.350)	8.630*** (0.006)	10.293*** (0.289)
Observations	644	644	966	966
R-squared	0.546	0.601	0.652	0.709
Number of id	161	161	161	161

Note. School district level fixed effect is included in each model above. Robust standard errors in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

Table 10: DID Estimation of the School Finance Reform in Vermont with Comparison State Maine and Rhode Island on per Pupil Spending in Poor School Districts (Including Eight School Districts)

	Four Years of Data		Six Years of Data	
	No Control	Control	No Control	Control
	(1)	(2)	(3)	(4)
Reform	0.070*** (0.019)	0.044*** (0.016)	0.058*** (0.016)	0.037*** (0.014)
1997.year			0.031*** (0.006)	0.036*** (0.006)
1998.year	0.001 (0.017)	0.017 (0.015)	0.035** (0.017)	0.049*** (0.016)
1999.year	0.067*** (0.012)	0.068*** (0.010)	0.101*** (0.011)	0.103*** (0.010)
2000.year	0.117*** (0.012)	0.119*** (0.011)	0.150*** (0.011)	0.153*** (0.012)
2001.year			0.214*** (0.011)	0.207*** (0.012)
Logarithm of enrollment		-0.219*** (0.055)		-0.294*** (0.050)
Percentage of free lunch students		-0.075 (0.058)		-0.135** (0.054)
Constant	8.655*** (0.006)	10.012*** (0.336)	8.626*** (0.007)	10.459*** (0.307)
Observations	655	594	983	908
R-squared	0.317	0.375	0.486	0.566
Number of id	164	162	164	162

Note. School district level fixed effect is included in each model above. Robust standard errors in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

Table 11: DID Estimation of the School Finance Reform in Vermont with Comparison State Maine and Rhode Island on per Pupil Spending in Rich School Districts (Averaged Data)

	Four Years of Data		Six Years of Data	
	No Control	Control	No Control	Control
	(1)	(2)	(3)	(4)
Reform	-0.062*** (0.024)	-0.109*** (0.034)	-0.049** (0.021)	-0.080** (0.034)
Time	0.090*** (0.014)	0.087*** (0.013)	0.130*** (0.014)	0.117*** (0.017)
Logarithm of enrollment (6years)				-0.051 (0.094)
Percentage of free lunch students (6years)				-0.228 (0.158)
Logarithm of enrollment (4years)		-0.155* (0.093)		
Percentage of free lunch students (4years)		-0.369** (0.172)		
Constant	9.051*** (0.006)	10.013*** (0.534)	9.029*** (0.005)	9.385*** (0.541)
Observations	328	251	328	250
R-squared	0.174	0.267	0.395	0.397
Number of id	164	131	164	130

Note. School district level fixed effect is included in each model above. Robust standard errors in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

Table 12: DID Estimation of the School Finance Reform in Vermont with Comparison State Maine on per Pupil Spending in Rich School Districts

	Four Years of Data		Six Years of Data	
	No Control	Control	No Control	Control
	(1)	(2)	(3)	(4)
Reform	-0.021 (0.023)	-0.063** (0.026)	-0.018 (0.022)	-0.056** (0.027)
1997.year			0.000 (0.000)	0.027*** (0.008)
1998.year	0.008 (0.012)	0.031** (0.013)	0.035*** (0.009)	0.054*** (0.014)
1999.year	0.083*** (0.018)	0.092*** (0.014)	0.042*** (0.013)	0.113*** (0.017)
2000.year	0.098*** (0.018)	0.135*** (0.020)	0.113*** (0.020)	0.155*** (0.022)
2001.year			0.128*** (0.019)	0.213*** (0.024)
Logarithm of enrollment		-0.247*** (0.086)	0.189*** (0.019)	-0.224*** (0.077)
Percentage of free lunch students		-0.209* (0.124)		-0.113 (0.100)
Constant	9.030*** (0.008)	10.440*** (0.494)	8.993*** (0.010)	10.261*** (0.439)
Observations	592	493	888	767
R-squared	0.140	0.253	0.254	0.344
Number of id	148	140	148	141

Note. School district level fixed effect is included in each model above. Robust standard errors in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

Table 13: DID Estimation of the School Finance Reform in Vermont with Comparison State Rhode Island on per Pupil Spending in Rich School Districts

	Four Years of Data		Six Years of Data	
	No Control	Control	No Control	Control
	(1)	(2)	(3)	(4)
Reform	0.016 (0.029)	-0.014 (0.032)	0.034 (0.027)	0.009 (0.028)
1997.year				0.016 (0.010)
1998.year	-0.025 (0.016)	-0.003 (0.018)	-0.023 (0.016)	0.011 (0.020)
1999.year	0.024 (0.024)	0.040* (0.023)	0.012 (0.023)	0.044* (0.024)
2000.year	0.051* (0.028)	0.067** (0.029)	0.037 (0.026)	0.066** (0.029)
2001.year			0.097*** (0.023)	0.118*** (0.025)
Logarithm of enrollment		-0.719*** (0.091)		-0.640*** (0.162)
Percentage of free lunch students		0.002 (0.119)		0.106 (0.104)
Constant	8.978*** (0.010)	13.208*** (0.544)	8.977*** (0.010)	12.721*** (0.956)
Observations	344	267	430	437
R-squared	0.116	0.334	0.242	0.491
Number of id	86	83	86	86

Note. School district level fixed effect is included in each model above. Robust standard errors in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

Table 14: DID Estimation of the School Finance Reform in Vermont with Comparison State Maine and Rhode Island on per Pupil Spending in Rich School Districts (Without Free Lunch Variable)

	Four Years of Data		Six Years of Data	
	No Control (1)	Control (2)	No Control (3)	Control (4)
Reform	-0.015 (0.021)	-0.034 (0.021)	-0.011 (0.021)	-0.040* (0.022)
1997.year			0.032*** (0.008)	0.028*** (0.007)
1998.year	0.008 (0.011)	0.006 (0.009)	0.039*** (0.012)	0.035*** (0.011)
1999.year	0.076*** (0.016)	0.077*** (0.014)	0.103*** (0.018)	0.106*** (0.016)
2000.year	0.095*** (0.016)	0.127*** (0.019)	0.121*** (0.017)	0.153*** (0.020)
2001.year			0.178*** (0.018)	0.205*** (0.021)
Logarithm of enrollment		-0.282*** (0.071)		-0.261*** (0.069)
Constant	9.034*** (0.007)	10.634*** (0.401)	9.000*** (0.009)	10.486*** (0.393)
Observations	640	640	960	960
R-squared	0.139	0.252	0.250	0.333
Number of id	160	160	160	160

Note. School district level fixed effect is included in each model above. Robust standard errors in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

Table 15: DID Estimation of the School Finance Reform in Vermont with Comparison State Maine and Rhode Island on per Pupil Spending in Rich School Districts (Including Eight School Districts)

	Four Years of Data		Six Years of Data	
	No Control	Control	No Control	Control
	(1)	(2)	(3)	(4)
Reform	-0.012 (0.021)	-0.056** (0.024)	-0.012 (0.021)	-0.046* (0.024)
1997.year			0.031*** (0.008)	0.025*** (0.007)
1998.year	0.007 (0.011)	0.028** (0.012)	0.039*** (0.012)	0.050*** (0.012)
1999.year	0.075*** (0.016)	0.082*** (0.013)	0.102*** (0.018)	0.101*** (0.015)
2000.year	0.095*** (0.016)	0.128*** (0.019)	0.120*** (0.017)	0.143*** (0.020)
2001.year			0.178*** (0.017)	0.197*** (0.021)
Logarithm of enrollment		-0.250*** (0.085)		-0.223*** (0.075)
Percentage of free lunch students		-0.211* (0.124)		-0.130 (0.098)
Constant	9.030*** (0.007)	10.521*** (0.508)	8.998*** (0.009)	10.308*** (0.442)
Observations	652	547	978	854
R-squared	0.144	0.250	0.249	0.337
Number of id	163	155	163	156

Note. School district level fixed effect is included in each model above. Robust standard errors in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

Appendix Two: Robustness Check of the DID Estimations for New Hampshire

Table 1: DID Estimation of the School Finance Reform in New Hampshire with Comparison State Maine and Rhode Island on per Pupil Spending in All School Districts (Averaged Data)

	Four Years of Data		Six Years of Data	
	No Control (1)	Control (2)	No Control (3)	Control (4)
Reform	-0.024*** (0.009)	-0.028*** (0.010)	-0.020** (0.010)	-0.020* (0.011)
Time	0.083*** (0.006)	0.088*** (0.006)	0.119*** (0.006)	0.124*** (0.007)
Logarithm of enrollment (6years)				-0.113** (0.044)
Percentage of free lunch students (6years)				-0.011 (0.176)
Logarithm of enrollment (4years)		-0.071* (0.037)		
Percentage of free lunch students (4years)		-0.010 (0.113)		
Constant	8.912*** (0.002)	9.368*** (0.236)	8.895*** (0.002)	9.618*** (0.306)
Observations	836	720	836	713
R-squared	0.420	0.434	0.549	0.579
Number of id	418	383	418	381

Note. School district level fixed effect is included in each model above. Robust standard errors in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

Table2: DID Estimation of the School Finance Reform in New Hampshire with Comparison State Maine on per Pupil Spending in All School Districts

	Four Years of Data		Six Years of Data	
	No Control (1)	Control (2)	No Control (3)	Control (4)
Reform	-0.027*** (0.009)	-0.039*** (0.009)	-0.026** (0.010)	-0.039*** (0.011)
1998.year			0.034*** (0.004)	0.033*** (0.004)
1999.year	0.044*** (0.005)	0.041*** (0.004)	0.078*** (0.006)	0.073*** (0.005)
2000.year	0.085*** (0.007)	0.096*** (0.007)	0.119*** (0.007)	0.132*** (0.009)
2001.year	0.130*** (0.007)	0.137*** (0.008)	0.164*** (0.008)	0.171*** (0.009)
2002.year			0.201*** (0.010)	0.201*** (0.009)
Logarithm of enrollment		-0.174*** (0.044)		-0.247*** (0.037)
Percentage of free lunch students		-0.170** (0.081)		-0.231*** (0.078)
Constant	8.882*** (0.004)	9.996*** (0.277)	8.847*** (0.004)	10.428*** (0.238)
Observations	1,528	1,369	2,292	2,090
R-squared	0.306	0.381	0.396	0.484
Number of id	382	371	382	375

Note. School district level fixed effect is included in each model above. Robust standard errors in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

Table 3: DID Estimation of the School Finance Reform in New Hampshire with Comparison State Rhode Island on per Pupil Spending in All School Districts

	Four Years of Data		Six Years of Data	
	No Control (1)	Control (2)	No Control (3)	Control (4)
Reform	0.023 (0.019)	0.016 (0.018)	0.047** (0.023)	0.034 (0.021)
1998.year			0.023*** (0.006)	0.031*** (0.006)
1999.year	0.039*** (0.008)	0.050*** (0.008)	0.061*** (0.011)	0.082*** (0.011)
2000.year	0.053*** (0.015)	0.072*** (0.014)	0.060*** (0.021)	0.091*** (0.019)
2001.year	0.106*** (0.016)	0.125*** (0.016)	0.106*** (0.021)	0.139*** (0.020)
2002.year			0.171*** (0.023)	0.189*** (0.021)
Logarithm of enrollment		-0.717*** (0.147)		-0.690*** (0.101)
Percentage of free lunch students		0.198 (0.218)		0.109 (0.167)
Constant	8.695*** (0.008)	13.401*** (0.968)	8.668*** (0.010)	13.280*** (0.677)
Observations	264	233	396	357
R-squared	0.418	0.609	0.550	0.719
Number of id	66	66	66	66

Note. School district level fixed effect is included in each model above. Robust standard errors in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

Table 4: DID Estimation of the School Finance Reform in New Hampshire with Comparison State Maine and Rhode Island on per Pupil Spending in Poor School Districts (Averaged Data)

	Four Years of Data		Six Years of Data	
	No Control (1)	Control (2)	No Control (3)	Control (4)
Reform	-0.023* (0.013)	-0.017 (0.015)	-0.020 (0.016)	-0.005 (0.019)
Time	0.093*** (0.007)	0.090*** (0.007)	0.140*** (0.010)	0.132*** (0.009)
Logarithm of enrollment (6years)				-0.039 (0.080)
Percentage of free lunch students (6years)				-0.092 (0.221)
Logarithm of enrollment (4years)		0.015 (0.048)		
Percentage of free lunch students (4years)		0.053 (0.093)		
Constant	8.724*** (0.003)	8.613*** (0.342)	8.711*** (0.004)	8.996*** (0.578)
Observations	280	238	280	236
R-squared	0.598	0.633	0.679	0.751
Number of id	140	129	140	128

Note. School district level fixed effect is included in each model above. Robust standard errors in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

Table 5: DID Estimation of the School Finance Reform in New Hampshire with Comparison State Maine on per Pupil Spending in Poor School Districts

	Poor School Districts			
	Four Years of Data		Six Years of Data	
	No Control (1)	Control (2)	No Control (3)	Control (4)
Reform	-0.020 (0.015)	-0.018 (0.016)	-0.021 (0.017)	-0.012 (0.016)
1998.year			0.033*** (0.005)	0.034*** (0.005)
1999.year	0.056*** (0.007)	0.053*** (0.006)	0.085*** (0.008)	0.082*** (0.007)
2000.year	0.102*** (0.010)	0.099*** (0.010)	0.132*** (0.011)	0.126*** (0.010)
2001.year	0.159*** (0.011)	0.153*** (0.011)	0.186*** (0.012)	0.175*** (0.011)
2002.year			0.257*** (0.015)	0.236*** (0.012)
Logarithm of enrollment		-0.137** (0.056)		-0.201*** (0.051)
Percentage of free lunch students		-0.100 (0.062)		-0.186** (0.074)
Constant	8.691*** (0.006)	9.591*** (0.365)	8.659*** (0.007)	10.004*** (0.336)
Observations	512	454	768	694
R-squared	0.515	0.557	0.610	0.688
Number of id	128	126	128	127

Note. School district level fixed effect is included in each model above. Robust standard errors in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

Table 6: DID Estimation of the School Finance Reform in New Hampshire with Comparison State Rhode Island on per Pupil Spending in Poor School Districts

	Four Years of Data		Six Years of Data	
	No Control (1)	Control (2)	No Control (3)	Control (4)
Reform	0.023 (0.019)	0.016 (0.018)	0.047** (0.023)	0.034 (0.021)
1998.year			0.023*** (0.006)	0.031*** (0.006)
1999.year	0.039*** (0.008)	0.050*** (0.008)	0.061*** (0.011)	0.082*** (0.011)
2000.year	0.053*** (0.015)	0.072*** (0.014)	0.060*** (0.021)	0.091*** (0.019)
2001.year	0.106*** (0.016)	0.125*** (0.016)	0.106*** (0.021)	0.139*** (0.020)
2002.year			0.171*** (0.023)	0.189*** (0.021)
Logarithm of enrollment		-0.717*** (0.147)		-0.690*** (0.101)
Percentage of free lunch students		0.198 (0.218)		0.109 (0.167)
Constant	8.695*** (0.008)	13.401*** (0.968)	8.668*** (0.010)	13.280*** (0.677)
Observations	264	233	396	357
R-squared	0.418	0.609	0.550	0.719
Number of id	66	66	66	66

Note. School district level fixed effect is included in each model above. Robust standard errors in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

Table 7: DID Estimation of the School Finance Reform in New Hampshire with Comparison State Maine and Rhode Island on per Pupil Spending in Rich School Districts (Averaged Data)

	Four Years of Data		Six Years of Data	
	No Control (1)	Control (2)	No Control (3)	Control (4)
Reform	-0.031* (0.018)	-0.063*** (0.022)	-0.018 (0.020)	-0.047* (0.026)
Time	0.072*** (0.013)	0.099*** (0.020)	0.090*** (0.013)	0.117*** (0.022)
Logarithm of enrollment (6years)				-0.080 (0.071)
Percentage of free lunch students (6years)				0.174 (0.218)
Logarithm of enrollment (4years)		-0.081 (0.058)		
Percentage of free lunch students (4years)		0.122 (0.206)		
Constant	9.134*** (0.005)	9.570*** (0.323)	9.115*** (0.005)	9.533*** (0.429)
Observations	278	244	278	242
R-squared	0.244	0.277	0.336	0.351
Number of id	139	127	139	127

Note. School district level fixed effect is included in each model above. Robust standard errors in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

Table 8: DID Estimation of the School Finance Reform in New Hampshire with Comparison State Maine on per Pupil Spending in Rich School Districts

	Rich School Districts			
	Four Years of Data		Six Years of Data	
	No Control (1)	Control (2)	No Control (3)	Control (4)
Reform	-0.032* (0.018)	-0.068*** (0.023)	-0.023 (0.021)	-0.073*** (0.027)
1998.year			0.035*** (0.009)	0.032*** (0.008)
1999.year	0.036*** (0.011)	0.030*** (0.008)	0.074*** (0.014)	0.066*** (0.010)
2000.year	0.067*** (0.016)	0.105*** (0.023)	0.101*** (0.017)	0.148*** (0.026)
2001.year	0.109*** (0.016)	0.136*** (0.025)	0.136*** (0.018)	0.173*** (0.027)
2002.year			0.123*** (0.019)	0.155*** (0.026)
Logarithm of enrollment		-0.240*** (0.074)		-0.285*** (0.069)
Percentage of free lunch students		-0.344** (0.173)		-0.272* (0.149)
Constant	9.103*** (0.007)	10.545*** (0.433)	9.073*** (0.009)	10.737*** (0.399)
Observations	508	462	762	693
R-squared	0.152	0.294	0.159	0.279
Number of id	127	122	127	122

Note. School district level fixed effect is included in each model above. Robust standard errors in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

Table 9: DID Estimation of the School Finance Reform in New Hampshire with Comparison State Rhode Island on per Pupil Spending in Rich School Districts

	Four Years of Data		Six Years of Data	
	No Control (1)	Control (2)	No Control (3)	Control (4)
Reform	-0.042 (0.027)	-0.024 (0.020)	-0.030 (0.034)	-0.011 (0.024)
1998.year			0.009 (0.010)	0.020** (0.009)
1999.year	0.004 (0.009)	0.023*** (0.007)	0.021 (0.013)	0.050*** (0.012)
2000.year	0.069** (0.028)	0.076*** (0.023)	0.077** (0.034)	0.092*** (0.027)
2001.year	0.094*** (0.024)	0.089*** (0.019)	0.091*** (0.030)	0.104*** (0.023)
2002.year			0.133*** (0.034)	0.140*** (0.028)
Logarithm of enrollment		-0.718*** (0.060)		-0.834*** (0.060)
Percentage of free lunch students		-0.159 (0.199)		0.347** (0.163)
Constant	9.047*** (0.008)	13.737*** (0.378)	9.048*** (0.011)	14.205*** (0.372)
Observations	260	237	390	354
R-squared	0.167	0.600	0.168	0.586
Number of id	65	65	65	65

Note. School district level fixed effect is included in each model above. Robust standard errors in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

VITA

Yangyong Ye was born in a village in Henan province of China. He completed his bachelor degree in the area of Agricultural Resources and Environmental Science in 2007 at the China Agricultural University. And he earned his master degree in Education with emphasis on the justiciability of educational law in China at the Beijing Normal University in 2010. He is working at the Renmin University of China as an assistant professor.