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THREE ESSAYS ON QUASI-EXPERIMENTAL ANALYSES OF LOCAL POLITICAL ECONOMY

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BY
MASATAKA HARADA

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To my family.

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I literally started my Ph.D. program from the scratch. I did not even know what budget constraint meant. For such a person to finish this dissertation would not have been possible without the guidance and the help of several individuals whom I can never sufficiently thank.

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ABSTRACT

This dissertation is comprised of three independent essays that examine how exogenous law enforcement, historical events, and neighborhood environments shape public policies and political behaviors. The topics of each essay — strategic policy making, social capital, and Tiebout sorting — mirror the major policy issues currently faced by local governments in the U.S., specifically the accumulation of government deficits due to ethnic conflict and political competitions, the decaying democratic system due to eroding social capital, and residential segregation based on race and income. The causal analyses of these theories are often hampered by the unobservability of strategic intention, the time-invariability of social capital, and the endogeneity problem due to residential sorting. To overcome these challenges, my essays utilize exogenous variations of historical events, empirical implication of formal models, and various quasi-experimental methods, which identify the causal mechanisms of interest with a solid statistical foundation.

CHAPTER 1

THE VOTING RIGHTS ACT OF 1965 AND STRATEGIC POLICYMAKING IN THE SOUTH

ABSTRACT

This study uses historical data from the 1965 Voting Rights Act (VRA) to examine the effect of strategic policymaking on policy outcomes. Strategic policymaking refers to the exploitation of future policy resources by an incumbent government when it anticipates the policy change by a future government. In the South, the segregationist governments immediately after the enactment of the VRA still stayed in office but anticipated the future policy change that would result from minority voters acquiring the right to franchise. This political context provides an ideal setting for testing the theory of strategic policymaking. Through analysis of county panel data analysis from the 1960s, this study finds that segregationist governments with large budgets increased long-term debts, education spending, and highway spending significantly when compared to the rest of the country. This finding supports a version of strategic policymaking, namely strategic use of debts, and is consistent with anecdotal evidence indicating that resistance to school integration through the creation of all-white suburban schools is one of the primary motives for bond issues.

1.1 Introduction

This study uses the dynamic change in the voting population due to the Voting Rights Act of 1965 (henceforth VRA) as a source of exogenous variations and investigates the policy outputs of local governments in the United States during the 1960s to uncover the role of strategic policymaking in fiscal deficits. Strategic policymaking refers to the exploitation of future policy resources by an incumbent government when it anticipates a policy change by the future government. That is, the study posits that an incumbent government seeks to spend as much budgets as possible for its supporters while it is in a position to do so.

Past studies have shown that the VRA contributed to expansion of state and local governments. Prior to passage of this legislation, state and local governments in the South provided minimal public services (Key 1984[1949]).[1] As a result of the VRA, this situation began to change in the late 60s. The VRA also delegitimized any devices for vote denial, enfranchised poorer minority voters who preferred greater spending on redistributive policies, and thereby moved median policy to a fiscally liberal direction (Husted and Kenny 1997). For example, Husted and Kenny (1997) report that state per capita welfare expenditures doubled between 1964 and 1976. More generally, the expansion of the franchise is considered one of the major factors in the growth of government (Abrams and Settle 1999; Lott and Kenny 1999).

One important yet unremarked aspect of the VRA is that state and local governments in the South experienced a unique transitional phase that provides an ideal environment for studying strategic policymaking. The passage of key legislation during the mid-60s made Southern segregationist governments expect that future policy change was inevitable. In order to stay in power they would need to incorporate the preferences of new minority voters. Otherwise, they would be forced to hand their offices to a new government that better represented an expanded voting population. In either case, segregationist governments were

1. See Donohue, Heckman and Todd (2002) for an example of minimalistic education policy in the South.

able to determine public policies anticipating that in the future these policies would change. Public policies made from such anticipation must necessarily be quite different from both preceding minimalistic policies and subsequent welfare-oriented policies.

In fact, this transitional period is one of the eras during which strategic policymaking was most commonly observed. About a week after passage of the VRA, Congress of Racial Equality, a civil rights organization, filed a lawsuit that sought to block the sale of $6 million of school bonds in Monroe, Louisiana unless the city proposed a plan for school desegregation. The central issue in the suit was that "the bond sales will be used to perpetuate alleged discriminatory segregationist practices" (Allan 1965). Moreover, voters in Jackson, Mississippi passed a school bond issue in 1964. In Jackson, the passage of a school bond issue was quite rare at that time because the majority of voters were white, but the public schools were, and still are, mainly for blacks. In fact, it was not until 1991 that the city again passed a school bond issue. The reason the bond issue in 1964 passed was to build a new school "to provide an 'escape hatch' for white parents who planned to remove their children (Glaser 2002)."[2]

Through analysis of county panel data analysis from the transitional period, this study finds that local governments covered by Section 5 of the VRA with large per capita budgets increased long-term debts, education expenditures, and highway expenditures at higher rates than those in the rest of the United States. Consistent with one version of the formal theories of the strategic use of debts, these spending patterns indicate that governments with both strong strategic motivations and capacities to draft flexible budgets, issued more debts in order to tie the hands of future governments and spent the borrowed money to continue segregation practices.

The remainder of the paper is organized as follows. In the following section, I provide

2. The latter case happened before the VRA became effective, but some radical counties might well have preempted the policy changes due to the Civil Rights movement and thus committed to strategic policymaking.

some details on the VRA, and explain the desirability of the VRA as a subject to study strategic policymaking. The third section introduces four theories that might explain the spending patterns during the transitional period and corresponding predictions. The fourth section describes the research design, the fifth section tests these theories, and the sixth section summarizes and draws conclusions.

1.2 The Voting Rights Act of 1965

The Voting Rights Act of 1965, enacted on August 6, 1965, was a groundbreaking law in the sense that it granted the federal government unilateral power to intervene in election administration of state and local governments. This made the VRA one of the most effective desegregation policies the era (Davidson 1994).[3] One important feature of the VRA is that it banned literacy tests and put tough restrictions on poll taxes in state elections.[4] An abundance of studies shows that these institutional barriers suppressed minority voting (Key 1984[1949]; Ashenfelter and Kelley 1975; Filer, Kenny and Morton 1991; Alt 1994). Another important feature of the VRA is that it designated a state or county government as a covered jurisdiction if it implemented any discriminatory test or device, *or* if voting registration rates or voting turnout in the presidential election of 1964 among the voting population was lower than 50%. The 1965 VRA designated the entire states of Alabama, Georgia, Louisiana, Mississippi, South Carolina, and the Commonwealth of Virginia as well

3. The VRA was enacted as a five-year temporary legislation, but this study assumes that government officials anticipated the VRA would not lapse in five years without substantial improvement of minority representation. In fact, the VRA was enacted through the bipartisan support of non-southern members of Congress in 1965. The Supreme Court ruled the VRA was constitutional (e.g., *South Carolina v. Katzenbach*, 1966), and so the VRA was extended in 1970, 1975, 1982 and 2006.

4. Specifically, even after the passage of the 24th Amendment in 1964 banning poll taxes, some Southern states continued to implement the poll tax until the Supreme Court struck it down in 1966. To cope with their noncompliance, Section 10 of the VRA enabled the Attorney General to "institute", or challenge, the use of poll taxes, thus effectively restricting its use.

as 40 out of 100 counties in North Carolina as covered jurisdictions.[5] Once designated, covered jurisdictions need pre-clearance from the federal government for any change in rules that could affect public elections. Also, upon the request of residents, federal officials can review the voter registration process and monitor the elections in covered jurisdictions. Thus, the VRA severely limited the ability of segregationist governments to discourage minority representation.

These features let segregationist governments anticipate the significant increase of the minority voting population following the enactment of the VRA. Indeed, black voter registration rates among VRA-covered states rose from 27.9% before the VRA to 54.7% after it passed (Rosenberg 2008).[6] Then, it is reasonable to assume that segregationist governments expected that minority voters would start affecting policymaking in the near future.[7]

Compared to analyzing regime changes in mundane electoral cycles, focusing on the political context around the VRA has the following empirical advantages. First, it enables us to quantify a government's strategic motivation in response to the future change in voting population. The VRA let Southern segregationist governments anticipate that they would lose some electoral competitiveness. Because historic governmental reluctance to enfranchise minorities indicates their difficulty in acquiring electoral support from minority voters, any policy changes observed immediately after the enactment of the VRA were intended

5. The composition of covered jurisdictions does not change for the period analyzed in this study. See `http://www.justice.gov/crt/about/vot/sec_5/covered.php` for the complete list of covered jurisdictions and the time they were designated. Specifically, several counties, such as Monterey County and Yuba County in California, which were added to the list of covered jurisdictions in March 1971, should not be classified as covered jurisdictions during the entire period of analysis because the Census in 1972 reflects the policymaking earlier than March, 1971.

6. The percentage figures were calculated with population weights. 40% of North Carolina's population is included in population figures for the VRA-covered states.

7. Even if the median voter theorem does not hold, the future regime change is sufficient, but not necessary, for policymakers to have such an expectation because it does not require minority voters to have a majority status in order to let politicians reflect their policy preferences. In fact, Cameron et al. (1996) shows that in the South it takes about 40% black voting-age population for a black candidate to win in a House election with a half chance, and less than a 10% black voting-age population for a Democratic candidate (after realignment) to win with a half chance.

not to acquire electoral support from new minority voters but to benefit the supporters of the segregationist governments. Putting it frankly, these governments would have behaved more nicely to minority voters if they had thought they could acquire minority votes after enfranchisement.

Second, the political context around the VRA offers a *once-and-for-all* change in voting population because the devices for vote denial are unlikely to be restored in the future. This context maximizes the possibility of detecting strategic policymaking, which otherwise is, a secretive and latent process. Intuitively, the situation in which the current government loses the next election but retakes power in the future and the situation in which it never regains power once lost should provide different strategic incentives to any current government. The segregationist governments after VRA characterized by the latter situation are more likely to engage in predatory behavior because cooperation does not yield long-run benefits. This once-and-for-all situation also better fits the formal models on fiscal policy that rely on a finite-stage game (Persson and Svensson 1989; Tabellini and Alesina 1990). On the other hand, empirical studies on fiscal policies typically analyze the former situation in which political parties constantly compete with each other to seize power (Grilli, Masciandaro and Tabellini 1991; Crain and Tollison 1993; Alt and Lowry 1994; Franzese 2000; Pettersson-Lidbom 2001; Lambertini 2009). In such a situation, the governments in power may choose not to exploit the financial resources of future governments. This would allow the governments to avoid the prisoner's dilemma scenario, in which the government in power exploits future resources until it goes bankruptcy. It would also prevent, should the government return to power, the need to pay back more debt principal than it originally issued. Thus, with the data for which regime changes are frequently observed, it can be hard to detect strategic policymaking or to determine whether a null finding indicates non-existence of strategic policymaking or a problem in the research design.[8]

8. This empirical difficulty does not mean that once-and-for-all regime changes never occur in mundane political cycles. For example, if a party leader has an individual preference distinct from that of his or her

1.3 Theories

Policy intention is one of the hardest things to quantify. Aggregated data do not contain any information about policy intention. Policy makers are also unlikely to reveal their true intents particularly when their policies are predatory or strategic, as was the case for the segregationist governments. However, the information about which governments prioritized what policies in which period illuminates the black box of policymaking. This section presents four theoretical policymaking accounts of Southern governments during the transitional period immediately following passage of the VRA and corresponding predictions about how each policy output might *change* from the pre-VRA period.[9]

The following discussion excludes the theory of fiscal illusion, which posits that a government overspends its budgets because it tends to evaluate marginal benefits from spending more than marginal costs (Buchanan et al. 1987; Mueller 2003). Various factors contribute to fiscal illusion, such as miscalculation of the future revenue stream and/or amortization plan, irrational discount on future marginal benefits, or lack of understanding about the importance of fiscal discipline. These accounts, however, require researchers to apply different behavioral principles to different actors a posteriori, which does not weather rigorous academic scrutiny (Tabellini and Alesina 1990; DeWolff 1998).

1.3.1 No strategic policymaking

Let us first consider the situation in which strategic policymaking was absent, that is, *the expectation of* the future change in the voting population did not affect the policymaking of the segregationist governments. Then, the segregationist governments switched their segregation policy to a more liberal one without experiencing the transitional phase. This

party, he or she might perceive his or her incumbency as a once-and-for-all opportunity to carry out her policy goals, and may be motivated to commit strategic policymakings.

9. The predictions focus not on the *level* of spending but on the *change* in spending to make the predictions compatible to the difference in differences estimation.

situation serves as a baseline with which other theoretical accounts are contrasted.

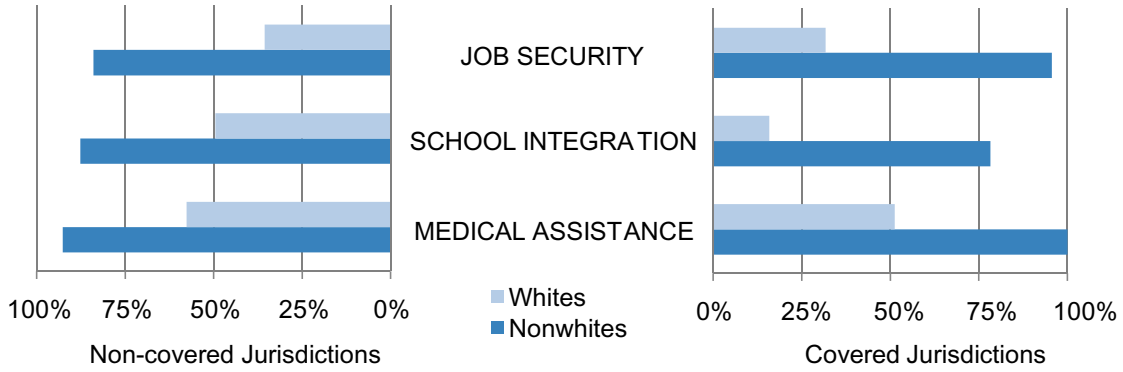

Figure 1.1: Difference in issue positions between white and nonwhite voters by covered jurisdictions and non-covered jurisdictions. *Source*: American National Election Studies Cumulative Data File, 1948-2004 (ICPSR 8475).

The primary goal for segregationist governments in the 60s was to maintain racial homogeneity in their living environments. As indicated by the aforementioned anecdotes, one of their largest concerns was school integration. As indicated by Figure 1, there was deep cleavage on this issue between whites and nonwhites in VRA-covered states. Light blue (or light gray) bars represent the proportion of white respondents in the 1964 American National Election Studies (Sapiro, Rosenstone, and the National Election Studies 2007) who thought the federal government should commit to the issues labeled in the center, namely job security, school integration, and medical assistance. Dark blue (or dark gray) bars are the counterparts for black respondents. The policy preferences between whites and nonwhites differed for all policy areas. Above all, the preferences of those living in the VRA-covered states were strikingly different on the issue of school integration. This figure also indicates their strong abhorrence of redistributive policies such as medical assistance and job security. Furthermore, relocation to the suburbs was considered a versatile solution to avoid facing these differences (Oliver 2001). Backed by generous governmental support for home mortgage programs and tax deductions, developmental policies (e.g. investments in infras-

tructure, development of the Interstate Highway System) accelerated suburbanization in this period (Jackson 1987).

Thus, using Peterson's classic policy typology (1981) that distinguishes among developmental, redistributive, allocation, and education policy, the segregationist governments would have spent more on developmental and/or education policies than on redistributive policies during the transitional period. However, in terms of the *change* in expenditures, these spending items would have neither increased nor decreased since the pre-VRA period in the absence of strategic policymaking.

1.3.2 Strategic policymaking

The theory of strategic policymaking simply posits that a government spends the budget while it can on its preferred policies because it expects someone will repay any debts in the future.[10] Because such motivation does not arise if the current government remains in office or a future government has the same policy preferences, the strength of the strategic motivation is determined as the product of the likelihood of a regime change and the expected difference between the two governments in policy preferences. Previous studies showed that the heterogeneities in age, race and ethnicity, income, and partisanship within a jurisdiction contribute to the expected difference in policy preferences (Alesina and Drazen 1991; McCubbins 1991; Heller 1997; Persson and Tabellini 2000).[11]

In this sense, the VRA provides the best context for studying strategic policymaking because it drastically changed the socioeconomic and political profile of the voting population. After enactment of the VRA, the voting population, particularly in the Southern states,

10. As modeled by Aghion and Bolton (1990), governments have an option to renege on debts, but this option is often difficult to exercise in practice because the governments trying to renege cannot convince voters that responsibility is entirely the responsibility of the previous government.

11. See Alesina, Baqir, and Easterly (1999) for an alternative theory that claims ethnic heterogeneity leads to less spending.

became poorer, racially more heterogeneous, and politically more liberal. If the theory of strategic policymaking is true, the segregationist governments would anticipate policy change in the future and would be motivated to spend more on their preferred policies while they stayed in power, particularly in the policy areas that might not attract enough attention from the future government.

Thus, the segregationist governments with strategic motivation should have spent more on education and developmental policies in the transitional period than they did in the pre-VRA period. However, these same governments should not have changed the level of redistributive policies as they had been kept at a minimal level.

1.3.3 Strategic use of debts

The theory of strategic use of debts (henceforth SUD) is a subset of strategic policymaking motivated by the following concern. Although the theory of strategic policymaking is intuitive and simple, it cannot explain why strategic interaction reaches equilibrium. If the current government spends the budget in the way it wants, the future government will also spend the budget on its preferred policy in retaliation. Furthermore, anticipating the response of the future government, the current government would borrow as much as possible thereby depleting future financial resources in order to prevent retaliation. Thus, strategic policymaking is fraught with bankruptcy. However, this disequilibrium is rarely observed in real-world politics.

The necessity of exploring equilibrium conditions for strategic policymaking motivated formal theorists to develop a series of studies on the SUD (Persson and Svensson 1989; Aghion and Bolton 1990; Alesina and Tabellini 1990; Tabellini and Alesina 1990; DeWolff 1998; Persson and Tabellini 2000). Despite wide variations in their settings and assumptions, theorists commonly suppose that two policy makers have different preferences over two types of goods and that the marginal utilities from these goods are diminishing. The latter assumption of

the diminishing marginal utility[12] plays a critical role in keeping strategic policymaking in the equilibrium path because spending an entire budget on the government's preferred policy does not maximize its long-term utility unless the policy preferences between two competing governments are completely opposite.

Each theory of SUD typically requires other important assumptions for its existence. To find such an assumption and interpret its empirical implication without distortion, I chose the formal theory that most closely fits the political context during the transitional period of the VRA, namely Tabellini and Alesina (1990).[13] Their model is a two-stage perfect information game between the current government and the future government, both of which represent the policy preferences of the median voter in each period. A critical feature of their theory is that although voters' preferences do not change over time, the current government is motivated to borrow from the future government due to an exogenous shock to the voting population that occurs between two periods and the resulting difference in the median policy preferences. This description of the game closely resembles the political situation in the South during the 60s, in which the segregationist governments, representing whites, expected a major increase in the minority voters due to the enactment of the VRA.

In their model, the key assumption that guarantees the formal existence of SUD is that *a government represented by the median voter becomes less risk-averse as its consumption of public goods increases.*[14] Just like the law of diminishing marginal utility, risk-aversion lets a government allocate its budget equally to different goods. Therefore, this assumption implies that as total per capita spending goes down, a government becomes more risk-averse,

12. Concave utility function and risk-aversion have very similar implications for the diminishing marginal utilities although their formal definitions are different.

13. The summary of their model is presented in Appendix A.

14. Note that this condition only guarantees that the optimal level of borrowing in the first stage is positive. To observe SUD *empirically*, a government may need to satisfy this condition at higher level. Yet, we cannot distinguish whether a null finding originates from its failure to satisfy the formal condition or its failure to satisfy the threshold for SUD to be empirically detectable.

and as it becomes more risk-averse, it more equally allocates its budget to different goods.[15] That is, a small government tends to maintain an equal allocation of public goods. This is no more than a formal statement of typical policymaking by a government facing a tight budget constraint. In fact, Tabellini, the first author of the article, gives the following intuitive account.

> [W]hen resources are scarce we all agree that there are some priorities, such as spending on health and defense, but when resources abound, we can afford to spend in different ways and choose a more divergent mix of public goods. (354; Persson and Tabellini 2000)

Knowing this property of the common utility function, the current government can effectively force a future government to allocate its budgets equally to all essential policies by creating deficits, thus forcing a future government to face tighter budget constraints and increasing its degree of risk-aversion.[16]

An empirical implication of this assumption is that the size of the government has a positive multiplier effect on strategic policymaking. The smaller the size of a government, the more difficult it becomes to implement strategic policymaking because a small budget does not allow the current government to reflect its strategic motivation in policies. Thus, budget allocations of small governments are similar regardless of whether they have strategic motivations. On the other hand, a big government, which is less risk-averse, has the capacity to translate its strategic motivation into action. In sum, the theory of SUD proposes the same predications as strategic policymaking, but adds that as government size becomes larger, the greater the differential effect of strategic policymaking.

15. The size of the government is defined on a per capita basis because the formal model is solved to maximize the median voter's individual utility function.

16. This account holds true even when the future government forgoes the retirement of the debts through refinancing, because it will still face a large amount of interest payment and a small capacity (or a high premium) to issue new bonds.

1.3.4 Early median voter shift

Finally, let us consider the case in which a large majority of minorities had already been enfranchised by the time the policy outputs were recorded in the 1967 census, and the median voter of this new voting population decided public policies. This could either be a result of rapid progress in minority voter registration or the Southern governments' commitment to the policies preferred by minority voters in expectation of future support from them. Although the historical facts do not necessarily correspond with these accounts, they pose an important observational equivalence to consider because school integration might be also accompanied by the increase in education spending and long-term debts.

However, the key distinction between early median voter shift and strategic policymaking is that the former was also accompanied by the increase in redistributive policies. New minority voters enfranchised by the 1965 VRA often needed more public assistance than did the existing voting population (Husted and Kenny 1997, Lott and Kenny 1999, Besley and Case 2003). Thus, if new minority voters affected policymaking during the transitional period, we should observe an increase in welfare expenditure as well as education expenditure and long-term debts.

1.3.5 Summary of the prediction

The fourth column in Table 1 summarizes the predictions of the four theories discussed above. If strategic policymaking exists, we expect to observe increases in education and developmental policies as well as long-term debts. If only the subset of strategic policymaking (i.e. SUD) is supported, these increases would be observed only among the governments with large budgets.[17] If welfare expenditures also increased, the result is likely to be caused

17. The prediction on long-term debts directly originates from the formal theory. On the other hand, the predictions on policy outputs are inferred from the prediction on long-term debts. This is because the borrowed money should have been spent on preferred policies of the segregationist governments.

by the early median voter shift. If none of these mechanisms was at work, we would not observe any increase in policy outputs.

Theory	Actors	Policies	Observed periods: from pre-VRA to transitional	Observed periods: from transitional to post-transitional
No Strategic Policymaking	Segregationist Governments	Education	→	↑
		Developmental	→	→
		Longterm Debts	→	n/a
		Redistibutive	→	↑
Strategic Policymaking	Segregationist Governments	Education	↑	→
		Developmental	↑	↓
		Longterm Debts	↑	n/a
		Redistibutive	→	↑
Strategic Use of Debts	Segregationist Governments with Large Budgets	Education	↑	→
		Developmental	↑	↓
		Longterm Debts	↑	n/a
		Redistibutive	→	↑
Early Median Voter Shift	Segregationist Governments	Education	↑	→
		Developmental	→	→
		Longterm Debts	n/a	→
		Redistibutive	↑	→

↑: The spending increased more in the segregationist governments than in the rest of the country.
↓: The spending increased less in the segregationist governments than in the rest of the country.
→: The spending increased at the same rate in the segregationist govs as in the rest of the country.
n/a: The theory does not provide clear predicitons.

Table 1.1: The summary of the predictions from each theory.

Finally, the fifth column in Table 1 summarizes the policy change from the transitional to the post-transitional period. If strategic policymaking did not occur during the transitional period, we would observe an increase in education expenditures among Southern governments during the post-transitional period, but not any change in developmental policy. In this instance, these governments should have switched from minimalistic segregation policies to

more liberal ones, such as school integration, without experiencing the transitional phase. On the other hand, if the median voter shift occurred in the post-transitional period, the predictions for the early median voter shift apply to the change in policy outputs from the transitional period to the post-transitional period. That is, we would observe an increase in welfare expenditures and a decrease in developmental expenditures but would not observe any change in education expenditures because they had already been high during the transitional period to build an “escape hatch". In addition, if early median voter shift were the case, no significant increase or decrease in policy outputs would be observed thereafter.

1.4 Research Design

1.4.1 Data

The main purpose of this study is to examine the effect of strategic policymaking on policy outcomes. The counties in the United States from 1957 to 1972 serves this purpose quite well.[18] Because the primary function of local governments in the United States is to produce local public goods, and because bond issues are often subject to a referendum, their characteristics can be mapped in a single-dimensional policy space. Moreover, market mechanisms such as the capitalization of local public goods to home values (Fischel 2001) make local governments responsive to the policy preferences of the median voter.

County-level analysis has several other desirable features for empirical analysis. The sample size is large with little sampling bias.[19] The county borders do not change due to municipal incorporation and secession. Also, within any given county, the range of services is nearly the same. This last point is particularly important, because if governments provide

18. Counties in Alaska and Hawaii were excluded. The set of years included in the sample varies by analysis.

19. For example, cities were routinely sampled only when they each had 25,000 or more inhabitants.

a different range of services, a given amount of per capita budget may differently affect their levels of risk-aversion. Some counties experienced an untypically large demographic change. They were excluded from analysis because the changes in these counties do not precisely represent within-unit variations.[20] In addition, observations that have any missing values are excluded from the analysis. These processes drop the total number of observations per year from 3,111 to 2,784.

Data are collected from the following sources. Finance data were collected from the Census of Governments for 1957, 1962, 1967, and 1972. These censuses aggregate fiscal statistics for all local governments within a county, including municipalities, townships, school districts, and other special districts. That is, the statistics of one county present composite figures for several local governments.[21] Socioeconomic data are collected from County Data Books for 1962, 1967, 1972, and 1977 (Haines and ICPSR 2010), which contain the census data collected five years ago.

1.4.2 Timing

With a few reasonable assumptions, the timing of the census collection provides one of the best divisions to distinguish the pre-VRA, transitional, and post-transitional periods. Ideally, the division between the pre-VRA and transitional periods should be at the time when local governments had motivation for SUD, but this timing is unobservable and may vary by government. To establish a reasonable division, the following three assumptions are made according to the standard procedure of budgetary implementation:

20. Specifically, counties were excluded from the analysis if the area of a county changed more than 5% or if populations changed more than 2.5 deviations away from the average change rate between any two successive censuses.

21. The budgets of the governments that have few political overtones, such as sewage management districts, only account for a small fraction of the total budgets. Thus, I assume inclusion of these governments should not affect the finding. Moreover, because this study supports the hypothesis on SUD, the aggregation of multiple statistics does not change the study's substantive findings.

1. It takes one fiscal year for local governments to draft budget proposals, and they implement these proposals in the following fiscal year.

2. Segregationist governments had an expectation of change in the voting population no later than August 7, 1965, when the VRA was enacted.

3. Segregationist governments could reflect their strategic motivation in the following year's budget proposal immediately after the enactment of the VRA.

Given these three assumptions, if part of the budget was drafted after August 7, 1965, for the purpose of this study it is considered to have been drafted during the transitional period. The timing for collecting financial data by the 1967 Census of Governments provides the ideal division for these three periods.[22] Specifically, the data minimize the possibility of an early median voter shift due to local elections after the VRA went into effect because this census reflects fiscal decisions for the period immediately following VRA enactment in 1965.[23] Financial data of the 1962 Census of Governments, the nearest preceding fiscal year available, are used as the data for the pre-VRA period. Finally, the data of the post-transitional period are collected from the 1972 Census of Governments.

Because policy outputs in the fiscal year of 1971 were recorded the 1972 Census, if they were drafted in the preceding year, the 1972 Census reflects the policy decisions in the end of the fiscal year 1970. Then, by the end of the fiscal year 1970, all governments experienced at least one election since the enactment of the VRA. Thus, the statistics of the 1972 Census reflect the policy preferences of the new voting populations.

22. Further detail on the timing issue is discussed in Appendix B.

23. Only one county (Clinch, GA), for which the fiscal year ends on July 31, was dropped from the sample because it could not execute strategic policymaking according to the above assumptions. The coefficients remain unchanged, regardless of whether the county is included in the analysis.

1.4.3 Variables

The dependent variables are logged per capita expenditures on education, welfare, and highways and logged per capita long-term debts outstanding. The first three variables make up the majority of the general expenditures of counties.[24] I consider welfare and highway expenditures as representative statistics of redistributive and developmental policies. The statistics of long-term debts is the aggregation of general obligation (GO) and revenue bonds, which make up 60% and 40% of the long-term debts of municipalities, respectively (Census of Governments 1967).[25] GO bonds are issued to capital investments from which direct collection of usage fees is difficult, and they are typically repaid by property taxes. Because the interest payments and the retirement of debt principal constrain future budgets, they would be the ideal dependent variable to test SUD. Revenue bonds can be issued for projects for which the financing is repaid by the projects themselves (e.g. tollways). The type of enterprises that can be financed by revenue bonds is limited. Thus, they have limited impact on a government's future budget constraints and may not be as responsive as GO bonds to policy makers' strategic motivation.[26] Short-term debts are mostly issued to make up an unforeseen revenue shortage and may not capture the dynamics of the regime change.

The key explanatory variables are the dummy variable that identifies the segregationist governments and the proportion of the nonwhite population. These two variables both contribute to the expected difference in policy preferences and the likelihood of regime change between the pre-VRA and the transitional periods, which determine the strength of strategic motivation. The dummy variable of the segregationist governments is coded "1" if the county

24. The percentage of spending breakdown for the average county in the 1967 Census of Governments is as follows: 18% education, 21% welfare, and 16% highway expenditures.

25. These bonds are also referred to as full faith and credit bonds or nonguaranteed bonds.

26. Here, issue of revenue bonds is assumed not to loosen budget constraints of the future government. As long as this assumption holds, the inclusion of revenue bonds in long-term debts causes either no bias or attenuation bias, which does not change the substantive findings of this study, as we still find significant evidence for the theory of SUD.

was designated as a covered jurisdiction in the VRA. All counties in Alabama, Georgia, Louisiana, Mississippi, South Carolina, and Virginia, as well as 40 out of 100 counties in North Carolina, were covered by the VRA, which identifies 501 counties out of 2,784 as VRA-covered counties. Given our lack of knowledge about how far the effect of the VRA spread, I also use an alternative coding for the segregationist governments according to historical definitions of the South. Specifically, the variable takes "1" if the county is a part of the eleven former Confederate States of America. The seven states covered by Section 5, along with Florida, Texas, Arkansas, and Tennessee constitute the Confederate States of America, which identifies 992 counties out of 2,784 as Southern counties.[27]

Control variables include county and year fixed effects as well as socioeconomic variables. Importantly, county fixed effects, account for all observed and unobserved five-year time-invariant unit characteristics such as party structure, political culture, and any time-invariant county- and state-level confounding factors such as adoption of home rule or restrictions on rolling over the deficit. Year fixed effects control for national time trends. Furthermore, within-counties, time-varying socioeconomic trends are accounted for with the following control variables: median family income, unemployment rate, the ratio of urban population, the ratio of the elderly population, land area, population, and rate of population growth for ten years. A similar set of control variables is found elsewhere (usually without unit fixed effects) in the empirical studies of local governments (Oliver 2001; Berry 2009).

Variables are transformed in several ways. First, all dependent variables are transformed into their natural logarithms because the same monetary increase in expenditures or debts can represent different policy preferences, depending on the size of the baseline budgets.[28]

27. These key explanatory variables receive a reasonable concern that the interaction term between these two would better capture an expected discontinuity of policy preferences. However, as presented in Table A3, this variable does not have a significant impact on policy outcomes due to multi-collinearity. As discussed in Appendix C in detail, these two key explanatory variables represent the same idea, but their correlation coefficient is 0.673, which is almost the maximum possible value given the value assignments of these variables. Thus, entering this interaction term as well as its main effect variables suffers from strong multi-collinearities.

28. Zero values in the pre-transformed variable are replaced with the minimum positive value of the pre-

This transformation changes the way regression coefficients are interpreted from a monetary to a percent basis. All pecuniary variables are adjusted with a GDP deflator and shown in prices of 2000. Control variables are linearly interpolated so that they represent the condition of the one year before policies were implemented. Due to limitation of space, descriptive statistics are presented in Appendix D.

1.4.4 Estimation Methods

Excepting the theory of SUD, the hypotheses are tested with difference in differences estimation (henceforth DD). DD is one of the most standard methods for so-called "natural experiments" because it distinguishes causal association from contemporaneous correlation and treatment-specific change from global time trends (Meyer 1995, Imbens and Wooldridge 2009). In this framework, the enforcement date of the VRA bounds the pre-VRA and transitional periods, and this division reflects whether policy makers could have an *anticipation* of the change in the voting population. With control variables, the DD is estimated as β in the following regression model.

$$\ln\left(policy\ output_{c,t}\right) = \alpha + \beta treatment_c \cdot \text{VRA}_t + \sum \gamma_k ctrl_{k_{c,t}} + t_t + u_c + \varepsilon_{c,t} \qquad (1.1)$$

where subscripts c denotes county and t denotes time, which is either 1962 or 1967 for the main analysis, and $policy\ output_{c,t}$ is one of the four dependent variables. The key explanatory variable is $treatment_c \cdot \text{VRA}_t$, which is the interaction term between the treatment variable, $treatment_c$, and the year dummy variable for the transitional period, VRA_t. The time-invariant treatment variable, $treatment_c$, is subsumed by county fixed effects. t_t and u_c represent year and county fixed effects, $ctrl_{k_{c,t}}$ denotes one of k control variables, β and γ_k are

transformed variable to prevent them from becoming missing cases. The fraction of the observations that have zero values in the dependent variables is very small. For example, only 19 counties out of 2,784 in the sample had zero long-term debts outstanding in 1967. The main findings do not change substantially regardless of whether this substitution is made.

regression coefficients, α is a constant, and $\varepsilon_{c,t}$ is an error term, which is clustered by county. This model tells us whether a segregationist government increased more spending after the VRA when it expected a future change in the voting population. Specifically, β represents how much *more* the percentage of per capita spending of the segregationist governments increased from the pre-VRA to the transitional period compared to their counterparts in the rest of the counties.

Although the theory of SUD tested in this study is only one version of the various theories of SUD, namely Tabellini and Alesina (1990), its equilibrium condition is rightly represented by the method of the triple differences. Their theory predicts that the government issues *more* debts when it expects *greater* differences in future policy preferences and has *larger* budgets that enable it to reflect its strategic intention in policies. Thus, the triple interaction term between the identifier of the transitional period, the identifier of segregationist governments, and the size of the budgets corresponds to equilibrium in their model. Formally, the triple difference estimator is defined as λ in the following regression model.

$$\begin{aligned}\ln\left(policy\ output_{c,t}\right) = \alpha + \beta treatment_c \cdot \text{VRA}_t + \delta \text{VRA}_t \cdot budget_c \\ + \lambda treatment_c \cdot \text{VRA}_t \cdot budget_c + \sum \gamma_k ctrl_{k_{c,t}} + t_t + u_c + \varepsilon_{c,t}\end{aligned} \tag{1.2}$$

where $budget_c$ denotes government size defined as logged per capita total general revenue in 1967.[29] Total general revenue in 1967 arguably best reflects the government size when SUD was feasible in budget drafting. However, it excluded the new debts issued in the same fiscal year and interest payments,[30] which are the primary source of potential endogeneity. In Appendix F, I further estimate Equation (2) replacing the variable of government size with its instrumental variable to address the potential endogeneity problem.

29. The treatment variable ($treatment_c$), the year dummy variable (VRA_t), the variable of government size ($budget_c$), and the interaction term between the treatment variable and the variable of government size ($treatment_c \cdot budget_c$) are subsumed by the county fixed effects.

30. The interest payments are manually subtracted from the statistics.

1.5 Results

1.5.1 Strategic policymaking

Table 2 presents the estimation results of Equation (1), with which the three theories– excepting SUD– are tested. Specifically, the table shows the DD estimates of being the segregationist government and the proportion of the nonwhite population during the transitional period on logged per capita education, highway, welfare expenditures, and long-term debts outstanding.[31] All estimates are controlled for by county and year fixed effects as well as socioeconomic control variables. The standard errors are clustered at the county-level to account for the positive serial correlation within the county.

The first three columns use the two indicators of the segregationist governments, and the fourth column uses the proportion of the nonwhite population. In the top panel, these key explanatory variables have statistically significant and positive effects on logged per capita education expenditures in all models. Specifically, Model (3) shows that when the indicator of covered jurisdictions and that of the former Confederate states are both entered into the model, the latter maintained the statistical power. This indicates that the Southern counties, not only VRA-covered counties, increased education spending. Substantively, the Southern counties increased per capita education expenditures 5.9% plus or minus 2.6% more than the rest of the country from the pre-VRA to the transitional periods.[32] The estimated 5.9%

31. The coefficients of control variables omitted from Table 2 are presented in Table A4.

32. $\exp(\beta) = \exp(0.057) \approx 1.0587$. The statement of uncertainty is based on a 95% confidence interval. Alternatively, using the estimate in Model (4), the average Southern counties with 23.5% of the nonwhite population increased per capita education expenditures 3.2% plus or minus 2.3% more than the rest of the country, of which the average ratio of the nonwhite population is 3.1%.

Dependent Variable	ln(per capita education expenditure)			
Exlanatory Variables	(1)	(2)	(3)	(4)
Year 1967 × Covered Jurisdiction	0.047 **		0.015	
	(0.018)		(0.020)	
Year 1967 × Former Confederate States		0.057 ***	0.050 ***	
		(0.013)	(0.014)	
Year 1967 × % Nonwhite Population				0.156 ***
				(0.058)
Dependent Variable	ln(per capita highway expenditure)			
Exlanatory Variables	(5)	(6)	(7)	(8)
Year 1967 × Covered Jurisdiction	−0.029		−0.017	
	(0.034)		(0.034)	
Year 1967 × Former Confederate States		−0.026	−0.018	
		(0.025)	(0.024)	
Year 1967 × % Nonwhite Population				−0.040
				(0.088)
Dependent Variable	ln(per capita long-term debts outstanding)			
Exlanatory Variables	(9)	(10)	(11)	(12)
Year 1967 × Covered Jurisdiction	−0.008		−0.051	
	(0.048)		(0.052)	
Year 1967 × Former Confederate States		0.045	0.068 *	
		(0.038)	(0.040)	
Year 1967 × % Nonwhite Population				0.026
				(0.133)
Dependent Variable	ln(per capita welfare expenditure)			
Exlanatory Variables	(13)	(14)	(15)	(16)
Year 1967 × Covered Jurisdiction	0.073		0.055	
	(0.079)		(0.096)	
Year 1967 × Former Confederate States		0.052	0.028	
		(0.061)	(0.074)	
Year 1967 × % Nonwhite Population				0.376 *
				(0.199)
Control Variables	Yes	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	Yes
Fiscal Years Included	1962, 67	1962, 67	1962, 67	1962, 67
Number of Counties	2784	2784	2784	2784
Number of Observations	5568	5568	5568	5568

Note: County-clustered standard errors are in parentheses.
* $p<.10$, ** $p<.05$, *** $p<.01$ in two-tailed test.

Table 1.2: The test of the theory of strategic policymaking.

increase in per capita education spending amounts to $28.9 per capita or approximately $1.1 million per county.[33] Between the1962 and the 1967 Censuses, the average per capita education expenditures among Southern counties increased from $361 to $530, and $28.9 accounts for 17.1% of this increase.

However, the results in the other panels suggest that the increase in education spending did not necessarily exploit the future policy resources and are not sufficient to verify the theory of strategic policymaking. In the second panel, none of the key explanatory variables shows any statistically significant effect on the change in per capita highway expenditures. Furthermore, in the third panel, only the coefficient of the dummy variable of the former Confederate states is even weakly statistically significant in Model (11). This indicates that only the counties in the peripheral South statistically significantly increased per capita long-term debts outstanding, which is inconsistent with the theory of strategic policymaking because the governments in the deep South should have a stronger strategic motivation.

The early median voter shift is also unlikely to be the underlying mechanism. In the bottom panel, the proportion of the nonwhite population has a weak but statistically significant and positive effect. Thus, the overall findings in Table 2 seem rather consistent with the explanation by the early median voter shift. However, the following analysis shows that the increase in welfare spending was caused by the actual policy shifts of the non-Southern counties where the nonwhite population was predominant, while the increase in the education expenditures was not.

In Figure 2, each dot represents an estimated coefficient of *Year1967* $\times$ *% Nonwhite Population* with a different subsample. Each subsample includes only the counties for which the percentage of nonwhite population in 1967 is below the cutoff point in the horizontal axis.[34]

33. See Appendix E for how to obtain $28.9. The average population of the Southern counties in 1967 was 37,868. Thus, $\$28.9 \times 37,868 = \$1,094,385$.

34. For example, the right end plot represents the coefficient estimated with all counties, which is identical to that of Model (4) in Table 2. Likewise, the left end plot represents the coefficient estimated with the counties for which the percentage of the nonwhite population in 1967 is below 10%.

In the upper panel, most of the coefficients on logged per capita education expenditures are statistically positively significant, and the size of coefficient does not diminish or even slightly increases after minority-majority counties are excluded from the sample. On the other hand, the coefficient on per capita welfare expenditures quickly becomes insignificant once several minority-predominant counties are excluded from the subsample. This indicates that large per capita welfare spending of these minority-majority counties makes the coefficient of Model (16) in Table 2 significant.

In sum, no single theory explains the results in Table 2, and they are likely to result from the combination of multiple mechanisms. The statistically significant coefficients in the analysis of education expenditures and the upper panel of Figure 2 indicate that the increase in education spending occurred in the Southern counties where white population were still dominant. However, the increase in education spending was not accompanied by an increase in highway expenditures or long-term debts, except for the peripheral South. Thus, the segregationist governments possibly increased education spending by using intergovernmental subsidies. Because the Civil Rights Act of 1964 stipulates the submission of an integration plan in order to receive federal subsidies, the segregationist governments might have chosen to spend the subsidies for school integration. However, the historical evidence shows that school integration did not start in the deep South during the transitional period, although it was already in progress in the peripheral South.[35] Perhaps, the Southern counties might simply have been trying to catch up with the other counties in the quality of public education during the transitional period.

35. See Figure A2 for the progress on school integration.

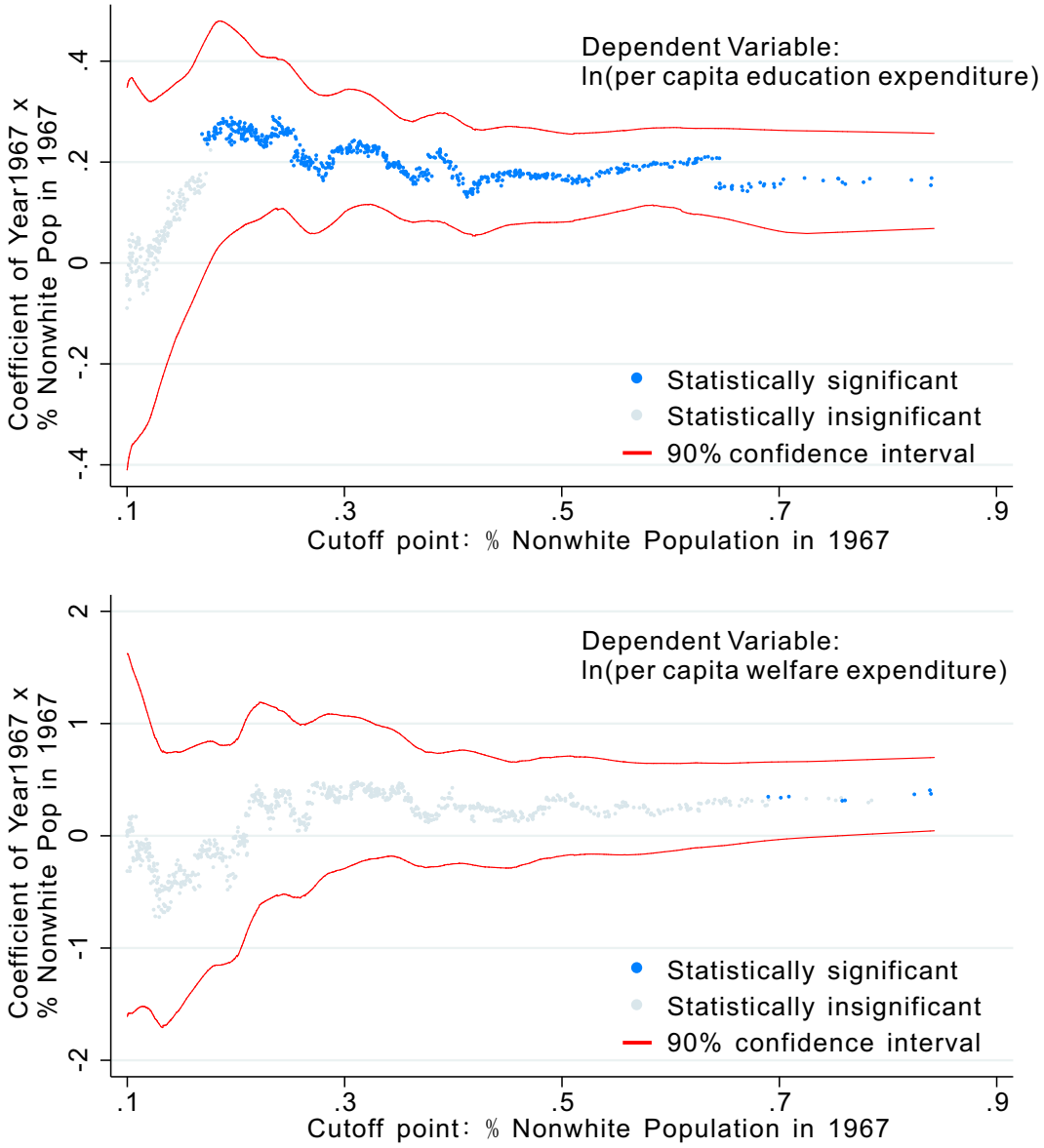

Figure 1.2: Testing whether the significant effect of the proportion of the nonwhite population is driven by the minority-majority counties.

1.5.2 Strategic use of debts

Table 3 shows the results of the test of SUD by Tabellini and Alesina (1990), for which the empirical implication is that governments with large budgets have more capacity to reflect their strategic motivation in policies. I test this prediction by examining whether the coefficient of the triple differences (λ) in Equation (2) is statistically positively significant. Although the theory of SUD only predicts the increase in long-term debts, the table presents the results of all four dependent variables because, if SUD is supported, the segregationist governments should have spent more on their preferred policies (i.e. education and highways).[36]

In the first panel, the triple interaction term is statistically positively significant only in Models (1) and (3). This indicates that only the counties that were covered by Section 5 of the VRA with a large budget issued more long-term debts per capita than their counterparts did. The statistically insignificant coefficient of the triple interaction term in Model (2) indicates that SUD was caused by factors shared among counties in the deep South, but not among counties in the peripheral South (e.g. the VRA-covered status that allowed the federal government's unilateral intervention in their election procedures or their strong desire to perpetuate segregation).[37]

The second and third panels show the same statistical patterns as the first panel. The triple interaction terms with the identifier of the VRA-covered counties in Models (5), (7), (9) and (11) have statistically significant and positive effects on education and highway expenditures. However, those with the identifier of the former Confederate states in Models (7) and (11) do not, when these two kinds of the triple interaction terms are both included

36. The coefficients of control variables omitted from Table 3 are presented in Table A5.

37. An insignificant coefficient in Column (4) is due to the unbalanced data and does not change the interpretation of the result. There are only a few counties that have a high percentage of nonwhite population and large per capita revenue. Those are mostly northern counties with big cities, where the minorities' policy preferences had already been incorporated.

Dependent Variable: logged per capita	long-term debts outstanding				education expenditure			
Exlanatory Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Year 1967 × Covered Jurisdiction × ln(per capita total revenue in 1967)	**0.537** *** **(0.219)**		**0.601** ** **(0.240)**		**0.290** *** **(0.077)**		**0.286** *** **(0.087)**	
Year 1967 × Covered Jurisdiction	0.143 ** (0.058)		0.100 (0.064)		0.140 *** (0.027)		0.093 *** (0.029)	
Year 1967 × Former Confederate States × ln(per capita total revenue in 1967)		**0.162 (0.139)**	**−0.111 (0.137)**			**0.090** * **(0.046)**	**−0.025 (0.048)**	
Year 1967 × Former Confederate States		0.111 ** (0.045)	0.078 (0.048)			0.109 *** (0.016)	0.076 *** (0.017)	
Year 1967 × % Nonwhite Population × ln(per capita total revenue in 1967)				**0.330 (0.407)**				**0.395** *** **(0.129)**
Year 1967 × % Nonwhite Population				0.132 (0.134)				0.282 *** (0.067)
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fiscal Years Included	1962, 67	1962, 67	1962, 67	1962, 67	1962, 67	1962, 67	1962, 67	1962, 67
Number of Counties	2784	2784	2784	2784	2784	2784	2784	2784
Number of Observations	5568	5568	5568	5568	5568	5568	5568	5568

Note : * p<.10, ** p<.05, *** p<.01 in two-tailed test. County-clustered standard errors are in parentheses.
Quantities of interest are indicated in **boldface**.

Table 1.3: The test of the theory of strategic use of debts.

Dependent Variable: logged per capita	highway expenditure				welfare expenditure			
Exlanatory Variables	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Year 1967 × Covered Jurisdiction × ln(per capita total revenue in 1967)	**0.479** *** **(0.135)**		**0.626** *** **(0.144)**		**0.356** **(0.260)**		**0.559** **(0.355)**	
Year 1967 × Covered Jurisdiction	0.114 *** (0.041)		0.135 *** (0.043)		0.188 * (0.109)		0.186 (0.132)	
Year 1967 × Former Confederate States × ln(per capita total revenue in 1967)		**0.098** **(0.075)**	**−0.176** *** **(0.066)**			**−0.041** **(0.192)**	**−0.264** **(0.265)**	
Year 1967 × Former Confederate States		0.029 (0.026)	−0.017 (0.026)			0.093 (0.076)	0.027 (0.091)	
Year 1967 × % Nonwhite Population × ln(per capita total revenue in 1967)				**0.573** * **(0.314)**				**0.378** **(0.440)**
Year 1967 × % Nonwhite Population				0.139 (0.088)				0.556 ** (0.235)
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fiscal Years Included	1962, 67	1962, 67	1962, 67	1962, 67	1962, 67	1962, 67	1962, 67	1962, 67
Number of Counties	2784	2784	2784	2784	2784	2784	2784	2784
Number of Observations	5568	5568	5568	5568	5568	5568	5568	5568

Note : * p<.10, ** p<.05, *** p<.01 in two-tailed test. County-clustered standard errors are in parentheses.
Quantities of interest are indicated in **boldface**.

Table 1.3: *continued.*

in the estimation model. On the other hand, the last panel shows that none of the triple interaction terms has a statistically significant effect on welfare expenditures.

The findings in Table 3 are quite consistent with predictions made by the theory of SUD. That is, the VRA-covered counties that anticipated a great change in future policy preferences and had large enough budgets to execute SUD issued extra bonds to perpetuate segregation through the construction of all-white schools in the suburbs while keeping the level of welfare minimal. This is indicated by the increase in education and highway expenditures and no change in the welfare expenditures. The VRA-covered counties were not only motivated to spend the budget on their preferred policies while in power, but also to reduce future welfare spending by letting future governments face tighter budget constraints by overissuing local bonds.

Using the conservative coefficients estimated from an instrumental variable (IV) in Table A7 in Appendix F,[38] the VRA-covered counties for which the instrumented logged per capita total revenue in 1967 that was one standard deviation greater than the mean increased per capita long-term debts outstanding 20.8% more than did their counterparts (noncovered counties with one standard deviation greater logged per capita total revenue in 1967) from the pre-VRA to the transitional periods.[39] The estimated 20.8% effect of SUD amounted to $105 per capita or a $3.5 million increase in debts outstanding per county.[40] From the pre-VRA to the transitional periods, the estimated per capita long-term debts outstanding among the VRA-covered counties with one standard deviation greater instrumented logged per capita revenues increased from $430 to $589, and $105 accounts for about two thirds of this increase. On the other hand, having one standard deviation greater logged per capital total revenue increased per capita long-term debts outstanding only by $5.8 per capita or

38. See the next subsection and Appendix F for the detail of IV regression.

39. $\exp(\beta) \times \exp(\lambda \cdot 1\text{sd increase in government size}) = \exp(0.096) \times \exp(0.405 \times 0.23) \approx 1.208$.

40. See Appendix E for how to obtain $105. The average population of the VRA-covered counties in 1967 was 33,708. Thus, $\$105 \times 33,708 = \$3,539,340$.

$195,506 in the average covered county.[41]

1.5.3 Robustness checks

The statistical findings in the previous subsections are also robust to different model specifications and are distinct from observational equivalences. The details are discussed in Appendix F, although I summarize the main points here. First, I re-estimated the models in Table 3 using an instrumental variable (IV) to address the potential endogeneity between the variable of government size (logged per capita total revenue in 1967) and the dependent variable (logged per capita long-term debts outstanding) and obtained consistent results. Second, the estimated effects are distinct from regional variations. The results do not change when Model (1) of Table 2 is estimated with nearest-neighbor matching (Abadie et al. 2004), and when Models (1), (5), (9), and (13) in Table 3 are estimated with the regional time trend variables.[42] Third, the changes in policy outputs from the transitional to the post-transitional periods are consistent with the predictions. That is, a huge increase is observed in welfare spending from the transitional to the post-transitional periods, while highway spending decreased during the same period. This also supports the view that strategic actors had accurate expectations about how policies would change when the preferences of minority voters were reflected in public policies. Finally, there are no trend effects in the VRA-covered counties. I ran regressions similar to Models (1) to (4) in Table 3, using the two preceding census figures (i.e. 1957 and 1962) and changing the dependent variable to logged per capita total debts outstanding,[43] but the coefficients of the triple interaction terms are all statistically insignificant.

41. See Appendix E for how to obtain $5.8.

42. Because the models in Table 3 include a three-way interaction term, the matching estimator is not available. Thus, the regional trend variables are used as an alternative.

43. This change was made due to the unavailability of long-term debts outstanding in the year 1957.

1.6 Conclusion

Previous studies have predominantly focused on how the actual change in the median voter affects policy outcomes. However, the mechanism by which anticipation of the change shapes public policies has not been well investigated. This study builds on the empirical implications of one version of the theory of SUD by Tabellini and Alesina (1990) and finds that VRA-covered counties with large budgets statistically significantly increased long-term debts, education spending and highway spending during the transitional period more than did the rest of the country. That is, it takes both a strong motive and sufficient financial leeway for the governments to exercise SUD.

From these results, one can articulate this causal explanations. The most preferred strategy for the VRA-covered counties was to maintain white dominance in their jurisdictions through the vote denial of minorities. Because the VRA prohibited this strategy, segregationist governments inevitably anticipated that minority voters would start influencing the policy process, which meant that emphasis on policy priorities would shift from developmental to redistributive policies. This *anticipated* change caused serious anxiety about education policy where the deepest issue cleavage existed. The segregationist governments with enough financial capacity passed additional bond issues to build schools in the suburbs as "escape hatches", expecting that the minority voters would pay the outstanding debts upon taking office.

This study purposefully focused on the segregationist governments in the 1960s, but actual examples for SUD are abundant. Gentrification, selective relocation, large-scale real estate development, the operation of a new large-scale manufacturing plant, natural disasters, and the mass influx of immigrants are all good examples to demonstrate SUD in action, although most of them are not suitable for empirical studies.

Finally, the implication of the study goes beyond the empirical support of SUD. This study presents rare empirical evidence of the segregationist governments' efforts to limit

policy outputs. I believe this is a meaningful addition to an abundance of previous studies on Southern politics that have confirmed the segregationist states' efforts to limit the power of policy *inputs* for non-whites such as voter registration (Alt 1994) or representation (Grofman and Davidson 1994). Furthermore, this study empirically demonstrates one of the causal mechanisms by which incumbent governments left a negative legacy for future generations.

1.7 Appendix A: Formal Model by Tabellini and Alesina (1990)

The model starts with the following assumption about the following budget constraints:

$$g_1 + f_1 \leq 1 + b$$

$$g_2 + f_2 \leq 1 - b$$

where g_t and f_t denote two types of public goods, the subscript represents the periods $t \in 1, 2$, and b represents the amount of debts. The government in each period receives one unit of budget. The government in Period 1 can either borrow ($b \geq 0$) or save ($b \leq 0$), while the government in Period 2 must balance the budget. Thus, when the government in Period 1 borrows heavily, the government in Period 2, which must repay the debts, will face a tighter budget constraint. The total utility of a voter i is represented as:

$$W^i = \alpha^i \cdot u(g_1) + (1 - \alpha^i) \cdot u(f_1) + \alpha^i \cdot u(g_2) + (1 - \alpha^i) \cdot u(f_2)$$

where $\alpha^i \in [0, 1]$ represents voter i's preference for the public good g, and $u(\cdot)$ is a well-behaved utility function. Under majority rule, a government in each period implements the policies that reflects the median voter's policy preference.[44] Thus, the budgets are allocated so that they maximize the utilities of the median voters.

Because players have perfect information, subgame perfect equilibrium is obtained by backward induction. First, the government in Period 2 solves the following constrained utility maximization problem:

$$\max_{g_2, f_2} \alpha_2^m \cdot u(g_2) + (1 - \alpha_2^m) \cdot u(f_2) \qquad s.t.\ g_2 + f_2 \leq 1 - b \tag{1.3}$$

44. See Tabellini and Alesina (1990) for further mathematical discussion about the existence of equilibrium with this setting.

where α_t^m is the preference of the median voter in period t. From the first order condition of Equation 1, the optimal supply schedules of the two public goods in Period 2, $g_2^*(b)$ and $f_2^*(b)$, are obtained as functions of the amount of debt accumulated during Period 1. Next, given the supply schedule in Period 2, the government in Period 1 maximizes this constrained maximization problem:

$$\max_{g_1,f_1,b} \alpha_1^m \cdot u(g_1) + (1-\alpha_1^m) \cdot u(f_1) + \mathtt{E}[\alpha_1^m \cdot u(g_2^*(b)) + (1-\alpha_1^m) \cdot u(f_2^*(b))] \\ s.t.\ g_1 + f_1 \leq 1 + b \tag{1.4}$$

In Period 1, the government allocates its budget between two goods, and chooses the level of debts that will affect policy decisions in Period 2. From the first and second order conditions of Equation 2, Tabellini and Alesina (1990) deduced the following two propositions.

Proposition 1: If the concavity of the utility function is decreasing in consumption of two public goods, the optimal amount of public debt in Period 1 is always positive, regardless of the policy preference of the median voter in Period 2. Formally, the concavity index is $\lambda(x) \equiv -u''(x)/u'(x)^2$. Then,

$$\frac{\partial \lambda(x)}{\partial x} < 0 : \ x \in (0,1) \Rightarrow b^* > 0 \ \forall \ \alpha_2^m \in [0,1] \tag{1.5}$$

Proposition 2: Under the same necessary conditions in Proposition 1, the further the preference of the median voter in Period 2 is expected to deviate from that of the median voter in Period 1, the more public debts will be issued by the government in Period 1.

1.8 Appendix B: Detail of the Timing Issue

This appendix explains the rationale of the three assumptions regarding the timing of policymaking in the *Timing* section.

1. It takes one fiscal year for local governments to draft budget proposals, and they implement these proposals in the following fiscal year.

This assumption seems innocuous, but one concern is that some local governments have biennial legislative and budget cycles (Fisher 2006), and their budgets might have been drafted before the VRA. However, this possibility only affects against finding any evidence for strategic policymaking, which this study found. Therefore, this concern does not change the substantive findings of this study. Later, I discuss in detail how the timing of the recording of government statistics is affected by these biennial cycles.

2. All policymakers in the treatment group had an expectation of change in the voting population no later than August 7, 1965, when the VRA was enacted.

The VRA was proposed by President Johnson on March 17, 1965, passed the Senate on May 11, passed the House of Representatives on July 10, and was enacted on August 7, 1965. Although the media coverage until August was not extensive, politicians' sources of information are much richer than those of ordinary people. In fact, one of the two anecdotes in the *Introduction* occurred in 1964, and the original article of the other anecdote reports that bond-desegregation suits had been filed twice before this case. Thus, politicians in the segregationist states were well aware of the potential ramification of the VRA. Importantly, their knowing the future change in the voting population earlier than August 7, 1965 only makes it harder to find evidence for strategic policymaking. Because this study find some evidence, it does not change the substantive findings of this study.

3. Policymakers could reflect their strategic motivation in the following year's budget proposal immediately after the enactment of the VRA.

Table A1 shows when the financial statistics that made up the 1967 Census of Governments were drafted and implemented if Assumption 1 holds. In the top row, the fiscal year started July 2. That is, the budgets were drafted from July 2, 1964 to July 1, 1965 and implemented from July 2, 1965 to July 1, 1966, which was the earliest possible date. At the bottom row, the fiscal year started July 1, and the budget were drafted from July 1, 1965 to June 30, 1966 and implemented from July 1, 1966 to June 30, 1967, which was the last possible date that financial data were recorded in the 1967 Census of Governments. If Assumption 3 holds, the strategic policymaking might not have been feasible if the fiscal year had started in the period between July 2 and August 6.[45]

In some cases, this assumption looks a bit too extreme. For example, if the fiscal year of a government began on August 7, it had only August 7, 1965 to draft the budget with strategic motivation. However, almost all local governments ended their fiscal year at the end of the month. In fact, only one county (Clinch, GA) ended its fiscal year on July 31. Thus, unless the fiscal year of a government ended at the end of July, local governments had at least three weeks to reflect their strategic motivation in the budget. With three weeks, they might have been able to initiate the procedure of referendum for a school bond issue, for instance. The important point is that the Census of Governments recorded the statistics immediately after the enactment of the VRA.

1.9 Appendix C: Correlation between Binary and Continuous Variables

The purpose of this section is to create the conversion chart from the correlation between two continuous variables to that between a continuous variable and a binary variable and vice versa. The following Monte Carlo experiment is motivated by the fact that the correlation

45. Note this is an approximation in the sense that the 1967 Census of Governments only reports the date of the beginning of the fiscal year for each county government, not other governments within the county.

between a continuous variable and a binary variable never reaches to one because of the difference in the measurements. The experiment proceeds as follows.

1. Generate two continuous random variables with a certain correlation, say $\rho_o = .99$, and record the value of ρ_o as the original correlation. The number of observations is set at 2784 so that the sample represents the same number as that of the county jurisdictions used in the analysis.

2. Transform one of the variables into a binary one according to its size. Specifically, 501 of 2784 observations are coded to 1 and the others are coded to 0, so that the number of the observations assigned to 1 is the same as that of the VRA-covered jurisdictions.

3. Calculate the correlation between the transformed binary variable and the untransformed continuous one and record the value of the updated correlation (ρ_u).

4. Repeat from Step 1 to Step 3 by changing the value of the original correlation slightly until it reaches $\rho_o = 0$.

5. Plot the values of ρ_o and ρ_u as the coordinates in the plane.

Figure A1 shows the conversion chart, of which the horizontal axis represents the correlation between a continuous variable and a binary variable, and the vertical axis represents that between two continuous variables. The dotted line explains that the correlation between the indicator of the VRA-covered jurisdiction and % nonwhite population in 1967 is 0.673, which is converted to .997 if these two variables *were* both continuous variables. This implies that these two variables are conceptually as well as statistically very similar variables.

1.10 Appendix D: Descriptive Statistics

Table A2 presents the descriptive statistics of the dependent variables (logged and non-logged), the key explanatory variables, the control variables, and the variable of government

size. For comparison, the statistics are calculated for each distinct combination of year and the status of the segregationist governments. Table A2 shows that the maximum value of non-logged dependent variables in the control group is several times larger than those in the treatment group. Therefore, it is appropriate to measure the policy effect on a percentage basis rather than on a monetary basis, and thus this study uses log-transformed dependent variables. The average % nonwhite population for the VRA-covered counties is significantly higher than the counterpart of the non-covered jurisdictions. This is not a problem because they are both the key explanatory variable that represent the same concept particularly given the high correlation between these variables as discussed in Appendix C. Moreover, the matching estimator in Table A9 shows that the main result is robust to the selection on observables. Except for this variable, *land area* has the largest variation across the different groups. Yet, to my knowledge, no other theory can explain why geographically small counties issued more debts between 1962 and 1967 than did large counties. The other control variables distribute fairly equally across the different treatment statuses.

1.11 Appendix E: Calculation of Substantive Significance

This appendix explains the processes used to calculate the substantive effect of being a former Confederate states on logged per capita education expenditures from the estimates of Model (2) in Table 2 and the substantive effect of being a VRA-covered jurisdiction on logged per capita long-term debts outstanding from the estimates Model (1) in Table A7.[46] Whenever possible, the quantities are recovered from the estimated coefficients such as β or γ. Otherwise, the values from the descriptive statistics in Table A2 substitute for the mean statistics in the equations.

46. I use the estimates obtained from the IV discussed in Appendix F because these estimates are more conservative.

Substantive significances of being the former Confederate states on per capita education expenditures. The substantive effect of being the former Confederate states on education expenditures is defined as $\mathbf{E}\left(\overline{edu_{\text{CSA},67}}\right) - \mathbf{E}\left(\overline{edu_{\text{CSA},67}}|\text{CSA}=0\right)$. Let us first calculate the predicted value of per capita education expenditures in 1967 using the difference in differences estimator, $\beta(=0.057)$, in Equation (1).

$$\begin{aligned}
\beta &= \left[\ln\left(\mathbf{E}\left(\overline{edu_{\text{CSA},67}}\right)\right) - \ln\left(\overline{edu_{\text{CSA},62}}\right)\right] - \left[\ln\left(\overline{edu_{ctrl,67}}\right) - \ln\left(\overline{edu_{ctrl,62}}\right)\right] \\
&= \ln\left(\frac{\mathbf{E}\left(\overline{edu_{\text{CSA},67}}\right)/\overline{edu_{\text{CSA},62}}}{\overline{edu_{ctrl,67}}/\overline{edu_{ctrl,62}}}\right) \\
\Leftrightarrow \exp(\beta) &= \frac{\mathbf{E}\left(\overline{edu_{\text{CSA},67}}\right)/\overline{edu_{\text{CSA},62}}}{\overline{edu_{ctrl,67}}/\overline{edu_{ctrl,62}}} \\
\Leftrightarrow \mathbf{E}\left(\overline{edu_{\text{CSA},67}}\right) &= \exp(\beta)\cdot\overline{edu_{\text{CSA},62}}\cdot\frac{\overline{edu_{ctrl,67}}}{\overline{edu_{ctrl,62}}} \\
&= 1.059\times 361.5\times\frac{661.7}{485.3} \\
&= 521.8
\end{aligned}$$

where

$\beta \equiv$ the difference in differences estimator

$\mathbf{E} \equiv$ expectation operator

$edu \equiv$ per capita education expenditures

CSA $\equiv$ identifier of the former Confederate states

62 and 67 $\equiv$ identifier of pre- and post-VRA

On the other hand, the counterfactual amount of per capita education expenditures in 1967 when these counties were not covered by Section 5 of the VRA can be calculated simply

by dropping $\exp(\beta)$ from the final equation.[47]

$$\begin{aligned}
\mathbf{E}\left(\overline{edu_{\text{CSA},67}}|\text{CSA}=0\right) &= \overline{edu_{\text{CSA},62}} \cdot \frac{\overline{edu_{ctrl,67}}}{\overline{edu_{ctrl,62}}} \\
&= 361.5 \times \frac{661.7}{485.3} \\
&= 492.9 \\
\therefore 521.8 - 492.9 &= 28.9 \quad \square
\end{aligned}$$

Substantive effect of being the VRA-covered counties of which the budget size is 1 SD larger than the mean on per capita long-term debts outstanding. The substantive effect of SUD with respect to the covered status is defined as $\mathbf{E}\left(\overline{debts_{\text{CJ},67,1\text{SD}}}\right) - \mathbf{E}\left(\overline{debts_{\text{CJ},67,1\text{SD}}}|\text{CJ}=0\right)$. The predicted value of per capita long-term debts outstanding in 1967 is calculated using the difference in difference estimator, $\beta(=0.096)$, and the triple difference estimator, $\lambda(=0.405)$, in Model (1) in Table A7. Government size is measured as logged per capita total revenue in 1967, and is instrumented by the previous census year's logged per capita total revenue.[48] In the following calculation, I evaluate the substantive effect of the designation of the VRA-covered jurisdiction on the logged per capita long-term debts outstanding in 1967 when government size increases by one standard deviation from the mean.[49]

47. This quantity is not the same as the average per capita education expenditures of the control group.

48. Because its logged scale has no practical meaning, I measure the amount of one standard deviation without re-transforming it into its non-logged form. This also avoids unnecessary complication in the triple differences technique.

49. The mean statistics for the counties of which budget size is 1 SD larger than the mean are the estimated values from the panel regression. The codes are available from the author.

$$
\begin{aligned}
1\text{SD} \cdot \lambda = & \Big(\Big[\ln \Big(\mathbf{E} \Big(\overline{debts_{\text{CJ},67,1\text{SD}}} \Big) \Big) - \ln \Big(\overline{debts_{\text{CJ},62,1\text{SD}}} \Big) \Big] \\
& - \Big[\ln \Big(\overline{debts_{ctrl,67,1\text{SD}}} \Big) - \ln \Big(\overline{debts_{ctrl,62,1\text{SD}}} \Big) \Big] \Big) \\
& - \Big(\Big[\ln \Big(\overline{debts_{\text{CJ},67,0\text{SD}}} \Big) - \ln \Big(\overline{debts_{\text{CJ},62,0\text{SD}}} \Big) \Big] \\
& - \Big[\ln \Big(\overline{debts_{ctrl,67,0\text{SD}}} \Big) - \ln \Big(\overline{debts_{ctrl,62,0\text{SD}}} \Big) \Big] \Big) \\
= & \ln \left(\frac{\mathbf{E} \Big(\overline{debts_{\text{CJ},67,1\text{SD}}} \Big) / \overline{debts_{\text{CJ},62,1\text{SD}}}}{\overline{debts_{ctrl,67,1\text{SD}}} / \overline{debts_{ctrl,62,1\text{SD}}}} \right) \\
& \left(\frac{\overline{debts_{\text{CJ},67,0\text{SD}}} / \overline{debts_{\text{CJ},62,0\text{SD}}}}{\overline{debts_{ctrl,67,0\text{SD}}} / \overline{debts_{ctrl,62,0\text{SD}}}} \right)
\end{aligned}
$$

$$
\begin{aligned}
\Leftrightarrow \exp(1\text{SD} \cdot \lambda) &= \frac{\dfrac{\mathbf{E} \Big(\overline{debts_{\text{CJ},67,1\text{SD}}} \Big) / \overline{debts_{\text{CJ},62,1\text{SD}}}}{\overline{debts_{ctrl,67,1\text{SD}}} / \overline{debts_{ctrl,62,1\text{SD}}}}}{\dfrac{\overline{debts_{\text{CJ},67,0\text{SD}}} / \overline{debts_{\text{CJ},62,0\text{SD}}}}{\overline{debts_{ctrl,67,0\text{SD}}} / \overline{debts_{ctrl,62,0\text{SD}}}}} \\
&= \frac{\dfrac{\mathbf{E} \Big(\overline{debts_{\text{CJ},67,1\text{SD}}} \Big) / \overline{debts_{\text{CJ},62,1\text{SD}}}}{\overline{debts_{ctrl,67,1\text{SD}}} / \overline{debts_{ctrl,62,1\text{SD}}}}}{\exp(\beta)} \\
\Leftrightarrow \mathbf{E} \Big(\overline{debts_{\text{CJ},67,1\text{SD}}} \Big) &= \exp(\beta) \cdot \exp(1\text{SD} \cdot \lambda) \cdot \overline{debts_{\text{CJ},62,1\text{SD}}} \cdot \frac{\overline{debts_{ctrl,67,1\text{SD}}}}{\overline{debts_{ctrl,62,1\text{SD}}}} \\
&= \exp(0.096) \times \exp(0.230 \times 0.405) \times 430.5 \times \frac{864.2}{737.6} \\
&= 609.3
\end{aligned}
$$

where

$\lambda \equiv$ the triple differences estimator

$debts \equiv$ per capita long-term debts outstanding

$\text{CJ} \equiv$ identifier of the VRA-covered jurisdiction

$1\text{SD} \equiv$ the one standard deviation larger size of government

The counterfactual amount of per capita long-term debts outstanding in 1967 when the counties were not designated as the VRA-covered jurisdiction can be calculated simply by dropping $\exp(\beta)$ and $\exp(1\text{SD} \cdot \lambda)$ from the final equation.

$$\begin{aligned}
\mathbf{E}\left(\overline{debts_{\text{CJ},67,1\text{SD}}}|\text{CJ}=0\right) &= \overline{debts_{\text{CJ},62,1\text{SD}}} \cdot \frac{\overline{debts_{ctrl,67,1\text{SD}}}}{\overline{debts_{ctrl,62,1\text{SD}}}} \\
&= 430.5 \times \frac{864.2}{737.6} \\
&= 504.3 \\
\therefore \mathbf{E}\left(\overline{debts_{\text{CJ},67,1\text{SD}}}\right) - \mathbf{E}\left(\overline{debts_{\text{CJ},67,1\text{SD}}}|\text{CJ}=0\right) &= 609.3 - 504.3 \\
&= 105 \quad \square
\end{aligned}$$

Substantive effect of having the 1SD larger budget size than the mean among the VRA-covered counties on per capita long-term debts outstanding. Next, I evaluate the substantive effect of having the 1SD larger budget size than the mean on the logged per capita long-term debts outstanding in 1967 among the VRA-covered counties. The quantity is defined as $\mathbf{E}\left(\overline{debts_{\text{CJ},67,1\text{SD}}}\right) - \mathbf{E}\left(\overline{debts_{\text{CJ},67,0\text{SD}}}\right)$ and calculated as follows.[50]

$$\exp(1\text{SD} \cdot \lambda) = \frac{\overline{debts_{\text{CJ},67,1\text{SD}}}/\overline{debts_{\text{CJ},62,1\text{SD}}}}{\overline{debts_{ctrl,67,1\text{SD}}}/\overline{debts_{ctrl,62,1\text{SD}}}} \exp(\beta)$$

$$\Leftrightarrow \frac{\overline{debts_{\text{CJ},67,1\text{SD}}}/\overline{debts_{\text{CJ},62,1\text{SD}}}}{\overline{debts_{ctrl,67,1\text{SD}}}/\overline{debts_{ctrl,62,1\text{SD}}}} = \exp(1\text{SD} \cdot \lambda) \cdot \exp(\beta)$$

Therefore,

50. Because some estimates are subsumed by the county fixed effects, we cannot evaluate the counter factual amount $\mathbf{E}\left(\overline{debts_{\text{CJ},67,1\text{SD}}}|size = 0\text{SD}\right)$.

$$\exp(1\text{SD}\cdot\lambda) = \frac{\overline{debts_{\text{CJ},67,1\text{SD}}}/\overline{debts_{\text{CJ},62,1\text{SD}}}}{\overline{debts_{ctrl},67,1\text{SD}}/\overline{debts_{ctrl},62,1\text{SD}}} \Bigg/ \frac{\mathbf{E}\left(\overline{debts_{\text{CJ},67,0\text{SD}}}\right)/\overline{debts_{\text{CJ},62,0\text{SD}}}}{\overline{debts_{ctrl},67,0\text{SD}}/\overline{debts_{ctrl},62,0\text{SD}}}$$

$$= \frac{\exp(1\text{SD}\cdot\lambda)\cdot\exp(\beta)}{\frac{\mathbf{E}\left(\overline{debts_{\text{CJ},67,0\text{SD}}}\right)/\overline{debts_{\text{CJ},62,0\text{SD}}}}{\overline{debts_{ctrl},67,0\text{SD}}/\overline{debts_{ctrl},62,0\text{SD}}}}$$

$$\Leftrightarrow \mathbf{E}\left(\overline{debts_{\text{CJ},67,0\text{SD}}}\right) = \exp(1\text{SD}\cdot\lambda)\cdot\exp(\beta)\cdot\frac{\overline{debts_{ctrl},67,0\text{SD}}/\overline{debts_{ctrl},62,0\text{SD}}}{\exp(1\text{SD}\cdot\lambda)/\overline{debts_{\text{CJ},62,0\text{SD}}}}$$

$$= \exp(\beta)\cdot\frac{\overline{debts_{ctrl},67,0\text{SD}}/\overline{debts_{ctrl},62,0\text{SD}}}{1/\overline{debts_{\text{CJ},62,0\text{SD}}}}$$

$$= \exp(0.096)\times\frac{854.9/701.4}{1/449.8}$$

$$= 603.5$$

$$\therefore \mathbf{E}\left(\overline{debts_{\text{CJ},67,1\text{SD}}}\right) - \mathbf{E}\left(\overline{debts_{\text{CJ},67,1\text{SD}}}\right) = 609.3 - 603.5$$

$$= 5.8 \quad \square$$

1.12 Appendix F: Robustness Checks

The empirical analysis highlights two main findings. First, the Southern governments, not only the VRA-covered counties, increased education spending during the transitional period. Second, the VRA-covered counties with a large budget increased long-term debts, education spending, and highway spending. This section checks the robustness of these findings.

Re-estimation of Table 3 with IV. In Table 3 in the main manuscript, government size is measured by logged per capita total general revenue in 1967. Because this variable and the four dependent variables of policy outputs are determined simultaneously, I must address the potential bias due to this endogeneity problem. However, the following analysis shows that the endogeneity does not change the findings in the manuscript.

First, it must be emphasized that the model does not evaluate the effect of government size. The quantity evaluated by Equation (2) is the differential effect of the effects of being the VRA-covered county during the transitional period between the different sizes of government. That is, Equation (2) only evaluates whether the effect of strategic policymaking for the governments with a certain budget size (i.e., 1 SD larger than the average) is larger than the counterpart for the governments with an average budget size. Thus, unless there is a reason to believe that the difference in the percent change in policy outputs between the VRA-covered counties and the others is correlated with policy outputs, the estimates should not be confounded in substantive sense.

Empirical evidence also supports this argument. I re-estimated Models (1) to (4) in Table 3 by instrumenting the variable of government size using its lagged variable. In the first stage, I regressed logged per capita total revenue on its lagged variable as well as all covariates that are used in the second stage regression as presented in Table A6. Then, its predicted values in 1967 are used as government size. The chi-square and F statistics in the first stage regressions are both large enough to pass the underidentification test and weak identification test.[51]

I present the main results of the IV regressions in Table A7.[52] Models (1) to (4) in the first panel show that the coefficients of the triple interaction terms in Models (1) and (3)

51. These statistics are calculated with the Stata program, `xtivreg2` (Schaffer 2005; Angrist and Pischke 2009).

52. The different IVs are used depending on what the treatment variable is. The IV estimated in each column of Table A6 is used to produce the IV estimates in Table A7. For example, the IV estimated in Column (3) of Table A6 is used to produce the IV estimates in Column (3) of Table A7.

have statistically weak but significant effects on long-term debts, which is consistent with the original models in Table 3. Although the first panel presents the main results for the prediction of SUD and the other panels are its subsidiaries, other panels show the results that are mostly consistent with the original models. The triple interaction terms in the second panel for education expenditures are not statistically significant, but the sign of the coefficients are still consistent.

Early median voter shift. One of the most probable scenarios against the finding is that the inclusion of new voting population led to the larger size of governments (Husted and Kenny 1997, Lott and Kenny 1999, Besley and Case 2003), which resulted in the increase in per capita education expenditures or per capita long-term debts. In the main manuscript, I concluded that this is not the case based on the changes in welfare expenditure in each period. Here, I report another piece of evidence that supports this conclusion.

The variation in the ending dates of fiscal years provides another reason why the early median voter shift is not the case. If the timing of data collection lagged behind the actual period when the strategic policymaking was feasible, the effect of the early median voter shift confounds the results. On the other hand, if the timing of data collection precedes the actual period when the strategic policymaking was feasible, the inclusion of irrelevant observations in the post-treatment period only has an attenuation bias, which is less problematic in this study. Although the analysis using the electoral dates of all local governments after the VRA probably provides the ideal test for the early median voter shift, these dates are prohibitively difficult to obtain. However, the information about the ending dates of the fiscal years allows us to conduct a similar analysis.[53]

Suppose the early median voter shift also increased per capita education expenditure and

53. Note that the ending dates of the fiscal years are of county governments, not of all local governments within a county. Thus, the following analysis relies on the assumption that local governments usually have the same ending dates of fiscal years as those of the county government.

per capita long-term debts outstanding, contrary to my argument. If the early median voter shift followed the strategic policymaking, the effect of the strategic policymaking should have been stronger as more time passed after the VRA was enacted until the fiscal year closed. This is because as the more time passed after the VRA was enacted, local governments were more likely to have an election including the newly enfranchised voters, and the data were more likely to be confounded by the early median voter shift that could increase various expenditures. Thus, if the early median voter shift really has a confounding effect, the newer the financial data collected in the 1967 Census of Governments, the more confounding, or the larger the coefficient of the key explanatory variables. Because the data included in the 1967 Census of Governments is the newest when the fiscal year ends on June 30, 1967 and the oldest when it ends on July 1, 1966, the ending dates of fiscal years are coded in such a way that the newer dates are assigned larger values. Then, the three-way interaction term between the ending dates of fiscal year and *Year1967* $\times$ *Covered Jurisdiction* and the four-way interaction term between the ending dates of fiscal year and *Year1967* $\times$ *Covered Jurisdiction* $\times \ln$*(per capita total revenue in 1967)* in should not be positive if the early median voter shift has no confounding effect.

Table A8 shows that both the three-way interaction term and the four-way interaction term are statistically insignificant and negative. This indicates that the effect of the strategic policymaking is not increasing in the interval between the enforcement date of the VRA and the date of data collection, which contradicts the prediction based on the early median voter shift.

Southern effects. The historically defined former Confederate states as well as the VRA-covered jurisdictions and the geographically defined Southern region are often observationally equivalent but are conceptually different. A natural concern is that the indicators of the former Confederate states and the VRA-covered jurisdiction might spuriously capture

idiosyncratic trends of the South. If the characteristics of the regional South can be represented by observable socioeconomic characteristics, the matching technique can address this issue because the matching method enables us to examine whether the historically defined South are statistically significant after controlling for the characteristics of the geographically defined South.

First, I calculate the first differences of all variables to account for unit heterogeneities. Without matching, this first-difference estimator is identical to the DD estimator in Equation (1), as is clear from the comparison between Model (1) of Table A9 and Model (2) in Table 2. Then, the VRA-covered jurisdictions are matched with non-covered jurisdictions on the first-differentiated covariates using the nearest-neighbor matching technique.[54] The quantity of interest is the average treatment effect (ATE). When the observations are matched on all covariates, the first-differenced proportion of the nonwhite population is still unbalanced between the VRA-covered counties and noncovered counties. Therefore, I performed exact matching on the first-differenced proportion of the nonwhite population.[55] These two nearest-neighbor matching estimators are presented in Models (2) and (3). Both the estimates are statistically positively significant, and the coefficients are larger than their counterpart without matching. Thus, the findings in Table 2 are not due to the difference in the observed characteristics between the geographically defined South and the rest of the United States.

Because the matching technique cannot be performed for the triple differences estimation, the robustness check for Table 3 is conducted in the following way. Models (1), (5), (9) and (13) of Table 3 are re-estimated with region-year fixed effects. Specifically, counties are

54. Specifically, I used `nnmatch` program for Stata (Abadie et al. 2004).

55. Because the first-differenced proportion of the nonwhite population is a continuous variable, literal exact matching is impossible. Instead, `nnmatch` multiplies the importance of the first-differenced % nonwhite population by 1,000. This option makes the first-differenced proportion of the nonwhite population well-balanced, but leaves other covariates unbalanced.

classified into the four U.S. Census Bureau regions,[56] and the interaction terms between these regional dummies and the indicator of *Year1967* are entered to the regression instead of year fixed effects. All the models in Table A10 present the estimated coefficients that are very close to their counterparts without region-year fixed effects (reproduced in the third and fourth columns). This indicates that the observed increase in the long-term debts outstanding among the VRA-covered counties is independent of the regional trend.

Trend effects. The robustness check for trend effects is intended to eliminate the possibility that the finding in Table 3 is an artifact of unobserved time trends that continuously affected either the treatment group or the control group. To test trend effects, this analysis uses the observations from the 1957 and 1962 census, logged per capita total general revenue in 1962 as the variable of government size, and logged per capita education expenditures and logged per capita total debts outstanding as a dependent variable.[57] The analysis is otherwise the same as that for Models (1) to (4) in Table 3.

All the models show that the coefficients of the key explanatory variables, *Year1962* $\times$ *Covered Jurisdiction* $\times \ln$*(per capita total revenue in 1962)*, are statistically insignificant. Thus, the possibility that the strategic policymaking and SUD are artifacts of underlying time trends is dismissed.

Policy change from the transitional to the post-transitional periods. Finally, let us look at how the policies changed in the post-transitional period. Table A12 uses the observations from the 1967 and 1972 census and is otherwise the same as Table A4.

56. The breakdown of the four Census regions is as follows. West: WA, OR, CA, ID, NV, MT, WY, UT, AZ, CO, and NM. Midwest: ND, SD, NE, KS, MN, IA, MO, WI, IL, MI, IN, and OH. Northeast: NY, PA, NJ, VT, NH, ME, MA, RI, and CT. South: OK, TX, AR, LA, KY, TN, MS, AL, WV, VA, DE, MD, NC, SC, GA, and FL.

57. Long-term debts outstanding is not available for 1957.

As predicted in Table 1, if strategic policymaking or SUD occurred during the transitional period, welfare spending would increase, highway spending would decrease, and education spending would not change from the transitional period. Table A12 indicates that these spending items changed exactly as predicted. These results confirm the prediction that because local governments would more or less reflect the policy preferences of minority voters who were enfranchised by the VRA by 1971, they prioritize redistributive policies over developmental policies. The reason why we do not observe any change in the education expenditures is that school integration advanced dramatically during this period, as shown in Figure A2. Thus, it is not surprising that the education expenditures in the Southern counties are not decreasing. To explain these results in strategic way, it was the anticipation of these policy changes that motivated the segregationist governments during the transitional period to spend more on their preferred education and developmental policies before they left office.

1.A Appendix Tables and Figures

	Budget Drafting		Budget Implementing		
	Begin	End	Begin	End	
Strategic policy making might not be possible	Jul. 2, 1964	Jul. 1, 1965	Jul. 2, 1965	Jul. 1, 1966	← *Earliest possible data to be included in 1967 Census*
	Aug. 7, 1964	Aug. 6, 1965	Aug. 7, 1965	Aug. 6, 1966	
	VRA was enacted on Aug. 7, 1965.				
Strategic policy making was possible	Aug. 8, 1964	Aug.7, 1965	Aug. 8, 1965	Aug.7, 1966	*Last possible data to be included in 1967 Census* ←
	Jul.1, 1965	Jun.30, 1966	Jul.1, 1966	Jun.30, 1967	

Table 1.A.1: The illustration of when budgets were drafted, implemented, and recorded.

Variable	Obs.	Mean	S.D.	Min.	Max.
per capita highway expenditures ($000s)	5568	0.13	0.10	0.00	1.13
logged per capita highway expenditures ($000s)	5568	-2.45	1.17	-8.33	0.12
per capita education expenditures ($000s)	5568	0.53	0.27	0.00	13.1
logged per capita education expenditures ($000s)	5568	-0.71	0.37	-6.29	2.57
per capita welfare expenditures ($000s)	5568	0.05	0.08	0.00	1.03
logged per capita welfare expenditures ($000s)	5568	-4.53	2.27	-10.6	0.03
per capita long-term debts outstanding ($000s)	5568	0.74	0.81	0.00	33.5
logged per capita long-term debts outstanding ($000s)	5568	-0.63	0.97	-7.22	3.51
per capita total general revenue ($000s)	5568	0.93	0.41	0.12	8.37
logged per capita total general revenue ($000s)	5568	-0.15	0.39	-2.16	2.12
Non White Population Rate (%)	5568	0.11	0.16	0.00	0.84
Median Family Income ($000s)	5568	23.8	6.72	6.98	49.8
Unemployment Rate (%)	5568	0.05	0.02	0.00	0.20
Urban Population (%)	5568	0.32	0.27	0.00	1.00
Elderly Population (%)	5568	0.11	0.03	0.03	0.24
Land Area (000s sq.mi.)	5568	0.92	1.19	0.00	18.57
Population Growth [10yr] (%)	5568	1.03	0.16	0.63	1.74
Population (000,000s)	5568	0.05	0.20	0.00	6.64

Table 1.A.2: Descriptive statistics.

	Covered					Not-covered				
Year	Obs.	Mean	S.D.	Min.	Max.	Obs.	Mean	S.D.	Min.	Max.
per capita highway expenditures ($000s)										
1962	501	0.07	0.06	0.00	0.44	2283	0.13	0.10	0.00	0.93
1967	501	0.08	0.06	0.00	0.33	2283	0.15	0.11	0.00	1.13
logged per capita highway expenditures ($000s)										
1962	501	-3.26	1.57	-8.33	-0.81	2283	-2.34	0.94	-8.33	-0.07
1967	501	-3.17	1.69	-8.33	-1.11	2283	-2.23	0.98	-8.33	0.12
per capita education expenditures ($000s)										
1962	501	0.34	0.09	0.01	0.91	2283	0.46	0.17	0.17	4.35
1967	501	0.50	0.19	0.00	3.57	2283	0.64	0.34	0.01	13.1
logged per capita education expenditures ($000s)										
1962	501	-1.13	0.28	-4.38	-0.09	2283	-0.82	0.30	-1.75	1.47
1967	501	-0.74	0.38	-6.29	1.27	2283	-0.51	0.33	-4.47	2.57
per capita welfare expenditures ($000s)										
1962	501	0.02	0.02	0.00	0.14	2283	0.05	0.08	0.00	0.73
1967	501	0.02	0.03	0.00	0.17	2283	0.06	0.09	0.00	1.03
logged per capita welfare expenditures ($000s)										
1962	501	-5.63	2.20	-10.6	-1.94	2283	-4.33	2.15	-10.6	-0.31
1967	501	-5.51	2.42	-10.6	-1.78	2283	-4.28	2.24	-10.6	0.03
per capita long-term debts outstanding ($000s)										
1962	501	0.45	0.36	0.00	2.34	2283	0.70	0.62	0.00	18.1
1967	501	0.67	1.10	0.00	18.0	2283	0.85	0.95	0.00	33.5
logged per capita long-term debts outstanding ($000s)										
1962	501	-1.16	1.01	-7.22	0.85	2283	-0.63	0.90	-7.22	2.90
1967	501	-0.91	1.18	-7.22	2.89	2283	-0.45	0.92	-7.22	3.51
per capita total general revenue ($000s)										
1962	501	0.55	0.13	0.28	1.53	2283	0.86	0.33	0.17	5.07
1967	501	0.79	0.22	0.36	3.59	2283	1.12	0.45	0.12	8.37
logged per capita total general revenue ($000s)										
1962	501	-0.62	0.23	-1.28	0.43	2283	-0.22	0.36	-1.76	1.62
1967	501	-0.26	0.24	-1.02	1.28	2283	0.05	0.35	-2.16	2.12

Table 1.A.2: *continued*

	Covered					Not-covered				
Year	Obs.	Mean	S.D.	Min.	Max.	Obs.	Mean	S.D.	Min.	Max.
Non White Population Rate (%)										
1962	501	0.35	0.20	0.00	0.83	2283	0.05	0.10	0.00	0.84
1967	501	0.33	0.19	0.00	0.84	2283	0.05	0.10	0.00	0.84
Median Family Income ($000s)										
1962	501	16.8	4.45	7.12	29.1	2283	22.6	5.97	6.98	42.1
1967	501	21.6	4.88	9.73	36.5	2283	27.0	6.45	8.49	49.8
Unemployment Rate (%)										
1962	501	0.05	0.02	0.01	0.11	2283	0.05	0.03	0.00	0.20
1967	501	0.05	0.02	0.01	0.11	2283	0.05	0.02	0.00	0.17
Urban Population (%)										
1962	501	0.25	0.24	0.00	1.00	2283	0.33	0.27	0.00	1.00
1967	501	0.26	0.24	0.00	1.00	2283	0.34	0.28	0.00	1.00
Elderly Population (%)										
1962	501	0.09	0.02	0.03	0.18	2283	0.11	0.03	0.03	0.22
1967	501	0.10	0.02	0.03	0.19	2283	0.12	0.03	0.03	0.24
Land Area (000s sq.mi.)										
1962	501	0.53	0.25	0.00	1.61	2283	1.01	1.30	0.03	18.6
1967	501	0.53	0.25	0.00	1.59	2283	1.01	1.30	0.03	18.6
Population Growth [10yr] (%)										
1962	501	1.01	0.16	0.72	1.74	2283	1.03	0.18	0.63	1.72
1967	501	1.02	0.14	0.74	1.63	2283	1.03	0.14	0.65	1.63
Population (000,000s)										
1962	501	0.03	0.06	0.00	0.64	2283	0.06	0.22	0.00	6.14
1967	501	0.03	0.06	0.00	0.64	2283	0.06	0.23	0.00	6.64

Table 1.A.2: *continued*

	Former Confederate States					Other States				
Year	Obs.	Mean	S.D.	Min.	Max.	Obs.	Mean	S.D.	Min.	Max.
per capita highway expenditures ($000s)										
1962	992	0.08	0.06	0.00	0.73	1792	0.14	0.10	0.00	0.93
1967	992	0.09	0.06	0.00	0.45	1792	0.16	0.11	0.00	1.13
logged per capita highway expenditures ($000s)										
1962	992	-2.99	1.33	-8.33	-0.31	1792	-2.23	0.91	-8.33	-0.07
1967	992	-2.89	1.41	-8.33	-0.79	1792	-2.13	0.95	-8.33	0.12
per capita education expenditures ($000s)										
1962	992	0.36	0.12	0.01	1.21	1792	0.49	0.17	0.17	4.35
1967	992	0.53	0.20	0.00	3.57	1792	0.66	0.36	0.01	13.1
logged per capita education expenditures ($000s)										
1962	992	-1.06	0.30	-4.38	0.19	1792	-0.77	0.29	-1.75	1.47
1967	992	-0.69	0.35	-6.29	1.27	1792	-0.47	0.32	-4.47	2.57
per capita welfare expenditures ($000s)										
1962	992	0.02	0.03	0.00	0.22	1792	0.06	0.08	0.00	0.73
1967	992	0.02	0.03	0.00	0.25	1792	0.07	0.09	0.00	1.03
logged per capita welfare expenditures ($000s)										
1962	992	-5.68	2.09	-10.6	-1.51	1792	-3.94	2.04	-10.6	-0.31
1967	992	-5.58	2.24	-10.6	-1.41	1792	-3.91	2.14	-10.6	0.03
per capita long-term debts outstanding ($000s)										
1962	992	0.58	0.44	0.00	3.26	1792	0.70	0.65	0.00	18.1
1967	992	0.77	0.90	0.00	18.0	1792	0.85	1.03	0.00	33.5
logged per capita long-term debts outstanding ($000s)										
1962	992	-0.89	1.01	-7.22	1.18	1792	-0.63	0.89	-7.22	2.90
1967	992	-0.64	1.07	-7.22	2.89	1792	-0.47	0.93	-7.22	3.51
per capita total general revenue ($000s)										
1962	992	0.61	0.20	0.28	1.99	1792	0.91	0.33	0.17	5.07
1967	992	0.84	0.25	0.36	3.59	1792	1.18	0.47	0.12	8.37
logged per capita total general revenue ($000s)										
1962	992	-0.54	0.28	-1.28	0.69	1792	-0.15	0.35	-1.76	1.62
1967	992	-0.22	0.26	-1.02	1.28	1792	0.11	0.35	-2.16	2.12

Table 1.A.2: *continued*

	Former Confederate States					Other States				
Year	Obs.	Mean	S.D.	Min.	Max.	Obs.	Mean	S.D.	Min.	Max.
Non White Population Rate (%)										
1962	992	0.25	0.20	0.00	0.83	1792	0.03	0.06	0.00	0.84
1967	992	0.23	0.19	0.00	0.84	1792	0.03	0.06	0.00	0.84
Median Family Income ($000s)										
1962	992	17.5	4.75	7.12	32.2	1792	23.8	5.65	6.98	42.1
1967	992	22.0	4.95	9.41	38.1	1792	28.2	6.21	8.49	49.8
Unemployment Rate (%)										
1962	992	0.05	0.02	0.00	0.15	1792	0.05	0.03	0.00	0.20
1967	992	0.05	0.02	0.01	0.12	1792	0.05	0.02	0.00	0.17
Urban Population (%)										
1962	992	0.28	0.26	0.00	1.00	1792	0.33	0.28	0.00	1.00
1967	992	0.30	0.26	0.00	1.00	1792	0.34	0.28	0.00	1.00
Elderly Population (%)										
1962	992	0.10	0.03	0.03	0.21	1792	0.12	0.03	0.03	0.22
1967	992	0.11	0.03	0.03	0.24	1792	0.12	0.03	0.03	0.24
Land Area (000s sq.mi.)										
1962	992	0.65	0.43	0.00	6.21	1792	1.07	1.43	0.03	18.6
1967	992	0.65	0.43	0.00	6.21	1792	1.07	1.43	0.03	18.6
Population Growth [10yr] (%)										
1962	992	1.00	0.17	0.67	1.74	1792	1.04	0.17	0.63	1.72
1967	992	1.02	0.14	0.72	1.63	1792	1.03	0.14	0.65	1.63
Population (000,000s)										
1962	992	0.04	0.08	0.00	1.29	1792	0.06	0.24	0.00	6.14
1967	992	0.04	0.09	0.00	1.54	1792	0.07	0.25	0.00	6.64

Table 1.A.2: *continued*

Dependent Variable	logged per capita			
Exlanatory Variables	education expenditure	highway expenditure	long-term debts	welfare expenditure
Year 1967 × Covered Jurisdiction × % Nonwhite Population	0.0113 (0.130)	-0.0525 (0.217)	0.228 (0.353)	0.52 (0.477)
Year 1967 × Covered Jurisdiction	0.0211 (0.0337)	-0.0175 (0.0653)	-0.0468 (0.103)	-0.0706 (0.124)
Year 1967 × % Nonwhite Population	0.126 ** (0.0595)	0.0252 (0.129)	-0.0481 (0.202)	0.167 (0.323)
Covered Jurisdiction × % Nonwhite Population	0.719 (0.977)	0.105 (2.117)	2.535 (4.082)	6.693 (5.902)
Year 1967 (Baseline Year = 1962)	0.288 *** (0.0244)	0.139 *** (0.0334)	0.192 ** (0.0750)	-0.0429 (0.111)
% Nonwhite Population	-1.736 *** (0.643)	-1.19 (1.428)	-6.288 *** (1.968)	-1.888 (3.754)
Median Household Income ($000s)	0.00261 (0.00471)	-0.00882 (0.00629)	0.00609 (0.0139)	0.00862 (0.0199)
% Unemployment Rates	-0.0988 (0.480)	0.0782 (0.807)	3.975 ** (1.631)	-0.184 (2.638)
% Urban Population	-0.0316 (0.145)	-0.139 (0.221)	-0.291 (0.331)	-0.067 (0.808)
% Elderly Population	0.781 (0.815)	0.625 (1.439)	-3.783 * (2.272)	4.961 (3.457)
Land Area (sq.mi)	1.052 * (0.631)	0.107 (1.023)	0.314 (1.975)	-4.73 (5.894)
% Population Growth [10 year]	0.0193 (0.0568)	0.114 (0.0930)	0.566 *** (0.184)	-0.359 (0.349)
Population (000,000s)	-0.0634 (0.143)	0.163 (0.429)	0.398 (0.363)	-0.545 (0.996)
Constant	-1.846 *** (0.612)	-2.445 ** (0.956)	-0.931 (1.929)	-0.715 (5.550)
Within R-square	0.571	0.06	0.071	0.008
County Fixed Effects	Yes	Yes	Yes	Yes
Fiscal Years Included	1962, 67	1962, 67	1962, 67	1962, 67
Number of Counties	2784	2784	2784	2784
Number of Observations	5568	5568	5568	5568

Note: County-clustered standard errors are in parentheses.
* $p<.10$, ** $p<.05$, *** $p<.01$ in two-tailed test.

Table 1.A.3: The differential effects of the proportion of the nonwhite population on the strength of strategic policymaking.

Dependent Variable	ln(per capita education expenditure)			
Exlanatory Variables	(1)	(2)	(3)	(4)
Year 1967 × Covered Jurisdiction	0.047 **		0.0151	
	(0.0184)		(0.0203)	
Year 1967 × Former Confederate States		0.057 ***	0.0502 ***	
		(0.0131)	(0.0141)	
Year 1967 × % Nonwhite Population				0.156 ***
				(0.0575)
Year 1967 (Baseline Year = 1962)	0.296 ***	0.288 ***	0.289 ***	0.286 ***
	(0.0246)	(0.0244)	(0.0245)	(0.0244)
% Nonwhite Population	-1.966 ***	-1.574 ***	-1.466 ***	-1.495 ***
	(0.498)	(0.553)	(0.526)	(0.488)
Median Household Income ($000s)	0.0021	0.00274	0.00244	0.00307
	(0.00469)	(0.00465)	(0.00466)	(0.00468)
% Unemployment Rates	-0.0374	-0.00382	0.0113	-0.139
	(0.500)	(0.493)	(0.498)	(0.490)
% Urban Population	-0.0206	-0.0544	-0.051	-0.0369
	(0.145)	(0.144)	(0.144)	(0.146)
% Elderly Population	0.932	0.175	0.239	0.851
	(0.816)	(0.849)	(0.837)	(0.827)
Land Area (sq.mi)	0.975	1.086 *	1.062 *	1.076 *
	(0.615)	(0.621)	(0.625)	(0.641)
% Population Growth [10 year]	0.00867	-0.0227	-0.0175	0.0214
	(0.0564)	(0.0559)	(0.0561)	(0.0567)
Population (000,000s)	-0.0111	-0.14	-0.123	-0.102
	(0.136)	(0.140)	(0.136)	(0.139)
Constant	-1.71 ***	-1.737 ***	-1.735 ***	-1.862 ***
	(0.602)	(0.606)	(0.609)	(0.620)
Within R-square	0.57	0.572	0.572	0.571
County Fixed Effects	Yes	Yes	Yes	Yes
Fiscal Years Included	1962, 67	1962, 67	1962, 67	1962, 67
Number of Counties	2784	2784	2784	2784
Number of Observations	5568	5568	5568	5568

Note: County-clustered standard errors are in parentheses.
* $p<.10$, ** $p<.05$, *** $p<.01$ in two-tailed test.

Table 1.A.4: The effects of strategic policymaking on per capita education expenditures.

Dependent Variable	ln(per capita highway expenditure)			
Exlanatory Variables	(1)	(2)	(3)	(4)
Year 1967 × Covered Jurisdiction	-0.0289		-0.0174	
	(0.0339)		(0.0340)	
Year 1967 × Former Confederate States		-0.0258	-0.0181	
		(0.0249)	(0.0235)	
Year 1967 × % Nonwhite Population				-0.0404
				(0.0876)
Year 1967 (Baseline Year = 1962)	0.139 ***	0.143 ***	0.142 ***	0.142 ***
	(0.0331)	(0.0332)	(0.0330)	(0.0335)
% Nonwhite Population	-1.137	-1.194	-1.317	-0.992
	(0.933)	(0.904)	(0.996)	(1.080)
Median Household Income ($000s)	-0.00866	-0.00913	-0.00878	-0.0093
	(0.00628)	(0.00638)	(0.00627)	(0.00640)
% Unemployment Rates	0.047	0.0469	0.0294	0.107
	(0.788)	(0.790)	(0.791)	(0.784)
% Urban Population	-0.138	-0.123	-0.127	-0.134
	(0.220)	(0.221)	(0.221)	(0.221)
% Elderly Population	0.67	0.993	0.919	0.647
	(1.408)	(1.538)	(1.543)	(1.422)
Land Area (sq.mi)	0.0767	0.018	0.0454	0.0319
	(1.010)	(1.003)	(1.007)	(1.008)
% Population Growth [10 year]	0.117	0.132	0.126	0.116
	(0.0924)	(0.0922)	(0.0929)	(0.0930)
Population (000,000s)	0.176	0.236	0.216	0.203
	(0.411)	(0.409)	(0.409)	(0.414)
Constant	-2.428 ***	-2.416 ***	-2.419 ***	-2.390 **
	(0.938)	(0.931)	(0.932)	(0.946)
Within R-square	0.06	0.06	0.06	0.059
County Fixed Effects	Yes	Yes	Yes	Yes
Fiscal Years Included	1962, 67	1962, 67	1962, 67	1962, 67
Number of Counties	2784	2784	2784	2784
Number of Observations	5568	5568	5568	5568

Note: County-clustered standard errors are in parentheses.
* $p<.10$, ** $p<.05$, *** $p<.01$ in two-tailed test.

Table 1.A.4: *continued*

Dependent Variable	ln(per capita long-term debts outstanding)			
Exlanatory Variables	(1)	(2)	(3)	(4)
Year 1967 × Covered Jurisdiction	-0.00803		-0.0512	
	(0.0483)		(0.0519)	
Year 1967 × Former Confederate States		0.0452	0.068 *	
		(0.0376)	(0.0398)	
Year 1967 × % Nonwhite Population				0.0256
				(0.133)
Year 1967 (Baseline Year = 1962)	0.192 **	0.186 **	0.183 **	0.19 **
	(0.0760)	(0.0772)	(0.0769)	(0.0758)
% Nonwhite Population	-5.399 ***	-4.361 **	-4.723 **	-5.069 ***
	(1.925)	(2.039)	(2.062)	(1.956)
Median Household Income ($000s)	0.00552	0.00496	0.00598	0.00531
	(0.0141)	(0.0142)	(0.0142)	(0.0143)
% Unemployment Rates	4.097 ***	4.214 ***	4.163 ***	4.112 ***
	(1.582)	(1.583)	(1.584)	(1.578)
% Urban Population	-0.303	-0.333	-0.344	-0.307
	(0.330)	(0.331)	(0.332)	(0.331)
% Elderly Population	-3.67	-4.391 *	-4.608 **	-3.725
	(2.245)	(2.284)	(2.324)	(2.276)
Land Area (sq.mi)	0.371	0.408	0.489	0.37
	(1.962)	(1.950)	(1.954)	(1.956)
% Population Growth [10 year]	0.557 ***	0.539 ***	0.521 ***	0.561 ***
	(0.182)	(0.183)	(0.183)	(0.183)
Population (000,000s)	0.298	0.205	0.147	0.286
	(0.328)	(0.338)	(0.329)	(0.336)
Constant	-0.908	-0.935	-0.942	-0.935
	(1.921)	(1.916)	(1.913)	(1.917)
Within R-square	0.071	0.072	0.072	0.071
County Fixed Effects	Yes	Yes	Yes	Yes
Fiscal Years Included	1962, 67	1962, 67	1962, 67	1962, 67
Number of Counties	2784	2784	2784	2784
Number of Observations	5568	5568	5568	5568

Note: County-clustered standard errors are in parentheses.
* $p<.10$, ** $p<.05$, *** $p<.01$ in two-tailed test.

Table 1.A.4: *continued*

Dependent Variable	ln(per capita welfare expenditure)			
Exlanatory Variables	(1)	(2)	(3)	(4)
Year 1967 × Covered Jurisdiction	0.0728		0.0551	
	(0.0791)		(0.0956)	
Year 1967 × Former Confederate States		0.0524	0.0279	
		(0.0610)	(0.0740)	
Year 1967 × % Nonwhite Population				0.376 *
				(0.199)
Year 1967 (Baseline Year = 1962)	-0.0252	-0.032	-0.0289	-0.0492
	(0.110)	(0.111)	(0.111)	(0.110)
% Nonwhite Population	-0.621	-0.734	-0.344	1.169
	(2.743)	(2.828)	(2.850)	(2.886)
Median Household Income ($000s)	0.00625	0.00754	0.00644	0.00761
	(0.0201)	(0.0200)	(0.0201)	(0.0200)
% Unemployment Rates	0.219	0.191	0.246	0.0552
	(2.610)	(2.605)	(2.609)	(2.601)
% Urban Population	-0.0686	-0.0979	-0.0855	-0.11
	(0.804)	(0.808)	(0.812)	(0.803)
% Elderly Population	5.5	4.882	5.115	5.195
	(3.454)	(3.581)	(3.577)	(3.449)
Land Area (sq.mi)	-4.766	-4.631	-4.718	-4.569
	(5.870)	(5.868)	(5.887)	(5.947)
% Population Growth [10 year]	-0.402	-0.436	-0.417	-0.365
	(0.349)	(0.350)	(0.346)	(0.349)
Population (000,000s)	-0.641	-0.766	-0.703	-0.853
	(0.973)	(1.018)	(1.000)	(1.008)
Constant	-0.377	-0.398	-0.391	-0.75
	(5.520)	(5.525)	(5.532)	(5.599)
Within R-square	0.005	0.005	0.005	0.007
County Fixed Effects	Yes	Yes	Yes	Yes
Fiscal Years Included	1962, 67	1962, 67	1962, 67	1962, 67
Number of Counties	2784	2784	2784	2784
Number of Observations	5568	5568	5568	5568

Note: County-clustered standard errors are in parentheses.
* $p<.10$, ** $p<.05$, *** $p<.01$ in two-tailed test.

Table 1.A.4: *continued*

Dependent Variable	ln(per capita long-term debts outstanding)			
Exlanatory Variables	(1)	(2)	(3)	(4)
Year 1967 × Covered Jurisdiction × ln(per capita total revenue in 1967)	0.537 **		0.601 **	
	(0.219)		(0.240)	
Year 1967 × Covered Jurisdiction	0.143 **		0.1	
	(0.0578)		(0.0637)	
Year 1967 × Former Confederate States × ln(per capita total revenue in 1967)		0.162	-0.111	
		(0.139)	(0.137)	
Year 1967 × Former Confederate States		0.111 **	0.0783	
		(0.0445)	(0.0478)	
Year 1967 × % Nonwhite Population × ln(per capita total revenue in 1967)				0.33
				(0.407)
Year 1967 × % Nonwhite Population				0.132
				(0.134)
Year 1967 × ln(per capita total revenue in 1967)	0.0545	0.105	0.101	0.0874
	(0.0550)	(0.0676)	(0.0676)	(0.0624)
% Nonwhite Population × ln(per capita total revenue in 1967)				2.451
				(4.424)
Year 1967 (Baseline Year = 1962)	0.174 **	0.162 **	0.164 **	0.178 **
	(0.0759)	(0.0775)	(0.0772)	(0.0760)
% Nonwhite Population	-5.628 ***	-4.534 **	-4.858 **	-5.224 ***
	(1.959)	(2.048)	(2.063)	(1.847)
Median Household Income ($000s)	0.00912	0.00876	0.00866	0.00644
	(0.0141)	(0.0143)	(0.0142)	(0.0143)
% Unemployment Rates	4.033 **	3.958 **	3.972 **	3.823 **
	(1.588)	(1.595)	(1.589)	(1.600)
% Urban Population	-0.296	-0.277	-0.349	-0.245
	(0.332)	(0.335)	(0.330)	(0.339)
% Elderly Population	-3.806 *	-5.202 **	-5.17 **	-3.918 *
	(2.235)	(2.325)	(2.348)	(2.280)
Land Area (sq.mi)	0.225	0.294	0.528	0.5
	(1.962)	(1.952)	(1.948)	(1.948)
% Population Growth [10 year]	0.644 ***	0.685 ***	0.602 ***	0.675 ***
	(0.184)	(0.189)	(0.190)	(0.186)
Population (000,000s)	0.244	0.0478	0.031	0.0999
	(0.321)	(0.328)	(0.324)	(0.343)
Constant	-0.897	-0.949	-1.025	-1.106
	(1.919)	(1.910)	(1.901)	(1.901)
Within R-square	0.077	0.075	0.079	0.073
County Fixed Effects	Yes	Yes	Yes	Yes
Fiscal Years Included	1962, 67	1962, 67	1962, 67	1962, 67
Number of Counties	2784	2784	2784	2784
Number of Observations	5568	5568	5568	5568

Note: County-clustered standard errors are in parentheses.
* $p<.10$, ** $p<.05$, *** $p<.01$ in two-tailed test.

Table 1.A.5: The effects of SUD on per capita long-term debts outstanding.

Dependent Variable	ln(per capita education expenditure)			
Exlanatory Variables	(1)	(2)	(3)	(4)
Year 1967 × Covered Jurisdiction × ln(per capita total revenue in 1967)	0.29 ***		0.286 ***	
	(0.0765)		(0.0873)	
Year 1967 × Covered Jurisdiction	0.14 ***		0.0929 ***	
	(0.0266)		(0.0289)	
Year 1967 × Former Confederate States × ln(per capita total revenue in 1967)		0.0901 *	-0.0246	
		(0.0463)	(0.0479)	
Year 1967 × Former Confederate States		0.109 ***	0.0759 ***	
		(0.0164)	(0.0165)	
Year 1967 × % Nonwhite Population × ln(per capita total revenue in 1967)				0.395 ***
				(0.129)
Year 1967 × % Nonwhite Population				0.282 ***
				(0.0670)
Year 1967 × ln(per capita total revenue in 1967)	0.0743 ***	0.108 ***	0.105 ***	0.0754 ***
	(0.0176)	(0.0198)	(0.0198)	(0.0189)
% Nonwhite Population × ln(per capita total revenue in 1967)				-0.289
				(1.295)
Year 1967 (Baseline Year = 1962)	0.283 ***	0.27 ***	0.272 ***	0.273 ***
	(0.0244)	(0.0242)	(0.0243)	(0.0244)
% Nonwhite Population	-2.245 ***	-1.724 ***	-1.561 ***	-2.01 ***
	(0.490)	(0.543)	(0.513)	(0.534)
Median Household Income ($000s)	0.00431	0.0053	0.0046	0.00458
	(0.00464)	(0.00459)	(0.00459)	(0.00463)
% Unemployment Rates	-0.204	-0.28	-0.223	-0.363
	(0.505)	(0.498)	(0.502)	(0.498)
% Urban Population	0.00696	-0.00627	-0.0288	0.0396
	(0.139)	(0.140)	(0.139)	(0.142)
% Elderly Population	0.768	-0.581	-0.383	0.642
	(0.810)	(0.849)	(0.834)	(0.822)
Land Area (sq.mi)	0.903	1.059 *	1.081 *	1.138 *
	(0.611)	(0.615)	(0.623)	(0.650)
% Population Growth [10 year]	0.101 *	0.1 *	0.0808	0.128 **
	(0.0601)	(0.0603)	(0.0606)	(0.0610)
Population (000,000s)	-0.0557	-0.279 *	-0.239 *	-0.238
	(0.133)	(0.146)	(0.135)	(0.147)
Constant	-1.736 ***	-1.787 ***	-1.809 ***	-1.995 ***
	(0.596)	(0.600)	(0.603)	(0.631)
Within R-square	0.58	0.58	0.583	0.579
County Fixed Effects	Yes	Yes	Yes	Yes
Fiscal Years Included	1962, 67	1962, 67	1962, 67	1962, 67
Number of Counties	2784	2784	2784	2784
Number of Observations	5568	5568	5568	5568

Note: County-clustered standard errors are in parentheses.
* $p<.10$, ** $p<.05$, *** $p<.01$ in two-tailed test.

Table 1.A.5: *continued*

Dependent Variable	ln(per capita highway expenditure)			
Exlanatory Variables	(1)	(2)	(3)	(4)
Year 1967 × Covered Jurisdiction × ln(per capita total revenue in 1967)	0.479 ***		0.626 ***	
	(0.135)		(0.144)	
Year 1967 × Covered Jurisdiction	0.114 ***		0.135 ***	
	(0.0413)		(0.0431)	
Year 1967 × Former Confederate States × ln(per capita total revenue in 1967)		0.0977	-0.176 ***	
		(0.0747)	(0.0656)	
Year 1967 × Former Confederate States		0.0285	-0.0172	
		(0.0262)	(0.0264)	
Year 1967 × % Nonwhite Population × ln(per capita total revenue in 1967)				0.573 *
				(0.314)
Year 1967 × % Nonwhite Population				0.139
				(0.0882)
Year 1967 × ln(per capita total revenue in 1967)	0.0815 ***	0.111 ***	0.107 ***	0.0872 ***
	(0.0257)	(0.0290)	(0.0290)	(0.0326)
% Nonwhite Population × ln(per capita total revenue in 1967)				-2.166
				(2.027)
Year 1967 (Baseline Year = 1962)	0.121 ***	0.123 ***	0.126 ***	0.124 ***
	(0.0326)	(0.0328)	(0.0324)	(0.0334)
% Nonwhite Population	-1.456	-1.35	-1.474	-1.897
	(0.921)	(0.896)	(0.972)	(1.026)
Median Household Income ($000s)	-0.00526	-0.00641	-0.00696	-0.00708
	(0.00611)	(0.00619)	(0.00610)	(0.00635)
% Unemployment Rates	-0.106	-0.237	-0.188	-0.13
	(0.804)	(0.809)	(0.805)	(0.810)
% Urban Population	-0.114	-0.0724	-0.139	-0.0252
	(0.224)	(0.227)	(0.223)	(0.229)
% Elderly Population	0.482	0.209	0.367	0.368
	(1.388)	(1.527)	(1.522)	(1.410)
Land Area (sq.mi)	-0.0485	-0.0162	0.167	0.0679
	(0.976)	(0.980)	(0.994)	(0.986)
% Population Growth [10 year]	0.228 **	0.26 ***	0.188 **	0.248 ***
	(0.0935)	(0.0968)	(0.0948)	(0.0952)
Population (000,000s)	0.117	0.0914	0.107	0.058
	(0.404)	(0.415)	(0.402)	(0.418)
Constant	-2.441 ***	-2.465 ***	-2.535 ***	-2.535 ***
	(0.906)	(0.911)	(0.921)	(0.923)
Within R-square	0.08	0.07	0.082	0.074
County Fixed Effects	Yes	Yes	Yes	Yes
Fiscal Years Included	1962, 67	1962, 67	1962, 67	1962, 67
Number of Counties	2784	2784	2784	2784
Number of Observations	5568	5568	5568	5568

Note: County-clustered standard errors are in parentheses.
* $p<.10$, ** $p<.05$, *** $p<.01$ in two-tailed test.

Table 1.A.5: *continued*

Dependent Variable	ln(per capita welfare expenditure)			
Exlanatory Variables	(1)	(2)	(3)	(4)
Year 1967 × Covered Jurisdiction × ln(per capita total revenue in 1967)	0.356		0.559	
	(0.260)		(0.355)	
Year 1967 × Covered Jurisdiction	0.188 *		0.186	
	(0.109)		(0.132)	
Year 1967 × Former Confederate States × ln(per capita total revenue in 1967)		-0.0405	-0.264	
		(0.192)	(0.265)	
Year 1967 × Former Confederate States		0.0928	0.027	
		(0.0760)	(0.0912)	
Year 1967 × % Nonwhite Population × ln(per capita total revenue in 1967)				0.378
				(0.440)
Year 1967 × % Nonwhite Population				0.556 **
				(0.235)
Year 1967 × ln(per capita total revenue in 1967)	0.0969	0.158 **	0.153 **	0.111
	(0.0694)	(0.0778)	(0.0779)	(0.0712)
% Nonwhite Population × ln(per capita total revenue in 1967)				-8.647
				(5.873)
Year 1967 (Baseline Year = 1962)	-0.0415	-0.0471	-0.0424	-0.0696
	(0.110)	(0.111)	(0.111)	(0.110)
% Nonwhite Population	-0.984	-0.892	-0.548	-0.516
	(2.780)	(2.833)	(2.843)	(3.041)
Median Household Income ($000s)	0.00899	0.00822	0.0068	0.0108
	(0.0200)	(0.0199)	(0.0200)	(0.0199)
% Unemployment Rates	-0.00133	-0.245	-0.13	-0.239
	(2.624)	(2.633)	(2.631)	(2.626)
% Urban Population	-0.0318	-0.049	-0.0921	0.0232
	(0.807)	(0.808)	(0.806)	(0.816)
% Elderly Population	5.286	3.947	4.349	4.805
	(3.487)	(3.702)	(3.704)	(3.479)
Land Area (sq.mi)	-4.854	-4.466	-4.431	-4.684
	(5.865)	(5.885)	(5.910)	(5.864)
% Population Growth [10 year]	-0.283	-0.317	-0.353	-0.221
	(0.348)	(0.357)	(0.360)	(0.349)
Population (000,000s)	-0.698	-0.918	-0.835	-0.835
	(0.975)	(1.028)	(1.002)	(1.060)
Constant	-0.412	-0.551	-0.594	-0.833
	(5.511)	(5.531)	(5.540)	(5.525)
Within R-square	0.007	0.007	0.008	0.009
County Fixed Effects	Yes	Yes	Yes	Yes
Fiscal Years Included	1962, 67	1962, 67	1962, 67	1962, 67
Number of Counties	2784	2784	2784	2784
Number of Observations	5568	5568	5568	5568

Note: County-clustered standard errors are in parentheses.
* $p<.10$, ** $p<.05$, *** $p<.01$ in two-tailed test.

Table 1.A.5: *continued*

Dependent Variable	ln(per capita total revenue in time *t*)			
Exlanatory Variables	(1)	(2)	(3)	(4)
ln(per capita total revenue in time *t-1*)	**-0.127** ***	**-0.141** ***	**-0.135** ***	**-0.141** ***
	(0.024)	**(0.025)**	**(0.025)**	**(0.025)**
Year 1967 × Covered Jurisdiction	0.064 ***		0.048 ***	
	(0.010)		(0.011)	
Year 1967 × Former Confederate States		0.048 ***	0.026 ***	
		(0.008)	(0.009)	
Year 1967 × % Nonwhite Population				0.179 ***
				(0.026)
Year 1967 (Baseline Year = 1962)	0.273 ***	0.269 ***	0.271 ***	0.264 ***
	(0.014)	(0.014)	(0.014)	(0.014)
% Nonwhite Population	-1.645 ***	-1.724 ***	-1.390 ***	-1.279 ***
	(0.328)	(0.329)	(0.335)	(0.346)
Median Household Income ($000s)	0.003	0.004	0.003	0.004
	(0.003)	(0.003)	(0.003)	(0.003)
% Unemployment Rates	-0.504	-0.522	-0.477	-0.638 *
	(0.329)	(0.329)	(0.330)	(0.327)
% Urban Population	0.075	0.051	0.061	0.060
	(0.080)	(0.081)	(0.081)	(0.082)
% Elderly Population	1.486 ***	0.951 **	1.143 **	1.453 ***
	(0.442)	(0.457)	(0.455)	(0.446)
Land Area (sq.mi)	0.829 **	0.949 ***	0.874 **	0.955 **
	(0.368)	(0.363)	(0.370)	(0.385)
% Population Growth [10 year]	-0.029	-0.060	-0.043	-0.017
	(0.036)	(0.037)	(0.036)	(0.037)
Population (000,000s)	-0.110	-0.223	-0.168	-0.217
	(0.134)	(0.177)	(0.152)	(0.156)
Constant	-1.127 ***	-1.158 ***	-1.147 ***	-1.312 ***
	(0.354)	(0.350)	(0.356)	(0.369)
Within R-square	0.78	0.78	0.78	0.78
County Fixed Effects	Yes	Yes	Yes	Yes
Angrist-Pischke Chi-square statistics (*df*=1)	27.16 ***	32.21 ***	29.53 ***	32.82 ***
Angrist-Pischke F-statistics (*df*=1)	27.1 ***	32.14 ***	29.46 ***	32.74 ***
Fiscal Years Included	1962, 67	1962, 67	1962, 67	1962, 67
Number of Counties	2784	2784	2784	2784
Number of Observations	5568	5568	5568	5568

Note: County-clustered standard errors are in parentheses. The exclusion restriction is indicated in **boldface**. * p<.10, ** p<.05, *** p<.01 in two-tailed test.

Table 1.A.6: The first stage regression of IV estimation.

Dependent Variable	ln(per capita long-term debts outstanding)			
Exlanatory Variables	(1)	(2)	(3)	(4)
Year 1967 × Covered Jurisdiction × IV[ln(per capita total revenue in 1967)]	0.405 *		0.543 **	
	(0.230)		(0.247)	
Year 1967 × Covered Jurisdiction	0.0964		0.0756	
	(0.0609)		(0.0659)	
Year 1967 × Former Confederate States × IV[ln(per capita total revenue in 1967)]		0.0256	-0.194	
		(0.136)	(0.133)	
Year 1967 × Former Confederate States		0.0681	0.051	
		(0.0437)	(0.0468)	
Year 1967 × % Nonwhite Population × IV[ln(per capita total revenue in 1967)]				-0.332
				(0.384)
Year 1967 × % Nonwhite Population				-0.0113
				(0.138)
% Nonwhite Population × IV[ln(per capita total revenue in 1967)]				-3.566
				(1.578)
Year 1967 × IV[ln(per capita total revenue in 1967)]	-0.00162	0.057	0.0542	0.0444
	(0.0576)	(0.0725)	(0.0725)	(0.0644)
Year 1967 (Baseline Year = 1962)	0.181 **	0.177 **	0.176 **	0.198
	(0.0758)	(0.0774)	(0.0772)	(0.0754)
% Nonwhite Population	-5.376 ***	-4.432 **	-4.808 **	-5.483
	(1.960)	(2.043)	(2.060)	(1.947)
Median Household Income ($000s)	0.00806	0.00596	0.00649	0.00355
	(0.0141)	(0.0143)	(0.0142)	(0.0141)
% Unemployment Rates	4.166 ***	4.066 **	4.037 **	3.836
	(1.593)	(1.599)	(1.596)	(1.606)
% Urban Population	-0.306	-0.309	-0.364	-0.305
	(0.333)	(0.334)	(0.329)	(0.336)
% Elderly Population	-3.697 *	-4.78 **	-4.789 **	-3.837
	(2.245)	(2.340)	(2.363)	(2.277)
Land Area (sq.mi)	0.207	0.419	0.58	0.244
	(1.976)	(1.957)	(1.963)	(1.971)
% Population Growth [10 year]	0.576 ***	0.596 ***	0.521 ***	0.617
	(0.185)	(0.189)	(0.190)	(0.187)
Population (000,000s)	0.259	0.135	0.0985	0.469
	(0.317)	(0.332)	(0.327)	(0.353)
Constant	-0.831	-0.971	-0.992	-0.847
	(1.932)	(1.916)	(1.914)	(1.929)
Within R-square	0.073	0.072	0.075	0.077
County Fixed Effects	Yes	Yes	Yes	Yes
Fiscal Years Included	1962, 67	1962, 67	1962, 67	1962, 67
Number of Counties	2784	2784	2784	2784
Number of Observations	5568	5568	5568	5568

Note: County-clustered standard errors are in parentheses. * $p<.10$, ** $p<.05$, *** $p<.01$ in two-tailed test. See Table A6 for the results of the first stage regressions.

Table 1.A.7: The second stage regression of IV estimation of Table 3.

Dependent Variable	ln(per capita education expenditure)			
Exlanatory Variables	(1)	(2)	(3)	(4)
Year 1967 × Covered Jurisdiction × IV[ln(per capita total revenue in 1967)]	0.11		0.162	
	(0.105)		(0.111)	
Year 1967 × Covered Jurisdiction	0.0776 **		0.0523	
	(0.0347)		(0.0364)	
Year 1967 × Former Confederate States × IV[ln(per capita total revenue in 1967)]		-0.0316	-0.0859 *	
		(0.0496)	(0.0461)	
Year 1967 × Former Confederate States		0.064 ***	0.048 ***	
		(0.0174)	(0.0170)	
Year 1967 × % Nonwhite Population × IV[ln(per capita total revenue in 1967)]				-0.288
				(0.247)
Year 1967 × % Nonwhite Population				0.119
				(0.0877)
% Nonwhite Population × IV[ln(per capita total revenue in 1967)]				-4.083 ***
				(0.730)
Year 1967 × IV[ln(per capita total revenue in 1967)]	0.00874	0.0441 **	0.0429 **	0.0261
	(0.0168)	(0.0191)	(0.0191)	(0.0185)
Year 1967 (Baseline Year = 1962)	0.292 ***	0.286 ***	0.286 ***	0.297 ***
	(0.0245)	(0.0245)	(0.0245)	(0.0243)
% Nonwhite Population	-1.992 ***	-1.61 ***	-1.517 ***	-1.955 ***
	(0.501)	(0.556)	(0.526)	(0.493)
Median Household Income ($000s)	0.00285	0.00253	0.00232	0.00098
	(0.00469)	(0.00469)	(0.00469)	(0.00461)
% Unemployment Rates	-0.0454	-0.132	-0.108	-0.358
	(0.507)	(0.501)	(0.507)	(0.508)
% Urban Population	-0.0165	-0.0447	-0.0527	-0.0378
	(0.143)	(0.144)	(0.143)	(0.140)
% Elderly Population	0.906	-0.0538	0.0534	0.765
	(0.817)	(0.861)	(0.844)	(0.808)
Land Area (sq.mi)	0.932	1.158 *	1.146 *	0.924 *
	(0.618)	(0.622)	(0.629)	(0.537)
% Population Growth [10 year]	0.023	0.00336	-0.00526	0.0654
	(0.0594)	(0.0595)	(0.0601)	(0.0588)
Population (000,000s)	-0.0249	-0.173	-0.155	0.101
	(0.134)	(0.141)	(0.135)	(0.176)
Constant	-1.695 ***	-1.791 ***	-1.788 ***	-1.742 ***
	(0.603)	(0.606)	(0.610)	(0.530)
Within R-square	0.571	0.572	0.573	0.598
County Fixed Effects	Yes	Yes	Yes	Yes
Fiscal Years Included	1962, 67	1962, 67	1962, 67	1962, 67
Number of Counties	2784	2784	2784	2784
Number of Observations	5568	5568	5568	5568

Note: County-clustered standard errors are in parentheses. * $p<.10$, ** $p<.05$, *** $p<.01$ in two-tailed test. See Table A6 for the results of the first stage regressions.

Table 1.A.7: *continued*

Dependent Variable	ln(per capita highway expenditure)			
Exlanatory Variables	(1)	(2)	(3)	(4)
Year 1967 × Covered Jurisdiction × IV[ln(per capita total revenue in 1967)]	0.402 ***		0.606 ***	
	(0.146)		(0.153)	
Year 1967 × Covered Jurisdiction	0.0863 **		0.123 ***	
	(0.0398)		(0.0417)	
Year 1967 × Former Confederate States × IV[ln(per capita total revenue in 1967)]		-0.0135	-0.243 ***	
		(0.0739)	(0.0602)	
Year 1967 × Former Confederate States		-0.00299	-0.0363	
		(0.0252)	(0.0258)	
Year 1967 × % Nonwhite Population × IV[ln(per capita total revenue in 1967)]				0.382
				(0.346)
Year 1967 × % Nonwhite Population				0.0756
				(0.0879)
% Nonwhite Population × IV[ln(per capita total revenue in 1967)]				-2.141 ***
				(0.648)
Year 1967 × IV[ln(per capita total revenue in 1967)]	0.0462 *	0.083 ***	0.0793 ***	0.0522
	(0.0256)	(0.0297)	(0.0296)	(0.0332)
Year 1967 (Baseline Year = 1962)	0.125 ***	0.134 ***	0.134 ***	0.135 ***
	(0.0327)	(0.0327)	(0.0324)	(0.0331)
% Nonwhite Population	-1.28	-1.28	-1.432	-1.684
	(0.925)	(0.904)	(0.974)	(1.133)
Median Household Income ($000s)	-0.00586	-0.00864	-0.00853	-0.00878
	(0.00611)	(0.00615)	(0.00609)	(0.00629)
% Unemployment Rates	-0.0239	-0.182	-0.171	-0.0791
	(0.806)	(0.811)	(0.808)	(0.815)
% Urban Population	-0.115	-0.0965	-0.147	-0.0629
	(0.223)	(0.226)	(0.221)	(0.228)
% Elderly Population	0.546	0.499	0.631	0.46
	(1.395)	(1.538)	(1.535)	(1.410)
Land Area (sq.mi)	-0.0787	0.0961	0.197	0.0144
	(0.994)	(1.000)	(1.014)	(0.976)
% Population Growth [10 year]	0.184 *	0.197 **	0.132	0.212 **
	(0.0938)	(0.0962)	(0.0948)	(0.0943)
Population (000,000s)	0.121	0.155	0.152	0.154
	(0.412)	(0.408)	(0.397)	(0.404)
Constant	-2.384 ***	-2.494 ***	-2.505 ***	-2.441 ***
	(0.924)	(0.930)	(0.939)	(0.915)
Within R-square	0.070	0.063	0.074	0.075
County Fixed Effects	Yes	Yes	Yes	Yes
Fiscal Years Included	1962, 67	1962, 67	1962, 67	1962, 67
Number of Counties	2784	2784	2784	2784
Number of Observations	5568	5568	5568	5568

Note: County-clustered standard errors are in parentheses. * $p<.10$, ** $p<.05$, *** $p<.01$ in two-tailed test. See Table A6 for the results of the first stage regressions.

Table 1.A.7: *continued*

Dependent Variable	ln(per capita welfare expenditure)			
Exlanatory Variables	(1)	(2)	(3)	(4)
Year 1967 × Covered Jurisdiction × IV[ln(per capita total revenue in 1967)]	0.174		0.438	
	(0.275)		(0.357)	
Year 1967 × Covered Jurisdiction	0.129		0.147	
	(0.115)		(0.135)	
Year 1967 × Former Confederate States × IV[ln(per capita total revenue in 1967)]		-0.189	-0.333	
		(0.192)	(0.254)	
Year 1967 × Former Confederate States		0.0484	0.00301	
		(0.0766)	(0.0896)	
Year 1967 × % Nonwhite Population × IV[ln(per capita total revenue in 1967)]				-0.464
				(0.638)
Year 1967 × % Nonwhite Population				0.342
				(0.249)
% Nonwhite Population × IV[ln(per capita total revenue in 1967)]				-5.455 ***
				(1.906)
Year 1967 × IV[ln(per capita total revenue in 1967)]	0.0458	0.114	0.11	0.0918
	(0.0711)	(0.0821)	(0.0821)	(0.0737)
Year 1967 (Baseline Year = 1962)	-0.0332	-0.0304	-0.0293	-0.0402
	(0.110)	(0.111)	(0.112)	(0.111)
% Nonwhite Population	-0.772	-0.789	-0.503	0.436
	(2.784)	(2.840)	(2.853)	(2.860)
Median Household Income ($000s)	0.00761	0.00494	0.00434	0.00528
	(0.0200)	(0.0200)	(0.0202)	(0.0200)
% Unemployment Rates	0.113	-0.169	-0.0969	-0.443
	(2.622)	(2.633)	(2.635)	(2.616)
% Urban Population	-0.0453	-0.0908	-0.111	-0.0926
	(0.806)	(0.803)	(0.803)	(0.806)
% Elderly Population	5.394	4.44	4.745	4.968
	(3.481)	(3.690)	(3.690)	(3.476)
Land Area (sq.mi)	-4.829	-4.311	-4.352	-4.745
	(5.870)	(5.885)	(5.911)	(5.799)
% Population Growth [10 year]	-0.347	-0.406	-0.427	-0.252
	(0.348)	(0.359)	(0.360)	(0.349)
Population (000,000s)	-0.674	-0.806	-0.755	-0.6
	(0.977)	(1.007)	(0.991)	(0.987)
Constant	-0.376	-0.595	-0.586	-0.65
	(5.516)	(5.531)	(5.539)	(5.462)
Within R-square	0.006	0.006	0.007	0.013
County Fixed Effects	Yes	Yes	Yes	Yes
Fiscal Years Included	1962, 67	1962, 67	1962, 67	1962, 67
Number of Counties	2784	2784	2784	2784
Number of Observations	5568	5568	5568	5568

Note: County-clustered standard errors are in parentheses. * $p<.10$, ** $p<.05$, *** $p<.01$ in two-tailed test. See Table A6 for the results of the first stage regressions.

Table 1.A.7: *continued*

Dependent Variable	logged per capita	
Exlanatory Variables	education expenditure	long-term debts outstanding
Year 1967 × CJ × ln(per capita total revenue in 1967) × Ending Date of Fiscal Year		**-0.0686** **(0.197)**
Year 1967 × ln(per capita total revenue in 1967) × Ending Date of Fiscal Year		0.185 *** (0.0569)
Year 1967 × Covered Jurisdiction × ln(per capita total revenue in 1967)		0.548 ** (0.243)
Year 1967 × ln(per capita total revenue in 1967)		0.0242 (0.0495)
Year 1967 × Covered Jurisdiction × Ending Date of Fiscal YR	**-0.00621** **(0.0176)**	-0.0109 (0.0575)
Year 1967 × Ending Date of Fiscal Year	-0.00434 (0.00615)	0.0273 (0.0188)
Year 1967 × Covered Jurisdiction	0.0438 ** (0.0184)	0.136 ** (0.0676)
Year 1967 (Baseline Year = 1962)	0.294 *** (0.0246)	0.157 ** (0.0760)
% Nonwhite Population	-1.972 *** (0.487)	-5.519 *** (1.968)
Median Household Income ($000s)	0.00249 (0.00476)	0.0158 (0.0143)
% Unemployment Rates	-0.0559 (0.503)	3.635 ** (1.614)
% Urban Population	-0.0159 (0.151)	-0.443 (0.342)
% Elderly Population	1.046 (0.820)	-3.418 (2.282)
Land Area (sq.mi)	0.962 (0.617)	0.352 (1.976)
% Population Growth [10 year]	0.00822 (0.0577)	0.65 *** (0.185)
Population (000,000s)	0.0107 (0.138)	0.032 (0.361)
Constant	-1.723 *** (0.606)	-1.147 (1.946)
Within R-square	0.569	0.084
County Fixed Effects	Yes	Yes
Fiscal Years Included	1962, 67	1962, 67
Number of Counties	2784	2784
Number of Observations	5568	5568

Note: County-clustered standard errors are in parentheses. Quantities of interest are indicated in **boldface**. * p<.10, ** p<.05, *** p<.01 in two-tailed test.

Table 1.A.8: The differential effects of the ending dates of the fiscal year on the strength of strategic policymaking.

Dependent Variable	Δ ln(per capita education expenditure)		
	No matching	Nearest neighbor matching	
Exlanatory Variables	(1)	(2)	(3)
Δ Year 1967 × Former Confederate States	**0.057** ***	**0.105** ***	**0.065** **
	(0.013)	**(0.024)**	**(0.025)**
Δ % Nonwhite Population	-1.574 ***	n/a	n/a
	(0.554)		
Δ Median Household Income ($000s)	0.003	n/a	n/a
	(0.005)		
Δ % Unemployment Rates	-0.004	n/a	n/a
	(0.493)		
Δ % Urban Population	-0.054	n/a	n/a
	(0.144)		
Δ % Elderly Population	0.175	n/a	n/a
	(0.849)		
Δ Land Area (sq.mi)	1.086 *	n/a	n/a
	(0.621)		
Δ % Population Growth [10 year]	-0.023	n/a	n/a
	(0.056)		
Δ Population (000,000s)	-0.140	n/a	n/a
	(0.140)		
Constant	0.288 ***	n/a	n/a
	(0.024)		
R-square	0.020	n/a	n/a
Exact match on % Nonwhite Population		No	Yes
Years First Differenced	1962, 67	1962, 67	1962, 67
Number of Observations	2784	2784	2784

Note: Δ represents the first difference between two censuses. Robust standard errors are in parentheses. Quantities of interest are indicated in **boldface**. * $p<.10$, ** $p<.05$, *** $p<.01$ in two-tailed test.

Table 1.A.9: The first differenced and the nearest neighbor matching estimators of the test of strategic policymaking.

Dependent Variable: logged per capita	education	highway	long-tm debts	welfare
Exlanatory Variables	(1)	(2)	(3)	(4)
Year 1967 × Covered Jurisdiction ×	0.229 ***	0.480 ***	0.546 **	0.345
ln(per capita total revenue in 1967)	(0.078)	(0.136)	(0.219)	(0.267)
Year 1967 ×	0.140 ***	0.078 ***	0.045	0.108
ln(per capita total revenue in 1967)	(0.023)	(0.029)	(0.059)	(0.090)
Year 1967 × Covered Jurisdiction	0.092 ***	0.118 ***	0.148 **	0.183
	(0.028)	(0.042)	(0.062)	(0.119)
Year 1967 × South	0.235 ***	0.132 ***	0.138	0.131
	(0.029)	(0.037)	(0.090)	(0.110)
Year 1967 × Northeast	0.347 ***	0.122 ***	0.168 **	-0.055
	(0.027)	(0.038)	(0.085)	(0.123)
Year 1967 × West	0.237 ***	0.119 ***	0.186 **	-0.066
	(0.026)	(0.035)	(0.079)	(0.138)
Year 1967 × Midwest	0.264 ***	0.132 ***	0.182 **	-0.096
	(0.029)	(0.036)	(0.091)	(0.118)
% Nonwhite Population	-1.597 ***	-1.540	-5.655 ***	-1.027
	(0.497)	(0.949)	(2.035)	(2.822)
Median Household Income ($000s)	0.006	-0.007	0.009	0.011
	(0.005)	(0.007)	(0.016)	(0.021)
% Unemployment Rates	-0.065	-0.109	3.823 **	0.931
	(0.513)	(0.818)	(1.623)	(2.684)
% Urban Population	-0.028	-0.103	-0.330	0.123
	(0.141)	(0.225)	(0.340)	(0.824)
% Elderly Population	-0.837	0.508	-3.848	6.425
	(0.852)	(1.503)	(2.459)	(3.926)
Land Area (sq.mi)	0.825	-0.054	0.292	-5.110
	(0.631)	(0.973)	(1.957)	(5.823)
% Population Growth [10 year]	0.066	0.232 **	0.647 ***	-0.285
	(0.060)	(0.096)	(0.183)	(0.35)
Population (000,000s)	-0.186	0.151	0.319	-1.077
	(0.136)	(0.411)	(0.388)	(1.124)
Constant	-1.539 **	-2.408 ***	-0.940	-0.420
	(0.618)	(0.908)	(1.926)	(5.428)
Within R-square	0.585	0.081	0.077	0.009
County Fixed Effects	Yes	Yes	Yes	Yes
Fiscal Years Included	1962, 67	1962, 67	1962, 67	1962, 67
Number of Counties	2784	2784	2784	2784
Number of Observations	5568	5568	5568	5568

Note: County-clustered standard errors are in parentheses. Quantities of interest are indicated in **boldface**. * p<.10, ** p<.05, *** p<.01 in two-tailed test.

Table 1.A.10: Re-estimation of SUD with region-year fixed effects.

Dependent Variable	ln(per capita total debts outstanding)			
Exlanatory Variables	(1)	(2)	(3)	(4)
Year 1962 × Covered Jurisdiction × ln(per capita total revenue in 1962)	**-0.024** **(0.174)**		**0.139** **(0.195)**	
Year 1962 × Covered Jurisdiction	0.067 -0.097		0.265 ** (0.113)	
Year 1962 × Former Confederate States × ln(per capita total revenue in 1962)		**-0.157** **(0.115)**	**-0.122** **(0.122)**	
Year 1962 × Former Confederate States		-0.164 *** -0.057	-0.235 *** -0.066	
Year 1962 × % Nonwhite Population × ln(per capita total revenue in 1962)				**0.197** **(0.375)**
Year 1962 × % Nonwhite Population				0.052 (0.206)
Year 1962 × ln(per capita total revenue in 1962)	0.059 -0.054	0.016 -0.069	0.008 (0.070)	0.005 -0.056
% Nonwhite Population × ln(per capita total revenue in 1962)				-7.110 (5.857)
Year 1962 (Baseline Year = 1957)	-0.045 (0.090)	0.008 (0.092)	0.004 (0.092)	-0.031 (0.091)
% Nonwhite Population	3.524 (2.202)	1.237 (2.187)	2.205 (2.224)	-1.364 (2.609)
Median Household Income ($000s)	0.0286 (0.0191)	0.0210 (0.0194)	0.0194 (0.0194)	0.0282 (0.0195)
% Unemployment Rates	7.381 *** (1.975)	6.934 *** (1.997)	6.24 *** (2.042)	7.433 *** (1.978)
% Urban Population	-0.379 (0.541)	-0.107 (0.556)	-0.0913 (0.551)	-0.330 (0.546)
% Elderly Population	0.0683 (2.365)	0.188 (2.415)	1.440 (2.470)	-0.530 (2.363)
Land Area (sq.mi)	-11.63 (10.07)	-10.60 (9.926)	-12.85 (9.891)	-10.72 (9.977)
% Population Growth [10 year]	1.13 *** (0.298)	1.046 *** (0.298)	1.077 *** (0.299)	1.04 *** (0.297)
Population (000,000s)	-0.513 ** (0.255)	-0.398 * (0.231)	-0.313 (0.212)	-0.403 (0.266)
Constant	7.730 (9.312)	7.176 (9.187)	9.045 (9.146)	7.157 (9.222)
Within R-square	0.030	0.032	0.036	0.030
County Fixed Effects	Yes	Yes	Yes	Yes
Fiscal Years Included	1957, 62	1957, 62	1957, 62	1957, 62
Number of Counties	2784	2784	2784	2784
Number of Observations	5568	5568	5568	5568

Note: County-clustered standard errors are in parentheses. Quantities of interest are indicated in **boldface**. * p<.10, ** p<.05, *** p<.01 in two-tailed test.

Table 1.A.11: Robustness check for the trend effects.

Dependent Variable	ln(per capita education expenditure)			
Exlanatory Variables	(1)	(2)	(3)	(4)
Year 1972 × Covered Jurisdiction	-0.0004		0.0216	
	(0.0209)		(0.0224)	
Year 1972 × Former Confederate States		-0.0242	-0.0336 **	
		(0.0157)	(0.0165)	
Year 1972 × % Nonwhite Population				0.00207
				(0.0724)
Year 1972 (Baseline Year = 1967)	0.0328	0.036	0.0343	0.0323
	(0.0602)	(0.0604)	(0.0601)	(0.0520)
% Nonwhite Population	0.191	-0.2	-0.0034	0.214
	(0.564)	(0.521)	(0.574)	(0.803)
Median Household Income ($000s)	0.02 ***	0.02 ***	0.0201 ***	0.02 ***
	(0.00612)	(0.00613)	(0.00612)	(0.00548)
% Unemployment Rates	-0.0684	-0.135	-0.122	-0.0678
	(0.740)	(0.741)	(0.743)	(0.731)
% Urban Population	-0.0957	-0.06	-0.0601	-0.0969
	(0.194)	(0.194)	(0.193)	(0.195)
% Elderly Population	0.293	0.546	0.587	0.291
	(0.870)	(0.914)	(0.908)	(0.874)
Land Area (sq.mi)	-0.335	-0.355	-0.397	-0.335
	(0.821)	(0.831)	(0.825)	(0.822)
% Population Growth [10 year]	-0.301 ***	-0.272 **	-0.255 **	-0.3 ***
	(0.116)	(0.117)	(0.122)	(0.114)
Population (000,000s)	-0.084	-0.0132	0.00324	-0.0861
	(0.192)	(0.199)	(0.201)	(0.199)
Constant	-0.464	-0.478	-0.485	-0.467
	(0.819)	(0.826)	(0.823)	(0.805)
Within R-square	0.25	0.25	0.25	0.25
County Fixed Effects	Yes	Yes	Yes	Yes
Fiscal Years Included	1967, 72	1967, 72	1967, 72	1967, 72
Number of Counties	2784	2784	2784	2784
Number of Observations	5568	5568	5568	5568

Note : County-clustered standard errors are in parentheses.
* $p<.10$, ** $p<.05$, *** $p<.01$ in two-tailed test.

Table 1.A.12: The effect of the actual change in the median voter on per capita education expenditures.

Dependent Variable	ln(per capita highway expenditure)			
Exlanatory Variables	(1)	(2)	(3)	(4)
Year 1972 × Covered Jurisdiction	-0.0636 *		0.0188	
	(0.0354)		(0.0362)	
Year 1972 × Former Confederate States		-0.117 ***	-0.126 ***	
		(0.0249)	(0.0238)	
Year 1972 × % Nonwhite Population				-0.13
				(0.0899)
Year 1972 (Baseline Year = 1967)	0.0424	0.0496	0.0481	0.0594
	(0.0541)	(0.0530)	(0.0536)	(0.0596)
% Nonwhite Population	1.876 **	0.976	1.147	1.97 *
	(0.943)	(0.886)	(0.972)	(1.103)
Median Household Income ($000s)	0.00343	0.00379	0.00385	0.00169
	(0.00599)	(0.00591)	(0.00593)	(0.00640)
% Unemployment Rates	-0.496	-0.708	-0.697	-0.367
	(1.202)	(1.193)	(1.194)	(1.200)
% Urban Population	0.115	0.248	0.248	0.133
	(0.340)	(0.342)	(0.342)	(0.338)
% Elderly Population	0.472	1.535 *	1.57 *	0.353
	(0.859)	(0.914)	(0.910)	(0.848)
Land Area (sq.mi)	2.067	1.872	1.836	1.934
	(1.679)	(1.658)	(1.670)	(1.674)
% Population Growth [10 year]	0.0917	0.25 *	0.265 *	0.0682
	(0.142)	(0.145)	(0.146)	(0.148)
Population (000,000s)	-0.547	-0.234	-0.22	-0.463
	(0.343)	(0.357)	(0.353)	(0.354)
Constant	-4.721 ***	-4.794 ***	-4.801 ***	-4.542 ***
	(1.647)	(1.636)	(1.634)	(1.670)
Within R-square	0.026	0.035	0.035	0.025
County Fixed Effects	Yes	Yes	Yes	Yes
Fiscal Years Included	1967, 72	1967, 72	1967, 72	1967, 72
Number of Counties	2784	2784	2784	2784
Number of Observations	5568	5568	5568	5568

Note : County-clustered standard errors are in parentheses.
* $p<.10$, ** $p<.05$, *** $p<.01$ in two-tailed test.

Table 1.A.12: *continued*

Dependent Variable	ln(per capita long-term debts outstanding)			
Exlanatory Variables	(1)	(2)	(3)	(4)
Year 1972 × Covered Jurisdiction	-0.0323		0.035	
	(0.0495)		(0.0546)	
Year 1972 × Former Confederate States		-0.0874 **	-0.103 ***	
		(0.0349)	(0.0373)	
Year 1972 × % Nonwhite Population				0.0971
				(0.139)
Year 1972 (Baseline Year = 1967)	0.116	0.124	0.121	0.0923
	(0.0933)	(0.0928)	(0.0933)	(0.0923)
% Nonwhite Population	-0.332	-1.248	-0.928	0.902
	(1.755)	(1.715)	(1.788)	(1.732)
Median Household Income ($000s)	0.00162	0.00186	0.00196	0.00309
	(0.00997)	(0.00992)	(0.00994)	(0.00986)
% Unemployment Rates	1.61	1.425	1.446	1.66
	(2.199)	(2.202)	(2.203)	(2.196)
% Urban Population	0.727	0.837 *	0.836 *	0.662
	(0.486)	(0.484)	(0.484)	(0.479)
% Elderly Population	-0.236	0.596	0.662	-0.363
	(1.314)	(1.358)	(1.370)	(1.328)
Land Area (sq.mi)	0.0286	-0.0936	-0.161	0.00462
	(2.285)	(2.280)	(2.278)	(2.296)
% Population Growth [10 year]	1.237 ***	1.352 ***	1.379 ***	1.275 ***
	(0.257)	(0.258)	(0.264)	(0.257)
Population (000,000s)	-0.1	0.14	0.167	-0.205
	(0.402)	(0.437)	(0.441)	(0.409)
Constant	-2.117	-2.169	-2.182	-2.259
	(2.224)	(2.220)	(2.221)	(2.233)
Within R-square	0.047	0.049	0.049	0.047
County Fixed Effects	Yes	Yes	Yes	Yes
Fiscal Years Included	1967, 72	1967, 72	1967, 72	1967, 72
Number of Counties	2784	2784	2784	2784
Number of Observations	5568	5568	5568	5568

Note : County-clustered standard errors are in parentheses.
* p <.10, ** p <.05, *** p <.01 in two-tailed test.

Table 1.A.12: *continued*

Dependent Variable	ln(per capita welfare expenditure)			
Exlanatory Variables	(1)	(2)	(3)	(4)
Year 1972 × Covered Jurisdiction	1.082 ***		0.969 ***	
	(0.0994)		(0.125)	
Year 1972 × Former Confederate States		0.595 ***	0.172 *	
		(0.0789)	(0.0982)	
Year 1972 × % Nonwhite Population				2.025 ***
				(0.230)
Year 1972 (Baseline Year = 1967)	-0.494 ***	-0.423 **	-0.502 ***	-0.747 ***
	(0.191)	(0.193)	(0.191)	(0.197)
% Nonwhite Population	-0.512	-8.369 **	0.488	-3.438
	(3.889)	(3.877)	(3.911)	(3.759)
Median Household Income ($000s)	0.0424 **	0.039 *	0.0418 **	0.0694 ***
	(0.0206)	(0.0207)	(0.0205)	(0.0211)
% Unemployment Rates	8.932 **	8.61 *	9.207 **	6.742
	(4.297)	(4.391)	(4.315)	(4.365)
% Urban Population	0.846	0.664	0.663	0.613
	(1.294)	(1.331)	(1.307)	(1.332)
% Elderly Population	1.324	-2.005	-0.183	3.423
	(2.748)	(2.869)	(2.855)	(2.798)
Land Area (sq.mi)	-12.46 **	-10.29 **	-12.14 **	-10.24 **
	(5.071)	(5.110)	(5.083)	(5.183)
% Population Growth [10 year]	-0.419	-1.413 **	-0.656	-0.0742
	(0.626)	(0.642)	(0.626)	(0.630)
Population (000,000s)	3.345 **	2.16	2.897 **	2.08
	(1.422)	(1.346)	(1.390)	(1.297)
Constant	5.315	5.766	5.424	2.524
	(4.733)	(4.785)	(4.749)	(4.845)
Within R-square	0.075	0.048	0.077	0.052
County Fixed Effects	Yes	Yes	Yes	Yes
Fiscal Years Included	1967, 72	1967, 72	1967, 72	1967, 72
Number of Counties	2784	2784	2784	2784
Number of Observations	5568	5568	5568	5568

Note : County-clustered standard errors are in parentheses.
* $p<.10$, ** $p<.05$, *** $p<.01$ in two-tailed test.

Table 1.A.12: *continued*

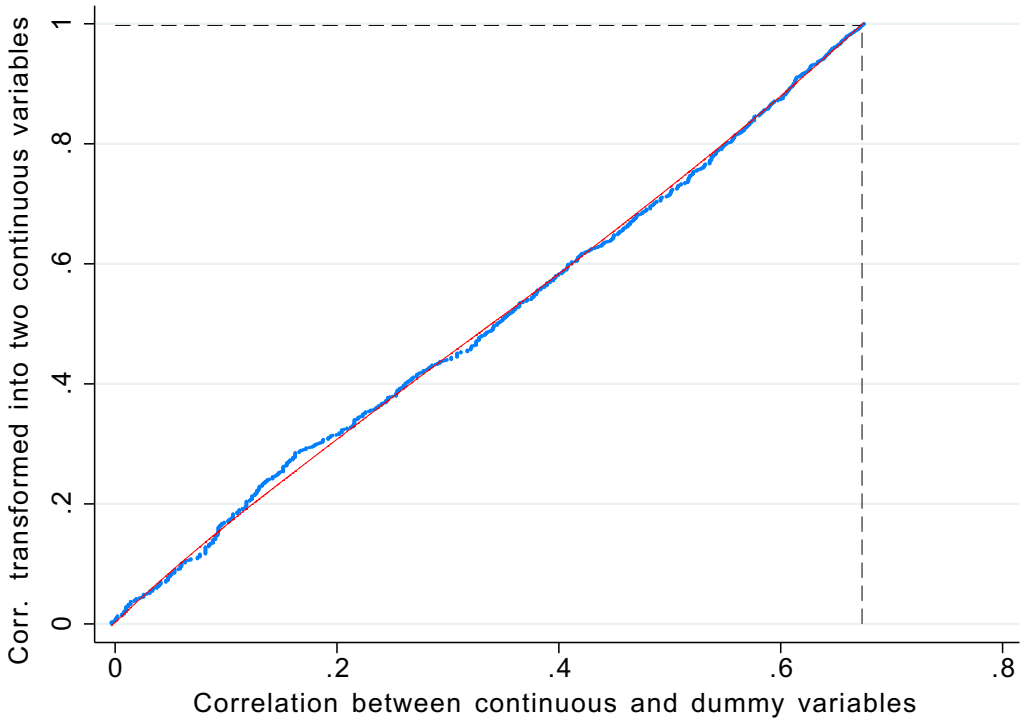

Figure 1.A.1: Conversion table of the correlation coefficients from between one continuous and one dummy variables to between two continuous variables when 501 out of 2784 observations are treated.

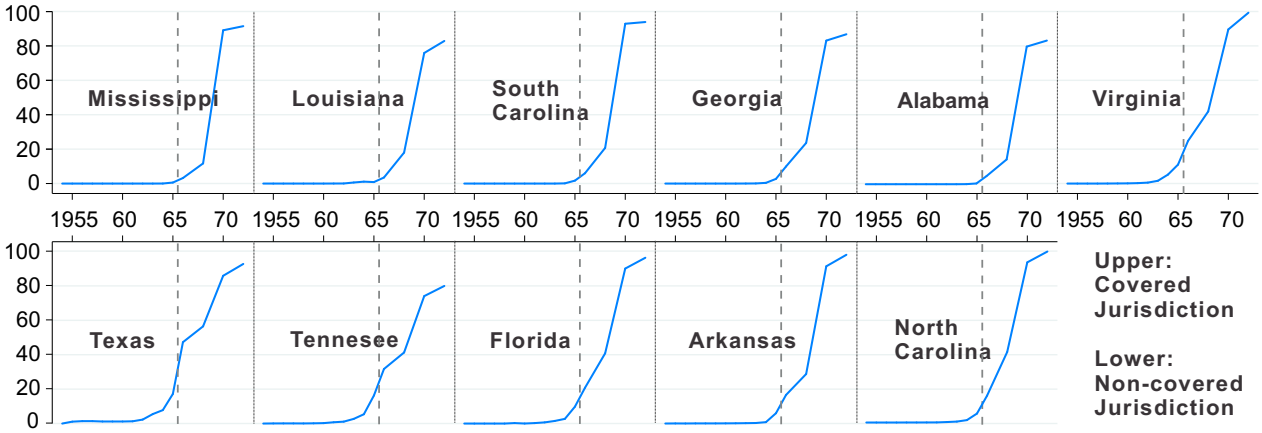

Figure 1.A.2: Proportion of black children in elementary and secondary school with whites (%). *Note*: 40 out of 100 counties in North Carolina were covered by the VRA. *Source*: Rosenberg (2008) pp.433-434.

CHAPTER 2

LEGACY OF WAR ON DEMOCRATIC PARTICIPATION: THE LONG-LASTING EFFECTS OF THE AIR RAIDS IN JAPAN DURING WWII

ABSTRACT

War has been considered as one of the means to achieve democratic transition, but this may not be without a cost. This study demonstrates the effects of war on democratic participation using the air raids in Japan during World War II. Prefectures where more houses were destroyed and more people were killed by air raids record lower voter turnout decades later than those which escaped from air raids, controlling for baseline prewar voter turnout and an extensive set of covariates. These findings also pass various robustness checks such as trend effects. Additionally, the effects are stronger when the elections draw less public attention. Although these results can be interpreted in many ways, one probable explanation is the destruction of neighborhood social capital. Confirmatory analysis suggests that among several types of voluntary associations, only the membership rate of neighborhood associations is negatively correlated with the damage due to the air raids.

2.1 Introduction

Effective or not, the use of force has been considered as one of the policy options to achieve democratic transition (Pape 1996), but defeated belligerent countries often face a number of difficulties in postwar governance. This raises a concern about the negative legacy of war. That is, the use of force sometimes forces the target country to adopt a democratic regime, but the mass-destruction that comes with war might overshadow long-term political participation in that country. This study examines this claim using historical data from Japanese prefectures. Specifically, this study uses the air raids in Japan during World War II (WWII) as a source of exogenous variations, and compares the changes in voter turnout at the national elections from prewar to postwar periods between the prefectures that incurred various degrees of damage.

It must be emphasized in the beginning that the scope of this study has a natural limitation: voter turnout may capture one aspect of the multifaceted political participation, and air raids can capture only some aspects of the devastating consequences of war. Also, no observational study can eliminate the possibility that WWII generated an unobserved impact similar to what the air raids have, and this unobserved impact might cause the change in the voter turnouts. In spite of these limitations, the findings from this study contributes to the literature of political science in a number of ways.

First, this study provides rare findings on the long-term effect of war on democratic participation. The study of the effect of war on democratic participation has significant implication to the U.S. foreign policy for the next few decades. As we see in the case of Iraq or Afghanistan, the success of military intervention is judged by the success in the postwar governance not by the success in bringing the democratic regime. Also, the study of the impact of the air raids is getting more important because of the general strategic shift from land power to air power in military operation. In spite of the potential policy relevance, a surprisingly small number of studies have been done on this topic.

Comparison with the related studies highlights this point. A number of studies have been done on the deterrent effect of democracy on war known as “democratic peace" (e.g., Maoz and Russett 1993; Bueno de Mesquita et al. 1999), the effect of war on regime change and leadership survival (Bueno de Mesquita and Silverson 1995) and the effects of war on economic outcomes such as the “Phoenix Factor" (Organski and Kugler 1984), and the“Theory of Distributional Coalitions" (Olson 1982), or the recent attempts to estimate the causal effect of the war (Davis and Weinstein 2002, Miguel and Roland 2006, and Acemoglu, Hassan, and Robinson 2010). On the other hand, only a few studies examine the effects of war on democratic participation (Bellows and Miguel 2009; Blattman 2009). Furthermore, these two studies concern mainly the short-term effects of the victimization of civil war violence on individual political participation, which are qualitatively different from this study that examines the long-term effects of the air raids on mass political behavior.

On a related point, the data that contain sufficient information for the causal inference of the effect of war on democratic participation rarely exist. Causal inference in observational studies requires the data to contain certain information such as the dependent variable of the pre-intervention period, a good instrumental variable, an extensive set of covariates to satisfy conditional independence assumption, and/or an exogenous threshold of treatment assignment for regression discontinuity, none of which occurs frequently.

The primary reason is war itself. Warfare often partly or entirely destroys the data. Also, belligerent entities sometimes do not collect statistics for financial or national security reasons. Even when the data exist, if a war caused regime change, the variables before and after a war may not be recorded in a comparable manner. For example, voter turnout in an autocracy may represent the degree of political repression whereas the counterparts in democracy represent political participation. Also, the simultaneity of war and regime change makes it hard to estimate the effect of war separately from the effect of regime

change.[1] Because Japan rapidly modernized its system of governance during the Meiji era (1868-1912), the detailed historical statistics including voter turnout are available at the prefecture level dating back to 1890.[2] Also, the fact that Japan experience the democratic periods both pre- and post WWII enables us to separate the effect of war from that of regime change. Thus, these historical statistics, combined with the damage due to the air raids during WWII, provide an ideal and unique opportunity to examine the causal effect of war on democratic participation.

The second contribution of this study is that its findings shed light on the effects of natural disasters on political participation. War damage is more broadly classified as a disaster, and this characterization is particularly suitable for the air raids in Japan. One of the primary differences between war and natural disaster is whether an event was man-made or not, and the assailants often foment hatred toward the occupation forces that may bring a new regime. However, Japan after WWII experienced no major opposition toward occupation forces, which helps distinguish the physical effects of war from the emotional effects. From the empirical viewpoint, the air raids in Japan have many advantages over other natural disasters in measuring causal effects. The air raids hit 147 cities (Asahi Shimbun Company 2004), and most of the strikes were made during the six months prior to the end of WWII, whereas a natural disaster such as an earthquake or a hurricane usually affects a small area, which limits the number of observations. The pooling of historical disasters also raises econometric complications. Furthermore, panel data is hardly available in disaster study because it is unlikely that an opinion survey with a sufficient number of respondents is conducted at disaster sites before and after a disaster happens.

Finally, this study provides rare causal evidence of the positive effect of social capital on political participation. The causal estimation of time-invariant factors such as institutions

1. A similar identification problem exists in the relationship between economic growth and regime change (Przeworski and Limongi 1993).

2. Some statistics are available from 1920.

or social capital is emerging as one of the most important research agendas in social science (Acemoglu, Johnson, and Robinson 2001, Guiso et al. 2008). These factors often occupy an important place in explaining political participation, but their invariabilities make it extremely difficult to distinguish their causal effects from contemporaneous correlations. As mentioned later, this study finds several pieces of evidence which imply the air raids destroyed neighborhood social capital, which is a considerable progress from past studies that cannot eliminate observational equivalences due to contemporaneous correlations.

Using the data of 20th-century Japan, the external validity of this study is certainly limited, but the limitation is probably less than what one thinks. Some may think that no other modern war and conflict can be compared with the air raids during WWII in terms of the extent of damage. However, the air raids simply provide the best empirical setting. At a local level, many areas have suffered considerable loss of lives and property through conflicts, wars, and natural disasters. Also, advances in military technology do not necessarily diminish the external validity of this study, either. No modern weapon guarantees the precise bombing of the hostile force unless land assets provide precise information, which is hard to obtain when the offensive side cannot rely on its land power. In short, the use of force cannot eliminate unintended destruction, and as long as this is the case the findings from mid-20th century Japan retain substantial importance.

To overview the results, the air raids have had long-lasting negative impacts on postwar voter turnout. That is, the prefectures that experienced severe air raids marked a larger decrease in postwar voter turnout than those not bombed severely. Also, the effects are statistically significantly negative only at the Upper House elections, and they are particularly strong when an election records low voter turnout nationally. The findings pass several robustness checks. Also, the air raids account for $10 \sim 40\%$ of the decrease in the postwar voter turnout at the Upper House elections. The confirmatory analysis suggests that the air raids have changed the level of social capital, which lowers the likelihood of voting out of

civic duty.

2.2 Background

2.2.1 Transitions of the Political Regimes

In this section, I briefly document the transitions of the political regimes in pre- and postwar Japan. This should shed light on the comparability between the pre- and postwar political regimes. Although I have surveyed a variety of literature, both in Japanese and in English, the featured quotations are mostly lifted from *A Modern History of Japan: From Tokugawa Times to the Present* (Gordon 2003), which is a standard Japanese history textbook. Relying on a single established source organizes my interpretation of this history and thus reduces its arbitrary quality.

The Empire of Japan adopted a bicameral system, which consisted of the House of Peers and the House of Representatives (henceforth the Lower House). Because the members of the House of Peers were not publicly elected, the following discussion focuses only on the Lower House until the end of the war.

The Japanese public electoral system began in 1890 with many restrictions. Eligibility to vote was limited to men aged 25 and over who paid national taxes of more than 15 Yen (about 1,150 USD in the year 2000)[3] per year. As such, only 1.1 percent of the population was eligible to vote. Also, the voting was open ballot. Following elections gradually relaxed these restrictions in response to the surge of the universal suffrage movement. Specifically, a secret ballot has been introduced ever since the second general election. The tax threshold was lowered to 10 Yen in 1900, and to 3 Yen in 1919. Accordingly, eligible voters accounted for 5.5 percent of total population in 1924.

3. This calculation is based on the fact that consumer prices increased 8,300 times from 1873 to 2000. Therefore, one U.S. Dollar was calculated as 108 Yen at the rate of 2000.

Democracy in prewar Japan reached its height during the several years after the introduction of universal suffrage. In 1925, the Imperial Diet finally amended the general election law to extend suffrage to all males aged 25 and over. Yet, it was only after WWII that the government granted female suffrage. As a result, the fraction of eligible voters dramatically increased from 5.5 percent to 20 percent of the population.[4] But in the same year, the Diet enacted the Peace Preservation Law, which prohibited ideas that endangered Imperial polity or that fomented communism. Although the Diet enacted this law to counterbalance an overly rapid democratic transition due to universal suffrage, the special political police repeatedly abused their power to suppress free speech during WWII. During 1925 to 1932, three elections were held, and one of the regular procedures of the constitutional government, namely the appointment of the head of the leading party to a Prime Minister, was established.[5]

The May 15 Incident in 1932 is considered the beginning of the militaristic period in Japan. This incident's failed coup d'état, which was led by radical military officers, ended the democratic convention, and Japan gradually shifted to the war regime. Furthermore, the February 26 Incident in 1936, another failed coup attempt by *Kōdō-ha*, the radical military faction, killed some of the central political figures. Terrified politicians gave further power to *Tōsei-ha*, the other military faction who "rejected terrorist violence," to purge the *Kōdō-ha*. However, *Tōsei-ha* "were hardly moderate in other ways" (198).[6] The Second Sino-Japanese War began in 1937. In 1938, the Diet ratified the National General Mobilization Law, which allowed the government to "issue any orders necessary — without Diet approval — 'to control material and human resources'" (212). Two elections were held after the May 15 Incident, one in 1936 and another in 1937. Elections during this period were somewhat restricted.

4. Apparently, the low percentage is due to the rapidly growing young population and the lack of female suffrage.

5. This meant, in effect, that the senior statesmen recommended the leader of the leading party as a Prime Minister to the Emperor, who had the imperial prerogative under the constitution.

6. In fact, Hideki Tōjō, who initiated the Pacific War, was one of the leaders of *Tōsei-ha*.

For example, "[a]ny speaker who criticized 'fascism' or even mentioned a 'gap between the military and the people' was sure to be given a warning or halted outright" (196). However, Gordon (2003) also notes that Minseitō and Seiyūkai "were still vigorous enough in the Diet session of 1937-38 to force the government to delay or slightly modify its legislative agenda" (216). In 1940, the government dissolved all political parties. In their place, the government formed the *Imperial Rule Assistance Association* (IRAA), "[a] sort of political cheerleading squad" (216).[7]

In December 1941, Japan initiated the Pacific War with the U.S. The last general election under Imperial Japan was held a few months later when Japan still enjoyed its short-lived success. The special political police often interfered with electoral campaigns of those not endorsed by the IRAA, and the IRAA aimed to get all of its 466 candidates elected in the contest for 466 seats. Nevertheless, 85 non-IRAA candidates were elected. Moreover, about the two thirds of the IRAA's candidates were incumbents who previously belonged to either Minseitō or Seiyūkai. As a result, "[m]any party men, whether those serving in the IRAA or those elected as independents, continued to command the local loyalty of their constituents" (216). A few months later, the Battle of Midway in June 1942 became the turning point of the war situation in which Japan switched from the offensive to the defensive. Moreover, "[t]he capture of Saipan in July 1944 placed the main islands in range of American bombers", and "[t]he war was essentially lost at this point"(212) although Japan did not officially announced surrender until August 1945.

The Supreme Commander for the Allied Powers (SCAP) ruled occupied Japan "indirectly, implementing changes through the existing Japanese bureaucracy" until 1952, when the Allied occupation ended. In 1946, the last Imperial Diet was held to enact the Constitution of Japan, which largely reflected the opinion of the U.S. Two of these constitutional changes

7. To be exact, because IRAA was not a political association, the members of the Diet were encouraged to join its political association, which was named the *Imperial Rule Assistance Political Association.*

are particularly important to this study. First, the Diet granted voting rights to all citizens *including females* aged 20 and over. Second, the House of Peers was replaced with the House of Councilors (henceforth the Upper House), of which members are nationally elected every three years.[8] Under the new constitution, the Upper House has less constitutional power than the Lower House has. For example, the ultimate power to select the Prime Minister and pass the budget has been granted to the Lower House. The first general election after WWII was held in 1946. This election was unique in the sense that voters casted multiple votes and each prefecture contained only one or two multimember districts.

After the first election, the electoral system did not experience many changes. The Lower House introduced a medium-sized constituency system, a variation of the at-large election, which lasted until 1994. After 1994, the combination of the first past the post system and proportional representation was introduced. On the other hand, the Upper House had implemented essentially the same system, which consisted of the combination of a medium-sized constituency system and a nationwide constituency since the beginning. A proportional representation system replaced a nationwide constituency in 1983, but these are similar in that members are elected through nationwide competition. The political regime did not experience much change for most of the latter half of the 20th century, as well. Particularly, after the merger of the Liberal Party and the Democratic Party in 1955, the Liberal Democratic Party held the office until 1993.[9]

8. In each election, half of the seats are up for re-election. Therefore, the term of the member of the Upper House is six years.

9. It is misguided, however, to define the political system of postwar Japan as a quasi-democracy rather than a democracy. First, the new constitution is based on democratic principles. It was written by the SCAP, whose officers were mostly Americans. Second, postwar Japan does not preclude the possibility of the change of government. In fact, the first publicly-elected prime minister after WWII was selected from the Social Democratic Party of Japan. Third, LDP should be regarded as the coalition between several different factions. In fact, the LDP lost its office in 1993 because of the defection of some of the factions from LDP.

2.2.2 Air Raids

Detailing the air raids helps evaluate whether the damage due to the air raids provides sufficient exogenous variations for causal inference or not. To claim the randomness of the treatment assignment is, however, not this section's goal; in fact, it is impossible to make such claim a in observational studies. Rather, the intent of the following paragraphs is to justify the key identifying assumption necessary to estimate causal effect, which is the absence of a treatment-specific time-trend effect. That is, there should be no factor other than the air raids that explains the difference of the changes in voter turnout from prewar to postwar periods between the prefectures that were heavily bombed and those were not.

The strategic bombing of Japan did not cause major damage for the first three months. After the fall of Saipan in July 1944, the Allies developed the ability to carry out massive air raids.[10] Major General Hansell directed the strategic bombing of Japan. To take advantage of the high-altitude cruising capability of the B-29s, he ordered the pilots to fly at high-altitude (more than 30,000 feet) and to target military plants and facilities. Although "Japan's air defenses were helpless against high-flying B-29s" (212) and the damage to the Allies was minimal, the Allied air raids rarely damaged their targets.[11]

The strategy of the Allies dramatically changed after Major General LeMay became in charge of the strategic bombing of Japan in January 1945. He realized that many civilians were supporting war indirectly, so LeMay felt that the weakening of the home front was necessary to defeat Japan. Therefore, he completely changed the bombing strategy. LeMay changed the bombing methods from precision bombing at high-altitudes, to indiscriminate bombing at low-altitudes (about 6,000 feet), which targeted all populated areas in Japan. This strategic change, coupled with the use of firebombs, brought the immediate result of

10. A couple of surprise attacks such as the Doolittle Raid were attempted by medium bombers, but the physical damage due to these attacks were very limited.

11. High-altitude bombing was particularly susceptible to the jet stream, the strong wind blowing in the sky over Japan, but the Allies did not have much information about the jet stream during WWII.

weakening the home front, even though it put the Alllied bombers at higher risk.

To summarize the damage due to the air raids, the air raids targeted 138 cities several hundred times and "left nine million homeless and killed nearly two hundred thousand civilians. The two atomic bombs killed an additional two hundred thousand people immediately. Furthermore, [a]nother one hundred thousand or more bomb victims died in the following months and years because of the lingering effects of radiation sickness." Citing the Unites States Strategic Bombing Survey (1947), Davis and Weinstein (2002) document the damage due to the air raids as follows. "The air raids destroyed almost half of all structures in these cities—a total of 2.2 million buildings. Two-thirds of productive capacity vanished. Three hundred thousand Japanese were killed. Forty percent of the population was rendered homeless. Some cities lost as much as half of their population owing to deaths, missing, and refugees."

The damage due to the air raids was intense enough to affect people's sociopolitical behaviors. Comparing these bombings to another major disaster in recent history should give readers a sense for the immense scale of damage the air raids caused in Japan. With regard to Hurricane Katrina, this storm hit the city of New Orleans on August 29, 2005 and killed about 1,599 people in Louisiana,[12] of which the fatality rate (0.036 percent) would rank 39th on the scale of the air raids out of 46 prefectures. From this damage, the city of New Orleans has recovered only 81 percent of its pre-Katrina population after five years (Speyrer et al. 2010).

The damage due to the air raids has some desirable properties for quantitative analysis. First, because of LeMay's indiscriminate bombing strategy, the air raids left varying degrees of damage all over the nation. At the top of Figure 1, the prefectures are color-coded into five categories, depending on the intensity of the damage, as measured by the ratio of the

12. The death toll includes about 135 missing persons.

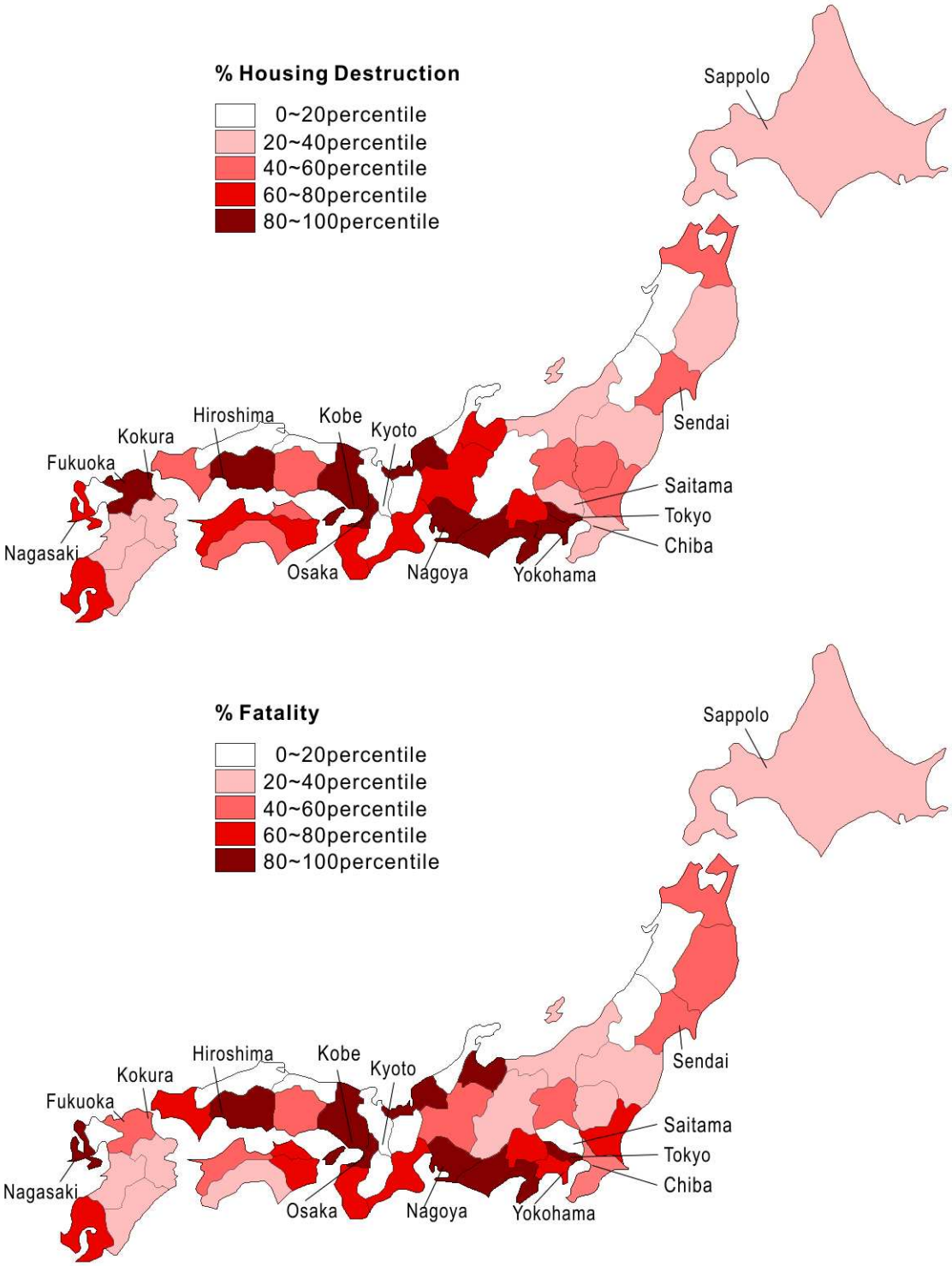

Figure 2.1: The geographical variation of the damage due to the air raids. Top: the damage is measured by % housing destruction, bottom: the damage is residualized by control variables.

housing destruction (top) and by the fatality rates (bottom). Also, the locations of the large cities are indicated with their respective city names.[13] Although the damages concentrate on the prefectures with large cities, this concentration is controlled for by unit fixed effects.

Second, the damage due to the air raids is exogenous to the voter turnout before the war's ended, not to mention after the war. In other words, the issue of endogeneity arises if voter turnout affects the severity of the air raids. For example, one could argue the following: if high voter turnout substantively indicated public support for the imperial regime, and the U.S. targeted the cities where supporters of the regime were more likely to live, the causality goes in the opposite direction, and the effect of the air raids is inappropriately magnified. However, this account is inaccurate. LeMay indiscriminately targeted all populated areas in Japan, and every prefecture, or the unit of analysis,[14] contains at least one populated area. Also, the fear of the air raids was unlikely to be the factor that motivated the voters in the populated areas to go to vote. The government held the last election when the war was still in favor of Japan, so few people recognized the air raids as an imminent danger.

Third, several natural and contingent factors created variations in the damage. Certain weather conditions, such as strong winds or low humidity, on the day of the bombing could amplify the fire storm. The landscape also affected the damage. The atomic bomb dropped in Nagasaki was one and half time as powerful as the one dropped in Hiroshima, but it killed about half as many people as the one dropped in Hiroshima because Nagasaki's pitched and mountainous landscape limited the diffusion of radiation. Also, the city of Sappolo, one of the largest cities in Japan, was hardly bombed because its location was far from Saipan (David and Weinstein 2002).

Finally, the selection process of where to drop the atomic bombs created another type of variation, although this process was a political decision, not a random one. Specifically,

13. Nagasaki is not a big city but listed as one of the two atomic bomb sites.

14. Okinawa is excluded from the analysis.

once a city was designated as a candidate site, it was going to receive a damage in either direction that is different from what it would have received otherwise. Three months before the Allies dropped the two atomic bombs on Hiroshima and Nagasaki, they had selected six candidate sites.[15] The Allies ordered the ordinary bombers not to bomb these candidate sites so that they could later evaluate the damage due to the atomic bomb. Even Kyoto, which has an abundant cultural heritage, suffered little damage, not because of its cultural value but because of its selection as a candidate city for atomic bombing until June 1, 1945.[16] Selection of the dropping zone on the day of the bombing was also subject to the weather. In fact, the primary target for the second atomic bomb was Kokura (currently Kitakyūshū), but it was dropped on the secondary target, Nagasaki, due to the low visibility in the sky over Kokura.

2.2.3 Causal Pathways

The destruction of various capitals due to the air raids suggests that various causal pathways need to be considered in explaining the negative link between the air raids and the postwar voter turnout. Although it is prohibitively difficult to make and test an exhaustive list of potential causal mechanisms, listing major theoretical possibilities facilitates the interpretation of the empirical results.

One potential mechanism is the destruction of human capital. Injuries and resulting disabilities could affect their educational and/or economic achievements, and individual income and education are well known as the strongest predictors for political participation (Wolfinger and Rosenstone 1980). Parental deaths could affect their children's educational

15. The six candidate sites were Hiroshima, Kyoto, Kokura, Niigata, Yokohama, and Nagasaki. Several criteria were used for the selection. To mention a couple of them, the site should not be previously bombed, and the size of the city center should be more than three miles in diameter.

16. After Secretary of War Stimson strongly opposed dropping an atomic bomb on Kyoto on that day, Kyoto was removed from the list, and the order remained effective until the war's end.

and economic prospects, which would also lower voter turnout. An identifying characteristics for this mechanism is that the air raid effects in this context should diminish over time as the number of afflicted people decreases.

The air raids had impacts beyond human capital and fundamentally changed the way in which communities developed thereafter. The fire storm caused severe damage particularly to residential buildings, which were made of wood and paper. The total destruction often blurred the borderlines between neighboring houses, those who lost their homes sometimes left their neighborhoods seeking the help of relatives in other areas, or entire households were killed. Thus, the destruction due to the air raids made it easier to redevelop the area. Figure 2 shows the house maps of the two cities, namely Kanazawa and Yokohama. Kanazawa did not suffer from the air raids at all, while Yokohama was heavily bombed. Kanazawa in 2010 in the top panel shows old streetscape: many houses are built with no space between them and their neighboring houses, some houses do not abut on a street, and residential blocks take various shapes other than rectangular. The same characteristics are visible in Yokohama in 1930 in the middle panel. On the other hand, Yokohama in 1963 in the bottom panel shows a drastically different geographical configuration. Residential blocks are built on a grid, most houses have access to the pavement, and houses are built with some space between them and neighboring houses, which reflects new building codes.

The above discussion proposes the destruction of physical capital as one of the potential causal mechanisms. Destruction of voting places and infrastructures may temporarily increase the cost of voting. Also, the air raids created a number of vast vacant lands, which accelerated the construction of housing complex. If these buildings attracted new residents such as the poor, and their propensity to vote were lower than that of the original residents, the construction would decrease postwar voter turnout.[17] An identifying characteristic for

17. It must be noted, however, that public housing in Japan has been offered to both low and middle income families. Thus, the effect of the construction of housing complex is not necessarily negative.

Figure 2.2: The long-lasting impact of the air raid on community development. Top: Kanazawa in 2010 (No damage), middle: Yokohama in 1930 (Heavily bombed), bottom: Yokohama in 1963. *Source*: Mapion (URL: `http://www.mapion.co.jp/`), and City of Yokohama (URL: `http://www.city.yokohama.jp/me/machi/kikaku/cityplan/gis/3000map.html`).

this mechanism is its association with socioeconomic trend. If postwar urbanization is the underlying mechanism of the decrease in postwar voter turnout, the effects of the air raids on postwar voter turnout should decrease significantly by controlling for socioeconomic trends.

Finally, the possibility that the air raids destroyed social network must be considered. The role of social network on voter turnout differs by whether one views election as an instrument of policy goals or as a civic duty. According to the former view, traditional societies often develop the organizations for rent-seeking such as political machines or distributional coalitions (Olson 1982), which mobilize constituents. The destruction of such network would undermine their mobilizing power and encourage postwar economic development through efficient resource allocations, both of which negatively affect voter turnout. From the latter perspective, social network functions as social capital that promote active political participation (Putnam, Leonardi and Nanetti 1993). Hence, the destruction of social network would lower the voter turnout out of the sense of civic duty. Important distinction between these two mechanisms are their associations with electoral importance. Voting out of civic duty is the most prominent in unimportant elections, while it is inefficient to mobilize constituents in such elections.

2.3 Research Design

2.3.1 Data

The dataset for this study is created by merging two datasets. The first dataset is Historical Statistics in Japan (Statistics Bureau of Japan 2010). Specifically, this study uses prefectural level panel data. All variables, such as socioeconomic variables and election statistics, are collected from this dataset except the variables for the air raids.[18]

The second type of dataset are the records of the air raids. The most reliable data

18. This dataset is available online at `http://www.stat.go.jp/data/chouki/index.htm`.

are found in two magazines, *Weekly Asahi Encyclopedia of Japanese History*(Shukan Asahi Hyakka Nihon no Rekishi) (Asahi Shimbun Company 2004) and *Weekly Yomiuri*(Shukan Yomiuri) (Yomiuri Shimbun Company 1968). The publishers of these magazines are Japan's two largest news paper companies. For simplicity, I will call the former *the Asahi data* and call the latter *the Yomiuri data*.[19] These air raid data were created by updating the most reliable existent data at that time using their local network. The Asahi data is based on the *Nationwide Historical Study on War Damage* (Zenkoku Sensai Shijitsu Chosa Houkokusho) (Office of Cabinet Secretary 1979)[20]. The Yomiuri data is based on the *Complete Report on the damage by Pacific War in Japan* (Economic Stabilization Agency 1947). As for the estimations of the damage due to the air raids, more recent data tend to be more accurate because they have richer information on the whereabouts of those who went missing during the war. Therefore, I rely on the Asahi data as a primary data source because it was created more recently. However, given the difficulty in estimating the accurate extent of the damage, I also use the Yomiuri data as a robustness check.[21]

This study defines the prewar period as 1928, when the first general national election was held under universal, to 1942, when the last election before the war's end was held. The inclusion of three election years after the May 15 incident in 1932 (1936, 1937, and 1942) may be controversial, because the activities of the political parties were restricted after the incident, and all parties were dissolved in 1940. However, this definition of the prewar period is fairly reasonable. As discussed in the previous section, the compositions of the incumbents and the way they ran electoral campaigns were largely unchanged even in this period. Moreover, to my knowledge, no restriction was placed on the voters. Thus,

19. This study collected the Yomiuri data from the book *The Air Raid In Japan*(Nihon no Kushu) (Matsuura et.al, 1981).

20. The details of the survey methodology (in Japanese) are available at the following URL: `http://www.soumu.go.jp/main_sosiki/daijinkanbou/sensai/virtual/report/pdf/s52_53/s52_53_01.pdf`

21. The data of the air raids that are downloadable from Wikipedia contain many errors and should not be used (as of Sep., 2010).

including the prewar period from 1932 to 1942 should not cause the loss of comparability between the pre- and postwar periods. Also, the exclusion of the observations in these three years reduces the number of prewar observations to half, which decreases the reliability of the estimates. Importantly, the models are estimated using three alternative definitions of pre- and postwar periods in the robustness check, and the results are consistent with the main findings.

I define the beginning of the postwar period as the year 1952, when SCAP's occupation ended and Japan regained sovereignty. The first reason for this definition is that SCAP purged many powerful incumbents in the Imperial Diet from public service during its occupation, which affected voting behavior, especially those who determined their voting behavior based on personal voting. Second, most of the covariates are not available immediately after the war. Thus, the ending of the occupation in 1952 provides a reasonable breakpoint to start the postwar period. Importantly, the results are still robust when the same models are estimated with the postwar period starting from 1947. The ending year of the postwar period is 2001, which is determined based on the availability of the latest census, because the most recent population census was conducted in 2010, of which the results are not yet available (as of Jan. 2011). Therefore, the main analysis includes the elections up to 2001.

As a unit of analysis, this study uses 46 prefectures, excluding Okinawa. The prefectures are institutionally equivalent to states in the U.S., but their authorities are limited and areas cover less than five percent of the area of an average American state. The air raids targeted cities not prefectures. Okinawa is excluded from the analysis because of the lack of this city's postwar statistics on account of it being under the occupation of the U.S. until May 15, 1972. Also, unlike the other prefectures, its civilian casualties came about mostly due to ground battles and group suicides. Some may wonder whether the statistics at the city level would better explain the relation between the air raids and voter turnout. However, voter turnout for prewar elections is unfortunately not available at the city level.

Moreover, there are a couple of reasons why it is more appropriate to use the prefecture as the unit of observation. First, each prefecture guarantees a certain level of comparability, because all prefectures contained at least one potential target city for the air raids during WWII. On the other hand, the issue of sample selection emerges as a difficult problem with city data. This is because some cities were probably too small to be potential targets for the air raids. Second, many cities have experienced incorporations and cessions with neighboring municipalities after WWII. Therefore, it is very difficult to treat a given city as a stable unit across the time-series. In contrast, the borders between prefectures have been virtually unchanged since 1876.[22] Third, the central cities of each prefecture are often located far from the prefectural border,[23] which reduces the problem of spatial correlations.

2.3.2 Variables

This study uses prefectural-level voter turnout at the national elections as a dependent variable. During the prewar period, voter turnout data are only available from the Lower House elections because the members of the House of Peers were not publicly elected. After WWII, the House of Peers was replaced with the House of Councilors (the Upper House), of which members are publicly elected. Therefore, two kinds of voter turnout data are available for the postwar period. This study uses both in its empirical analyses because the comparison between the results from different postwar election data may reveal useful patterns.

In spite of a broad consensus that election is the most important form of political participation in democracy, several issues that arise due to the use of voter turnout as a dependent variable must be addressed. First, as mentioned in the beginning, voter turnout may capture

22. The only noticeable changes are the retrocession of the Ogasawara Islands to Tokyo in 1968 and the gradual coastal reclamations for agricultural or urban development.

23. This is mainly due to historical reasons. Most of the central cities before WWII were castle towns from the Edo Period. In the beginning of the Edo Period, the main city and the castle had to be centrally located for defense purposes. Because the Shogunate Government of Edo prohibited local lords from building any new castles, the location of the cities did not change during the Edo Period.

only one aspect of multifaceted political participation. In particular, content voters may not feel any need to express their voices (Hirschman 1970). The discussion in Appendix B shows that this concern is not empirically applied to both pre- and postwar Japan. Second, voter eligibilities between pre- and postwar periods are different. The franchise was restricted to males during the prewar period and was extended to females after WWII, but the main postwar voter turnout data do not distinguish genders. Therefore, in Appendix C, I clarify the assumptions necessary to make a valid inference from the original data, and I conduct a robustness check with an additional dataset that contains gender specific voter turnout, which shows that the difference in the eligibility does not change the findings.

The first measurement for the damage due to the air raids is *the logged ratio of the housing destruction*. One of the advantages of this measure is its high relevancy to the number of homeless. Although the air raids caused a wide array of damage, deprivation of housing affected the largest population; as many as 40 percent of the entire population found itself homeless (Gordon 2003). This variable is defined as:

$$\ln(\%Damage_i) = \ln \frac{\texttt{Number of Destroyed Houses}}{\texttt{Number of Households in 1944}} \tag{2.1}$$

where the subscript i represents the prefecture.[24] The measurements of the numerator and denominator do not necessarily correspond: some families might own more than one house, and multiple families might live in one house, but these situations presumably do not occur in a way that causes bias. The number of households in 1944 is an interpolated value from the statistics compiled in 1930 and 1950.[25] The air raid variables are transformed into natural logarithms, which reflects a reasonable assumption about the relationship between the air

24. Before log-transformation, the observations with zero-values are replaced with the minimum positive values in the observations. The number of units that takes zero values in the sample is nine out of 46 for the ratio of the housing destruction and six for the fatality rates.

25. The use of postwar statistics in the denominator may raise the issue of endogeneity. Therefore, I conducted a robustness check by changing the denominator to *Number of Households in 1930*, and the result is almost the same.

raids and voter turnout. With negative coefficients, the log-transformed variables indicate that the size of the marginal negative effect on voter turnout is decreasing in the damage due to the air raids, which means that some proportion of people voted in postwar elections, no matter how severely their area was bombed. It is not difficult to imagine such people — voters who have strong partisanship or an extreme issue position, core members of interest groups, candidates, and their friends and relatives.

Acknowledging that the ratio of the housing destruction may explain only some aspects of the damage, this study uses the fatality rate as an alternative measurement of the damage. It is probably not appropriate to use this variable as a main treatment variable because the number of deaths cannot directly impact voter turnout in the sense that the people killed are included neither in the voting population nor in the number of voters in postwar elections. Nevertheless, the fatality rate is a good proxy for the severity of damage caused by the air raids primarily due to the homogeneity of the measurement unit (a human body) compared to buildings and traceability of the death toll later on. The information about someone's death is relatively easy to gather retrospectively, because unlike injuries or housing damages, a person's death must be officially reported, and the whereabouts of neighbors was communally shared knowledge in wartime Japan. Thus, this study uses *the logged fatality rates* as the alternative measurement for the damage due to the air raids. The logged fatality rates highly correlates with the ratio of the housing destruction ($\rho = 0.885$), and are defined as follows:

$$\ln(\%Fatality_i) = \ln \frac{\texttt{Number of People Killed}}{\texttt{Total population in 1944}} \tag{2.2}$$

The control variables listed below are selected so that they cover almost all variables available throughout the time-series used in this study. They are is at least as extensive as those seen in other studies that attempt to estimate the causal effects of war (David and Weinstein 2002, Acemoglu, Hassan, and Robinson. 2010). Because most of the covariates are

collected every five or ten years, the variables for election years are created by interpolating the two neighboring years from which statistics are collected.[26]

α_s: Prefectural fixed effects

γ_t: Year fixed effects

$x1_{s,t}$: Population growth due to natural increase
$= \frac{\texttt{Natural population increase from } t\texttt{-5 to } t}{\texttt{Total population at } t}$

$x2_{s,t}$: Population growth due to social change
$= \frac{\texttt{Population change due to social change from } t\texttt{-5 to } t}{\texttt{Total population at } t}$

$x3_{s,t}$: Population density $(thousands/km^2) = \frac{\texttt{Total population}}{\texttt{Area}}$

$x4_{s,t}$: Percentage of foreign population $= \frac{\texttt{Foreign population}}{\texttt{Total population}}$

$x5_{s,t}$: Percentage of city population $= \frac{\texttt{City population}}{\texttt{Total population}}$

$x6_{s,t}$: Number of family members per household $= \frac{\texttt{Total population}}{\texttt{Number of households}}$

$x7_{s,t}$: Percentage of single-person households $= \frac{\texttt{Number of single-person households}}{\texttt{Number of households}}$

$x8_{s,t}$: Number of doctors per capita $= \frac{\texttt{Number of doctors}}{\texttt{Total population}}$

$x9_{s,t}$: Percentage of male population $= \frac{\texttt{Male population}}{\texttt{Total population}}$

Prefectural fixed effects are most important and require careful interpretation. They subsume various important confounders including pre-existing *levels* of both observables and unobservables, historical facts such as being ancient capital and/or having cultural heritage, and geographical characteristics such as having many hills and/or having isolated islands.

26. The only exception is the year 2001. Inclusion of the year 2001 requires one-year of extrapolation, but I decided to include this year because the increase in the number of observations by 46 probably exceeds the demerit caused by one-year extrapolation.

Year fixed effects also subsume national electoral tides. With prefectural fixed effects, other covariates control for only within-unit socioeconomic trends, but their variations help remove some of the important confounding effects.[27]

2.3.3 Estimation Method

To utilize the availability of prewar voter turnout, the hypotheses are tested with difference in differences estimations (DD). DD minimizes the possibility of observational equivalences. Particularly, DD can distinguish causal effects from contemporaneous correlations, and treatment-specific trends from overall trends. The quantity of interest is the difference in the changes in voter turnout between pre- and postwar periods *between* the prefectures that suffered from the air raids and those that did not. When the treatment variable is continuous, as in this study, DD compares the observations of which damages are different by 1 unit; otherwise, the interpretation remains unchanged. If the air raids have any negative effects on political participation, the changes for the former should be larger than those for the latter. Depending on how the air raid effects are characterized, one of the following two models is assumed, each of which provides a different interpretation of the data.

27. For example, if public housing complexes built in bombing places after WWII attracted new residents from other prefectures whose propensity to vote were lower than the original residents, the effects of the air raids are confounded by those of migration. However, socioeconomic trends such as the trend of population growth due to social change, that of percentage of single-person households, and that of number of family members per household control for this confounding.

MODEL 1: Constant effect of the air raids

$$y_{s,t} = \alpha_s + \gamma_t + \beta \ln(\%Demage_i) \cdot \mathbf{1}^{t \in T^{\text{after}}} + \sum_{t \in T^{\text{ctrl}}} \sum_{k=1}^{9} \delta_{kt} x_{k_{s,t}} \cdot \mathbf{1}^{t=T^{\text{ctrl}}} + \epsilon_{s,t}$$

MODEL 2: Time-varying effects of the air raids

$$y_{s,t} = \alpha_s + \gamma_t + \sum_{t \in T^{\text{after}}} \beta_t \ln(\%Demage_i) \cdot \mathbf{1}^{t=T^{\text{after}}} + \sum_{t \in T^{\text{ctrl}}} \sum_{k=1}^{9} \delta_{kt} x_{k_{s,t}} \cdot \mathbf{1}^{t=T^{\text{ctrl}}} + \epsilon_{s,t}$$

where $y_{s,t}$ is the voter turnout in the national elections in a prefecture s at time t,[28] β and β_t are the quantities of interest, or DD, $\mathbf{1}^t$ is the dummy variable that takes 1 if t is included in a set of specified years or equal to a given year, and takes 0 otherwise, T^{after} is a set of election years after WWII. T^{ctrl} is a set of election years with which interaction terms of covariates are created, $x_{k_{s,t}}$ and δ_{kt} are the control variables and their coefficients, and $\epsilon_{s,t}$ is an error term.[29]

Model 1 assumes that the air raids affect voter turnout at a constant rate over time. For the clarity of the interpretation, the test for the main argument and the robustness checks are conducted with this model. On the other hand, Model 2 allows the effects to vary over time, reflecting our ignorance about the trends of the effects of the air raids. Once Model 1 is supported, Model 2 can be used to determine the further characteristics of the air raid effects. This econometric model has an identical link function, in which the dependent variable follows normal distribution. Because prefecture-level voter turnout represents an aggregation of more than hundreds of thousands of voters within each prefecture, the distribution of voter

28. Specifically, t takes the following values: $t \in \{1928, 1930, 1932, 1936, 1937, 1942, 1952, 1953, 1955, 1958, 1960, 1963, 1967, 1969, 1972, 1976, 1979, 1980, 1983, 1986, 1990, 1993, 1996, 2000\}$ when the voter turnout data from the Lower House are used, and $t \in \{1928, 1930, 1932, 1936, 1937, 1942, 1953, \cdots,$ every three years until 2001$\}$ when the voter turnout data from the Upper House are used after WWII.

29. This model includes the time-constant effect of the air raids when the model is estimated with random effects.

turnout follows the binomial distribution, which is approximated by the normal distribution. The coefficients of the air raid variables (β and β_t) are interpreted as the difference in the changes in voter turnout before and after WWII between two prefectures in which the respective damage from the air raids differs by one unit. The air raids have a negative impact on voter turnout when $\beta < 0$ or $\beta_t < 0$.

A set of years with which the interaction terms of covariates are created, T^{ctrl}, is specified in two different ways. The first set of years contains only the election years before WWII. The interaction terms created from this specification, named the *prewar control*, control for only within-unit trends of the covariates in the prewar period. The second set of years contains all election years. The interaction terms created from this specification, named the *pre- and postwar control*, are the standard set of control variables in DD. Models are estimated in this way, because one of the interests of this study is the degree to which the air raid effects are independent of postwar socioeconomic trends, and the comparison of the estimates obtained from these two specifications serves a purpose. These sets of control variables are quite extensive and nest many other specifications.[30]

2.4 Results

2.4.1 Main Results from Constant Effect Model

Let us first look at the effect of the air raids under the assumption of the constant effect, which is followed by a calculation of substantive significance, robustness checks, and the estimation of the heterogeneous effect model. Descriptive statistics are discussed in Appendix A. On the chance that the design effect, or the Moulton factor, underestimates the standard errors (Bertrand, Duflo, and Mullainathan 2004), all the standard errors are estimated with

30. The estimates are robust to the different specifications of control variables, and they are available from the author upon request.

clustered bootstrap sampling with 10,000 iterations unless otherwise stated.[31]

Dependent variable: Voter turnout for the Lower House elections	(1)	(2)	(3)	(4)	(5)	(6)
After WWII ×ln(% Damage)	-0.252 (0.189)	-0.201 (0.184)	-0.274 (0.282)	-0.257 (0.222)	**-0.400** **(0.222)**	**-0.369** **(0.212)**
R-square	0.470	0.465	0.476	0.676	0.480	0.715
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Prefecture Fixed Effects	No (RE)	Yes	Yes	Yes	Yes	Yes
Set of Control Variables	No	No	Before	Full	Before	Full
Population Weight	No	No	No	No	Yes	Yes
Number of Units (Prefectures)	46	46	46	46	46	46
Number of Observations	1104	1104	1104	1104	1104	1104

Note: OLS estimates. Standard errors are in parentheses. The coefficient estimates in **Bold** is statistical significant at p<.05 in one-tailed test. Columns 1 to 4 are estimated with clustered bootstrapping with 10,000 iterations, where unit of cluster is prefecture. R-squares are overall R-squares.

Table 2.1: The air raid effects of the housing destruction on the voter turnout for the lower house elections.

Table 1 reports the estimated coefficients of Model 1 with the voter turnout from the Lower House. All columns include year fixed effects. The regression in Column 1 includes only year fixed effects and is estimated with random effects. The regression in Column 2 uses prefecture (unit) fixed effects but does not include any other covariates. Columns 3 and 4 show the main results. The regression in Column 3 adds prewar control variables to that in Column 2, and the regression in Column 4 further adds postwar control variables. The regressions in Columns 5 and 6 re-estimate Columns 3 and 4 with population weights.[32] These columns provide a useful information about whether the prefectural difference in the population has overestimation bias or not.

31. Another reestimation is performed following Angrist and Pischke (2009) in Table A4. Specifically, all years are collapsed into two periods, namely before and after WWII, and the models are estimated based on the variables averaged by each period.

32. Specifically, STATA's sampling weight (`pweight`) is used. Because the bootstrap estimation is not available with population weights, the standard errors in Columns 5 and 6 are estimated with robust standard errors clustered by prefecture, which can be biased downward.

Column 3 shows an estimated coefficient for the logged ratio of the housing destruction ($\ln(\%Damage_i)$) of -0.274 (s.e.=0.281), and other columns show similar coefficients and standard errors. Although all coefficients are negative, standard errors are too large to reject the null hypothesis. Also, statistically significant results are not found using three other air raid variables (the logged fatality rates from the Asahi data, the logged ratio of the housing destruction, and the logged fatality rates from the Yomiuri data).[33] Thus, the air raids have a marginal impact on postwar voter turnout in the Lower Housed.

This result, however, must be interpreted with reservation. The Lower House election data can be noisy as indicated by large standard errors and low R-squares. The institutional importance of the Lower House activates partisanship, issue saliency, mobilization, and media coverage, which makes the voter turnout of the Lower House higher than that of the Upper House. Thus, in the next analysis, the postwar voter turnout data of the Lower House are replaced with those of the Upper House, which presumably suffers less from these noises.

Table 2, which reports the re-estimated results of Table 1 by changing the series of postwar election data from the Lower House to the Upper House, now shows statistically significantly negative effects of the air raids on voter turnout in all columns. For the main result, Column 3 shows an estimated coefficient for the logged ratio of the housing destruction ($\ln(\%Damage_i) \cdot \mathbf{1}^{t \in T^{\text{after}}}$) of -0.636 (s.e.=0.342), which is statistically significant at the 10 percent level, and the bias-corrected confidence interval (CI) is statistically significantly negative at the 5 percent level. The overall R-square in Column 3 is a high value of 0.756, which allows a little room for any unobserved confounder to change the results.

A few points about the other columns are worth mentioning. First, inclusion of socioeconomic variables increases the negative effect of the air raids about 50 percent. This means

33. Results are not listed, but available from the author.

Dependent variable: Voter turnout for the Upper House elections	(1)	(2)	(3)	(4)	(5)	(6)
After WWII ×ln(% Damage)	**-0.475 (0.228)**	**-0.409 (0.224)**	**-0.636 (0.342)**	**-0.617 (0.251)**	**-0.798 (0.264)**	**-0.740 (0.224)**
R-square	0.739	0.737	0.756	0.834	0.755	0.842
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Prefecture Fixed Effects	No (RE)	Yes	Yes	Yes	Yes	Yes
Set of Control Variables	No	No	Before	Full	Before	Full
Population Weight	No	No	No	No	Yes	Yes
Number of Units (Prefectures)	46	46	46	46	46	46
Number of Observations	1058	1058	1058	1058	1058	1058

Note: OLS estimates. Standard errors are in parentheses. The coefficient estimates in **Bold** is statistical significant at p<.05 in one-tailed test. Columns 1 to 4 are estimated with clustered bootstrapping with 10,000 iterations, where unit of cluster is prefecture. R-squares are overall R-squares.

Table 2.2: The air raid effects of the housing destruction on the voter turnout for the upper house elections.

that the omitted control variables in Column 2 have a positive bias to β. Second, the comparison between Column 3 and Column 4 shows that the coefficients of the air raid variable are almost the same size. Because Column 4 adds postwar control covariates, the effect of the air raids is almost independent from other postwar socioeconomic trends. Also, the comparison of the estimated coefficients between Columns 5 and 6 *and* Columns 3 and 4 show that the air raid effects become stronger when the models are estimated with population weights. This indicates that the concentration of the population and the damage on urban prefectures do not cause overestimation bias. In the following section, the Upper House election data are used for the robustness checks.

2.4.2 Robustness Checks

Different air raid variables. The disagreements in the number of deaths and the number of destroyed houses between different data sources demonstrate that the air raid variables suffer from some measurement errors. Hence, estimating the effects with other air raid variables

provides a good robustness check. As noted earlier, fatality rates can be a good proxy for the damage due to the air raids. Also, these two types of air raid variables are also available from the Yomiuri data. Thus, three new air raid variables are created.

Dependent variable: Voter turnout for the Upper House elections	Table 4 (3)	Table 4 (4)	(1)	(2)	(3)	(4)	(5)	(6)
Data Source	Asahi		Yomiuri		Asahi		Yomiuri	
After WWII ×ln(% Damage)	**-0.636 (0.342)**	**-0.617 (0.251)**	**-0.428 (0.249)**	**-0.433 (0.178)**				
After WWII ×ln(% Fatality)					**-0.753 (0.455)**	**-0.704 (0.335)**	-0.498 (0.353)	**-0.557 (0.268)**
R-square	0.756	0.834	0.754	0.825	0.746	0.830	0.743	0.832
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Prefecture Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Set of Control Variables	Before	Full	Before	Full	Before	Full	Before	Full
Population Weight	No	No	No	No	No	No	No	No
Number of Units (Prefectures)	46	46	46	46	46	46	46	46
Number of Observations	1058	1058	1058	1058	1058	1058	1058	1058

Note: OLS estimates. Standard errors are in parentheses. The coefficient estimates in **Bold** is statistical significant at p<.05 in one-tailed test. All columns are estimated with clustered bootstrapping with 10,000 iterations, where unit of cluster is prefecture. R-squares are overall R-squares.

Table 2.3: The air raid effects in four different measurements on the voter turnout for the upper house elections.

Table 3 presents the re-estimated results of Columns 3 and 4 in Table 2 by changing the air raid variable from the logged ratio of housing destruction from the Asahi data to the three alternative variables. The regressions in Columns 1 and 2 use the logged ratio of the housing destruction from the Yomiuri data. The Yomiuri data and the Asahi data mainly differ in the way they count destroyed houses because the denominator is common. In four prefectures, the entry of one data source is more than twice as large as or less than a half of the entry of the other. Yet, these two variables are in general similar, and the correlation coefficient is high ($\rho = 0.937$). The coefficient of the air raid variable with the Yomiuri data

is slightly smaller than the counterparts of Columns 3 and 4 in Table 2, but the coefficients do not change with the addition of postwar controls.

The regressions in Columns 3 to 6 use the logged fatality rates from the Asahi data (in Columns 3 and 4) and from the Yomiuri data (in Columns 5 and 6). The correlation between these two variables is high ($\rho = 0.923$). These two variables mainly differ in the way they handle the numbers of missing persons. The Yomiuri data classify all missing persons as being killed, but the Asahi data maintains the distinction between being killed and missing. For example, the death toll in Hiroshima is 222,997 in the Yomiuri data, but 120,948 in the Asahi data.[34]

Columns 3 to 6 in Table 3 show consistently negative coefficients regardless of the inclusion of postwar control variables, and most of the estimates are statistically significant. Also, the estimations with the Asahi data yield larger coefficients in an absolute sense than do their counterparts with the Yomiuri data. This may imply that the Yomiuri data suffer from measurement errors and corresponding attenuation bias. It is not surprising that the Asahi data have more precise information given the date the two data sets were compiled (the Yomiuri data in 1968, the Asahi data in 2004). In all, the effects of the air raids are robust to the various measurements of damage.

Different pre- and postwar periods. One possible criticism of the findings is that prewar observations after the May 15 incident are recorded under a non-democratic regime and are not comparable to data accumulated during the postwar period, and therefore these observations should be removed from the analysis. Previously, I argued why it is reasonable not to remove them. However, it is more convincing to provide further evidence that the results are robust to the alternative definitions of pre- and postwar periods.

34. The transformation to the natural logarithms also largely moderates measurement errors of extreme values.

Dependent variable: Voter turnout for the Upper House elections	Table 4 (3)	Table 4 (4)	(1)	(2)	(3)	(4)	(5)	(6)
Prewar periods	1928-1942		1928-1932		1928-1942		1928-1942	
Postwar periods	1953-2001		1953-2001		1947-2001		1953-2010	
After WWII ×ln(% Damage)	**-0.636 (0.342)**	**-0.617 (0.251)**	-0.607 (0.412)	-0.509 (0.328)	**-0.619 (0.332)**	**-0.605 (0.248)**	**-0.644 (0.330)**	**-0.602 (0.244)**
R-square	0.756	0.834	0.740	0.840	0.744	0.810	0.773	0.840
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Prefecture Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Set of Control Variables	Before	Full	Before	Full	Before	Full	Before	Full
Population Weight	No	No	No	No	No	No	No	No
Number of Units (Prefectures)	46	46	46	46	46	46	46	46
Number of Observations	1058	1058	920	920	1150	1150	1196	1196

Note: OLS estimates. Standard errors are in parentheses. The coefficient estimates in **Bold** is statistical significant at p<.05 in one-tailed test. All columns are estimated with clustered bootstrapping with 10,000 iterations, where unit of cluster is prefecture. R-squares are overall R-squares.

Table 2.4: The air raid effects in four different observation periods on the voter turnout for the upper house elections.

Table 4 presents the estimated coefficients of which observed periods are different in the following way. The regressions in Columns 1 and 2 use only the data from the democratic period of prewar Japan during 1925 to 1932. Three elections were held during this period in 1928, 1930, and 1932.[35] Otherwise, the model keeps the same postwar period as the original analysis. The point estimate in Column 1 ($b_2 = -0.607$) is almost unchanged from the one in Column 3 in Table 2 ($b_2 = -0.636$). Although these coefficients are marginally not statistically significant, this is probably due to the reduction of the prewar observations by half. The regressions in Columns 3 and 4 add the postwar observations in 1947 and 1950 to the original analysis. The original analyses do not include these years because Japan was still under occupation and only a short time had passed since the end of the war. The regressions in Columns 5 and 6 instead add the observations taken from the most recent elections, namely 2004, 2007, and 2010. The original analyses do not use these observations because

35. The election in 1932 was held in February, which was before May 15 Incident.

the control variables are only available until 2000, and it requires extensive extrapolation to include the voter turnout data from these years. Columns 3 to 6 show the estimated coefficients and the standard errors similar to those in the original analysis. As a whole, the effects of the air raids are robust to the various definitions of pre- and postwar periods.

Trend effects. One of the concerns in using DD estimation is that unobserved trends affect only the prefectures bombed heavily, or only those bombed lightly, which cause spurious effects on the voter turnout data. For example, if heavily bombed areas have experienced rapid urbanization *independently of the air raid* and it has facilitated poor interpersonal relation, a negative effect similar to that from the air raids is observed. The following procedure is useful to test the existence of trend effects. First, each of the pre- and postwar groups is divided into two *placebo pre- and postwar groups*. Thus, the placebo postwar group in the original prewar group and the placebo prewar group in the original postwar group are fictitious. Then, the analyses similar to Columns 3 and 4 in Table 2 are conducted using the observations of the placebo pre- and postwar groups in the original prewar group *and* in the original postwar group, respectively. If any confounding trend exists, the air raid variables still have similar effects to the original analysis, which imply the air raid effects are spurious.

Columns 1 and 2 in Table 5 report the estimated coefficients of the air raid effects by dividing postwar observations into the placebo prewar group (1947 and 1950) and the placebo prewar group (every three years from 1953 until 2001). Column 1 shows an estimated coefficient for the logged ratio of the housing destruction ($\ln(\%Damage_i)$) of -0.193 (s.e.=0.371). The point estimate is less than one third of the counterpart of the original analysis, and the standard error is also too large. Inclusion of postwar control variables in Column 2 does not change this finding. In Columns 3 and 4 in Table 5, trend effects in the prewar period are tested. Prewar observations are divided into the placebo prewar group (1928) and the

Dependent variable: Voter turnout for the Upper House elections	Table 4 (3)	Table 4 (4)	(1)	(2)	(3)	(4)
Placebo prewar periods	1928-1942		1947, 1950		1928	
Placebo postwar periods	1953-2001		1953-2001		1928-1942	
After WWII ×ln(% Damage)	**-0.636 (0.342)**	**-0.617 (0.251)**	-0.101 (0.379)	-0.193 (0.371)	0.153 (0.243)	0.206 (0.229)
R-square	0.756	0.834	0.610	0.704	0.188	0.162
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Prefecture Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Set of Control Variables	Before	Full	Before	Full	Before	Full
Population Weight	No	No	No	No	No	No
Number of Units (Prefectures)	46	46	46	46	46	46
Number of Observations	1058	1058	874	874	276	276

Note: OLS estimates. Standard errors are in parentheses. The coefficient estimates in **Bold** is statistical significant at p<.05 in one-tailed test. All columns are estimated with clustered bootstrapping with 10,000 iterations, where unit of cluster is prefecture. R-squares are overall R-squares.

Table 2.5: Checking the possibility of the trend effects by using placebo pre- and postwar periods.

placebo prewar group (1930, 1932, 1937, 1938, and 1942). Columns 3 and 4 show positive and statistically insignificant coefficients, which is clearly counterevidence to trend effects. Thus, the possibility that the air raid effects are artifacts of trend effects is dismissed.

2.4.3 Substantive Effects

The substantive effects are calculated based on the estimates of Column 3 in Table 2 and Column 3 in Table 3, and results are presented in Figure 3. The blue (and longer) bars represent the average estimated change in voter turnout from the prewar to the postwar periods, and the red (and shorter) bars represent the change due to the ratio of the housing destruction (in the upper figure) and due to the fatality rates (in the lower figure).[36] How

36. In this calculation, the values of the logged treatment variables are used as if they are real measurements. This is because the econometric interpretation of the coefficient of log-transformed independent variable when the dependent variable is not log-transformed requires baseline values in the denominator to calculate the percentage increase of the independent variable. However, interesting quantities in this study

much decrease in postwar voter turnout the air raids can account for differs by prefecture. For example, using Tokyo's postwar Upper House election data, 4.86 percentage points, or a 25.3 percent decrease is due to the air raids using the scale of the ratio of the housing destruction. In general, the air raids account for $10 \sim 40$ percent of the postwar voter turnout, depending on the degree of the damage. Thus, these estimates indicate the substantially large effects of the air raids.

2.4.4 Results from Heterogeneous Effect Model

The next important question is how the air raid effects change year by year, and what explains the observed trends. To answer this question, the time-varying effects are estimated using Model 2.[37] Figure 4 shows the range plots of the 90 percent confidence interval of these estimates, the scale for which is shown in the left vertical axis. Also, the previous analyses in Table 1 and 2 find that the air raids have stronger effects for the Upper House elections than for the Lower House elections, which suggests that public attention, or national average voter turnout, may explain the trend of the time-varying air raid effects. Thus, the line plot of the nationwide voter turnout for each of the Upper House elections is overlaid to the range plots, the scale for which is shown in the right vertical axis.

In the figure, most of the confidence intervals lie below 0, with the point estimates ranging from -1.10 to -0.136 regardless of the number of years since the end of war, which indicates that the air raids have long-lasting negative effects.[38] The trend of the effects

are the changes from no damage, and we cannot have 0 percent damage in the denominator.

37. The specification of the control variables to obtain the estimates in the figure is the pre- and postwar control. With this specification, the indirect air raid effects mediated by socioeconomic factors are mostly removed from the figure.

38. Whether the figure shows a decreasing trend or not is a tough decision. As mentioned later, the air raid effects are positively correlated with national voter turnout. Although the air raid effects still show

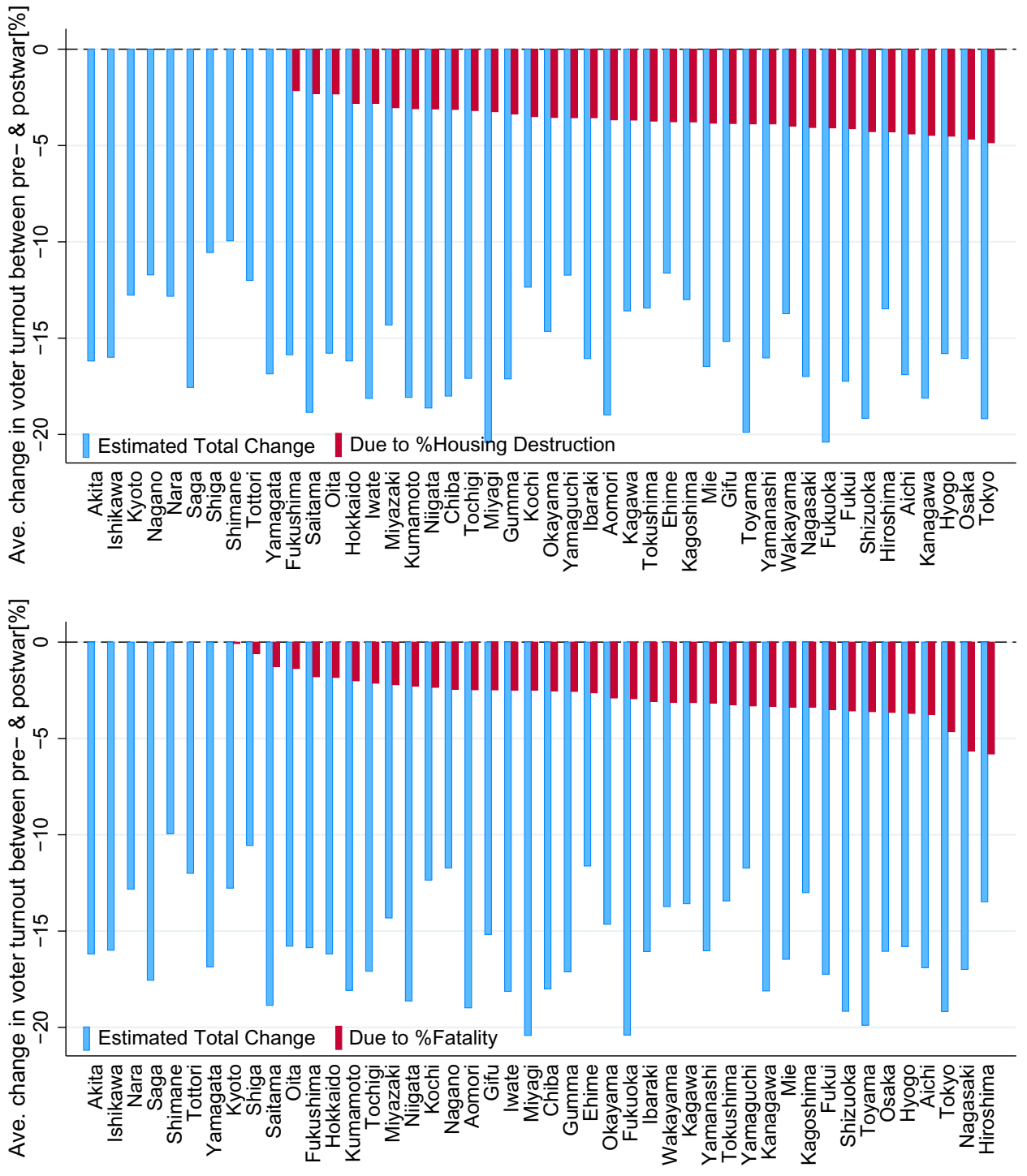

Figure 2.3: Substantive significance of the air raid effects on voter turnout.

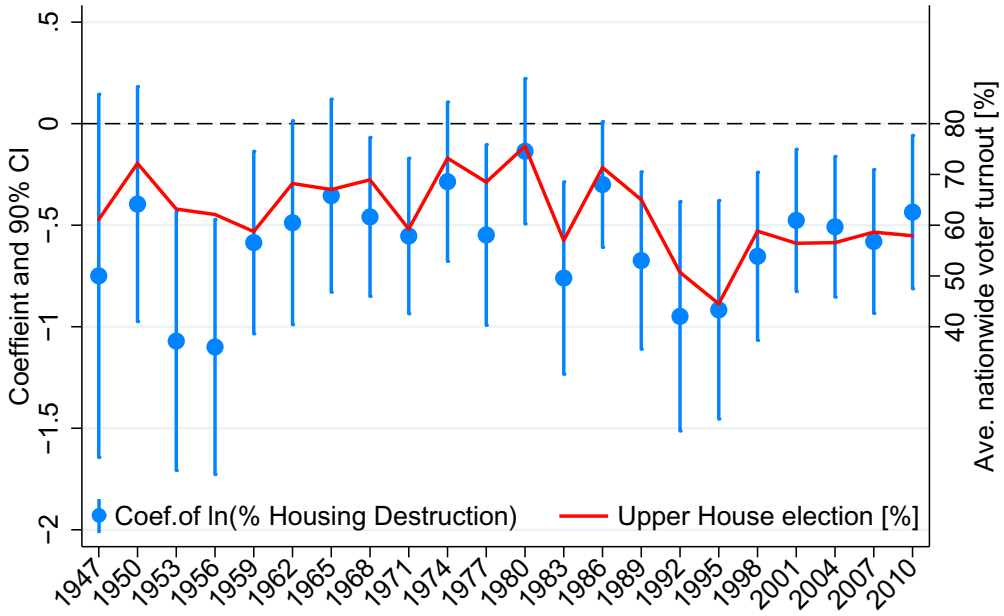

Figure 2.4: Trends of heterogeneous air raid effects and nationwide voter turnout.

seems random at first glance. However, once the trends are interpreted in combination with the trends of nationwide voter turnout, it turns out that these two variables highly correlate each other ($\rho = 0.626$). Also, it must be noted that high correlation is only observed when the coefficients are estimated with the pre- and postwar control. The correlation between nationwide voter turnout and the air raid effects estimated with prewar control variables is only $\rho = 0.286$. This implies that the air raids primarily affect the elections that are not considered to be important for many people, and this relationship is independent of socioeconomic factors.[39] In the next section, I further investigate the mechanism of the air raid effects based on the findings thus far and other supplemental analyses.

significantly negative effects in recent elections, recent elections recorded quite low voter turnout. Therefore, the air raid effects relative to voter turnout are not necessarily large, but rather smaller than those in previous years. (Notice that only the point estimates of the recent elections are located higher than national voter turnout.)

39. On a related point, the strong correlation is unlikely to be an statistical artifact due to the negative correlation between intercepts and slopes. The correlation coefficient between the coefficients of the air raid effects and those of the year fixed effects are only $\rho = 0.154$.

2.5 Discussion

Although a series of analyses have confirmed the negative causal effects of the air raids, the causal *mechanism* as to why the air raids decrease the voter turnout still remains unsolved. Admittedly, the causal *mechanism* cannot be tested with the data available, and any inference about the mechanism must depend on the findings thus far and additional non-causal analysis. Bearing this limitation in mind, I attempt to provide the most probable answer as well as to eliminate other theoretical possibilities. As a first step, the empirical findings are summarized as follows.

1. The air raids have negative effects on voter turnout (from Table 2).
2. The effects are long-lasting (from Figure 3).
3. The effect is almost independent of contemporaneous socioeconomic factors (from Columns 3 and 4 in Table 2).
4. The effect is strong when the election records low voter turnout (from Table 1, Table 2, and Figure 3).

From Finding 2, the decrease in voter turnout is probably not caused by the decrease in human capital. For example, if injuries from the air raids made it hard to vote or those killed in the war were more likely to have voted had they survived, such effects should diminish as time passes because the proportion of affected people in the population decreases over time. However, Figure 3 shows the air raid effects are negative even 60 years after the air raids.

Also, Finding 3 implies that urbanization does not mediate the air raid effects. The air raids might clear a vast open space which was initially occupied by small houses, and this combined with government reconstruction subsidies might facilitate the subsequent urban developments. However, the estimated air raid effects in Columns 3 and 4 of Table 2 are

almost the same, which indicates the subsequent urban developments have little impact on the postwar voter turnout.[40]

Finding 4 means that the air raids destroyed something that promotes voting, particularly when the election is not important for many voters. Two interpretations are possible depending on which of two distinctive views on voting one adopts, namely the instrumental view and the consumptive view (Campbell 2006). They differ in the purpose of voting: the former assumes that voting is the instrument to obtain desired policy outputs and the latter assumes that voting itself is the purpose.

If one adopts an instrumental view, those who vote in an election with low national voter turnout should have a strong interest in policy outcomes. Perhaps, the air raids weakened the distributional coalitions (Olson 1982) in the heavily bombed prefectures. In the context of Japanese politics, construction and agricultural workers have played active roles in elections in exchange for public works contracts and part-time jobs during the agricultural off-season. This exchange between ballots and governmental subsidies has propped up the administration of the Liberal Democratic Party (LDP) (Hirose 1981). Nevertheless, there are a couple of reasons why I suspect this possibility. First, the air raid effects remain strong when the mobilization by LDP is known to be weak. In duodecennial *Idoshi*, the year of the Boar in the Oriental Zodiac, a quadrennial unified local election and a triennial Upper House election are held in the same year. It is empirically known that the Upper House elections in *Idoshi* record lower voter turnout because the heads of municipalities, who often electorally support the Upper House members, spend their resources for their local elections, and cannot mobilize the constituents in the Upper House elections. Therefore, if the air raid effects affect voter turnout through mobilization, the coefficients in *Idoshi* should be smaller. However, Figure 3 shows that in *Idoshi* (1947, 1959, 1971, 1983, 1995, and 2007), both the voter turnout and the coefficients take smaller values than those in other years. Second, if

40. Davis and Weinstein (2002) also find that the air raids did not affect the postwar economic growth of the cities.

mobilization is the reason, the point estimates using the Lower House election data must be as strong as those estimated with the data of the Upper House elections because the Lower House, which has the power to pass budgets, is more important in distributing pork barrels. Yet, the comparison between Table 1 and 2 denies this account.

Thus, the mechanism is better explained from the consumptive view. According to this view, those who vote in an election with low voter turnout derive a great deal of satisfaction out of the act of voting itself. Such satisfaction sometimes originates from a feeling of fulfilling one's civic duty, or from a relief from the tremendous social costs associated with abstention such as a bad reputation. The previous accounts have already excluded human or economic capital-based accounts of the air raid effects. Thus, the air raid effects are likely to be mediated by something other than individual attributes that facilitates political participation, and this description closely corresponds to the notion of *social capital*.[41]

There is suggestive evidence that the air raids lower the density of the neighborhood social network. Table 6 presents the estimated coefficients of the effects of the percentage of destruction by the air raids on the membership rates of three types of voluntary associations — namely, voluntary social service, hobbies and amusements, and sports, of which data is collected from *The Basic Survey on Social Life* (Bureau of Statistics 1976). The analysis uses a single year survey from 46 prefectural capitals merged with the variable of the proportion of stricken cities' area, and the model is estimated with group probit. Due to data limitations, this estimation cannot control for the baseline dependent variable. Therefore, the results leave the possibility that the pre-existing variations in membership rate and the air raids produce contemporaneous correlations. However, this analysis provides detailed information on social capital, the suspected mediation factor. Table 6 shows only that the participation

41. According to Coleman (1990), social capital comprises the social ties, networks, interpersonal trust, and other factors that facilitate any kind of productive activity and that are not reducible to individual attributes.

Dependent Variable: Membership rates of the following associations in 1976	Voluntary Social Service	Hobbies and Amusements	Sports
% Destruction by Air Raid	**-0.00222** **(0.00075)**	-0.000124 (0.000394)	-0.000369 (0.000298)
Poppulation in 1940 (in million)	0.0374 (0.0108)	0.00642 (0.00479)	-0.00163 (0.00439)
Ratio of Marriage in 1943 per population	-11.2 (17.2)	-0.208 (6.795)	-8.08 (6.74)
Poppulation Growth from 1940 to 2000	-0.0197 (0.0144)	0.00792 (0.00717)	-0.00082 (0.00727)
Constant	-0.141 (0.183)	-1.412 (0.087)	-0.746 (0.060)
Number of Units (Prefectural Capitals)	46	46	46
Number of Respondents	28192	28192	28192

Note: Grouped probit regression. Population variable is the number of respondents in each survey unit (prefectural capital). The coefficient estimates in **Bold** is statistical significant at p<.05 in one-tailed test. Robust standard errors clustered by prefectural capitals are in parentheses.

Table 2.6: The air raid effects of the housing destruction on the level of involvement in voluntary groups.

ratio of voluntary social services is negatively affected by the air raids with the statistical significance at the 1 percent level. Importantly, in Japan the variable of voluntary social service association is almost equivalent to the variable of neighborhood association.[42] Moreover, neighborhood associations have an interesting historical background. In many places, neighborhood associations preexisted informally, but in the late 1930s they were reorganized nationwide as *Tonari-gumi*, the lowest level of IRAA for military mobilization. After WWII, *Tonari-gumi* was dissolved by the order of SCAP, but neighborhood associations remained as voluntary organizations.

42. The same data source reports 63 percent of respondents answer voluntary social service activities are performed with their neighbors, and 18 percent answered those activities are performed with their friends or acquaintances. Because friends or acquaintances often include neighbors the fraction of people who perform voluntary social service activities with their neighbors is quite high.

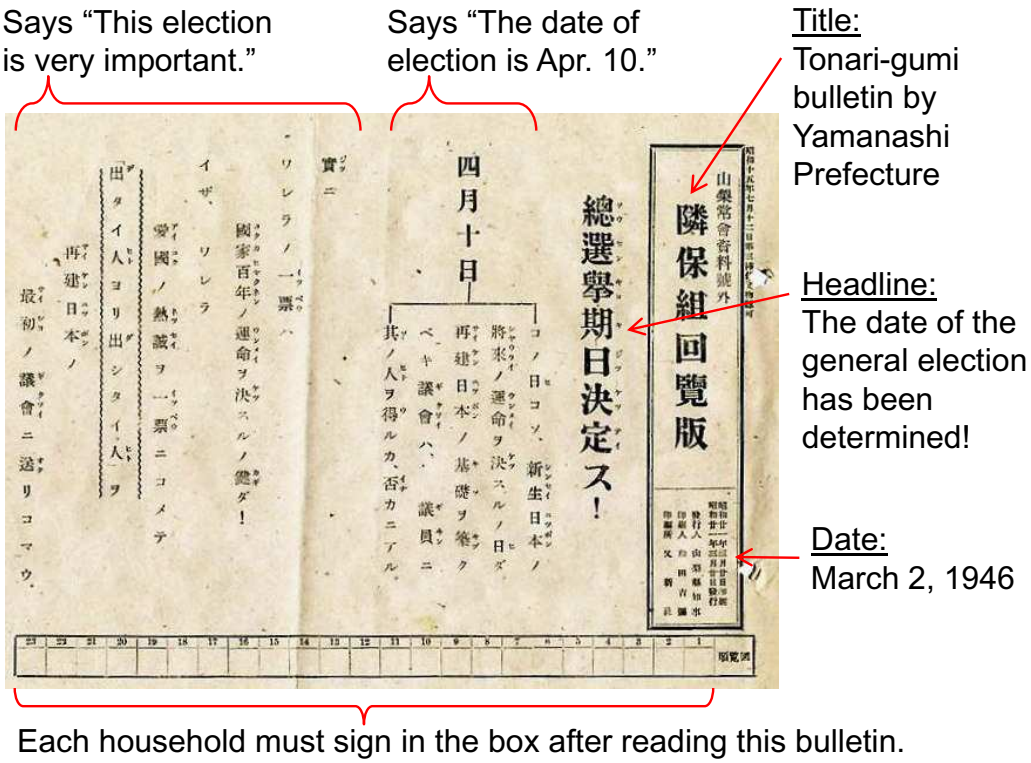

Figure 2.5: Example of pass-along bulletin.

Neighborhood associations perform multiple social services such as community cleanup, annual festivals, fire drills or neighborhood watches, but perhaps the most important function is the circulation of pass-along bulletins. Figure 5 presents an example of the bulletin, which was circulated before the first postwar election. In the election month, the bulletin contains the reminder of an upcoming election along with the date and the location of the polling place, which provides voters essential information. Importantly, the bulletin also has a monitoring function. Each householder must sign in one of the boxes on the bottom of the figure after reading the bulletin and deliver it to a person next door.

Thus, I argue that what the air raids destroyed was social capital, particularly the neighborhood social network represented by such entities as neighborhood associations, which promote voting out of civic motivation. This account also corresponds to the fact that the broadest damage created by the air raids was homelessness. Nine million people lost their homes due to the air raids. In the bombed places, neighborhood social networks were lost because some neighbors were killed, some left their community, some buildings were rebuilt, and new families moved in.

2.6 Conclusion

In this paper, I started by estimating the causal effect of the air raids on political participation, particularly voter turnout in national elections. A series of analyses confirm the negative and long-lasting effects of the air raids, and these effects are particularly strong when the national voter turnout is low. Of course, these findings cannot be claimed without reservation. The damage due to the air raids may coincidentally have high correlation with other unknown changes that occurred after WWII. Nevertheless, these findings are robust to several alternative specifications, and I believe this study makes great advances in understanding the causal effects of war on democratic participation.

The study also sheds light on the causal pathway of war on democratic participation. The

summary of the findings indicates that the most probable mediation factor that links the damage due to the air raids and the decrease in postwar voter turnout is the neighborhood social network. The mass-destruction by the air raids lowered the level of social capital, and weak social ties presumably decreased the likelihood of voting out of civic motivation.

Returning to the initial concern, although the use of force must be considered only as a last resort, policy makers need to plan the military operation so that it minimizes the destruction of neighborhood social networks. The conditions under which residents give up returning to their neighborhoods require another extensive study, but a wide-spread and long-lasting attack may make the affected residents abandon their neighborhood regardless of the intensity of the attack. Furthermore, the importance of repopulation of the original residents and re-construction of the neighborhood social network needs to be emphasized in postwar governance.

2.7 Appendix A: Descriptive Statistics

Table A1 presents descriptive statistics. The statistics are segmented by pre- and postwar, as well as the severity of the damage. Specifically, prefectures are divided in half based on the rank of the ratio of the housing destruction from the Asahi data. The first four rows presents statistics from the air raid variables. The rows in the middle cluster offer statistics from the prewar period, which contains the most important information. Those in the bottom cluster are statistics from the postwar period. In the middle rows, one can see that prefectures heavily bombed lost fewer people due to social change, were more densely populated, had more aliens, and had more urban populations. Otherwise, left and right groups have almost the same characteristics. Simply stated, even though the air raids was indiscriminate, the damages still concentrated in urbanized prefectures. However, the difference in the degree of prewar urbanization is subsumed by the prefectural fixed effects. Also, as shown in the analysis, the coefficients of the air raid effects remain almost unchanged regardless of whether postwar socioeconomic variables are controlled for. That is, the air raid effects are largely independent of socioeconomic trends.

2.8 Appendix B: Does high voter turnout mean high democratic participation?

The validity of assigning positive connotation to high voter turnout may need to be scrutinized. Many empirical studies use high voter turnout synonymously with high democratic performance (Bennett and Resnick 1990, Putnam 1993), and this study conforms to this practice. However, it is important to clarify whether this assumption holds in pre- and postwar Japan. At minimum, voter turnout positively correlates with democratic performance in a fundamental sense. If a government decides policies completely independently of the result of an election, few voters bother to vote. Thus, positive voter turnout to some degree

reflects voters' confidence to the political regime.

However, one possible objection to this interpretation is that a well-functioning democracy does not necessarily lead to high turnout because voters who feel satisfied with the current political system do not need to express their voice (Hirschman 1970). Fortunately, some evidence suggests this objection is not the case in Japan. First, Japanese voters were less motivated when their opinion were unlikely to be reflected in the politics. Gordon (2003) notes that "[v]oter turnout fell sharply by the mid-1930s. As few as 60 percent of eligible voters went to the polls in urban districts." (196) In fact, after the May 15 incident in 1932, elder statesmen chose the Prime Ministers not from the leader of the leading party but from those who could obtain the consent of the military. The decline in voter turnout corresponds with the timing of this incident. Secondly, opinion survey indicates a positive correlation between reported voter turnout and political efficacy. Using the second wave of Japan Election Studies II (Miyake et al. 1993), a one standard deviation increase in political efficacy[43] increases the probability of voting at the general election in 1993 by about 4.5 percentage points plus or minus 1.3 percentage points.[44] Moreover, the effect of political efficacy does not diminish to scale.[45] Thus, at least in Japan, it is safe to say that higher voter turnout indicates a better-functioning democracy.

2.9 Appendix C: Calculating the bias due to the difference in the voting populations.

The franchise of the national general election before WWII was restricted to the male, and it was after WWII when the franchise was extended to female. This appendix intends to

43. Exact question sentence is, "Do you think an election allows for ordinary people's opinions to be reflected in politics?", and the respondents evaluated this statement in 5-point scale.

44. The statement of uncertainty is based on 95 percent confidence interval.

45. Specifically, the model is re-estimated with the second degree orthogonalized polynomial of political efficacy as an additional covariate. The results are available from the author upon request.

calculate the bias due to the difference in the voting populations between pre- and postwar periods and to clarify the assumption to ignore the bias.

First consider the model in which hypothetically both pre- and postwar data are complete. That is, the prewar voter turnout for female is available. Also, for simplicity, let's assume the following models where the effects of other covariates are already taken into account, and the damage due to the air raids is assumed to be binary.

$$y_{s,t} = \alpha + \beta \mathbf{B}_s + \gamma \mathbf{T}_t + \delta \mathbf{B}_s \cdot \mathbf{T}_t + \epsilon_{s,t}$$

where $\mathbf{B}$ is the dichotomized air raid variable, and $\mathbf{T}$ is the dummy variable for the postwar years. Then, the quantities of interest the Difference in Differences estimator is calculated as follows.

$$\begin{aligned}
\mathrm{E}[\Delta] &= \left(\overline{y_{s,t}}^{\mathbf{B}=1}_{\mathbf{T}=1} - \overline{y_{s,t}}^{\mathbf{B}=1}_{\mathbf{T}=0}\right) - \left(\overline{y_{s,t}}^{\mathbf{B}=0}_{\mathbf{T}=1} - \overline{y_{s,t}}^{\mathbf{B}=0}_{\mathbf{T}=0}\right) \\
&= \{(\alpha + \beta + \gamma + \delta) - (\alpha + \beta)\} - \{(\alpha + \gamma) - (\alpha)\} \\
&= (\gamma + \delta) - \gamma \\
&= \delta
\end{aligned}$$

Thus, δ is the true effect from the air raids. Now, consider the case in which the prewar voter turnout for female is *not* missing unlike in reality, and male and female are differently affected by the air raids. Formally,

$$\begin{aligned}
y_{m_{s,t}} &= \alpha_m + \beta_m \mathbf{B}_s + \gamma_m \mathbf{T}_t + \delta_m \mathbf{B}_s \cdot \mathbf{T}_t + \epsilon_{m_{s,t}} \\
y_{f_{s,t}} &= \alpha_f + \beta_f \mathbf{B}_s + \gamma_f \mathbf{T}_t + \delta_f \mathbf{B}_s \cdot \mathbf{T}_t + \epsilon_{f_{s,t}}
\end{aligned}$$

Then, the unbiased parameters are represented as the weighted average of the gender-

and period-based voter turnout, which is formally,

$$\alpha = p_m\alpha_m + (1-p_m)\alpha_f$$
$$\beta = p_m\beta_m + (1-p_m)\beta_f$$
$$\gamma = p_m\gamma_m + (1-p_m)\gamma_f$$
$$\delta = p_m\delta_m + (1-p_m)\delta_f$$

where p_m is the proportion of male voters. However, the prewar voter turnout for female is missing in reality. Therefore, $\overline{y_{s,t}}_{\mathbf{T}=0}^{\mathbf{B}}$ needs to be replaced with $\overline{y_{m_{s,t}}}_{\mathbf{T}=0}^{\mathbf{B}}$. Then, the DD estimator is calculated as follows.

$$\begin{aligned}
\mathtt{E}[\tilde{\Delta}] &= \left(\overline{y_{s,t}}_{\mathbf{T}=1}^{\mathbf{B}=1} - \overline{y_{m_{s,t}}}_{\mathbf{T}=0}^{\mathbf{B}=1}\right) - \left(\overline{y_{s,t}}_{\mathbf{T}=1}^{\mathbf{B}=0} - \overline{y_{m_{s,t}}}_{\mathbf{T}=0}^{\mathbf{B}=0}\right) \\
&= \{(\alpha+\beta+\gamma+\delta) - (\alpha_m+\beta_m)\} - \{(\alpha+\gamma) - (\alpha_m)\} \\
&= \beta + \delta - \beta_m \\
&= \delta + p_m\beta_m + (1-p_m)\beta_f - \beta_m \\
&= \delta + (1-p_m)(\beta_f - \beta_m)
\end{aligned}$$

Thus, the second term in the last expression, $(1-p_m)(\beta_f - \beta_m)$, is the bias due to the difference in the populations between two periods. Because β represents the time-invariant difference in voter turnouts between the prefectures that had the air raids and those that did not, $\beta_m = \beta_f$ means that this quantity should not be different between men and women. Because women were not permitted to vote before WWII, β_f is a counterfactual quantity. Unless there is a strong reason to believe this is wrong, it seems innocuous to assume $\beta_m = \beta_f$. At the same time, this leaves a room for additional analysis if such counterargument arises.

I also collected the new dataset for postwar male voter turnout in the most recent four

elections from Statistics Bureau of Japan,[46], and estimated the air raid effects replacing original postwar election data with this alternative data. Columns 1 and 3 in Table A2 show the consistent findings as the main results. The insignificant coefficients in Columns 2 and 4 do not affect the findings because bad controls are intentionally included in these models to examine the mediation effects as I did in the original analysis.

46. The data is available online at the following URL: `http://www.soumu.go.jp/senkyo/senkyo_s/data/index.html`.

2.A Appendix Tables and Figures

Variable	Mean	S.D.	Max.	Min.
Ratio of the destroyed houses (Asahi) [%]	10.3	12.5	58.3	0.00
Ratio of the destroyed houses (Yomiuri) [%]	10.3	13.7	65.0	0.00
Fatality rates (Asahi) [%]	0.38	1.15	6.16	0.00
Fatality rates (Yomiuri) [%]	0.48	1.79	11.3	0.00

	Variable	Upper half in %Damage				Lower half in %Damage			
		Mean	S.D.	Max.	Min.	Mean	S.D.	Max.	Min.
Prewar period (1925-1942)	Voter turnout (Lower House) [%]	80.3	6.31	91.2	53.6	82.6	5.79	92.8	63.8
	Population growth (natural increase) [%]	7.31	1.39	11.6	4.48	7.58	1.93	11.1	4.06
	Population growth (social change) [%]	-0.59	5.07	15.2	-6.95	-3.15	2.42	5.2	-7.67
	Population density [per sq. km]	465	666	3154	88.2	170	89.0	459	30.4
	Ratio of aliens [%]	1.15	1.15	5.39	0.06	0.53	0.60	3.82	0.05
	Ratio of urban population [%]	28.0	19.3	93.6	3.37	15.5	11.2	70.0	2.31
	Size of household	4.92	0.36	5.99	4.08	5.11	0.50	6.16	3.98
	Ratio of single-person household [%]	5.80	1.29	8.15	3.23	4.99	1.38	7.98	2.92
	Ratio of doctors in population [%]	0.07	0.02	0.16	0.04	0.07	0.02	0.15	0.04
	Male ratio [%]	49.6	1.42	52.9	45.5	48.9	1.24	52.2	44.5
Postwar period (1952-2001)	Voter turnout (Lower House) [%]	72.9	7.21	88.5	53.1	75.8	7.30	88.0	53.4
	Voter turnout (Upper House) [%]	63.9	9.82	85.3	36.6	67.6	10.1	87.5	35.9
	Population growth (natural increase) [%]	4.00	2.33	10.4	-0.58	3.80	2.29	10.1	-1.06
	Population growth (social change) [%]	-0.86	4.62	21.1	-12.0	-1.70	4.51	18.6	-11.5
	Population density [per sq. km]	812	1285	5564	137	276	272	1833	57.4
	Ratio of aliens [%]	0.62	0.51	2.51	0.03	0.36	0.38	1.99	0.05
	Ratio of urban population [%]	65.5	16.5	100	20.5	59.0	13.0	88.2	20.8
	Size of household	3.80	0.76	5.78	2.23	3.99	0.82	5.84	2.47
	Ratio of single-person household [%]	13.4	8.02	41.4	2.50	12.0	7.61	31.2	2.22
	Ratio of doctors in population [%]	0.13	0.04	0.27	0.06	0.13	0.04	0.26	0.03
	Male ratio [%]	48.6	1.02	51.6	46.4	48.4	0.85	50.8	46.7

Note: The statistics are calculated from the observations of the election years.

Table 2.A.1: Descriptive statistics.

Dependent variable: Voter turnout for the Upper House elections	(1)	(2)	(3)	(4)
Gender of Postwar Voting Population	Male and Female		Male Only	
Post-WWII Periods	1998-2010		1998-2010	
After WWII ×ln(% Damage)	**-0.666 (0.277)**	-0.432 (0.294)	**-0.714 (0.279)**	**-0.503 (0.300)**
R-square	0.809	0.904	0.849	0.904
Year Fixed Effects	Yes	Yes	Yes	Yes
Prefecture Fixed Effects	Yes	Yes	Yes	Yes
Set of Control Variables	Before	Full	Before	Full
Population Weight	No	No	No	No
Number of Units (Prefectures)	46	46	46	46
Number of Observations	506	506	506	506

Note: OLS estimates. Standard errors are in parentheses. The coefficient estimates in **Bold** is statistical significant at p<.05 in one-tailed test. All columns are estimated with clustered bootstrapping with 10,000 iterations, where unit of cluster is prefecture. R-squares are overall R-squares.

Table 2.A.2: Checking the bias from using voter turnout data in which male and female are not distinguished.

Dependent variable: Voter turnout for the Upper House elections	Table 4 (3)	Table 4 (4)	(1)	(2)	(3)	(4)	(5)	(6)
Excluded Prefecture	None		Tokyo		Hiroshima		Nagasaki	
After WWII ×ln(% Damage)	**-0.636 (0.342)**	**-0.617 (0.251)**	**-0.644 (0.344)**	**-0.630 (0.249)**	**-0.604 (0.344)**	**-0.609 (0.246)**	**-0.671 (0.346)**	**-0.634 (0.253)**
R-square	0.756	0.834	0.754	0.825	0.746	0.830	0.743	0.832
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Prefecture Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Set of Control Variables	Before	Full	Before	Full	Before	Full	Before	Full
Population Weight	No	No	No	No	No	No	No	No
Number of Units (Prefectures)	46	46	45	45	45	45	45	45
Number of Observations	1058	1058	1035	1035	1035	1035	1035	1035

Note: OLS estimates. Standard errors are in parentheses. The coefficient estimates in **Bold** is statistical significant at p<.05 in one-tailed test. All columns are estimated with clustered bootstrapping with 10,000 iterations, where unit of cluster is prefecture. R-squares are overall R-squares.

Table 2.A.3: Re-estimation of Columns 3 and 4 in Table 2 by removing three heavily damaged prefectures.

Dependent variable: Voter turnout for the elections of the following House	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Lower House				Upper House			
After WWII ×ln(% Damage)	-0.107 (0.236)	-0.106 (0.159)	-0.091 (0.259)	-0.100 (0.195)	-0.439 (0.286)	**-0.413** **(0.182)**	-0.446 (0.298)	**-0.375** **(0.211)**
R-square	0.322	0.455	0.318	0.562	0.734	0.635	0.732	0.673
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Prefecture Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Set of Control Variables	Before	Full	Before	Full	Before	Full	Before	Full
Population Weight	No	No	Yes	Yes	No	No	Yes	Yes
Number of Units (Prefectures)	46	46	46	46	46	46	46	46
Number of Observations	92	92	92	92	92	92	92	92

Note: OLS estimates. The coefficient estimates in **Bold** is statistical significant at p<.05 in one-tailed test.Robust standard errors clustered by prefecture are in parentheses. R-squares are overall R-squares.

Table 2.A.4: Re-estimation of the air raid effects with collapsed data.

CHAPTER 3

THE MATCHED NEIGHBORHOOD THEORY

ABSTRACT

This study introduces the matched neighborhood theory, which posits that the proximity of one individual's type, when compared to her neighbors', independently determines the level of political participation. This account has not previously been explored because of the difficulty associated with identifying an individual's type. This study addresses that challenge through a novel application of propensity score matching. Both graphical and statistical analyses show that having many like-minded neighbors discourages political participation. Specifically, the proximity of individual's type has a stronger effect on political activities requiring a strong incentive to change an outcome. The study casts doubt on the *a priori* adoption of treatment homogeneity in the study of neighborhood effects and submits rare evidence for an instrumental view of political participation.

3.1 Introduction

This brief essay introduces the matched neighborhood theory, which posits that the proximity of one individual's type, when compared to her neighbors' (the type proximity, henceforth), independently determines the level of political participation through a novel application of propensity score technique and non-parametric estimation. The type proximity is defined as the neighborhood characteristic gap between an individual's *actual* living place and her *predicted* living place as predicted by her individual attributes. Specifically, this study considers neighborhood average household income as a main neighborhood characteristic of interest. This study finds that she is more likely to participate in politics when she is predicted to live in a poor neighborhood, but is instead living in a wealthy neighborhood, or vice versa.

The type proximity is a mathematical combination of a specific neighborhood environment and individual personal attributes. It has not been quantified in past studies due to the difficulties inherent in identifying an individual's type. In fact, a single factor of a researcher's choice is hardly sufficient to represent such a type. For example, personal poverty is not necessarily synonymous with high propensity to live in a poor neighborhood. Rather, the propensity to live in a poor neighborhood, or more precisely the predicted level of neighborhood average household income, should be determined as a complex function of an individual's attributes. For this reason, adding an interaction term between an individual's income and the average household income of a particular neighborhood in the regression is not sufficient.[1]

This study addresses this challenge by using the de-dimensionalizing property of the propensity score technique. The resulting measure of the type proximity has both methodological and theoretical foundations in the literature, as will be explained in the next section. The third section describes the research design, while the fourth section presents the results.

1. In fact, as shown in Table A1, such an interaction term does not have an independent explanatory power for political participation.

Finally, the fifth section concludes.

3.2 Theory

3.2.1 Uni-directional model.

Previously, neighborhood effects had been assumed to be uni-directional. That is, when a neighborhood has more or less of a given characteristic (e.g. average household income), a better or worse outcome is brought to the neighborhood. This assumption was reasonable given the fact that the primary concerns of the researchers who studied neighborhood effects in the early period were urban problems such as urban poverty or low educational attainments.

In political science, researchers claimed that neighborhoods with concentrated poverty often lack both physical and social resources such as public safety or active political organization (Cohen and Dawson 1993; Alex-Assensoh and Assensoh 2001). For instance, the high incidence of crime in urban ghettos increases the costs of political activity (e.g. voting, political gathering, canvassing, etc.). Residents in such areas tend to be socially isolated, and people with a sparse social network are less likely to engage in political and social activities.

As the mirror image of concentrated poverty, suburbanization has created homogenous, affluent neighborhoods that maintain social norms (Huckfeldt 1979), social capital (Putnam 1995), and social networks (Verba, Schlozman and Brady 1995) that encourage political participation. A high level of social capital lowers the costs of obtaining necessary information for political activity, while increasing the costs of losing reputation by abstention (Dunne, Reed, and Wilbanks 1997), both of which promote voter turnout.

What is common in these studies is the uni-directional assumption of neighborhood effects. In their theory, lower average household income is assumed to discourage political participation. Likewise, a neighborhood with higher average educational achievement is

assumed to encourage political participation. Thus, their models take the following form.

$$y_{ni} = \alpha + \beta \mathcal{N}_n + ctrl_{ni} + \epsilon_{ni} \tag{3.1}$$

where y_{ni} is a level of political participation of an individual, i, living in a neighborhood, n, $\mathcal{N}_n$ is a neighborhood characteristic of interest, *ctrl* represents the component explained by control variables, and ϵ_{ni} represent an error term. As the model suggests, the neighborhood effect is estimated as β.[2]

3.2.2 Bi-directional model.

The next generation of researchers studying neighborhood effects was skeptical about *a priori* assumption about uni-directional neighborhood effects. For example, Oliver (2001) took an agnostic position as to whether ethnic or income homogeneity caused by suburbanization encourages or discourages political participation, and let the data speak for itself. In fact, contrary to Hackfeldt's (1979) study, he found that neighborhood affluence deters political participation.

Campbell's (2006) *dual motivation theory* casts doubt on uni-directional neighborhood effects. Even though his geographic unit of interest is county and his spatial characteristic of interest is political heterogeneity (i.e., concentration of partisanship within a geographic unit), his argument applies to the broad studies of neighborhood effects. His theory claims that when political heterogeneity is low, individuals are encouraged to vote because "social norms facilitating civically motivated collective action are stronger" (civic motivation). Likewise, when political heterogeneity is high, people are motivated to vote because "their individual vote has a greater chance of affecting the election's outcome" (political motiva-

2. Whether β represents the *causal* effect of some neighborhood characteristic or the spurious correlation induced by unobserved individual attributes that affect individuals' choice of living places is still open to dispute.

tion).

In the regression framework, Campbell's dual motivation theory is represented by the quadratic term of a neighborhood effect. In fact, he presents voter turnout as a convex (open-up) function of political heterogeneity. That is, voter turnout is lowest when his index of political heterogeneity takes a middle value. His model takes the following form, and the neighborhood effect is predicted from β and γ.

$$y_{ni} = \alpha + \beta \mathcal{N}_n + \gamma {\mathcal{N}_n}^2 + ctrl_{ni} + \epsilon_{ni} \tag{3.2}$$

3.2.3 Matched neighborhood theory.

This logical extension of dual motivation theory, however, casts doubt on itself. If individuals can behave differently depending on the value of a neighborhood characteristic, nothing warrants that different individuals should behave in the same way under the same neighborhood environment. For example, a Republican-dominant neighborhood is very homogenous for supporters of the Republican Party, but is very heterogeneous for Democrats. Even for two people with the same partisanship, an individual may be more inclined to conform to one neighbors' partisanship than the other. Of course, "different people behave differently" is hardly informative, but differences are worth being explored if they have some systematic patterns.

I argue that a neighborhood effect is heterogeneous, but this heterogeneity is explained by a very simple principle. Specifically, people decide whether to participate in a given political activity based on *whether their neighbors are like themselves.* For convenience, let us call this proposition the *matched neighborhood theory.* People are considered to live in a matched neighborhood if their type is close to that of their neighbors. Whether living in a matched neighborhood encourages or discourages political participation largely depends on the nature of the activity and is an empirical question. However, no matter which way

the theory works, the individuals' behavior is theoretically underpinned by well-recognized theories, namely the Tiebout-Hirschman and cross-pressure theories.

The Tiebout-Hirschman theory posits that a matched neighborhood discourages political participation. This theory states that low political participation reflects people's low demand for political activity. If voters find an ideal community by voting with their feet (Tiebout 1956), they will not bother voting with their hands because someone else will represent their policy position. Sorting is another expression for this behavior (Bishop 2008). Along the same logic, if people are dissatisfied with the political situation, they would exercise "voice" behavior (Hirschman 1970). The so-called "threat hypothesis" in ethnic politics also falls into this category (Key 1949; Schoenberger and Segal 1968; Taylor 1998; Enos 2011).

On the other hand, cross-pressure theory posits that a matched neighborhood encourages political participation. People may well abstain from expressing political opinions that conflict with their neighbors'. They may seek to alter their perception or behavior (Festinger 1957), or may yield to cross pressure and stop confronting the conflict (Berelson, Lazarsfeld, and McPhee 1954; Gimpel and Shaw 2004). Social identity theory provides a similar explanation; it claims that human beings have a natural tendency to develop an identity within a given social group regardless of whether the identity is rationally underpinned (Tajfel et al. 1971; Alesina and La Ferrara 2000).

As is shown later in Figure 3, the way these theories affect various political activities seems intuitive. That is, when people engage in a political activity to enjoy its process, a matched neighborhood tends to encourage participation. On the other hand, when engaged in a political activity to affect its outcome, a matched neighborhood tends to discourage participation.

These behavioral accounts are not new. In fact, they are essentially the same as political motivation and civic motivation as explained in the dual motivation theory. However, the critical difference between the two theories is that while dual motivation theory assumes

that a given neighborhood characteristic has two opposing effects depending on its value and that a certain value of the characteristic affects everyone in the same way (i.e., the homogenous treatment effect),[3], the matched neighborhood theory claims that the direction of the neighborhood effect depends on the nature of the activity, and a certain value of the characteristic affects people differently depending on who they are (i.e., the heterogeneous treatment effect).

Living in a matched neighborhood is different from having a dense social network (Verba, Schlozman and Brady 1995) or having political discussion partners (Kenny 1992). These theories assume that the dense network encourages political participation monotonically while a matched neighborhood may discourage the participation, as this study finds.

3.2.4 Neighborhood characteristic = Average type of the neighbors.

One remaining question concerns how to quantify an individual's type. Since the study of neighborhood effects began, scholars were concerned with the possibility that neighborhood effects can vary by an individual's type (Wright 1977; Huckfeldt 1979; Oliver and Mendelberg 2000). Their strategy, adding an interaction term between a neighborhood characteristic and some individual-level covariates of the researcher's choice, is not sufficient to identify the individual's type. In fact, as shown in Table A1, an interaction term between an individual's income and neighborhood average household income does not have much independent explanatory power. Also, creating the interaction term with a handmade index can hardly escape the charge 'lack of objectivity'.

This study addresses this challenge by using the propensity score technique. As the name suggests, the propensity score is the probability that a subject is assigned to the treatment group and is represented by the predicted value of the treatment variable. More importantly,

3. Carsey (1995) makes the same distinction for these two motives, but he also assumes homogenous neighborhood effects.

under the strong ignorability assumption,[4] observations that share the same propensity score can be considered comparable counterfactuals (Rosenbaum and Rubin 1983).[5] In other words, the propensity score enables us to represent an individual's attributes in a single dimension once we specify the treatment variable of interest. This study regards neighborhood average household income as a "continuous treatment variable" and estimates the propensity score by employing the "Generalization of Propensity Score" technique (Imai and van Dyk 2004), an extension of interval matching to continuous treatment variables.[6]

Most importantly, the propensity score framework enables us to regard *a neighborhood characteristic as the average type for those who live in neighborhoods with the same characteristic* without adding any further assumptions. The basic idea is outlined here. A neighborhood characteristic of an individual's living place is explained by the person's type and error. As long as the estimation of the propensity score satisfies the strong ignorability assumption, the error is independent and identically distributed. Therefore, the average of the error components among the residents living in the neighborhoods with the same treatment status will approach zero as the number of observations per treatment status increases. Even if the treatment status is different in each neighborhood, the propensity score estimation minimizes the error components.

The proximity of an individual's type to the neighbors', or the type proximity, is approximated by the distance between the value of a neighborhood characteristic and the individual's

4. Strong ignorability assumption is also referred to as: conditional independence assumption; absence of selection on unobservable; absence of omitted variable bias; absence of unmeasured confounding (Angrist and Pischke 2009).

5. Strong ignorability assumption is a strong assumption. However, almost all observational studies in political science implicitly adopt this assumption (Ho et al. 2007). In addition, no standard methodology is operative when the treatment effect is suspected to be heterogeneous, as is the case in this study (Imbens and Wooldridge 2009).

6. Under the propensity score framework, neighborhood level characteristics must satisfy the "Stable Unit Treatment Value Assumption," which states that the treatment status of one individual does not affect the outcome of another (Imai and van Dyk, 2004). Since one's individual-level characteristics constitute a subset of neighborhood characteristics, there exists a systematic correlation between these two variables. However, I argue such correlation is negligible because neighborhood effects are estimated at the tract-level, where several thousand people reside within each tract.

propensity score. For this purpose, the treatment variable, neighborhood average household income, is transformed into a zero-skewness logarithm so it follows the normal distribution.[7] If the distance is zero, an individual's type exactly corresponds with the neighbors', and that person will find many very similar people in the neighborhood. Thus, the model for the matched neighborhood theory takes the following form:

$$\begin{aligned} y_{ni} &= \alpha + \beta\widehat{\mathcal{N}}_{ni} + \gamma\mathcal{N}_n + \delta(\widehat{\mathcal{N}}_{ni} - \mathcal{N}_n)^2 + ctrl + \epsilon_{ni} \\ &= \alpha + \beta\theta_{ni} + \gamma\overline{\theta_n} + \delta(\theta_{ni} - \overline{\theta_n})^2 + ctrl + \epsilon_{ni} \end{aligned} \tag{3.3}$$

where $(\widehat{\mathcal{N}}_{ni} - \mathcal{N}_n)^2$ represents the key explanatory variable, the proximity of an individual's type to the average of her neighbors, and the second line rewrites the equation using θ_{ni}. If δ is statistically significant regardless of its sign, the matched neighborhood theory is supported. Furthermore, the positive coefficient provides a support for the Tiebout-Hirschman theory, since a matched neighborhood obviates the necessity of political participation. On the other hand, the negative coefficient indicates cross-pressure theory because a matched neighborhood facilitates political participation. In this model, the uni-directional model is explicitly captured by $\gamma\mathcal{N}_n$. Also, the bi-directional model can be thought of as a special case of the heterogeneous model, in which an individual's type plays no rule (i.e., $\widehat{\mathcal{N}}_{ni} = \theta_{ni} = 0$).

3.2.5 Data

This study uses two kinds of data to create the dataset. One is "Social Capital Community Benchmark Survey, 2000 (restricted use version)" (Saguaro Seminar 2000; SCCBS henceforth), which contains individual survey responses. These data are used to construct the dependent variables and individual-level control variables. The respondents were sampled

7. Henceforth, zero-skewness logged variables are simply referred to as logged variables.

from 51 Primary Sampling Units and interviewed by phone. The adjusted cooperation rate was 41.6%. The other data source is US census data from 1990 and 2000 found in "Geolytics Neighborhood Change Database (NCDB)," from which neighborhood-level characteristic variables are created. The two sets of data are matched at the tract level with a 94% success rate, and the total number of samples matched is 26,454.[8]

This study also uses a subsample with 17,994 observations that consisting of whites who had lived in the current neighborhood for at least one year. The subsample is constructed in this way because new residents need extra time to get accustomed to life in their neighborhood, and under the heterogeneous treatment assumption, people with different ethnic or racial backgrounds are likely to respond differently to a given neighborhood characteristic.

It is often the case that survey data suffer from a high attrition rate at the analysis stage due to missing responses, and SCCBS is no exception. About 26% of the observations have missing cases, which leaves 13,461 complete observations, but the analysis with complete cases could be biased. Therefore, multiple imputation was performed using the STATA add-on program,*ice* (Royston, 2005).[9][10]

8. Since matching with 6-digit codes performed poorly (less than 5% success), only the first 4 digits are used. Because the omitted last 2-digits are mainly used to adjust minor changes within/between tracts, this adjustment should not affect the results significantly.

9. Options were set as follows: Number of imputations = 10, Number of cycles = 50, sampling method = bootstrapping. This setting should be intensive enough to perform reliable analysis given the rate and extent of missing cases. None of the neighborhood-level covariates was included in MI since the data generation process is different. In other words, missing values were imputed solely from individual level covariates and outcome variables.

10. In the generation of contour plots such as Figure 1, multiple imputations were not available. Therefore, the subsample consisting of observations with no-missing cases and the imputed observations from the 10 imputed datasets was created. Then, the oversampling of imputed observations was adjusted by dividing the weight of imputed observations by 10.

3.3 Research Design

3.3.1 Dependent variables.

The dependent variables are collected so the analysis covers a wide range of political and social activities. Some of them are indices, and others are survey items. The following analysis mainly reports the results from four indices. Three of them are constructed by the investigators of SCCBS, because those researchers often have a better sense of which variables successfully represent what they are supposed to represent. Yet, neither statistical nor substantive findings change, regardless of which indices are used.

`Political Participation:` a 10-point scale created from the summation of 10 dummy variables: political knowledge,[11] political interest, attended a public meeting, attending a political meeting, member of an ethnic/civil rights group, member of a neighborhood association, member of a political group, member of a local reform group, participating in a march, and signing a petition.

`Electoral Politics:` the first principal factor score of the following variables: voting in the 2000 Presidential election, registered voter, political interest, political knowledge, and reading a newspaper (SCCBS preset variable).

`Organizational Activism:` the first principal factor score of the following variables: number of group involvements, frequency of attending club meetings, frequency of attending public meetings, and serving as an officer or on a committee (SCCBS preset variable).

`Protest Index:` the arithmetic mean of the following variables: participating in a march, participating in a political group, attending a political meeting, member of a local

11. Political knowledge and political interests are originally 4-scale variables, but reduced to binary variables in creating this index.

reform group, member of an ethnic/civil rights group, signing a petition, and member of a labor union (SCCBS preset variable)

3.3.2 Neighborhood-level variables.

The primary neighborhood characteristic of interest is the average household income, but in order to control for the effect of other neighborhood characteristics, the following three neighborhood-level characteristics are included as control variables in both the propensity score estimation and the regressions. All neighborhood level variables are calculated at census tract-level and are transformed into zero-skewness logarithms. Also, all neighborhood level variables are calculated as the weighted average of the 1990 and 2000 census so they represent the average characteristics in the previous five years (1996–2000).

Average Household Income: treatment variable, the tract-average of the household income.

% White Population: control variable, the proportion of white population to total population within a tract.

Income Heterogeneity: control variable, heterogeneity of income distribution calculated as the index of qualitative variation (IQV)[12], and

Population Density: the number of people living within a tract divided by the area of the tract.

12. IQV is the measurement of heterogeneity, which is formally defined as follows. $IQV = \frac{K}{K-1}\sum_{i=1}^{K} p_i^2$ *where*
$K \equiv$ Number of categories
$p_i \equiv$ Proportion of observations that fall into a category i
In calculating IQV for income, eight income categories are used.

3.3.3 Control variables.

The following control variables are used in estimating the propensity score. They are selected in such a way that they are not the consequence of the treatment variable to avoid post-treatment bias (Ho et al. 2007). Also, the ordinal and continuous variables are transformed into zero-skewness logarithms.

Dummy Variables: `female`, `Spanish speaker`, `Protestants`, `Catholics`, `other religions` (baseline = no religion), `married`, `divorced or separated`, `widowed` (baseline = never married), `homeowner`, `Internet access`.

Ordinal/Continuous Variables: `age`, `years of education`, `individual household income`, `length of time living at a same address`, `health status`, `number of times called for interview`, `political ideology`, `strong political ideology`, `working time per week`, `number of friends`, `ethnic diversity of friends`.

3.3.4 Propensity score estimation.

An individual's type is estimated as follows. First, individual-level covariates are transformed into the same number of orthogonal principal component factors.[13] Then, these factors and neighborhood-level control variables are entered into the right-hand side of the following non-parametric model to estimate the propensity score.

$$\mathcal{N}_n = \sum_{j=1}^{19} f_j(z_{j_{ni}}) + \sum_{k=1}^{3} g_k(m_{k_n}) + \epsilon_{ni} \tag{3.4}$$

where $f_j(\bullet)$ and $g_k(\bullet)$ are smooth functions, $z_{j_{ni}}$ is one of the principal component factors,

13. This procedure is for purely technical purposes. Because dummy variables do not have enough variation, they cannot be entered to the non-parametric estimation unless they are transformed into continuous orthogonal variables with multiple values.

and m_{k_n} is one of the neighborhood-level control variables. These smooth functions are estimated with the Stata add-on program, `GAM` (Royston and Ambler 2002).[14] The results of this procedure are described in Appendix B.

3.4 Results

3.4.1 Graphical evidence.

The matched neighborhood theory concerns the distance between logged average household income and its predicted value, or the propensity score. This distance represents the type proximity or, the key explanatory variable. Regression analysis with the control variables provides further detail of the results such as the level of statistical significance, but the contour plots in Figures 1 and 2 are most eloquent of the main story. These contour plots are estimated with the following non-parametric model.

$$y_{ni} = h(\widehat{\mathcal{N}}_{ni}, \mathcal{N}_n) + \epsilon_{ni} \tag{3.5}$$

where y_{ni} is the level of political participation, $\mathcal{N}_n$ is the logged average household income, and $\widehat{\mathcal{N}}_{ni}$ is the propensity score, or an individual's type. The smooth function, $h(\bullet)$, is estimated so that it fits the joint distribution of $\widehat{\mathcal{N}}_{ni}$ and $\mathcal{N}_n$, using the R-package, `mgcv` (Wood 2006). This simple specification is sufficient to test the matched neighborhood theory because this smooth function captures all meaningful interaction terms between these two variables, if such interactions exist. The evidence of the matched neighborhood theory is observed as *a valley or a mountain along the 45-degree line* in the contour plot, along which the value of the logged average household income is equal to the propensity score. That is, an individual's type perfectly matches her neighbors'.

14. See Beck and Jackman (1998) for the introduction of the generalized additive model.

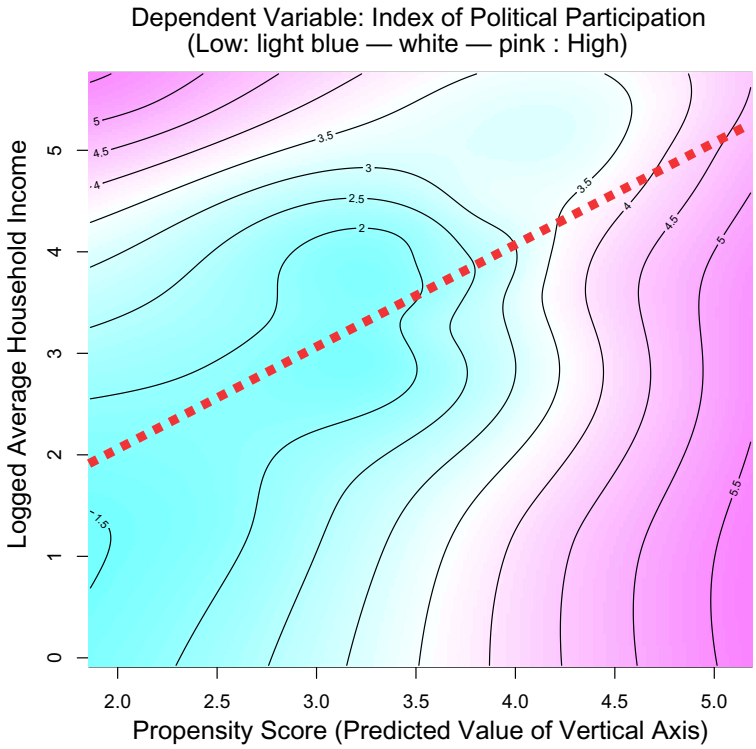

Figure 3.1: The level of political participation as a smooth function of logged average household income and its propensity score

Figure 1 shows how the levels of `Political Participation` (on the collapsed height dimension) vary at different logged average household incomes (on the y axis) and their propensity score, or the individual's type (on the x axis). Any point on the same contour represents an equal level of political participation, with the level indicated by the small letters in each contour. For example, an individual whose logged average household income and its propensity score are both 4 participated in 3 kinds of political activities out of 10. Also, in the figure, the areas in pink (or the dark areas in the top left and in the right) represent high levels of political participation and those in light blue (or the pale areas from the center to the bottom left) represent low levels.

Note that the level of political participation is low along the 45-degree line indicated by the red dotted line. This is consistent with the matched neighborhood theory, particularly

the Tiebout-Hirschman theory. Along the dotted line, the value of logged average household income and its predicted value, or the propensity score, correspond. That is, people are living in a perfectly matched neighborhood, where they can find many like-minded people. The figure shows that when people live in such a neighborhood, they participate less in politics.

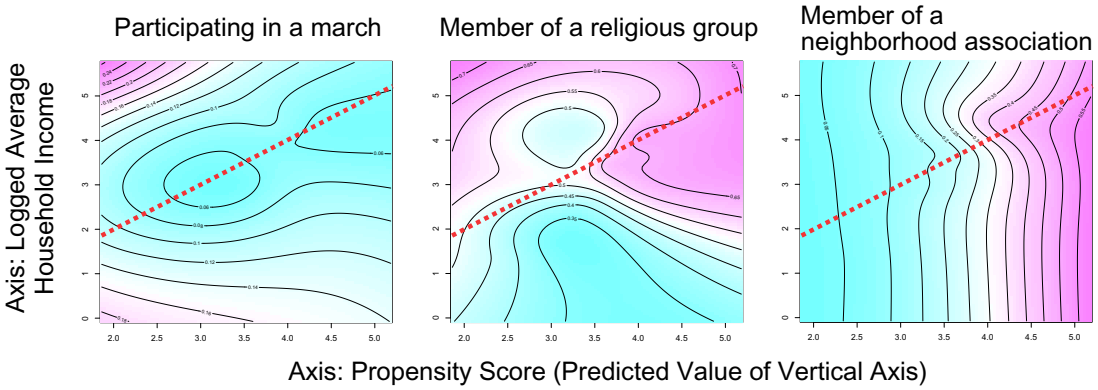

Figure 3.2: Probabilities of varying types of political participation as a smooth function of logged average household income and its propensity score

The diagonal valley in Figure 1 is neither a universal nor incidental observation. Rather, the pattern of a contour plot is closely associated with the nature of each political activity. The substantive meaning of these patterns will be discussed later with Figure 3, but the three contour plots in Figure 2, created from three different political activities, show distinct patterns.

The left panel uses `participation in a march` as a dependent variable. Although the figure shows a diagonal valley along the 45-degree line, the propensity score in the horizontal dimension does not increase the probability of participating in a march. The center panel uses `membership of a religious group` such as a church or a synagogue. People are more likely to be a member of a religious group as logged average household income increases. However, once this uni-directional effect is taken into account, the figure reveals another pattern, which is the diagonal mountain along the 45-degree line. Finally, the right panel shows the result of the `membership of a neighborhood association`. Several vertical

contours indicate that the membership of a neighborhood association is solely determined by one's propensity score, or an individual's type.

3.4.2 Statistical evidence.

Statistical properties of the previous figures are examined with the following regression model.

$$y_{ni} = \alpha + \beta\widehat{\mathcal{N}}_{ni} + \gamma\mathcal{N}_n + \delta|\widehat{\mathcal{N}}_{ni} - \mathcal{N}_n| + \sum_{k=1}^{3} \lambda_k m_{k_n} + \epsilon_{ni} \tag{3.6}$$

where m_{k_n} is one of the neighborhood-level control variables. $|\widehat{\mathcal{N}}_{ni} - \mathcal{N}_n|$ represents the key explanatory variable, the proximity of an individual's type to her neighbors'. This type of proximity is measured as the absolute value of the difference between the logged average household income and its propensity score instead of the quadratic term in Equation (3), but they are substantively equivalent and have the same level of statistical significance. The absolute term is preferred to the quadratic term simply because the matched neighborhood theory does not say anything about whether the marginal effect of the type proximity is increasing.

This estimation model is parsimonious but is sufficient to estimate the parameters under the strong ignorability assumption. In a linear model with balanced data, adding a propensity score is equivalent to adding a whole set of covariates to control for the confounding factors of β. Also, the proximity does not require any control variable because it is equal to the absolute value of the error term in the propensity score estimation with Equation (4). In any case, the statistical significance in Table 1 does not change even when all control variables are used instead of the propensity score. A small number of regressors that are powerful, but have distinct meanings, often make the interpretation of the regression easy and sensible (Achen 2002).

Dependent Variable	Index of Political Participation				
Explanatory Variables	(1)	(2)	(3)	(4)	(5)
Logged Ave.Household Income		0.693 ***		0.069	0.066
		(0.079)		(0.094)	(0.095)
Propensity Score (PS)			1.752 ***	1.682 ***	1.783 ***
			(0.102)	(0.112)	(0.108)
| Logged Ave.HH Inc. - PS |	**0.468** ***	**0.506** ***	**0.373** ***	**0.381** ***	**0.382** ***
	(0.097)	**(0.146)**	**(0.089)**	**(0.089)**	**(0.098)**
Logged % White Population					−0.143 ***
					(0.038)
Logged Income Heterogeneity					0.290 ***
					(0.057)
Logged Population Density					0.080 ***
					(0.032)
Constant	2.472 ***	−0.107	−3.992 ***	−3.991 ***	−4.963 ***
	(0.071)	(0.314)	(0.383)	(0.385)	(0.364)
Number of Observations	17,994	17,994	17,994	17,994	17,994

Note: Coefficients are estimated with multiple imputation. Sampling-site clustered standard errors are in parentheses. Quantities of interest are indicated in **Boldface**.
* $p<.10$, ** $p<.05$, *** $p<.01$ in two-tailed test.

Table 3.1: The effect of type proximity to neighbors on political participation

As indicated by the clear pattern in Figure 1, the effect of the type proximity has an independent explanatory power in regression. Table 1 presents the results of the effect of the type proximity on the index of political participation (`Political Participation`). The coefficients of the type proximity consistently show positive effects on political participation (i.e. its effects are not so attenuated by the inclusion of the propensity score and other neighborhood variables.) According to the estimates of Model (5) in Table 1, one standard deviation change in the type proximity increases the level of political participation by 0.9%.

The coefficients of the type proximity for the other indices of political participation are also statistically positively significant and robust to the inclusion of the control variables. In Table 2, the variable of `Electoral Politics` shows slightly weaker coefficients. This is

Explanatory Variable	| Logged Ave.HH Inc. - PS |				
Dependent Variables	(1)	(2)	(3)	(4)	(5)
Electoral Politics	0.087 **	0.103 **	0.048	0.053	0.079 **
	(0.043)	(0.045)	(0.041)	(0.037)	(0.038)
Organizational Activism	0.126 ***	0.136 ***	0.097 ***	0.094 ***	0.132 ***
	(0.028)	(0.046)	(0.030)	(0.030)	(0.035)
Protest Index	0.281 ***	0.295 ***	0.243 ***	0.241 ***	0.226 ***
	(0.065)	(0.080)	(0.063)	(0.065)	(0.066)
Control Variables					
Logged Ave.Household Income	No	Yes	No	Yes	Yes
Propensity Score (PS)	No	No	Yes	Yes	Yes
Other Neighborhood Ctrl Vars.	No	No	No	No	Yes
Number of Observations	17,994	17,994	17,994	17,994	17,994

Note: Coefficients are estimated with multiple imputation. Sampling-site clustered standard errors are in parentheses. * $p<.10$, ** $p<.05$, *** $p<.01$ in two-tailed test.

Table 3.2: The effect of the type proximity to the neighbors on other indices of political participation

probably because this index includes variables that are less associated with neighborhood environments such as voting in a Presidential election or voter registration. These positive coefficients substantively imply that having like-minded neighbors discourages political participation and support the Tiebout-Hirschman theory in political participation, at least when neighborhood affluence is concerned.

Note that the independent effects of the type proximity variable are not statistical artifacts. Surely, the error term in the propensity score estimation would have an effect independent of the propensity score in the outcome equation if included. This *is* a statistical artifact because the error is what is not explained by the propensity score. However, the type proximity is *an absolute value of the error* and is independent of the error by construction. Thus, this variable cannot have an independent effect unless it really captures substantively unexplained variation.

The comparison of the effect of the type proximity among various political activities

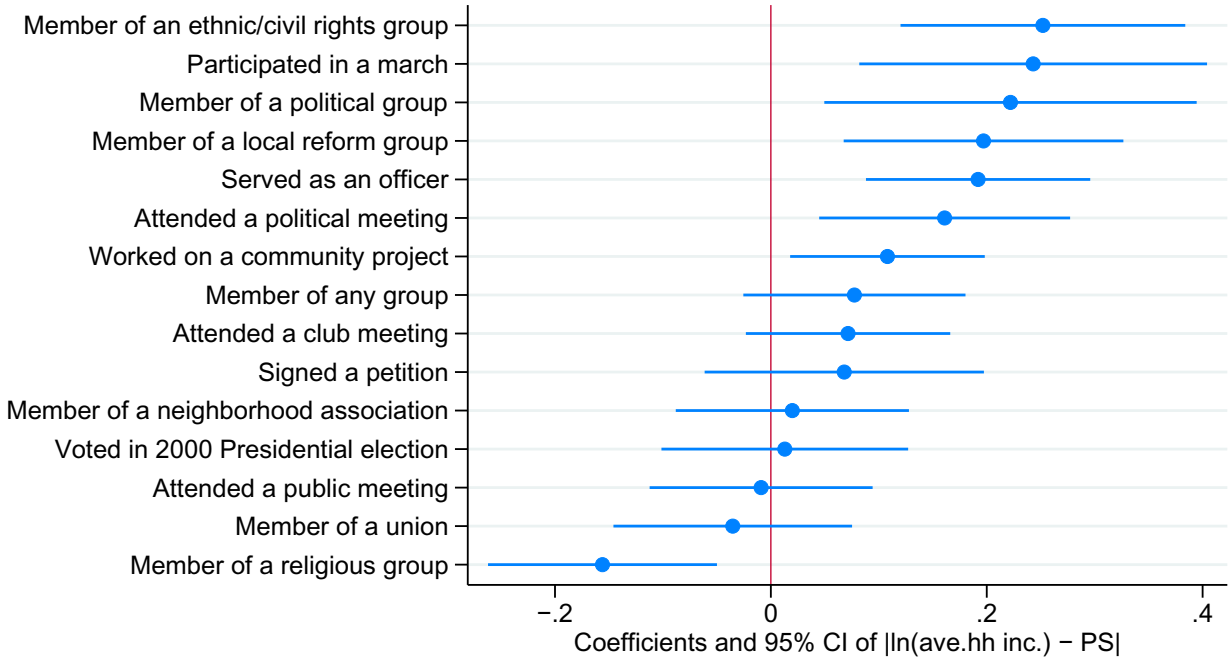

Figure 3.3: Comparison of the effects of the type proximity among various political activities

also supports the Tiebout-Hirschman view of political participation. Figure 3 shows the coefficients of the type proximity and their 95% confidence intervals for 15 different political activities. These dependent variables are all binary, and the coefficients are estimated with probit regression. Otherwise, the models are the same as Model (5) in Table 1.

Figure 3 shows that a group of the activities in the top as well as the bottom share some commonalities. In the top of the figure, the type proximity has a strong positive effect on variables such as membership in an ethnic/civil rights group, political group, and a local reform group; participation in a march, and serving as an officer. These activities are voluntary, and often require a strong motive for changing the outcomes. Positive coefficients on these variables indicate that people do not commit to these activities when their goals have already been achieved or when someone else is likely to represent their objectives.

On the other hand, the type proximity has a negative effect on the membership of a religious group. That is, people are more likely to be the member of a religious group when their types are close to their neighbors'. People join a religious group to enjoy themselves,

not to change a social outcome. Worship is more fruitful with those who share the same religious beliefs, and it would be easier for them to find their place of worship in a matched neighborhood. Finally, the type proximity has neither a positive nor a negative effect on signing a petition, voting in a Presidential election, the membership of a union or a neighborhood association, and attending a public meeting. These activities are often characterized as passive, non-local, non-political, and mandatory.

3.5 Conclusion

The study proposed the matched neighborhood theory and presented both graphical and statistical evidence to support the theory. Specifically, the analysis supports the Tiebout-Hirschman theory's view of political participation based on neighborhood income level. That is, when people can find many neighbors like themselves, it deters political participation. This simple account has not been explored previously because of the methodological difficulty in identifying an individual's type. This study addresses that challenge by applying the propensity score technique and using a neighborhood characteristic as the average type of the residents.

One caveat to the conclusion of this study is that the study does not identify causal mechanisms. However, the key insights from the matched neighborhood theory hold true regardless of the direction of causality. For example, some might think that those who vigorously participate in political activities, when compared to those who participate less, do not care whether their neighbors are like themselves. This explanation sounds like an alternative account for the empirical observation. Actually, this is just another way of saying that those who do not want to participate in politics care about the proximity of their type to their neighbors', which is consistent with Tiebout's (1956) concept of "vote with feet."

Finally, this study reveals what neighborhood income level might represent. Conventionally, it has been thought that neighborhood income level represents the degree of universal

virtue or vice a neighborhood possesses. However, this study shows that neighborhood income level also represents the average type of neighbors, and that the same neighborhood environment can be perceived differently depending on the person.

3.6 Appendix A: Estimation of Propensity Score

GAM is used in propensity score estimation to remove all systematic trends in the distribution of predicted values. Although strong ignorability assumption can never be explicitly tested, Figure A1 shows that GAM does a good job in producing non-systematic predictions.

3.A Appendix Tables and Figures

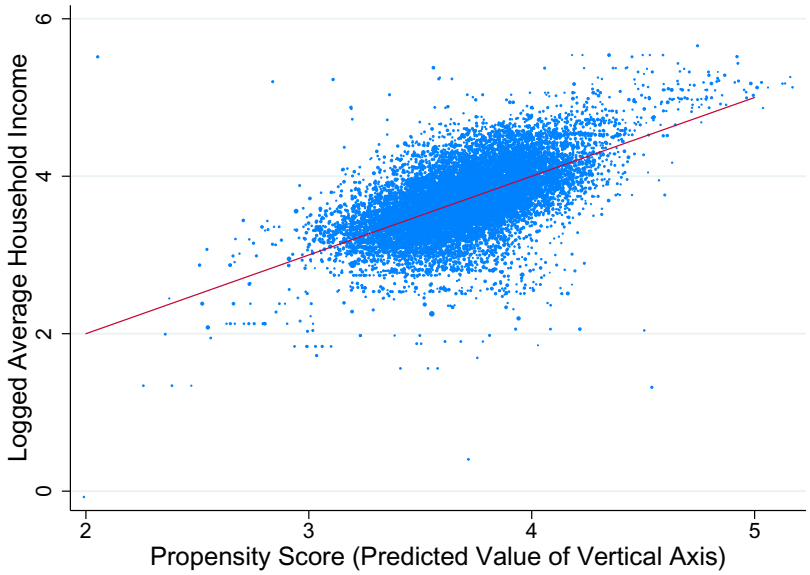

Figure 3.A.1: The scatter plot of logged average household income against its predicted value (propensity score) *Note*: The imputed observations are not included.

Dependent Variable	Index of Political Participation				
Explanatory Variables	(1)	(2)	(3)	(4)	(5)
Logged Ave.Household Income		−4.205 ***		−0.350	−0.629
		(0.225)		(2.195)	(2.021)
Logged Household Income			1.380 ***	1.269 *	1.136 *
			(0.063)	(0.710)	(0.657)
Logged Ave.Household Income	**0.068** ***	**0.394** ***	**0.030** ***	**0.060**	**0.085**
× Logged Household Income	**(0.006)**	**(0.018)**	**(0.006)**	**(0.191)**	**(0.177)**
Logged % White Population					−0.074 *
					(0.041)
Logged Income Heterogeneity					0.170 ***
					(0.042)
Logged Population Density					0.124 ***
					(0.029)
Constant	−0.315	1.323 ***	−14.59 ***	−13.30	−12.24
	(0.273)	(0.272)	(0.655)	(8.106)	(7.505)
Number of Observations	18,086	18,086	18,086	18,086	18,086

Note: Coefficients are estimated with multiple imputation. Sampling-site clustered standard errors are in parentheses. Quantities of interest are indicated in **Boldface**.
* $p<.10$, ** $p<.05$, *** $p<.01$ in two-tailed test.

Table 3.A.1: The effect of the interaction term between personal household income and neighborhood average household income on political participation

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