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## THREE ESSAYS ON GOVERNANCE, INEQUALITY, AND SOCIAL EQUITY

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Sarah Ausmus Smith, Student

Dr. Edward T. Jennings, Major Professor

Dr. Rajeev Darolia, Director of Graduate Studies

THREE ESSAYS ON GOVERNANCE, INEQUALITY, AND SOCIAL EQUITY

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DISSERTATION

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A dissertation submitted in partial fulfillment of the  
requirements for the degree of Doctor of Philosophy in the  
Graduate School at the University of Kentucky

By

Sarah Ausmus Smith

Lexington, Kentucky

Director: Dr. Edward T. Jennings, Professor of Public Policy and Administration

Lexington, Kentucky

2022

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## ABSTRACT OF DISSERTATION

### THREE ESSAYS ON GOVERNANCE, INEQUALITY, AND SOCIAL EQUITY

Comprised of three essays, my dissertation is linked by a common focus: the relationship between state or local governance arrangements and inequality or facets of social equity. I draw upon a range of literatures to motivate my research questions and inform my methodologies—welfare and social policy, public economics, intergovernmental relations, public finance and management.

In the first essay, I ask: does localizing welfare governance impact geospatial access to the social safety net? This is an important question because proximity is highly salient to program utilization. I geocode the location of human services nonprofits from tax filings in eight states using ArcGIS and create measures of access for low-income neighborhoods over 17 years. I leverage the 1996 welfare reform, which enabled states to devolve more policymaking discretion to local governments, to examine the responsiveness of nonprofits to changes in welfare governance with respect to geospatial accessibility. One of my main findings is that low-income neighborhoods in states that chose to localize welfare had less access post-reform to program revenues, a proxy for government contracts and services.

In the second essay, I study the relationship between state government wages and privatization. Governments have used public sector employment to support a variety of goals, including social equity and economic development, but privatization, as an NPM reform, may shift that focus. My empirical analysis shows that state privatization of service delivery is associated with decreases in the public sector wage premium, but that these effects are not driven by gender, race, or low levels of educational attainment. The quality of implementation conditions these effects. I also find that privatization is associated with a lower public sector wage premium for middle-class workers.

In the third essay, I and a co-author leverage a 2003 Arkansas state law requiring school district reorganization via an enrollment cutoff to evaluate the effects of consolidations on rural communities' population, number of schools, and property values using a propensity score matched difference-in-differences design. We estimate that the reform led to reductions in population, community schools, and property value assessments. We also find that communities with greater shares of racial minorities may have been disproportionately affected with respect to population loss.

KEYWORDS: [governance, decentralization, social policy, social equity, public management, privatization]

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THREE ESSAYS ON GOVERNANCE, INEQUALITY, AND SOCIAL EQUITY

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Date

## DEDICATION

To Nathan—I always choose you. And to my family, who have always believed in me.

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This work reflects the support of a whole community of colleagues, friends, and family. I am deeply grateful to my advisor, Dr. Ed Jennings, for his guidance and wisdom over the past few years. This work is many times better due to his encouragement and thoughtful advice. I also thank the members of my dissertation committee, all of whom have also provided mentorship and expertise: Drs. Annelise Russell, Richard Waterman, and Rajeev Darolia. I have learned so much from each of you, both in and out of the classroom. To Drs. Eugenia Toma and J.S. Butler—you’ve not only taught me exceptionally well, you have also always been in my corner. I never would have pursued this degree if it were not for the wider Martin School faculty I encountered in the MPA program; your passion is truly infectious. In particular, I thank Dr. Ron Zimmer for his patience and kindness—I learned so much from co-authoring with you. Lastly, I cannot overstate the positive impact of my classmates, both at the Martin School and in the broader community of scholars. For every draft read, coffee break, sympathetic word, advice given, laugh shared, and listening ear—thank you, thank you, thank you. Your friendship has buoyed me through challenging moments, particularly as the pandemic upended all our lives.

Lastly, some data provided by Zillow through the Zillow Transaction and Assessment Dataset (ZTRAX). More information on accessing the data can be found at <http://www.zillow.com/ztrax>. The results and opinions are those of the author(s) and do not reflect the position of Zillow Group.



## TABLE OF CONTENTS

ACKNOWLEDGMENTS .....	iii
LIST OF TABLES .....	vi
LIST OF FIGURES .....	vii
CHAPTER 1. INTRODUCTION .....	1
CHAPTER 2. THE DEVOLVED SOCIAL SAFETY NET: IMPLICATIONS FOR ACCESS? .....	3
2.1 Introduction .....	3
2.2 Background: TANF Implementation .....	5
2.2.1 Impacts of State versus Local Administration .....	7
2.3 A Theory of Nonprofits, Spatial Access and Welfare Reform .....	10
2.4 Methodology and Data .....	14
2.4.1 Case Selection .....	14
2.4.2 Measurement and Estimation .....	17
2.5 Results .....	24
2.6 Discussion and Conclusion .....	32
CHAPTER 3. DOES U.S. STATE GOVERNMENT PRIVATIZATION INCREASE SOCIOECONOMIC INEQUALITY? .....	35
3.1 Introduction .....	35
3.2 Background: State and Local Privatization .....	36
3.3 Government Reform and Inequality: Theory .....	37
3.4 Methodology and Data .....	41
3.4.1 Measurement: State Privatization .....	42
3.4.2 Measurement: Wages and Inequality .....	47
3.5 Results .....	50
3.6 Discussion and Conclusion .....	57
CHAPTER 4. THE IMPACTS OF SCHOOL DISTRICT CONSOLIDATION ON RURAL COMMUNITIES: EVIDENCE FROM ARKANSAS REFORM .....	59
4.1 Introduction .....	59
4.2 The Ongoing Decentralization Debate .....	60
4.3 Theoretical Framework .....	63
4.4 Policy Background: Education Reform in Arkansas .....	65
4.5 Data Collection .....	66
4.6 Research Design .....	72

4.7	<i>Results.....</i>	74
4.8	<i>Discussion and Conclusion .....</i>	84
APPENDICES .....		86
<i>APPENDIX 1. THE DEVOLVED SOCIAL SAFETY NET: IMPLICATIONS FOR ACCESS? .....</i>		87
<i>APPENDIX 2. DOES STATE GOVERNMENT PRIVATIZATION INCREASE SOCIOECONOMIC INEQUALITY? .....</i>		94
<i>APPENDIX 3. THE IMPACTS OF SCHOOL DISTRICT CONSOLIDATION ON RURAL COMMUNITIES .....</i>		97
REFERENCES .....		98
VITA.....		107

## LIST OF TABLES

Table 2.1 Descriptive Statistics By Treatment Status in 1990.....	15
Table 2.2 Welfare Governance by Treatment Status .....	16
Table 2.3 County-Level Measures of Geospatial Access for Low Income Neighborhoods .....	21
Table 2.4 Governance Structure .....	22
Table 2.5 Covariates: Environment, Clients, Policy Treatment .....	23
Table 2.6 Welfare Governance & Geospatial Accessibility to the Social Safety Net for Low-Income Neighborhoods .....	25
Table 2.7 Welfare Governance & Geospatial Accessibility to the Social Safety Net for Low-Income Neighborhoods: Tract-Level Analysis .....	27
Table 2.8 Welfare Governance, Political Ideology, & Geospatial Access to the Social Safety Net.....	29
Table 2.9 Welfare Governance & Racial Gaps in Geospatial Access to the Social Safety Net: County-Level Analysis .....	31
Table 2.10 Welfare Governance & Geospatial Access for the Black Population: Tract-Level Analysis .....	32
Table 2.11 Summary of Main Findings .....	33
Table 3.1 Privatization Implementation Quality Descriptive Statistics .....	46
Table 3.2 Privatization Implementation Quality Factor Analysis .....	46
Table 3.3 Inequality by Occupational Class .....	49
Table 3.4 State Privatization and Public Sector Wages .....	50
Table 3.5 Moderating Effects of Privatization Implementation Quality on Wages .....	52
Table 3.6 State Privatization, Public Wages for Racial and Ethnic Minority Workers....	54
Table 3.7 State Privatization and Public Wages for Female Workers .....	55
Table 3.8 State Privatization, Public Wages for Workers Without Higher Education .....	56
Table 3.9 State Privatization, Public Wages, and Class .....	57
Table 4.1 Treatment & Control Places Weighted Descriptive Statistics, 2000 .....	69
Table 4.2 Treatment & Control Property Value Assessments, Pre-Treatment.....	71
Table 4.3 The Effects of School District Reorganization on Community Population.....	74
Table 4.4 The Racially-Disparate Effects of School District Reorganization on Community Population .....	77
Table 4.5 More to Lose? Population Analysis of Towns with Schools.....	78
Table 4.6 The Effects of School District Reorganization on Community Schools .....	79
Table 4.7 The Effects of School District Reorganization on Assessed Value.....	82
Table 5.1 Data Sources for Chapter 2 Analyses .....	87
Table 5.2 Summary Statistics .....	89
Table 5.3 Welfare Governance & Geospatial Accessibility to the Social Safety Net for Low-Income Neighborhoods: Tract-Level Analysis .....	91
Table 5.4 Data Sources for Chapter 3 Analyses .....	94
Table 5.5 Data Sources for Chapter 4 Analyses .....	97

## LIST OF FIGURES

Figure 2.1 Geospatial Access to HSOs by Neighborhood Income .....	19
Figure 2.2 Geospatial Access to HSO Revenue by Neighborhood Income .....	19
Figure 2.3 Geospatial Access to HSO Program Revenue by Neighborhood Income.....	20
Figure 2.4 Geospatial Access by Discretion Type.....	28
Figure 2.5 Marginal Effects of Ideology, by Reform & Time on Program Revenues .....	30
Figure 2.6 Marginal Effects of Ideology, by Reform & Time on HSO Counts .....	30
Figure 3.1 Reported State Agency Privatization Over Time, By Agency Function .....	43
Figure 3.2 Average Levels of Reported State Contracts.....	44
Figure 3.3 State Privatization Over Time .....	45
Figure 3.4 Budget-Weighted Implementation Quality Index Over Time .....	47
Figure 3.5 Wage Inequality by Sector .....	49
Figure 3.6 Conditioning Effects of Implementation Quality on State Privatization.....	54
Figure 4.1 Map of Municipalities, Treatment School Districts, and Pool of Potential Control Districts.....	68
Figure 4.2 Racial Composition Over Time, By Treatment Status .....	70
Figure 4.3 Distribution of Racial Composition by Treatment Status, Year 2000.....	70
Figure 4.4 Pre-Treatment Population Means .....	73
Figure 4.5 Population Event Study Using Matching (NN=2, Population Count) .....	76
Figure 4.6 Population Event Study Using Matching (NN=2, Population Count and Nonwhite Share) .....	76
Figure 4.7 Number of Schools Event Study (NN=2, Population Count).....	81
Figure 4.8 Number of Schools Event Study (NN=2, Population Count and Nonwhite Share) .....	81
Figure 4.9 Property Assessments Event Study (NN=1).....	84
Figure 4.10 Disaggregated Privatization Measures .....	96

## CHAPTER 1. INTRODUCTION

While each essay in this dissertation focuses on a distinct policy area for state and local governments, i.e. welfare reform, state government privatization, and school districts, they are linked by underlying themes—new public management reforms, social equity and inequality, and the relationship between governance arrangements and the communities they serve. My analyses explore potential tradeoffs in the expression of local preferences and equity for marginalized groups. My dissertation supports the assertion that *how* governments choose to implement policies matters.

In Chapters 2 and 3, I study the devolution of welfare policymaking authority to local governments and state government privatization, respectively. The “devolution revolution,” whereby policymaking and implementation are decentralized, and the introduction of market mechanisms, including privatization, are hallmarks of New Public Management. The NPM movement has been international in scope, but took off in the 1990s in the United States (Thompson and Riccucci 1998, Alonso et al. 2015). The Clinton administration brought many of these reforms to the fore, including through welfare reform in 1996.

In Chapter 2, I use federal welfare reform to examine the responsiveness of nonprofits to the decentralization of policymaking, particularly the geographic accessibility of human service organizations relative to the neighborhoods that most need them. My results indicate that low-income neighborhoods under localized welfare governance arrangements have less geospatial access to human service organizations, but that this effect is attenuated by political ideology. I also find that, contrary to what we might expect from the literature, devolving policymaking authority is not associated with increased racial gaps in geospatial access to nonprofits.

Chapter 3 considers the relationship between state government privatization and public sector wages. Privatization of state government services is commonplace, but our understanding of its effects is limited by data availability. I study the relationship between state contracting and public sector wages. Governments have used public sector employment to support a variety of goals, including social equity and economic

development, but privatization, as a NPM reform in the United States, may shift the focus towards efficiency. My empirical analysis shows that state privatization of service delivery is associated with decreases in the public sector wage premium, but that these effects are not driven by gender, race, or low-levels of educational attainment. The quality of implementation conditions these effects. I also find that privatization is associated with a lower public sector wage premium for middle-class workers.

The tenets of NPM may produce initiatives that appear in conflict; while welfare reform sought to decentralize discretion, the state of Arkansas sought to do the opposite in the name of *increasing* efficiency in the provision of public education. In Chapter 4, I and a co-author study the effects school district consolidations on rural Arkansan communities, namely community population, schools, and property value assessments. These reforms are commonplace in the United States; the number of school districts in the last fifty years has drastically declined. As a result of a 2003 special legislative session, the Arkansas state government mandated reorganization for districts falling below a given enrollment. We estimate that this reform led to reductions in population counts, community schools, and property value assessments for towns in the treated districts.

My dissertation shows that the relationship between social equity, inequality, and NPM reforms is nuanced. In Chapter 2, I find that low-income neighborhoods in localized welfare governance arrangements have less access to nonprofits after welfare reform. In Chapter 3, I show that privatization is associated with a decreasing wage premium for state government employees, particularly for middle-class occupations. I do not find disparate impacts on the basis of race in either essay, which stands in contrast with my findings in Chapter 4. School district consolidation in Arkansas led to losses in community population, schools, and home values; communities with increasing shares of nonwhite populations were disproportionately affected with respect to community population.

## CHAPTER 2. THE DEVOLVED SOCIAL SAFETY NET: IMPLICATIONS FOR ACCESS?

### 2.1 Introduction

Where you live shapes the access you have to the social safety net. While much has been written about the impacts of devolving authority over anti-poverty programs to the states on access to welfare benefits and caseloads, less is understood about its impacts on access to social services. The individual needing support is faced with navigating a system that is geographically heterogeneous in its offerings as state and local governments have greater degrees of discretion in its design and implementation, but is also a fragmented network of public and private sources of relief as governments increasingly rely on contracts and partnerships for service delivery (Fording et al. 2007, Allard 2009). Given this importance of place, I ask: does localizing welfare policymaking (i.e. second-order devolution) influence geospatial access to the nonprofit social safety net? Geographic access to social services is especially important today, as anti-poverty programs rely increasingly on services over cash assistance and prioritize work and self-sufficiency. I leverage the 1996 federal welfare reform, which some states used to further decentralize welfare policymaking, to assess the impacts of increasing local government authority on the geographic accessibility of human service organizations relative to the neighborhoods that most need them. My results indicate that low-income neighborhoods under localized welfare governance arrangements have less geospatial access to human service organizations, but that this effect is attenuated by political ideology. I also find that, contrary to what we might expect from the literature, devolving policymaking authority is not associated with increased racial gaps.

This is an important question, because distance from service providers is not simply a minor logistical problem for the poor. In his book “Out of Reach,” Allard (2009) argues access to social services is not equitable, not just on the basis of benefit levels or program eligibility across jurisdictional boundaries, but also geospatially as our society relies more and more on service-based supports (such as employment training) over cash assistance. Allard surveys nonprofit human service providers in three urban settings (Los Angeles, Chicago, and the District of Columbia) and finds that almost two-thirds of service

providers were located in neighborhoods with low or moderate levels of poverty, and “living in neighborhoods highly segregated by race—often high-poverty neighborhoods—significantly diminishes access to the safety net” (77). Locational decisions for the nonprofits in his study are shaped by the need to generate revenue, suitable space availability, program-specific needs, and affordability constraints. Two-thirds of the agencies’ caseloads were reportedly within a three-mile radius, indicating that geospatial distance is an important facet of access. He concludes that, in a system where NPOs or private agencies bid for contracts from governments, no one is directly concerned with spatial mismatches in access—policymakers concentrate on funding, policy design, and eligibility for siloed programs but may not consider this big picture. However, others have found that nonprofits are more spatially responsive to high levels of poverty in other cities (Peck 2008).

Geographic inaccessibility introduces administrative burdens to utilization via transportation costs—target populations may face barriers in finding the time and resources to travel to a provider, and via learning costs—a resident is more likely to know a program is available if it is nearby. Reliable transportation, such as a car, is positively associated with welfare recipients finding and maintaining gainful employment (Ong 2002, Gurley and Bruce 2005). Increasing travel distance from the closest human services administrative office has a negative impact on the probability a family receives a child care subsidy (Herbst and Tekin 2012). In the case of WIC take-up, pregnant women are more likely to sign up if they live near a WIC clinic, but also if that clinic is part of non-health department facilities—these might have greater visibility for their target audience (Rossin-Slater 2013). Furthermore, proximity was also associated with better health outcomes for mom and baby. Proximity to mental health and substance abuse treatment had a positive relationship with utilization for women receiving welfare in Detroit (Allard et al. 2003).

However, the connection between public funding, governance, and geospatial access to social services has not been fully explored. For example, Marwell and Gullickson (2013) ask the question: is there spatial match between need and distribution of state and local government dollars? They examine contracts with nonprofits in New



York City and find there is a “weak spatial match” for services to some targeted populations in need, and that the likelihood of funded organizations in poor neighborhoods is heterogeneous by type. This paper seeks to further explore the relationship between geospatial access to human service organizations by considering the influence state and local government choices in policy design and implementation may have on access.

## 2.2 Background: TANF Implementation

The provision of the social safety net has undergone several transformations in the United States over the past century, as we have oscillated between local and federal provision (Allard 2009). While the states pioneered new policies to support the poor in the early 20th century, such as mothers’ aid programs, the federal government took unprecedented steps to combat poverty during the Great Depression and in the post-WWII era, implementing the New Deal and War on Poverty entitlement programs (Franko and Witko 2018). Funding continued to grow at all levels of government through the 1970s and 1980s, but there was also a shift from the provision of cash assistance to the promotion of economic self-sufficiency which culminated in the passage of the Personal Responsibility and Work Opportunity Reconciliation Act in 1996 (Allard 2009). The PRWORA replaced the Aid to Families with Dependent Children (AFDC) program with Temporary Assistance for Needy Families (TANF), emphasizing a work-based safety net for low-income families with children via block grants to the states (Berger, Cancian et al. 2018). These block grants, and their requisite state contributions, can be used by states to provide benefits or services aligning with the four purposes of the program: assistance for needy families to care for children; support of parental economic self-sufficiency; prevention of out-of-wedlock pregnancies; and the promotion of two-parent families (Assistance 2018).

Besides its emphasis on work, the passage of the Personal Responsibility and Work Opportunity Reconciliation Act in 1996 is at the nexus of two other themes in the evolution of welfare—devolution and the reliance on non-cash assistance via services. States have a great amount of discretion in how that funding is allocated between

programs, and the rules governing eligibility and participation; the degree of variation is exemplified in the share of TANF spent on basic assistance—Indiana spent roughly 3.6 percent of its TANF funding on basic assistance, while Kentucky spent 69 percent in 2019.<sup>1</sup> And, as the value of the TANF block grant has decreased over time, other forms of aid have become more prominent, including subsidized health insurance, subsidized child care, food stamps, and more (Allard 2009, Berger et al. 2018).

States have also had the option to further decentralize governance by passing discretion over the design and implementation of their TANF programs to local or regional entities. While some states had historically devolved administration of welfare to local governments prior to welfare reform, TANF provided new opportunities to expand such devolution (Gainsborough 2003). In recent years, roughly half of TANF recipients live in states with county-administered welfare programs (Hahn et al. 2015). Debate around second-order devolution has centered on whether these arrangements magnify the possible effects of first-order devolution, namely: are they more efficient and responsive, by way of providing services that are better aligned with problems and context unique to the locality; and does SOD introduce more interjurisdictional competition, incentivizing an under-provision of services to the poor?

Welfare has continually been shaped by federalism—where an individual lives has enormous impacts on the types of services available, level of benefits, and the process by which they are obtained. There is substantial heterogeneity in the categorical allocation of TANF funding between states (Fusaro 2021). And, increasing state discretion over federally funded anti-poverty programs has also led to greater interstate variation in benefits inclusivity and generosity “resulting in highly unequal access and benefits provided through the same programs in different states” (Bruch et al. 2018). These differences in TANF sanction stringency, work requirements, and time limits have been attributed to not only political ideology and competition, but also the percentage of births to unmarried mothers and size of the minority population in a state (Soss et al. 2001, Fellowes and Rowe 2004). Debates over the determinants of redistribution generosity have been shaped by the “race to the bottom” hypothesis—the idea that states with more

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<sup>1</sup> Author’s calculations using publicly available ACF expenditure data.

generous benefits would attract the poor, and thus lead to under-provision of welfare due to competition; evidence for this dynamic has been mixed at the state level (Volden 2002, Berry et al. 2003, Bailey and Rom 2004).

It is broadly accepted that welfare reform in general increased work participation and decreased welfare caseloads (Ewalt and Jennings Jr 2004, Blank 2007). However, that does not necessarily mean that economic well-being has unambiguously increased as a result. For example, while we see a decrease in the overall poverty rate in the years immediately following welfare reform (Blank 2002), it is not clear that welfare reform reduced poverty. Funneling an increasing amount of support through work-driven programs may undercut the safety net's capacity to support the most vulnerable during times of economic crisis. During the Great Recession, SNAP benefits and unemployment insurance rolls increased to meet the need, but TANF was not as responsive, indicating that the "most disadvantaged were more affected in the Great Recession than we would have expected from prior cycles" (Bitler and Hoynes 2016). Ewalt and Jennings Jr (2014) also observe a contraction in spending on TANF and expansion in Medicaid spending in the years immediately after the Great Recession.

Congress has not increased funding for TANF since the 1996 reform and as a result, the grant program has steadily decreased in value due to inflation (Falk 2021). However, its passage continues to mark a major shift in the emphasis of the social safety net from cash assistance to promoting work. The most important form of cash assistance today, the earned income tax credit, is directly tied to paid work. While the federal government spent roughly \$18.3 billion on cash assistance via AFDC in 1972, in 2012 it spent \$5.2 billion on TANF and \$67.5 billion on the earned income tax credit (Chaudry et al. 2016).<sup>2</sup>

### 2.2.1 Impacts of State versus Local Administration

States have had the ability to further decentralize governance by passing discretion in designing and implementing welfare policy to local or regional entities, often referred to as second-order devolution (SOD). While 15 states had devolved some

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<sup>2</sup> These figures are in 2014 dollars, per authors.

authority over the administration of welfare down to local governments pre-reform, there has not been as much post-reform decentralization as had been expected regarding eligibility and benefits levels (Gainsborough 2003). Interestingly, as Gainsborough observes, the states that had been state-administered under AFDC but had devolved significant authority under the TANF reform landed on programs that increased ties between workforce and welfare programs.

Scholarship on second-order devolution has centered on whether these arrangements magnify the possible effects of first-order devolution, namely: are they more efficient and responsive, by way of providing services that are better aligned with problems and context unique to the locality; and does SOD result in under-provision of services to the poor, either via interjurisdictional competition or local preferences. Shifting discretion over TANF to local or regional jurisdictions in the years immediately following welfare reform is associated with a decreased caseload, higher sanction rates, and better wage and employment outcomes at exit, empirical findings which support both the “race to the bottom” and efficiency/responsiveness hypotheses (Kim and Fording 2010). The consequences of SOD may also be a result of social construction and control; welfare receipt can become racialized as the share of clients is increasingly Black, but heterogeneously dispersed within a state—second order devolution allows local governments to engage in paternalistic behavior towards these clients perceived as “tough cases” (Soss et al. 2008).

Soss et al. (2011) argue new public management innovations in Florida (which has a highly decentralized and privatized TANF program) such as market-based competition and performance accountability, interact to create perverse incentives regarding sanctions—for-profit contractors were more likely to sanction welfare clients, “hard to serve” clients were more likely to be sanctioned, and negative performance feedback also increased sanction rates. They point out the tensions inherent in market-based competition and performance management; providers are not incentivized to share innovations or learning when they are competing with each other, and while second-order devolution “prizes locally-tailored solutions” for unique communities, it also promotes generalizability of innovation.

Second-order devolution could also have implications for which policy goals get prioritized and implemented in practice—if increased discretion and authority at the local level allows for the tailoring of interventions to unique local conditions, we would expect to observe greater diversity in policy approaches in response to place-specific problems or preferences. In a textual analysis of post-reform county welfare plans in California, a state that has devolved a significant amount of discretion to local governments, Sheely (2018) observes some variation in priorities, but finds that these differences had little to do with sanction/exemption rates.

However, the literature has not fully explored how second-order devolution has impacted the implementation of anti-poverty programs—the impacts of specific governance arrangements and services offered. For example, SOD states differ on which aspects of the design of their TANF program are left to county governments and the amount of oversight. In a 2015 descriptive study of four localized TANF states, Hahn and her co-authors write that county-administered TANF programs are delivered by county employees, at least partially funded by county governments, and involve county elected officials in decision-making but that these states differ in the amount of authority localities are given with regards to TANF eligibility requirements, benefit levels, service offerings, delivery models, and oversight. Many of these localized welfare states also have a history of not only devolving authority over the implementation of TANF, but also the administration of other federal programs like SNAP (Kogan 2017). Most recently, McBrayer (2020) concludes that increasing female, politically Democratic, and ward-based county official composition is associated with less punitive TANF outcomes in New Jersey, a county-administered TANF state.

While much of this literature on devolution focuses on broadly defined administrative structures and the (dis)incentives they create, devolution is not the only source of heterogeneity in program participation or social policy outcomes. Community political culture and norms also influence street-level bureaucratic behavior, too. Politically conservative street-level bureaucrats may be more supportive of policies that create administrative burden, voicing concerns about fraud, waste, and client deservingness (Bell et al. 2021). Local political conservatism is associated with an

increased probability of sanction for welfare recipients in Florida (Fording, Soss et al. 2007). While the previous authors focus on the increased discretion bureaucrats in a highly decentralized system may have, these effects can also be seen in other settings. Riccucci (2005) draws attention to the incongruence between the state’s goals and those of frontline staff in Michigan, which administers TANF centrally and allows very little discretion to local governments. The SNAP program is relatively centralized at the federal level compared to TANF—it has one set of eligibility rules, but states have administrative responsibility which they can delegate to local governments (Kogan 2017). While others have found that second-order devolution does impact welfare outcomes, Kogan (2017) finds that local public support of redistribution was positively associated with county-level SNAP participation, but administrative decentralization had no effect.

### 2.3 A Theory of Nonprofits, Spatial Access and Welfare Reform

The literature on the governance of the social safety net is both wide and deep, but there are still important unanswered questions. I’ve highlighted a variety of strands in the scholarship that have considered the factors shaping welfare benefit generosity and the impacts governance structures, including privatization, have on sanction rates and employment outcomes for recipients. However, as I noted in the previous section, basic assistance (i.e. cash benefits) only represents a small portion of TANF spending—roughly a fifth of TANF dollars nationwide were used towards basic assistance in 2019, declining from a quarter in 2015.<sup>3</sup> Even for anti-poverty programs that provide direct or indirect cash assistance, such as the EITC or food stamps, there are administrative burdens that may require the assistance of a third party to navigate (Moynihan et al. 2015). A review of state and county TANF implementation plans also reveal state and local governments are advancing the four purposes of this block grant through programs and services targeting a variety of subpopulations.<sup>4</sup> Welfare agencies leverage their community networks for services—they make referrals and contract with nonprofit providers to address barriers to self-sufficiency for low-income families. This network of

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<sup>3</sup> Author’s calculations using publicly available ACF expenditure data.

<sup>4</sup> Obtained by author via open records requests and publicly available documents.

nongovernment service providers is a key component of the social safety net today. Governments can collaborate with nonprofits, either via formalized contractual arrangements or more informal coordination.

Given both the hierarchical and horizontal nature of this system, I use the political economy logic of governance proposed by Lynn, Heinrich and Hill (2000) to explore the ways institutional arrangements structure incentives with regards to the implementation of the social safety net. The federal and state governments impose constraints on local communities' spending, but in relying on non-governmental organizations to provide services, we also observe a highly networked service delivery model. The reduced form logic of governance from Lynn, Heinrich, and Hill (2000) is:

$$Outcome = F(Environment, Client, Treatment, Structure, Management)$$

A governance outcome is a function of environmental factors, client characteristics, treatments (ex: organizational mission, program participation, sanctions, benefit generosity, services), structure (level of integration, centralization, organizational type), and management practices. In this paper, I examine the response of the broader social safety net to the government's choice to localize welfare, a structural choice. The outcome of interest is spatial access to social services for high-need neighborhoods. Why might we expect the level of government responsible for implementing TANF to matter to spatial access to nonprofit human service providers?

Theory from nonprofit management and federalism literature lays the foundation. First, nonprofits are responsive to government funding. In particular, scholars of nonprofit management have debated whether nonprofits complement or substitute for government services (Young 2006). While nonprofits may fill the stopgap for diverse interests that are not served by majority-supported government action, there is also evidence for cooperation and interdependence. As Salamon and Toepler (2015) write, nonprofits may be attractive partners for governments because they can be more responsive to emerging problems, provide a greater diversity of services tailored to client needs, and utilize volunteers. And in a joint test of these theories, Lecy and Van Slyke (2013) conclude that there is more evidence of interdependence than substitution in nonprofit growth—government grants and contracts constitute an important form of

revenue. Social services also constitute an important area of contracting for both state and local governments. Van Slyke (2003) argues that privatization, particularly for social services, is politically symbolic—politicians want to signal their commitment to making government smaller and more efficient. According to a 2010 report from the Urban Institute, government contracts and grants constituted the single largest source of funding for 60 percent of human service nonprofits (Boris et al. 2010). Allard’s (2009) work in three American cities highlights the public funding dependence human service organizations exhibit, particularly for those that do locate near high-poverty neighborhoods.

Nonprofits are also responsive to changes in public policy priorities. In studying the birth and death of human services nonprofits in 53 metropolitan areas through the lens of population ecology, Twombly (2003) found that minimal and moderate levels of AFDC waiver experimentation were associated with increased nonprofit entry, concluding the “increased discretion of states and localities to vary their provision of human services may have created new opportunities for nonprofit providers.” He argues that the shift away from income support to self-sufficiency in the era of welfare reform is an important environmental signal to nonprofits regarding policy priorities. In the context of Lacey and Van Slyke’s (2013) work, this makes sense—nonprofits are responding to a major possible source of revenue for their services.

Second, local governments may be more effective coordinators within their community than state agencies. In the spirit of the local responsiveness argument, local government actors may be best suited to influence local nonprofit networks, in directly funding HSOs or referring clients to them. On the one hand, local policymakers may know their communities best and tailor to local needs (Gainsborough 2003, Kim and Fording 2010, Sheely 2018). Hahn et al (2015) report that officials in SOD states “hold strong beliefs in the value of county administration,” echoing the local responsiveness argument, stating ““We believe that we know our communities better, and if we have that flexibility with how we spend our dollars we can figure out what’s specific to our communities and we can partner... with nonprofits”” and highlighting their own flexibility relative to state bureaucracies (76). In studying government grant and citizen



donations to Habitat for Humanity, Coupet and Schehl (2021) show that government grant dollars are responsive to nonprofit performance—local governments may be best positioned to observe performance and respond accordingly. Gainsborough (2003) also writes about this potential for greater flexibility—second-order welfare devolution provides local governments “greater latitude... to contract with nonprofit agencies and private, for-profit companies” for social services. This local knowledge and responsiveness could result in partnering with nonprofits located near high-need neighborhoods, as local officials have special knowledge of needs and will want to influence the network of providers, directly or indirectly. This effect would be magnified for more progressive communities, following the thinking that local governments are positioned to match services to preferences.

*Benevolent Efficiency Argument: High-needs neighborhoods will have increased geospatial access (number, provider capacity) to human service organizations.*

*Political Responsiveness Argument: Second-order devolution will magnify the effects of political ideology on geospatial access—more progressive communities will have greater access.*

On the other hand, this same governance structure could have the opposite effect—for example, local governments could use their superior knowledge of local conditions to select partners on performance at the expense of other factors. Local leaders with greater discretion could direct resources to nonprofits in the hopes of increasing performance, which could lead to cream-skimming. Nonprofits with the capacity to demonstrate performance may not be the ones that are located in high-need neighborhoods. And, if local governments compete with each other (Tiebout 1956), there is an incentive to underprovide welfare resources in general to the poor. So, localizing welfare policy could lead to fewer safety net resources being geographically available to high-need neighborhoods.

The work of Soss, Fording et al. (2008) also provides a darker view of devolution that would support an argument for less access to services on the basis of race; they argue that localizing TANF policymaking is less about efficiency and responsiveness, but more about paternalism and social control. Local governments in this view are particularly well

situated to enforce punitive systems of social control over a population whose racial composition is heterogeneously dispersed. In this framework, localizing welfare would not only reduce spatial accessibility in general, it would also disproportionately reduce access for racial minorities. In essence, localizing welfare could amplify disparities in high-need neighborhoods access on the basis of race (Allard 2009, Garrow 2014). This scholarship would suggest alternative hypotheses:

*Under-provision Argument: Increasing local welfare policymaking authority will lead to decreased geospatial accessibility for low-income neighborhoods.*

*Racialized Welfare Argument: Increasing local welfare policymaking authority will lead to greater racial disparities in geospatial accessibility.*

## 2.4 Methodology and Data

It is often very challenging to study the impacts of institutional arrangements on a given outcome, because change is slow and infrequent. However, the Personal Responsibility and Work Opportunity Reconciliation Act of 1996 provided states the opportunity to change how welfare is governed; while states could locally administer TANF's predecessor program, the TANF block grant expanded discretion in governance, enabling states to designate more policymaking authority as they saw fit. I use this policy change as the treatment in a series of pooled cross section regressions of eight states from 1990 to 2007. Using the typology in Gainsborough (2003) and Hahn et al. (2015) as a starting point, I have four treatment states: Colorado, Minnesota, North Carolina, and Ohio; and four control states: Arizona, Illinois, Georgia, and Pennsylvania. In the subsections below, I lay out my procedure for test case selection, data collection, and estimation. I conclude by presenting summary statistics.

### 2.4.1 Case Selection

I chose the four treatment states because they provide a rich array of demographic, ideological, and administrative heterogeneity across geographic regions. Of these states, Colorado stands out as having the greatest change from pre-TANF to post-TANF (Hahn, Kassabian et al. 2015). These states also vary in their administrative

arrangements—in some, counties contribute only to administrative costs, while in others the counties also contribute to the costs of benefits. These states not only devolve TANF to the counties, they also all administer SNAP at the county-level, and counties in these states are also responsible for contributing towards the cost of Medicaid.<sup>5</sup> I selected the control states on the basis of their comparability to the treatment states with respect to demographics, geography, and their economies. For example, there is a state from the West, Midwest, Rust Belt, and South in each group. I present mean values for a variety of demographic and economic features of these states by treatment status in the table below, prior to treatment. In both cases, the populations are predominantly urban. The control states are larger and have slightly larger racial and ethnic minority populations. However, they are very similar in their average college attendance, labor force participation (overall and by industry), AFDC caseloads, and poverty rates.

Table 2.1 Descriptive Statistics By Treatment Status in 1990

	<b>Treatment</b>	<b>Control</b>
Total Population	6,286,311	8,363,922
Percent Urban	69.19%	76.06%
Percent Hispanic	4.14%	7.58%
Percent Black	9.71%	13.49%
Educational Attainment	20.78%	19.65%
Labor Force Participation	67.78%	64.73%
Manufacturing	10.51%	8.92%
Agriculture	47.64%	50.16%
AFDC Caseload, Per 1K	14.39	15.17
AFDC Benefit, Family of 3	\$373.50	\$338.50
Poverty Rate	12.55%	13.55%

*Source: University of Kentucky Center for Poverty Research, NHGIS Census, Bureau of Economic Analysis*

I also considered the welfare governance arrangements each state adopted with AFDC and subsequently with TANF. How “county-administered” is defined varies by source. In this paper, I focus on how much authority to design and implement TANF

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<sup>5</sup> According to a [report](#) from the National Association of Counties, some 18 states have counties contributing to the costs of Medicaid. Counties in some states have also played a role in designing managed care organizations, as well as enrolling and monitoring recipients. See: Verdier, J.M., 1999. The Role of Local Governments in Medicaid Managed Care: Issues and Options. Center for Health Care Strategies. Mathematica.

program(s) is given directly to county governments. The table below summarizes the amount of discretion each state provides over the two general forms of TANF assistance: cash payments and services. All of the treatment states devolve authority over services to county governments; of the control states, I categorize all as being state-administered, even if the implementing state agency operates offices in all counties (Georgia being a prime example). Benefit levels and eligibility are determined at the state level for all control states, but there is greater heterogeneity in the treatment ones. In the table below, “BA” denotes “Basic Assistance” and refers to the traditional welfare caseload; “ST” refers to “Short Term” and encompasses formal diversion or short term assistance payment programs. While the state sets benefit and eligibility standards for the state of Colorado, county officials are members of the state Board that approves these guidelines. Counties can also choose to have their own short-term aid programs, in addition to the state program. In Minnesota, benefits and eligibility for the Minnesota Family Investment Program (basic assistance) and the Diversionary Work Program (short term assistance) are set by the state government; counties may elect to provide emergency assistance. North Carolina allows greater discretion over TANF to select “electing” counties, who get approval to deploy their own programs. In Ohio, the state has control over eligibility and benefit levels for its Ohio Works First program, but counties have discretion over creating diversion programs—these diversion programs can include cash payments, and counties may design their own eligibility thresholds, target groups, and benefit caps for these short-term benefits.

Table 2.2 Welfare Governance by Treatment Status

	<b>AFDC</b>		<b>TANF</b>		
	<b>(Pre-Treatment)</b>		<b>(Post-Treatment)</b>		
	Admin	Nonfed Costs	Benefits (BA) Discretion	Benefits (ST) Discretion	Services Discretion
<i>Treatment States</i>					
Colorado	County	Shared	State	Both	County
Minnesota	County	Shared	State	Both	County
North Carolina	County	Shared	State, “Electing” Counties	Both	County
Ohio	County	Shared	State	County	County
<i>Control States</i>					

Table 2.2 (continued)

Arizona	State	State	State	State	State
Illinois	State	State	State	State	State
Georgia	State	State	State	State	State
Pennsylvania	State	State	State	State	State

*Source: Author's categorization from Hahn et al (2015), HHS Annual Reports to Congress, and state planning documents.*

## 2.4.2 Measurement and Estimation

How can neighborhood spatial access be measured? In their paper assessing spatial accessibility of social services, Marwell and Gullickson (2013) geocode the location of service organizations from contracts for social services relative to community districts and health areas in New York City, and then sum the funding for contracts within these administrative boundaries, scaling by population. Others have computed travel distance to the nearest location—in their study of geographic accessibility of child care subsidies, Herbst and Tekin (2012) use the distance in miles to the closest human services office. I utilize an approach similar to that of Allard (2009) to construct a measure of accessibility. Allard (2009) constructs service accessibility scores (overall and by service type) based on service provider surveys that collected data on funding and clients. He identified the locations of the responding agencies and used a two-step process to calculate a metropolitan-weighted accessibility score of agencies within a three-mile radius of a census tract.<sup>6</sup> This distance is used, as the NPOs reported that most clients came from within this radius of their locations.

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<sup>6</sup> Allard's (2009) accessibility score is as follows. In the first step, W represents the monthly number of clients served for agencies of type X in a three-mile radius. P is the number of poor residents also in a three-mile radius. Essentially, the first step compares the population served to the potential demand:

$$IA_i = \frac{\Sigma(WX_i)}{\Sigma(P_i)}$$

In the second step, Allard (2009) scales a given tract's score by the average score of all tracts within the metropolitan area:

$$SAS_i = \frac{IA_i}{\frac{1}{N} \sum_{i=1}^N IA_i}$$

I used the Urban Institute’s National Center for Charitable Statistics Data Archive core files, which detail tax filings for exempt organizations with annual revenues above \$25,000, to construct measures of geospatial access scaled by neighborhood population. I geocoded the address for each nonprofit by fiscal year and used ArcGIS to calculate the number of human service nonprofits, total revenue, and program revenue within a 3 mile radius of the tract centroid.<sup>7</sup> This is an imperfect measure of geospatial access. For example, a nonprofit may have one office, but provide services at other site(s). I also include program revenue in addition to total revenue as proxy for possible government contract revenues. I used NTEE codes to identify human service organizations. A full list of those codes can be found in the appendix, but generally these organizations fall into the categories salient to the four goals of TANF—basic needs, remedial adult education and job training, basic needs, and family-based services. Organizations not included in this list are nonprofit child care providers and churches. Child care providers are not included because those services are also largely provided by for-profit businesses and longitudinal data is not readily available for these businesses.

I then categorize neighborhoods as extremely low-, low-, middle-, and high-income using the ratio of tract household median income to the county household median income from the decennial census (Bischoff and Reardon 2014). An extremely low-income tract is defined as having less than 50 percent of the county median income; low-income, less than 80 percent; middle-income between 80 and 120 percent; and high-income tracts as above 120 percent. In the graphs below I present the average values for number of HSOs, total revenue, and program revenue in a 3-mile radius, scaled by tract population. Scaling by tract population is imperfect—the nonprofits nearby a neighborhood do not serve only the population in that given tract. There is also an observable drop in the first graph at the year 2000; the tract units change with the decennial census, as do their demographic estimates.

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<sup>7</sup> I used the [Texas A&M GeoServices](#) for geocoding and NHGIS shapefiles for Census tracts in ArcGIS. More information can be found in the appendix.

Figure 2.1 Geospatial Access to HSOs by Neighborhood Income

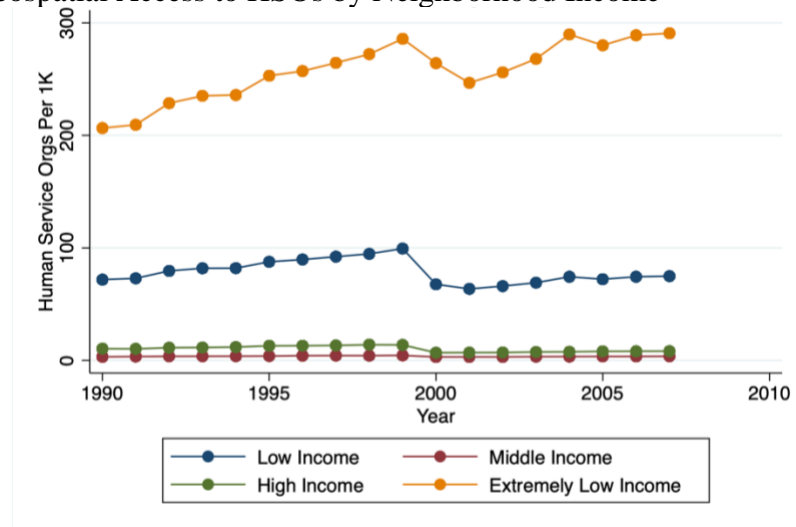


Figure 2.2 Geospatial Access to HSO Revenue by Neighborhood Income

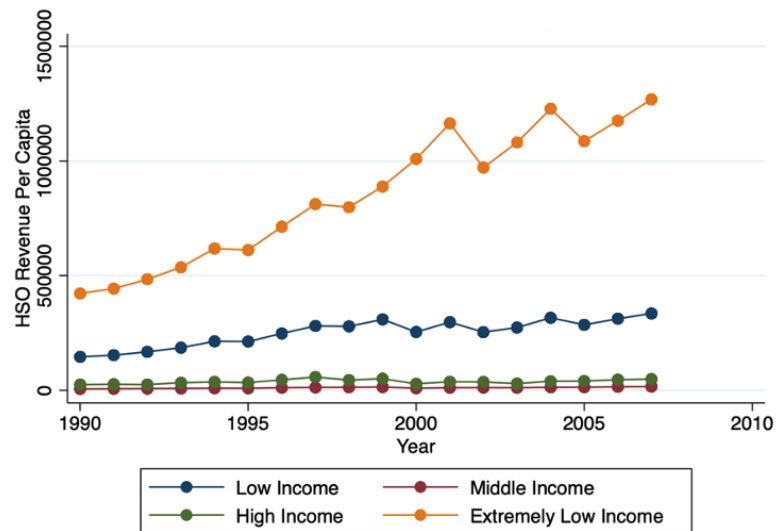
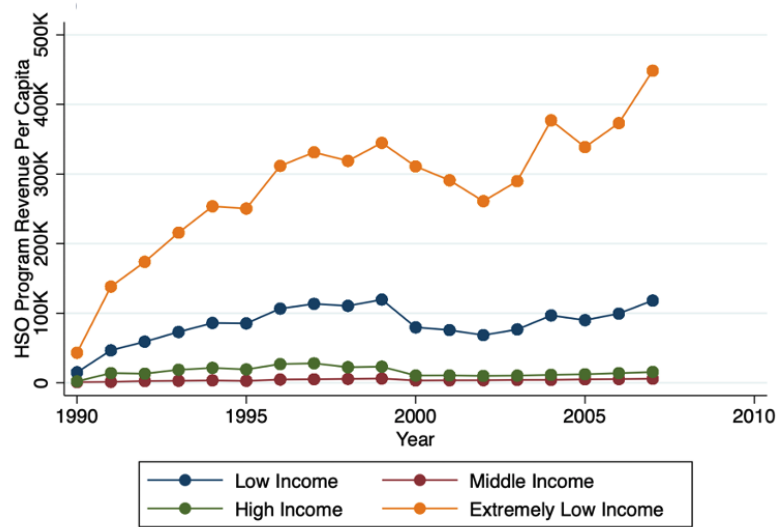


Figure 2.3 Geospatial Access to HSO Program Revenue by Neighborhood Income



These graphs show that geospatial access for extremely low-income neighborhoods has grown over time, but that access is somewhat stagnant for low-income neighborhoods. Overall, low-income neighborhoods also have greater geospatial access in these states than middle- and high-income neighborhoods. What these graphs do not reveal is how prevalent extreme values are in these data. For example, over seven percent of the low-income tracts have zero HSOs in a 3-mile radius. As Marwell and Gullickson (2013) also found in their study of social service contracts in New York City neighborhoods, the distribution of the tract-level data presents problems for standard OLS regressions. There are many neighborhoods that have an accessibility value of zero, and there are some that have extremely high values. This distribution results in an extremely poor-fitting OLS regression model. Transforming the data by using logs would not solve this issue, as the log of zero is undefined. And, it is also inappropriate to introduce tract-level fixed effects due to data constraints, as Census tracts change boundaries within a county over time. While efforts have been made to integrate these data using Census blocks, variables crucial to this analysis are not integrated at this time.

I considered two options: transforming the neighborhood-level values using an inverse hyperbolic sine function, or aggregate the data to the county-level. The inverse hyperbolic sine is similar to a natural log, but retains zeroes without further manipulation (Bellemare and Wichman 2020). The interpretation of this transformation is also similar



to that of a natural log for values not approaching zero. While this transformation has become a popular method in public policy analysis and economics to address this very data distribution issue (Dow et al. 2019, Friedson et al. 2021, Dee and Penner forthcoming), it does not solve for the tract-level fixed effects issue above, leading to models that are not as well fitted as my preferred option: creating county-level population-weighted averages for accessibility of low-income neighborhoods. The benefit of aggregating to the county-level is the ability to apply county-level fixed effects, resulting in models that are more readily interpretable and better fit. I present estimates using the inverse hyperbolic sine in the appendix and briefly discuss them in the results section as a robustness check.

In order to examine racial gaps in access in an aggregate manner, I also compute measures of racial gaps in access— instead of weighting by total neighborhood population, I weight population by race. For ease of comparison, I collapse racial categories to calculate the difference between two—white and people of color. Instead of using the total tract population to weight the county average, I use the total white population and total nonwhite population to measure access by race. I summarize these measures in the table below by reporting the mean and the range (in parentheses) for each variable.

Table 2.3 County-Level Measures of Geospatial Access for Low Income Neighborhoods

	<b>Overall</b>	<b>People of Color</b>	<b>White</b>	<b>Gap</b>
Number of HSOs (per 1K) <sup>8</sup>	1.81 (0-28.35)	1.74 (0-26.08)	1.75 (0 – 26.09)	0.01 (-0.001- 1.01)
Total Revenues (Per Capita)	\$3,226.80 (\$0-213,114.50)	\$3,056.30 (\$0-186,806.50)	\$3,076.03 (\$0-188,074.10)	\$15.11 (-\$3.70 – 2,877.84)
Program Revenues (Per Capita)	\$1,232.68 (\$0-118,323.50)	\$1,166.46 (\$0-105,806.90)	\$1,173.73 (\$0-105,902.30)	\$5.51 (-\$3.01-1,104.09)
<i>Key: Mean (Min - Max)</i>	N=6,630	N=6,630	N=6,620	N=6,620

<sup>8</sup> I scale this variable up so that the results are easier to digest.

As a reminder, all of these numbers are population-weighted averages for a given county. On average people living in low-income tracts have geospatial access to 1.81 HSOs per one thousand residents, \$3,227 in total HSO revenues per capita, and \$1,233 in HSO program revenues per capita. A positive gap indicates people of color in low-income neighborhoods have fewer resources available to them on average than their white counterparts in the same county. While there is a very small racial gap in access to the number of nonprofit human service providers, revenues, and program revenues on average, we also observe substantial variation in that gap.

I use the weighted averages for low-income neighborhood geospatial access (the number of HSOs per one thousand residents, total revenue per capita, program revenue per capita) as dependent variables in cross-sectional pooled regressions with dummy variables for both year and county to examine the relationship between welfare governance and the nonprofit social safety net overall. To explore the connections between governance and racial gaps in access, I use the racial differences between these three measures of spatial access for low-income neighborhoods. I measure governance structure using the variables in the table below.

Table 2.4 Governance Structure

<b>Component</b>	<b>Description</b>
Post-Welfare Reform	Dummy variable for all years after 1996, after passage of welfare reform.
Second-Order Devolution State	Time invariant; equal to one if state devolves TANF authority to county governments
SOD*Post Reform	Interaction term for the above two variables.
TANF Benefits Discretion	Scale for policymaking authority regarding short-term TANF benefits (i.e. emergency or diversion benefits). Coded 1 for ability to add to a state program; 2 for devolved programmatic responsibility.
TANF Services Discretion	Dummy variable for states that devolve policymaking re: services offered to counties.
Self-Generated County Revenue	Share of revenue that is raised by county governments and not through intergovernmental transfers.
Number of HSOs (per capita)	Human service organization density, scaled by population.
Total HSO Revenues (per capita)	Sum of all HSO revenues, scaled by population.

Table 2.4 (continued)

Total HSO Program Revenues (per capita)	Sum of all HSO program revenues, scaled by population.
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The variables most salient to testing the theory presented above are the first four, which operationalize facets of policymaking authority devolved to county governments via welfare reform. Second-order devolution states vary in the amount and type of discretion that is passed through to counties. One important area over which the treatment states vary is with regards to benefits; some counties in North Carolina have had the option to design their own eligibility and benefit schedules, for example, but in all other states the ongoing basic assistance (i.e. what is often referred to as “welfare”) is set at the state-level. There is greater variation in the amount of authority counties have regarding short-term benefits—these payments are usually one time and are not counted in caseload reporting to the federal government. These short-term benefits are often referred to as emergency assistance or welfare diversion. Minnesota, Colorado, and North Carolina have state-set programs for these benefits, but counties may choose to augment those programs with their own—these counties were coded as “1”. In Ohio, I argue counties have more discretion over these programs over short-term benefits because all policymaking responsibility is devolved; these counties were coded as “2”.

I also control for features of the nonprofit sector, including density and overall capacity (measured by revenues). In addition to these measures of welfare governance structure, I include political, economic, and demographic controls in all regressions.

Table 2.5 Covariates: Environment, Clients, Policy Treatment

Variable	Description
<i>Policy Treatment</i>	
State Government Spending, Per 1K	Total state spending per one thousand residents, minus intergovernmental transfers.
County Government Spending, Per 1K	Total county spending per one thousand residents.
Public Assistance, Per Capita	Sum of public assistance distributed within a county, scaled by the population; county-level.
<i>Potential Client Characteristics</i>	
Population <200% Federal Poverty Line	Share of the population in households with less than twice the federal poverty line; county-level.

Table 2.5 (continued)

Children in 2-Parent Married Households	Share of children in families with married parents; county-level.
AFDC/TANF Child-Only Case Share (State)	Share of cases that are child-only; state-level.
Female Labor Force Participation	Share of the working-age female population currently in the labor force; county-level.
<i>Environment – Political, Economic, Demographic</i>	
Total Population	Total number of residents; county-level.
Urban Share	Share of residents in urban areas; county-level.
Racial Residential Segregation Index	Theil index measuring dispersion by racial groups in Census tracts; county-level.
Population Share – White	Percent of the population identifying as white; county-level.
State Government Ideology	Nominate score measuring state government progressivism.
POTUS Democratic Vote Share	Share of votes for Democratic candidate in last presidential election; county-level.
State Economic Growth	Percent change in gross domestic product; state-level.
State Income Inequality	Gini coefficient for income; state-level.

## 2.5 Results

The following table reports the results for several regressions. The dependent variables are listed in the first row: number of human service organizations per one thousand residents, total revenues per capita, and program revenues per capita. I do not include the variables for discretion type in these first three regressions. Generally, the number of human service organizations near low-income neighborhoods is unrelated to welfare governance, with two exceptions. As the share of self-generated revenue increases by one percentage point, geospatial access decreases by 0.25 HSOs per one thousand residents. If we consider county self-generated revenue share as a proxy for autonomy, this provides some weak evidence for the under-provision argument—increasing local policymaking authority results in less access for the poor. And as we might expect, the county density of HSOs has a positive relationship with geospatial access for low-income neighborhoods.

For access to total and program revenue, we see a consistent story—after the devolution of welfare, there is less geospatial access to nonprofit funding for poor neighborhoods. An average low-income neighborhood has access to nearly 700 dollars less per capita in nearby nonprofit annual program revenues. We also see higher levels of county financial independence are associated with decreases in access to nonprofit funding nearby. Counter-intuitively, human service organization density is inversely related with geospatial access to revenues for poor neighborhoods.

Geospatial access for low-income neighborhoods has a positive relationship with state and local government spending. The magnitudes for state and county government expenditures reveal an interesting dynamic—they are both larger for total revenues than for program revenues. This is consistent with the idea that governments use a mix of grants and contracts with nonprofit providers. Public assistance benefits per capita has a negative relationship across all specifications—this might indicate substitution between availability of cash benefits and service provision by nonprofits. This relationship is echoed by the state share of AFDC/TANF caseload that are children-only cases, which may be on assistance for longer since these cases are not subject to the same requirements and time limits as adult cases.

Table 2.6 Welfare Governance & Geospatial Accessibility to the Social Safety Net for Low-Income Neighborhoods

	(1) Number of HSOs, Per 1K in Poor Neighborhoods		(2) Total Revenues, Per Capita in Poor Neighborhoods		(3) Program Revenues, Per Capita in Poor Neighborhoods	
	<i>Coeff.</i>	<i>S.E.</i>	<i>Coeff.</i>	<i>S.E.</i>	<i>Coeff.</i>	<i>S.E.</i>
Post-Welfare Reform	-0.04	0.20	1886.31	1552.09	936.89	811.78
Second-Order Devolution State	-	-	-	-	-	-
SOD*Post Reform	-0.01	0.03	<b>-1608.28***</b>	<b>387.41</b>	<b>-692.21***</b>	<b>155.71</b>
Self-Generated County Revenue	<b>-0.25**</b>	<b>0.10</b>	<b>-6188.33***</b>	<b>1261.90</b>	<b>-2940.97***</b>	<b>765.29</b>
Number of HSOs (per capita) in County	<b>1506.58**</b> *	<b>201.86</b>	<b>-12200000***</b>	<b>2332136</b>	<b>-5975739***</b>	<b>799093.90</b>
Total HSO Revenues (per capita) in County	0.00	0.00	<b>11.05***</b>	<b>3.96</b>	0.35	0.95

Table 2.6 (continued)

Total HSO Program Revenues (per capita) in County <i>Policy Treatment</i>	0.00	0.00	-1.33	4.07	<b>10.48***</b>	<b>1.85</b>
State Government Spending, Per 1K County	0.00	0.00	<b>1.79***</b>	<b>0.56</b>	<b>1.09***</b>	<b>0.32</b>
Government Spending, Per 1K Public	<b>0.0002***</b>	<b>0.0000</b>	<b>2.77***</b>	<b>0.76</b>	<b>0.72***</b>	<b>0.17</b>
Assistance, Per Capita	<b>-0.002***</b>	<b>0.0005</b>	<b>-27.52***</b>	<b>4.67</b>	<b>-8.27***</b>	<b>1.44</b>
<i>Potential Client Characteristics</i>						
Population <200% Federal Poverty Line	<b>1.29**</b>	<b>0.50</b>	<b>35366.44***</b>	<b>4381.18</b>	<b>16539.39***</b>	<b>2349.21</b>
Children in 2- Parent Married Households	<b>-13.44***</b>	<b>2.37</b>	<b>110523.40***</b>	<b>16592.82</b>	<b>44633.81***</b>	<b>10530.90</b>
AFDC/TANF Child-Only Case Share (State)	-0.24*	0.14	<b>-9277.42***</b>	<b>1639.47</b>	<b>-4969.29***</b>	<b>904.81</b>
Female Labor Force Participation	-0.23	0.38	<b>8560.17***</b>	<b>2444.33</b>	<b>6172.30***</b>	<b>1338.30</b>
<i>Environment – Political, Economic, Demographic</i>						
Total Population	0.00	0.00	0.00	0.00	0.00	0.00
Urban Share	0.22*	0.12	<b>-6195.65***</b>	<b>964.30</b>	<b>-3094.86***</b>	<b>495.37</b>
Racial Residential Segregation Index	<b>-2.56***</b>	<b>0.38</b>	<b>-31452.30***</b>	<b>4273.23</b>	<b>-13271.99***</b>	<b>1956.96</b>
Population Share – White	-0.11	0.58	<b>-27669.66***</b>	<b>5378.22</b>	<b>-11044.07***</b>	<b>1905.45</b>
State Government Ideology	0.00	0.00	<b>-19.26***</b>	<b>5.89</b>	<b>-5.92**</b>	<b>2.54</b>
POTUS Democratic Vote Share	<b>0.02***</b>	<b>0.00</b>	<b>147.54***</b>	<b>18.80</b>	<b>47.43***</b>	<b>8.40</b>
State Economic Growth	-0.13	0.57	<b>17283.58***</b>	<b>5818.49</b>	6366.14*	3379.53
State Income Inequality	1.52	1.02	<b>-17922.72**</b>	<b>8795.32</b>	-3127.26	3390.64
Year Dummies	Y		Y		Y	
County Dummies	Y		Y		Y	

Table 2.6 (continued)

*** p<0.01,			
**p<0.05,	R <sup>2</sup> =0.98	R <sup>2</sup> =0.85	R <sup>2</sup> =0.71
*p<0.10	N=6,630	N=6,630	N=6,630
Robust S.E.s			

The other covariates for potential client characteristics and economic context show accessibility is positively responsive to overall poverty rates. This is in line with the expectation that nonprofits are meeting a demand or need for services. But, the positive relationship between female labor force attachment and the revenue-based measures for accessibility does not conform to this. It is also surprising to see lower levels of accessibility for more urban and white communities, and in states with more progressive state governments. However, we also see higher levels of accessibility for less segregated counties and those that voted Democratic in the last presidential election. I also run the same models on tract-level data using the inverse hyperbolic sine to transform the dependent variables, as a robustness check. The full results are reported in the appendix, but I show select results below.

Table 2.7 Welfare Governance &amp; Geospatial Accessibility to the Social Safety Net for Low-Income Neighborhoods: Tract-Level Analysis

	(1) Number of HSOs, Per 1K (inverse hyperbolic sine)		(2) Total Revenues, Per Capita (inverse hyperbolic sine)		(3) Program Revenues, Per Capita (inverse hyperbolic sine)	
	<i>Coeff.</i>	<i>S.E.</i>	<i>Coeff.</i>	<i>S.E.</i>	<i>Coeff.</i>	<i>S.E.</i>
Post-Welfare Reform	-0.05	0.12	<b>1.69***</b>	<b>0.26</b>	<b>2.83***</b>	<b>0.28</b>
Second-Order Devolution						
State	-	-	-	-	-	-
SOD*Post						
Reform	0.00	0.02	-0.07*	0.04	<b>-0.12***</b>	<b>0.04</b>

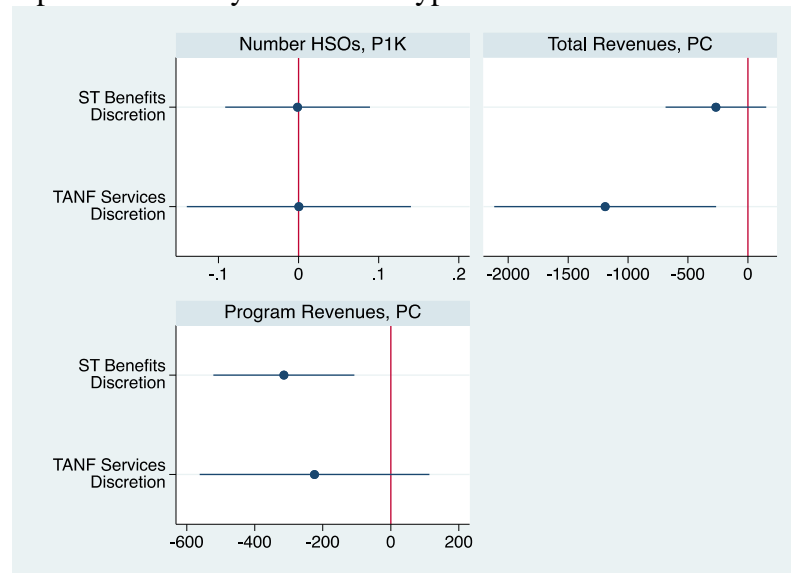
*** p<0.01,			
**p<0.05,	R <sup>2</sup> =0.54	R <sup>2</sup> =0.64	R <sup>2</sup> =0.60
*p<0.10	N=69,074	N=69,074	N=69,074
Robust (S.E.s)			

The relationship between welfare governance and the number of HSOs is unchanged; total revenues statistically weakens; and the estimates for program revenues is consistent with the county-level model with respect to direction and significance. Second-order devolution is associated with a 12 percent decline in program revenues per

capita near low-income neighborhoods. These models all include year and county fixed effects, and as expected, the amount of variance explained for these tract-level regressions is less than their county-level counterparts.

Next, I explore the relationship between geospatial accessibility and the kinds of discretion devolved to county governments. I replace the indicator variable for post reform second-order devolution states with the two variables indicating the amount and type of discretion. All of the SOD states were coded as “1” for services discretion in the post-reform era, but differed by the amount of discretion they had over benefits. I present select coefficients in the figure below. Consistent with the previous results, breaking out devolution type has no statistically significant relationship with geospatial access by number of HSOs. However, we see that more devolved authority over short-term TANF benefits is associated with less geospatial access to nonprofit program revenues, but has no effect with respect to total revenues.

Figure 2.4 Geospatial Access by Discretion Type



I now turn to the interactions between race, politics, and welfare governance. For ease of reading, I only report the results for the governance structure variables. I operationalize local political ideology via the Democratic party vote share in the last presidential election; in the previous results, there was a statistically significant and positive relationship between Democratic vote share and accessibility. I interact that variable with the post welfare reform, SOD variable to test whether localized welfare



governance magnifies local political preferences. There is some evidence that this is the case—for every one point increase in Democratic vote share, there is an additional 20 dollar per capita increase in access to nearby program revenues, and an 0.01 per one thousand residents increase in number of HSOs nearby in localized welfare communities.

Table 2.8 Welfare Governance, Political Ideology, & Geospatial Access to the Social Safety Net

	(1) Number of HSOs		(2) Total Revenues		(3) Program Revenues	
	<i>Coeff.</i>	<i>S.E.</i>	<i>Coeff.</i>	<i>S.E.</i>	<i>Coeff.</i>	<i>S.E.</i>
Post-Welfare Reform	0.11	0.20	1630.28	1656.23	764.66	958.78
Second-Order Devolution State	-	-	-	-	-	-
SOD*Post Reform	<b>-0.47***</b>	<b>0.11</b>	<b>-2468.63***</b>	<b>574.81</b>	<b>-1504.67***</b>	<b>267.68</b>
SOD*Post	<b>0.01***</b>	<b>0.003</b>	20.37	18.91	<b>19.74***</b>	<b>7.12</b>
Reform*Democrat						
POTUS Democratic Vote Share	<b>0.02***</b>	<b>0.003</b>	<b>140.63***</b>	<b>21.64</b>	<b>42.93***</b>	<b>9.03</b>
*** p<0.01, **p<0.05, R <sup>2</sup> =0.98 R <sup>2</sup> =0.85 R <sup>2</sup> =0.72						
*p<0.10 N=6,630 N=6,630 N=6,630						
Robust S.E.s						

However, the base effect of devolving policymaking authority of the reform is still negative. To illustrate these cross-cutting trends, I plot marginal effects for the interactions between reform, administrative structure, and political ideology in Figure 2.5 and 2.6. I show the predicted values by governance for the pre-period on the left, and post-period on the right in both figures. In the pre-period, the predicted values for program revenues is not particularly responsive to community progressivism. However, on the right, we see that the magnitude of change is greater. Even though local progressivism predicts greater accessibility for second-order devolution communities, those effects attenuate the initial finding that low-income neighborhoods have less geospatial access to human service nonprofit providers under localized welfare governance regimes on the basis of program revenues.

Figure 2.5 Marginal Effects of Ideology, by Reform & Time on Program Revenues

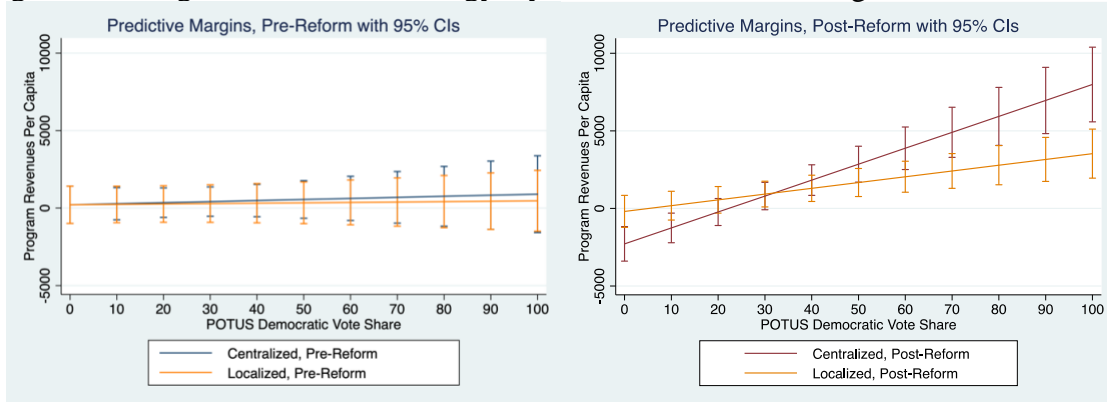
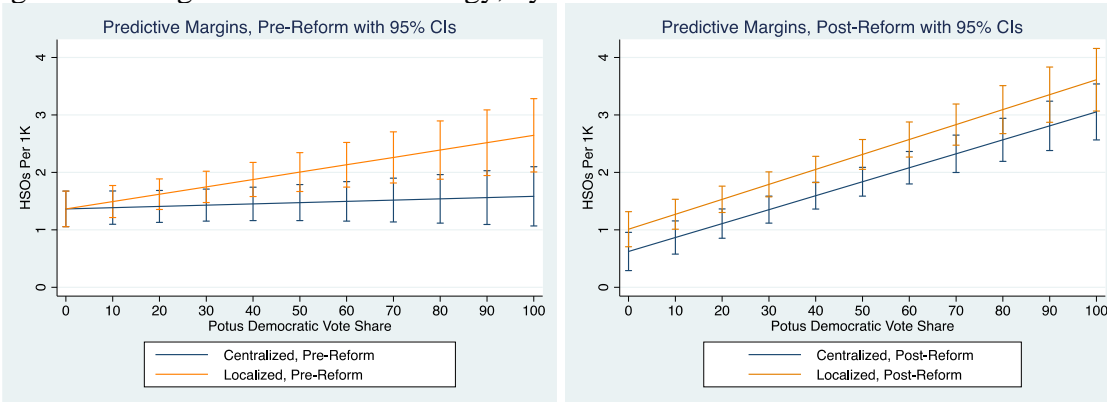


Figure 2.6 Marginal Effects of Ideology, by Reform & Time on HSO Counts



In Figure 2.6, we see that the number of HSOs near low-income neighborhoods is not particularly different on the basis of ideology for centralized welfare states in the pre-period, which changes in the post period. Across time, more progressive communities predict more HSOs near low-income neighborhoods in localized welfare states. However, the magnitudes are greater in the post-period. Taken together, these graphs show that geospatial accessibility, regardless of governance structure, was less responsive to community progressivism in the AFDC era. Welfare reform increased policy discretion; the post-reform predicted values support the argument that increasing policy discretion allows communities to exert greater influence over the broader social safety net.

In the table below, I report the results for analyses of racial gaps in geospatial access; I use the same covariates, but only list the coefficients of interest. For these measures, the dependent variable is the difference between the population weighted

average broken out by race within a county. A positive number indicates a widening gap within a county, with people of color having fewer resources near them comparable to their white counterparts in low-income neighborhoods. My results do not support greater racial disparities in localized welfare communities. In general, the racial gap decreased in the post-reform era—for example, the gap decreased by an average of 124 dollars in total revenues near low-income neighborhoods. There was no meaningful relationship between administrative structure and the racial gap.

Table 2.9 Welfare Governance & Racial Gaps in Geospatial Access to the Social Safety Net: County-Level Analysis

	(1) Racial Gap in Number of HSOs near Low-Income Neighborhoods		(2) Racial Gap in Total Revenues near Low- Income Neighborhoods		(3) Racial Gap in Program Revenues near Low-Income Neighborhoods	
	<i>Coeff.</i>	<i>S.E.</i>	<i>Coeff.</i>	<i>S.E.</i>	<i>Coeff.</i>	<i>S.E.</i>
Post-Welfare Reform	<b>-0.06***</b>	<b>0.01</b>	<b>-123.75***</b>	<b>27.9 2</b>	<b>-60.13***</b>	<b>14.03</b>
Second-Order Devolution State SOD*Post Reform	-	-	-	-	-	-
	-0.003	0.002	-1.89	4.56	-0.66	2.01
*** p<0.01, **p<0.05, *p<0.10 Robust (S.E.s)	R <sup>2</sup> =0.71 N=6,620		R <sup>2</sup> =0.75 N=6,620		R <sup>2</sup> =0.62 N=6,620	

As a robustness check, I return to the tract-level data using inverse hyperbolic sine transformations. Instead of looking at gaps, I simply interact the treatment with tract variables for tract racial and ethnic composition. The effects of localizing welfare were not magnified by the Black population share for total nor program revenues; however, there was a positive relationship for the number of HSOs. A one percentage point increase in the Black share of the neighborhood is associated with a 0.21 percent increase in the number of nearby human service nonprofits. In all three specifications, geographic accessibility is increasing as the Black population increases. These results are broadly consistent with the findings in the previous table.

Table 2.10 Welfare Governance & Geospatial Access for the Black Population: Tract-Level Analysis

	(1) Number of HSOs, Per 1K (inverse hyperbolic sine)		(2) Total Revenues, Per Capita (inverse hyperbolic sine)		(3) Program Revenues, Per Capita (inverse hyperbolic sine)	
	<i>Coeff.</i>	<i>S.E.</i>	<i>Coeff.</i>	<i>S.E.</i>	<i>Coeff.</i>	<i>S.E.</i>
Post-Welfare Reform	-0.03	0.12	<b>1.70***</b>	<b>0.26</b>	<b>2.84***</b>	<b>0.28</b>
Second-Order Devolution State	-	-	-	-	-	-
SOD*Post Reform	<b>-0.07***</b>	<b>0.02</b>	-0.07	0.05	<b>-0.15***</b>	<b>0.05</b>
SOD*Post Reform*% Black	<b>0.21***</b>	<b>0.03</b>	0.02	0.06	0.08	0.06
Population Share – Black	<b>0.20***</b>	<b>0.02</b>	<b>0.81***</b>	<b>0.04</b>	<b>1.42***</b>	<b>0.07</b>
*** p<0.01, **p<0.05, *p<0.10 Robust (S.E.s)	R <sup>2</sup> =0.54 N=69,074		R <sup>2</sup> =0.64 N=69,074		R <sup>2</sup> =0.60 N=69,074	

## 2.6 Discussion and Conclusion

The social safety net today blends services and funding from public and private sources. In this paper, I assess the relationship between geospatial access to the nonprofit social safety net and welfare governance arrangements over time. In Table 2.11, I summarize the main results. My findings support the argument that government administrative choices relating to policy reforms have important implications for the broader network of social services. I did not find any support for the benevolent efficiency argument. Devolving policymaking discretion to county governments was associated with decreased access for low-income neighborhoods across several specifications, but that effect was somewhat attenuated by local political ideology. I differentiated by discretion type and find that the impacts of increasing discretion over short term benefits programs varied. There was no relationship with the number of nonprofits near low-income neighborhoods, but it did seem to matter for program revenues. Welfare reform increased discretion across the board with regards to implementation, which I highlighted in the marginal effects of ideology broken out by

time period and reform governance. Nonprofits near low-income neighborhoods took in more program revenues as community progressivism increased.

Table 2.11 Summary of Main Findings

<i>Dependent Variable</i>	<b>SOD*Post Welfare Reform</b>	
	<i>Coeff.</i>	<i>S.E.</i>
County-Level Analyses		
Number of HSOs, Per 1K	-0.01	0.03
Total Revenues, Per Capita	-1608.28***	387.41
Program Revenues, Per Capita	-692.21***	155.71
Tract-Level Analyses (inverse hyperbolic sine)		
Number of HSOs, Per 1K	0.00	0.02
Total Revenues, Per Capita	-0.07*	0.04
Program Revenues, Per Capita	-0.12***	0.04

\*\*\*p<0.01, \*\*p<0.05, \*p<0.10, Robust S.E.s

I also did not find much support for the racialized welfare argument; second order devolution was not associated with larger racial gaps in the number nor revenues of human service organizations near low-income neighborhoods. In general, there were smaller racial gaps in access for the post-reform era. This outcome points to important new lines of inquiry— do nonprofits complement or substitute government funding with respect to racial (in)equity? More analysis is needed to understand how nonprofits respond to racial disparities in access to government services. Do nonprofits perpetuate disparities generated by public policy, or mitigate them?

This paper has many limitations. Using the location from tax filings is not a perfect match for service locations. Some nonprofits may have multiple service locations, which is completely lost in this data set. Deploying a 3-mile radius hopefully recovers some of that, but it is by no means perfect. These tax filings also do not capture every nonprofit. In addition, I do not account for houses of worship nor for-profit providers that may fill in the gaps. The primary focus of this study is geospatial accessibility, which other scholarship has shown is important to utilization. However, I do not observe actual participation. I stress this point as it relates to the racial accessibility gap in particular; geospatial accessibility is but one measure of administrative burden. There are many other ways that access may be restricted on the basis of race.

Despite these limitations, this paper lays the foundation for further research regarding the impacts of governance on nonprofits and policy outcomes of anti-poverty programs, as I do not examine the mechanisms for the relationships I observe. For example, qualitative field work or a survey could reveal any differences in the ways welfare offices interact with their broader network on the basis of policymaking authority. Are social safety net network partnerships (contracts, grants, referrals) influenced by the amount of policymaking discretion? Do local officials prioritize measurable performance over other considerations in their partnerships? Do they favor larger organizations with more capacity over smaller ones, or vice versa? The modern social safety is complex and epitomizes the strengths and weaknesses of our federal system of governance; it is important that we continue to study the effects implementation have on accessibility.

## CHAPTER 3. DOES U.S. STATE GOVERNMENT PRIVATIZATION INCREASE SOCIOECONOMIC INEQUALITY?

### 3.1 Introduction

Government privatization today is ubiquitous. Policymakers have turned to businesses and nonprofits to provide everything from garbage collection to space shuttles. While scholars have debated the conditions that increase the likelihood a government will privatize and whether or not doing so achieves the outcomes advocates promote, there has been a relative dearth of research examining the effects these implementation choices have on state government employees. State governments can provide opportunities to those who are penalized in the labor market for their gender or race (Llorens et al. 2008, Lewis et al. 2018). In this article, we study the relationship between state government contracting and state government wages, focusing on its implications for various forms of inequality. While the amount of income inequality and social inequity we as a society are willing to tolerate is an ideological question, there are also important implications for democratic processes, political participation, economic growth, and stability. Previous research has highlighted the importance state labor market policy can have regarding income inequality (Kelly and Witko 2012, Hatch and Rigby 2015, Bucci 2018); we show that public management decisions are another potential source of influence.

This article contributes to the literature in several ways. I extend theory regarding the effects of administrative reforms on social inequality outcomes, and we increase our understanding of an understudied area of public management, state government contracting. The findings highlight the need for further study of the effects of state government privatization on broader social outcomes. The empirical work shows that state privatization of service delivery decreases the public sector wage premium, but that these effects are not driven by gender, race, or low-levels of educational attainment. The quality of implementation conditions these effects. We also find that privatization is associated with a lower public sector wage premium for middle-class workers.

### 3.2 Background: State and Local Privatization

According to public choice theory, contracting out public service delivery may produce cost savings and limit the opportunity for rent-seeking by politicians and bureaucrats, given the assumption that the market is sufficiently competitive and thus efficient (Bel and Fageda 2007). These gains in efficiency are also dependent on transaction costs associated with setting and enforcing contracts (Petersen et al. 2019). Alternatively, others have argued that the choice to contract is not solely a function of economic efficiencies, but also is the result of political calculus (Price and Riccucci 2005, Fernandez et al. 2008). While contracts are popular objects of study, privatization could also refer to vouchers, public-private partnerships, franchise rights, grants or subsidies, asset sales, or even the reliance on volunteerism and private donations (Auger 1999).

Much of the public administration literature relating to contracts in the U.S. setting has focused on the local context. Scholars have considered the importance of both service features, such as asset specificity, and government and market characteristics, including the level of competition, fiscal constraints, and political conditions, to the mode of delivery selected (Brown and Potoski 2003, Van Slyke 2003, Fernandez, Ryu et al. 2008, Bel and Fageda 2009, Levin and Tadelis 2010, Hefetz and Warner 2012, Rho 2013, Bel and Fageda 2017, Alonso and Andrews 2020). Evidence regarding the effects of local government contracting on cost savings and service quality is mixed (Perry and Babitsky 1986, Boyne 1998, O'Toole Jr and Meier 2004, Leland and Smirnova 2009, Bel et al. 2010, Rho 2013).

By contrast, the predictors of state contracting have not been as widely explored. According to Brudney et al. (2005), one of the few studies on the topic, perceived competition, potential for cost savings, and political context were not significant predictors of state contracting, but some service types were more prone to contracting overall, including social services, health, and transportation. The research regarding state privatization in the last two decades has focused more on policy outcomes. Fully privatized state child protective service systems are associated with increased likelihood of family reunification overall, but also increased likelihood of “aging out” of the foster care system (Elgin and Carter 2020). States have been increasingly turning towards



privately managed care organizations to administer Medicaid, which can lead to poorer health outcomes and does not necessarily result in cost savings (Duggan 2004, Aizer et al. 2007, Duggan and Hayford 2013).

While contracting is seen as a way to reduce costs through competition and economies of scale, choosing to rely on privatization can also have implications for other governance outcomes. Kelleher and Yackee (2009) assert government privatization provides “a new pathway for organized interests to lobby public managers,” as contracting increases interactions (and opportunities to lobby) with contractors. And by definition, contracting reduces management capacity and can “sacrifice key public interest values” and de-emphasize the democratic process (Brown et al. 2006). Entrepreneurship replaces values of fairness, representation, transparency, and accountability (Box 1999). Many local governments, particularly in rural places, are also faced with weak markets for services (Girth et al. 2012). Contracting nonprofits may limit competition by becoming specialized monopolists—the result being a reduction in public management capacity as public managers have diminished oversight capacity, institutional memory, and policy expertise relative to contractors (Van Slyke 2003). With incomplete contracts, contractors have incentives to cream skim clients or reduce quality of service provided (Hart et al. 1997); contractual performance measures can also create adverse incentives (Dias and Maynard-Moody 2007). However—these expectations do not always bear out. For example, in one study, for-profit JTPA training providers were more likely to serve disadvantaged clients and had better short-term employment outcomes (Heinrich 2000). And, jurisdictions may enhance competition by allowing for public and private bids and service redundancy (Miranda and Lerner 1995, Auger 1999).

### 3.3 Government Reform and Inequality: Theory

Privatization also impacts public sector employment. Federal employees report decreasing levels of agency performance and job satisfaction as outsourcing increases (Lee et al. 2019). In the Danish context, Petersen et al. (2021) show that transferring employment from the public to private sector decreases wages, and increases health and social insurance costs. In the realm of public schools, there is some evidence that teachers

in charter schools receive lower wages than in a traditional public school setting (Epple et al. 2016, Weber and Baker 2018, Buerger and Harris 2020). At the time of their review, Epple, Romano et al. (2016) show that charter school teachers tended to have less experience, education, and lower base pay than their public school counterparts. More broadly, as local governments increase their use of for-profit organizations to provide services, they decrease full-time employment while also increasing part-time public employment (Fernandez et al. 2007). Average hourly wages for part-time work were lower than for full-time work.

The relationship between public sector employment and privatization, particularly at the state level, has not been fully explored. This is important, because public sector employment has been used as policy instrument to combat poverty and various forms of inequality. For example, President Roosevelt created the Works Progress Administration during the Great Depression to get the unemployed back to work. Alesina et al. (2000) argues that public employment may also serve as a form of redistribution for cities. A commonly held value in public administration is that the government should “serve as a model employer” in addressing sex- and race-based gaps in employment and wages. Sex and minority representation in state governments is positively associated with public-private wage differentials, meaning in places where these groups make more in the public sector relative to the private *ceteris paribus*, they are more represented in the public sector workforce (Llorens, Wenger et al. 2008).

In connection to government reforms reducing the public workforce in the past few decades, Getha-Taylor (2019) writes, “... public employment has been described as a pathway to the middle class, and the contraction of the public service is connected to the shrinking middle class.” Indeed, the public sector has been insulated from the increase in wage inequality experienced by the private sector (Katz and Krueger 1991, Borjas 2002). Many public sector employees are also unionized, utilizing collective bargaining to increase wages and benefits (Anzia and Moe 2015). Businesses have a profit-maximizing objective function. Contracting “street-level” public services could result in downward pressure on these workers’ wages, particularly for those already experiencing marginalization in the workplace. The intuition of this assertion is supported by recent

work from Lewis, Pathak et al. (2018). They examine compensation parity between private sector and state/local government employees and find that workers with lower levels of education, women, and minorities make more in state and local government jobs relative to the private sector, while the opposite holds true for those with more education and in “higher ranking” occupations. Occupations where the public sector dominates, the relative pay is also higher than in the private sector according to their results. Their study stands out from others reviewed because they use the Census American Community Survey datasets, while others rely on the Current Population Survey from the Department of Labor, and they also account for the value of benefits in addition to wages.

Given this evidence, I argue that increasing levels of state government contracting, as a “market-based governance” reform in the new public management tradition (Lee, Lee et al. 2019), will exert, on average, a downward pressure on public employee wages. Unlike governments, which are accountable to their citizens and may have a number of objectives, private companies in contrast are primarily driven to maximize profit and minimize cost (Petersen, Andersen et al. 2021). In the 1990s, the movement to “reinvent” government emphasized the importance of reducing red tape, focusing on results, decentralizing and flattening organizational structures, and a market orientation in public administrative reforms—in a sense, making government act more like a private firm (Thompson and Riccucci 1998). Also described as New Public Management (NPM), this re-orientation of the public sector has produced a number of new innovations, the effects of which have been mixed (Alonso, Clifton et al. 2015).

*H<sub>1</sub>: Increasing reliance on state government contracting will be associated with a smaller average public sector wage premium.*

We expect these effects will be magnified by implementation quality. The possible pathways are twofold. If contracting for service delivery is perceived as successful, state employment may face greater competition from the private market, again decreasing the wage premium as job security becomes more tenuous. On the other hand, public managers may observe the success in implementation and seek to emulate within their own agencies, which may also be observed in lower state employment wage premiums. In either case:

*H2: The relationship between state privatization and public sector wages will be conditioned on the quality of implementation; increasing quality will amplify the negative relationship between public sector wages and state contracting.*

Public service privatization is often a means to pursue the goals of this movement; we suggest privatization reshapes public sector employment such that the secondary goals and values of public employment are de-emphasized in favor of running government more like a business. As a result, we argue that the positive effects public sector employment has on wages for workers who are female, racial minorities, and less educated will be eroded by increasing levels of state privatization.

*H3: The negative effect of state privatization will be magnified for female, racial minority, and less educated workers.*

While we anticipate that higher levels of contracting will be associated with a lower public sector wage premium on average, there is also the potential for differences by occupational class. The public sector has been insulated from the rising inequality in the private market. However, if contracting does not simply transfer service delivery to non-government organizations, but also reshapes public sector employment, we can expect public executives and other “top earners” to have increasing wages, while public workers at the other end of the distribution to experience lower wages—in effect, increasing income inequality for public sector employees.

*H4: State privatization will be associated with increasing income inequality—lower wages for lower- and middle-class workers in state government, and higher wages for upper-class workers in state government.*

We test these expectations using publicly available survey data, which is described in greater detail in the following section.

### 3.4 Methodology and Data

One of the reasons state government privatization has been understudied compared to local government contracting is relative data availability. Scholars have often relied on survey data purchased from the International City/County Management Association to study local government service delivery choices (Bel and Fageda 2017). Such data have not been generally available for state governments. There are new opportunities to study the state government context using the American State Administrators Project (ASAP) dataset, made recently available to the public (Yackee and Yackee 2021). We use the responses to survey questions about contracting in respondents' agencies in 1998, 2004, and 2008 to construct budget-weighted measures of state privatization. To assess the relationship between these management choices and wages, we use Current Population Survey Annual Social and Economic Supplement microdata available through the Integrated Public Use Microdata Series (IPUMS), and other publicly available data sources, to construct a pooled cross-sectional dataset spanning the years 1998 to 2012.

We use OLS regressions to examine the relationship between state privatization and public employee wages, controlling for individual-specific characteristics, state characteristics, and fixed effects for industry, occupation, states, and years. The dependent variable of interest is the natural log of wage income. We test our hypotheses using the following:

$$(1) \ln(wage)_{its} = Public_i + StatePriv_{st} + Public_i * StatePriv_{st} + WorkerCharacteristics_i \\ + Industry_i + Occupation_i + StateFeatures_{st} + StateDummy_s \\ + YearDummy_t$$

$$(2) \ln(wage)_{its} = Public_i + StatePriv_{st} + ImpQual_{st} + Public_i * StatePriv_{st} + ImpQual_{st} \\ * Public_i + ImpQual_{st} * StatePriv_{st} + ImpQual_{st} * Public_i * StatePriv_{st} \\ + WorkerCharacteristics_i + Industry_i + Occupation_i + StateFeatures_{st} \\ + StateDummy_s + YearDummy_t$$

The first equation is used for the first, third, and fourth hypotheses. We compare public sector workers to private sector workers using a dummy variable,  $Public_i$ , with occupation ( $O_i$ ) and industry ( $D_i$ ) dummy variables. We interact a budget-weighted measure of privatization,  $StatePriv_{st}$ , with public employment status. We restrict the

data on individual characteristics to explore the relationship between state privatization, wages, gender, race, educational attainment, and occupational class. Lastly, we construct an index of privatization quality from ASAP survey responses to assess the moderating effects of implementation on public sector wages, which is presented as a three-way interaction in equation 2 to test the second hypothesis. A full list of the control variables and their sources can be found in the appendix.

#### 3.4.1 Measurement: State Privatization

The American State Administrators Project survey was completed by leaders of state public agencies in all 50 states every four years from 1964 to 2008. The topics covered included respondents' personal and professional backgrounds, agency operations, and intergovernmental relations. While some questions were asked in every iteration, others came and went. The 1974 wave had the most responses—1,587. However, the response rate declined in later years. For example, the most salient question to this study was answered by 1,127 respondents in 1998, 908 in 2004, and 676 in 2008. The falling response rate raises questions about the representativeness of responses, particularly for 2008. Telephone interviews were conducted with a small sample of non-respondents to verify that they did not systematically differ using a smaller battery of “attitudinal and attribute” survey questions (Yackee and Yackee 2021). After analysis, the survey investigators concluded that the survey respondents were representative. However, it does not appear that this check included questions about contracting. This survey dataset is reported to represent the “full range of administrative agencies active across the 50 states” (Palus and Yackee 2016).

This study relies on a series of questions regarding agency contracting. In the years 1998, 2004, and 2008, the ASAP survey asked, “Currently, about what percentage of your agency’s budget is allocated to contracting for delivery of services to the public?” Respondents selected one of the following options—0 percent, 5 percent or less, 6 to 20 percent, 21 to 40 percent, or over 40 percent. Follow-up questions asked about the types of organizations agencies contracted, how much the level had changed, and reported quality ratings for service costs, responsiveness to the public, competition, public satisfaction, and ability to monitor.

The amount of contracting varies over time by agency function (figure 3.1). Some of the median values are fairly stable over time (ex: non-fiscal staff, natural resources), and there is not a clear upward trend as we might expect for any of these functional areas. We observe highest median values for income security/social services, transportation, and health. In figure 3.2, the overall mean and reported agency budget-weighted mean for agency privatization are shown. In both cases, the highest average levels of outsourcing were reported in 1998. There was an average decrease in 2004, which stabilized in 2008. The budget-weighted averages are consistently higher than the raw mean in all three survey waves. Drilling down by state, the vast majority saw decreases in contracting from 1998 to 2008, following the overall trend (Appendix). It is very difficult to validate these longitudinal trends against other data sources, as this is the only publicly available national survey that has asked state officials to estimate the prevalence of contracting in their agency over time. However, these trends mirror those in local governments; Warner and Hefetz (2012) note the relative stability of contracting over time in municipal governments, with local privatization at its peak in 1997.

Figure 3.1 Reported State Agency Privatization Over Time, By Agency Function

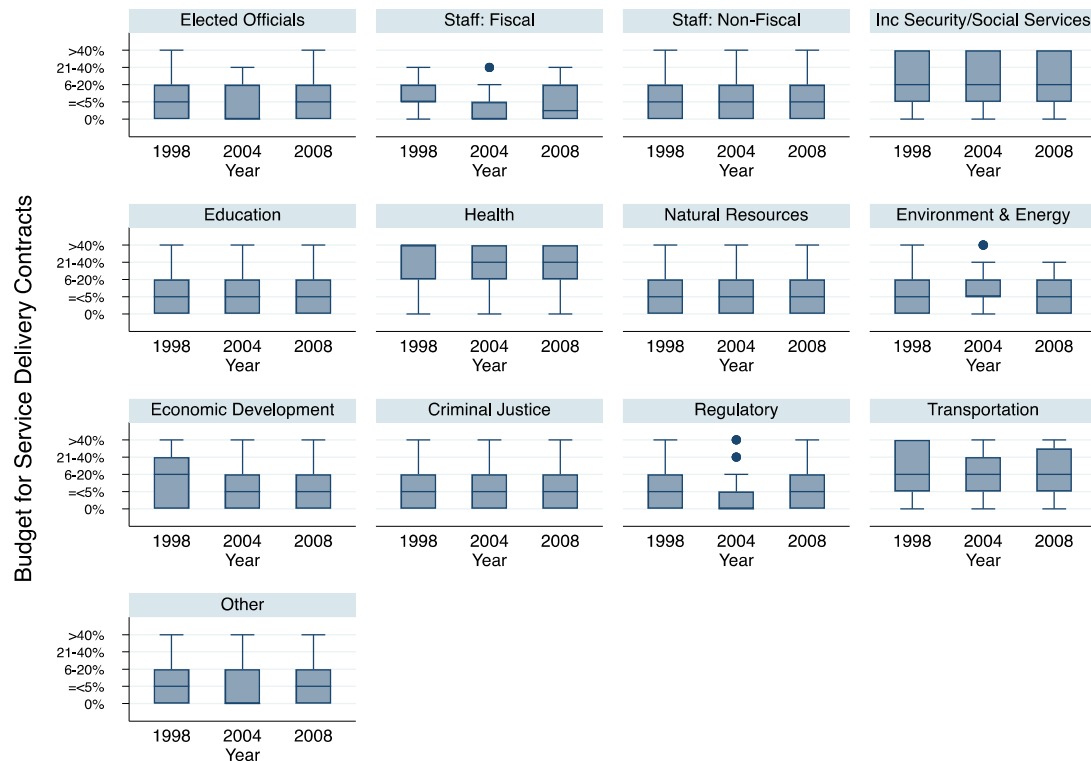
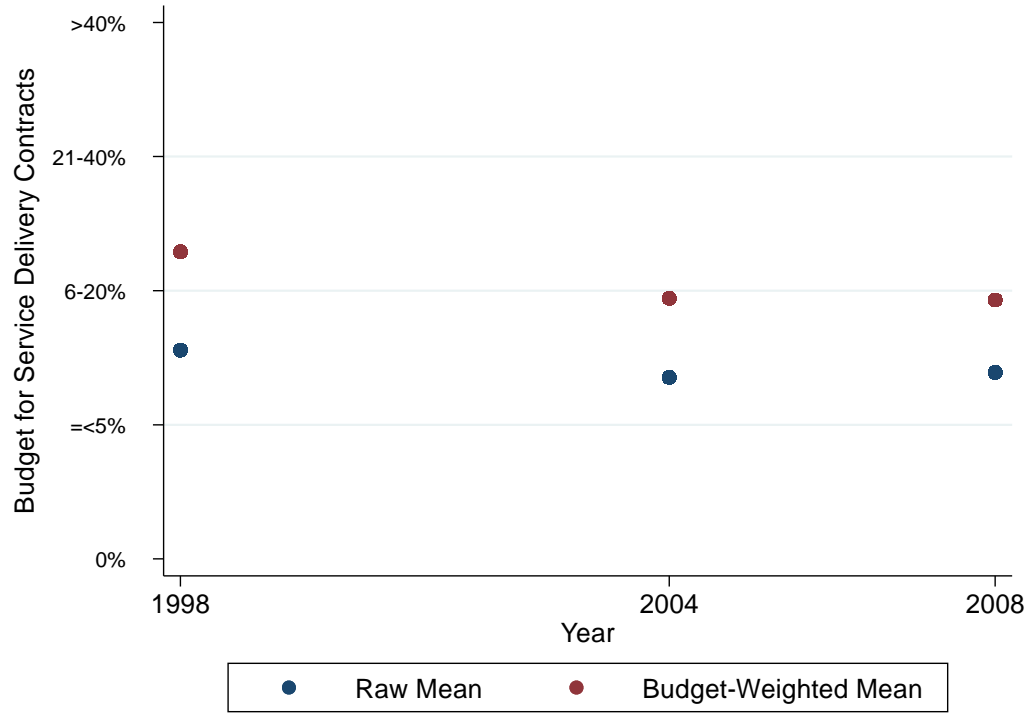


Figure 3.2 Average Levels of Reported State Contracts



We use these survey responses and annual state financial data from the Census Annual Survey of State Government Finances to construct annual budget-weighted measures of state privatization. For each state, we match the respondents' reported agency function (e.g. education, health) to budget functions where possible. For the administrative functions (fiscal, non-fiscal staff, elected officials, other), we constructed a "general government" category. If there was more than one response within these categories, we used the respondents' reported agency budgets to weight a functional average. We then multiplied each value by the corresponding budget function's share of the state budget. More succinctly:

$$StatePrivatization_{st} = \sum A_{fst} * B_{fst}$$

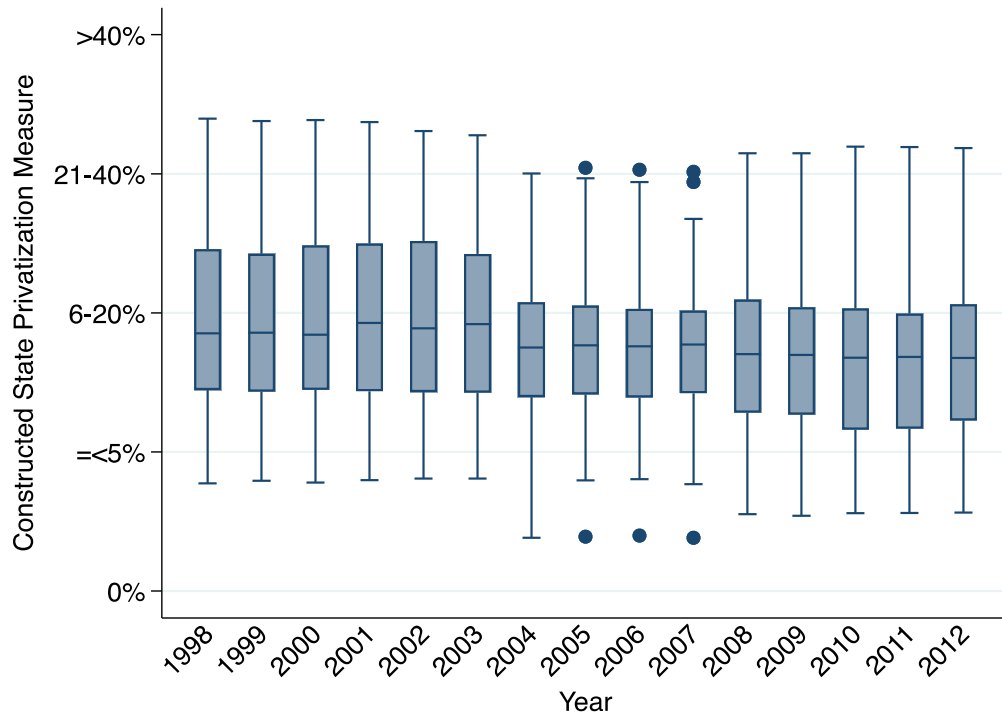
$S$  and  $t$  denote state and year, and  $f$  is budget function.  $A$  is the weighted average response to the ASAP survey question for each budget function, which is multiplied by  $B$ , the functional share of the state government's budget. We carried forward the survey responses from 1998 to 2003, 2004 to 2007, and 2008 to 2012 to create annual measures



using updated annual budget. If a budget area did not have a response for a given year, the overall state average response was used.

Across all years, the mean value for the budget-weighted state privatization measure is 1.82, which is approaching the survey response of “6 – 20 percent” of the agency budget. Figure 3 shows that there is a slight decrease in the constructed privatization measure over time, which mirrors the trends observed in the raw survey data as discussed above. However, there continues to be wide variation; there are states in 1998 and 2012 with scores below the 5 percent threshold and above the “21 – 40 percent” mark as well.

Figure 3.3 State Privatization Over Time



Survey respondents were also asked to report the impacts (recoded as negative one – decrease, zero – no change, and one – increase) of contracting on service quality, cost, responsiveness to the public, and the agency’s accountability. Administrators were also asked to rate competition, public satisfaction, and their ability to monitor public contracts using a five-point scale, with one being “very low” and five being “very high.” I used the survey responses to compute a budget-weighted state average for each year,

which we then use in a factor analysis to create an index of privatization implementation quality. Below are the summary statistics. States on average report contracting somewhat increases quality, public responsiveness, and agency accountability, but has a negligible impact on cost. Respondents rated competition, public satisfaction, and the agency's ability to monitor as slightly higher than "moderate."

Table 3.1 Privatization Implementation Quality Descriptive Statistics

<b>Variable</b>	<b>Mean</b>	<b>Range</b>
Quality	0.54	-0.60 – 1
Cost	0.05	-0.98 – 0.99
Public responsiveness*	0.53	-0.90 – 1
Agency accountability*	0.24	-0.94 – 1
Competition*	3.21	1.13 – 4.94
Public satisfaction*	3.44	1.64 – 4.58
Agency's ability to monitor*	3.45	1.58 – 4.95

*Budget-weighted state averages.*

*\*Only reported for 2004, 2008 surveys*

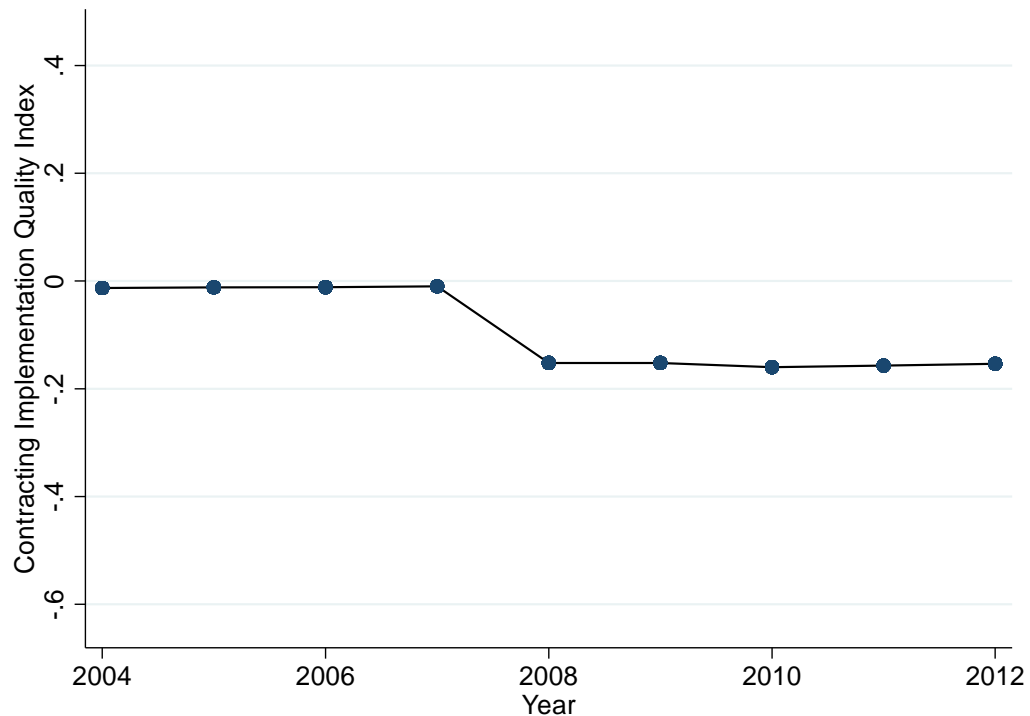
We then use factor analysis to construct an index using the raw survey responses, which produced one factor with an eigen value greater than one. All of the factors, except competition and cost, had loadings greater than the absolute value of 0.3; we excluded competition and cost from the final index as a result. To further assess the appropriateness of the final index, we compute Cronbach's alpha. The scale reliability coefficient is 0.698. As a rule of thumb, public administration scholars target a value greater than 0.70, but it is not an exact science (Cho and Kim 2015, Berman and Wang 2016). Given how close this value is to 0.70 and the lack of alternative measures, we argue it is an acceptable level of reliability.

Table 3.2 Privatization Implementation Quality Factor Analysis

<b>Index Components</b>	<b>Factor 1</b>	<b>Uniqueness</b>
Quality	0.68	0.53
Cost	-	-
Public responsiveness	0.67	0.58
Agency accountability	0.51	0.74
Competition	-	-
Public satisfaction	0.53	0.72
Agency's ability to monitor	0.44	0.80

At this stage, we use the predicted index values for each individual survey respondent to produce state-level values using the same budget-weighted procedure used for state privatization. Because many of these measures are only available from the 2004 and 2008 surveys, the analysis years using this index are similarly restricted. In figure 3.4, the connected line shows the average index value; the gray shaded area represents the standard deviation. We see that the level of average level of quality decreased at the time of the 2008 survey, relative to the previous wave.

Figure 3.4 Budget-Weighted Implementation Quality Index Over Time



### 3.4.2 Measurement: Wages and Inequality

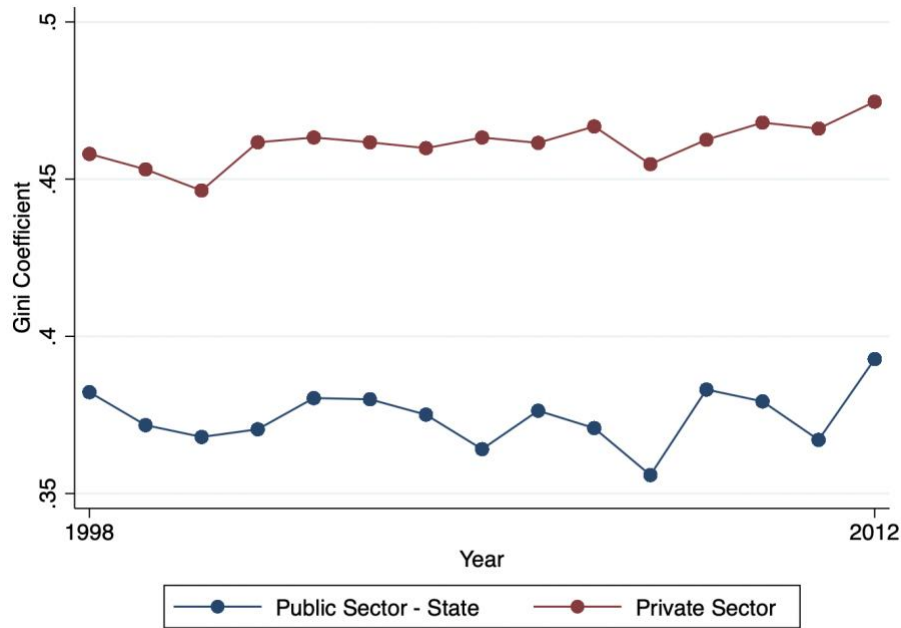
Scholars studying public-private sector wage differentials are faced with a number of measurement choices that will have large impacts on the final analysis. The main issue at hand is the degree of comparability in occupation and industry between workers—for example, is it appropriate to compare the wages of a municipal analyst to that of a production line worker in a car factory? Because we are studying contracting—transferring service delivery from state employees to private counterparts—it is appropriate to limit our sample to occupational classes that are comparable to those in the

public sector. To achieve this, we identified the occupations of the state government employees in the ASEC survey responses and eliminated private sector responses that did not fall within those same categories. I also removed federally-employed, locally-employed, unemployed, retired, and self-employed responses.

To study the impact of privatization on public wages, I use the natural log of earned income as the dependent variable in all OLS specifications and restrict the sample to particular demographic and economic subgroups to further assess relationships, such as female, Black, Hispanic, and workers with a high school diploma or less. In all specifications, the unit of observation is the individual worker. I include demographic controls from the ASEC survey (ex: number of children) and state level factors (ex: unemployment rate). I apply ASEC-specified population weights in all OLS regressions, as well.

I also consider the effect of state privatization on income inequality. One option would be compute a state-level measure of inequality, such as the Gini coefficient, for public and private sector employment to use in a regression. There are two problems with this option. Computing a state-year-public sector Gini relies on a relatively small number of observations in some states; and this procedure collapses the number of observations down from nearly a million to only 750 and erases much of the fine-grain detail. For descriptive purposes, I did use our data set to compute the national Gini coefficient, broken out by sector and year (figure 3.5). State employees had less wage inequality than their counterparts in the private sector. Inequality has risen steadily in the private sector, but in a much noisier fashion for the public sector.

Figure 3.5 Wage Inequality by Sector



Instead, I categorize occupation classes into upper-, middle-, and lower-class buckets. I then use the same OLS framework to study trends in each of these groups at the individual worker observation to understand how state privatization may relate to the distribution of income overall for state government workers. If state privatization is associated with downward trends for public sector wages in the lower and middle class categories, or upward trends in the upper class—this would suggest greater income inequality overall. Using either approach requires caution in interpreting results—the Current Population Survey replaces wage earnings above \$999,999 with values generated from demographic characteristics to protect respondents’ privacy, so inequality will be understated.<sup>9</sup>

Table 3.3 Inequality by Occupational Class

Lower Class	Middle Class	Upper Class
Health support services (ex: nursing aides)	Business, financial operations	Management
Food service	Life, physical, social science	Computer science, math
Building maintenance	Community and social services	Architecture and engineering
Personal care	Education	Legal
Farming, fishing, forestry		Physicians and surgeons

<sup>9</sup> More information about this process can be found here: [https://cps.ipums.org/cps/topcodes\\_tables.shtml](https://cps.ipums.org/cps/topcodes_tables.shtml)

Table 3.3 (continued)

Transportation (ex: bus driver)	Arts, sports, media Healthcare (excluding physicians) Protective services Sales and office Construction Repair and maintenance Production
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### 3.5 Results

In the first regression reported in table 3.4, I show that state employees have 2.5 percent wage premium relative to private sector peers, controlling for a battery of personal and structural characteristics, including dummy variables for industry, occupation, state, and year. Women overall in this specification earn 20.5 percent less than men. White people earn 2.9 percent more than people of color, and Hispanic people also earn 4.5 percent less. Employees with a high school diploma or less schooling earn 15.5 percent less than others, *ceteris paribus*. I present these results as a point of reference for the results in the second regression in this table, which includes the state privatization measure and an interaction term for state privatization and public sector employment. In this specification, state employees earn 6 percent more than private workers, but that premium is diminished by reported state contracting, which has a positive relationship with the natural log of wages. Taking the effects of the interaction and base terms for state privatization and public employment together, for every additional point on the continuous privatization scale, state employees see an additional 0.8 percent decrease in wages compared to the private sector, holding all else constant. These estimates support the first hypothesis—higher levels of state government privatization are associated with a lower public sector wage premium on average.

Table 3.4 State Privatization and Public Sector Wages

	(1) Natural Log of Wages	(2) Natural Log of Wages
State Government Employment	.02467*** (.005498)	.05957*** (.0113)

Table 3.4 (continued)

State Emp*Privatization		-.01946*** (.005345)
State Privatization		.01118*** (.002149)
Female	-.2051*** (.002329)	-.2052*** (.002329)
Hispanic	-.04515*** (.002996)	-.0451*** (.002996)
White	.02863*** (.00258)	.02862*** (.00258)
Ed Attain: <=HS Diploma	-.1546*** (.002103)	-.1546*** (.002103)
Ed Attain: Grad School	.2137*** (.003796)	.2137*** (.003796)
Immigrant	-.04521*** (.002917)	-.04513*** (.002917)
Veteran	-.0008837 (.003423)	-.000861 (.003423)
Married	.09613*** (.002)	.09614*** (.002)
Number of Children	-.001547 (.0008254)	-.001556 (.0008254)
Age	.08247*** (.0005199)	.08248*** (.0005198)
Age^2	-.0008258*** (6.108e-06)	-.0008259*** (6.107e-06)
Usual Hours Worked	.02461*** (.0001188)	.02461*** (.0001188)
Health Status	-.04942*** (.001027)	-.04944*** (.001027)
Occupation Indicators	Y	Y
Industry Indicators	Y	Y
<i>State Economic, Demographic Characteristics</i>		
Interest Group Density	2.500e-06 (3.736e-06)	3.903e-06 (3.740e-06)
Union Density	.005024*** (.001067)	.005048*** (.001067)
Citizen Ideology	-.0003813 (.0002433)	-.0003781 (.0002433)
State Govt Ideology	-.0001439** (.00005561)	-.00009724 (.00005647)
State Unemployment	-.006524*** (.001173)	-.005628*** (.001185)

Table 3.4 (continued)

State Population	7.210e-10 (2.274e-09)	9.981e-10 (2.275e-09)
State % Urban	-.2223** (.08391)	-.2254** (.08392)
State % White	.2267 (.1735)	.2597 (.1737)
State % Hispanic	.6922** (.2329)	.7274** (.2331)
State % College	.6986*** (.2022)	.6605** (.2023)
State Govt Revenue (pc)	.006768*** (.001573)	.006657*** (.001573)
State Indicators	Y	Y
Year Indicators	Y	Y
Observations	946,304	946,304
$R^2$	0.538	0.538

Standard errors in parentheses \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

Are the effects of privatization affected by the quality of implementation? To explore these relationships, I introduce a three-way interaction between privatization, the implementation quality scale, and public sector employment using the same set of controls. Because coefficients for three-way interactions are challenging to interpret (table 3.5), I computed the net privatization coefficient for given levels of implementation quality to present graphically at given levels of implementation quality.

Table 3.5 Moderating Effects of Privatization Implementation Quality on Wages

	(1) Natural Log of Wages
State Employment	.03997** (.01395)
Implementation Quality Scale	.01443 (.01343)
State Emp*Quality Scale	.08839** (.03111)
State Privatization	.007457* (.00329)
State Emp*Privatization	-.0107 (.007123)
Quality Scale*Privatization	-.01294 (.007293)
State Emp*Privatization*Quality Scale	-.0522**



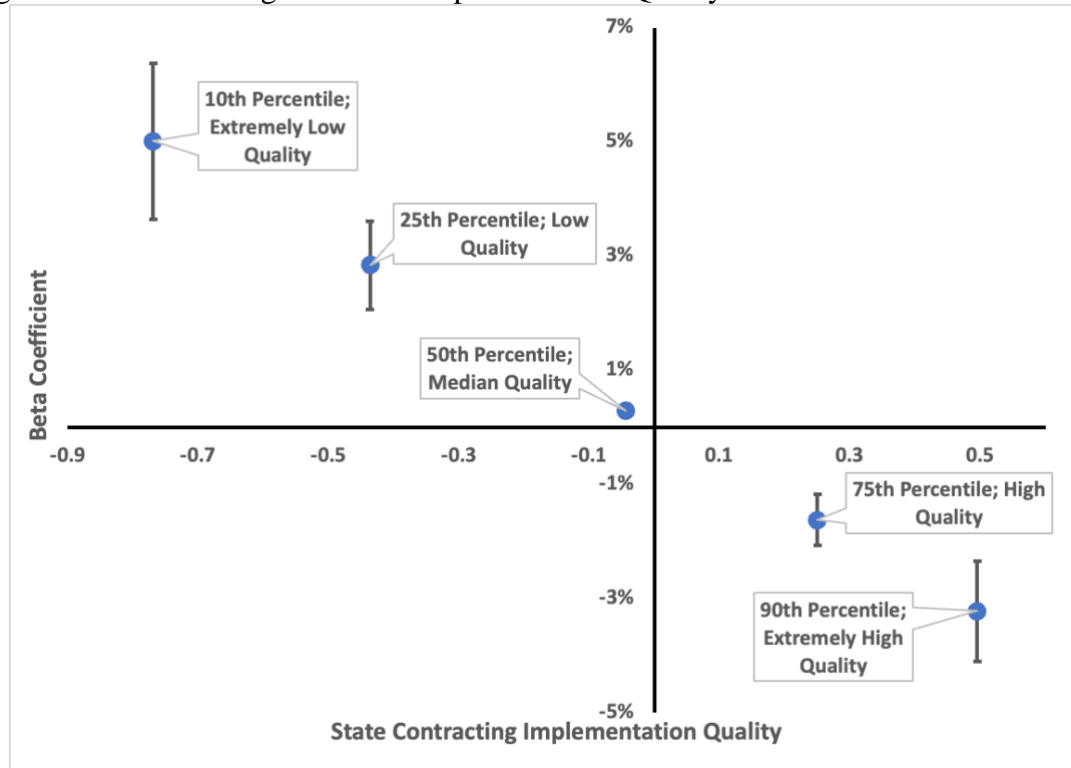
Table 3.5 (continued)

	(.01744)
Employee Characteristics	Y
State Economic, Demographic Characteristics	Y
Year Indicators	Y
Observations	593,932
$R^2$	0.540

Standard errors in parentheses \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

Figure 3.6 reports the net beta coefficient for state privatization, conditional on the implementation quality scale on public sector employee wages. These effects are statistically significant across the board ( $p < 0.001$ ). The y-axis is the beta coefficient for state privatization and the x-axis represents a given value of implementation quality. At a low level of contract quality (25<sup>th</sup> percentile,  $x = -0.44$ ), state government employees maintain a wage premium of roughly 2.8 percent. However, as the quality scale increases (beta estimates to the right lower quadrant), that coefficient is no longer positive. At a high level of implementation quality (75<sup>th</sup> percentile,  $x = 0.25$ ), the net coefficient is -1.6 percent. These findings support the second hypothesis—the perceived implementation quality conditions the effect of privatization. States where the implementation is perceived as well-executed have lower public sector wages.

Figure 3.6 Conditioning Effects of Implementation Quality on State Privatization



I then restrict the data on the basis of race/ethnicity, gender, and educational attainment to understand whether or not these relationships differ on those social equity dimensions. Taken together, I do not find support for the third hypothesis. Black workers in state government positions earn 3 percent more than those in the private sector, but state government privatization does not have a statistically significant relationship with earnings (table 3.6). By contrast, Hispanic workers in the naïve specification (column 3) do not have the same public sector pay increase. When state contracting is considered, a similar dynamic to that of the overall specification appears—Hispanic state government employees earn 13.4 percent more, which is diminished by state privatization.

Table 3.6 State Privatization, Public Wages for Racial and Ethnic Minority Workers

	(1) <b>Black</b>	(2) <b>Black</b>	(3) <b>Hispanic</b>	(4) <b>Hispanic</b>
State Employment	.03019* (.01362)	.06413* (.0295)	.03165 (.01766)	.1339*** (.0317)
State Emp*Privatization		-.01868 (.01424)		-.05819*** (.01536)
State Privatization		.01269		.0139*

Table 3.6 (continued)

		(.00739)		(.006779)
Employee Characteristics	Y	Y	Y	Y
State Economic, Demographic Characteristics	Y	Y	Y	Y
Year Indicators	Y	Y	Y	Y
Observations	94,114	94,114	151,096	151,096
$R^2$	0.442	0.442	0.418	0.418

Standard errors in parentheses \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

Women in the public sector earn 5.1 percent more than their private sector counterparts (table 3.7), which is more than double the magnitude for that of the overall specification. As state privatization increases, women in general have higher wages, and the premium for state employment increases to 7.2 percent. However, the interaction between the two is insignificant. Workers with a high school diploma or less do not benefit from state government employment, *ceteris paribus* (table 3.8). Both of these findings are counter to what others have found. When accounting for privatization, workers with low levels of education in states that have more state privatization do earn slightly more—0.9 percent for every additional point on the privatization scale.

Table 3.7 State Privatization and Public Wages for Female Workers

	(1) Natural Log of Wages	(2) Natural Log of Wages
State Employment	.05089*** (.006761)	.07188*** (.01397)
State Emp*Privatization		-.01173 (.006731)
State Privatization		.009806** (.00301)
Employee Characteristics	Y	Y
State Economic, Demographic Characteristics	Y	Y
Year Indicators	Y	Y
Observations	461,801	461,801
$R^2$	0.518	0.518

Standard errors in parentheses \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

Table 3.8 State Privatization, Public Wages for Workers Without Higher Education

	(1) Natural Log of Wages	(2) Natural Log of Wages
State Employment	.01312 (.01221)	.002739 (.0275)
State Emp*Privatization		.005677 (.01242)
State Privatization		.009066** (.003322)
Employee Characteristics	Y	Y
State Economic, Demographic Characteristics	Y	Y
Year Indicators	Y	Y
Observations	407,965	407,965
$R^2$	0.490	0.490

Standard errors in parentheses \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

The last table shows results for six different regressions to test the fourth hypothesis; each is restricted to workers that fall in occupation classes identified as upper, middle, and lower class using the categorization described in the previous section. Upper-class workers earn less in public sector settings according to the baseline results in the first column; however, higher levels of state privatization are associated with lower wages for this group. Given earnings over a million dollars are recoded, these results could be biased and should be treated with caution. The middle-class results are more straightforward—public sector employment is associated with a 4.5 percent increase in wages in the first regression; in the second, I see that there is downward pressure on this premium as state privatization levels increase. Taking the base and interaction terms together, for every additional point increase in state privatization, there is a 1.2 percent decrease in the wage premium for middle class workers. With respect to earnings for the lower class specifications, there is no statistically significant relationship for the variables of interest. These findings provides some weak support for the fourth hypothesis—there are occupational class-based differences, namely with the middle class.

Table 3.9 State Privatization, Public Wages, and Class

	(1) Upper Class	(2) Upper Class	(3) Middle Class	(4) Middle Class	(5) Lower Class	(6) Lower Class
State Employment	-.06756*** (.01376)	.002676 (.02566)	.04462*** (.00646)	.08927*** (.01322)	.02019 (.01459)	-.02296 (.03431)
State Emp *Privatization		-.03896** (.01204)		-.02485*** (.006235)		.02429 (.01686)
State Privatization		.00733 (.004721)		.01259*** (.002691)		.008002 (.005044)
Employee Characteristics	Y	Y	Y	Y	Y	Y
State Economic, Demographic Characteristics	Y	Y	Y	Y	Y	Y
Year Indicators	Y	Y	Y	Y	Y	Y
Observations	150,385	150,385	583,969	583,969	211,950	211,950
$R^2$	0.407	0.407	0.471	0.471	0.444	0.444

Standard errors in parentheses \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$ 

### 3.6 Discussion and Conclusion

Taken together, the results support the argument that state government privatization erodes the public sector wage premium, all else being equal. The relationship is moderated by the quality of implementation—if states are more successful in achieving their goals with respect to New Public Management principles, the public wage premium is particularly affected. Privatization does not just transfer service delivery to private workers; it seems to make public employment look a little bit more like the private sector.

However, there is little support for the idea that traditionally marginalized groups are disproportionately affected compared to their privately employed peers. Privatization does erode the wage premium for Hispanic public sector workers, compared to their private sector counterparts. There seems to be little relationship between the wages of state government workers and privatization for those who are female, Black, or less

educated. This set of findings runs counter to what I might expect from other scholarship (Llorens, Wenger et al. 2008, Lewis, Pathak et al. 2018).

Lastly, what are the implications for income inequality? Without privatization in the specification, upper class workers in the public sector earn 6.8 percent less and middle-class workers earned 8.9 percent more. Introducing state privatization into the model reveals that upper class workers still earn less, but the effect is much more striking for public middle-class workers—at very high levels of privatization, their wage premium is eliminated. Because income is top-coded in this data set, the findings must be interpreted with caution for the upper class cohort. Nonetheless, privatization as an NPM tool does seem to be associated with lower levels of income for middle-class workers. Getha-Taylor (2019) argues that “shrinking” government also shrinks the middle class; our analysis highlights an additional pathway through which that might be the case.

This analysis has several limitations. This work is purely associational and uses imprecise survey categories to approximate the main variables of interest regarding the amount of quality of contracting; future studies could examine the specific cases of mass privatization and its impact on public sector employees using non-survey methods. The survey also is limited by time; the contracting questions were only asked in three iterations. And, this study does not explore the relationship between privatization and other facets of social equity, such as representation, both writ large and in positions of authority within an agency. Despite these limitations, this article contributes to public administration scholarship in several important ways—it uses a newly public dataset to advance our understanding of state privatization at a national level over time, and it studies the impact of an important New Public Management innovation on social equity and inequality in government. This article also lays the groundwork for research examining the impact of other NPM reforms, such as decentralization and flattening hierarchies, on social equity outcomes in the public sector.

## CHAPTER 4. THE IMPACTS OF SCHOOL DISTRICT CONSOLIDATION ON RURAL COMMUNITIES: EVIDENCE FROM ARKANSAS REFORM

### 4.1 Introduction

Governance in the United States is increasingly decentralized as a result of New Public Management reforms to increase competition and local responsiveness. However, public school districts are an exception—in the past fifty years, the number of public school districts has declined drastically in the United States, from over 40,000 to under 14,000 (de Brey et al. 2019). Faced with demographic pressures, many state governments have used incentives and sanctions to induce rural school districts in particular to consolidate, effectively centralizing administration, in an effort to increase economic efficiency and performance (Gordon and Knight 2006, Gordon and Knight 2008). Yet, very little research focuses on the unique problems faced by rural policymakers with regards to public education and its reform, even as a quarter of America’s schoolchildren are attending schools in towns or rural places today (de Brey et al. 2019). In this paper, we estimate the impact of school district consolidation on rural communities using a novel policy change in Arkansas. Rural schools are not only educating students; they are a source of economic activity, local identity, culture, and civic engagement (Schafft 2016). We argue that when districts are forced to consolidate, it signals the removal of residential amenities leading to loss of population and housing values.

Using a propensity score matched difference-in-differences research design which capitalized on a state policy to induce consolidation, we find that district reorganization led to a population reduction equivalent to 13 to 15 percent pre-reform levels. Furthermore, communities of color were disproportionately impacted—for every ten percentage point increase in racial minority population share, there is an associated 38-person reduction in population. The inducement to consolidate also led to a 0.19 to 0.25 reduction in community schools and \$1,300 reduction in assessed property values. Taken together, these results indicate that residents of rural communities value local governance of public education, and when faced with the potential loss of local responsiveness, “vote with their feet.”

## 4.2 The Ongoing Decentralization Debate

Scholarly and political debate regarding local government fragmentation, and possible reforms aimed at reducing it, often revolve around the “optimal” number of local governments. Proponents of fragmentation often invoke the Tiebout hypothesis—interjurisdictional competition between many local governments for residents promotes economic efficiency and provides for a plurality of preferences in local goods and services (Howell-Moroney 2008). More jurisdictions in a decentralized system means more choice. Advocates for consolidation point out that self-sorting can have negative consequences, such as creating and perpetuating racial segregation and income inequality; consolidation could increase economies of scale in public administration, decrease racial segregation, and enhance economic development (Leland and Thurmaier 2005, Jimenez and Hendrick 2010).

The debate surrounding school district consolidation echoes these broader themes. State financial incentives, economies of scale, distance, and the degree of match between districts regarding educational spending preferences are all important factors to rural districts contemplating consolidation (Gordon and Knight 2006). Consolidation could reduce the burden of fixed administrative costs and allow for more specialized classes and facilities, but conversely, there are also concerns larger districts could be less responsive to their local communities (Leach et al. 2010).

However, district reorganization has also been viewed as a policy tool to professionalize and “urbanize” rural schools, with the goal of reducing the out-migration of high-achieving young adults from their rural farm communities (Cubberley 1922, Theobald 2021). Moreover, these century-old anxieties were racialized—“If the schools offered students an urban education, they could stay home instead of moving to cities where the racial stock of the country threatened to be diluted” (Theobald 2021). The public administration literature contextualizes this by placing it squarely in the ongoing debate regarding the interplay between politics and administration (Demir and Nyhan 2008). In local government, 20<sup>th</sup> century institutional reforms such as the introduction of



city manager roles, at-large representation, and nonpartisan elections were intended to improve the rationality and efficiency of government—moving away from politics based on “community cleavages—class, ethnic, racial, or religious” (Morgan and Pelissero 1980). Scholars have reasoned that the reforms aimed at enhancing expertise and professionalism in administration may come at the expense of political responsiveness, particularly for minority groups within a community. For example, such administrative reforms are associated with lower levels of political participation via elections (Hajnal and Lewis 2003), providing support for the notion that reforms aimed at de-politicizing and professionalizing government may also undermine democratic processes. School districts, like municipalities, are local governments leveraging institutional reforms to increase their expertise, the theory of action being increased administrative capability would lead to service quality rivaling that of urban centers.

While the phenomenon is rampant, there have been relatively few studies of school district consolidations in the education policy and public administration literature; the existing scholarship has focused on student and district financial outcomes. For example, Leach, Payne et al. (2010) found consolidation led to modest improvements in test scores, but that the effects differed by student socioeconomic status and may have been driven by simultaneous funding changes. A recent working paper leverages the same Arkansas state policy cutoff that we use here to examine student achievement using a regression discontinuity design, concluding there is little evidence of impacts on English/language arts and math test scores (McGee et al. 2021). Duncome and Yinger (2007) found that for a given level of performance in New York state, rural school district consolidation led to lower operating costs by achieving economies of scale. In their estimations using 12 consolidations, they predict savings on the order of more than 20 percent for the consolidation of two 300-student districts, with declining savings as districts get larger. Rural district consolidations in Iowa were not associated with decreases in quality measures— pupil-teacher ratio, mean school size, dropout rate (Gordon and Knight 2008).

In examining alternative outcomes, scholars found that district and school size do impact student outcomes. In Texas, Jones, Toma, and Zimmer (2008) found that larger

districts are associated with lower average daily attendance, as individual schools have less direct financial incentive to improve student attendance than in a smaller district. Increasing school size leads to reductions in student achievement as there is less competitive pressure with fewer schools to compete for students, more bureaucracy, and potentially less parental engagement as larger schools can be intimidating for parents (Eberts et al. 1990, Borland and Howsen 1992, Brasington 1999, Hoxby 2000, Driscoll et al. 2003, Foreman-Peck and Foreman-Peck 2006, Kuziemko 2006). When school districts consolidate, school closures are often expected by constituents, as district leadership may choose to close schools for both academic or financial reasons (De la Torre and Gwynne 2009). Closures can have short term negative impact on scholastic achievement (Engberg et al. 2012, Brummet 2014, Beuchert et al. 2018), but the effects of school closures also extend beyond the immediate class cohort—post-closure cohorts can benefit high performing students (Bifulco and Schwegman 2020).

The limited scholarship that does consider the effects of school district reorganizations on local communities has often relied on qualitative or cross-sectional quantitative methods. Sell and Leistritz (1997) use a mixed-methods approach including descriptive statistics, interviews, and a survey of parents in eight consolidated North Dakotan districts to assess population loss, civic participation, quality of life, and economic activity. The survey respondents report a decline in civic organization participation and quality of life after consolidation for communities “losing” their school, relative to respondents in “host” communities. Brasington (2004) studies the relationship between 1991 home sales in Ohio with district consolidation; they find that consolidation is associated with a decrease in housing value. However, neither of these papers addresses the endogeneity of reorganization—integrating two or more districts is often the result of declining populations. They also rely on cross-sectional or self-reported assessments at a single point in time. In sum, this literature provides insights into the theoretical debate surrounding school consolidation as well evidence on the effects of costs and performance, but it does not fully consider the effects school consolidation has on communities over time accounting for selection bias. In this paper, we address this void in the literature.

### 4.3 Theoretical Framework

There is a dearth of scholarship measuring the impacts these reforms, directly or indirectly (district size, school size, and school closure), have on their local communities outside of the classroom. We argue that district reorganization changes the calculus for rural populations' residential choices, despite the theory of action held by some reformers in pursuit of increasing desirability through professionalization. Local schools are a highly visible cornerstone of small towns; they are key actors in community development not only as employers and educators, but also as civic, cultural, and economic institutions, around which communities can organize and progress towards shared community development goals (Schafft 2016). Schools provide a conduit for the creation of social capital, which may be important to economic mobility and public administration performance (Coffé and Geys 2005, Andrews 2012, Chetty et al. 2018). Fischel (2009) argues that the spillovers from the “network of social capital that is fostered by public schools” benefits adults without school-age children within a community.

Consolidation could signal community disinvestment in a publicly provided service, as it could lead to the local school closing, which is important to local life (Schafft 2016). When districts reorganize to achieve economies of scale, the perceived risk of school closure increases. From an economic perspective, reorganization signals a reduction in the local amenities by both removing an important institution and increasing travel time to get to a new school. The public school system is also a source of stable employment—relocating the district office, for example, could incent employees to move to be closer to work. Often these communities are already experiencing population decline, and the merging of school districts could accelerate the decline by removing or relocating preferred amenities.

Through the public administration lens, centralizing districts through consolidation weakens the link between the community served and its government in an effort to professionalize management; administrators serving a larger base have less incentive to be responsive to minority group interests, for instance. In contrast, centralizing bureaucratic functions could mean a reduction in local oversight (Whitford 2002). Decentralization can allow for greater responsiveness to local preferences for

public services according to Oates (1993); it is also associated with perceptions of increased performance, as well (Moynihan and Pandey 2005). We argue that rural communities could value their local districts for these reasons as well as the local amenities they create. Consolidation attenuates these perceived benefits.

Previously literature has established a strong link between school quality and housing values. In their review, Nguyen-Hoang and Yinger (2011) conclude that the willingness to pay for a one standard deviation increase in test scores hovers around four percent. These estimates are consistent in international contexts (Machin 2011). More recent work has attempted to isolate the channels through which school quality is capitalized—using data from England in a regression discontinuity design, Gibbons et al. (2013) estimate that schools’ “value-added” achievement can increase housing prices at a magnitude similar to that of overall indicators of school quality. Conversely, home values in Los Angeles did not respond in a similar manner to a public accountability campaign disseminating information on school and teacher “value-added” data (Imberman and Lovenheim 2016). However, these studies narrowly focus on the effects school quality has on housing values in urban areas and do not consider the effects schools, as the heart of many these rural communities, have on housing values. To our knowledge, this paper is the first to assess the housing market’s response to district reorganization reforms in a rural context, acknowledging rurality may shape communal preferences in ways that are distinct from urban areas.

We study the impact of school district reorganizations on rural communities by using a statewide reform in Arkansas mandating small districts reorganize on the basis of an enrollment threshold. This cutoff allows us to reduce bias in our estimates by constructing a more plausible control group. We present results from several models, using the same difference-in-differences framework. First, we estimate impacts of reorganization on town population, finding that consolidation led to a 62 to 70-person reduction in town population overall; this represents roughly 13 to 15 percent of the average pre-period treatment town population. We also show some evidence that these effects disproportionately impact communities with a higher proportion of people of color. Because the public discourse often couples consolidation with closure, we assess

the relationship between consolidation and school counts, finding as expected that towns affected by consolidation had 0.19 to 0.25 fewer schools post-treatment. Third, we use data obtained from Zillow (2021) to estimate the effects of consolidation on tax assessed property values. Our results indicate district reorganization reduces property values by over a thousand dollars, a value equal to roughly 14 percent of the average pre-period treated parcel, which suggests that the reduced level of local amenities associated with a nearby school has been capitalized into the values of homes. While policymakers may pursue government reforms to increase cost effectiveness, enhance client outcomes, or increase administrative professionalism, our results show that these reforms can have broader implications for communities at large and may work at cross purposes to their intents.

#### 4.4 Policy Background: Education Reform in Arkansas

Studying the effects of school district consolidation on rural communities is challenging due to the inherent endogeneity. Communities with declining populations or economic activity may choose to consolidate for those exact reasons, for example. To address this problem, we leverage a plausibly exogenous policy change in Arkansas to reduce this source of bias. In 2002, the Arkansas state Supreme Court ruled public education funding unconstitutional in a suit brought by a school district arguing the existing system did not guarantee equitable access. The Supreme Court called upon the legislature to address deficiencies (Loyd 2019). In response, the governor called a 2003 special session, during which the legislature passed the Public Education Reorganization Act (Holley 2015). Widespread administrative consolidation was the governor's original intent, but the resulting legislation took a narrower approach (McGee, Mills et al. 2021). Under Act 60, school districts must reorganize only if enrollment falls below an average annual enrollment of 350 students; the affected districts may voluntarily submit a plan for reorganization for the state board of education's approval, or the state board may impose one per regulation (2020).

To comply, districts could consolidate or be annexed. In a consolidation, two or more school districts dissolve to re-form a new one, and in an annexation, an existing

district (often larger) expands its boundaries to incorporate the smaller dissolving district (Johnson 2006). In practice, a consolidation results in a new school board, but in an annexation the annexing district often maintains control of the school board (Johnson 2006). There was an immediate impact of this policy—almost 90 percent of the resulting reorganizations occurred within two years.

While many states may offer financial incentives to encourage consolidation, the Arkansas reform exogenously imposes an enrollment cutoff that can be used to provide a more plausible counterfactual. In addition, Arkansas provides an excellent test case for our research question because much of the state is rural—there are only three major metropolitan statistical areas with estimated populations above 100,000—Little Rock, Northwest Arkansas (home to the University of Arkansas and several major corporations including Walmart), and Jonesboro. Overall, nearly 85 percent of Arkansas school districts are in rural areas.

#### 4.5 Data Collection

Our analyses focus on the impacts of school district consolidation on their communities, apart from impacts on student achievement or district finances. We have three dependent variables of interest: total population counts, number of community schools, and total tax assessed property value. For the models focusing on community population and schools, we obtained town population counts and an array of covariates from the decennial censuses.<sup>10</sup> To measure the potential impact of district reorganization on property value assessments, we obtained historical property assessment data from the Zillow Transaction and Assessment Database (ZTRAX). Our assessment data cover years from 2000 to 2015. We use the property address to identify parcels that fall within the same towns identified for the other analyses.

For all analyses, we started by identifying school districts that were subject to Act 60 per publicly available documents from the Arkansas Department of Education. At this

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<sup>10</sup> At the time of this writing, 2020 estimates breaking down age, poverty, and educational attainment were not available. So, we carried forward data from the 2015-2019 American Community Survey for covariates as needed.

stage, we had a list of 74 treated districts to match to Census-designated places. We limited our list to districts that were treated before 2010. For a control group, we selected districts that had an average enrollment between 350 and 700 students for the years 1999 to 2003 and were not party to an annexation or consolidation in the future.<sup>11</sup> We used this cutoff to identify districts that would decrease biases in our estimation on unobservable factors relating to district size. We used the location addresses for individual schools and district offices from the Common Core Data file from 2000 to identify corresponding towns, and GIS boundary files from the Census to identify other towns that fell completely within the boundaries of our treatment and control districts. We excluded towns that had both a treatment and control school, and/or schools from an outside district; some district boundaries split towns—there were ten towns that fell into this category that were removed from our dataset. After these cleaning processes, we had a pool of 77 treatment and 71 control towns; 20 treatment and 22 control towns did not have a school in the year 2000 but fell within the school district borders. To further increase the comparability of the control group, we also employ a propensity match procedure on pre-trends.

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<sup>11</sup> Which is to say, we excluded districts as control that were incorporating the treatment districts due to this policy change.

Figure 4.1 Map of Municipalities, Treatment School Districts, and Pool of Potential Control Districts

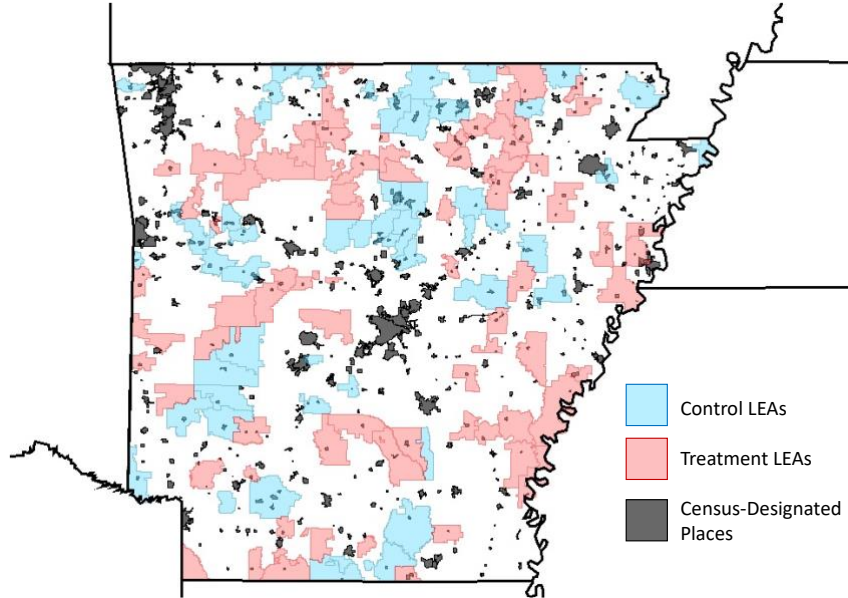


Figure 4.1 shows the treatment districts (red), potential control districts (blue), and incorporated places in Arkansas (grey). This map illustrates the distribution of treatment and the pool of potential control districts from which the matching procedure draws—they are both dispersed throughout the state, and their boundaries do not overlap. The biggest urban commercial areas are Little Rock in central Arkansas, and the cluster of towns in northwest Arkansas, which is home to the flagship public university and several corporate headquarters. Most of the areas studied do not fall in the metro area for either.

To create a strong control group with similar demographics and similar pre-trends for our population and school count regression analysis, we used propensity score matching to identify the two nearest neighbors (with replacement) in the potential pool of control towns for each treatment community. We matched on pre-treatment town population and nonwhite population percent (years 1990 and 2000) to construct two control groups—the first of the two control groups is reflected in column II in Table 4.1 and a secondary control group that focuses on creating a strong control group by race is reflected in the third column. In all analyses, we use the frequency weights produced through the nearest neighbor matching procedure.



Table 4.1 Treatment &amp; Control Places Weighted Descriptive Statistics, 2000

	(I) Treatment	(II) Control (NN=2) Pop. Matched	(III) Control (NN=2) Pop. Matched Race Matched
Total Population	477.29 (33 – 1,867)	462.58 (74 – 2278)	441.75 (74 – 1850)
College Attainment	8.07% (0 – 34.62)	8.23% (0 – 23.73)	8.68% (0 – 23.73)
Labor Force Participation	76.39% (50.94 – 87.90)	78.20% (52.25 – 90.97)	79.51% (56.76 – 90.97)
Poverty Rate	22.84% (0.81 – 63.64)	18.04% (4.28 – 39.30)	20.41% (4.28 – 39.30)
Under 18 Share	26.13% (9.54 – 33.33)	24.73% (11.88 – 38.64)	26.52% (11.88 – 38.64)
Over 65 Share	16.99% (9.54 – 33.33)	18.01% (5.68 – 45.70)	16.54% (5.68 – 32.67)
Racial Minority Population Share	23.81% (0 – 99.39)	8.01% (0 – 58.11)	20.21% (0 – 58.11)
Number of Schools	1.42 (0 – 4) N = 77	1.29 (0 – 3) N = 58 unweighted N = 77 weighted	1.37 (0 – 2) N = 50 unweighted N = 77 weighted

Mean (Min – Max). Source: NHGIS Census Data. Analytical weights applied.

In Table 4.1, we compare demographics for the towns in the final analyses, prior to treatment. The process of identifying comparable untreated towns produced a control group that looks very similar to the treated towns in the pre-period with respect to total population, education levels, labor force participation, poverty, age, and schools (columns I and II). The biggest observable difference in these data is the racial composition. The treated towns have roughly three times more people of color than the control towns. To further understand the differences in racial composition, we consider the distribution of racial minority population share in the unweighted data. Figure 4.2 shows that treatment towns have sustained higher shares of nonwhite residents over the course of the period studied, on average. However, the percent of the population that is a racial minority has increased overall in both groups.

Figure 4.2 Racial Composition Over Time, By Treatment Status

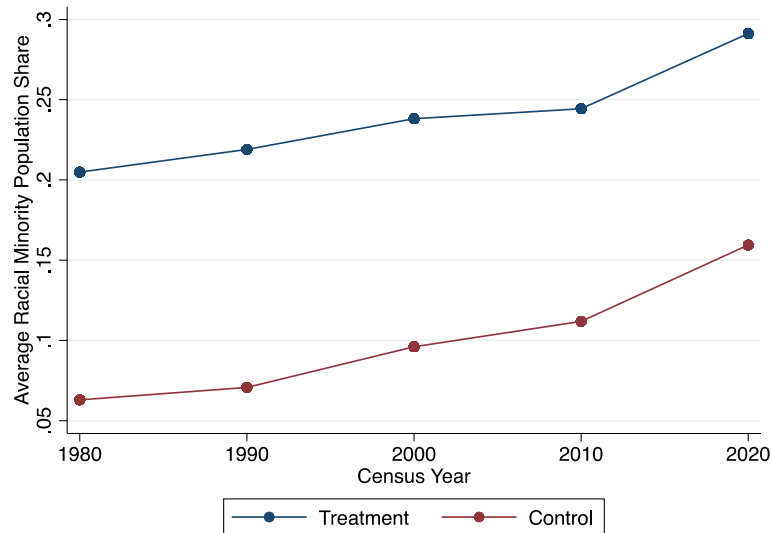
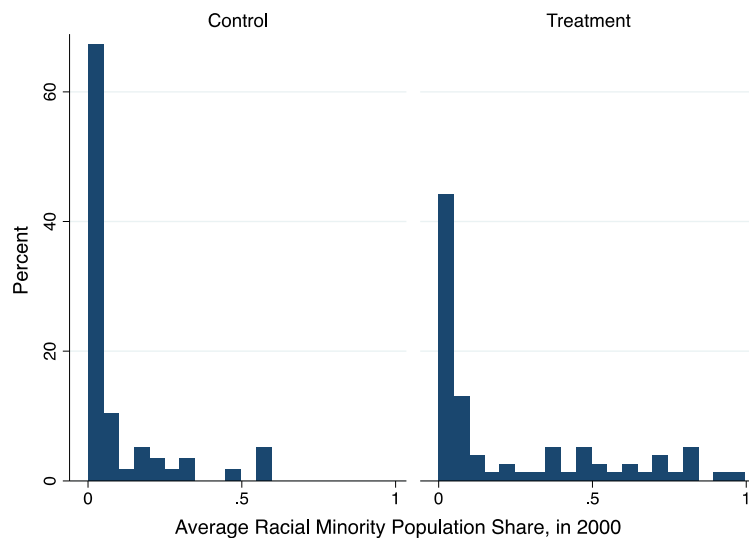


Figure 4.3 illustrates the distribution of nonwhite population share in the year 2000 by treatment status. In both cases, the data skew towards zero, but is much more pronounced for the control pool. We also note that the maximum value for the treatment group, 99 percent, is not an outlier—there are 14 treated towns that have values exceeding the maximum value found in the control group, 58 percent.

Figure 4.3 Distribution of Racial Composition by Treatment Status, Year 2000



Because of this difference in racial composition, we deployed a propensity match to create an additional control group; the descriptive statistics for this alternate control

group are in column III. Adding this additional match variable had the intended effect of increasing pre-treatment comparability with respect to racial composition but reduces the sample of control communities a bit. Therefore, we consider this a secondary control group to serve as a sensitivity analysis.

We used a similar approach to the analysis of property values—we propensity score matched the nearest neighbor on assessed value per square foot and total town population prior to reform. Because there were inconsistent gaps in the parcel panels, we matched on the first observed value prior to 2004. We matched on the population count for the year 2000, the assessed value per square foot, and the assessment year. Because we started with half a million observations, we opted to use only the single nearest neighbor with replacement. Using the pre-trend years as a baseline limits our analysis to those parcels that show up in both. While the strategy will significantly decrease our sample size, we believe this process increases the quality of our estimates.

Table 4.2 Treatment & Control Property Value Assessments, Pre-Treatment

	<b>Treatment</b>	<b>Control (NN=1)</b>
Total Assessed Value	\$9,212.07 (100 – 192,950)	\$11,949.52 (60 – 89,940)
Lot Size (Sq Ft)	744,072.20 (2,178 – 27,900,000)	522,040.90 (2,178 – 35,000,000)
Residential – Multi Unit	0.04% (0 – 1)	0.33% (0 – 1)
Residential – Apartments	0.01% (0 – 1)	0.00% (0 – 1)
Residential – Single Family	90.70% (0 – 1)	89.83% (0 – 1)
Residential – Mobile Home(s)	7.61% (0 – 1)	9.85% (0 – 1)
	N = 6,731	N = 4,812 unweighted N = 6,754 weighted

Mean (Min – Max). Source: Zillow (2021). Authors’ analysis.

The properties in the matched control group on average have higher assessed values and smaller lot sizes. However, roughly 9 out of 10 parcels across groups are zoned single-family residential. The remainder almost exclusively consist of mobile homes. The omitted category are “other.” The historical property value data was limited in its detail.

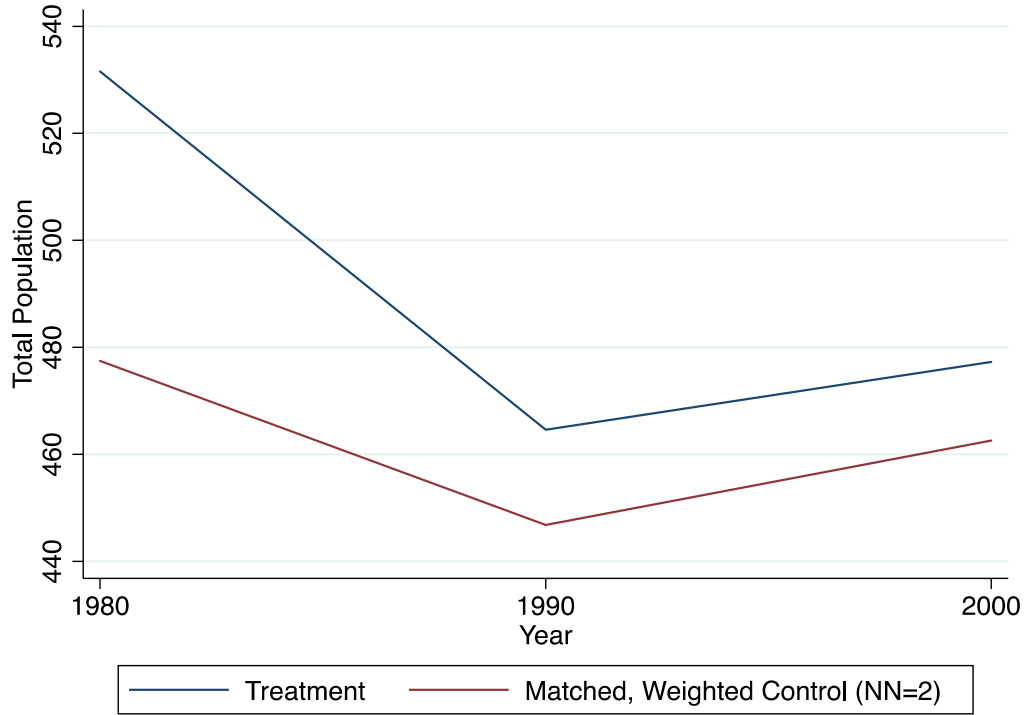
We introduce additional controls for local housing stock, which we discuss in greater detail in the next section.

#### 4.6 Research Design

We use population counts, number of schools, and property value assessments as dependent variables in a propensity score matched difference-in-differences (DID) event analysis design. While the enrollment cutoff specified in the legislation forcing treatment would lend itself to a regression discontinuity design, that was not feasible due to the small number of observations near the cutoff; comparing towns at the cut-point would have significantly decreased power. For example, only five of our control districts had average enrollments under 400 students from the pre-adoption years, which covered nine towns. Therefore, we conceptually take advantage of the intuition of a regression discontinuity to construct the control group, as outlined in the data section, and follow the same logic for all analyses. This policy change is exogenous; while communities have some say in what their reorganization will look like, they still must reorganize with another district per state law.

An identifying assumption of the DID framework is the need for pre-treatment parallel trends. We presented some descriptive statistics comparing treated and control communities in the data section to attest to comparability. In the figure below, we show historic trends in population by treatment status to highlight general trends without controlling for covariates. The control group in this figure are the weighted nearest neighbors. While the levels are slightly different, they follow the same trend over time—a dip in the 1990 census, with an increase in the year 2000.

Figure 4.4 Pre-Treatment Population Means



We have two choices in designating treatment timing in our model—using the year of the universal policy lever, the passage of the 2003 special session reform, or allowing for staggered treatment timing by using the consolidation date instead. We chose to use a universal treatment lever, which could be considered an intent-to-treat analysis (ITT). We chose this designation for two reasons. First, our conceptual argument rests, in part, on perceived risks. For small rural communities, the passage of this bill was likely highly visible. An increased sense of risk regarding reorganization and losing their schools is an important signal to residents considering leaving. Therefore, the ITT analysis avoids an anticipation effect. Second, the overwhelming majority of our test cases were reorganized shortly after policy adoption—of our treatment towns, 60 were treated (i.e. consolidated or annexed) before the 2005 school year. We applied this same framework to all dependent variables in a panel regression:

$$(1a) \quad Pop_{pt} = TreatPost_{pt} + Treat_p + Post_t + Demo_{pt} + Econ_{pt} + Year_t + Place_p + e$$

$$(1b) \quad Pop_{pt} = TreatPost_{pt} * POC_{pt} + TreatPost_{pt} + Treat_p + Post_t + Demo_{pt} + Econ_{pt} + Year_t + Place_p + e$$

$$(2) \quad SchoolCount_{pt} = TreatPost_{pt} + Treat_p + Post_t + Demo_{pt} + Econ_{pt} + Year_t + Place_p + e$$

$$(3) \quad Value_{lt} = TreatPost_{lt} + Treat_l + Post_t + Features_{lt} + Community_{lt} + Year_t + Parcel_l + e$$

In equations 1a, 1b, and 2,  $p$  is place and  $t$  denotes time. Equation 1b interacts treatment ( $Treat_p$ ) with racial minority population share ( $POC_{pt}$ ) to examine the treatment effect by race. For all the models, we include a vector of demographic ( $Demo_{pt}$ ) and economic controls ( $Econ_{pt}$ ), alongside year ( $Year_t$ ) and Census-designated place fixed effects ( $Place_p$ ), to estimate the effect of this reform on population counts ( $Pop_{pt}$ ) and school counts ( $SchoolCount_{pt}$ ). For the third equation, the unit of observation is property parcel. Parcel value is a function of similar controls but includes property features ( $Features_{lt}$ ) and parcel fixed effects ( $Parcel_l$ ) in lieu of town fixed effects. To compensate for the lack of property-level data on building features, we include variables from the Census on town housing stock, including total number of housing units, median number of rooms, occupation density, and owner-occupied ratios ( $Community_{lt}$ ).

#### 4.7 Results

We estimated the effects of this government reform on community population first using the weights produced from the matching procedure in panel regressions. We present the results below for two nearest neighbors in Table 4.3. In the base model (column I), the treated communities had an average population loss of 62 people post-reform. This is equal to roughly 13 percent of the population of the average treatment town in the 2000 Census. In our sensitivity analysis using pre-treatment population counts and racial composition (column II), the magnitude of treatment is slightly larger—nearly 70 people.

Table 4.3 The Effects of School District Reorganization on Community Population

	(I) Pop. Matched	(II) Pop. Matched Race Matched
Treatment*Post	<b>-61.87**</b> (21.23)	<b>-69.56*</b> (27.51)
Treatment Group	0 (.)	0 (.)

Table 4.3 (continued)

Post Period	-62.4 (31.63)	-85.82 (44.85)
<i>Community Characteristics</i>		
Under 18 Percent	<b>610.9**</b> (186.5)	<b>561.4**</b> (171.7)
Over 65 Percent	35.9 (100.9)	-139.5 (130.4)
Racial Minority Percent	-95.41 (161.8)	112.7 (195.9)
Educational Attainment	155 (79.56)	<b>340.2*</b> (130.9)
Labor Force Participation	-39.04 (57.47)	-121.3 (86.06)
Poverty Rate	22.56 (44.18)	66.77 (59.19)
School Count	11.37 (20.29)	29.47 (17.83)
Year Fixed Effects	Y	Y
Place Fixed Effects	Y	Y
Constant	<b>329.4***</b> (85.23)	<b>327***</b> (86.21)
Observations	672	632
$R^2$	0.285	0.300

Standard errors in parentheses. \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

We used the same covariates in event analyses, omitting the year 2000 as a baseline (Figures 4.5 and 4.6). The pre-treatment coefficients are not statistically different from zero in either specification, providing support for the identifying assumptions of our model. The event studies also reveal that the magnitude of population loss is steady over time in the base model, but that the effects may be short lived in the second model matching on both pre-treatment nonwhite percent and total population.

Figure 4.5 Population Event Study Using Matching (NN=2, Population Count)

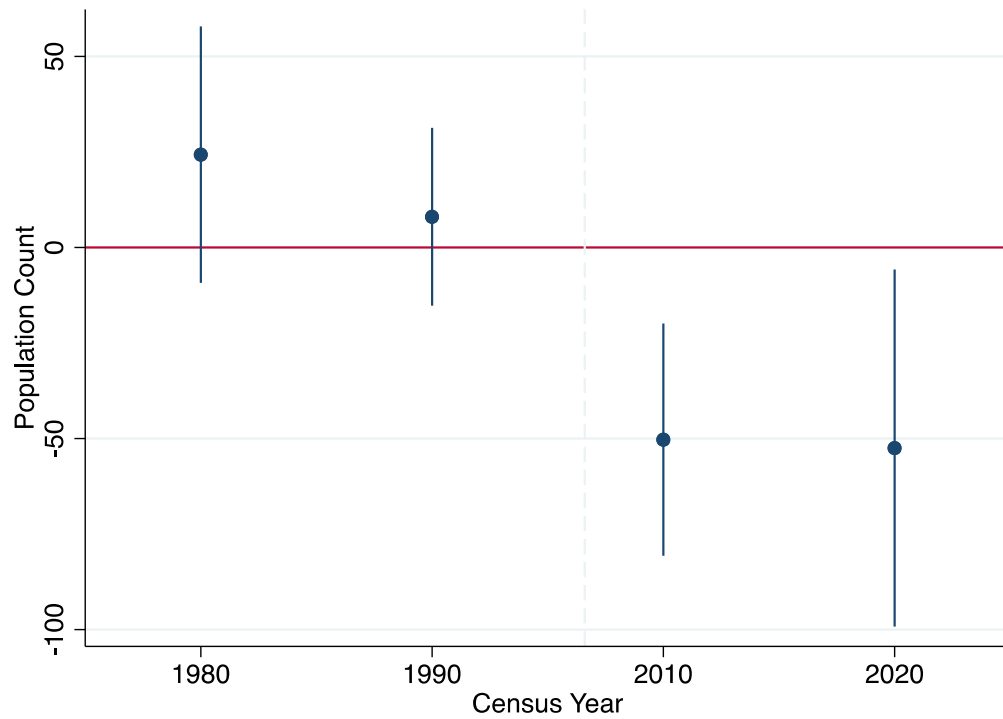
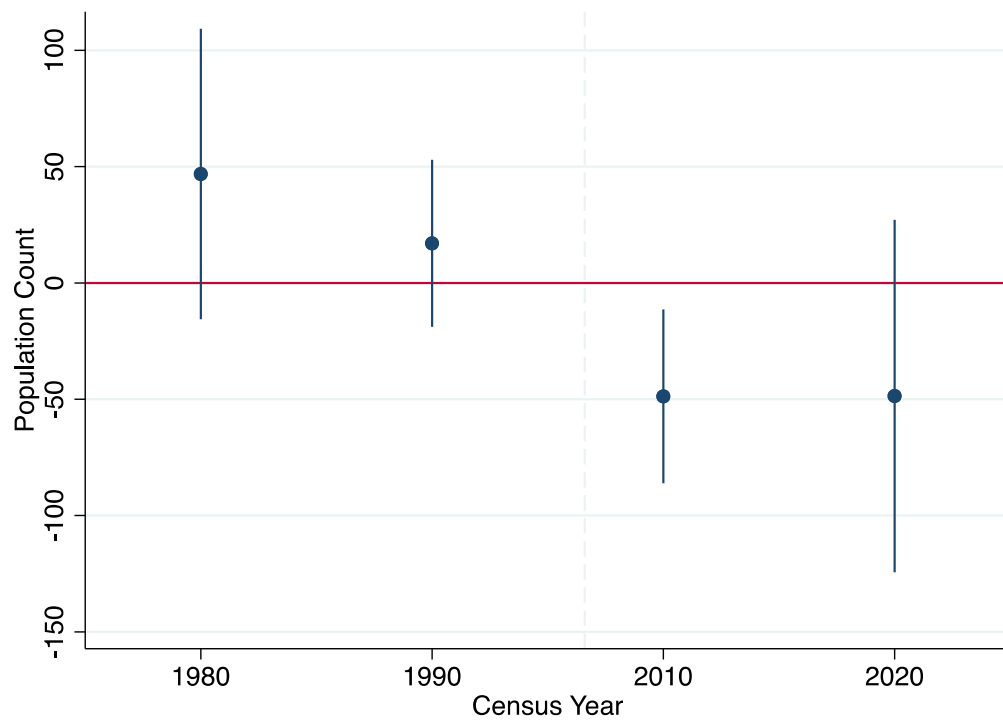


Figure 4.6 Population Event Study Using Matching (NN=2, Population Count and Nonwhite Share)





To understand if there were racially-disparate impacts of this reform, we interact the nonwhite population percentage with treatment status (Table 4.4). We use the same matching as above—only total population in the pre-period for two nearest neighbors in the first column, and adding nonwhite population share as an additional criteria in the second column. In the first regression, a ten percentage point increase in the racial minority population share is associated with a 38-person reduction in population for treated towns. The post-reform treatment term is no longer significant in this specification. This implies that communities with larger racial minority populations had a disparate response to consolidation. This result is mirrored in the second regression (column II).

Table 4.4 The Racially-Disparate Effects of School District Reorganization on Community Population

	(I) Pop. Matched	(II) Pop. Matched Race Matched
Treatment*Post	32.69 (18.85)	31.9 (24.11)
Treatment*Post*POC	<b>-378.9***</b> (76.83)	<b>-381.3***</b> (79.32)
Treatment Group	0 (.)	0 (.)
Post Period	<b>-75.59**</b> (28.52)	<b>-102.3*</b> (43.13)
<i>Community Characteristics</i>		
Under 18 Percent	<b>374*</b> (146.9)	<b>332.5*</b> (129.4)
Over 65 Percent	-71.25 (87.93)	<b>-240.5*</b> (117.6)
Racial Minority Percent	36.17 (130.5)	224.1 (181.5)
Educational Attainment	126.8 (69.79)	<b>306.3*</b> (128.4)
Labor Force Participation	-26.73 (53.39)	-98.13 (78.17)
Poverty Rate	.8143 (39.91)	39.99 (57.53)
School Count	5.235 (16.36)	24.38 (15.15)
Year Fixed Effects	Y	Y
Place Fixed Effects	Y	Y

Table 4.4 (continued)

Constant	<b>407.8***</b> (76.34)	<b>394.1***</b> (75.11)
Observations	672	632
$R^2$	0.427	0.423

Standard errors in parentheses \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

One of the perceived risks of district consolidation is loss of community schools. The majority of the towns in our dataset had at least one school; however, 36 of the 135 towns did not have a school prior to this reform, but fell within the boundaries of the treatment or control pool of school districts. If district consolidation is perceived as a loss of control over local education policy, it would stand to reason the towns with existing schools prior to reform may have a stronger reaction as they potentially have more to lose. To further explore this expectation, we limit the regression analysis to treatment and control towns that had a school at some point in the pre-period. The general trend in these additional analyses on population yield the same general conclusion. Matching only on population (column I), the magnitude is somewhat larger—treated towns with schools experienced a population loss of 76 people, relative to the control towns with schools. This coefficient is roughly 22 percent larger than that in the analysis inclusive of towns without schools including towns without schools in the pre-period (column I, Table 4.3). Matching on pre-trends in population and racial composition (column II), the estimated magnitude is almost identical to that in Table 4.3 (column II).

Table 4.5 More to Lose? Population Analysis of Towns with Schools

	(I) Pop. Matched	(II) Pop. Matched Race Matched
Treatment*Post	<b>-75.65**</b> (28.18)	<b>-69.94*</b> (34.81)
Treatment Group	0 (.)	0 (.)
Post Period	-66.55 (45.59)	-122 (67.07)
<i>Community Characteristics</i>		
Under 18 Percent	<b>743.10***</b> (260)	<b>865.80***</b> (211)
Over 65 Percent	62.35 (165.30)	-90.64 (206.2)

Table 4.5 (continued)

Racial Minority Percent	-324.20 (217.30)	39.81 (241.7)
Educational Attainment	86.87 (160.90)	402.6 (217.7)
Labor Force Participation	-99.20 (92.07)	-240.8 (144.3)
Poverty Rate	46.26 (70.67)	59.63 (82.86)
School Count	-5.2 (20.86)	17.15 (19.6)
Year Fixed Effects	Y	Y
Place Fixed Effects	Y	Y
Constant	<b>472***</b> (139)	<b>411.40**</b> (155.90)
Observations	495	465
$R^2$	0.319	0.337

Standard errors in parentheses \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

We also directly test this expectation that consolidation leads to loss of schools in a local community. In Table 4.6, we show that towns in consolidated districts did see a statistically significant reduction in their number of schools, but that this effect was not magnified by the racial composition of the community using either control group. So, while towns in consolidated districts had roughly 0.19 to 0.25 fewer schools post-reform, that impact was not disproportionately distributed based on race.

Table 4.6 The Effects of School District Reorganization on Community Schools

	<b>Pop. Matched</b>		<b>Pop. Matched Race Matched</b>	
	(I)	(II)	(III)	(IV)
Treatment*Post	<b>-.2478***</b> (.07114)	<b>-.2175*</b> (.08888)	<b>-.1916*</b> (.08055)	-.1583 (.09646)
Treatment*Post*POC		-.1197 (.2038)		-.1238 (.2023)
Treatment Group	0 (.)	0 (.)	0 (.)	0 (.)
Post Period	<b>-.1682*</b> (.06682)	<b>-.172*</b> (.06621)	<b>-.3037**</b> (.1151)	<b>-.3085**</b> (.1145)
<i>Community Characteristics</i>				
Under 18 Percent	-.03371 (.3144)	-.1085 (.3114)	-.3871 (.4618)	-.4608 (.4554)

Table 4.6 (continued)

Over 65 Percent	-.0938 (.3411)	-.1275 (.3385)	-.2148 (.3503)	-.2473 (.3443)
Racial Minority Percent	<b>-.6939*</b> (.3288)	<b>-.651*</b> (.306)	-.7233 (.5811)	-.6859 (.5795)
Educational Attainment	.3508 (.2541)	.3412 (.2526)	.5149 (.3042)	.503 (.3047)
Labor Force Participation	-.2849 (.1985)	-.2804 (.1948)	-.3426 (.2574)	-.3345 (.2553)
Poverty Rate	-.1116 (.1485)	-.1183 (.1486)	.1473 (.1805)	.1383 (.1811)
Year Fixed Effects	Y	Y	Y	Y
Place Fixed Effects	Y	Y	Y	Y
Constant	<b>1.776***</b> (.2391)	<b>1.797***</b> (.2412)	<b>2.032***</b> (.3723)	<b>2.051***</b> (.3724)
Observations	672	672	632	632
$R^2$	0.176	0.178	0.197	0.198

Standard errors in parentheses \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

Figures 4.7 and 4.8 report coefficients for event studies on the number of schools in treatment and control communities. The trends in both regressions tell a consistent story. The coefficients for the two periods prior to the reform are not statistically significant from zero in both cases, again providing support for the parallel trends assumption. The effect of consolidation on the number of schools within a community is also not particularly responsive to time; the reduction hovers around 0.2 for both 2010 and 2020.

Figure 4.7 Number of Schools Event Study (NN=2, Population Count)

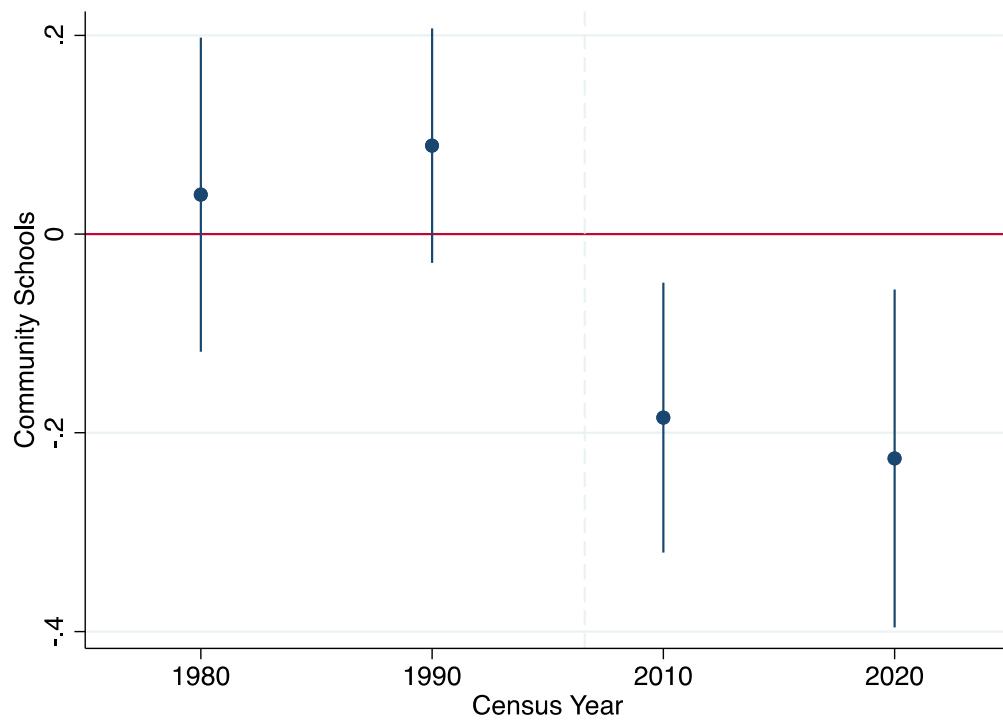
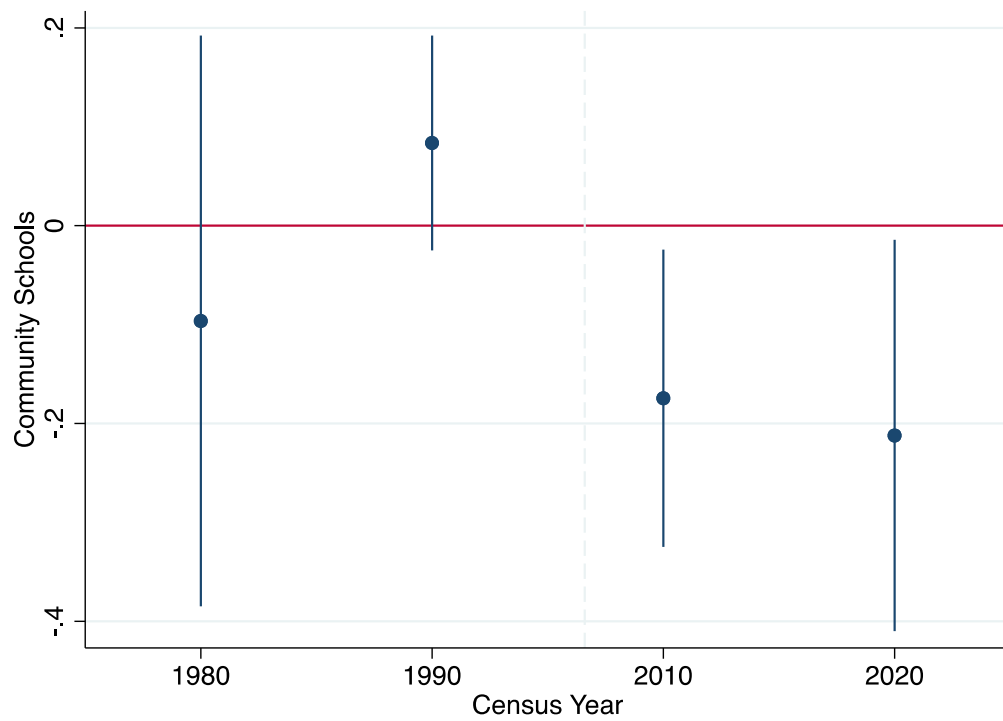


Figure 4.8 Number of Schools Event Study (NN=2, Population Count and Nonwhite Share)



Next, we turn to property values. The table below shows the average treatment effects of the reform on property assessments using the matching procedure described in the data section. The full complement of covariates is included in the regression in column I, while column II reports results for an additional interaction with racial composition. District reorganization led to a decline in assessed value of roughly 1,300 dollars. While larger nonwhite population shares were associated with lower assessed values, there is no evidence that the interaction between community racial composition and consolidation augmented that effect.

Table 4.7 The Effects of School District Reorganization on Assessed Value

	(I) Base Model	(II) Racial Minority Interaction
Treatment*Post	<b>-1314***</b> (144.3)	<b>-1384***</b> (190.7)
Treatment*Post*POC		337.1 (405.2)
Treatment Group	-260.5 (594.4)	-207.5 (604.3)
Post Period	<b>4155***</b> (480.5)	<b>4066***</b> (526.4)
<i>Community Characteristics</i>		
Percent Racial Minority	<b>-6468**</b> (2405)	<b>-6897**</b> (2659)
School Count	<b>152.7*</b> (74.81)	<b>169*</b> (77.4)
Poverty Rate	<b>7373***</b> (1569)	<b>7430***</b> (1569)
Educational Attainment	<b>3163*</b> (1552)	<b>3191*</b> (1551)
Under 18 Percent	<b>7.08**</b> (2.327)	<b>7.223**</b> (2.341)
Over 65 Percent	<b>10.4*</b> (5.145)	<b>10.64*</b> (5.197)
Total Housing Units	<b>-9.13*</b> (3.695)	<b>-9.253*</b> (3.739)
Median Number of Rooms	<b>1643***</b> (386.4)	<b>1636***</b> (386.7)
<1 Occupant Per Room Share	1372 (780.1)	1178 (914.9)
Owner-Occupied Ratio	<b>-1977***</b> (493.9)	<b>-2012***</b> (507.5)

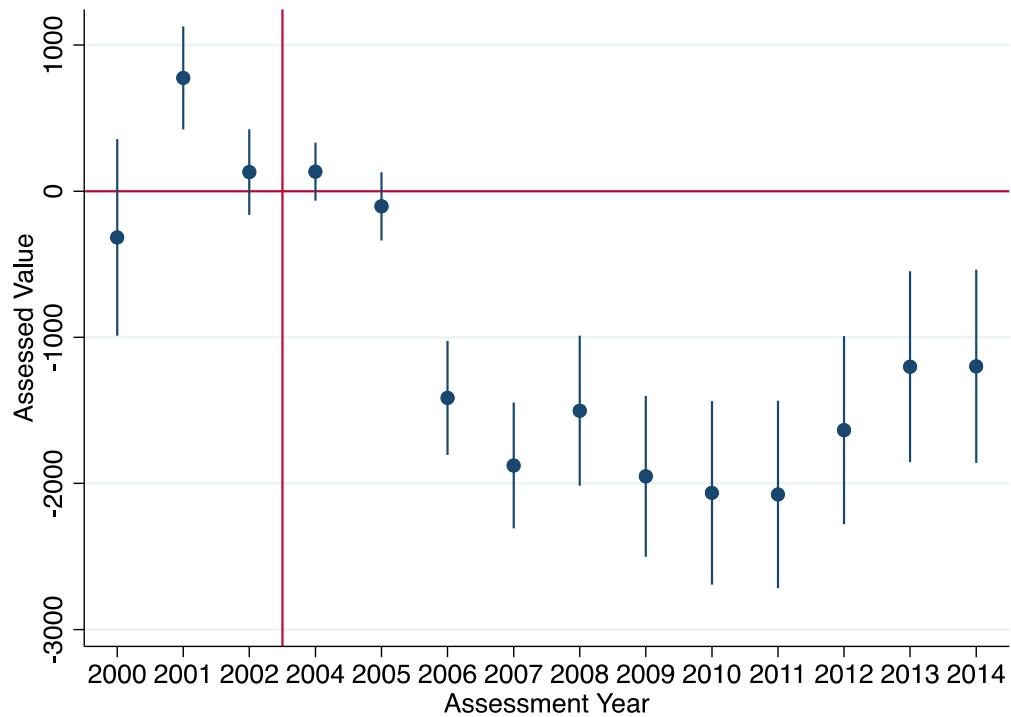
Table 4.7 (continued)

	<i>Property Characteristics</i>	
Lot Size (Sq Ft)	<b>.001732<sup>***</sup></b> (.0002548)	<b>.001732<sup>***</sup></b> (.0002547)
Multi-Family Residence	<b>8207<sup>***</sup></b> (711.1)	<b>8208<sup>***</sup></b> (711.1)
Apartments	1518 (3244)	1514 (3244)
Single-Family Residence	<b>6514<sup>***</sup></b> (486.1)	<b>6512<sup>***</sup></b> (486)
Mobile Home(s)	<b>5629<sup>***</sup></b> (462.3)	<b>5630<sup>***</sup></b> (462.3)
Year Fixed Effects	Y	Y
Parcel Effects	Y	Y
Constant	<b>-6260<sup>**</sup></b> (2360)	<b>-5929<sup>*</sup></b> (2565)
Observations	46,599	46,599
$R^2$	0.179	0.179

Standard errors in parentheses <sup>\*</sup>  $p < 0.05$ , <sup>\*\*</sup>  $p < 0.01$ , <sup>\*\*\*</sup>  $p < 0.001$

To examine the effects of consolidation on property values more closely, we also deploy an event study using the full array of covariates (Figure 4.9). The year 2003 is used as a baseline, and is omitted. Relative to 2003, the pre-treatment years are somewhat noisy and there is not a clear bias in the property values on the basis of treatment group status. However, after the implementation of the reform, there is a clear downward trend in property values after 2005, *ceteris paribus*. After a few years the magnitude decreases.

Figure 4.9 Property Assessments Event Study (NN=1)



#### 4.8 Discussion and Conclusion

In this paper, we examine the impact of rural school district reform on the communities they serve. We find that consolidation decreases town population, community schools, and property values. We also find some evidence that communities with larger racial minority populations may be disproportionately affected by this reform. Taken together, these results indicate that local communities value local institutions in rural settings. Our study does have limitations due to data constraints; for example, we could not include unincorporated places for study, so generalizability is reduced. Our analysis of property value assessments also lacks fine-grained parcel information and does not have as many pre-treatment years as we would prefer. We combat this problem through including town-level housing data and deploying a nearest neighbor matching strategy. Despite this approach, our pre-trend is limited in length; further work is needed to confirm our findings in other contexts.



Despite these limitations, we contribute to scholarship in three ways: we explore new ways efforts to increase capacity in local governments may impact constituents; we increase our understanding of the impacts of a common education reform on community outcomes; and we focus our study on the rural context. Ultimately, our empirical work highlights potential tradeoffs in government centralization and professionalization. Local districts contribute to the character of their communities and provide a public service beyond the immediate clients. Future work could further examine how such professionalization reforms impact democratic processes, accessibility, and equity.

## APPENDICES

## APPENDIX 1. THE DEVOLVED SOCIAL SAFETY NET: IMPLICATIONS FOR ACCESS?

Table 4.8 Data Sources for Chapter 2 Analyses

Description		Source
<i>Dependent Variables</i>		
Number of Human Service Organizations Per 1K (3-mi radius)	<ul style="list-style-type: none"> <li>• Geocoded locations by fiscal year using Texas A&amp;M Geocoding Services.</li> <li>• Used ArcGIS to calculate number within 2 mile radius of Census tract centroid.</li> <li>• Divided by total population of Census tract.</li> <li>• Weighted average for low-income neighborhoods; county-level.</li> </ul>	(Urban Institute 2021) (Manson et al. 2021) (DW 2021)
Total Human Service Organizational Revenues Per Capita (3-mi radius)	See above; but for total revenues.	(Urban Institute 2021) (Manson, Schroeder et al. 2021) (DW 2021)
Total Human Service Organizations Program Revenues Per Capita (3-mi radius)	See above; but for program revenues.	(Urban Institute 2021) (Manson, Schroeder et al. 2021) (DW 2021)
Number of HSOs – Racial Gap	<ul style="list-style-type: none"> <li>• Calculated the weighted average for low-income neighborhoods for white and non-white populations.</li> <li>• Subtracted non-white from white to calculate gap.</li> <li>• County-level measure</li> </ul>	(Urban Institute 2021) (Manson, Schroeder et al. 2021) (DW 2021)
HSO Revenues – Racial Gap	See above; but for total revenues.	(Urban Institute 2021) (Manson, Schroeder et al. 2021) (DW 2021)
HSO Program Revenues – Racial Gap	See above; but for program revenues.	(Urban Institute 2021) (Manson, Schroeder et al. 2021) (DW 2021)

Table 5.1 (continued)

<i>Governance Structure</i>		
Post-Welfare Reform	Dummy variable for all years after 1996, after passage of welfare reform.	Author's calculation
Second-Order Devolution State	Time invariant; equal to one if state devolves TANF authority to county governments	Hahn et al (2015), Gainsborough (2003)
SOD*Post Reform	Interaction term for the above two variables.	Author's calculation
TANF Benefits Discretion	Dummy variable for states that devolve policymaking re: benefits to counties.	HHS TANF Reports to Congress (2000, 2006), state plans
TANF Services Discretion	Dummy variable for states that devolve policymaking re: services offered to counties.	HHS TANF Reports to Congress (2000, 2006), state plans
Self-Generated County Revenue	Share of revenue that is raised by county governments and not through intergovernmental transfers.	Census of Governments, calculated by author.
Number of HSOs (per capita)	Human service organization density, scaled by population; county-level.	(Urban Institute 2021) (Manson, Schroeder et al. 2021) (DW 2021), calculated by author.
Total HSO Revenues (per capita)	Sum of all HSO revenues, scaled by population; county-level.	(Urban Institute 2021) (Manson, Schroeder et al. 2021) (DW 2021), calculated by author.
Total HSO Program Revenues (per capita)	Sum of all HSO program revenues, scaled by population; county-level.	(Urban Institute 2021) (Manson, Schroeder et al. 2021) (DW 2021), calculated by author.
<i>Policy Treatment</i>		
State Government Spending, Per 1K County	Total state government spending, scaled by population.	Census of Governments, calculated by author.
Government Spending, Per 1K	Total county government spending, scaled by population.	Census of Governments, calculated by author.
Public Assistance, Per Capita	Share of county population on public assistance, per decennial census.	(Manson, Schroeder et al. 2021)
<i>Potential Client Characteristics</i>		
Population <200% Federal Poverty Line	Share of population below twice the poverty line; county-level.	(Manson, Schroeder et al. 2021)

Table 5.1 (continued)

Children in 2-Parent Married Households	Share of children in 2-parent households; county-level.	(Manson, Schroeder et al. 2021)
AFDC/TANF Child-Only Case Share (State)	Share of AFDC/TANF caseload that are child-only cases; state-level.	UK Center for Poverty Research
Female Labor Force Participation	Percent of working age women in the labor force; county level.	(Manson, Schroeder et al. 2021)
<i>Environment – Political, Economic, Demographic</i>		
Total Population	Count of residents; county-level.	(Manson, Schroeder et al. 2021)
Urban Share	Percent of population living in urban areas; county-level.	(Manson, Schroeder et al. 2021)
Racial Residential Segregation Index	Theil index of racial segregation; county-level.	(Manson, Schroeder et al. 2021), author's calculations.
Population Share – White	Percent of population that is white; county-level	(Manson, Schroeder et al. 2021)
State Government Ideology	State government ideology nominate score.	(Berry et al. 1998)
POTUS Democratic Vote Share	Share of votes for Democratic candidate in the last presidential election.	(Leip 2021)
State Economic Growth	Percent change from previous year in GDP; state-level.	UK Center for Poverty Research
State Income Inequality	Gini coefficient for income calculated using tax returns.	(Frank 2009)

Table 4.9 Summary Statistics

Variable	Mean	Standard Dev.	Min.	Max.
Post-Welfare Reform	0.60	0.49	0.00	1.00
Second-Order Devolution State	0.51	0.50	0.00	1.00
SOD*Post Reform	0.31	0.46	0.00	1.00
TANF Benefits Discretion	0.12	0.32	0.00	1.00
TANF Services Discretion	0.31	0.46	0.00	1.00
Self-Generated County Revenue	0.62	0.17	0.15	1.00
Number of HSOs (per capita)	0.00	0.00	0.00	0.00
Total HSO Revenues (per capita)	45.69	106.34	0.00	1259.58
Total HSO Program Revenues (per capita)	20.13	60.26	0.00	983.97

Table 5.2 (continued)

State Government Spending, Per 1K	3231.12	1023.68	1489.11	6211.13
County Government Spending, Per 1K	836.28	751.45	77.49	6641.64
Public Assistance, Per Capita	71.39	51.44	6.31	270.79
Population <200% Federal Poverty Line	0.27	0.08	0.06	0.58
Children in 2-Parent Married Households	0.22	0.03	0.12	0.30
AFDC/TANF Child-Only Case Share (State)	0.30	0.17	0.03	0.85
Female Labor Force Participation	0.56	0.06	0.38	0.75
Total Population	148492.40	348196.40	2334.00	5376741.00
Urban Share	0.52	0.26	0.00	1.00
Racial Residential Segregation Index	0.13	0.11	0.00	0.58
Population Share – White	0.85	0.15	0.19	1.00
State Government Ideology	48.19	13.17	20.98	69.70
POTUS Democratic Vote Share	42.65	9.29	16.17	80.69
State Economic Growth	0.06	0.02	0.15	0.01
State Income Inequality	0.57	0.02	0.53	0.64

### Human Service Organization Designation

To be included in the data set, nonprofits had to have one of the following NTEE classifications for at least 49 percent of the years observed. I used the list from Allard (2009) as a starting point, and added for my purposes.

- B60
- F20 F21 F22 F30 F32
- J20 J21 J22
- K30 K31 K34 K35 K36 K40
- L21 L40 L41 L30 L80
- P20 P22 P24 P27 P28 P29 P30 P40 P42 P43 P44 P84 P45 P46 P50 P51 P52 P60  
P62 P72 P73 P80 P86
- O20 O21 O22 O23 O30 O31

Table 4.10 Welfare Governance & Geospatial Accessibility to the Social Safety Net for Low-Income Neighborhoods: Tract-Level Analysis

	(1) Number of HSOs, Per 1K (inverse hyperbolic sine)		(2) Total Revenues, Per Capita (inverse hyperbolic sine)		(3) Program Revenues, Per Capita (inverse hyperbolic sine)	
	<i>Coeff.</i>	<i>S.E.</i>	<i>Coeff.</i>	<i>S.E.</i>	<i>Coeff.</i>	<i>S.E.</i>
Post-Welfare Reform	-0.05	0.12	<b>1.69***</b>	<b>0.26</b>	<b>2.83***</b>	<b>0.28</b>
Second-Order Devolution						
State	-	-	-	-	-	-
SOD*Post Reform	0.00	0.02	-0.07*	0.04	<b>-0.12***</b>	<b>0.04</b>
Self-Generated						
County						
Revenue	0.09	0.07	0.17	0.17	-0.16	0.18
Number of HSOs (per capita)	<b>2,468.19***</b>	<b>109.36</b>	<b>5,091.60***</b>	<b>199.36</b>	<b>4,876.62***</b>	<b>217.56</b>
Total HSO Revenues (per capita)	-0.00	0.00	-0.00	0.00	<b>-0.0008***</b>	<b>0.0001</b>
Total HSO Program Revenues (per capita)	-0.00	0.00	<b>0.0007***</b>	<b>0.0002</b>	<b>0.002***</b>	<b>0.0002</b>
			<i>Policy Treatment</i>			
State Government Spending, Per 1K	-0.00	0.00	<b>-0.0003***</b>	<b>0.00007</b>	0.00	0.00
County Government Spending, Per 1K	-0.00	0.00	0.00	0.00	0.00	0.00
Public Assistance, Per Capita	<b>-0.001**</b>	<b>0.0003</b>	-0.00	0.00	0.00	0.00
			<i>Potential Client Characteristics</i>			
Population <200% Federal Poverty Line (County)	-0.67*	0.37	0.99	0.91	0.02	0.96

Table 5.3 (continued)

Children in 2- Parent Married Households (County)	<b>-9.88***</b>	<b>1.20</b>	<b>-13.37***</b>	<b>2.29</b>	<b>-17.30***</b>	<b>2.53</b>
AFDC/TANF Child-Only Case Share (State)	0.09	0.08	-0.23	0.18	<b>-0.67***</b>	<b>0.19</b>
Female Labor Force Participation (Tract)	<b>-1.96***</b>	<b>0.06</b>	<b>-2.42***</b>	<b>0.09</b>	<b>-2.27***</b>	<b>0.10</b>
<i>Environment – Political, Economic, Demographic</i>						
Total Population (County)	-0.00	0.00	<b>0.0000004**</b>	<b>0.00</b>	-0.00	0.00
Urban Share (Tract)	<b>1.04***</b>	<b>0.03</b>	<b>5.87***</b>	<b>0.10</b>	<b>4.83***</b>	<b>0.09</b>
Racial Residential Segregation Index (County)	<b>-1.16***</b>	<b>0.21</b>	-0.48	0.57	<b>-3.65***</b>	<b>0.59</b>
Population Share – Black (Tract)	<b>0.25***</b>	<b>0.02</b>	<b>0.81***</b>	<b>0.04</b>	<b>0.66***</b>	<b>0.04</b>
Population Share – Hispanic (Tract)	<b>0.28***</b>	<b>0.03</b>	<b>1.50***</b>	<b>0.06</b>	<b>1.43***</b>	<b>0.07</b>
State Government Ideology POTUS Democratic Vote Share (County)	<b>0.002***</b>	<b>0.0004</b>	0.00	0.00	0.00	0.00
State Economic Growth State Income Inequality	-0.22	0.33	0.52	0.71	0.32	0.76
	<b>1.40**</b>	<b>0.56</b>	<b>2.80**</b>	<b>1.17</b>	<b>2.82**</b>	<b>1.27</b>
Year Dummies	Y		Y		Y	
County Dummies	Y		Y		Y	



\*\*\* p<0.01,  
\*\*p<0.05,  
\*p<0.10  
Robust S.E.s

$R^2=0.54$   
N=69,074

$R^2=0.64$   
N=69,074

$R^2=0.60$   
N=69,074

## APPENDIX 2. DOES STATE GOVERNMENT PRIVATIZATION INCREASE SOCIOECONOMIC INEQUALITY?

Table 4.11 Data Sources for Chapter 3 Analyses

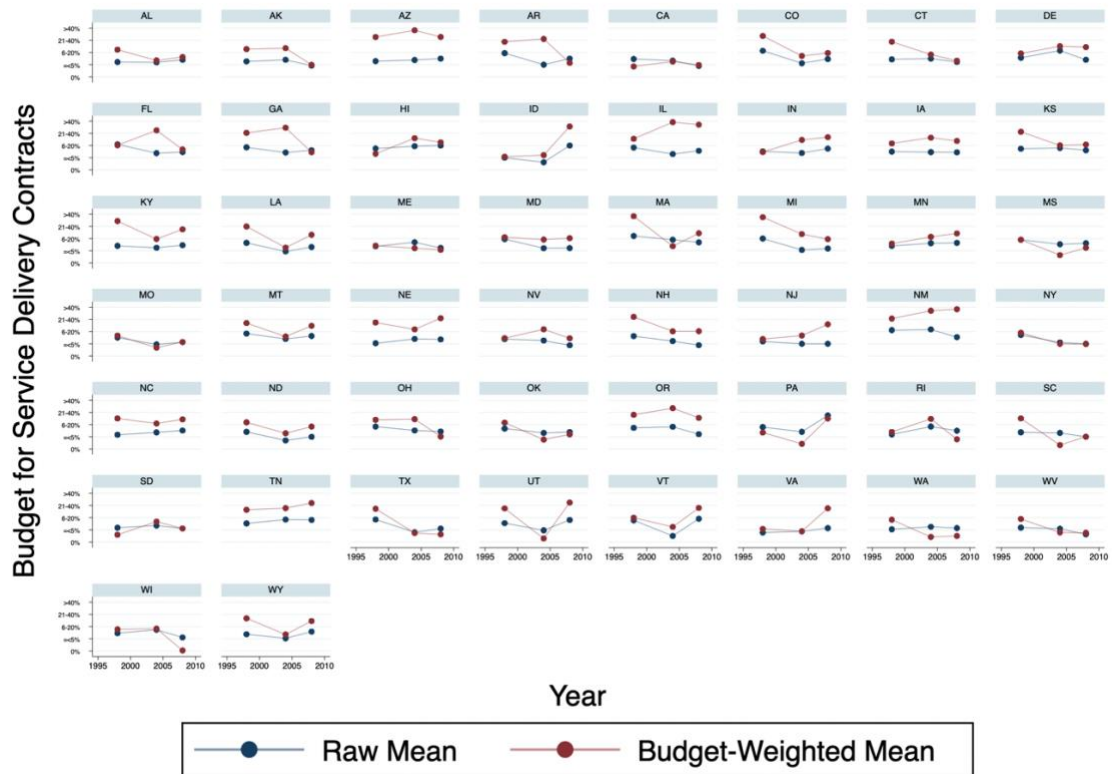
	Description	Source
State Government Employment	Dummy variable; constructed	(Flood et al. 2021)
State Emp*Privatization	Interaction term; constructed	Author
State Privatization	Budget-weighted measure of state privatization; constructed from ASAP surveys.	(Yackee and Yackee 2021)
Female	Dummy variable; constructed	(Flood, King et al. 2021)
Hispanic	Dummy variable; constructed	(Flood, King et al. 2021)
White	Dummy variable; constructed	(Flood, King et al. 2021)
Ed Attain: <=HS Diploma	Dummy variable; constructed	(Flood, King et al. 2021)
Ed Attain: Grad School	Dummy variable; constructed	(Flood, King et al. 2021)
Immigrant	Dummy variable; constructed	(Flood, King et al. 2021)
Veteran	Dummy variable; constructed	(Flood, King et al. 2021)
Married	Dummy variable; constructed	(Flood, King et al. 2021)
Number of Children	Count of children	(Flood, King et al. 2021)
Age	Count	(Flood, King et al. 2021)
Age^2	Age squared; constructed	(Flood, King et al. 2021)
Usual Hours Worked	Count	(Flood, King et al. 2021)
Health Status	Self-reported scale, with 1 being “excellent” and 5 being “poor”	(Flood, King et al. 2021)
Occupation Indicators	Dummy variables; constructed	(Flood, King et al. 2021)
Industry Indicators	Dummy variables; constructed	(Flood, King et al. 2021)
<i>State Economic, Demographic Characteristics</i>		
Interest Group Density	Number of registered interest groups.	(Grossmann et al. 2021) (Lowery et al. 2015)

Table 5.4 (continued)

Union Density	Share of workforce represented by a union.	(Grossmann, Jordan et al. 2021)
Citizen Ideology	Measure of state citizen ideology; higher is more liberal.	(Kelly and Witko 2014) (Grossmann, Jordan et al. 2021) (Berry, Ringquist et al. 1998)
State Govt Ideology	NOMINATE score measuring state government ideology	(Grossmann, Jordan et al. 2021) (Berry, Ringquist et al. 1998)
State Unemployment	Unemployment rate	(Grossmann, Jordan et al. 2021)
State Population	Total count	(Manson et al. 2021)
State % Urban	Percent of population in urban areas; constructed.	(Manson, Schroeder et al. 2021)
State % White	Percent of population that is white; constructed.	(Manson, Schroeder et al. 2021)
State % Hispanic	Percent of population that is Hispanic; constructed.	(Manson, Schroeder et al. 2021)
State % College	Percent of adult population that has a college degree; constructed.	(Manson, Schroeder et al. 2021)
State Govt Revenue (pc)	State spending per capita; constructed.	(Bureau 1998-2012)
State Indicators	Dummy variables; constructed	(Grossmann, Jordan et al. 2021)
Year Indicators	Dummy variables; constructed	Author

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Figure 4.10 Disaggregated Privatization Measures



### APPENDIX 3. THE IMPACTS OF SCHOOL DISTRICT CONSOLIDATION ON RURAL COMMUNITIES

Table 4.12 Data Sources for Chapter 4 Analyses

<i>Variable</i>	<i>Description</i>	<i>Source</i>
Total population	Census data	(Manson, Schroeder et al. 2021)
Treatment	Dummy variable	Arkansas Department of Education
Bachelor's degree rate*	Census data	(Manson, Schroeder et al. 2021)
Labor force participation*	Census data	(Manson, Schroeder et al. 2021)
Poverty rate*	Census data	(Manson, Schroeder et al. 2021)
Population <75% FPL*	Census data	(Manson, Schroeder et al. 2021)
Under 18 share*	Census data	(Manson, Schroeder et al. 2021)
Over 65 share*	Census data	(Manson, Schroeder et al. 2021)
Non-white share	Census data	(Manson, Schroeder et al. 2021)
School count	Scaled per capita	National Center for Education Statistics
School closure	Indicator	National Center for Education Statistics
Lot size (sq ft)	Calculated	(Zillow 2021)
Multi-family residence	Indicator	(Zillow 2021)
Apartments	Indicator	(Zillow 2021)
Single-family residence	Indicator	(Zillow 2021)
Mobile home(s)	Indicator	(Zillow 2021)
Total housing units	Census data	(Manson, Schroeder et al. 2021)
Median number of rooms	Census data	(Manson, Schroeder et al. 2021)
<1 Occupant Per Room Share	Census data	(Manson, Schroeder et al. 2021)
Owner-Occupied Ratio	Census data	(Manson, Schroeder et al. 2021)

\*Values for 2020 carried forward from latest available ACS estimates.

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## VITA

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#### EDUCATION

- 2017 Master of Public Administration  
Martin School of Public Policy and Administration, University of Kentucky
- 2012 Bachelor of Arts, Philosophy, minor in Political Science  
Bachelor of Arts, Spanish  
Summa Cum Laude  
University of Kentucky

#### PUBLICATIONS

Ron Zimmer, Richard Buddin, Sarah Ausmus Smith, Danielle Duffy. (2021). “Nearly three decades into the charter school movement, what has research told us about charter schools?” The Routledge Handbook of the Economics of Education.

#### EXPERIENCE

- 2018 – 2022 Research Assistant  
University of Kentucky
- 2016 – 2017 Finance Graduate Fellow  
Lexington-Fayette Urban County Government
- 2016 Graduate Fellow  
Council of State Governments
- 2012 – 2015 Project Manager  
Epic Systems

#### HONORS, SCHOLARSHIPS, AND GRANTS

- 2018 – 2022 Research Assistantship, Martin School
- 2018 – 2021 Summer Research Assistantship, Martin School
- 2017 – 2018 Teaching Assistantship, Martin School
- 2017 Outstanding MPA Student, Martin School
- 2017 Pi Alpha Alpha National Honor Society
- 2015 – 2017 Graduate Assistantship
- 2015 Daniel R. Reedy Quality Achievement Fellowship Award