School District Boundaries and Segregation: Evidence from Secessions

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Abstract

Using a novel dataset of 175 unique instances since 1985 of United States metropolitan areas seeking to secede and form new, independent school districts, this paper examines the causal effect of a school district secession on segregation within the originally defined district boundaries. Synthetic controls are used to compute counterfactual estimations of how segregation would have progressed in that district absent a secession. This paper finds that secession causes significant increases in income segregation. Additionally, secession produces significant increases in racial segregation in districts that are less racially homogenous at the beginning of the period analyzed in this paper. I verify these results with a battery of sensitivity tests, assessing the stability of the estimates to changes in the specification of the control pool used by the synthetic control. As a further test, I find no significant increase in segregation districts that attempted and failed to secede.

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1 Introduction

In this paper, I seek to determine the extent to which trends in income and racial segregation in a region can be explained by the granularity of the school districts in that area. In places with more school districts, there is greater opportunity for parents to move in search of optimal investments in education. To the extent that these chances to move are limited by the family's wealth or income, more choice can fuel greater segregation. There has been a good deal of research on the positive effects of greater public school competition upon school quality (Hoxby 2000a, Sandström and Bergström 2005). In this paper, I seek to investigate whether there are negative side effects of a specific instance of the creation of greater degrees of district choice, municipalities seceding from larger school districts. If more fracturization significantly increases segregation, this could serve as argument against secessions, which seem to be gaining in popularity. Identifying the causal effect of forming a new district can substantiate an argument for the institution or maintenance of larger school districts nationwide. If policymakers hope to reduce residential segregation, changing school district boundaries can serve as a powerful tool. Additionally, any determination of negative effects of fracturization could provide justification for some of the existing financial incentives meant to induce school districts to coalesce.

School districts are the canonical example of Tiebout choice wherein "residential choices determine the quality of, and expenditures on, local public goods" (Hoxby 2000a). Presumably, more granular school districts, and thus greater Tiebout choice, may lead to greater levels of income segregation. In the United States it tends to be the case that a good deal of each school district's revenue comes from the state or local level. On the local level, school districts are funded by property taxes. Chetty and Friedman's (2011) paper *Does Local Tax Financing of Public Schools Perpetuate*

Inequality?, frames the situation as follows: “If richer families sort into school districts that spend more to produce higher quality education, and quality of education has a causal impact on children’s incomes, then local financing of public schools may propagate income inequality” (Chetty and Friedman 2011). By financing schools on such a hyper-local level, the United States paves the way for the creation of small pockets of parents who have similar demand for educational spending as well as the means to pay for it. This could allow for increased income segregation compared to a counterfactual with broader funding bases. There is evidence that more segregated neighborhood can depress less financially-secure children’s potential for inter-generational economic mobility and contribute to greater levels of income inequality (Chetty et al. 2014).

Previous papers have claimed that racial segregation is often associated with income segregation (Chetty et al. 2014), but in addition to purely income-based mechanisms, greater fracturization could affect racial segregation through sorting. If families prefer to live in school districts with more students of their own race, then the existence of more districts in a region can exacerbate racial segregation. Alesina, Baqir, and Hoxby (2004) find evidence from the formation of school districts in the United States of preferences for racially homogenous school districts. The authors hypothesize that this preference for homogeneity is caused by some combination of people of the same race holding “similar preferences over public policies than those who do not” and of people preferring to interact with others of their own race (Alesina, Baqir, and Hoxby 2004).

The question of how fracturization affects segregation has not been adequately assessed, largely because most school district boundaries within the United States are longstanding legal artifacts. Thus, it is difficult to separate the effects of districting choices from other regional and local characteristics. For example, the Southeastern United States tends to have county-wide school districts and is somewhat of a standout in this way. However, the Southeast has its own regional characteristics and history that makes analyzing segregation in this region using observations of school district size infeasible. The endogeneity inherent to the historical processes that regulated school district formation makes accurately teasing out a causal effect of greater district granularity impossible. Some existing papers on district size use instrumental variables to find exogenous mechanisms that produce greater granularity. An example of such an instrumental variable is the number of streams in a region (Hoxby 2000a). Notably, this method may not transfer well to assessing outcomes like segregation, because such natural barriers could also be relevant to patterns and processes that affect segregation, presenting an endogeneity problem for this analysis. For example, a stream or river may have also been a convenient boundary for other segregatory policies, such as redlining,[1] making it impossible to distinguish the effect of school district boundaries from other segregatory factors.

Another common method of identifying school district level causal effects involves analyzing instances of districts consolidating with one another. This is a robust area of the literature since the general trend in the United States has been a decrease in total number of school districts over time. In 1939 there were 117,108 school districts, a figure that dropped to 13,588 by 2010. The bulk of these unifications, however, tended to occur in rural areas where populations are more homogenous, and segregation is more or less a non-issue. The areas where changes in school district boundaries could reasonably affect segregation tend to be urban or suburban. As such, districts that willingly merge are generally quite different from districts where segregation is a topic of concern.

Given these difficulties, some studies on this topic do not seek to control for the underlying

[1]Redlining refers to the Federal Housing Administration's policy of deeming mortgages neighborhoods with lots of African-American residents as too risky to insure. These neighborhoods "experienced a marked increase in racial segregation" beginning in the 1930s, and peaking in the 1970s (Aaronson et al. 2017).

confounding causes of fragmentation and instead simply report the relationship between the observed fragmentation and segregation (Bischoff 2008). However, a causal identification of school district granularity's effects on segregation is of particular interest from an economic perspective because it identifies the effects of shifting K-12 educational choices on segregation through both families' residential choices and district policies. The decision of where to live and raise children is in many ways an economic one, and thus can be usefully modeled. Policy makers can better make use of levers available to them if they understand how changing these investment opportunities can induce changes in choices families make.

In this paper, I provide a novel improvement to the existing literature by identifying and making use of examples of instances where a municipality or other area attempted to secede and form a new school district. These secessions can be thought of as shocks to the level of district fracturization. I find 175 instances in the United States of attempted secessions in the past decades. This method of analysis provides a causal framework to handle concerns of endogeneity regarding the size of school districts and segregation in that area. This provides a substantive contribution to the literature on fracturization relating to secession that is purely observational at this point in time (Bischoff 2008, Taylor, Frankenberg, and Siegel-Hawley 2019). These aforementioned secessions allow for causal estimates. Additionally, they constitute the vast majority of changes to district boundaries in non-rural areas. This provides a mechanism to analyze changes in fracturization, albeit for school districts that could reasonably experience a secession. This sets these districts apart, somewhat limiting the application of any findings about the causal impact of fracturization on segregation to districts that could be motivated to secede.

One reason a municipality may wish to secede is to raise revenue for their schools or lower their tax burden or both. This is a possible strategy for municipalities that have property values far exceeding the average of their school district. In most states, property taxes make up a substantial portion of US school districts' operating budgets. This funding mechanism fosters a situation in which wealthier areas could substantially raise revenue for their local schools by seceding and stopping sharing tax dollars with the rest of the district. Alternately, by seceding and no longer paying a disproportionate amount per person to this school district, these municipalities could shift this money from education to other priorities. Thus, in many cases[2] there are clear financial incentives to secede.

It is also possible that for some districts considering a secession, the funding problem is less critical of a concern than questions of provision. Many states pay such high portions of the costs of public education that parents are far more concerned with the boundaries of the district, that define their children's peer group, rather than where the money to pay for this school is coming from. Alesina, Baqir, and Hoxby (2004) provide evidence from school district merger patterns in the United States of a trade-off between economies of scale and racial and income heterogeneity. This provides evidence that, separate from questions of school funding, in districts that have heterogeneity in race or income, parents may wish to form their own, more homogenous district. In a previous paper, Hoxby (2000b) hypothesizes about positive effects of more affluent peers. One dimension she considers is the example of wealthier parents that "purchase learning resources that get spread over a classroom" (Hoxby 2000b). This potential motivation to ensure wealthier or same racial group peers, or both, is consistent with the politics of exclusion in which political boundaries can serve to regulate public policy to isolate and protect the residents of this jurisdictions residents (Danielson 1976, Bischoff 2008).

[2]Most, but not all, states fund public education at least partly through property taxes on a local level. California is a notable exception, and has completely centralized school financing. These mechanisms are not at play in states with centralized school financing

Yet another possible motivation for secession is a desire for local control over district choices. I observe, in my review of secessions, secessions that do not change finances or racial makeup in a meaningful way, but rather ensure control over a local school. Regardless of why the district wishes to split off, in most cases there is a way to do so. In most states[3] there is a mechanism to form a new district which culminates in a vote.

In this analysis, I use the somewhat random occurrence of the timing of secession events as a source of quasi-exogenous shocks to district granularity. Secessions occur so rarely and are so often driven by small groups of well-connected and driven parents that the existence of such attempts within similar districts is somewhat randomly distributed. The tediousness of secession requirements, which often require some combination of signature collection, public hearings, and feasibility studies ensures that this is not a process that happens on a whim, but instead was driven by a determined individual or group. Furthermore, similar states have differing policies regarding secessions. For example, there were 15 observed secessions within Alabama in the time period in question, but none within other similar Southeastern states such as Georgia, Mississippi, and South Carolina. This is due to the vast differences in legal procedure for secession on a state by state basis. Alabama allows secessions, and the others either do not, or set very stringent standards for approval of a district secession. Additionally, timing of the secessions includes a great deal of variability. There are instances of states suddenly beginning to allow secessions, such as Maine in 2011. Additionally, many of the districts in the southern states operated for decades under a de-segregation order supervised by the Department of Justice–and could not secede until this was lifted. Prior research by Byron Lutz has shown that "once the process of dismissal begins, there is

[3] In some situations a school board can trigger the split without a vote or outside agreement. Nine of the thirty states that have a provision in the state code allowing secessions do not require a vote (EdBuild 2019). However, even within these states, school secessions are usually accompanied by action by voters.

an element of randomness in the length of time it takes for a district to be dismissed. Decisions are often appealed, adding further randomness to the date" (Lutz 2011). This is in addition to other time-variant quasi-random policy changes or attitude changes that could affect secessions.

In this paper, I present the methods used to create a novel dataset of instances of United States school districts attempting to secede since 1985. I use the definitions of school district boundaries at the beginning of the time period in question as units of interest when computing segregation so as to keep the geographic units constant.[4] This allows me to determine how much segregation has increased in this geographic area, that formerly was one district, as a result of district(s) seceding and increasing fracturization. I first use this novel dataset to analyze the effect of secessions with a propensity matching technique. I construct comparison groups for districts in Alabama that experience a secession by matching with similar districts in other Southeastern states that happen to have much more stringent legal standards to secede. I find that, in part due to the limited sample of Alabama districts that were seceded from, this strategy does not succeed in identifying any significant effect of secession.

To make use of all instances of districts being seceded from, I use a synthetic controls methodology as a systematic way to choose logical control groups for counterfactual estimates. For each district that is seceded from, I create a "synthetic version" of that district with similar covariates and pre-secession trends. I find significant increases in entropy based measures of income segregation as a result of a school district's secession when considering all districts. I am able to make valid statistical inference claims regarding these estimates by bootstrap sampling from placebo synthetic control estimates to analyze of average effects of secession in a manner consistent with methods

[4]For the vast majority of districts, this historical boundary is the same as the current boundary so this equivalent to finding segregation within this district. In the case of districts where secessions have occurred, I calculate segregation over the area that formed a single district at the beginning of this analysis.

used in Acemoglu et al. (2016). Additionally, I consider the effects of secession in districts that were more or less racially homogenous at the beginning of this study separately by separating districts into groups based on whether they had more or less white students, as a percentage, than the national average at the beginning of our analysis.[5] I find that the groups that began with fewer white students have significant increases in racial segregation as a result of secession.

These estimates are validated by considering sensitivity tests. I find a confidence interval of estimates produced by the aforementioned empirical strategy by varying the control group used to produce synthetic controls. I can then compare this confidence interval to the bootstrapped confidence interval used for statistical inference. I find in nearly all specifications, income segregation within the original boundaries of a school district increases as a result of secession relative to the counterfactual. The results are further validated by conducting a placebo analysis using the districts identified within this novel dataset as having attempted and failed to secede, for whatever reason. I consider these districts in the same manner as I do for the districts that successfully seceded. I do not find evidence that attempting to secede alone produces increases in any outcome of interest. If these placebo tests showed increases in segregation, it would be a compelling argument in favor of their being an omitted variable or factor, uncontrolled for by the synthetic controls, that is common amongst districts that wish to secede that causes the observed increases in segregation. This placebo specification does not show either potentially disqualifying factor.

[5] I define this for the year 1995, which is among the first years with reliable district level racial data for all states.

2 Legal Context

2.1 Supreme Court Decisions

When school districts began forming as independent political jurisdictions in the eighteenth and nineteenth centuries, the most pressing concern in drawing boundaries was travel time. Since then, the "twentieth-century history of American public education [has been] history of consolidation" (Hoxby 2000a). The number of school districts in the United States has decreased substantially in the past century, with the majority, but not all, of these consolidations occurring in rural areas. Instances of new districts being formed are few and far between. For the most part, school district boundary changes are a one-way process; districts become part of much larger areas, and very rarely is the reverse true. Broadly, the fragmentation of school districts has decreased in the past century as a result of regions agreeing that operating as a unified organization would be mutually beneficial.

After the landmark 1954 Supreme Court case, Brown v. Board,[6] some school districts–particularly in the South–began to try to use any tools available to resist racial desegregation. One common tool for such resistive actions was municipal secession from larger school districts. "Predominately white municipalities in Alabama, Arkansas, Virginia, Louisiana, and North Carolina, to name a few, attempted to secede from county-based school systems shortly after the counties were subject to school desegregation orders" (Wilson 2016). The school secession movement enjoyed this first wave of popularity until 1972, when the Supreme Court ruled in Wright v. Council of Emporia "that a municipality could not secede from a county-based school district if the effect

[6] Brown v. Board of Education of Topeka, 347 U.S. 483 is a landmark 1954 US Supreme Court decision ruling that racial segregation in public schools is unconstitutional. Subsequent rulings established laws regarding the desegregation process.

would be to impede a county school system's ability to desegregate pursuant to a federal court desegregation order." This ruling importantly established that the "courts should not be guided by the motivation of the officials but by the effect of the secession" (Wilson 2016). Since so many municipalities that may have been interested in seceding were covered by federal desegregation orders, this severely impeded the popularity of this movement just as it was getting going. However, a number of districts had already seceded at this point in time and were unaffected by this decision.

In the years that followed this ruling, the rate at which desegregation orders were enacted began to slow down. By 1990, there were nearly no new desegregation orders issued–although many districts were still operating under decades-old orders. In 1991, the entire environment of district compliance with Brown v. Board was upended with a Supreme Court ruling in *Board of Education of Oklahoma City v. Dowell* which defined "the requirements for a school district to be declared unitary and stipulate[d] that once a district achieves unitary status, it must be permanently released from court control." This decision defined a substantially easier pathway to being considered legally "desegregated." The requirements for lifting desegregation orders were further relaxed in the 1992 *Freeman v. Pitts* case and the federal enforcement authority was weakened in 1995's *Missouri v. Jenkins* decision (Lutz 2011). This cascade of decisions allowed many districts to be released from their desegregation orders, thus re-opening the door for secessions in areas where municipalities wished to leave their larger school district.

2.2 State Law

It is not the case that all school district secessions occur in areas that had previously been under desegregation orders. In modern years, there have been quite a few secessions in Maine, where a

2007 law “incentivized school districts to merge as a way to save money amid declining student enrollment.” Initially, Maine school districts would be financially penalized for not consolidating, but in 2011 “Governor Paul LePage signed a bill repealing those penalties” and with the path to smaller, local school districts opened up, Maine schools began seceding en masse (Felton 2019). Maine is just one example where state law and policy have been integral to secession rates.

There is a great deal of heterogeneity in policy amongst the thirty states with processes “codified in state law for towns and neighborhoods to secede from their school districts” in addition to twenty states with no way to secede (EdBuild 2019). Alabama notably has “very permissive rules that make it easy for school districts to leave a county-based district and form their own district” and seceding districts receive very little pushback from countywide agencies with the authority to challenge them (Wilson 2016). Just to Alabama’s east, Georgia’s state constitution “prohibits the creation of new independent school districts.” A community hoping to secede from its larger school district, then would “require action from the state legislature and a constitutional amendment.” These sorts of vast policy differences across state borders are not unique to the Southeast. In Maine, a town that is part of a regional school district “may secede by withdrawing from the regional district.” The withdrawal process involves a “petition and referendum process, approval from the Commissioner of Education, and a second referendum.” In neighboring New Hampshire, the withdrawal process “requires approval from the State Board of Education and a referendum in the district as a whole” (EdBuild 2019). This approval process that requires the district as whole to consent to a town seceding is much harder to win, since many districts do not wish to be seceded from.

The vast state-level differences in feasibility of secession, when combined with the somewhat arbitrary desegregation order release timeline, leads to a situation in which there can exist two very similar districts where one can secede and the other cannot. This determination is connected nearly fully to the state the district is in, and potentially what district court their desegregation order is assigned through. Additionally, some counties even have their own secession policies, a fact that further adds to the heterogenous legal landscape.

3 Literature Review

This paper is related to a vast literature on the influence of school district boundaries and policies on residential sorting patterns. More specifically, I touch upon how school district quality influences families' residential decisions as well as how school district boundaries and policies affect segregation. Additionally, the economic motivation for analyzing causes of segregation rests upon the effects of segregation.The identification strategy employed by this paper is related to a literature regarding both analyses of school district boundary changes and the synthetic control methodology separately.

3.1 Family Residential Choice

This paper is founded upon the premise that families make residential choices in response to public goods, in this case schools, and that the quality of these goods in turn responds to the demands of these families. The theoretical model is derived from Charles Tiebout's seminal 1956 paper, where he introduces this choice process and shows that unlike federal spending, local spending needn't be allocated in a non-optimal way compared to the private sector (Tiebout 1956). This theory alleges that property taxes can be considered similar to prices. Families accept these property taxes in choosing where to live. Unlike in a Walrasian equilibrium for goods where individuals must take prices and quality of goods as given, families are able to vote to influence the property tax amount as well as the quality of the school district. Tiebout (1956) further claims that if families are "fully mobile" then the local governments will produce the appropriate distribution of goods.

There are a number of papers that utilize Tiebout sorting between school districts. Hoxby (2000a) suggests that the degree of Tiebout choice in a metropolitan area can explain that area having more productive public schools and less private schooling. Urquiola (2005) similarly utilizes the Tiebout choice mechanism to claim that district concentration affects both the distribution and concentration of students who go to public schools. In addition to these applied papers, some of the theory about family mobility utilizes this concept. Brunner, Cho, and Reback (2012) estimate the effect of school choice programs on the housing market. The authors claim that because school choice allows for more free choice of public goods, people's housing choices become less tightly married to the local allocation of public services.

There is a breadth of literature founded upon the concept of family residential decisions being influenced largely by their demand for public goods. In addition to this, there are quite few papers solely analyzing families' educational demands. A literature review by Nguyen-Hoang and Yinger (2011) claims to show a demand for higher quality public schools. They find "that house values rise by below 4% for a one-standard deviation increase in student test scores." In a survey of the literature, that focuses on the papers that have appeared since 1999, they determine that most studies find significant effects of school quality. Nguyen-Hoang and Yinger (2011) find that nearly all of the results show that higher quality schools result in higher property values as expected.

3.2 School Districts and Segregation

There has been quite a bit of research on the forces that affect the creation and update of school district boundaries. Alesina, Baqir, and Hoxby (2004) investigate if trade-offs between heterogeneity and economies of scale where heterogeneity is investigated in terms of both income and race. The authors mainly examine the consolidation of school districts in the United States during the beginning of the twentieth century using exogenous changes in racial makeup driven by the world wars. They claim that there is evidence that “people avoid heterogeneity because they do not want to interact with” people of different race or income as well as evidence that “people avoid heterogeneity because different people prefer different public goods” (Alesina, Baqir, and Hoxby 2004). Thus, this paper suggests that school districts’ choices to merge, and the boundaries that they have today, are affected by a desire to increase homogeneity.

Nechyba (2003) provides a general equilibrium model for the effects of school finance on spatial income segregation. This model is used to investigate how different funding mechanisms for public schools affect income segregation. Notably, this model is designed to assess income segregation, and any investigation into racial segregation using his approach is induced through income segregation. The author investigates income segregation caused by 1) differences in house and neighborhood quality and 2) differences in school quality. In this paper, he concludes that efforts to equalize school spending have desegregating effects (Nechyba 2003).

In a sociological paper, Bischoff (2008) assesses the impact of school district boundary fragmentation on racial segregation, measured using an entropy index. Using a variety of sorting theories, the author concludes that racial segregation is a “potential negative consequence of school district fragmentation in metropolitan areas” (Bischoff 2008). The main theoretical results in this paper suggest that fragmentation does increase multiracial segregation between districts. Notably, Bischoff supports this claim by analyzing linear regressions of the effect of school district fragmentation on racial segregation with a number of controls. This does not provide a compelling causal estimate of the effect of fragmentation on segregation since endogeneity problems are still present. In this paper I provide a novel improvement by seeking to show Bischoff's hypothesis in a causal manner.

3.3 Effects of Segregation

Income segregation is frequently mentioned as one of the drivers behind the lack of intergenerational mobility in America. Chetty et al. (2014) find that areas with high levels of intergenerational mobility are marked by having "(i) less residential segregation, (ii) less income inequality, (iii) better primary schools, (iv) greater social capital, and (v) greater family stability." Chetty et al. utilizes a definition for segregation that I revisit in 4.2.1. Using this measure, the authors find that "segregation of poverty has a strong negative association with upward mobility, whereas segregation of affluence does not." The authors also suggest "the isolation of low-income families (rather than the isolation of the rich) may be most detrimental for low-income children's prospects of moving up in the income distribution" (Chetty et al. 2014). Earlier work by Cutler and Glaesser (1997) finds that black Americans in "more segregated areas have significantly worse outcomes than" black Americans in less segregated areas, by using a variety of instruments. They claim that greater segregation leads significantly worse outcomes in the dimensions of schooling, education, and single parenthood.

There is also a wealth of research on the effects of school segregation. Chetty and Friedman (2011) analyze the effects of income segregation on student outcomes. This paper links investment in education to lifetime earnings using data from the STAR experiment in Tennessee[7] connected to IRS earnings data to estimate the portion of the inter-generational correlation between child and parent income that can be explained by better quality schooling. The authors find that educational investment may account for 40% of the correlation between child and parent income, and hypothesize that income segregation, and the associated correlation with educational investment, thus depresses lower-income students' outcomes. Guryan (2004) finds positive benefits to desegregation, claiming that court-ordered desegregation in the 1970's decreased black students' high school dropout rates. Additionally, a study by Card and Rothstein (2007) claims to find "robust evidence that the black-white test score gap is higher in more segregated cities" by looking at SAT micro-data. In an older study on the topic, Hanushek et al. (2002) use Texas panel data on student achievement to assert that, independent of school quality, classmate achievement and other factors, black students in districts that are more integrated have greater achievement.

In all, there is strong evidence of the negative effects of residential segregation as well as the negative effects of school segregation. School district boundaries could affect both residential and school based segregation, by affecting the sorting of families into districts as well as determining school demographic makeups. There has been more study of the effects of racial segregation, but there is still a robust literature on the negative effects of income segregation.

3.4 School District Boundary Change Methodology

The only recent research using school secessions is a paper by Taylor, Frankenberg, and Siegel-Hawley (2019), a team of education policy researchers. In this paper, the authors claim that school district secession in the South contributes to higher levels of racial segregation. Notably this paper

[7]Project STAR was a randomized longitudinal study on class size conducted from 1985 to 1989.

does not present a causal framework for secessions' contribution to racial segregation. Instead, the authors observe the levels of segregation between 2000 and 2015 in a number of counties that experienced a school secession and claim that the portion of segregation attributable to between-school-district segregation within these has increased in that time period. Taylor, Frankenberg, and Siegel-Hawley (2019) do not control for any underlying factors that may have caused these changes, but report increases in segregation in the seven Southern counties they observe as having experienced a secession.

Additionally, there is a robust literature on the phenomenon of school consolidations. Much of it focuses on the cost savings and potential efficiencies of these mergers. Gordon and Knight (2009) seek to "explain political integration generally." Their method provides a simulation based framework to "explore the factors driving which specific subregional mergers take place." The authors "develop a simulation-based estimator that is rooted in the economics of matching." Additionally, Duncombe and Yinger (2007) identify economies of size for school district operating spending by analyzing school district mergers in rural New York. Although school district mergers form a robust area of literature in the analysis school district size, issues of segregation are not particularly relevant in rural areas as was mentioned above in 1, making this method is not particularly useful for this paper's analysis.

3.5 Synthetic Control Methodology

Abadie and Gardeazabal (2003) introduce the concept of a synthetic control in their analysis of the economic costs of terrorism on the Basque Country in Spain. They note that studying this single region is difficult because its economic outcomes are tied to those of Spain as a whole. Furthermore, the Basque country has a number of characteristics relevant to their outcomes of interest that further complicate identifying the effect of terrorism. To solve this, Abadie and Gardeazabal introduce the novel concept of creating a synthetic counterfactual version of the Basque country as a weighted average of other regions of Spain that matches the covariates of interest and economic trends before the incidence of terrorism.

Abadie, Diamond, and Hainmueller (2010) build upon Abadie and Gardeazabal (2003) by formalizing the use of the synthetic control method for statistical inference with the concept of placebo tests regarding the size of the observed effect. One such specification compares the observed effect to the hypothetical effects for units in which treatment status was assigned to control. In this paper, the authors suggest that this method is particularly useful in controlling for unobservables that affect the common time trends of both the treated and counterfactual groups.

In another follow-up paper, Abadie, Diamond, and Hainmueller (2015) further define the usage of synthetic controls to find p-value to be used in statistical inference. The authors claim that this can be "constructed by estimating in-space placebo effects for each unit in the sample and then calculating the fraction of such effects greater than or equal to the effect estimated for the treated unit," The framework in Abadie, Diamond, and Hainmueller (2015) is still limited to single treatment units.

A novel extension of the synthetic control framework to encompass situations with more than one treated unit is presented in Acemoglu et al. (2016). In this paper, the authors offer a framework to analyze the average treatment effect using synthetic controls by weighting the observed effects by how well the synthetic counterfactual matches the pre-treatment trends. Furthermore Acemoglu et al. (2016) defines a method of statistical inference in the case of more than one treated unit. The authors propose randomly drawing placebo treatment groups of equal size from among the control group. They argue that the average treatment effect can be deemed significant at 5% if it is outside of "the interval that contains the [2.5, 97.5] percentiles of the effect."

4 Study Design

4.1 Identifying Secession Attempts

The empirical design of determining the causal estimates of school district fracturization hinges upon finding instances of secession. The full collection of left-behind districts, seceding districts, and dates used in this analysis make up a new and novel dataset. Importantly, almost all instances of municipalities bringing a secession referendum to a vote are covered only locally and as such do not garner much national attention. A variety of strategies were needed to find as many of these votes as possible. These strategies can be broadly categorized as preliminary identification, news review, fuzzy match heuristics, and boundary analysis.

4.1.1 Preliminary Identification

A U.S. nonprofit specializing in the analysis of education data, EdBuild, released a report *Fractured: The Breakdown of America's School Districts* listing 128 instances of school district secession attempts since 2000 that were either successful, defeated, or still in progress. EdBuild formed their list of school district secession attempts in two main ways. The first of which involved surveying news stories, legislation and meeting minutes. This search was occasionally directed by identifying new districts in the publicly available Common Core of Data (CCD) from the National Center for Education Statistics (NCES). Additional districts were identified through correspondence with state officials about secessions (Edbuild 2019). This provided a jumping off point for my analysis.

4.1.2 News Review

I seek to augment the set of attempted secessions by finding examples of secessions not found by the team at EdBuild. These would be secessions that occurred either outside the time frame studied by EdBuild (2000 to present) or were simply overlooked. I began by conducting a news review using both Google and LexisNexis, for districts with combinations of key words and phrases including but not limited to "school district," "split," "secession," and "referendum." I then added properly identified districts to the dataset of found secessions. For districts identified within EdBuild report as having failed to secede, but without a record of when the secession attempt occurred, I found evidence of the timing of these failures by poring over news reports.

4.1.3 Fuzzy Match Heuristic

In addition to internet research I created a heuristic to identify secessions from data files provided by the CCD regarding each school in the U.S. for every school year from 1986-87 to 2013-14. I began by subsetting only to districts that could have possibly been formed through secession. The only such districts are new districts, which can be identified as districts whose uniquely assigned NCES identifier was present in 2013-14 but is not present in 1986-87.

In order to further narrow districts that can be considered to have been formed through secession I took advantage of the fact that, even after secession, many districts have at least one school with an unchanged name. These schools, despite changing districts, can be matched based on their name and zip code. For each new district I then determine what 1986-87 district the current schools came from.

I use the information about where these schools in the new district came from to filter out new districts formed through the unification of many districts, which is a much more common occurrence. To do so, I further subsetted to only districts that are made up of schools from a single 1986-87 district. With high confidence I conclude that the new district was formed by carving out parts from a single, existing, district. For additional certainty, I require that the left-behind district still exists in the last available year of the data. This filters out any instances of renaming. Lastly, I eliminated any districts with a name that indicates that they are a special education jurisdiction,[8] because the formation of special education districts occurs for very different reasons than secession. Thus, what is left is a collection of districts that have nearly definitely formed through secession. I finish this identification process by iterating over each CCD file to find the year in which the districts first appear in the data, which identifies the year the secession occurred.

4.1.4 Boundary Analysis

A secession is inherently a geographically observable event. Any segregation involves the chipping off of a municipality, or group of municipalities, from another district. NCES tracks the boundaries of each school district year over year. Thus, analyzing changes to the boundaries can identify districts that experience an action that creates, closes, or modifies an agency with an effect on other agencies' boundaries. This, combined with the capacity to identify when districts are new, can be used to identify seceding districts. By determining which district the seceding district is carving area out from, it is clear determine what district is being left behind. Since these boundaries are tracked on a yearly basis, it is possible to identify exactly what year the school district seceded.

[8]There exist some districts created specifically for the education of students with physical or mental disabilities. These districts often have names that convey this purpose, and have overlapping boundaries with other more general school districts.

4.2 Measuring Response Variables

4.2.1 Segregation Indices

The segregation indices that I calculate are all measures of multi-group racial or income segregation between schools. These are particularly relevant because the expected mechanism through which school district boundaries affect segregation is educational choice. I can find more granular and responsive estimates of changes in segregation by looking at segregation between school-aged children. To find these estimates, I measure a number of popular metrics of multi-group segregation. These indices conveniently are calculated using yearly NCES data for all years since 1987. This allows for the identification of changes in segregation experienced by students on a year to year basis. I use data at the school level for each of the districts within our dataset. I calculate a dissimilarity index D(Morgan 1975), an information theory index H (Theil 1972), as well as a relative diversity index R (Carlson 1992), all of which are described extensively in Reardon and Firebaugh 2002.

I begin by defining the following notation:

$$
\begin{aligned}
t_j &= \text{number of students in school} j \\
T &= \text{total number of students in the district} \\
\pi_m &= \text{proportion of students of group } m \text{ in the district} \\
\pi_{jm} &= \text{proportion of students of group } m \text{ in school } j
\end{aligned}
$$

in addition to these, I also define two measures of variation in group measurement which are defined below. These are Simpson's Interaction Index I (Lieberson 1969) and Theil's Entropy Index E. Both are methods for measuring diversity that become zero if all students are within the same group and are maximized when all groups have equal shares of the population.

$$I = \sum_{m=1}^{M} \pi_m (1 - \pi_m)$$
$$E = \sum_{m=1}^{M} \pi_m \ln\left(\frac{1}{\pi_m}\right)$$

Using this notation, the measures of segregation that will be used in the ensuing analysis are defined below:

$$D = \sum_{m=1}^{M} \sum_{j=1}^{J} \frac{t_j}{2TI} \mid \pi_{jm} - \pi_m \mid$$
$$R = \sum_{m=1}^{M} \sum_{j=1}^{J} \frac{t_j}{TI} \left(\pi_{jm} - \pi_m\right)^2$$
$$H = \sum_{m=1}^{M} \sum_{j=1}^{J} \frac{t_j}{TE} \pi_{jm} \ln\left(\frac{\pi_{jm}}{\pi_m}\right)$$

For these measures to be stable, it must be the case that the group definitions are stable over time. I therefore restrict the racial groups to: Hispanic, White Non-Hispanic, Black Non-Hispanic, and Asian. Although in some years there is data provided for the portion of Native American students in the school, I exclude this as a group, so as not to bias the indices in years where this data is present.

The requirement of group stability more severely limits the group definitions for income segregation. The only group level income data available on a yearly level is the number of students who are eligible for free lunch (FLE). This rather coarse group definition is used in Owens, Reardon, and Jencks (2016) to analyze income segregation in and between schools and schools districts. The authors mention that defining income segregation using free lunch eligibility "corresponds roughly to segregation of the bottom 20% of the income distribution from the top 80%." They further note that, “if segregation is changing elsewhere in the income distribution (e.g., if children from affluent families are becoming increasingly segregated from middle-income families)” and that these free lunch based on FLE-status will not capture the change” (Owens, Reardon, and Jencks 2016).

All of these indices satisfy a few base level desirable properties. For one, all are organizationally invariant, meaning that splitting up schools while maintaining racial proportions will leave the measure of segregation unchanged. Additionally, they are all size invariant, meaning that multiplying the counts of students of each group in all units by some constant factor would not affect the index of segregation. Importantly, Theil’s information theory index H is the only measure “that obeys the principle of transfers in the multigroup case.” Reardon and Firebaugh (2002) claim that failure to obey the principle of transfers is problematic for a segregation measure since then the measure will not register a decline in segregation when an individual of some group moves to a school where their group has a lower share of students (Reardon and Firebaugh 2002).

The three indices measure the same thing, segregation between groups, in slightly different ways. The dissimilarity index D is an exposure index and is the simplest. Historically dissimilarity has been among the most widely used segregation metrics. Reardon and Firebaugh (2002) propose an interpretation of the dissimilarity index as the percentage of all students who would have to switch schools to make each school representative of the district as a whole, divided by the percentage who would have to switch if the system started completely segregated. This index is most popularly used in two group segregation cases, since it fails to “satisfy the principle of exchanges” when there are two or more groups (Reardon and Firebaugh 2002). It is not particularly well suited for use in this case, but I present it so that our results can be compared to previous literature that uses this index.

Another segregation index R, or relative diversity, is included as an additional specification since it is a diversity ratio and has a relatively intuitive interpretation. This metric is one minus the ratio comparing the probability that two students from the same school are members of different groups and the probability that any two students are members of different groups (Reardon and Firebaugh 2002). This specification is somewhat better suited to multi-group segregation than D and provides an alternate specification of segregation to H.

The most useful segregation index for this analysis is the entropy index H. Reardon and Firebaugh (2002) find that "H is the most conceptually and mathematically satisfactory index, since it alone obeys the principle of transfers in the multigroup case. Moreover, H is the only multigroup index that can be decomposed into a sum of between- and within-group components." The entropy index is an information theory measure that can be interpreted as "one minus the ratio of the average within-unit population diversity to the diversity of the total population" (Reardon and Firebaugh 2002). The entropy index is quite popular and has been used on a number of papers measuring the effects of segregation on intergenerational mobility in the United States, the quality of government in a number of countries, as well as local conflict in Indonesia (Chetty et al. 2014, Alesina and Zhuravskaya 2011, Barron, Kaiser, and Pradhan 2004). This metric is also very commonly used in papers investigating school level segregation because it is particularly well suited to situations with discrete units and groups (Reardon et al. 2012, Owens, Reardon, and Jencks 2016). Additionally, it allows us to compare our results to other literature on this study's topic since it is used in a paper assessing the effects of school fracturization on segregation (Bischoff 2008), as well as a paper assessing school secessions' impact on segregation (Taylor, Frankenberg, and Siegel-Hawley 2019).

As a practical measure, as is mentioned elsewhere, in calculating these segregation indices I maintain the same definition for the district boundaries throughout the whole analysis, even as these district boundaries may have changed as a result of a secession event. Thus, instead of observing changes in segregation that are mechanical as a result of slicing the district, I observe the level of segregation aggregated over the district as it was defined at the start of this analysis. This is particularly useful to make sense of the results of this analysis, which attempts to assess the question of how segregation in an area that was at one point a unified school district is affected by a secession event.

4.2.2 Immediate Response

The primary statistics of interest are the aforementioned segregation indices. However, especially in situations where the shock was too recent for observable migration effects to take effect, some first-order effects on the district that was left behind are interesting in their own right. I consider what happens to districts after they are left behind. The outcomes that I am interested in are the percentage of white students, the total revenue per students, and the local revenue per student in the districts that were left behind.

4.3 Propensity Score Measures

The idiosyncratic differences in state policy about school district secessions can be exploited to surmise the effect of secession on a variety of outcomes of interest: the mean income of the district, the percentage of white students, the local revenue per student, the total revenue per student as well as all six definitions of segregation defined in 4.2.1. A notable limitation is that the districts that are being seceded from are likely quite different from the districts that are not seceded from. The racial and income related response variables of interest are quite likely to be endogenous to decisions about secession. As so, a simple comparison of seceded from districts to districts that never experienced a secession does not contain useful causal estimates for the effect of secession.

To rectify this problem for a select number of districts, I take advantage of the fact that although there are numerous secessions within the state of Alabama, its regional neighbors Mississippi, Georgia, and South Carolina have no successful secessions, and just one unsuccessful secession. As was mentioned in 2, it is relatively easy to secede in Alabama, whereas Georgia's state constitution explicitly prohibits the formation of new independent school districts. Secessions are also quite difficult to effectuate in both Mississippi and South Carolina. In Mississippi, any secession must be approved by both voters and the school board of the district that is being broken apart. This board must determine that the secession will not "seriously interfere with or impair the efficiency" of the district (EdBuild 2019). Thus–the leaders of the district that will have a section broken off must consent to its leaving, which is in many cases against their interest. South Carolina's laws are even more stringent. South Carolina has county-wide school districts that cannot be "divided except by an act of the General Assembly or with the approval of the County Board of Education." To gain approval from the County Board requires "the support of the entire state legislative delegation from the county... a petition signed by at least 80% of voters in the county district" or a petition signed by one third of voters and approved by a majority in a countywide referendum (EdBuild 2019).

Furthermore, Alabama, Mississippi, Georgia, and South Carolina are all states within the Deep South with a shared history of slavery and Jim Crow[9] as well as a number of other similarities in industry and outcomes today that logically may affect segregation. The districts in these three other Southeastern states form a set that could contain very similar districts to those that opted to secede. By quirks of quite different state laws, only the districts in Alabama have feasible paths to

[9]Jim Crow refers to a system of state and local laws in the Southern United States enforcing racial segregation.

secession–so the non-Alabama matched districts are ones that may hypothetically secede–but are blocked from doing so.

Appropriate counterfactuals are chosen with a propensity score matching (PSM) estimator. This estimator uses a logit model to predict a district's likelihood to secede on a number of racial and class base variables observed in the year 2000 using factors from a combination of the 2000 CCD and the US Census. Then, the districts that are in Alabama that secede are matched with three districts with the closest estimated likelihoods of secession within the comparison states. The model is shown below

$$secession = \beta_0 + \beta_1 single\_parents + \beta_2 low\_income + \beta_3 middle\_income + \beta_4 college\_plus + \beta_5 white + \beta_6 total\_rev + \beta_7 local\_rev + \beta_8 h\_race + \beta_9 h\_income$$

where *single_parents*, *low_income*, *middle_income*, and *college_plus* are all observed from the Census and are the portion of households in this district that are single parents, low-income (from \$0 to \$40,000), middle-income (\$40,000 to \$100,000) and of individuals who have a college degree or higher respectively. The variables *white*, *total_rev*, and *local_rev* come straight from the Common Core of Data, and are the percentage of white students, total revenue per student, and local revenue per student. The remaining two explanatory variables *h_race* and *h_income* are segregation indices–specifically the Entropy (H) index–calculated using CCD data.

This model can be used to create the appropriate comparison group for each district from Alabama of three districts from Georgia, Mississippi, and South Carolina. This methodology creates a sample of otherwise equivalent districts that due to the vast heterogeneity in Southeastern school secession policies on a state by state basis can be compared for valid causal estimates. Furthermore, the treated and comparison groups can be compared in a series of balance tests to ensure that their values for the covariates of note were similar. As a matter of practicality, I only analyze Alabama districts that experienced a secession after 2000.

In this analysis, I measure a set of ten outcome variables. From the 2015 CCD I observe the percentage of white students, local revenue per student, and total revenue per student as well as six definitions of segregation that I derive. Lastly, from the 2013-2017 ACS, I estimate the average household income within the district using a trapezoid sum approximation. Although this is not exact, it is the closest available approximation since income data is only reported in brackets.

4.4 Synthetic Control Methodology

4.4.1 Motivation

To look into the effects of secession on all districts that were seceded from–rather than just the districts within Alabama–I employ a different method of constructing comparison groups. Simple regression estimates of the effects of secession are plagued with endogeneity in the racial and wealth factors that would lead to a secession. Additionally, controlling for the observed dimensions that might affect a secession would not be sufficient since underlying causes and attitudes are unobserved but likely impactful. For most states that have observed secessions beyond Alabama, there is not a logical comparison group of neighboring states that fully unaffected by secessions. Additionally piecing together a series of propensity score methods would require a number of assumptions about states' similarities on unobserved characteristics that may not be justified.

Instead, I use an alternative method of estimating the counterfactual of what would have happened to our seceding districts in the absence of a successful secession in order to expand the sample of seceded from districts studied in this paper. I use the method of synthetic controls, introduced in Abadie and Gardeazabal (2003). I estimate the effects of secession on the percentage of white students, local and total revenue per student, as well as the aforementioned racial segregation indices. For each of these outcomes and for each secessionary district I wish to estimate what would have happened absent a secession. To do so I create a comparison group using a somewhat novel methodology, synthetic controls. I create a comparison group that matches the pretreatment outcome trends as closely as possible that can be used for causal estimates on the effects of secession.

4.4.2 Control Pool Selection

Each synthetic control district is a weighted average of a number of other districts. The goal is to create a synthetic district that matches the pre-treatment trends and relevant covariates as closely as possible. In order to do so, I must first define which districts should even be considered as potential controls. On a high level these are districts that are not affected by secession, that have sufficient observations, and are similar to the district of interest. I define these concepts formally. I define $\mathbf{M}$ to be the set of all districts experiencing a secession. As a matter of notation I say that the time of this secession for some district $i \in \mathbf{M}$ as $T_{s,i}$. I note that, for each district i there is a set of districts D_i that should be disqualified from consideration as potential controls by simple logic. Districts that neighbor i will likely have spillover effects from a secession as families in their area change their decisions about where to live. Additionally, I do not want to include as controls any districts that also experienced a secession in the relevant time frame. If in fact I did, I would be definitionally no longer observing the difference between a district that was seceded from and a counterfactual with no exposure to secession. I define $D_i = D_{dist,i} \cap D_{time,i}$ where $D_{dist,i}$ are the districts that are disqualified by being too close in distance to i and $D_{time,i}$ are the districts that are disqualified for experiencing secessions too close in time to the secession. In practice $D_{dist,i}$ is the districts that are directly neighbors with i using NCES boundary data, and, $D_{time,i} = \{j \in S : |T_{s,i} - T_{s,j}| \leq 10\}$ is the set of districts that experience a secession within 10 years in either direction of the secession experienced by i.

In this analysis, the covariates of interest are the racial demographics and household income information from the 1990 and 2000 Censuses and the five year rolling average ACS. I also include the number of students in the district on a yearly basis. To use all relevant information in forming a synthetic district i, I want to incorporate all of these covariates in the time periods for which I have observations. To do so, as a first step, I disqualify any districts that do not have observations for all covariates at all times that i has observations for them in the pre secession period. This balances the dataset, allowing the synthetic controls to be calculated correctly. Notably, since the data used in this analysis is administrative data provided by the National Center for Education Statistics (NCES) and the Census Bureau there is very high coverage of the variables of interest. As such, this step does not disqualify a large number of districts.

Both of the aforementioned steps remove districts that for either logical or practical reasons ought not to be present within the control pool. Due to the sheer number of districts, it is computationally infeasible to construct a synthetic control using the remaining set of districts–which number over 13,000. Thus, in order to minimize the role of covariates in forming the synthetic controls, and subsequently maximize the role of the trends, I choose the 100 districts that are the "closest" to i on the observed covariates. As a notion of closeness, I define "distance" from our treated district to be

$$d_{i,j} = \sum_{\hat{e}_{\gamma,i},\hat{e}_{\gamma,j},\hat{\sigma}_\gamma} \frac{|\hat{e}_{\gamma,i} - \hat{e}_{\gamma,j}|}{\hat{\sigma}_\gamma}$$

where $\hat{e}_{\gamma,i}$ and $\hat{e}_{\gamma,j}$ are specific estimates of some parameter–that may be a covariate or outcome, the notation is defined broadly–such that γ encompasses all estimates used in our analysis at all time frames. Furthermore, $\hat{\sigma}_\gamma$ is the observed standard deviation of this parameter γ. This process culls the set of thousands of potential controls to something that is computationally manageable for creating a synthetic control from.

4.4.3 Synthetic Control Construction

Having defined the group of districts that are potential controls, the next step in forming a synthetic control is finding the optimal weights to place on each district such that both the pre-trends in the outcome of note and the covariates are well matched. To begin, I observe information on $K+1$ districts, where for simplicity the first district is the seceding district and the next K districts are potential controls–meaning that they were not affected by any attempted secession. In this case, I define the treatment to be a successful secession. For districts that experienced multiple secessions, I make sure to define the treatment time to be the first secession.

Following Abadie et al. (2010) and Cavallo et al. (2012), Y_{it}^{cf} is defined to be the response variable that would be observed by district i at time t in the absence of a secession. This is defined for all $i = 1, ..., K+1$ and for all observed time periods, $t = 1, ..., T$. Similarly, I define Y_{it}^{s} to be the response variable that would be observed by district i at time t in the presence of a secession that occurred at time $T_{s,i}$. As a matter of practicality, for districts that have been seceded from multiple times, I define the time of treatment as the very first time that a secession occurs. The ensuing additional secessions are likely connected, and as such cannot be considered as independent treatment events.

We assume that there is no effect on the observables Y, of the treatment before time $T_{s,i}$. If this assumption were broken, and Y was affected through the anticipation of a secession that induces some migration, the expected pattern of such migration would involve same type families, or wealthy families, who wish to be in the more homogenous proposed district moving there. I would expect for this to increase segregation in the years before the secession. The synthetic control methodology would then match on a version of district i that has a more positive pre-secession segregation trend than what would have occurred in district i absent a secession movement. Although this would bias the estimates, as to bias the estimated gap induced by secession downwards. Since the estimated effects of segregation on secession are positive, this makes identification more difficult. Thus it should not call into question increases that are significant despite this downwards bias. Preferably the event would have no anticipation, but in this case the logical direction of the bias induced in anticipation of a secession goes against the direction of the effect, so it should not call into question the validity of any ensuing statistical inference. I continue with this specification with an assumption of no bias, meaning that Y_{it}^{s} is defined on all i and t as before, and for all i and all $t < T_{s,i}$ I assume that $Y_{it}^{cf} = Y_{it}^{s}$,

The statistic of interest is the effect of secession which is $\alpha_{it} = Y_{it}^{s} - Y_{it}^{cf}$. In order to estimate this, I define the following indicator variable that takes a value of 1 if the district is exposed to a secession at this time t

$$D_{it} = \begin{cases} 1 & \text{if } i = 1 \text{ and } t \geq T_{s,i} \\ 0 & \text{otherwise} \end{cases}$$

The only way that $D_{it} = 1$ is if $i = 1$, because by construction the only district experiencing a secession is the first one. I then estimate α_{it} with the following equation

$$Y_{it} = Y_{it}^{cf} + \alpha_{it} D_{it}$$

This equation allows analysis of what the effects on this district of secession in the years following the treatment.

For all $t > T_{s,i}$, definitionally $Y_{it} = Y_{it}^{s}$ so using the equation from before it is clear to see that $\alpha_{it} = Y_{it} - Y_{it}^{cf}$. In order to estimate α_{it} I must construct an estimate for the counterfactual. To estimate this counterfactual, I construct a vector ω of weights on the K observed districts that did not experience a secession. It must be the case that ω has a length of K, and that for each i, $\omega_i \geq 0$, and $\sum_i \omega_i = 1$. Furthermore, $\mathbf{Z_i}$ is defined to be a vector of observed predictors for Y_i that are not affected by the secession. This assumption holds if we assume that the covariates of interest do not contain large anticipation effects of a secession.

Having established these definitions, I seek to find some ω that satisfies

$$\begin{aligned}
Y_{1,1} &= \omega \cdot \mathbf{Y}_{-1,1} \\
Y_{1,2} &= \omega \cdot \mathbf{Y}_{-1,2} \\
&\cdots \\
Y_{1,T_{s,i}-1} &= \omega \cdot \mathbf{Y}_{-1,T_{s,i}-1} \\
\mathbf{Z}_1 &= \omega \cdot \mathbf{Z}_{-1}
\end{aligned}$$

In practice, however, an ω that satisfies all of these constraints perfectly is nearly impossible to find, so I use one that approximately holds, denoted as ω^*. In order to find the best possible values for ω^*, I use a battery of different convex optimization methods and choose the weighting that performs the best on a district by district basis. These methods are the Nelder-Mead (NM), Broyden–Fletcher–Goldfarb–Shanno (BFGS), bounded limited memory BFGS (L-BFGS-B), and conjugate gradient (CG) algorithms. Since there are so many constraints that I seek to match as closely as possible, no single one of these methods performs the best on a consistent basis. However, by consistently using all four algorithms, I increase the overall average quality of match.

Once a best weighting ω^* is chosen, it can be used to form an estimate for the effect of secession on the district that is being matched. As in Abadie et al. (2010) the effect can then be calculated as

$$\hat{\alpha}_{1,t} = Y_{1,t} - \omega^* \cdot \mathbf{Y}_{-1,t}$$

which can be repeated for each seceding district.

4.4.4 Estimating Effect

To estimate of the effect of a secession, I use the methodology proposed in Acemoglu et al. (2016) for aggregating estimated effects in situations in which synthetic controls are used for multiple treated units. I estimate the effect of a secession by weighing the estimated gaps in the post secession period by the synthetic control's quality of fit in the pre trend period. I then compute the cumulative average effect of the secession on the outcome of interest k years out from the secession event as $\hat{\phi}(k,\mathbf{M})$ below

$$\hat{\sigma}_i = \sqrt{\frac{\sum_{t<T_{s,i}} \left(Y_{i,t} - Y_{i,t}^{ctf}\right)^2}{\mid \{t : t < T_{s,i}\} \mid}}$$

$$\hat{\phi}(k,\mathbf{M}) = \frac{\sum_{i\in\mathbf{M}} \sum_{t=T_{s,i}}^{T_{s,i}+k} \frac{1}{\hat{\sigma}} \left(Y_{i,t} - Y_{i,t}^{ctf}\right)}{k \sum_{i\in S} \frac{1}{\hat{\sigma}}}$$

The estimated effect, $\hat{\phi}$, is an average of the observed effects within our sample, weighted by the average mean squared error within the pre trend. From here forward this will be referred to as a match quality weighted estimate (MQWE), which notably gives greater weight to matches that do a better job of matching pretreatment trends. This is an intuitive weighting scheme, since the difference between the actual and synthetic outcomes should "contain more information about the intervention when we are better able to predict" the trends of the seceding districts before the secession event (Acemoglu et al. 2015). A key assumption in computing treatment effects is that the counterfactual should accurately predict what would have happened absent a secession. I have confidence that the synthetic controls that are effective at matching the pre trends are also effective at predicting the counterfactual post trends. As a result, this estimator places more weight upon these more accurately matched districts' observed gaps.

4.4.5 Statistical Inference

In order to make meaningful causal statements using these estimates, I must consider some of the limitations of this study design, which like in Abadie et al. (2015) are the "small-sample nature of the data, the absence of randomization, and the fact that probabilistic sampling is not employed to select sample units." To conduct inference in this abnormal landscape so, I use placebo studies similar to those described in Abadie et al. (2015). The method of placebo analysis that I use compares the effects observed for districts that were actually treated with the effects observed by using the aforementioned synthetic control method for districts that never attempted to secede. This is commonly referred to as "in-place" placebo testing. I seek to estimate the distribution of "in-place" placebo effects by running the synthetic control methodology for a large fraction of the districts that were identified as potential controls. In this situation, due to practical computational constraints, I do not fully observe the distribution of placebo effects for all potential feasible control districts. It would be infeasible to do so for nearly 13,000 districts with a time frame of thirty years that could all be chosen as the $T_{s,i}$ for each of these districts.

Instead, I choose a random sample of 3,750 district time pairs as follows. I choose districts by sampling uniformly among all districts that ever were part of a control pool and were thus eligible to form a portion of the synthetic controls. I choose the associated time of the secession event for each selected district by sampling for secession year using a distribution that is identical to the distribution of observed $T_{s,i}$. This method guarantees that any effects on gaps that might have be caused by overall timing of the secession event will be controlled for by having an identical underlying distribution of secession years. I compute the effects of secession on each of these placebo districts.

Upon forming a large sample of in-place placebos, I use this distribution for statistical inference and to obtain p-values for my estimates. The p-value is the portion of placebo effects that are greater than the observed effect for treatment–which can be interpreted as a standard p-value. Notably this methodology does not produce confidence intervals on the magnitude of my observed effect, but it can be used to assess if this observed effect is statistically different than zero.

In order to assess the statistical significance of the estimates of $\phi(k,M)$ as calculated in 4.4.4 I use the bootstrap methodology. I choose a random sample of the placebo districts R with the requirement that $|R| = |M|$. I then calculate $\phi(k,R)$ as described above, for all k of interest. I repeat this methodology 5,000 times, forming a vector of randomly sampled sets of districts $\mathbf{R}$ with the sole requirement that $\forall R_j \in \mathbf{R}, \forall d \in R_j, d \notin M$. This ensures that R and $\mathbf{M}$ are disjoint, and no part of the bootstrapped estimate contains a seceding district. I assess the p-value of the estimate of some $\phi(k,\mathbf{M})$ as

$$p\left(\phi(k,M),\overrightarrow{\phi(k,\mathbf{R})}\right) = 2\min\left[percentile\left(\phi(k,M),\overrightarrow{\phi(k,\mathbf{R})}\right), 1 - percentile\left(\phi(k,M),\overrightarrow{\phi(k,\mathbf{R})}\right)\right]$$

where $\overrightarrow{\phi(k,\mathbf{R})}$ is a vector of all the bootstrapped match quality weighted estimates (MQWE). Intuitively, this specification leads to an observation for $\phi(k,\mathbf{M})$ being deemed statistically significant at 5% if it is not within the interval of the 2.5th and 97.5th percentiles of bootstrapped estimates. This is logically analogous to a two sided t-test. In words, the estimate is considered statistically significant at 5% if fewer than 2.5% of the bootstrapped samples of the same size as the seceded from districts made up of districts that did not experience a secession event have an estimate of MQWE that is of greater (or less, depending on the direction of the effect of the treatment) than the MQWE districts that have experienced a secession event.

Due to different motivations and dynamics in secessions in majority white districts, I expect to observe somewhat different segregation outcomes depending on the district's diversity. To assess this, I split out these effects to investigate the racially segregatory outcomes in regions where race could feasibly have been a motivating factor for secession. In these cases where I limit the set of observed districts, I also limit the placebo district pool to be made up of only districts that serve as potential controls for this subset of districts.

4.4.6 Sensitivity Test

Similar to Acemoglu et al. (2016), as a robustness check I seek to verify that the counterfactual predictions are not entirely dependent on just a couple matched control districts. In order to do so, I randomly hold out 5% of the potential controls identified from the methodology described in 4.4.2. I repeat this 15 times for each identified secession. In order to see the effects of these holdouts on the final statistic of interest, the MQWE, I create a bootstrapped sample of 5,000 MQWE observations by randomly choosing one observation of MQWE with held out controls for each treated district.

This bootstrap methodology produces a range of estimated counterfactuals that would have been produced with slightly different control group formation choice. Using this range, I can form a confidence interval for the value of the MQWE, had the control pool been formulated slightly differently, by finding the percentiles of the estimated effects. This confidence interval can then be compared to the confidence interval calculated in 4.4.5 to asses how different control pool composition could affect significance of estimated effects

4.4.7 Estimates on Failed Secessions

Next, I seek to determine if two alternate explanations that discredit the role of boundaries have support. The first is that the effect is the result a credible threat of secession. The second is that the observed effects are caused by something unobservable about districts that wish to secede that cannot be matched with synthetic controls.

To assess this, I define the set of districts that experienced an attempted but unsuccessful secession to be A. To find a true estimate of the effect of attempting a secession, I calculate $\phi(k,A)$ as described in 4.4.4, treating the attempted seceders as though they actually succeeded in seceding. Because the secession did not actually occur, the logical date of the secession event is not obvious in all cases. For districts that went to a referendum, that then failed, the faux secession date is clearly the year of this vote. Similarly, for districts whose secession hopes were shot down by a legal authority, the faux secession date is the year of that decision. For the districts whose secession movements petered out, for the sake of this analysis, I define the date to be a year after the most recent available media coverage of attempts for secession. The failed secessions are not identical to the successful secessions, so the set of placebo districts needs to be re-generated to rectify this. The districts that are ever selected as part of a failed secession's control pool are sampled using the underlying time distribution of failed secessions to estimate the confidence interval on placebo effects in a manner identical to the sampling procedure in 4.4.5. This provides an estimate of the effect on a similar group of districts of trying to secede and can be compared to the actual effects induced by boundary change and successful secessions. These effects can only not provide support for discrediting theories.

5 Data

5.1 Identifying Secessions

The school districts used in this analysis–those that have been identified to have seceded or attempted to secede from larger districts–form a novel dataset assembled especially for this project. This data was aggregated from a variety of sources, as outlined in 4.1 above. I used a list from EdBuild's original review of archival reporting (EdBuild 2019) as a jumping off point. The determination of additional districts, as well as finding the years associated with the unsuccessful secessions, was done using articles stored in the LexisNexis database as well as a wide variety of publicly available local newspaper articles. Many of the other districts were identified using school information by year from the Common Core of Data (CCD) with the heuristic described above in 4.1. In addition to these methods, secessions were identified using data about school district boundaries provided by the NCES.

5.2 Measuring Observables of Interest

I incorporated a wide variety of other data sources that had aggregate statistics on the district level. Each district has a unique NCES identifier, which allows for easy integration of information from a variety of agencies. The United States Department of Education has published data about each district for every year since 1987 as the CCD. From this I can observe relevant demographic, financial, and enrollment statistics for each year used in this analysis. Most of the statistics of note are covered from the CCD files on the district level. Additionally, the CCD includes racial demographics as well as free lunch enrollment counts on the school level for the same time frame. These estimates are necessary for the calculation of the segregation indices.

For the most part, the CCD data is high quality, but there are occasionally misreported statistics that will affect the segregation metrics. I rectify these by either dropping the observation or correcting it through other mechanisms. If there is a district where the count of free lunch eligible students is larger than the total number of students in the district, I recode this to be a year that did not observe FLE data because in these cases I have no observation of how many non-FLE students are in the district. However, if the sum of the racial observations is higher than the total number of students in the school, I luckily have observations of the count of students of each race, I replace the total number of students with this sum. If there is a district with a non-zero number of total students, and zero students in all of the racial categories, I recode this to be a year that did not have observed racial statistics. For any districts missing racial counts or with faulty totals, I also calculate racial data by aggregating school level variables to the district level. Using this, I replace any missing district level observations where there is racial data available on the school level by building up. For most districts this is not a problem, but this sort of data cleaning and correction is necessary to ensure that not only are the racial and income statistics correct, the segregation indices are well defined. The indices are designed to have values between 0 and 1 and any district level data that does not have portions summing to one can result in indices far outside of this range, and will adversely affect the validity of any estimate.

I augment the CCD level data with information about each district from the 1990 and 2000 decennial Censuses as well as five year averaged American Community Surveys (ACS), ranging from 2005-2009 to 2013-2017. These contain demographic information about all households in the district as well as information that is not contained within the common core of data such as adults' educational attainment, household incomes, and the ages of the population. Notably the Census has nearly full coverage of the population, whereas the ACS is a smaller sample augmented by administrative data. Other than as one of many propensity score matching outcomes, I exclusively use ACS data as covariates in this analysis.

As a practical note, when forming synthetic controls, I only include a specific ACS survey as a pre-secession covariate if the survey instance include no observations after the secession event. For example, for any observations from the 2007-2011 ACS to be used as a matching covariate the district in question must have experienced a secession event in 2012 or later. The logic of this is to exclude any observations after the secession, that may have been affected by the secession, so I ensure that I am not matching on post treatment effects. I combine all of this data into a large, unbalanced, panel dataset which contains descriptive statistics for each year of every school district in the United States, not only those that attempted to secede.

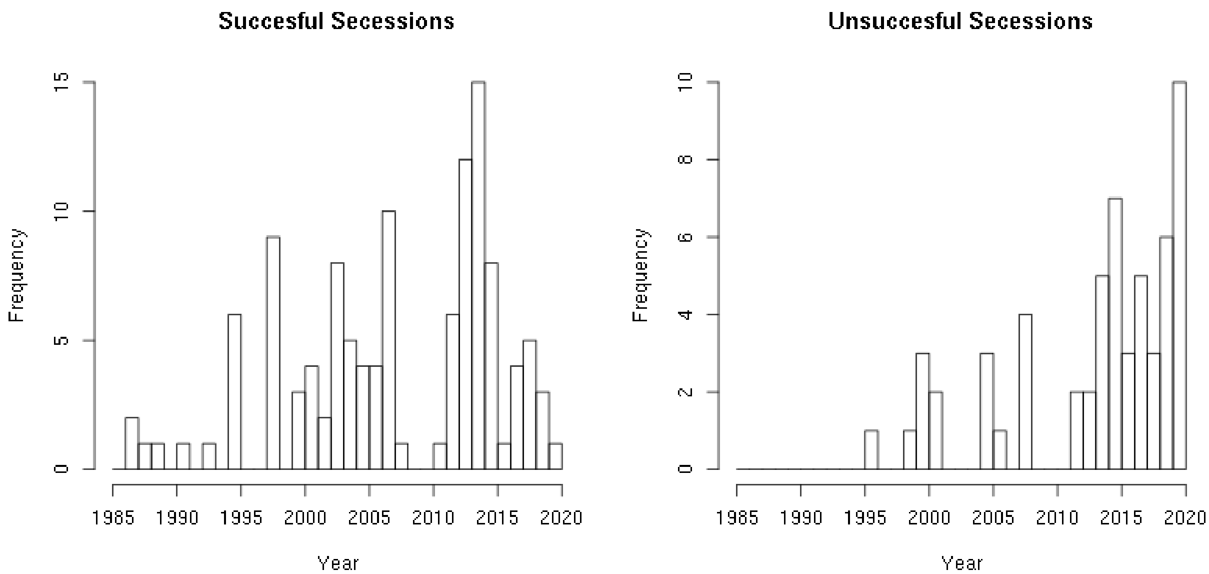
As part of the synthetic control pool formulation, I must be able to determine which districts are neighbors with one another. To do so, I use district boundary data from 2015 and 1995, collected by the US Census Bureau and published by NCES. which defines districts as a polygon. By using this information, it is possible to find which districts share boundaries. I define neighbors to be any districts that share a border at the 1995 or 2015 mark. Although there are observed secessions before 1995, that is the first available year for school district boundary data.

6 Results

6.1 Secession Identification

The three main methods of discovering secessions have a good deal of overlap in terms of which districts they identify. This lends credibility to the argument that each method is valid. Overall, I identify 175 unique secession attempts. To begin, 128 of these are identified by EdBuild. Another twenty-one were identified through the news review methodology. By analyzing the district boundary information, I was able to identify fifty-three secessions, eight of which had not been discovered through EdBuild or through news review. The heuristic defined in 4.1.3, using name consistency over time uncovers twenty-seven secessions, nineteen of which were not discovered by the other methodology. Of these, 175 secession attempts, 118 were successful secessions. Figure 1 presents the distribution of the years for which I identified any secession action. Fifty-six of the observed successful secessions occurred in 2010 or later, so these districts have a shortened post period in which the effects of secession can be observed. Of the 118 observed successful secessions, only thirty-six districts were left behind and still in existence in 2015 since so many municipalities seceded from the same district, and some of the left-behind districts ceased to exist after multiple secessions. However, we can still districts that ceased to exist. If they have any post secession observations, these are relevant. Additionally, these "disappeared" districts will still have values for segregation for all time periods. This is because segregation is calculated over this district's initial boundaries and there will still be some enrollment figures despite the initial educational agency ceasing to exist. As such, this study includes forty-five unique districts that we observe to have experienced a secession.

Figure 1: Time Distribution of Identified Secessions and Attempts



Note: Both time distributions are clearly skewed towards more recent years. This can likely be explained by some combination of it being easier to identify more recent secessions due to their news coverage, as well as the popular explanation that secessions are becoming more popular. The incomplete secessions are found at a much greater rate in more recent years, largely due to the fact that they are only identifiable through their inclusion in the EdBuild report and archival news searches.

I have identified fifty-seven unique unsuccessful secessions, which provides a large sample of districts similar to the districts that experienced a secession event in many ways aside from the actual occurrence of a secession. The year of occurrence associated with this districts that failed to secede in my analysis is calculated as is described in 4.4.7.

These unsuccessful secessions are far more difficult to identify than the successful instances. This difficulty is a partial explanation for why there are fewer unsuccessful observations. Notably, due to the ad hoc manner in which I discovered failed secession attempts, I cannot make any inferences about trends in popularity of secession within the United States using this dataset. However, beyond differences in frequency of news coverage and availability to this coverage, there seem to be no clear geographic biases in the process of finding these secession events. The full collection of secessions is likely quite similar to the map of secessions and attempts found in Figure 2, and has large heterogeneities in secession frequencies between states that could otherwise be considered quite similar.

Figure 2: Geographic Distribution of Identified Secessions and Attempts

Note: Seceding is very popular in some states and and virtually unheard of in others. There are clearly many secession attempts in Alabama, California, Illinois, Maine, Massachusetts, Tennessee and Wisconsin and very few, if any, in other states.

Since it is unlikely that the identified districts constitute the entirety of all secession attempts, there are obvious concerns about selection, particularly because the selection methodology is not randomly distributed. In all likelihood, based on the methods employed by EdBuild as well as the additional methods I use, the sample will be biased towards districts that receive more news coverage. This will not be a problem for either empirical strategy, since both propensity score matching and synthetic controls form, in different ways, counterfactuals for each district of interest based on observed covariates, as well as in the case of synthetic controls, pre secessions trends that account for unobservables. However, in applying the observations found in this paper, it is notable that they are somewhat more relevant to districts that would receive more media attention.

6.2 Descriptive Statistics

To begin to understand what happens immediately after a secession I compare the racial demographics and per pupil revenue information about the districts that seceded and the districts that never attempted departure. I look at these comparisons in 1995, before all but six of these secessions took place, and in 2015, the last year with high quality reported revenue data about the school districts.

The descriptive statistics for all school districts in the United States in 1995 can be found in Table 7 in the appendix. These overall values can be compared to the corresponding values for the districts that were eventually left behind, in Table 8. The districts that experience a secession event have more than twice the percentage of black students as the national average. Additionally, these left-behind districts receive more than twenty percent less in per-student revenue from local sources. The districts that were left behind tend to be more racially diverse at the outset and have less local revenue per student than the rest of the US.

Twenty years later, in 2015, the baseline racial demographics and revenue information nationwide changed substantially and are available in Table 9. Summary statistics for the districts that were left before 2015 can be found in Table 1. Those same figures for the districts that seceded before 2015 are found in Table 2.

The seceding districts have much larger revenues per student than the districts that were left-

Table 1: Descriptive Statistics from 2015, Left Districts

Statistic	Mean	Median	Min	Max	N	St. Dev.
Percent White	0.87	0.96	0.02	1.00	36	0.23
Percent Black	0.02	0.01	0.00	0.28	36	0.05
Local Revenue Per Student	4,401.65	2,833.09	812.90	17,518.72	36	4,020.64
Total Revenue Per Student	8,532.48	7,300.55	4,034.48	19,566.84	36	3,652.13

Table 2: Descriptive Statistics from 2015, Seceding Districts

Statistic	Mean	Median	Min	Max	N	St. Dev.
Percent White	0.66	0.66	0.03	1.00	40	0.24
Percent Black	0.10	0.01	0	1	40	0.20
Local Revenue Per Student	9,591.89	8,153.52	3,022.94	22,254.10	40	5,212.20
Total Revenue Per Student	16,734.68	14,824.13	9,346.22	38,708.33	40	5,986.70

behind. The average local and total revenue per student in the seceding districts are more than double the same figures for the left-behind districts. Additionally, on average, the seceding districts tend to have fewer white students and more black students than the districts that were left. Thus, the areas that secede tend to be more racially diverse than the district that they leave behind. This surprising observation could be connected to the fact that many of the observed more diverse cities leaving more racially homogenous countywide districts.

Next, I wish to begin to understand what factors are connected to a district being likely to attempt to secede, and what factors are connected to their likelihood of succeeding. I create an indicator variable $attempt_i$ that is one if district i attempted to secede and zero otherwise. Similarly, $secede_i$ is an indicator variable for successful secession. I run the logit model described in 4.3 for all districts in the United States, using these outcomes defined above and excluding districts that experienced an attempt before 2000. The results of this regression can be found in Table 10. Significant at the 5% level are the findings that fewer single parents and more racial segregation in

2000 have positive correlations with the likelihood of attempted and successful secessions. Also statistically significant is the finding that the portion of middle-income households is positively correlated with attempting, and the portion of low-income households[10] is positively correlated with succeeding in seceding. Lastly, it appears that a greater portion of college graduates is positively associated with attempted secession. These coefficients are not causal claims, nor does this model investigate interaction effects between the included covariates. These coefficients are useful in painting an introductory picture of what factors affect likelihood of secession, however they are not causal findings.

6.3 Propensity Score Methods

Within the state of Alabama, I observe twenty-two secession attempts, seventeen of which are successful. However, there are only seven districts that are being seceded from. Notably, my propensity score matching methodology does not match municipalities within larger districts on their likelihood to secede. Instead it matches districts on their likelihood to be seceded from, so the sample size of districts to be matched in this section of the analysis is at most seven. This is so much lower than the total number of successful secessions because the school districts in Alabama are countywide, so there can feasibly exist many seceding areas within one district. Due to the specification of this model, I must subset further to the five districts that are first seceded from after 2000 [11]so that the Census variables in the logit propensity score model are not affected by secession. All five of the observed failed secession attempts occurred in districts that also

[10]I define low-income households to be households making under $40,000 a year in the year 2000. I also say that middle-income households in 2000 are making $40,000 to $100,000 a year.

[11]There are two Alabama districts that experienced secessions before 2000, Jefferson County and Madison County. Hoover City seceded from Jefferson County in 1988, so I determine that the loss in accuracy of predicting the remaining five post-2000 secessions with 1990 Census data rather than the 2000 Census is not worth gaining one observation.

experienced a successful secession, making it impossible to differentiate the effects of an attempted secession from those of a successful secession with this methodology.

The subset of districts with observations for total revenue per student, local revenue per student, percent white, income as well as income and racial segregation includes 395 total districts within the four states of this subset. This number is relatively low, because similar to Alabama many of the districts in Georgia, Mississippi, and South Carolina are countywide.

I find the following results when comparing the outcomes of districts that in Alabama to their matched group of three comparison districts from Georgia, South Carolina, or Mississippi. The logit model used for this matching can be found in the appendix as Table 11. For this model, percentage of low-income households has a positive coefficient that is significant at the 10% level, which would indicate that within the Southeast, districts with more low-income households are more likely to experience a secession. This does not mean that low-income households will be among the seceding group, but rather that having more low-income households is predictive that the district overall will experience a secession event. Additionally, the percentage of single parents is significant at the 10% level, and the percentage of adults with a college degree or greater is significant at the 5% level. These factors decrease and increase the likelihood of secession respectively. Intuitively, districts with fewer single parents and more highly-educated adults appear more likely to attempt to secede.

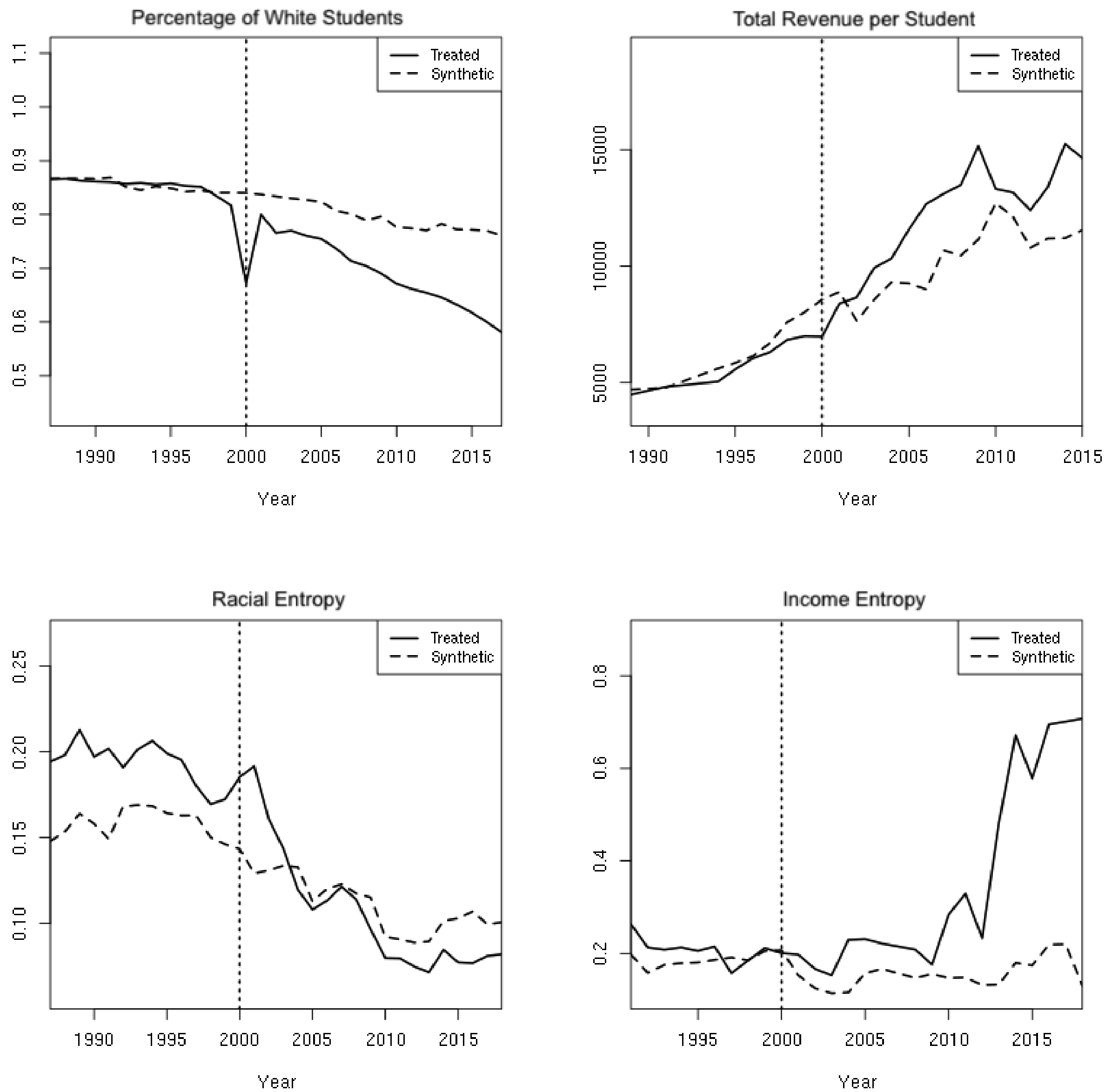
The results of this analysis can be found in Table 12. None of the results are significant, highlighting a significant drawback of having only five districts within the treated group. Notably, Alabama is among the states with the starkest legal contrasts with its neighboring states' secession policies where the neighboring states are still quite similar. Unless propensity score matching is used to find the likelihood that specific regions within secede, this method of analysis, applied to seceding districts, will remain plagued by small sample sizes.

6.4 Synthetic Controls

6.4.1 Individual Synthetic Controls

This method of analysis is built upon creating unique counterfactual districts for all of the seceded from districts in my sample. To provide a clear sense of what is going on, I include a selection of plots below in Figure 3 as a visual representation of what is being calculated by the synthetic controls. The specific district being shown is Middletown City School District in Middletown, Ohio. In 2000, a referendum was passed by the people of the City of Monroe to create a new district named Monroe Local School District. The plots below are plots of the absolute paths of a selection of outcomes of interest, and provide a justification for the need for synthetic controls to match a diversity of possible pre-treatment trends. The segregation metrics are calculated by analyzing the original boundaries of Middletown City School Districts. In the case of synthetic pre secession trends and covariates are calculated and then used to match to synthetic controls using the definition of segregation that considers the original boundary. Thus, any change in the definition of the unit of aggregation would show a mechanical change in secession.

Figure 3: A selection of Synthetic Control Estimates for Middletown City School District, OH

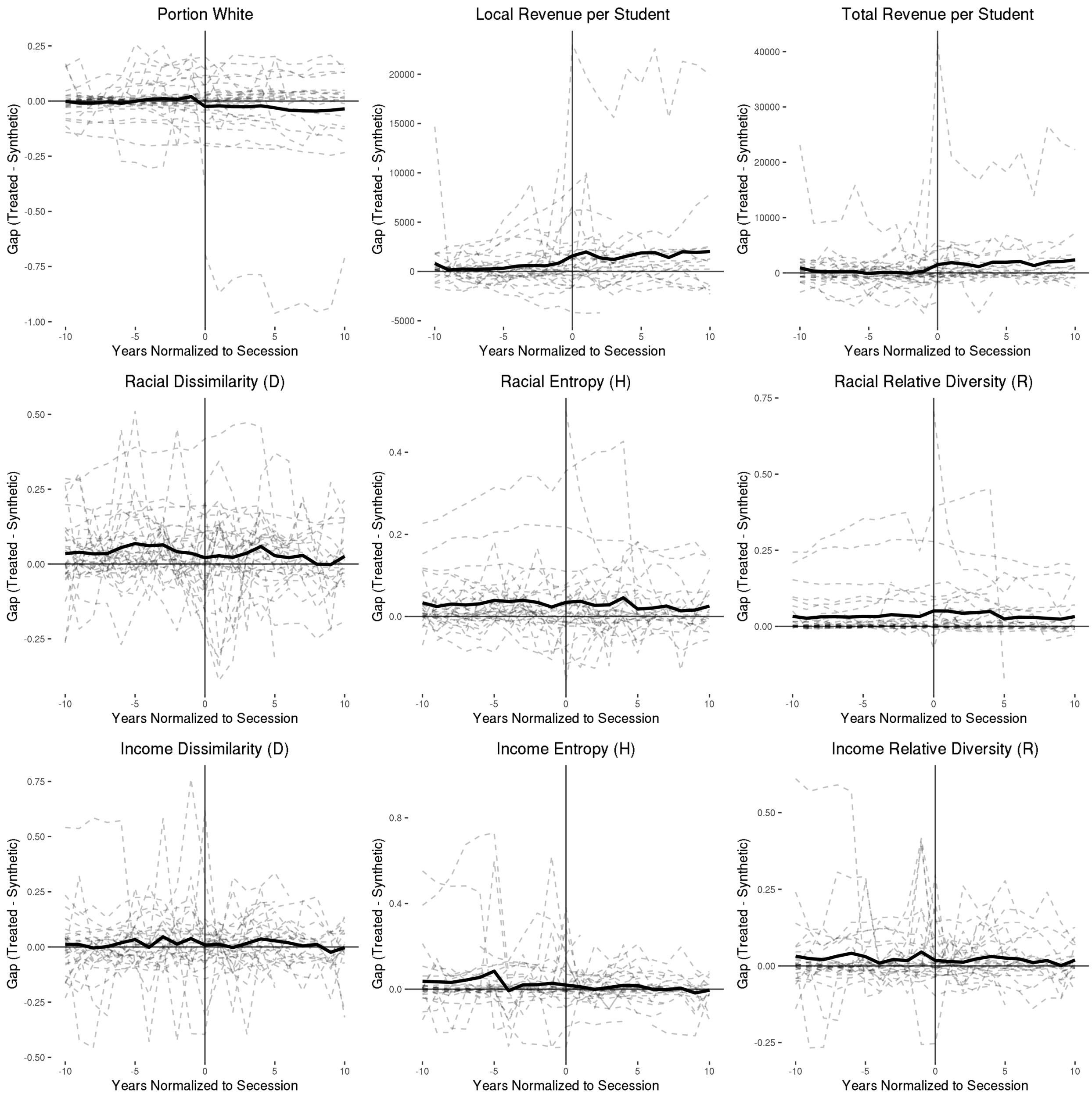


Note: The two top panels represent first-order effects that are observed on just the left-behind district. The bottom two panels are segregation indices calculated over the area that would have been Middletown City School District absent a secession. Notably, this is just one instance of a secession event, and the comparisons encapsulate just one unit's estimated effect. The "treated" line represents the actual data from Middletown City School District and the "synthetic" line represents the counterfactual created by the synthetic control.

As can be seen in Figure 3, many school districts, particularly the smaller ones, experience large changes in the outcomes of interest from year to year. This leads to some level of difficulty in matching pretrends and a good deal of noise. This is visually apparent in Figure 4 below which depicts for each seceded-from district, the gap between that district's outcomes and its counterfactual. This gap is the statistic of interest because it represents the change in outcome induced by secession. Additionally, for some districts represented in Figure 3, their gaps are not observed for the whole time frame presented. This is because some districts seceded either fewer than ten years after the first year in this data, 1987, or fewer than ten years before the final year, 2018. When I calculate $\phi(k, \mathbf{M})$ I exclude the districts that do not have observations up to year k so as not to attempt to extrapolate gaps beyond what was observed.

Another point that is visually expressed in Figure 4, particularly in the racial entropy panel, is the need for the MQWE as an estimator. There are a number of districts with consistently higher gaps in racial entropy than their synthetic control throughout all of the pre-period. This poor matching could be for whatever reason, be it a control pool with very different racial entropy or a failure of the optimization routines to find an optimal weighting. In these cases it is not compelling to claim that the continued large gaps after secession are caused by the secession. The MQWE will, by design, place very little weight on the gaps observed for districts with poorly matched pre-trends, and instead will place more weight on districts with well matched pre-trends.

Figure 4: Gaps Between Seceded-From Districts and Synthetic Controls



Note: The numerous grey lines in each of these panels represents the individual gaps between the treated group and the formulated synthetic controls. The average gap is represented by the thick black line. As a result of the large year to year changes in the data, some pre-secession estimated gaps are non-zero. The MQWE procedure controls for this by placing greater weight on the better matched districts.

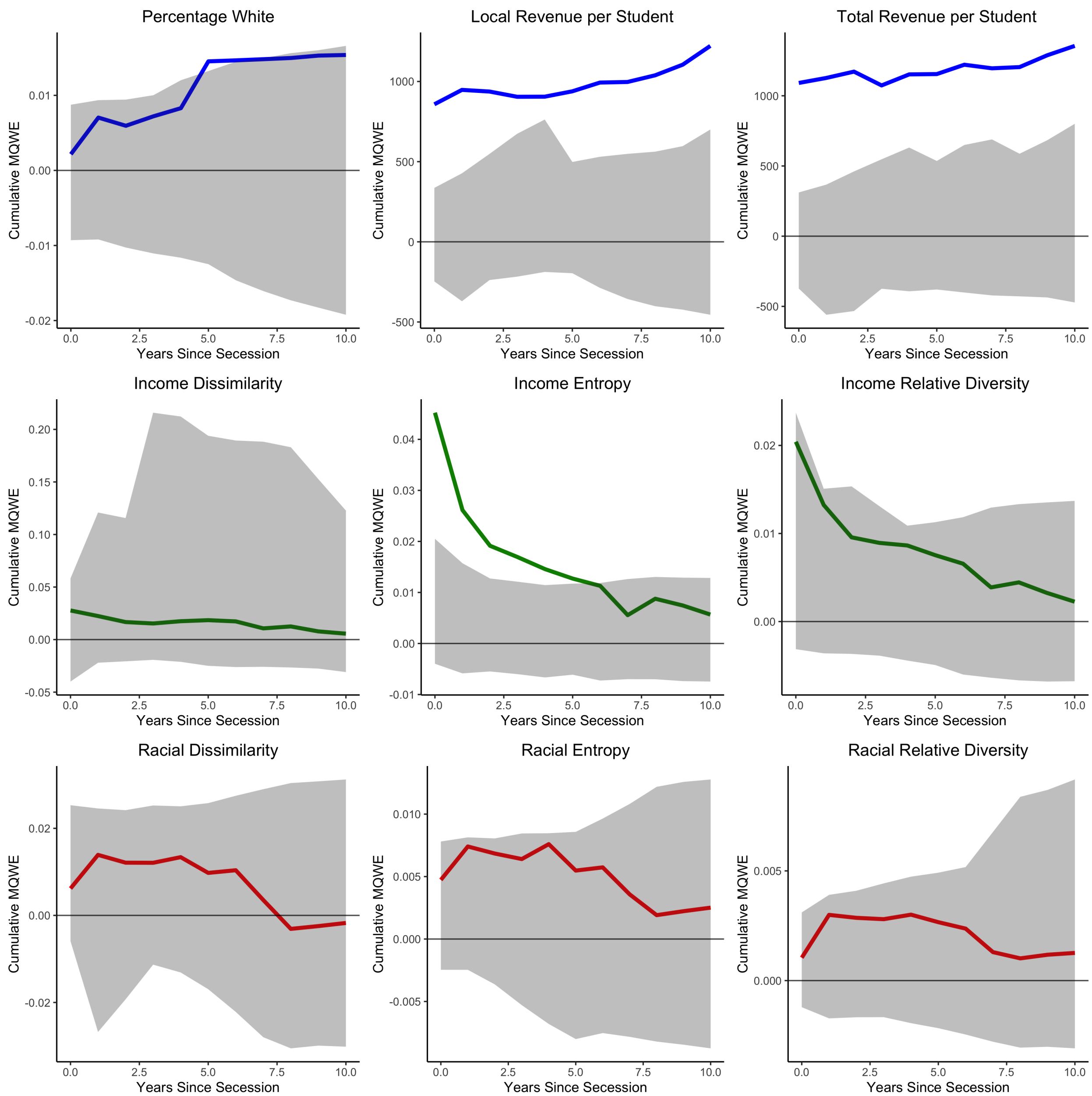
In addition to these visuals, average gaps after secession–between the seceded-from group and the synthetic group–can be found in the Appendix. The first-order effects can be found in Tables 13, 14, and 15. The average gaps for racial segregation are in Tables 16, 17, and 18. Additionally, the analogous results for income segregation can be found in Tables 19, 20, and 21. In nearly all time periods analyzed, the average effect, measured relative to the synthetic controls, is a modest increase in segregation. For all outcomes of interest, in almost all years, the observed range of gaps in each year is quite large, with the 25th percentile being negative and the 75th percentile effect being positive.

6.4.2 Statistical Inference Overall

I find the cumulative Match Quality Weighted Effects (MQWE) as described in 4.4.4 for the set of secession districts. In order to find appropriate p-values for these estimates, I run 5,000 bootstrapped samples of the 3,750 placebo districts that formed the control as described in Acemoglu et al. (2016). These effects are the cumulative average gap, meaning that for this outcome $\phi(k,\mathbf{M})$ is the average gap between the seceded-from districts and their synthetic control in the first k years. This outcome is useful because while there is a good deal of noise from year to year, sustained gaps are indicative of a treated effect, so this allows for more accurate and finely tuned identification, as used in Acemoglu et al. (2016).

A graphical representation of these effects can be seen below in Figure 5, in which the line represents estimated MQWE. The MQWE is significant at the 5% level if the aforementioned line is outside of the grey confidence intervals created using bootstrapped MQWE placebo observations. Notably, some of the trends in the lines are not the result of changes in the effects over time but can be explained by changes in composition of the group used to calculate the MQWE when districts do not have observations up to and beyond that point in time post-secession.

Figure 5: Match Quality Weighted Estimates with Confidence Intervals



Note: The grey area represents the 95% bootstrapped confidence intervals for the effects of secession on the outcomes of interest. The line is the observed MQWE for the group of districts that experienced a secession event. I find substantial increases in revenue per student as a result of a secession. Additionally, income entropy substantially increases in the years following a secession. One thing to note is that in the calculation of $\phi(k, \mathbf{M})$, as described in 4.4.4, if a district does not have observations up to time k it is not considered for this estimate. As such, each estimate that forms the line are not necessarily constructed from consistent sets of districts' gaps.

The first-order effects are presented in Table 3 below and represent the change to just the district that was left behind, with observations calculated over the new boundaries.[12] The effect of being seceded from on both total revenue and local revenue per student is a statistically significant increase. This is observed on the districts that are left behind, and is true for all time periods. These raises in revenue could be in some way connected to the fact that in some instances, the seceding district has to pay the left-behind district to purchase supplies and facilities that previously belonged to the district as a whole. This could explain some portion of the increase in local revenue.

Interestingly, the estimate for the change in the percentage of white students in the district that was left behind is a statistically significant increase. I break the observed trends down by the racial profile of the district in 6.4.3. This estimate can be in part be explained by different regional trends and by the fact that districts with a greater portion of white students have more stable racial enrollments hovering near one. As such, the synthetic control better matches those pre-trends, resulting in these districts receiving a greater weighting in the MQWE which biases the estimates in the direction districts with higher initial levels of white students.

The estimates for the effect of secession on racial segregation, as measured by the three indices defined in 4.2.1, can be found in Table 4 below. This analysis, which considers all the seceding districts as a group finds no evidence of any change in racial segregation as a result of secession. Further analysis of impacts of racial segregation by district characteristics can be found in 6.4.3.

Lastly, I present the estimated effect on income segregation, or more specifically the segregation between the poorest 20% of students (who qualify for free lunch) and the remaining 80% of students, can be found in Table 5 below. This metric is not well suited to detect more nuanced levels of income segregation, such as segregation between students of different incomes where

[12]This is different than the observations for segregation which hold the geographic unit of interest constant. These first order effects show how the district that has been left behind changes after secession.

Table 3: Cumulative Changes to First Order Effects

Years Since Secession	Percent White	Total Revenue per Student	Local Revenue per Student
0	0.00216	857.76***	1091.35***
1	0.00704	947.61***	1127.11***
2	0.00595	937.28***	1172.01***
3	0.00721	904.64**	1073.63***
4	0.0083	905.31**	1152.22***
5	0.01453**	938.76***	1154.56***
6	0.01466**	993.33***	1221.16***
7	0.01482*	996.95***	1195.58***
8	0.01498*	1038.52***	1204.23***
9	0.0153*	1104.92***	1288.23***
10	0.01538*	1221.98***	1355.21***

Note: It is highly statistically significant that both specifications of revenue per student increases. Additionally, after five years, the average increase in percentage of white students is statistically significant. In 6.4.3 I discuss this change in percentage of white student further.
Asterisks denote significance levels (***=1%, **=5%, *=10%).

Table 4: Cumulative Changes to Racial Segregation

Years Since Secession	Racial Dissimilarity	Racial Entropy	Racial Relative Diversity
0	0.00618	0.00474	0.00104
1	0.0139	0.00741*	0.00299*
2	0.01211	0.00685*	0.00286
3	0.01209	0.0064	0.0028
4	0.01338	0.0076*	0.003
5	0.00976	0.00548	0.00266
6	0.01037	0.00574	0.00237
7	0.00353	0.00357	0.0013
8	-0.0031	0.00191	0.00101
9	-0.00246	0.00223	0.00118
10	-0.00174	0.00251	0.00126

Note: There is weak evidence after two and after four years of increased in racial entropy in the sample as a whole. When considering all secessions together there is no evidence of increased racial segregation.
Asterisks denote significance levels (***=1%, **=5%, *=10%).

neither qualifies for free lunch. Despite the lack of ability to detect these other types of income segregation, there is still statistically-significant increases in income entropy, H, in the immediate years following a secession, indicating that secessions cause greater levels of income segregation. Notably, as mentioned in 4.2.1, entropy metrics are the most popular segregation index in modern literature regarding segregation (Chetty et al. 2014, Bischoff 2008, Taylor, Frankenberg, and Siegel-Hawley 2019).

Table 5: Cumulative Changes to Income Segregation

Years Since Secession	Income Dissimilarity	Income Entropy	Income Relative Diversity
0	0.02772	0.04523***	0.02042*
1	0.0224	0.02614***	0.01324*
2	0.01668	0.01915**	0.00956
3	0.01534	0.01693**	0.00893
4	0.01749	0.01457**	0.00864
5	0.01848	0.01272**	0.00754
6	0.01733	0.0113*	0.00657
7	0.01069	0.00556	0.00388
8	0.01253	0.00877	0.00445
9	0.00781	0.00745	0.00326
10	0.00564	0.00567	0.00226

Note: In the year of the secession, and the ensuing five years, there is evidence that income segregation in the most commonly used segregation index rises substantially. This effect fades with time which cannot be determined by this analysis as the fault of greater noise or changing composition of the districts that form the estimator.
Asterisks denote significance levels (***=1%, **=5%, *=10%).

6.4.3 Statistical Inference by District Race Profile

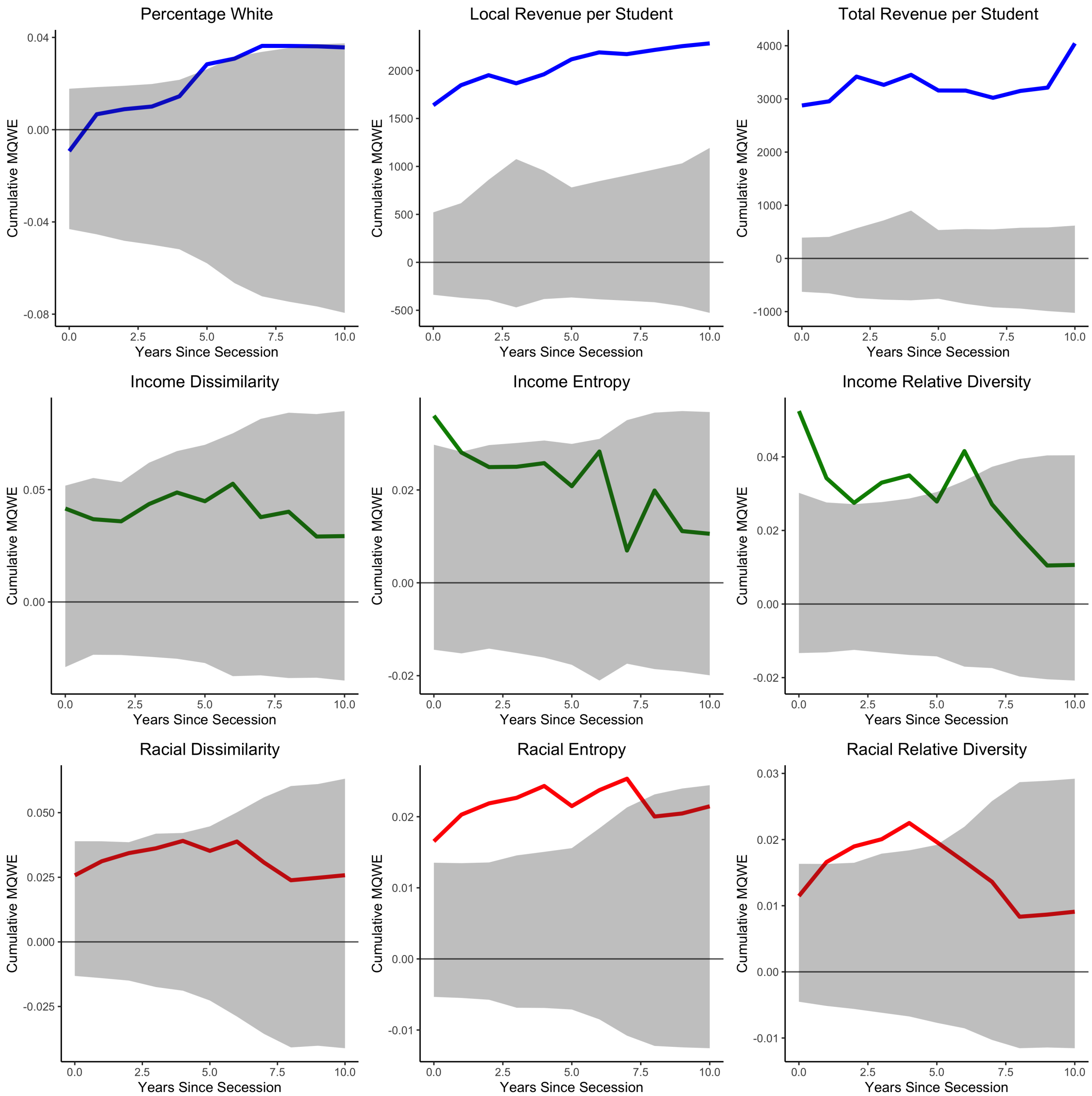
Within this analysis, there is a a great deal of heterogeneity in the reasons and motivations for seceding. There are some instances such as when the town of Sebago withdrew from School Administrative District 61 in Maine where the seceding district is just one school that serves ninety students. This secession was prompted by to fears that this elementary school would be shut down by the larger district (Junker 2017). A number of the observed secessions result in very small districts–mainly in rural, white areas. These secessions do not have much of an impact, if any, on segregation because there is very little racial heterogeneity in these areas to begin with.

On the other hand, there are also secessions like that of St. George in East Baton Rouge Parish in Maine that collected 18,000 signatures in their attempt to secede (Harris 2019). The racial effects in this secessions are likely quite different than those in Sebago, Maine. In order to determine if rural white districts were drowning out the effects of secession in more racially diverse districts, I split the set of secessions in two. I let the national average of percentage of white students in 1995, which is the first year with reliable racial data that covers most districts, serve as a border between districts that are deemed more or less white.[13] Intuitively, if the concern is that school district secessions can cause greater racial segregation, this can only really be a relevant concern in the case where the district was at most than 82.3% percent white in 1995. Using this split in the data I find the following results after having redrawn the bootstrap sample for each specification to only include districts that are part of the relevant racial split.

These results shown in Table 6 show a highly statistically significant effect of secession on racial segregation in districts that begin with more non-white students. Thus, it is fair to conclude that in these less white districts secession causes greater racial segregation. In addition to the racial entropy estimates, the estimates and associated p-values for the other outcomes of interest for the less white districts can be found in the appendix in Tables 22 and 23. Similar figures can be found for the more white districts in Tables 24, 25, and 26.

[13] The "more white" districts are those that had a higher percentage of white students in 1995 than the national average. Conversely, the districts that are "less white" are the ones that had as a percentage, fewer white students in 1995 than the national average. 1995 is chosen since it is the earliest year with low levels of missing district-level racial observations.

Figure 6: The Effect of Secessions in Less White Districts



Note: Although there are only seventeen districts that were seceded from that began with a higher percentage of non-white students than the national average, the ensuing effects on racial segregation are positive and significant. Additionally, there is a significant increase in income segregation in estimates of $\phi(k,\mathbf{M})$ for smaller values of k. There are not consistently positive effects on the percentage of white students in these districts.

Table 6: Cumulative Changes to Racial Segregation in Less White Districts

Years Since Secession	Racial Dissimilarity	Racial Entropy	Racial Relative Diversity
0	0.02575	0.01654**	0.01146
1	0.03121	0.02029***	0.0166**
2	0.03435*	0.02188***	0.01896**
3	0.03619*	0.02266***	0.02005**
4	0.03908*	0.02432***	0.0225**
5	0.03522	0.02149**	0.01962**
6	0.03883	0.02374**	0.01666
7	0.03073	0.02534**	0.01362
8	0.02385	0.02003*	0.00834
9	0.0248	0.02046*	0.00866
10	0.0258	0.02147*	0.00909

Note: Within the districts that began with fewer white students, as a percentage, than the national average in 1995 I observe clear and statistical increases in racial segregation as a result of school district secession.
Asterisks denote significance levels (***=1%, **=5%, *=10%).

6.4.4 Sensitivity Tests

In order to understand the effects of holding out random samples of districts that would otherwise form the control pool, I bootstrap a sample of the estimates of the effect of secession by sampling once from each district's set of estimates formed using a randomly selected sample of 95% of the regular control pool. I plot the 95% confidence interval of the MQWE atop the figure shown in Figure 5. The results of this analysis can be found in Figure 8 in the Appendix. It is visually clear that the estimation of the cumulative average effects of secession on income entropy remain significant, but depending on the sample, may more quickly cease to be so. Additionally, there are some early estimated effects on racial entropy that, depending on how the control pool was created, could have been considered significant. The effects on per student revenue maintain their significance for all estimates generated by the sensitivity tests in the relevant time periods.

6.4.5 Estimates on Failed Secessions

To lend credence to claims that the mechanism affecting these outcomes of interest are the tangible changes to district boundaries as a result of secession, rather than something underlying about seceding districts that was not controlled for, or simply the threat of secession, I consider districts that attempted and failed to secede. The estimates for all the outcomes of interest for these districts that fail at seceding are found in an identical manner to those for districts that succeed at seceding. The only difference is that rather than bootstrapping 5,000 estimates from a sample of 3,750 placebo districts, the sample from which I bootstrap in this case is 1,200 placebo districts. Additionally, as a result of there being fewer unique identified failed secessions, the sample size is forced to be 30 districts, rather than the 45 used for the successful secessions.

Results of this analysis can be found in Figure 9 in the Appendix. Additionally the specific estimates and their associated p-values can be found in Tables 27, 28, and 29. None of the outcomes of interest show any effect of secession at any time frame, other than a statistically-significant decrease in income entropy in the year of the failed secession, indicating that very short term a failed secession could decrease income segregation.

As such, there is not evidence of some unobserved factor that is unable to be matched in the pre-period shared by districts that wish to secede. Additionally, there is not evidence that the mechanism at play is the threat of a secession, or the rhetoric and public opinion surrounding secession, rather than changing district boundaries that affects segregation.

7 Conclusion

This study uses a novel dataset of school districts that have seceded from their larger school district in the United States to analyze the effects of splitting a district through secession. The districts that are seceded from are compared to counterfactual synthetic versions of themselves to find district level estimates of the effects of secession on segregation within the geographic area that the district covered before it was broken apart by secession. These district level estimates are then aggregated to find average treatment effects of secession for a variety of outcomes. The significance of these estimates can be estimated using bootstrap sampling as described in Acemoglu et al. (2016). I find that, when looking at all districts, school district secessions increases income segregation, as measured by an entropy index. For more racially diverse districts,[14] racial segregation rises significantly, as measured by a similar entropy index, in the years immediately after a secession relative to the counterfactual amount of expected segregation absent a secession.

There is a wealth of research on the negative impacts of segregation on both racial and income on a variety of factors including schooling, income mobility, employment, and single parenthood (Card and Rothstein 2007, Chetty et al, 2014, Cutler and Glaeser 1997). In many ways, income and racial segregation is segregation of opportunity. Greater racial and income segregation within schools keeps kids from privileged together, and apart from the rest of their peer cohort. The large black white school achievement gap has been in the past been attributed to these sorts of separations (Hanushek 2002).

This paper has important implications, beginning with its very obvious and direct application to the question of school district secessions. In many of the instances studied in this paper where

[14]In this context, racially diverse districts are defined to be districts that had fewer white students, as a percentage of enrollment, than the national average in 1995.

municipalities attempt to secede, supporters of secession claim that they long for "local control" over their children's education. While this may be true, there is evidence that the expected outcome of them gaining this local control is increases in segregation across multiple dimensions. Particularly in the case of more diverse districts, there is evidence that these secessions will cause increased racial segregation. This paper provides an improvement to the existing literature about school secessions, by providing causal evidence of this increase, rather than just observational conclusions.

Additionally, since secessions are among the only examples of districts splitting apart, this provides some evidence that breaking a district into smaller pieces could exacerbate income segregation and potentially racial segregation, at least in districts that are similar to districts that would wish to secede. This paper provides evidence of this claim by taking advantage of the idiosyncrasy in state-wide and local policies towards secession comparing the effects of secession to cases where districts attempted and failed to secede. Although they do not form a perfect counterfactual, districts that launch a secession movement, campaign for it, and watch it fail do not experience significant increases in segregation, providing an indication that the mechanism through which secession operates on segregation is district boundary change rather than other unobservables such as public opinion.

There is evidence that segregation amongst public school students has been rising in recent years (Reardon et al. 2012, Owens, Reardon, and Jencks 2016). Stopping this trend has proven to be a difficult task. The findings of this paper have important implications about what *not* to do in pursuit of that goal. Additionally, there is research indicating that school district secessions are rising in popularity (EdBuild 2019). This paper provides evidence that this secessionary trend is antagonistic to the goal of school desegregation, or even a goal of slowing school resegregation.

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A Appendix

Table 7: Descriptive Statistics from 1995, Overall

Statistic	Mean	Median	Min	Max	N	St. Dev.
Percent White	0.82	0.9	0	1	13,503	0.24
Percent Black	0.06	0.01	0	1	13,503	0.15
Local Revenue Per Student	3,502.20	2,586.96	0.00	93,700.00	13,503	3,556.76
Total Revenue Per Student	7,073.42	6,247.22	0.00	172,265.60	13,503	3,939.22

Table 8: Descriptive Statistics from 1995, Left-Behind Districts

Statistic	Mean	Median	Min	Max	N	St. Dev.
Percent White	0.79	0.97	0.06	1.00	39	0.31
Percent Black	0.16	0.01	0.00	0.94	39	0.28
Local Revenue Per Student	2,764.35	2,414.64	765.43	9,913.11	39	1,668.55
Total Revenue Per Student	6,212.78	5,896.57	3,986.00	15,649.48	39	2,017.54

Table 9: Descriptive Statistics from 2015, Overall

Statistic	Mean	Median	Min	Max	N	St. Dev.
Percent White	0.70	0.80	0.00	1.00	12,945	0.27
Percent Black	0.07	0.01	0.00	1.00	12,945	0.15
Local Revenue Per Student	7,824.87	5,428.99	0.00	595,256.40	12,945	13,271.39
Total Revenue Per Student	16,000.89	13,333.33	702.44	688,564.10	12,945	14,683.21

Table 10: Correlation of Secession Likelihood with Observables

	attempted	seceded
	(1)	(2)
single_parents	-0.00001^{**}	-0.00002^{**}
	(0.00001)	(0.00001)
i0k_40k	0.00002	0.0001^{**}
	(0.00002)	(0.00003)
i40k_100k	0.00003^{***}	-0.00002
	(0.00001)	(0.00003)
plus_college	3.261^{**}	0.516
	(1.278)	(2.228)
p_white	-4.122^{*}	-2.966
	(2.239)	(4.460)
p_black	-1.208	2.295
	(1.271)	(2.843)
pct_white_2000	2.006	4.129
	(1.517)	(2.574)
total_rev_per_s_2000	0.00000	0.00003
	(0.0001)	(0.0001)
local_rev_per_s_2000	-0.0001	-0.00003
	(0.0001)	(0.0001)
h_2000	6.397^{***}	6.104^{***}
	(1.130)	(1.491)
h_income_2000	0.571	0.958
	(1.211)	(1.464)
Constant	-4.108^{***}	-7.482^{***}
	(1.311)	(2.883)
N	8,703	8,703
Log Likelihood	-317.673	-188.571
Akaike Inf. Crit.	659.346	401.142

Notes: ***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.
The variables with statistically significant coefficients are concluded to have strong correlations with districts' choices to secede and their success at doing so.

Table 11: Model Used for Matching for Seceding Districts in Alabama and Neighboring States

	seceded
single_parents	-0.0004^{*}
	(0.0002)
i0k_40k	0.001^{*}
	(0.0004)
i40k_100k	-0.0001
	(0.0002)
plus_college	22.709^{**}
	(11.431)
p_white	122.920
	(90.547)
p_black	102.485
	(86.907)
pct_white_2000	-7.177
	(20.180)
total_rev_per_s_2000	0.0004
	(0.002)
local_rev_per_s_2000	-0.001
	(0.002)
d_2000	5.319
	(8.068)
d_income_2000	15.850
	(13.411)
Constant	-128.181
	(93.106)
N	413
Log Likelihood	-10.302
Akaike Inf. Crit.	44.604

Notes: ***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.
The portion of adults with a college education or greater can be noted to be the only statistically significant predictor in this case.

Table 12: Propensity Score Estimates of the Effect of Successful Secession on a Variety of Outcomes

Dependent variable:	Estimate:
Percent White	−0.033398
	(0.14676)
Total Revenue per Student	−490.86
	(456.97)
Local Revenue per Student	−648.38
	(673.09)
Mean Income	−9634.5
	(8196.7)
Racial Dissimilarity (D)	0.043849
	(0.088893)
Racial Entropy (H)	0.01901
	(0.066177)
Racial Relative Diversity (R)	0.012381
	(0.080337)
Income Dissimilarity (D)	0.050737
	(0.041171)
Income Entropy (H)	0.023432
	(0.025322)
Income Relative Diversity (R)	0.033872
	(0.033681)
Original number of observations	413
Original number of treated observations	5
Matched number of observations	5
Matched number of observations (unweighted)	15

Note: These effects of secession on a variety of outcomes using AL districts are not significant largely because of small sample size.

*p<0.1; **p<0.05; ***p<0.01

Table 13: Synthetic Gaps: Portion White

Statistic	N	Mean	St. Dev.	Min	Pctl(25)	Pctl(75)	Max
0	35	−0.025	0.159	−0.696	−0.029	0.031	0.200
1	34	−0.022	0.175	−0.870	−0.045	0.044	0.173
2	33	−0.026	0.166	−0.806	−0.063	0.033	0.209
3	32	−0.026	0.164	−0.786	−0.065	0.033	0.204
4	29	−0.022	0.175	−0.787	−0.062	0.046	0.196
5	26	−0.031	0.217	−0.962	−0.071	0.045	0.202
6	23	−0.042	0.220	−0.937	−0.044	0.039	0.166
7	19	−0.045	0.235	−0.913	−0.059	0.046	0.169
8	19	−0.045	0.246	−0.955	−0.081	0.045	0.197
9	19	−0.042	0.244	−0.938	−0.081	0.044	0.207
10	19	−0.035	0.196	−0.709	−0.094	0.044	0.167

Table 14: Synthetic Gaps: Total Revenue per Student ($)

Statistic	N	Mean	St. Dev.	Min	Pctl(25)	Pctl(75)	Max
0	34	1,494.687	7,369.564	−5,254.383	−821.762	1,476.979	41,365.210
1	30	1,856.755	4,282.248	−1,938.175	−447.220	2,840.985	21,110.820
2	28	1,589.097	3,901.022	−3,462.651	−123.720	1,886.146	18,785.350
3	26	1,144.294	3,922.763	−7,191.554	−344.932	1,744.332	16,735.090
4	23	1,945.425	4,531.777	−3,427.924	−191.675	1,989.611	20,092.890
5	18	1,947.774	4,570.618	−859.882	−404.109	1,727.968	18,233.400
6	18	2,068.583	5,453.227	−2,388.675	−431.713	1,758.856	21,900.630
7	18	1,324.487	3,612.912	−2,250.080	−327.585	1,678.034	13,991.080
8	18	1,993.393	6,460.224	−2,026.892	−683.423	1,344.792	26,613.830
9	18	2,057.095	5,739.593	−1,891.046	−267.318	2,223.131	23,529.800
10	14	2,364.537	6,238.651	−2,710.583	−695.906	1,916.404	22,281.860

Table 15: Synthetic Gaps: Local Revenue per Student ($)

Statistic	N	Mean	St. Dev.	Min	Pctl(25)	Pctl(75)	Max
0	32	1,582.518	4,670.049	−4,157.277	−509.984	1,924.465	23,054.390
1	29	1,974.658	4,576.477	−4,240.394	−253.691	2,231.719	19,802.240
2	27	1,378.679	3,806.420	−4,193.076	−414.371	2,134.469	17,420.850
3	25	1,207.796	3,407.041	−2,413.310	−546.346	1,721.239	15,620.600
4	22	1,567.962	4,463.029	−1,766.752	−200.637	1,769.448	20,524.470
5	17	1,882.291	4,665.917	−2,052.211	52.302	1,877.047	19,048.800
6	17	1,909.534	5,533.754	−1,723.851	−655.170	1,830.115	22,585.040
7	17	1,410.083	3,924.974	−1,545.455	−640.505	1,933.940	15,594.190
8	17	1,994.935	5,244.205	−1,773.121	−29.919	2,145.132	21,299.770
9	17	1,927.900	5,319.999	−1,987.611	−605.541	1,949.565	20,963.300
10	14	2,020.966	5,778.647	−2,306.499	−1,415.810	2,109.425	19,960.330

Table 16: Synthetic Gaps: Racial Dissimilarity (D)

Statistic	N	Mean	St. Dev.	Min	Pctl(25)	Pctl(75)	Max
0	35	0.021	0.140	−0.236	−0.077	0.106	0.419
1	36	0.027	0.176	−0.390	−0.026	0.132	0.431
2	36	0.022	0.143	−0.323	−0.028	0.101	0.463
3	35	0.036	0.117	−0.227	−0.023	0.075	0.472
4	34	0.059	0.124	−0.191	−0.020	0.096	0.458
5	31	0.028	0.121	−0.321	−0.022	0.097	0.371
6	28	0.021	0.094	−0.199	−0.034	0.067	0.344
7	23	0.029	0.098	−0.198	−0.026	0.079	0.223
8	20	−0.001	0.096	−0.251	−0.037	0.059	0.136
9	20	−0.002	0.122	−0.221	−0.067	0.045	0.272
10	20	0.026	0.101	−0.192	−0.031	0.102	0.213

Table 17: Synthetic Gaps: Racial Information Theory (H)

Statistic	N	Mean	St. Dev.	Min	Pctl(25)	Pctl(75)	Max
0	31	0.034	0.126	−0.156	−0.017	0.048	0.500
1	33	0.037	0.101	−0.124	−0.005	0.060	0.377
2	34	0.027	0.091	−0.126	−0.018	0.050	0.400
3	33	0.028	0.089	−0.120	−0.018	0.048	0.403
4	32	0.046	0.090	−0.065	−0.004	0.063	0.427
5	29	0.018	0.068	−0.089	−0.010	0.045	0.182
6	26	0.020	0.054	−0.043	−0.007	0.024	0.159
7	22	0.026	0.055	−0.041	−0.009	0.046	0.163
8	19	0.014	0.070	−0.131	−0.012	0.036	0.171
9	19	0.016	0.061	−0.066	−0.019	0.017	0.181
10	19	0.025	0.060	−0.055	−0.014	0.061	0.146

Table 18: Synthetic Gaps: Racial Relative Diversity (R)

Statistic	N	Mean	St. Dev.	Min	Pctl(25)	Pctl(75)	Max
0	34	0.051	0.145	−0.072	−0.003	0.020	0.706
1	35	0.050	0.096	−0.012	−0.002	0.068	0.406
2	35	0.043	0.089	−0.018	−0.003	0.052	0.421
3	34	0.045	0.091	−0.017	−0.001	0.047	0.441
4	33	0.049	0.091	−0.018	0.0004	0.057	0.451
5	30	0.025	0.067	−0.178	−0.001	0.047	0.224
6	27	0.030	0.058	−0.014	−0.002	0.039	0.212
7	23	0.030	0.058	−0.014	−0.003	0.045	0.197
8	20	0.026	0.063	−0.047	−0.006	0.025	0.193
9	20	0.024	0.064	−0.036	−0.007	0.028	0.208
10	20	0.032	0.066	−0.028	−0.003	0.036	0.200

Table 19: Synthetic Gaps: Income Dissimilarity (D)

Statistic	N	Mean	St. Dev.	Min	Pctl(25)	Pctl(75)	Max
0	33	0.009	0.162	−0.396	−0.040	0.053	0.617
1	34	0.012	0.057	−0.130	−0.014	0.044	0.112
2	34	−0.003	0.109	−0.295	−0.049	0.037	0.315
3	33	0.016	0.116	−0.394	−0.030	0.070	0.254
4	32	0.036	0.110	−0.164	−0.031	0.099	0.274
5	28	0.028	0.116	−0.185	−0.059	0.079	0.337
6	25	0.019	0.092	−0.116	−0.063	0.077	0.232
7	22	0.005	0.095	−0.199	−0.056	0.048	0.209
8	17	0.012	0.089	−0.142	−0.055	0.030	0.227
9	19	−0.024	0.079	−0.237	−0.069	0.051	0.070
10	19	−0.003	0.121	−0.319	−0.019	0.070	0.142

Table 20: Synthetic Gaps: Income Information Theory (H)

Statistic	N	Mean	St. Dev.	Min	Pctl(25)	Pctl(75)	Max
0	33	0.019	0.115	−0.270	−0.013	0.029	0.381
1	34	0.011	0.062	−0.150	−0.010	0.017	0.253
2	34	−0.001	0.060	−0.167	−0.023	0.017	0.205
3	33	0.008	0.062	−0.154	−0.012	0.044	0.137
4	32	0.018	0.070	−0.151	−0.015	0.039	0.206
5	28	0.017	0.068	−0.117	−0.011	0.051	0.211
6	25	−0.00002	0.067	−0.167	−0.020	0.020	0.138
7	22	−0.002	0.068	−0.205	−0.020	0.033	0.118
8	17	0.005	0.078	−0.212	−0.015	0.025	0.196
9	19	−0.018	0.071	−0.218	−0.028	0.014	0.093
10	19	−0.005	0.054	−0.142	−0.013	0.019	0.082

Table 21: Synthetic Gaps: Income Relative Diversity (R)

Statistic	N	Mean	St. Dev.	Min	Pctl(25)	Pctl(75)	Max
0	32	0.018	0.093	−0.253	−0.020	0.037	0.296
1	33	0.014	0.048	−0.080	−0.011	0.022	0.151
2	34	0.012	0.067	−0.090	−0.022	0.029	0.260
3	33	0.023	0.062	−0.083	−0.008	0.056	0.189
4	32	0.031	0.073	−0.066	−0.018	0.047	0.207
5	28	0.026	0.082	−0.128	−0.010	0.046	0.278
6	25	0.023	0.066	−0.082	−0.009	0.035	0.181
7	22	0.011	0.066	−0.138	−0.013	0.030	0.155
8	17	0.017	0.076	−0.105	−0.010	0.019	0.241
9	19	0.001	0.065	−0.107	−0.033	0.022	0.141
10	19	0.019	0.049	−0.081	−0.007	0.049	0.126

Table 22: Cumulative Changes to First Order Effects in Less White Districts

Years Since Secession	Percent White	Total Revenue per Student	Local Revenue per Student
0	-0.00932	1638.25***	2876.4***
1	0.00669	1846.84***	2954.81***
2	0.00888	1951.6***	3419.51***
3	0.01004	1866.8**	3264.21***
4	0.01448	1960.58**	3452.41***
5	0.02837**	2117.18***	3158.21***
6	0.03083*	2189.41***	3156.73***
7	0.03634**	2171.02***	3020.87***
8	0.03631**	2214.73***	3150***
9	0.03615*	2254.17***	3212.82***
10	0.03567*	2283.34***	4044.45***

Note: Differing from Table 3, in districts with a baseline of more non-white students, the percentage of students that are white in the left behind district does not increase. Asterisks denote significance levels (***=1%, **=5%, *=10%).

Table 23: Cumulative Changes to Income Segregation in Less White Districts

Years Since Secession	Income Dissimilarity	Income Entropy	Income Relative Diversity
0	0.04159	0.03598**	0.05246***
1	0.03683	0.02806*	0.03422**
2	0.0359	0.02492*	0.0275**
3	0.04359	0.025*	0.033**
4	0.04873	0.02578*	0.03497**
5	0.04484	0.02081	0.02787*
6	0.05262*	0.02827*	0.04157**
7	0.03776	0.00696	0.0271
8	0.04017	0.01987	0.0185
9	0.02912	0.01114	0.01048
10	0.02932	0.01056	0.01063

Note: In the first years after a secession, there is evidence of increased income segregation in these more diverse districts.
Asterisks denote significance levels (***=1%, **=5%, *=10%).

Table 24: Cumulative Changes to First Order Effects in More White Districts

Years Since Secession	Percent White	Total Revenue per Student	Local Revenue per Student
0	0.00849	378.75	377.34
1	0.00722	336.79	426
2	0.00436	227.95	380.61
3	0.00558	246.8	366.61
4	0.0045	201.52	455.01
5	0.00832	130.11	608.92**
6	0.00748	172.55	694.07**
7	0.00413	191.28	698.52**
8	0.00438	231.37	674.36**
9	0.00494	286.56	754.09**
10	0.0053	256.77	629.35*

Note: In the more white districts, we can see that there exists statistically more white students in the left behind districts as a result of a secession. This is in addition to increases in local revenue on a per student basis.
Asterisks denote significance levels (***=1%, **=5%, *=10%).

Table 25: Cumulative Changes to Income Segregation in More White Districts

Years Since Secession	Income Dissimilarity	Income Entropy	Income Relative Diversity
0	0.01749	0.05365***	0.01637*
1	0.0151	0.02993***	0.01085*
2	0.00746	0.02104**	0.00726
3	0.00231	0.01909**	0.00603
4	0.00237	0.0161**	0.00557
5	0.00631	0.01578**	0.00579
6	0.00382	0.01283*	0.00419
7	0.00023	0.00961	0.0029
8	0.00148	0.01219	0.0051
9	-0.00029	0.01158	0.00459
10	-0.00389	0.00874	0.0034

Note: Income entropy, the metric of income that is most of interest in this paper rises in the years following a secession in the more white districts.
Asterisks denote significance levels (***=1%, **=5%, *=10%).

Table 26: Cumulative Changes to Racial Segregation in More White Districts

Years Since Secession	Racial Dissimilarity	Racial Entropy	Racial Relative Diversity
0	-0.00849	-0.00452**	-0.00026
1	-0.00267	-0.00143	0.00132
2	-0.00478	-0.00274	0.00118
3	-0.00625	-0.0043	0.00106
4	-0.00621	-0.00367	0.00103
5	-0.01019	-0.00663**	0.00064
6	-0.00673	-0.00554	0.00048
7	-0.00981	-0.00651	-4e-04
8	-0.01803	-0.00777*	-0.00057
9	-0.01782	-0.00753	-0.00037
10	-0.01712	-0.00739	-3e-04

Note: These more white districts experience significant decreases in racial segregation as a result of secession, providing further indication that secession in this type of district differs substantially from less majority white districts.
Asterisks denote significance levels (***=1%, **=5%, *=10%).

Table 27: Cumulative Changes to First Order Effects in Districts with Failed Secession Attempts

Years Since Secession	Percent White	Total Revenue per Student	Local Revenue per Student
0	-0.00831	379.93*	173.48
1	-0.00614	225.04	-110.78
2	-0.01044	261.2	36.28
3	-0.01314	247.14	133.03
4	-0.00822	329.64	187.18
5	-0.00371	340.98	225.13
6	-0.00366	346.18	255.1
7	-0.00319	349.06	286.94
8	-0.0035	566.77	730.98*
9	-0.00389	564.25	751.46
10	-0.01498	526.22	770.81

Note: All estimates of first order effects are not statistically significant at the 5% level. These are effects that are expected when a district leaves, so this null result is unsurprising.
Asterisks denote significance levels (***=1%, **=5%, *=10%).

Table 28: Cumulative Changes to Income Segregation in Districts with Failed Secession Attempts

Years Since Secession	Income Dissimilarity	Income Entropy	Income Relative Diversity
0	0.0105	-0.01349***	-0.00343**
1	0.01928	0.00083	0.00837
2	0.01555	-5e-04	0.00169
3	0.02084	0.00094	0.00214
4	0.0209	3e-05	0.00168
5	0.02153	0.00419	0.00365
6	0.02049	0.00636	0.00193
7	0.00956	-0.00022	0.00086
8	0.00915	-0.00082	8e-04
9	0.00972	-0.00072	0.00094
10	0.00931	-0.00101	0.00081

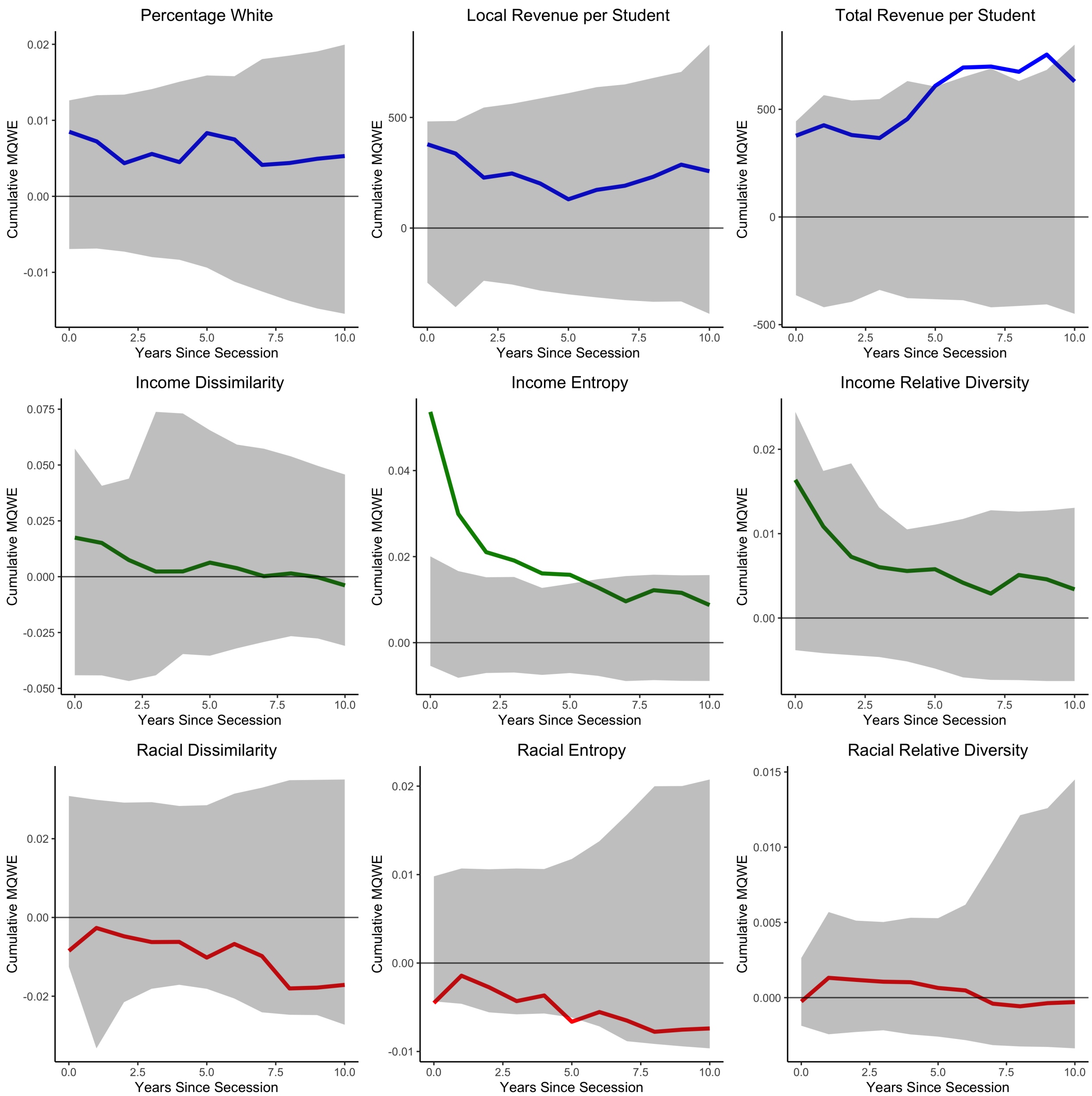
Note: There is a statistically significant decrease in income segregation in the year a failed secession attempt occurred, and no effect in any other time period.
Asterisks denote significance levels (***=1%, **=5%, *=10%).

Table 29: Cumulative Changes to Racial Segregation in Districts with Failed Secession Attempts

Years Since Secession	Racial Dissimilarity	Racial Entropy	Racial Relative Diversity
0	0.00962	0.00821	0.00328
1	0.013	0.00922	0.00458
2	0.01321	0.00969	0.0047
3	0.01401	0.01123	0.00563
4	0.01389	0.00924	0.00378
5	0.01684	0.00933	0.00353
6	0.01551	0.00877	0.00315
7	0.01398	0.0092	0.00427
8	0.01454	0.00974	0.00488
9	0.01484	0.00988	0.00536
10	0.01551	0.01008	0.00579

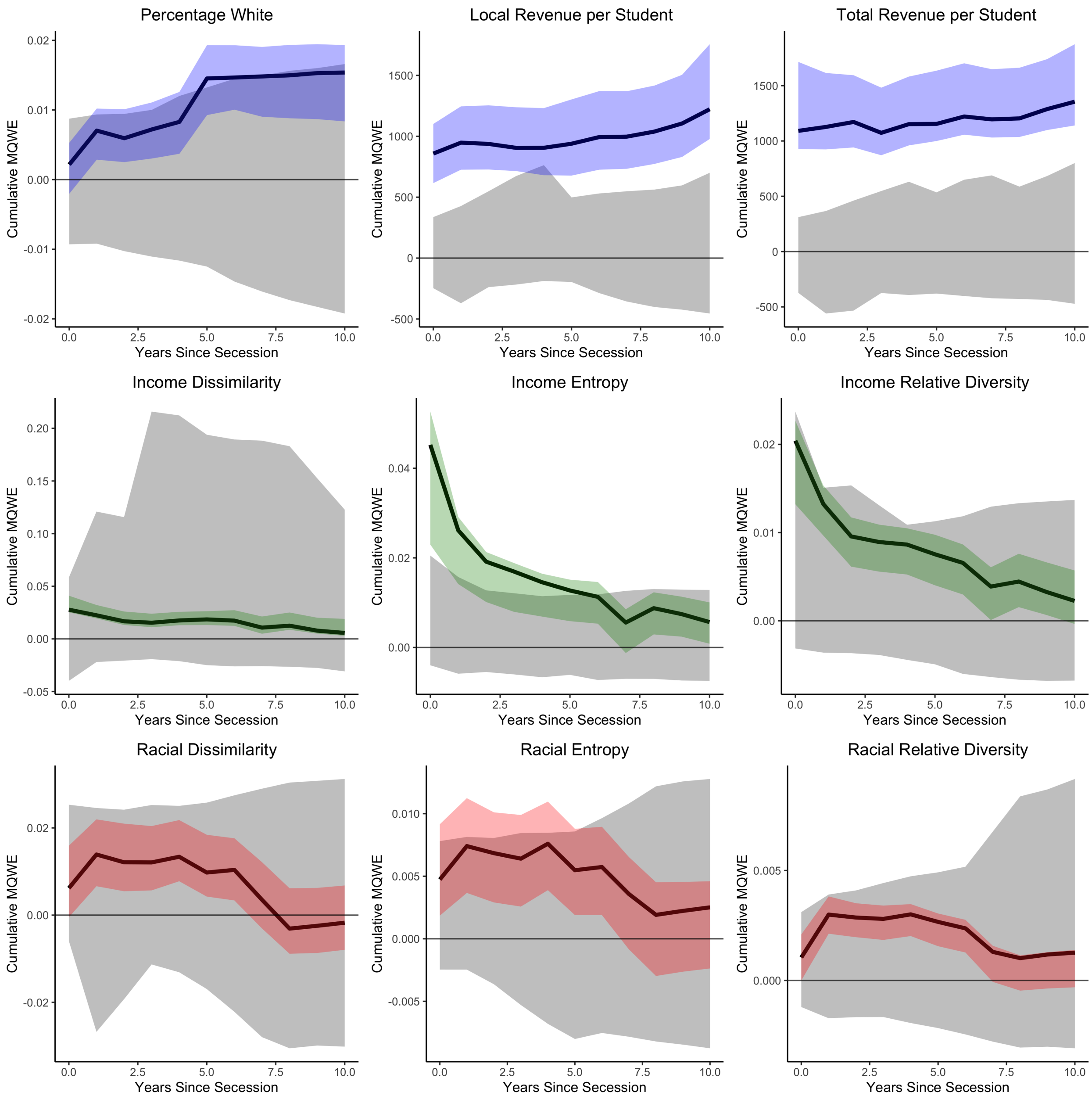
Note: I find no statistically significant effect on racial segregation from attempted secessions. Asterisks denote significance levels (***=1%, **=5%, *=10%).

Figure 7: The Effect of Secession on More White Regions



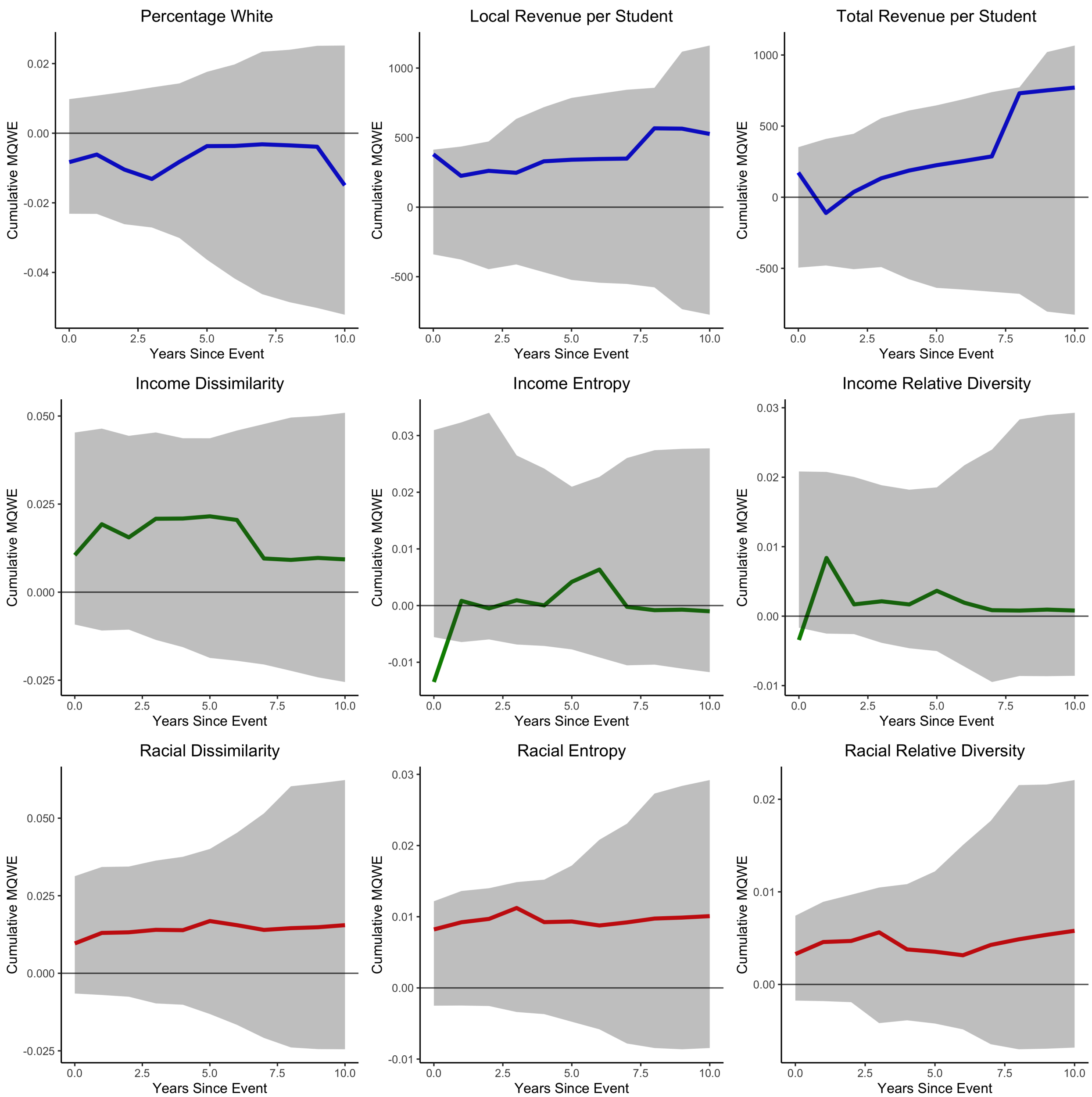
Note: In districts with higher proportions of white students, the effects of secession look quite different than the effects on more diverse districts (effects that can be seen in Figure 6). The income entropy increases substantially. Notably, the racial entropy calculated over the pre secession district boundaries decreases as a result of secession in a few time specifications.

Figure 8: Sensitivity Checks on Control Group Formation



Note: As in Figure 5, the grey area represents the 95% bootstrapped confidence intervals for the placebo effects and the black line is the MQWE of the effects of secession. The colored ribbons are 95% confidence interval this MQWE estimate s taken from the distribution generated by bootstrapping samples from each district's estimates when 5% of their control pool districts are randomly removed. Clearly, the specification of the control group can have substantial effect upon the estimate of the effect. Regardless of the specification, the effect on revenue remains highly significant for all estimates within this MQWE confidence interval. The same is true for income entropy at the earlier time periods, although as time passes some of the alternate formulations have less significant estimates of the effect.

Figure 9: Match Quality Weighted Estimates with Confidence Intervals for Failed Secessions



Note: The confidence intervals on the failed secession differ from those in Figure 5, because I generate a separate set of 5,000 bootstrapped estimates of the placebo effect using the districts that form the failed secessions' control pools with the same underlying distribution as shown for failed secessions in Figure 1. The effect of a failed secession is insignificant in all specifications other than income entropy in the year immediately after the event.