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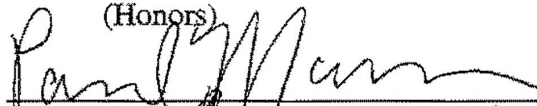
A thesis submitted in partial fulfillment of the requirement  
for the degree of Bachelor of Arts in Public Policy from  
The College of William and Mary

by

Kathleen Bryant

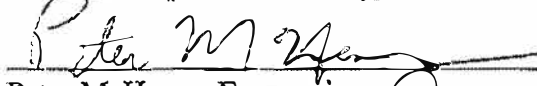
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# **Income Segregation Across Schools and The Shapes of School Attendance Zones**

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*Abstract:* Given the direct influence of socioeconomic diversity in schools on student achievement, it is important to try to understand the causes of rising income segregation across schools. My paper assesses whether school attendance zone gerrymandering contributes to income segregation across public schools within 129 of the largest school districts in the United States. I compare income segregation levels between actual school attendance zones and hypothetical school attendance zones that would exist in the absence of gerrymandering to determine if current zone shapes contribute to segregation. I also test for correlations between income diversity within school attendance zones and the shapes of attendance zones as quantified by spatial compactness measures commonly found in the political gerrymandering literature. I find that on average, irregularly-shaped, gerrymandered school attendance zones seem to better integrate, rather than further segregate students of different socioeconomic backgrounds.

*Acknowledgements:*

I am very grateful to Professor Paul Manna, my thesis advisor, for encouraging me to pursue this project as an honors thesis and for providing such consistent and helpful guidance over the past 8 months. I would not have been able to complete this project without his dedicated mentorship. I would also like to thank my fellow classmates in Professor Manna's K-12 Education Policy senior seminar, who provided friendly camaraderie and valuable input during the formative stages of this project. Additionally, many thanks to Professor Salvatore Saporito, Professor Peter McHenry, Professor Robert Rose, Professor Dan Maliniak, and the helpful staff of William & Mary's Center for Geospatial Analysis (CGA) for your technical assistance and support of my research over the past year. Lastly, I am grateful to the Society for Political Methodology for recognizing me as an Advanced Empirical Research on Politics for Undergraduates (AERoPUP) Fellow for my work on this project.

## *Introduction*

Starting with the Coleman Report of 1966, research has consistently shown that concentrated poverty in schools has a negative effect on student performance. The Coleman report famously found that “socioeconomic factors bear a strong relation to academic achievement” and that “a pupil's achievement is strongly related to the educational backgrounds and aspirations of the other students in the school” (Coleman 1966, 21-22). An analysis of fourth grade National Assessment of Educational Progress (NAEP) math test scores found that low-income students in middle-class schools outperformed middle-class students in high-poverty schools (Kahlenberg 2002, 4). Children living in Montgomery County, Maryland public housing projects that were randomly assigned to low-poverty schools had positive, statistically significant improvements in test scores compared to a control group of students in public housing who were assigned to high-poverty schools (Schwartz 2010). These studies and others like them demonstrate that an important component of improving academic performance among low-income students is to provide them with access to high quality, low-poverty, socioeconomically integrated schools.

Existing research has shown that concentrated poverty in schools has such a dramatic effect on student achievement because of factors like lower teacher quality, lower rates of parental involvement, and peer effects. In high-poverty schools, 21.5 percent of core classes are taught by a teacher with neither a certification nor a college major in the subject area taught, while only 10.9 percent of low-poverty core classes face this problem (Almy and Theokas 2010). High-poverty schools have consistently higher variation in teacher effectiveness than low-poverty schools, and this is because “the least effective teachers in high-poverty schools are much less effective than their counterparts in lower-poverty schools” (Sass et. al, 2010, 19). In terms of parental involvement, parents who take a more active role in their child's education tend to have

children with better reading and mathematics achievement as well as higher motivation to succeed in school (McWayne et. al, 371). However, socioeconomic status is the “primary predictor” of parent involvement in schools, and parent involvement in high-poverty schools is generally “abysmally low.” (Kahlenberg 2003, 62). Lastly, since high-poverty schools tend to have a higher share of low-performing students, the salience of peer effects shows that the academic performance of individual students is directly influenced by the performance of their peers (Borg, Borg and Stranahan 2012, 2; Gottfried 2014) The mean ability of a class in terms of reading and math performance in previous years is positively and statistically significantly related to student performance on individual tests, which suggests that students can be influenced by the aptitude and motivation levels of their peers (Gottfried 2014). If this is true, students in high-poverty, low-achieving classrooms are more likely to be low-performing individuals themselves in part because of the negative consequences of peer effects.

Despite the well-documented negative consequences of concentrated poverty in schools, income segregation across schools has increased in recent years. From 1990-2010, income segregation across school districts increased by over 15 percent, and income segregation across schools within school districts also increased by over 40 percent (Owens and Jencks 2016, 1159). Accordingly, the share of public school students attending high-poverty schools has increased from 12 percent in 1999 to 19 percent in 2011 (Snyder and Dillow 2013, 190). Because socioeconomic diversity in schools is so tied to student performance, it is important to try to understand the causes of school income segregation. This thesis does so by examining the following research question: To what extent does gerrymandering of school attendance zones contribute to income segregation within school districts? It is particularly important to evaluate the causes of segregation within school districts rather than between school districts following

the 1974 Supreme Court case *Milliken v. Bradley*. In *Milliken v. Bradley*, the Court ruled that federal courts do not have the authority to impose multi-district school desegregation plans that would create less segregated schools in compliance with *Brown v. Board of Education* (Hertz 2014). The Court's emphasis on local control of education in the *Milliken* decision has essentially made it impossible to implement desegregation plans across school districts, and so redrawing attendance zones within individual school districts is one of the last tools policymakers can legally use to create more socioeconomically and racially integrated schools.

### *Theoretical Framework and General Hypothesis*

The term “gerrymandering” originated in the early 19<sup>th</sup> century, when the Governor of Massachusetts, Elbridge Gerry, redrew such distorted, misshapen boundaries for a state senate district that the district's new shape was compared to a “salamander” and the district itself was referred to as a “Gerry-mander” (Trickey 2017). While this state senate district from 1812 was not the first politically manipulated district in U.S. history, it was the first district to coin the term “gerrymandering,” wherein legislative districts are irregularly drawn with the intention of giving a particular politician or political party an advantage during elections. Some scholars argue that just as legislatures can gerrymander the borders of legislative districts during the redistricting process, school districts have similar opportunities to gerrymander the attendance zones of public schools in order to adjust the racial and socioeconomic compositions of schools.

There are two competing theories about whether school attendance zones are gerrymandered to increase school segregation. Richards' (2014) theory of “student exchange,” which rests on the concept of NIMBYism, argues that school attendance zone gerrymandering increases segregation in schools. However, Saporito's (2017) application of Tobler's First Law as applied to segregation across schools suggests that compact school attendance zones should be more

segregated than irregularly shaped zones, and that gerrymandering should therefore reduce segregation in schools. Saporito's and Richards' theoretical and empirical disagreements hold constant across multiple papers (Richards 2014; Richards and Stroub 2015; Saporito and Van Riper 2016; Saporito 2017). The following paragraphs review this debate.

In her paper on racial segregation across school attendance zones, Richards (2014, 1125) cites the concept of “voter exchange through electoral gerrymandering,” where the boundaries of legislative districts are manipulated to create a population of voters that is more demographically advantageous to politicians by excluding some nearby voters and replacing them with voters that live further away. This means that politicians tasked with drawing electoral boundaries are effectively choosing their own voters when they gerrymander legislative districts. Richards (2014, 1125) then applies this concept of “voter exchange” to school attendance zones and generates a theory of “student exchange,” where school districts draw attendance zones to “zone in” certain students to certain schools and “zone out” others in a way that segregates ethnic/racial minority students. Therefore, Richards' (2014) theory of student exchange asserts that many school attendance zones are gerrymandered to segregate students by race within school districts, and she argues that her empirical research supports this theory. It thus follows that when applied to income segregation, this theory would predict that school attendance zones are gerrymandered to zone in wealthy students to low-poverty schools and zone out poor students from those same schools.

“Student exchange” theory is consistent with the theory of NIMBYism. NIMBY stands for “Not In My Backyard,” and it refers, in the context of housing development, to the opposition that new construction of multifamily homes (which generally serve low-income people) faces in affluent areas. Some of the main arguments used by NIMBY-proponents are that low-income

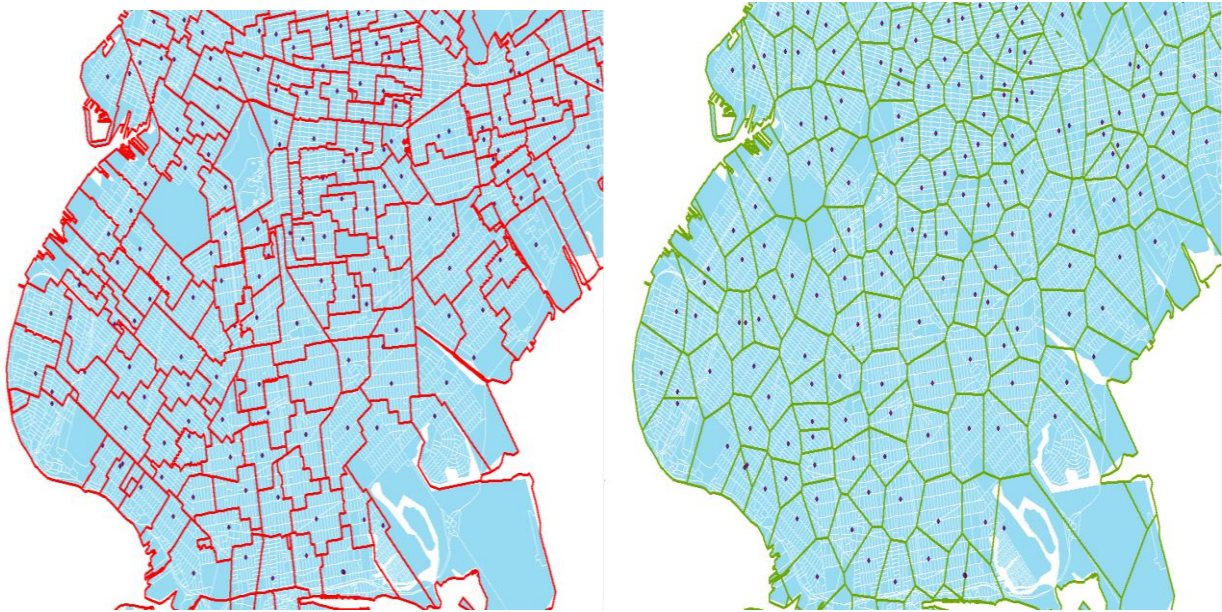


apartments will inevitably put a financial burden on school districts and drive down property values by bringing crime into their privileged communities (Obrinsky and Stein 2007, 4). Since the theory of NIMBYism suggests that people do not want low-income apartments in their housing markets because of their stereotypes about low-income people, it follows that these same people could want to prevent low-income students from attending high-achieving, low-poverty schools. Parents who choose to pay high property taxes in order to live in neighborhoods with excellent schools may view access to those schools as a zero-sum game and will thus be more likely to oppose bold socioeconomic integration efforts like student transfer initiatives or new enrollment policies (Rotherham 2010).

To test her theory, Richards (2014) constructs hypothetical attendance zones that would exist in the absence of gerrymandering, and then she compares the racial segregation levels in school districts produced by the hypothetical zones and actual school attendance zones. Richards (2014) constructs these hypothetical school attendance zones by using Thiessen polygons. A Thiessen polygon is a polygon built around one point such that the area enclosed in the polygon's boundaries is closest to that point relative to all other points. This would mean that hypothetical attendance zones would be drawn such that the entire area enclosed in one school's zone is closest to that school rather than any other school. Richards believes these Thiessen polygon attendance zones show what school attendance zones would look like in the absence of gerrymandering, because they are completely convex and optimally efficient (Richards 2014, 1128). To illustrate what Thiessen polygon attendance zones would look like, Figure 1 compares the actual attendance zones of New York City Public Schools to the hypothetical attendance zones for that district drawn using the Thiessen polygon method. The red borders represent the actual zones while the green borders represent the hypothetical zones. Richards conceptualizes

the Thiessen zones as perfectly non-gerrymandered because they lack any odd indentations or irregularities aside from those used to conform to the borders of the school district at large.

**Figure 1.** Actual (red) and hypothetical (green) attendance zones for New York City Public Schools.



Richards finds that “racial segregation among actual attendance zones is significantly higher than the segregation among Thiessen attendance zones,” with actual attendance zones segregating black and white students 0.002 percentage points more than the hypothetical zones (Richards 2014, 1141). She believes this demonstrates that actual attendance zones deviate from hypothetical, non-gerrymandered zones to segregate students by race within school districts. She also finds that while the differences in segregation levels among actual and hypothetical attendance zones were higher in areas experiencing rapid racial change, there was no relationship between segregation level differences and rapid socioeconomic change (Richards 2014, 1148). This would suggest on the surface that her theory of segregative attendance zone gerrymandering would only apply to racial, rather than income segregation. However, Richards (2014, 1148) admits that the measure of poverty she used, free-and-reduced-price lunch status, “is generally

considered a poor measure of socioeconomic status,” so this suggests that more research on income segregation is needed using higher-quality measures of income.

Several specific cases support Richards’ (2014) theory that school attendance zone gerrymandering increases segregation in schools. In her own paper, she shows a map visualizing one of the school attendance zones that she analyzed, and she uses the map to demonstrate that Hispanic and black census blocks were zoned out of attending a particular school with razor sharp precision (Richards 2014, 1139). There are also local news articles that support Richards’ theory that gerrymandered attendance zone shapes contribute to school segregation. For instance, reporting from Dallas about an attendance zone for the Mata School argues that the zone is irregularly shaped to zone in black and Latino families and zone out white families, which shows that gerrymandering resulted in segregated schools in that specific case (Robberson 2012; Saporito and Van Riper 2016). Additionally, Siegel-Hawley’s (2013, 580) account of Henrico County Public School’s rezoning process shows that the district’s new attendance zones “solidified extreme patterns of racial isolation within high school attendance areas” (Siegel-Hawley 2013, 605). Under the new zones, 17 percent of the County’s black high school aged students were zoned into a highly segregated school, compared to 0 percent of black students under the previous zones (Siegel-Hawley 2013, 605). All these examples demonstrate that there is already some documented evidence supporting Richards’ theory of “student exchange” and subsequent segregative attendance zone gerrymandering.

Meanwhile, in his paper on income segregation across school attendance zones, Saporito (2017, 5) cites Tobler’s First Law of geography, which states “nearby things are more similar than distant things.” When applied to the distribution of income groups within neighborhoods, Tobler’s Law suggests that “the income of a householder is more similar to his or her closest

neighbors than the income of his or her neighbors down the street,” and that “the income composition of two adjacent city blocks is likely more similar than the income composition of two city blocks located farther apart” (Saporito 2017, 5). Consequently, Saporito theorizes that because people who reside near each other are more likely to share socioeconomic characteristics, compact zones will be more segregated than gerrymandered zones by reinforcing existing patterns of neighborhood income segregation. By “breaking up” socioeconomically homogenous neighborhoods into different school attendance zones, drawing irregularly shaped zones through zone gerrymandering could therefore serve as a positive tool for economic integration.

The dramatic increase of residential segregation by income in recent decades supports Saporito’s theory. In 1980, 23 percent of low-income households lived in majority low-income census tracts, but by 2010, that share had climbed to 28 percent (Fry and Taylor 2012). Even more dramatically, only 9 percent of upper-income households lived in majority upper-income census tracts in 1980, but that share increased to 18 percent by 2010 (Fry and Taylor 2012). This means that by 2012, over one-third (34 percent) of all families lived in neighborhoods with median incomes significantly above or significantly below the median income of their metropolitan area (Reardon and Bischoff 2016). Overall family income segregation across neighborhoods within major metropolitan areas increased by approximately 27 percent between 1970 and 2012 (Reardon and Bischoff 2016). If people increasingly cluster into specific neighborhoods based on socioeconomic similarity, then Saporito’s theory that compact, “less gerrymandered” school attendance zones will only serve to reinforce residential inequalities clearly makes logical sense. Saporito’s theory is backed up to a degree even by Richards (2014,

1125), who concedes in her analysis that in some cases, gerrymandering has the potential to be “affirmative, zoning out more [...] similar students in favor of more dissimilar students.”

Saporito’s (2017) methodology differs from Richards’ methodology in several key ways. First, while the main statistical tools Richards (2014) uses are ttests to test for statistically significant segregation differences between actual and hypothetical attendance zones, Saporito (2017) constructs several regression models to test his theory. He regresses a measure of income segregation across school attendance zones on two measures of residential income segregation and two measures of school attendance zone shape. Using a regression model allows Saporito (2017) to explicitly control for other factors in his analysis, most importantly residential segregation, to parse out the individual effect of attendance zone shape / gerrymandering on income segregation across schools. Another difference between their methodologies is that while Saporito (2017) also uses Thiessen polygons in his analysis, Saporito (2017) does not agree with Richards that Thiessen polygons represent hypothetical, perfectly compact attendance zones that would exist in the absence of segregation. This is because Thiessen polygons do not necessarily produce attendance zones that are equally or even similarly populated, so they cannot in all cases be ideal or even realistic school attendance zones for districts to adopt. Saporito (2017) instead conceptualizes Thiessen polygons as neighborhood units within school districts, and he argues that district-level segregation measures calculated using Thiessen polygons are representative of residential segregation levels within school districts. Therefore, Saporito (2017) uses Thiessen polygons to calculate one of his two measures of residential segregation that he incorporates into his regression model as independent variables.

Yet another way that Saporito’s (2017) work differs from Richards’ (2014) work is that he quantifies the shapes of school attendance zones using spatial compactness measures found in

the political gerrymandering literature. In the gerrymandering literature, the mathematical definition of compactness is the extent to which a legislative district's shape "deviates from perfect circularity," but in layman's terms, compactness refers to the extent to which shapes appear "irregular," "bizarre," or "ugly" (Richards and Stroub 2015, 7). Saporito's (2017) two measures of compactness used in this paper – a concavity (CV) index and Convex Hull (CH) index - range from 0 to 1, with scores closer to 0 indicating attendance zone shapes that are perfectly compact and scores closer to 1 indicating zone shapes that are highly irregular (Saporito 2017, 1354). These indices are the measures of attendance zone shape that are incorporated into Saporito's (2017) regression models; Saporito (2017) uses the mean CV and CH scores of all the attendance zones in a school district to capture the effect of attendance zone shape on school income segregation across schools within school districts.

Saporito's (2017) regression results demonstrate that residential segregation "almost completely accounts for income segregation across schools," and that the regression coefficients associated with both measures of school attendance zone shape are negative. This demonstrates that "irregularly shaped attendance zones are associated with lower levels of income segregation across attendance zones" within school districts (Saporito 2017, 1359). He also finds that the school districts with lower levels of school income segregation than expected given their higher measures of residential segregation oftentimes have attendance zones that are highly irregular in shape (Saporito 2017, 1364). Saporito rules out that shape irregularities in those school districts are accidental or coincidental, because he shows that none of those bizarrely shaped zones result from proximity to topographic features like rivers, lakes, and mountains (Saporito 2017, 1365). All this evidence contributes to Saporito's (2017) argument that school attendance zone

gerrymandering, as defined by zone shape irregularity, is associated more with school integration rather than school segregation.

Other authors have engaged this debate over school attendance zone gerrymandering as well. Monarrez's (2018) recent work on racial segregation and school attendance zones is consistent with Saporito's (2017) theory and methodology. Monarrez (2018, 12) states in his paper that "districts can achieve more racial integration in schools by increasing the amount of distance students travel to school," and that "if residences are racially segregated, a 'neighborhood schools' assignment system will result in a racially segregated school system." He defines desegregation policy as officials working to create school attendance zones which are more integrated than neighborhood-based schools and he also uses Theissen polygons to show what attendance zones would look like under a strict neighborhood schools approach (Monarrez 2018, 16). In his paper, Monarrez (2018) echoes Saporito's (2017) conceptualization of school attendance zone gerrymandering, wherein irregular boundaries that deviate from perfectly compact Theissen polygon zones are more likely to integrate rather than segregate schools.

Some might argue that Richards and Saporito's diverging theories are the result of writing about different forms of segregation, for Saporito's (2017) paper focuses on income segregation while Richards' (2014) paper focuses on racial segregation. However, even when Saporito writes another paper focusing on racial segregation, his theoretical framework and empirical results still contradict those of Richards (Saporito and Van Riper 2016). In this paper, Saporito and Van Riper (2016) also use compactness measures common in the political gerrymandering literature to quantify the extent to which attendance zones are irregularly shaped. They find "modest, positive correlations between attendance zone irregularity and absolute racial diversity" and that almost all the most bizarrely shaped zones are also the most racially diverse. Additionally, they

find positive correlations between attendance zone irregularity and spatially distinct clustering of different racial groups. This evidence that irregularly shaped, gerrymandered attendance zones are more likely to produce racially integrated schools shows that Saporito's theories hold even when applied to the issue of racial segregation.

In a follow-up paper from Richards on racial segregation, where she relies on compactness measures similar to those used by Saporito, Richards argues that her empirical findings support her theory rather than Saporito's (Richards and Stroub 2015). Richards and Stroub (2015, 20) find that shape irregularity as quantified by compactness measures is "positively related to the proportion of whites in a school" and "negatively related to the proportion of students qualifying for free-and-reduced-price lunch." They also find that school districts facing higher rates of racial/ethnic demographic change are more likely to have higher levels of attendance zone gerrymandering across schools in the district (Richards and Stroub 2015, 21). Interestingly, they find that more segregated school districts generally have less gerrymandered attendance zones. They concede that this evidence could be used to support the argument that irregularly shaped attendance zones can be used affirmatively to create more integrated districts. However, they argue that when coupled with her findings on demographic change, it is more likely that gerrymandering is primarily employed as a tool to subvert higher levels of residential integration" (Richards and Stroub 2015, 22). By this, they mean districts that are already residentially segregated do not need to use gerrymandering to segregate students by race, and it is only in districts with more residential integration that gerrymandering is necessary to racially segregate schools. They argue that this would explain why districts with more gerrymandered boundaries tend to have lower levels of racial segregation across schools.



Both Richards and Saporito present persuasive evidence to support their theories and have well-documented methodologies and results, but Saporito's theory and findings are ultimately more persuasive. Richards' sweeping conclusions that "first grade attendance zone boundaries generally serve to segregate students by race and ethnicity" and that attendance zone gerrymandering is "primarily a means of excluding non-white and poor students from whiter and more affluent schools" are not satisfactorily borne out by the evidence she presents (Richards 2014, 1119; Richards and Stroub 2015, 1). The segregation differences that Richards (2014) finds between actual and hypothetical attendance zones are statistically significant, but it is hard to believe that 0.001-0.006 percentage point differences are substantively significant. Furthermore, Richards and Stroub's (2015, 22) finding that school districts with more gerrymandered attendance zones tend to be "substantially" less segregated clearly seems to support Saporito's argument that irregularly shaped zones reduce segregation, and Richards and Stroub even admit that a possible interpretation of their results involves integrative, "affirmative" gerrymandering. The disconnect between Richards' far-reaching conclusions and her actual evidence appears problematic. Therefore, my general hypothesis is that irregularly shaped, gerrymandered attendance zones are more likely to reduce rather than increase segregation, in accordance with Saporito's theory and empirical findings.

### *Research Methods & Data: Part I*

My research methodology consists of two parts. In the first part of my analysis, I apply the methods from Richards' (2014) work on racial segregation to the topic of income segregation. I construct Thiessen polygons around the schools in my sample to demonstrate what hypothetical attendance zones would look like in the absence of gerrymandering. I then calculate income segregation levels for each school district produced by the actual attendance zones and

the Thiessen attendance zones. Finally, I run a dependent ttest to test for statistically significant differences in the segregation levels generated by actual and hypothetical attendance zones for each school district. If actual school attendance zones produce statistically significantly higher levels of segregation on average than hypothetical attendance zones, that would suggest that Richards's theory is right and that actual attendance zones are gerrymandered to segregate students by income. If hypothetical school attendance zones have statistically significantly higher levels of segregation on average than actual attendance zones, that would suggest that Saporito's theory is right and that compact, "non-gerrymandered" zones contribute more to segregation by reinforcing residential segregation patterns. An important caveat is that quantitative analyses cannot parse out discriminatory intent, but my findings can still demonstrate whether attendance zone gerrymandering is consistent with what either Saporito's or Richards' theory would predict.

I measure income segregation across schools by using a dissimilarity index calculation that is modified to be an ordinal segregation measure. A simple dissimilarity index measures the extent to which the proportions of two different groups within sub-areal units match the proportions of two different groups present in a larger areal unit (Whitehurst, Reeves, and Rodrigue 2016). For example, in the context of school income segregation, a simple dissimilarity index could show the extent to which the proportions of poor people in each attendance zone mirror the proportion of poor people living in the entire school district. This measure ranges from 0 to 1, and the value of the simple dissimilarity index represents the proportion of people that would need to move into different attendance zones to make the income demographics of the zones match the income demographics of the district at large (Ibid). For example, a school district's dissimilarity index score of 0.4 would mean that 40 percent of the district's population would have to change attendance zones so that the proportions of poor populations in the zones

would match the proportion of the entire district. The dissimilarity index is therefore a measure of how evenly the population of interest is distributed across the sub-areas within a larger area.

For my analysis, it is necessary to adjust the simple dissimilarity index because my income variable, household income in the past 12 months, consists of 16 income brackets rather than just 2 categories. Additionally, household income is an ordinal variable because there is an inherent order to the ranges of each income bracket, and the simple dissimilarity index is calculated assuming that the two groups involved are unordered. To create a more appropriate segregation measure for my chosen income variable, I modify the simple dissimilarity index calculation using Reardon and Firebaugh's (2008) ordinal segregation approach. For each school attendance zone and each school district, I calculate the proportion of all households earning at or below the lowest income bracket (Less than \$10,000), the number of households earning at or below the lowest and second-lowest income bracket (Less than \$10,000 and \$10,000-\$14,999), and so on iteratively with the last proportion being the proportion of all households earning at or below the lowest 15 income brackets. These proportions generate a total of 15 different income groups. Next, using the calculations outlined in Figure 2, I calculate dissimilarity between the first income group and all income groups above it, the second income group and all income groups above it, and so on until 15 dissimilarity scores are produced for each school district. The final dissimilarity index, and therefore the final segregation score, for each school district is the average of these 15 dissimilarity scores. Each school district has two dissimilarity indices and therefore two segregation scores calculated in this manner – one from the distribution of the 16 income groups across actual attendance zones within school districts, and the second from the distribution of the 16 income groups across the hypothetical attendance zones (based on Thiessen polygons) within school districts. The actual value of the modified dissimilarity index does not

have a clear interpretation like the simple dissimilarity index does, but lower values of the modified dissimilarity index indicate lower levels of segregation while higher values of the index indicate higher levels of segregation.

**Figure 2.** Dissimilarity Index Calculation.

$$D_j = \frac{\sum_{i=1}^n [t_i |p_i - P|]}{2TP(1 - P)}$$

$D_j$  = Dissimilarity calculation for each school district, j  
 $t_i$  = The total population of each attendance zone, i  
 $T$  = The total population of each school district, j  
 $p_i$  = The ratio of an income group's population in each attendance zone to the total population of each attendance zone  
 $P$  = The ratio of an income group's population in each school district to the total population of each school district

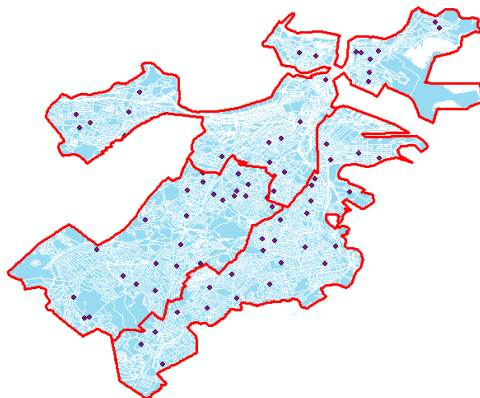
Once segregation levels are calculated for each school district based on both the actual and hypothetical attendance zones, I use a dependent ttest to determine if the difference in mean segregation levels produced by actual and hypothetical zones is statistically significantly different from zero. A dependent ttest rather than an independent ttest is used because the same sample of observation units, in this case the 129 school districts chosen for analysis, are used to generate both the segregation levels produced by the actual attendance zones and segregation levels produced by the hypothetical attendance zones (Laerd Statistics 2013). Additionally, I test whether the assumptions underlying the dependent ttest are met. I test if the differences between the actual and hypothetical zones' segregation levels are normally distributed using a qnorm plot and Skewness/Kurtosis tests for normality (Ibid). I also test for any outliers among the differences between actual and hypothetical zones' segregation levels using a box plot.

My analysis is focused solely on school attendance boundaries that apply to first graders. Broadly, I choose elementary school attendance boundaries because middle school and high school attendance boundaries are oftentimes so large that their borders are very similar to the borders of the entire school district, and so for those schools there would not necessarily be sufficient numbers of attendance zones within every district to conduct the analysis (Saporito 2016). I choose first grade specifically because “districts have different attendance zones for different grade levels,” and so it is necessary to choose one grade for consistency (Richards 2014). Additionally, both Saporito (2016, 2017) and Richards (2014) use first grade attendance zones in their analysis, and I want my work to be as consistent with theirs as possible for comparison purposes.

In terms of my process for choosing the 129 school districts and the 8,462 total attendance zones within these districts that I used in my analysis, I first identify the 151 largest school districts in the United States by enrollment. It is necessary to choose large, heavily populated school districts to make the Thiessen polygon method work properly. For this method, I need census blocks containing the income data to be as small as possible in order to avoid splitting census blocks across actual and hypothetical borders wherever possible and in order to produce substantively different results between the actual and hypothetical zones. Census blocks are smallest in densely-populated urban areas, and so it is necessary to rely on the large, heavily-populated school districts located in such areas for my analysis. 17 of the 151 largest school districts do not report their school attendance boundaries, so these districts must be excluded from the analysis (Phan 2015). Two of the 151 largest school districts, St. Lucie Public Schools in Florida and Garland Independent School District in Texas, have either district-wide school choice policies or open enrollment policies that render attendance zones irrelevant, so they are

excluded from analysis as well (Phan 2015; Garland Independent School District 2017). One of the 151 largest school districts, Sweetwater Union District in California, consists exclusively of high schools, and so this district also must be excluded due to my focus on first grade attendance zones (Phan 2015). Lastly, Boston Public Schools in Massachusetts and Lee County Public Schools in Florida must be excluded because they do not have one school zone drawn for each school; instead, children are zoned for multiple schools and parents must choose which school they prefer for their children. As an example, Boston's school district is pictured in Figure 3. Each purple point represents an elementary school and the red lines represent attendance zones. Note that there are multiple elementary schools housed within each school attendance zone. Because each school does not have its own attendance zone, comparing the Thiessen attendance zones for those schools to their actual attendance zones would be an apples-to-oranges comparison. Therefore, these districts should not be included with the other school districts in my analysis.

**Figure 3.** Boston City Public Schools



After these 23 school district exclusions, 129 school districts of the initial 151 remain for my analysis. A full list of these 129 school districts can be found in Appendix 1. Within these 129 school districts, I exclude all schools that serve exclusively pre-kindergartners and kindergartners as well as schools that serve students in second grade and above, to keep my analysis focused on first graders. I exclude all schools that are open-enrollment because schools that anyone in the district can attend do not have attendance zones. I also eliminate 556 magnet schools and charter schools because students generally either apply to those schools or receive admission through a lottery system. These eliminations initially result in a total of 8,483 total school attendance zones, but for some unknown reason, the Thiessen polygon tool in ArcGIS only generated hypothetical attendance zones around 8,462 of these schools. Considering those 21 missing schools constitute merely 0.2 percent of the sample, I feel confident that my results generated using the final total of 8,462 schools are still reliable.

I rely on several data sources for my analysis. For the attendance zones, I use the 2013-2014 school attendance boundary shapefile from the National Center for Education Statistics (NCES) at the primary school level (Phan 2015). I also retrieve x-y coordinates for the schools from the year 2013-2014 from the NCES and transform these coordinates into points in a shapefile using Excel and ArcGIS (Glander 2017). I obtain 29 state block-level shapefiles from National Historical Geographic Information System (NHGIS), clip these state files by the 129 school district borders and merge these files into one “national” block-group shapefile (Manson, Schroeder, Van Riper, and Ruggles 2017). I then spatially join block-level income data to both the actual and hypothetical attendance zones to aggregate the number of households in each of the 16 income brackets to both types of attendance zones.

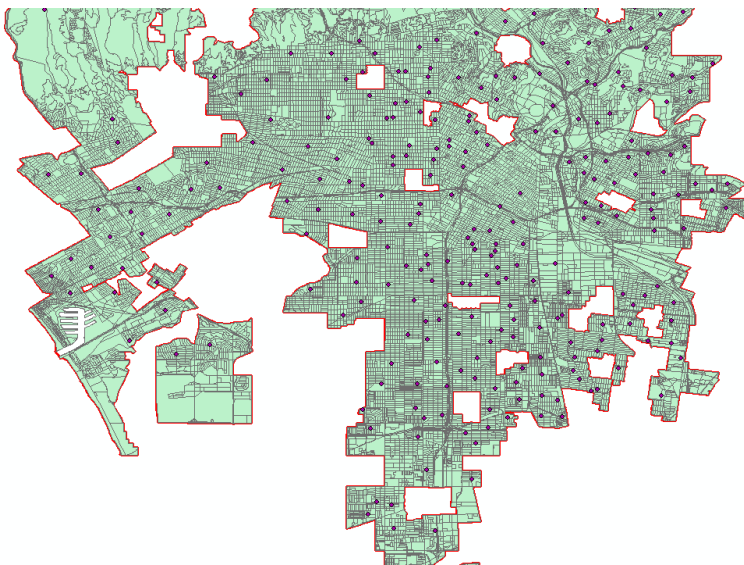
The American Community Survey (ACS) only produces 5-year estimates of “Household Income in the Past 12 Months (in 2012 Inflation-Adjusted Dollars)” at the block-group level. However, to minimize the problem of overlapping block groups across actual and hypothetical attendance zone boundaries during the spatial joining process, it is useful to allocate the block-group income data down to the block level. Census blocks can still overlap attendance boundaries, but blocks are often much smaller and contain less data per unit than block groups, so joining block-level income data to the attendance zones rather than block-group data will result in less overlapping overall. Saporito (2017) already completed this data allocation process for his own work, and he generously allowed me to use his block-level income data for this thesis. For this process, Saporito (2017, 1369) generated block-level income estimates by determining the proportion of families in each of the 16 income brackets in each block-group. Then, Saporito applied these proportions to the total populations of each block nested within each block-group. When this block-level data is spatially joined to the attendance zones, I allocate blocks that cross into multiple attendance zones to one zone based on the location of the block’s centroid. After income data is joined in this fashion to the actual and hypothetical attendance zones, I export the data from ArcGIS and into STATA for the segregation calculations and final ttest calculations.

One problem that arises when eliminating magnet and charter schools from my sample of schools is that because of the way the NCES organizes its school attendance boundary shapefile, the elimination of magnet and charter schools produces holes in the shapefile. This occurs because the NCES organizes its geospatial data such that each observation in the shapefile’s attribute table is a school, rather than a school attendance zone. If the shapefile had been laid out with each observation as a school zone, public school zones, charter zones, and magnet zones



would be able to overlap each other as they do in the real world, and the removal of charter and magnet zones would simply reveal the underlying public school zones that each area of the U.S. must have. However, because each observation in the shapefile is a school, each point in space is assigned to one school, and so the removal of magnet and charter schools from the sample results in holes in the shapefile. A visualization of this problem is pictured in Figure 4, using Los Angeles Unified School District as an example. Note the presence of holes scattered across the school district – these holes represent areas that constitute magnet and charter school attendance zones. The presence of holes is problematic primarily for the drawing of Thiessen polygons, because the Thiessen polygons constructed as the hypothetical zones for the remaining public schools must be drawn around the arbitrary holes that exist after excluding magnet and charter schools.

**Figure 4.** Visualization of Holes in Attendance Boundary Shapefile – Los Angeles Unified School District



While the presence of holes in some school district shapefiles is unfortunate, I do not believe that they will seriously affect my results. While 69 total school districts in the sample

have at least one magnet or charter school, which means all those school districts have at least one hole, magnet and charter schools comprise only 5 percent of all schools in 32 of those 69 districts. Furthermore, in 45 of those 69 districts, magnet and charter schools comprise at or less than 10 percent of all schools in those districts. There are only 9 school districts out of 129 where the percentage of magnet and charter schools is greater than 20 percent. Because the vast majority of school districts in the sample do not have a significant number of holes as a result of removing charter and magnet schools, I am confident that my results will still be reliable despite this problem.

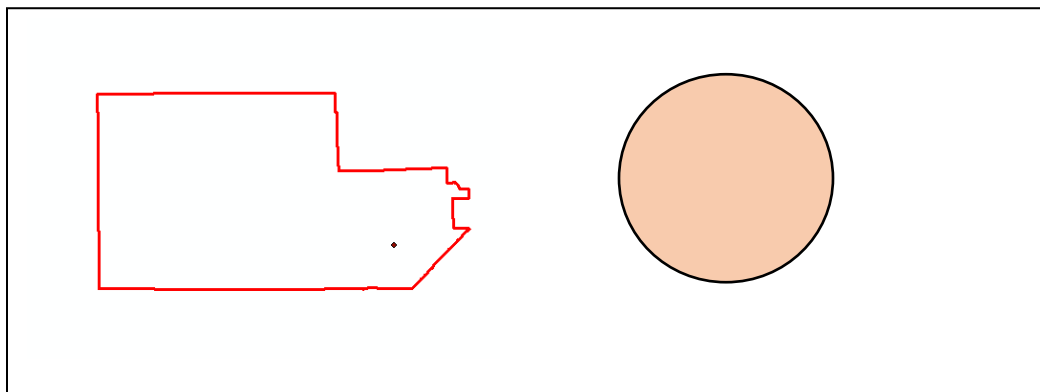
### *Research Methods & Data: Part II*

The second part of my analysis is based on Saporito and Van Riper's (2016) work, and I apply their method of studying racial segregation across schools to the topic of income segregation. One of Saporito and Van Riper's (2016, 6) critiques of Richards (2014) is that her methodology comparing actual and Thiessen polygons does not involve any direct measurements of attendance zone shapes. To determine the extent to which attendance zone gerrymandering affects income segregation, Saporito and Van Riper (2016) assert it is necessary to first quantify the shapes of attendance zones using measures of shape irregularity found in the legislative gerrymandering literature, and then test for correlations between attendance zone shape and indicators of racial diversity and integration. They argue that if irregularly shaped zones are more likely to have racially diverse populations, this suggests that zones are gerrymandered to integrate students. However, if irregularly shaped zones tend to have more racially homogenous populations, this suggests that zones are gerrymandered to further segregate students. I can apply Saporito and Van Riper's (2016) methodology to the topic of income segregation by calculating spatial compactness scores for each attendance zone in my sample, calculating measures of

income diversity within each attendance zone, and then testing for correlations between the spatial compactness scores and the income diversity measures.

I measure the irregularity of zone shapes by using two measures frequently used in the legislative gerrymandering literature – the Schwartzberg index and the Reock index. Each of these indices measure either indentation or dispersion, which are the two main spatial characteristics used to quantify compactness. Indentation refers to whether a zone’s perimeter is perfectly smooth or jagged with various concavities or protuberances. The Schwartzberg index is a measure of indentation, and it is calculated by comparing the perimeter of an attendance zone to the circumference of a hypothetical perfect circle that has the same area as the attendance zone (McGlone 2016). Higher values of the Schwartzberg index indicate more irregular shapes. A visualization of the Schwartzberg calculation using the example of Redland Elementary School in Miami-Dade County is included in Figure 5A, and the formula for the Schwartzberg index is included in Figure 5B. For each attendance zone, I calculate the radius of the hypothetical circle with an area equal to that of the attendance zone. Then, using this radius, I calculate the Schwartzberg score by dividing the perimeter of the attendance zone by the circumference of the hypothetical circle.

**Figure 5A.** Schwartzberg Index Visualization – Redland Elementary School



**Figure 5B.** Schwartzberg Index Calculation

$$1. r_i = \sqrt{\frac{A_i}{\pi}}$$

$$2. \text{Schwartzberg Index}_i = 1 - \frac{1}{\frac{P_i}{2\pi r_i}}$$

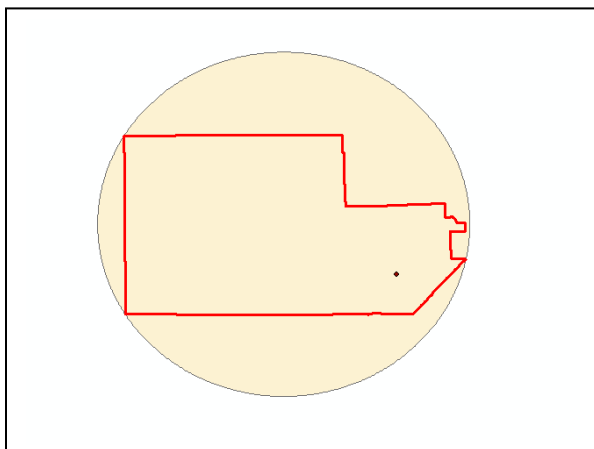
$A_i$  = Area of each attendance zone, i

$r_i$  = Radius of the hypothetical circle with the same area as each attendance zone, i

$P_i$  = Perimeter of each attendance zone, i

Dispersion refers to “the extent to which the area within a shape is tightly packed around its center or is elongated or stretched out” (Richards & Stroub 2015, 7). The Reock index is a measure of dispersion, and it is calculated by dividing the area of each attendance zone by the area of each attendance zone’s minimum bounding circle (McGlone 2016). A visualization of the Reock index calculation using the example of Redland Elementary School in Miami-Dade County is included in Figure 6A, and the formula for the Reock index is included in Figure 6B. Higher values of the Reock index indicate more irregular shapes.

**Figure 6A.** Reock Index Visualization – Redland Elementary School



**Figure 6B.** Reock Index Calculation

$$Reock\ Index_i = 1 - \frac{\text{Area of each attendance zone, } i}{\text{Area of each attendance zone's minimum bounding circle}}$$

My measures of income diversity are Simpson's measures of absolute and relative diversity, which are the same measures used by Saporito and Van Riper (2016). The formulas for Simpson's absolute and relative diversity indices are pictured in Figure 7 (Saporito and Van Riper 2016). When the absolute diversity index for an attendance zone equals one, the proportions of each income group included in the index are perfectly identical. Conversely, when the absolute diversity index for an attendance zone equals zero, all the households in the attendance zone belong to one income group. Therefore, more diverse attendance zones will have absolute diversity scores closer to one, and less diverse zones will have scores closer to zero. One drawback of this absolute measure is that it does not compare the income diversity levels of attendance zones with the diversity levels of their corresponding school districts, so I also calculate relative diversity to address this limitation. When the relative diversity index for an attendance zone equals one, the proportions of each income group included in the index mirror the socioeconomic proportions of its corresponding school district. Conversely, the relative diversity index for an attendance zone equals zero when all the households of an attendance zone belong to one income group. For these calculations, I divide the 16 income categories provided by the ACS into 5 income groups.

**Figure 7.** Simpson’s Absolute and Relative Diversity Indices

$$Absolute\ Diversity_i = 1 - \sum_{r=1}^K p_{ri}^2$$

$$Relative\ Diversity_i = 1 - (0.5 \sum_{r=1}^K |P_{rj} - p_{ri}|)$$

$p_r$  = proportion of people in income group  $r$  for each attendance zone,  $i$   
 $K$  = number of income groups included in the index  
 $P_r$  = proportion of people in income group  $r$  for each school district,  $j$

I opt to use these 5 groups rather than the original 16 income brackets in the diversity calculations because dividing each zone’s population into as many as 16 brackets results in smaller numbers of households per bracket. As a result, distributing the zone’s population across 16 brackets generally makes the proportions of people in each income bracket look very equal, which results in unrealistically high diversity scores for all the attendance zones. For instance, when the diversity index is calculated with 16 income groups, the mean absolute diversity score across all the zones is 0.905 and the median score is 0.912. Furthermore, the extent to which households are equally distributed across 16 different income brackets in each attendance zone is not really what I am interested in quantifying – I am more interested in the extent to which low-income, middle-income, and high-income households are distributed equally within attendance zones. Using all 16 categories seems to conceal variation in income diversity that could be happening broadly between higher-income and lower-income income brackets, so it makes sense to group the attendance zone households into a smaller number of income groups.

I define the 5 income groups used for the diversity calculations as “Lowest,” “Lower-Middle,” “Middle,” “Upper-Middle,” and “Highest,” and these income groups roughly

correspond to the national household income quintiles of the bottom 20 percent, the top 20 percent, and the 3 income quintiles in between. To construct each of these income groups, I allocate each of the 16 ACS “Household Income” brackets into the appropriate income group based on the household income limit associated with the national household income quintile for that income group. For example, for the “Lower-Middle” income group which seeks to approximate the household incomes of the second lowest national quintile, I allocate the ACS “Household Income” variables of “\$20,000-\$24,999, \$25,000-\$29,999, \$30,000-\$34,999, and “\$35,000-\$39,999” because the highest income a household could receive and still be part of the second lowest income quintile was \$39,764 in 2012. A complete record of this allocation process is found below in Table 1.

**Table 1.** Allocation of 16 ACS Household Income Brackets to 5 Income Groups

<b>Income Group Name.</b>	<b>National Household Income Quintile.</b>	<b>Annual Household Income Percentile Limits (2012).</b>	<b>ACS “Household Income in the Past 12 Months” income brackets used.</b>
Lowest	Lowest Fifth	20,599	Less than \$10,000; \$10,000-\$14,999; \$15,000-\$19,999
Lower-Middle	Second	39,764	\$20,000-\$24,999; \$25,000-\$29,999; \$30,000-\$34,999; \$35,000-\$39,999
Middle	Third	64,582	\$40,000-\$44,999; \$45,000-\$49,999; \$50,000-\$59,999
Upper-Middle	Fourth	104,096	\$60,000-\$74,999 \$75,000-\$99,999
Highest	Highest Fifth	No cutoff – includes all income above \$104,096	\$100,000-\$124,999 \$125,000-\$149,999 \$150,000-\$199,999 \$200,000 or more

My sample for this analysis includes all public-school attendance zones from the same 129 school districts included in Part I of my methodology, once again excluding magnet schools, charter schools, open-enrollment schools, and schools that do not serve first graders. This results in a final sample size of 8,483 attendance zones. Furthermore, I use the same school attendance boundary dataset and the same income data allocated to the block level as I did in Part I of my methodology. Block-level income data is also spatially joined to attendance zone boundaries using the same process as delineated in Part I of my methodology.

### *Specific Hypotheses*

Earlier in this thesis I stated my general hypothesis, which is that as Saporito's (2017) theory predicts, school attendance zone gerrymandering generally produces lower levels of income segregation across schools. Saporito (2017) believes that non-gerrymandered, perfectly compact attendance zones increase segregation by reinforcing the neighborhood-level socioeconomic homogeneity caused by residential segregation patterns. My specific hypothesis for Part I of my analysis is that, in accordance with Saporito's theory, hypothetical, perfectly compact attendance zones generated through the Thiessen polygon method will have higher dissimilarity index values on average and therefore produce higher levels of segregation than the actual attendance zones. My specific hypothesis for Part II of my analysis is, also in accordance with Saporito's theory, that more irregularly shaped attendance zones will have higher levels of income diversity than more compact zones.

### *Results: Part I*

The results from Part I of my research methodology are described below. The dependent ttest analyzing the difference between mean segregation levels produced by actual and



hypothetical attendance zones found that hypothetical, non-gerrymandered zones produced very slightly higher levels of income segregation across schools within school districts than actual zones. The results of this ttest can be found in Figure 4. These results demonstrate that among the 129 school districts, actual attendance zones had a mean segregation score of about 0.262 and the Thiessen attendance zones had a mean segregation score of 0.266. Even though this data shows school districts were on average slightly more segregated under the Thiessen attendance zones than under actual attendance zones, the difference between them is not statistically significant. Since the test results show a two-tailed p-value of 0.1270, which is greater than 0.05, we are unable to reject the null hypothesis that the difference between the mean segregation scores produced by actual and hypothetical attendance zones is equal to zero (Institute for Digital Research and Education 2017).

**Table 2.** Dependent ttest results for segregation levels produced by actual/original and hypothetical/Thiessen attendance zones.

```

Paired t test

```

Variable	Obs	Mean	Std. Err.	Std. Dev.	[95% Conf. Interval]	
thies_dis	129	.2664727	.0041641	.0472948	.2582334	.274712
og_dis	129	.2624422	.0048557	.0551499	.2528344	.27205
diff	129	.0040305	.002624	.0298027	-.0011615	.0092225

```

      mean(diff) = mean(thies_dis - og_dis)          t = 1.5360
Ho: mean(diff) = 0                                degrees of freedom = 128

Ha: mean(diff) < 0          Ha: mean(diff) != 0          Ha: mean(diff) > 0
Pr(T < t) = 0.9365          Pr(|T| > |t|) = 0.1270          Pr(T > t) = 0.0635

```

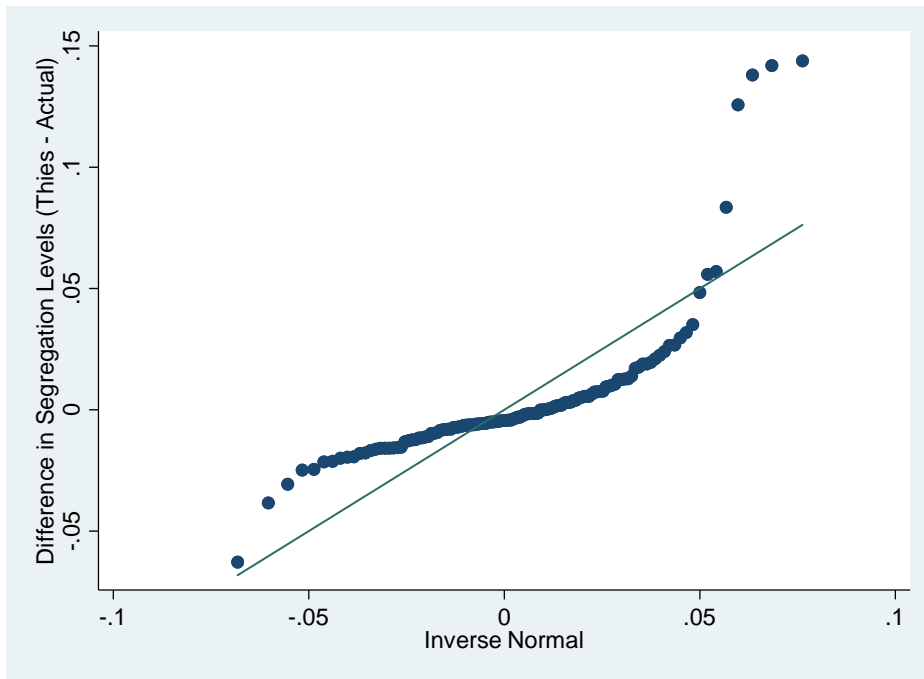
Depending on how we interpret Thiessen polygons, these findings can be interpreted in two ways. If we agree with Richards' (2014, p. 1119) conceptualization of Thiessen polygons, which is that they hypothetically represent what school attendance zones would look like in the

complete absence of gerrymandering, these findings demonstrate that attendance zone gerrymandering does not “generally exacerbate segregation,” because the segregation levels produced by the actual and non-gerrymandered zones are not meaningfully different from each other. If anything, attendance zone gerrymandering slightly reduces segregation, as the mean segregation levels produced by Thiessen, non-gerrymandered zones are slightly higher than segregation levels produced by actual zones. If we agree with Saporito’s (2017) conceptualization of Thiessen polygons, which is that they essentially represent neighborhood units within districts and can be used to measure residential segregation, then non-significant differences in segregation between actual school attendance zones and Thiessen polygon neighborhood units suggest school segregation and residential segregation are essentially one and the same.

For the results of this dependent ttest to be considered fully reliable, the distribution of the differences in dissimilarity index values produced by actual and hypothetical zones for each school district should be normally distributed. I test for normality by plotting the differences between the segregation levels produced by actual and hypothetical zones for each school district on a qnorm plot. On the qnorm plot, if the differences in segregation levels are normally distributed, the points on the graph will closely track the diagonal line which represents the values that the differences in segregation levels would take on if they were in fact perfectly normally distributed. We can see in Figure 8 that while the points in the middle of the line seem to track the line quite precisely, there are certain points at the top and bottom of the distribution that diverge dramatically from the line. Based on these irregularities, it is unclear from the qnorm plot alone whether the differences in segregation levels for each school district are normally distributed. Table 3 shows the results from a Skewness-Kurtosis test, which is a method of

testing for normality based on a null hypothesis that the differences in segregation levels are normally distributed. The results for this Skewness-Kurtosis test can be found in Figure 5. Since the p-value for this hypothesis test was 0.00, which is less than 0.05, we must reject the null that the differences in segregation levels are normally distributed. The fact that the differences in segregation levels between actual and hypothetical attendance zones for each school district do not appear to be normally distributed is concerning and calls the results of the ttest into question.

**Figure 8.** Qnorm Plot for the Difference in Segregation Levels Between Actual / Thiessen Zones



**Table 3.** Skewness-Kurtosis test for the Difference in Segregation Levels Between Actual / Thiessen Zones

```
. sktest diff
```

Variable	Skewness/Kurtosis tests for Normality				
	Obs	Pr(Skewness)	Pr(Kurtosis)	adj chi2(2)	joint Prob>chi2
diff	129	0.0000	0.0000	73.47	0.0000

It is not essential for the distribution of differences between the two types of segregation levels to perfectly follow a normal distribution. If the sample of differences is “large enough” or does not deviate dramatically from a normal distribution, the results of the dependent ttest can still be considered reliable. However, because the extent to which the sample is “large” or “approximately” normal are nebulous criteria, I used a Wilcoxon Matched-Pairs Signed-Rank test to test the robustness of my initial dependent ttest. This test serves the same purpose and has the same null hypothesis as a dependent ttest but does not rely on a normality assumption (Ghasemi & Zahediasl 2012). The results of the Wilcoxon Matched-Pairs Signed-Rank test are shown in Table 4. Because the p-value of this ttest was 0.41, which is greater than 0.05, we cannot reject the null hypothesis that the difference between the mean segregation levels of actual and hypothetical attendance zones is statistically different from zero. The consistency of the Wilcoxon test results with the initial dependent ttest’s results shows that the non-parametric nature of the data does not invalidate the initial dependent ttest’s findings

**Table 4.** Wilcoxon Signed-Rank Test for the Difference in Segregation Levels Between Actual / Thiessen Zones

```

Wilcoxon signed-rank test

```

sign	obs	sum ranks	expected
positive	54	3842	4192.5
negative	75	4543	4192.5
zero	0	0	0
all	129	8385	8385

```

unadjusted variance  180976.25
adjustment for ties      0.00
adjustment for zeros      0.00
-----
adjusted variance    180976.25

```

```

Ho: thies_dis = og_dis
      z = -0.824
      Prob > |z| = 0.4100

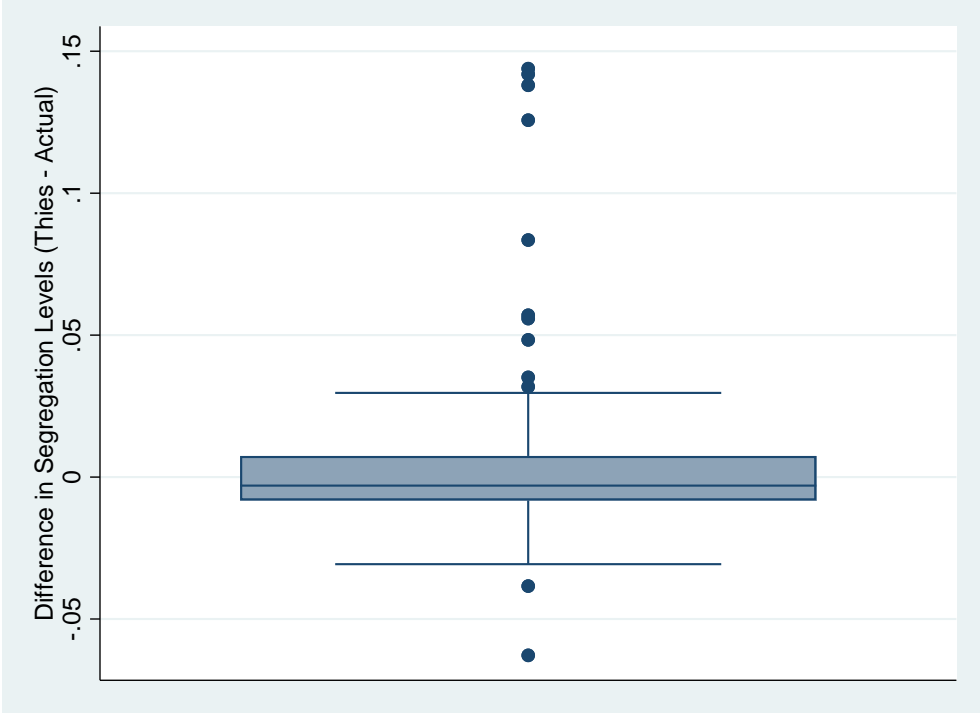
```

Another assumption underlying the dependent ttest is that there must be no significant outliers in the differences of dissimilarity index values produced by actual and hypothetical zones for each school district. I checked for the presence of outliers by constructing a box-and-whisker plot of the differences in segregation levels between actual and Thiessen zones across the school districts. Figure 9 displays this box-and-whisker plot, and it is clear from this plot that there are several school districts that are outliers. In the box-and-whisker plot, the top, middle, and bottom lines in the blue colored box respectively represent the 75<sup>th</sup>, 50<sup>th</sup>, and 25<sup>th</sup> percentile values of the data, in this case the differences in segregation levels between Thiessen and actual zones for each school district. The top whisker of the box-and-whisker plot is referred to as the upper adjacent value, which is equal to the 75<sup>th</sup> percentile value plus the product of the interquartile range (the difference between the 75<sup>th</sup> percentile and 25<sup>th</sup> percentile values) and 1.5. The bottom whisker of the box-and-whisker plot is referred to as the lower adjacent value, which is equal to the 25<sup>th</sup> percentile value minus the product of the interquartile range and 1.5. All the points lying beyond the upper and lower adjacent values are considered the outliers of the data. As Figure 9 shows, there are 2 school district outliers below the lower adjacent value and 10 school district outliers above the upper adjacent value on the box-and-whisker plot. To trust the validity of the initial dependent ttest, it is important to evaluate whether these outliers had a major effect on the ttest's results.

To once again test the robustness of the original ttest, I temporarily removed the 12 outliers from the sample and reran the ttest to see if the new results were dramatically different from the initial results. The results of this new ttest with the reduced sample can be found in Table 5. These results show that the mean segregation values generated by the actual and hypothetical attendance zones have barely changed. In the new ttest, Thiessen polygon

attendance zones, with a mean segregation level of 0.265, produced slightly less segregation than the actual attendance zones, with a mean segregation level of 0.267. However, this difference is still not statistically significant, as the p-value for this dependent ttest is 0.064, which is greater than 0.05. Based on the size of this p-value, we cannot reject the null hypothesis that the difference in mean segregation levels produced by actual and hypothetical zones is equal to 0. This new ttest shows that the outliers did not dramatically influence the outcome of the initial ttest, because while the mean segregation level of Thiessen attendance zones did go down after removing the outliers, there was still no statistically significant difference in segregation levels between the two types of attendance zones after removing the outliers.

**Figure 9.** Box-And-Whisker Plot for the Difference in Segregation Levels Between Actual / Thiessen Zones



**Table 5.** Dependent ttest with Outliers Removed.

Paired t test						
Variable	Obs	Mean	Std. Err.	Std. Dev.	[95% Conf. Interval]	
thies_~s	117	.2652054	.0044323	.0479428	.2564267	.2739842
og_dis	117	.2672558	.0044271	.0478863	.2584874	.2760242
diff	117	-.0020504	.0010954	.0118486	-.00422	.0001192
mean(diff) = mean(thies_dis - og_dis)				t =		-1.8718
Ho: mean(diff) = 0				degrees of freedom =		116
Ha: mean(diff) < 0		Ha: mean(diff) != 0		Ha: mean(diff) > 0		
Pr(T < t) = 0.0319		Pr( T  >  t ) = 0.0638		Pr(T > t) = 0.9681		

The outliers from the initial ttest are important to evaluate because as shown by Figure 9, they represent the few cases where the segregation levels produced by the actual and hypothetical attendance zones are meaningfully different from each other. The box-and-whisker plot in Figure 9 shows that the 25<sup>th</sup>, 50<sup>th</sup>, and 75<sup>th</sup> percentile values for the differences in segregation levels hover closely around 0. Even the upper and lower adjacent values for the differences in segregation levels between actual and hypothetical zones diverge from 0 by less than 0.05. The similarity of the segregation levels produced by actual and hypothetical zones for most school districts suggests that most actual school attendance zones do not differ greatly in shape from their Thiessen polygon counterparts. This is not an entirely surprising finding, as most school districts take a “neighborhood schools” approach to deciding which schools children will be zoned to attend. However, what is striking is that when the outlier school districts have meaningfully different levels of segregation between the hypothetical and actual school attendance zones, the Thiessen polygon attendance zones generally produce higher levels of segregation than the actual zones. This is illustrated by the fact that 10 of the 12 school district outliers have a positive rather than negative difference between Thiessen and actual attendance zones, which means that the segregation value produced by hypothetical, Thiessen zones must be

greater than the segregation value produced by the actual zones for each of those school districts. Furthermore, of these 10 school districts, 2 of them – Seminole School District and Wake County Public Schools – have implemented districtwide policies where they have explicitly altered their school attendance zones to create more socioeconomically integrated schools (Potter et. al 2016). This suggests that at least some school districts are actively redrawing their attendance boundaries to deviate from a strict neighborhood schools model with the goal of integrating public schools, and that these approaches are effective in reducing income segregation.

All in all, both the original ttest and the box-and-whisker plot demonstrate that on average, school districts do not have statistically significant differences in segregation produced by their actual attendance zones and the hypothetical, perfectly non-gerrymandered attendance zones that would exist if every child attended the school closest to them. However, for the outlier school districts that do produce more dramatically different segregation levels between actual and hypothetical zones, 5 times as many districts have actual attendance zones that are less segregated than their hypothetical Thiessen polygon zones. This suggests that deviating from Thiessen polygon zones, rather than attempting to conform to them, is the best way to draw attendance zones in order to reduce segregation.

### *Results: Part II*

The results from Part II of my research methodology are described below. As shown in Table 6, the correlations between absolute income diversity and the zone irregularity measures are very weak, with a very low negative correlation of -0.0508 for the Schwartzberg Index and a very low positive correlation of 0.0062 for the Reock Index. This suggests that zones with more



irregularly indented shapes tend to have very slightly less income diversity and that zones with more irregularly dispersed shapes tend to have very slightly more income diversity in absolute terms. However, this measure of absolute diversity is somewhat problematic because it does not take the diversity levels of each attendance zone’s encompassing school district into account. When diversity levels of school districts are factored into the calculations, the correlations between relative diversity and both zone irregularity measures increase. Relative diversity is correlated positively with both the Schwartzberg Index and the Reock Index, at 0.0942 and 0.0502 respectively. This suggests that zones with more irregularly indented and dispersed shapes tend to have slightly more income diversity relative to the diversity levels of their school districts at large. Therefore, while the positive direction of the relative diversity and shape irregularity correlations lends some support to Saporito’s argument that irregularly shaped, gerrymandered zones are more likely to have high levels of diversity than compact zones, the small size of these correlations suggests further analysis is needed.

**Table 6.** Correlation Coefficients for Zone Irregularity and Diversity Measures

	<b>Schwartzberg Index</b>	<b>Reock Index</b>
<b>Absolute Diversity</b>	-0.0508	0.0062
<b>Relative Diversity</b>	0.0942	0.0502

The positive yet weak correlations I find between both diversity measures and both measures of zone shape irregularity could be caused by low rates of school attendance zone gerrymandering across the school districts in my sample. Even if there is a more strongly positive relationship between the irregularity of attendance zone shapes and higher levels of income diversity, the correlations will not pick up on this strong relationship if most attendance

zones are not shaped irregularly. I have already produced some evidence that attendance zones are not significantly gerrymandered on average, as the original dependent ttest results from Part I of my results illustrate that the difference in segregation levels produced by actual and hypothetical, perfectly compact attendance zones does not statistically significantly deviate from zero.

Another way to determine the extent to which attendance zones are gerrymandered on average is by comparing the average compactness scores of U.S. congressional districts to the average compactness scores of the attendance zones in my sample (Saporito and Van Riper 2016, 13). Unfortunately, a universally accepted cutoff for compactness scores indicating substantial levels of gerrymandering does not exist in the gerrymandering literature or in legislative gerrymandering jurisprudence, so the best way to determine whether gerrymandering is “high” or “low” is through relative comparisons of compactness scores. In 2012, McGlone and Cheetham calculated the nationwide mean Reock and Schwartzberg scores for all U.S. congressional districts, and they found congressional districts had an average Reock score of 0.6271 and an average Schwartzberg score of 0.5388. They initially calculated their indices such that a score of zero represented maximum irregularity and a score of one represented maximum compactness, so I had to invert their average scores to make them comparable with the scores calculated in my analysis. These scores are higher than the average Reock and Schwartzberg scores of the school attendance zones in my sample. The attendance zones in my sample had a mean Reock score of 0.5771 and a mean Schwartzberg score of 0.3324. This evidence suggests that attendance zones are less gerrymandered in terms of dispersion and significantly less gerrymandered in terms of indentation than congressional districts, which lends support to the argument that attendance zones on average are not meaningfully gerrymandered. Therefore, the

low correlations depicted in Table 6 could be driven by low levels of attendance zone gerrymandering across the sample.

If it is true that attendance zones tend not to be very gerrymandered on average, I can better understand the effect of attendance zone gerrymandering on zone income diversity levels by analyzing the diversity levels of the attendance zones that are outliers in terms of shape irregularity and directly comparing those zones to the more compact zones. For each of the shape irregularity measures, I assign attendance zones to different shape categories – “Very Compact,” “Compact,” “Average,” “Irregular,” “Very Irregular,” and “Extremely Irregular.” I calculate two z-scores for each attendance zone to determine how many standard deviations each attendance zone’s Reock and Schwartzberg scores are from the mean Reock and Schwartzberg scores of the entire sample. For each measure, attendance zones are considered “very compact” if their irregularity score is less than -1.5 standard deviations from the mean, “compact” if their irregularity score is between -1.5 and -0.5 standard deviations from the mean, and “average” if their irregularity score is between -0.5 and 0.5 standard deviations from the mean. Attendance zones are considered “irregular” for each measure if their irregularity score is between 0.5 and 1.5 standard deviations from the mean, “very irregular” if their irregularity score is between 1.5 and 2.5 standard deviations from the mean, and “extremely irregular” if their irregularity score is greater than 2.5 standard deviations from the mean. There is no “extremely compact” category because there were only 3 attendance zones with Reock scores less than -2.5 standard deviations from the mean, and there were zero attendance zones with Schwartzberg scores less than -2.5 standard deviations from the mean. I base my classification scheme on the classification scheme used by Saporito and Van Riper (2016, 23).

Tables 7A and 7B display the mean relative diversity scores for each shape category and for each type of shape irregularity measure. For both the Reock and Schwartzberg indices, the average relative income diversity scores for very irregular and extremely irregular zones were several percentage points larger than the average relative diversity scores of very compact and compact attendance zones. Very compact and compact attendances zones in terms of the Reock index had average relative diversity scores of 0.832 and 0.837 respectively, and the average relative diversity scores increased to 0.855 and 0.857 for very irregular and extremely irregular attendance zones. There was an even greater difference in the average relative diversity scores of very compact and extremely irregular zones when quantifying attendance zone shapes using the Schwartzberg index. In terms of the Schwartzberg index, very compact zones had an average relative income diversity score of 0.834 while extremely irregular zones had an average relative diversity score of 0.878. These tables demonstrate that for both Reock scores and Schwartzberg scores, attendance zones consistently become more diverse on average as attendance zone shapes become more irregular.

**Table 7A.** Average Relative Diversity Scores for Different Attendance Zone Shapes: Reock

<b>Attendance Zone Shape</b>	<b>Shape Irregularity Measure</b>	<b>Average Relative Income Diversity Score for each shape type</b>	<b>N for each shape type</b>
Very Compact	Reock	<b>.8321353</b>	421
Compact	Reock	.83876562	2400
Average	Reock	.83793867	3184
Irregular	Reock	.84088838	1813
Very Irregular	Reock	.85535115	539
Extremely Irregular	Reock	<b>.85670942</b>	126

**Table 7B.** Average Relative Diversity Scores for Different Attendance Zone Shapes: Schwartzberg

Attendance Zone Shape	Shape Irregularity Measure	Average Relative Income Diversity Score for each shape type	N for each shape type
Very Compact	Schwartzberg	<b>.83376026</b>	361
Compact	Schwartzberg	.8384006	2467
Average	Schwartzberg	.84179014	3192
Irregular	Schwartzberg	.84613252	1864
Very Irregular	Schwartzberg	.86401433	581
Extremely Irregular	Schwartzberg	<b>.87841052</b>	127

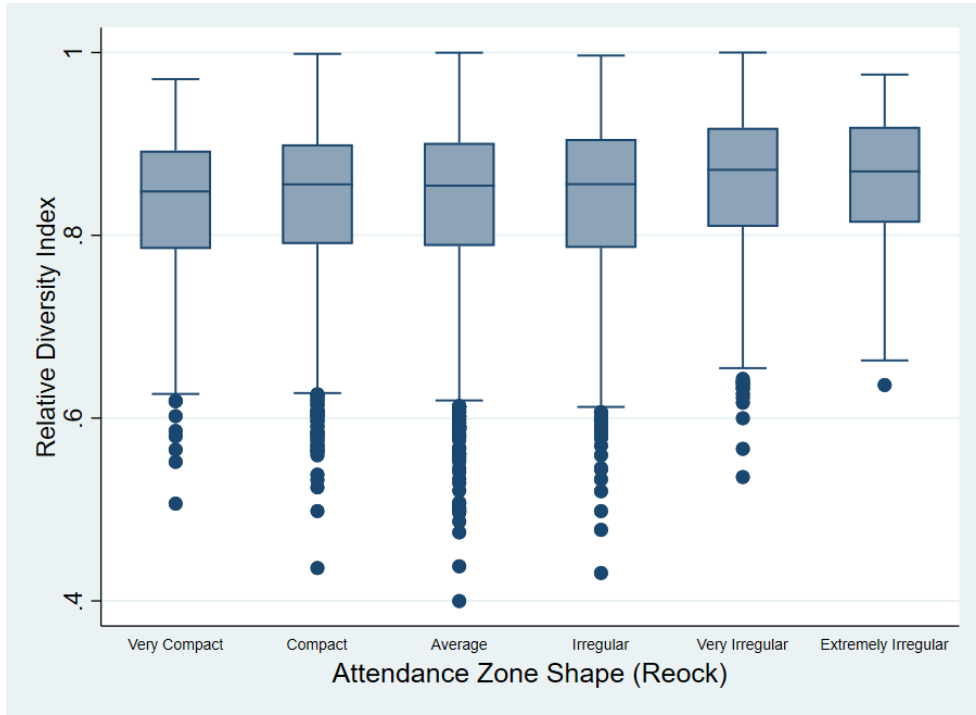
To check for the presence of outliers and to examine the spread of the relative diversity data for each of the shape irregularity categories, I construct box-and-whisker plots for the different attendance zone shape measures and the relative diversity index. As with the box-and-whisker plot in Part I of my results, the top, middle, and bottom lines in the blue colored boxes of each box plot respectively represent the 75<sup>th</sup>, 50<sup>th</sup>, and 25<sup>th</sup> percentile values of the data, in this case relative diversity score values. Once again, the top whisker of the box-and-whisker plot is referred to as the upper adjacent value, which is equal to the 75<sup>th</sup> percentile value plus the product of the interquartile range (the difference between the 75<sup>th</sup> percentile and 25<sup>th</sup> percentile values) and 1.5. The bottom whisker of the box-and-whisker plot is also referred to as the lower adjacent value, which is equal to the 25<sup>th</sup> percentile value minus the product of the interquartile range and 1.5. All the points lying beyond the upper and lower adjacent values are considered the outliers of the data.

Figure 10A shows box-and-whisker plots of relative diversity scores for each of the different attendance zone shape categories as quantified by the Reock index. As Figure 10A demonstrates, while very compact, compact, average, irregular, and very irregular zones all have

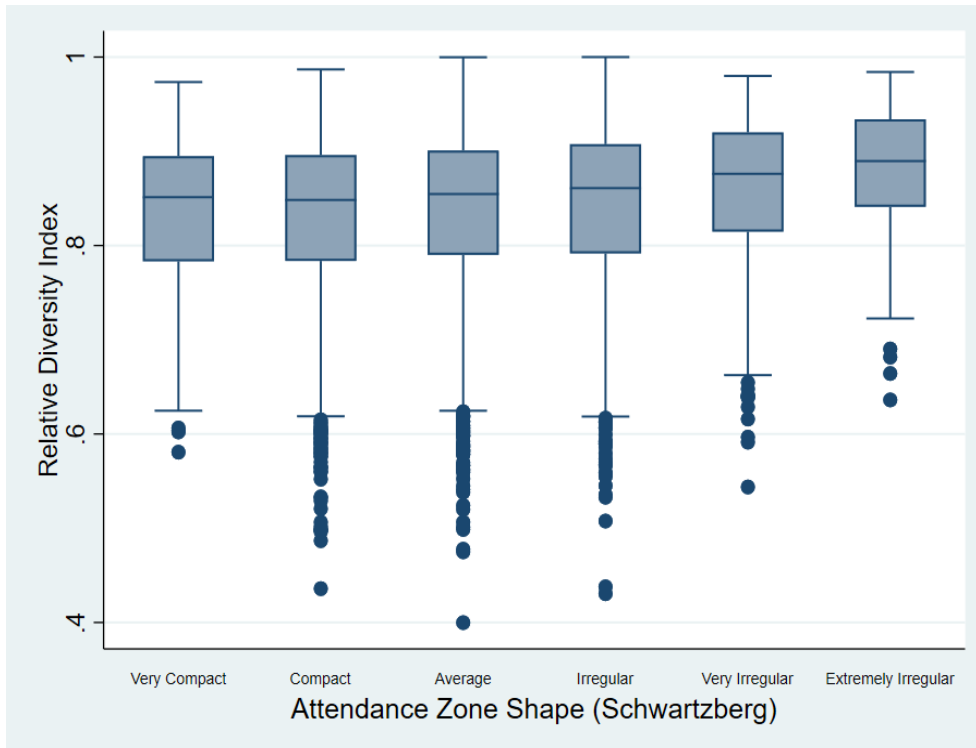
a substantial number of zone outliers with diversity scores lying below the lower adjacent values of each box plot, there is only one extremely irregular zone outlier with a diversity score that lies below the lower adjacent value of the box plot for extremely irregular zones. This suggests that of all the attendance zone shapes, extremely irregular zones in terms of Reock score have the most consistently high levels of diversity because they have significantly fewer low diversity score outliers.

Figure 10B shows box-and-whisker plots of relative diversity index values for each of the different attendance zone shape categories as quantified by the Schwartzberg index. There are roughly similar numbers of low relative diversity outliers for very compact and extremely irregular attendance zone shapes, but this seems to be because the lower adjacent value of the box plot for extremely irregular zone shapes is significantly higher than any other box plot's lower adjacent value. This lower adjacent value is probably significantly larger than those of the other box plots because the 75<sup>th</sup> percentile value of relative diversity scores for extremely irregular zones is much higher and the difference between the 25<sup>th</sup> and 75<sup>th</sup> percentile values of relative diversity scores is lower than those of the other zone shapes. This suggests that aside from those low outliers, extremely irregular zones have much higher relative diversity scores overall that are more tightly packed around their high mean relative diversity score than any other zone shape. Furthermore, all the outliers in the extremely irregular zones' box plot have noticeably higher relative diversity values than the outliers in the very compact zones' box plot.

**Figure 10A.** Box-And-Whisker Plot for Attendance Zone Shapes (Reock) and Relative Diversity Index



**Figure 10B.** Box-And-Whisker Plot for Attendance Zone Shapes (Schwartzberg) and Relative Diversity Index



In conclusion, the positive correlation between relative income diversity and both measures of shape irregularity, the Reock index and the Schwartzberg index, demonstrates that gerrymandered attendance zones tend to have higher levels of diversity. The weakness of these correlations could be attributed to the fact that on average, attendance zones tend not to be shaped irregularly. By assigning each attendance zone into “Very Compact,” “Compact,” “Average,” “Irregular,” “Very Irregular,” and “Extremely Irregular” categories, I can observe the mean levels of relative diversity for each shape type and for each measure of shape irregularity. The mean levels of relative diversity for extremely irregularly shaped zones were several percentage points higher than the mean levels of diversity for very compact zones across both the Reock Index and Schwartzberg Index, which confirms Saporito’s theory that irregularly shaped attendance zones tend to have higher levels of diversity. The box-and-whisker plot of the different attendance zone shapes under the Reock index suggests that extremely irregularly shaped zones not only have the highest mean relative diversity score, but that they are also the most consistently diverse because they have significantly fewer low-diversity outliers compared to all the other shape types. The box-and-whisker plot of the different attendance zone shapes under the Schwartzberg index also suggests that extremely irregularly shaped zones are the most consistently diverse, because the relatively high lower adjacent value of these zones’ box plot shows that the relative diversity scores are tightly packed around the high mean relative diversity score for irregular zones. Additionally, even the outliers of the extremely irregular zones still have higher levels of relative diversity than the outliers of all the other shape types. My findings that more irregularly shaped attendance zones have consistently higher levels of relative income diversity lend support to Saporito’s (2017) theory that attendance zone gerrymandering reduces income segregation across schools.



## *Implications*

My thesis sought to contribute to the existing debate over whether school attendance zone gerrymandering exacerbates or mitigates income segregation across schools. My findings suggest that on average, most attendance zones are not currently gerrymandered to dramatically deviate from attendance zones that assign all children to their closest neighborhood school. However, my findings show that among school districts with larger differences in segregation levels produced by actual and hypothetical attendance zones, there are significantly more school districts with actual attendance zones that produce lower levels of segregation than would otherwise exist under Thiessen zones. Furthermore, my results demonstrate that extremely irregularly shaped zones have substantially higher levels of income diversity than very compact zones, which suggests that gerrymandered school attendance zones are more likely to foster socioeconomic integration rather than income segregation. All this evidence suggests that Saporito's (2017) theory more accurately characterizes the relationship between school attendance zone gerrymandering and income segregation across schools than Richards' (2014) theory.

My findings do not suggest that all examples of school attendance zone gerrymandering will necessarily have an integrative effect. Richards (2014) and Siegel-Hawley (2013) make important contributions to the literature by pointing out specific examples of gerrymandered attendance zones that clearly do more to segregate than integrate students. In my own sample, the existence of several irregularly shaped zone outliers with unexpectedly low levels of income diversity suggests that some individual zones are indeed gerrymandered to increase socioeconomic homogeneity in schools. However, my results do suggest that on average,

drawing irregularly shaped, gerrymandered attendance zones seem to reduce segregation and promote economic integration.

Consequently, I would caution against some of Richards' policy recommendations, which include calling upon state and federal agencies to implement policies that establish a certain minimum standard of compactness for all attendance zones and implementing an oversight body to conduct investigations of districts that do not meet these compactness requirements (Richards and Stroub 2015, 27). In making these recommendations, Richards is clearly operating under the assumption that all attendance zone gerrymandering is automatically discriminatory and segregative. Saporito takes issue with both this assumption and these wide-reaching policy recommendations in one of his papers (Saporito 2017, 1350). My own analysis seems to suggest that Saporito (2017) is justified in his concern about Richards' (2014; 2015) policy prescriptions. It seems that if more school districts made an active attempt to draw irregular school attendance zones that cross-cut residentially segregated neighborhoods, school districts could build more socioeconomically diverse schools to better serve our nation's low-income children.

**Appendix 1: List of School Districts Analyzed, in order of enrollment size**

1. NEW YORK CITY PUBLIC SCHOOLS
2. LOS ANGELES UNIFIED
3. CITY OF CHICAGO SD 299
4. DADE
5. CLARK COUNTY SCHOOL DISTRICT
6. BROWARD
7. HOUSTON ISD
8. HILLSBOROUGH
9. ORANGE
10. HAWAII DEPARTMENT OF EDUCATION
11. FAIRFAX CO PBLC SCHS
12. PALM BEACH
13. GWINNETT COUNTY
14. DALLAS ISD
15. WAKE COUNTY SCHOOLS
16. MONTGOMERY COUNTY PUBLIC SCHOOLS
17. SHELBY CO
18. CHARLOTTE-MECKLENBURG SCHOOLS
19. PHILADELPHIA CITY SD
20. SAN DIEGO UNIFIED
21. DUVAL
22. PRINCE GEORGE'S COUNTY PUBLIC SCHOOLS
23. CYPRESS-FAIRBANKS ISD
24. COBB COUNTY
25. BALTIMORE COUNTY PUBLIC SCHOOLS
26. PINELLAS
27. JEFFERSON COUNTY
28. DEKALB COUNTY
29. POLK
30. FULTON COUNTY
31. ALBUQUERQUE PUBLIC SCHOOLS
32. SCHOOL DISTRICT NO. 1 IN THE COUNTY OF DENVER AND STATE OF C
33. PRINCE WILLIAM CO PBLC SCHS
34. AUSTIN ISD
35. BALTIMORE CITY PUBLIC SCHOOLS
36. DAVIDSON COUNTY
37. MILWAUKEE SCHOOL DISTRICT
38. ANNE ARUNDEL COUNTY PUBLIC SCHOOLS
39. FRESNO UNIFIED
40. GUILFORD COUNTY SCHOOLS
41. BREVARD
42. FORT BEND ISD
43. LOUDOUN CO PBLC SCHS
44. VA BEACH CITY PBLC SCHS

45. DAVIS DISTRICT
46. GRANITE DISTRICT
47. NORTH EAST ISD
48. PASCO
49. ALDINE ISD
50. DOUGLAS COUNTY SCHOOL DISTRICT NO. RE 1
51. WASHOE COUNTY SCHOOL DISTRICT
52. SEMINOLE
53. ARLINGTON ISD
54. MESA UNIFIED DISTRICT
55. ELK GROVE UNIFIED
56. EL PASO ISD
57. VOLUSIA
58. KNOX COUNTY
59. CHESTERFIELD CO PBLC SCHS
60. OSCEOLA
61. SAN FRANCISCO UNIFIED
62. SANTA ANA UNIFIED
63. CONROE ISD
64. PLANO ISD
65. PASADENA ISD
66. SAN ANTONIO ISD
67. CAPISTRANO UNIFIED
68. SAN BERNARDINO CITY UNIFIED
69. WINSTON SALEM/FORSYTH COUNTY SCHOOLS
70. JORDAN DISTRICT
71. HOWARD COUNTY PUBLIC SCHOOLS
72. LEWISVILLE ISD
73. CLAYTON COUNTY
74. CUMBERLAND COUNTY SCHOOLS
75. OMAHA PUBLIC SCHOOLS
76. HENRICO CO PBLC SCHS
77. SEATTLE PUBLIC SCHOOLS
78. COLUMBUS CITY SCHOOL DISTRICT
79. ATLANTA PUBLIC SCHOOLS
80. BROWNSVILLE ISD
81. TUCSON UNIFIED DISTRICT
82. SAN JUAN UNIFIED
83. KLEIN ISD
84. ANCHORAGE SCHOOL DISTRICT
85. PORTLAND SD 1J
86. OAKLAND UNIFIED
87. SACRAMENTO CITY UNIFIED
88. GARDEN GROVE UNIFIED
89. MANATEE
90. JEFFERSON PARISH

91. FRISCO ISD
92. CHARLESTON 01
93. DISTRICT OF COLUMBIA PUBLIC SCHOOLS
94. SOCORRO ISD
95. COLLIER
96. UNITED ISD
97. HAMILTON COUNTY
98. YSLETA ISD
99. RIVERSIDE UNIFIED
100. ADAMS 12 FIVE STAR SCHOOLS
101. MARION
102. EAST BATON ROUGE PARISH
103. LAKE
104. SARASOTA
105. CHANDLER UNIFIED DISTRICT #80
106. UNION COUNTY PUBLIC SCHOOLS
107. HORRY 01
108. CADDO PARISH
109. OKLAHOMA CITY
110. AURORA JOINT DISTRICT NO. 28 OF THE COUNTIES OF ADAMS AND A
111. CLOVIS UNIFIED
112. ESCAMBIA
113. HENRY COUNTY
114. FORSYTH COUNTY
115. FREDERICK COUNTY PUBLIC SCHOOLS
116. SALEM-KEIZER SD 24J
117. SD U-46
118. TULSA
119. FAYETTE COUNTY
120. CLEAR CREEK ISD
121. FONTANA UNIFIED
122. MESQUITE ISD
123. CHEROKEE COUNTY
124. CHESAPEAKE CITY PBLC SCHS
125. BEAVERTON SD 48J
126. STOCKTON UNIFIED
127. CORPUS CHRISTI ISD
128. RICHARDSON ISD
129. ANOKA-HENNEPIN PUBLIC SCHOOL DIST.

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