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# Independent Schools and Academic Achievement

## A Synthetic Control Approach on Swedish Municipalities

Anna Sandros Hansson (24375) and Folke Schröder Frykhammar (24481)

**Abstract:** During the early 1990's, the Swedish school system underwent a period of groundbreaking reforms, including the contentious independent school reform. The reforms coincided with a decline of Swedish results in international educational assessments, leading to a debate on whether independent schools positively or negatively contribute to students learning. This paper studies the effect of lower secondary independent schools on aggregate academic achievement and equality of schooling in multiple Swedish municipalities, in which an independent school has recently opened. Using the synthetic control method, this paper finds no evidence of an effect on aggregate academic achievement in any of the municipalities studied. However, using an educational value-added measure in a between municipality fixed effects framework, this paper finds an association between independent schools and rising inequality in schooling. The results indicate that the independent schools are not to blame for Sweden's declining results in international assessments and calls for policy makers to focus on other aspects of the school system to improve academic achievement.

**Keywords:** Education quality, Vouchers, Private Schools, Public Schools, School Privatization, Education Reform, Inequality, Synthetic Control

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| Discussants: | Urja Jain & Tzu-Hsi Chen |
| Examiner: | Kelly Ragan |

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# Contents

# 1 Introduction

Since 1995, the performance of Swedish students in international educational assessments such as the PISA and TIMSS has decreased substantially from a very high level[1]. The decline coincides with a period of groundbreaking reforms of the Swedish school system, including the contentious independent school reform. The reform granted privately operated schools, both for-profit and non-profit, public funding and let them compete with municipal schools. Whether these independent schools positively or negatively contribute to the country's educational goals of efficiency and equality[2], is both discussed in politics[3] and studied in academia, but yet no clear consensus prevails. Because quality of education is important for both private (Psacharopoulos & Patrinos, 2018) and societal economic outcomes (Moll et al., 2016; Benos & Karagiannis, 2016), establishing the true effect of independent schools is a worthwhile endeavor for researchers.

This paper will attempt to bring clarity to the effects of lower secondary independent schools on educational efficiency and equality, by studying the heterogeneous experiences of multiple Swedish municipalities, in which a lower secondary independent school has opened for the first time between 2011 and 2016. Firstly, we study the effects on efficiency using the synthetic control method. From municipalities that have never had an independent school, we construct a counterfactual for each of the municipalities studied. The results show that for most municipalities there is no effect large enough to distinguish it from noise, and for those that exhibit such an effect, robustness analysis invalidates the results. Thus, we do not find evidence of independent schools having an effect on aggregate educational results (efficiency) in the municipalities they locate in during the period we study. Secondly, we study equality by constructing an inequality measure, and studying its' development within and between municipalities. We define inequality as the

[1]Lately there has been some improvements but the latest PISA results have been criticized as a disproportionately high share of students have been excluded due to low proficiency in Swedish. Hence it should be interpreted cautiously. See table A.1 and A.2 in appendix A for a detailed overview of the development over time

[2]The Swedish school law statues that the educational system should provide students with knowledge, democratic values, and actively ensure that each student is given equal opportunity and access to education. Through the lens of an economist the goals boil down to efficiency and equality.

[3]See for example party leaders debate for the 2022 election by Aftonbladet or by SVT

within-municipality variation in school educational value-added, which takes into account the student body of each respective school. We conclude that inequality does not increase in the municipalities studied. However, inequality does seem to increase when comparing the treated municipalities to the municipalities in Sweden which have never had an independent school.

Remaining parts of the introduction will further detail the aim of the paper, its contribution and highlight its limitations. Section 2 provides background and context on the Swedish educational system. Section 3 reviews theory, previous work and presents the hypotheses. Section 4 covers the data and methodology used in the analysis. Section 5 presents the empirical results. Section 6 discusses the results, their policy implications, their generalizability, and concludes.

## 1.1 Aim, Research Question and Contribution

The aim of this paper is to understand the causal relationship between the establishment of independent schools in the lower secondary school and societal educational efficiency outcomes. Further, the analysis is developed with the goal of understanding the effects on equality. Thus, this paper will attempt to answer the research questions:

1. What is the municipal level aggregate effect on student performance in a municipality after the establishment of an independent school?

2. What is the municipal level aggregate effect on equality, after the establishment of an independent school?

Firstly, this paper contributes to the existing literature on independent schools by employing the synthetic control method, which allows for identification of effects by constructing counterfactual scenarios. To our knowledge, this approach has not been used to study the effects of independent schools on educational outcomes in previous work. Secondly, it sheds light on the heterogeneous experiences of independent schools by evaluating municipalities in isolation from each other. Thirdly, it considers not only the independent school's direct impact on efficiency and equality, but also consider externalities and spillovers (indirect effects) on municipal schools in the municipalities, which was one of the intended effects of the reforms.

## 1.2 Limitations of the Analysis

This paper does not uncover effects on an individual level and consequently, the scope of the analysis is limited to aggregate effects on student achievement within the municipality. Thus, the results are not applicable to individual students and should rather be used for policy evaluation of the educational system.

Moreover, because it studies particular municipalities, and since municipalities are structurally different, there are likely heterogeneous treatment effects, and hence limitations to external validity. With this in mind, the analysis is run on as many municipalities as possible, given the constraints imposed by data availability. From analysis of multiple municipalities, an understanding of the general trend can be formed.

Further, the setting of this paper is the lower secondary school and not the high school level, and as a consequence, only effects on the lower secondary level are evaluated. This is due to fundamental differences in sorting between lower secondary schools and high schools. For instance, when admitted to high school, selection is based on grades rather than time spent in school queues. The individual student is also more active in the high school choice, while at the lower grades parents are usually deciding. There is also greater heterogeneity between high schools than there is between lower secondary schools, as schools are allowed to specialize in particular tracks.

# 2 Background

## 2.1 The Reforms

In the late 1980s and the early 1990s the Swedish educational system underwent a period of rapid reform. Among other reforms in the curriculum, grading systems and teacher's education, the industrial organization of Sweden's educational system was comprehensively transformed (Gustafsson et al., 2016). Three propositions on decentralization, independent schools and free school choice were integral for this change and they remain substantially in effect today. These are presented below in chronological order of their implementation.

The decentralization reform shifted the overall responsibility for the execution of education in both compulsory and high school from the national government to municipalities. The municipalities were given extensive freedom to organize education as they saw fit. The national government's responsibility was to establish goals and ensure a system for quality control (Prop, 1990/91:18). The independent school reform allowed privately operated schools to receive public funding and compete on equal terms with the municipal schools. Both types were financed by a per student voucher, based on the average cost of education in the municipality. Independent schools were obliged to participate in the national program for school evaluation and were later required to follow the national curriculum (Prop, 1991/92:95). Furthermore, the free school choice reform allowed families to not only chose an independent school but also to choose freely among municipal schools in all municipalities (Prop, 1992/93:230).

This transformation can be regarded through a historical and international lens. Before the period of rapid reforms, the Swedish educational system was among the most centralized and tightly regulated systems in the developed world. After the reforms, the Swedish educational system was, and still is, considered both liberal and deregulated in an international comparison. Only Chile had enacted similar reforms, some states in the United States have allowed charter schools but under tight supervision and the UK, with a tradition of more heterogenous educational choices, has yet not opened for free competition (Gustafsson et al., 2016).

## 2.2 Regulation of the educational system

The Swedish school system is primarily regulated by the School Authority, *Skolverket*, and the School Inspectorate, *Skolinspektionen*. *Skolverket* is responsible for conducting national tests and supporting schools on educational matters. *Skolinspektionen* is responsible for ensuring that schools follow applicable laws, have an acceptable level of quality, and processes applications to establish independent schools (SFS 2010:800).

At the compulsory level schools are obligated to admit students if they have spare capacity. For municipal schools, students should primarily be admitted based on geographical proximity to the school, thereafter most municipalities admit students with siblings in the school and lastly parents' wishes are considered. For independent schools, no such rule exists, and they are free to choose among the grounds given in the school law on which to prioritize among prospective students, for example sibling priority or que time. The school choice is designed as opt-in, meaning students are placed in their closest municipal school if the parents do not actively choose (SFS 2010:800).

The overarching goal within the school system is regulated in the school law. It states that the school should provide its students with knowledge and democratic values. Schools also have a role to, together with parents, facilitate students' personal development. The goals should be met while taking students individual needs and abilities into consideration, implying that teaching methods should be adjusted to student needs and the schools should compensate for differences in students' background (SFS 2010:800).

# 3 Theory, Previous Research & Hypothesis

## 3.1 Theory

In this section, the theoretical predictions of independent schools under perfectly functioning markets will be reviewed. The predictions will then be discussed in the light of several market failures. Suppose that all families are maximizing their utility when choosing among schools and that they are actively making a choice. That is, all families rank schools on some set of educational qualities of each school. Naturally, families will enroll in the school that they prefer over all others. Initially, we will restrict families to only consider true educational quality which they for now are perfectly informed about. Further, assume that true educational quality translates directly to educational outcomes such as national test results. Thanks to the voucher system, all school choices have a private cost of zero, and thus families will only consider the utility derived from a particular school rather than make a trade-off between utility from education and other consumption. These families make up the demand side of the economy. On the supply side there are two distinct types of schools, the municipal and independent schools. Municipal schools have a political mandate to maximize the true educational quality subject to the budget allocated to them. The budget depends on how many students that particular school attracts. Independent schools, at least those who are for-profit, maximize profits subject to the education production function. Their profits are a function of the number of students they attract, as they receive a voucher payment for each student, and the quantities of educational inputs they buy.

While describing Sweden's educational system as a market is contentious, many of the arguments put forward in favor of the reforms in general and independent school reform in particular were based in micro-economic theory. Politicians argued that with the introduction of choice among diverse alternatives, schools, regardless of type, that perform poorly would eventually exit the market and that the matching between pedagogical profile and student needs would improve (Prop 1991/92:95). The first argument follows from simple micro-economic theory, under perfectly functioning markets. Relatively inefficient and unattractive municipal schools would be closed because families would switch to better schools which would lower the budget such that the bad schools cannot operate. Additionally, municipalities that aim to provide high quality education would actively seek to transfer students to better schools and close bad schools. Relatively inefficient independent schools would instead close because they would make losses, if they did not attract enough students. At the same time, new entrants to the market would have to provide higher true educational quality than the existing alternatives, as municipalities would not open a school with lower true quality and as independent schools would not enter if they cannot turn a profit, which they can do only if they attract students. This market entry behavior has been empirically confirmed for US charter schools (Ladd et al., 2017). These market forces should improve the economic efficiency of schools, that is, the educational quality provided for the same voucher amount should increase (Holmlund et al., 2014). On an organizational level, this improved economic efficiency is induced by powerful performance incentives in combination with decentralised decision making (Hanushek, 1995). To summarize, in perfectly functioning markets efficiency should unambiguously improve as schools in the municipality over time will converge to an equilibrium with better educational quality. As all families can choose better schools and are not forced into a certain school which can be good or bad, inequality might also decrease.

However, the Swedish school market does not function as politicians and theorists hoped for. Firstly, private and public preferences over educational alternatives are different, which is often why education is subsidized. The public paternalistically prioritizes true educational quality, while the private individual might appreciate other aspects of the education. These other aspects could be benefits that are substitutes for private consumption or unjustifiably high grades relative to a certain performance (Fredriksson & Vlachos, 2011). The latter, grade inflation, has been empirically confirmed in recent studies (Hinnerich & Vlachos, 2017; Edmark & Persson, 2021), but is a limited problem for national tests in mathematics (Skolinspektionen, 2019). Thus competition runs the risk of satisfying private demands rather than public demands for true educational quality (Vlachos, 2010).

Secondly, information asymmetry between schools and families is severe and it is the greatest when making school choices. This arises because families do not have a reliable comparison for school quality, as grades are unreliable, and because families are usually not pedagogically trained. Even after attending an education, families and students do not have a way to hold schools accountable for poor education, if they have realized the education they received was poor, which in of itself is unlikely (Vlachos, 2010). We theorize that these information asymmetries will shift the focus of families from true educational quality to benefits, such as handing out gym cards to students (Löfgren & Gustafsson, 2021), or other indicators of quality, for example attractive student bodies and rumours. The importance of other quality indicators are confirmed in Sweden (Dahlstedt & Fejes, 2018) and abroad (Urquiola & Hsieh, 2003). If families have a preference for true high quality education but are uninformed of school quality, then the value of information revelation is high, and parents are thus responsive to it. With more information revelation, the market through school choice could more effectively favor schools with higher educational quality (Sahlgren & Jordahl, 2016).

Thirdly, families experience high switching costs once they have chosen a school, driven by the need to adjust to new friends, teachers, and environments (Holmlund et al., 2014). The high switching costs could help inefficient schools to sustain sufficiently large student bodies and thus remain open, and prevent more efficient schools from entering the market as the perceived utility difference must be substantially higher to induce a switch. Lastly, making a choice is opt-in which means that not all families make a choice and then automatically enroll in their default municipal school. Similarly to the previous argument, this can sustain inefficient schools, and can also drive segregation along socio-economic groups, if residential segregation is significant. This is particularly detrimental for equality if a certain group is more prone to not make a choice, which is the case in Sweden where immigrants make choices to a lesser extent (OECD, 2015).

These market failures imply that the predictions under perfectly functioning markets will not hold. Rather, if families value other benefits and the marginal utility of those exceed that of spending more on true educational quality, then independent schools can attract students by offering a, from the private perspective, preferred consumption set with benefits. Hence, for independent schools, allocating some resources to benefits dominates spending everything on true educational quality. This strategy means that an independent school can offer worse true educational quality than some municipal school and still attract students and make profits. Although, independent schools could attract students from the municipal schools with the lowest true educational quality, and hence the educational outcome for those students increases. Moreover, independent schools do not have an incentive to provide a much higher true educational quality than the best municipal school even if it is very productive, because it could still attract students through benefits and any additional spending beyond that is deteriorating profits. Thanks to a true alternative to the municipal school the matching between student and pedagogical profile could increase, and thus student performance improves. But, as independent schools must comply with the national curriculum, pedagogical variation might not be large. This can be seen in contrast to the outcomes in the US charter schools where schools have substantial freedom as long as they are accepted by the school district (Chabrier et al., 2016). Furthermore, we theorize that existing independent schools have little incentive to invest in pedagogical innovations, because firstly, they cannot be protected from replication and secondly, high switching costs and information asymmetry imply that the rewards for pedagogical innovation are low.

The prediction on improved equality in educational outcomes is also at risk due to these market failures. Independent schools could effectively target a certain demographic by slightly adjusting its pedagogical profile and only marketing itself to educationally stronger groups. This is evident both for Swedish independent schools (Gustafsson et al., 2016) and for US charter schools (Ladd et al., 2017). Additionally, if only children from strong socio-economic groups are opting in for school choices, independent schools could effectively only attract educationally strong students. Due to peer effects this can also affect the educational outcome of the students, because lower achieving students benefit relatively more from an increase in peer achievement and are more affected by the spread in peer achievement than their higher achieving peers (Sund, 2009). If stronger students opt in to school choice, the loss of positive peer effects from these stronger students could exacerbate the performance of already weaker schools.

## 3.2 Previous Empirical Research

### 3.2.1 Efficiency of independent schools

Researchers studying the effect of the independent school reform draw conflicting conclusions. Early evidence from lower secondary school finds that students in municipalities with higher independent school attendance improved their final grades in mathematics, English and Swedish more than in municipalities with a low share. The effect, however, is small (Björklund et al., 2004). Another study finds that higher penetration of independent schools improves grades within the municipality. It concludes that most of this is attributable to indirect effects such as competition and spillovers (Böhlmark & Lindahl, 2007). In a follow up study these findings are confirmed for lower secondary school grades, high school grades, and tertiary education attainment dependent variables. In the same study, researchers dismiss alternative explanations such as differential trajectories, student background (peer) effects or grade inflation (Böhlmark & Lindahl, 2012). Furthermore, the above findings are confirmed using international mathematics assessments (Böhlmark & Lindahl, 2015). While the above estimates have been moderately positive, a study using municipal external procurement penetration as an instrument for independent school attendance penetration, finds large positive local average treatment effects[4] for independent school penetration (Sandström & Bergström, 2005). The above studies tries to capture the same effect as in this

[4]See for example Cunningham, Scott. The Mixtape, 2021. https://mixtape.scunning.com for further discussion on instrumental variables and the local average treatment effect theorem.

paper, but by using the synthetic control method this paper could further eliminate the self-selection problem associated with using between-municipal variation. Further, this paper complements the literature by uncovering the heterogeneous effects of municipalities and independent schools, rather than a nationwide effect.

Contrary to the above findings, an extension of Böhlmark & Lindahl (2015) finds that when accounting for multiple school level and individual controls, there is no effect on municipal aggregate grades from independent schools. Instead, the authors argue that the effects found in Böhlmark & Lindahl (2015) are a result of positive peer effects from student sorting (Hennerdal et al., 2020).

Researchers studying student individual performance have lately found negative effects of attending independent schools. A study using external regrading of high school national tests, concludes that the negative effect is larger for students with weak socio-economic background, who are most prone to attend independent schools, but for students with favorable background the impact is around zero (Hinnerich & Vlachos, 2017). Interestingly, for United States' charter schools the reverse is true (Chabrier et al., 2016). Another study nuances the above findings for the lower secondary school, in that only for-profit independent schools perform worse, but not non-profit. Further, it finds that competition among schools has a negative effect for students of weak socio-economic background (André et al., 2019). Finally, one study finds that independent high schools have a moderately large positive effect on teacher-assesed performance, but attribute this to teachers at independent schools being more likely to "up-grade" students. They base this claim comparing national test results to final grades (Edmark & Persson, 2021). This paper tries to tackle the issue of "up-grading" by using mathematics scores. Furthermore, indirect effects such as competition and spillovers are considered in this paper, which contrasts to the above research comparing students attending independent and municipal schools respectively.

While international studies on similar phenomena as independent schools are interesting, due to institutional differences, findings might not generalize to Sweden. In a Colombian school voucher lottery, receiving a voucher and therefore attending a private school caused higher academic achievement in the short and long run (Angrist et al., 2002, Angrist et al., 2006). A similar study about the Wisconsin voucher program, finds that private schools caused higher performance in mathematics but not reading (Rouse, 1998). Further evidence from the United States indicates that charter schools, which contrarily to private schools and similarly to Swedish independent schools are free to attend, have higher individual student performance value-added compared to public schools in both reading and math (Ladd et al., 2017). In the UK, parents could initiative a referendum on whether the school should become independent of the government or not. In a comparison between schools just above and below a majority vote, becoming independent had a moderate positive effect on student's performance (Clark, 2009). In Chile, which implemented a voucher system before Sweden, there is support for improved performance when accounting for student background, but not when controlling for school aggregates (Giaconi et al., 2022). An earlier study of Chile finds no evidence of improved performance, because the performance difference is completely driven by student sorting (Hsieh & Urquiola, 2006). Worth noting is that in Chile, schools are allowed to reject students.

### 3.2.2 Equality of independent schools

The variation in academic attainment between schools has increased since the reforms (Fredriksson & Vlachos, 2011). However, within school variation remains larger than between school variation. When controlling for socio-economic compositions of students in schools and municipalities, the decentralization and introduction of school choice has not increased the variation in academic attainment. This implies that family background has become more important for educational choices, rather than for educational outcomes. This could be because compensatory resource allocation and targeted educational efforts are working or because peer effects in education are small (Böhlmark & Holmlund, 2011). Another study dismisses effective compensatory resource allocation as an explanation but confirms that the association between parent's education, Swedish background and sibling performance and educational outcomes has not increased since the reforms (Holmlund et al., 2014). This paper will study equality considering student composition in the relevant geography, namely the municipality.

School segregation on student characteristics is primarily driven by increased residential segregation, but in areas with multiple school choices segregation is higher (Böhlmark et al., 2016). Independent schools are a mechanism through which school choices increase segregation (Holmlund et al., 2014). For example, US charter schools attract a specific demographic in terms of ethnicity and educational advantage by choosing their location, academic content, and other services such as transportation and lunches (Ladd et al., 2017). Endogenous location choices of independent schools are not a driver of segregation however (OECD, 2015), despite that schools are more likely to open in areas with higher average education, high share of immigrants and where expected profits are higher (Angelov & Edmark, 2016).

## 3.3 Hypothesis

### 3.3.1 Efficiency

Theory implies that under perfectly functioning markets, the introduction of independent schools should improve the efficiency of all schools, regardless of type. However, considering the market failures, the consequences on efficiency are ambiguous. We hypothesize that schools will compete on parameters that do not contribute to the educational goals, and therefore independent schools will not improve educational quality in a municipality. Thus, we believe that educational outcomes will not improve, but we remain agnostic about whether the net effect is zero or negative. Recent empirical research supports this hypothesis (Hennerdal et al., 2020; Hinnerich & Vlachos, 2017; André et al., 2019).

### 3.3.2 Equality

The question studied relates to whether inequality in educational outcomes conditional on student background within a municipality has increased following the opening of an independent school. The closest prior evidence is found in Böhlmark & Holmlund (2011), who find that despite increased segregation, the between school variation given the socio-economic composition of schools has not increased significantly following the reforms. This finding is implicitly confirmed by Holmlund et al. (2014) and Björklund et al. (2004), who find that the importance of family background in educational outcomes has not increased since the reforms. Theoretically, in a perfect school market, independent schools could improve equality, but the market failures imply that independent schools instead could increase inequality. Thus, although independent schools can be a driver through which school choice affects segregation, we do not expect that segregation to translate into higher inequality in true educational quality each respective student receives and in turn their academic performance.

# 4 Method and Data

## 4.1 Data

For the analysis two data sets are employed, one with municipal level data and one with school level data. The municipal level data set consists of results on national tests in mathematics, share of students with Swedish background, share of students with parents with more than high school education, share of people in the municipality with more than high school education, level of urbanization, school costs and teachers per student. Consistent data for national test scores are available from the year 2004 until year 2019, hence the period of analysis will be limited to these years. The outcome variable of interest is mathematics scores, which is measured on a scale from 0 to 20, where 0 corresponds to the grade F (fail), 10 corresponds to E, 12,5 corresponds to D, 15 corresponds to C, 17,5 corresponds to B and 20 corresponds to A. Because the number of municipalities included in the donor pool varies across the municipalities studied, the size of the data set also varies. All data in the data set is collected from publicly available sources provided by the Swedish authorities.[5]

The school level data set consists of results on national tests in mathematics, students with Swedish background, parents' educational level and share of boys. Mathematics scores is the outcome variable in this data set as well, and it is measured on the same scale as in the municipal data set. Due to constraints in the available data, the independent schools are only included in the data set from 2013. This results in a data set with observations of 413 schools in 138 municipalities over 14 time periods, corresponding to 3999 observations. All data is collected from publicly available sources provided by the Swedish authorities.

There are some missing data issues. Data for national tests are available between

[5] Please refer to table B.1 in appendix B for full list of data sources.

2004 and 2019, however results from 2018 are withdrawn as the test leaked in advance. Because the synthetic control method does not allow for missing data in the outcome variable, we use the average of 2017 and 2019 as the result for 2018, while treating it as missing in all other analyses. Furthermore, in Klippan the independent school closed in 2019, and thus we only conduct the analysis between 2004 and 2017. Älmhult has a similar problem, because in 2017 the independent school did not have a graduating class, but the school remained operational for other grades. This is ignored in the analysis, as the consequences of one year without a graduating class should not affect the dynamic in the municipality for that year, especially as the school remained operational for other grades. However, this should be taken into consideration, and the results from Älmhult must be interpreted in the light that all graduating students in 2017 attended municipal schools.

## 4.2 The efficiency analysis

For the efficiency research question, we employ the synthetic control method, following Abadie (2021), to study the development of 9th grade student mathematics performance in Swedish municipalities after the establishment of an independent lower secondary school. We define the treatment period as the first period in which a lower secondary school has registered 9th grade national tests. The aim is to construct a credible counterfactual for the development in the respective municipalities, had an independent school not opened. As an alternative to the synthetic control approach, both ordinary least squares (OLS) and Difference-In-Difference (DiD) could be considered. However, as an independent school only opens in a municipality in which it expects to be successful, independent schools are not randomly located. Because treatment is not random, the zero conditional mean assumption required for OLS is unlikely to hold. Thus, regular regressions are inappropriate for finding causal effects. DiD could, in theory, solve the problem above. But, it requires the identifying assumption of parallel trends, to yield causal estimates. Finding controls with close parallel trends for all municipalities has turned out to be in-feasible, and a linear combination of municipalities is an as good as, or better, comparison than any other single municipality (Abadie, 2021). Thus, we judge the synthetic control method to be the most appropriate to use when studying the research question at hand.

Informally, the synthetic control method works as follows. A synthetic municipality is constructed from a donor pool of other municipalities which have never had an independent lower secondary school. The donors are weighted such that the synthetic municipality as closely as possible replicates the ”treated” municipality on observed characteristics in the period before an independent school opened, or in other words when the “treatment“ occurred. A synthetic municipality which replicates the true one well in the pre-treatment period, can be expected to replicate the development in true municipality in the absence of the treatment and hence function as a counterfactual for the development in the municipality had an independent school not opened.

### 4.2.1 Formal synthetic control model

Following Abadie (2021), suppose there are J+1 municipalities for which there are data for k number of predictors over T number of periods, including $T_0$ periods before treatment. The k × 1 matrix $X_1$ collects average value of predictors for the treated municipality (j=1) over the pre-treatment period and the k × J matrix $X_0$ collects the average values of predictors for the donor pool consisting of J municipalities over the pre-treatment period. Additionally, the T × 1 matrix $Y_1$ contains the outcome variable for the treated municipality j=1 for each period and the T × J matrix $Y_0$ contains the outcome variable for the donor pool in each period. The synthetic control is constructed by choosing weights $w_j$ for each municipality in the donor pool, collected in the J × 1 matrix $W$ such that the norm (1) is minimized over the pre-treatment periods (Abadie, 2021).

$$\|X_1 - WX_0\| = \sqrt{[\sum_{h=1}^{k} v_h(X_{h,1} - w_2 X_{h,2} - ... - w_{j+1} X_{h,J+1})^2]} \qquad (1)$$

The donor weights $W$ are restricted such that they are within the interval [0,1] and sum to one, thus the treated municipality must lie within the convex hull of the donor pool. $v_h$ are positive constraints that should reflect the relative importance of each predictor in the choice of weights that minimizes (1). In a data-driven approach, weights $v_h$ are chosen such that $w_j(V)$ minimizes the mean square prediction error (2) over the pre-treatment period (Abadie, 2021).

$$\sum_{t \in T_0} (Y_{1,t} - w_2(V)Y_{2,t} - ... - w_{J+1}(V)Y_{J+1,t})^2 \tag{2}$$

Once optimal weights $W^*$ are found, the estimated treatment effect in a certain period $t > T_0$ is:

$$\hat{\tau_{1t}} = Y_1 t - \sum_{j=2}^{J+1} w_j^* Y_{jt} \tag{3}$$

### 4.2.2 Methodological considerations

Measuring educational performance in relation to competition and school choice over time and between schools is not trivial, as both test scores and grade point average can be subject to bias from teachers and schools' interest to perform well. Test scores and grade point average are, despite the risks, the most common choice when evaluating aggregate educational results, see for example Angrist et al. (2002) and Böhlmark & Lindahl (2015). To alleviate the risk of bias, it is commonly recommended to use test scores and grades in mathematics, as grading of such tests are subject to less subjectivity. Additionally, math is one of the subjects which students mostly learn in school, making it easier to isolate the school effect from the family effects (Gustafsson et al., 2016). Therefore, test scores in the 9th grade national mathematics test serve as the outcome variable in the following analysis.

When constructing the synthetic municipalities, a long enough pre-treatment period is needed to be able to credibly identify a counterfactual. At the same time, a large enough post-treatment period is needed to be able to evaluate treatment effects over time. To our knowledge, there are no clear consensus or guidelines in the literature on the number of pre- versus post-treatment periods needed to perform the analysis, instead we have reviewed published work employing the synthetic control method and judged ten pre-treatment periods to be sufficient for fitting the model. (See for example Sadeghi & Kibler (2022), Dahlman (2019) or Olsson & Bommana (2022)). As consistent data for national tests are available from 2004 until 2019, 16 periods are left to use in the analysis. The relatively small number of total time periods available in combination with the need to have at least ten pre-treatment periods limits the number of potential municipalities to analyze. After excluding municipalities with less than ten periods between 2004 and the year the first school established in the municipality, four treated municipalities are left for which synthetic controls can be constructed, namely Hässleholm, Katrineholm, Klippan and Älmhult. The analysis of these four municipalities will serve as the main results, however, after conducting the analysis the required number of pretreatment periods will be relaxed to seven. This gives an additional set of six treated municipalities to analyze, namely Ljungby, Gislaved, Hultsfred, Mariestad, Söderhamn and Falkenberg.

The donor pool is constructed by first, selecting municipalities which have not been treated in any period, that is they have never had an independent lower secondary school. Secondly, the method requires that there is at least one observation of each predictor in the pre-treatment period and that the outcome variable is complete in the full period. Thus, municipalities that do not meet those requirements are dropped. Lastly, neighboring municipalities to the treated unit are dropped, because the students in these neighboring municipalities could easily enroll in the new independent school, hence these municipalities could be considered treated at the same time.

Due to the limited number of pre-treatment periods, the data-driven selection of predictors suggested in Abadie (2021) is not possible. Instead, predictors that theory and previous research have shown to affect student performance, school quality and schools' response to independent schools are included. In line with previous research, educational background of parents and Swedish background of students are important for the performance of the student (Holmlund et al., 2014). Thus share of parents with some tertiary education and share of students with Swedish background, are included. Additionally, the share of adults with at least tertiary education in the municipality is included to reflect economic composition, status of education and availability of other academic support than parents. Further, number of students per teacher is included (teacher density), as it has been shown to have positive impact on student performance (Björklund et al., 2004). The average cost per student is also used as a predictor, as it is informative of teaching conditions and is positively related to results (Holmlund et al., 2014; André et al., 2019). Lastly, the level of urbanization is included to account for the switching costs between schools.

### 4.2.3 Identification challenges

When employing the synthetic control method, the ability to identify small effect sizes, the power, is a function of the model's pre-treatment fit. This implies that the method is unable to identify effect sizes smaller than the difference between the synthetic and real municipality in the pre-treatment period (the noise). Hence, if there is a true causal effect of an independent school, but the effect size is smaller than the noise in the pre-treatment period, it will not be detected.

Grade inflation could constitute a potential threat to identification if there are systematic differences in grading between independent and municipal schools. The method compares synthetic and real outcomes within a time period, meaning that grade inflation is unproblematic if it affects independent and municipal schools similarly in each time period. If there instead are systematic differences, then this will bias the estimates. The research on differences in grade inflation is mixed, where earlier studies found no support for independent schools grading systematically different from municipal schools (Böhlmark & Lindahl, 2007; Vlachos, 2010) while later studies have indicated that independent schools tend to grade tests more leniently or "up-grade" students (Hinnerich & Vlachos, 2017, Edmark & Persson, 2021). If the latter is the case for mathematics national tests, results will be positively biased. Although the bias is expected to be small because when the Swedish Schools Inspectorate has regraded tests in mathematics, the result from the regrading has overlapped with the schools grading on around 90% of the tests (Skolinspektionen, 2019). This is most likely driven by the fact that mathematics tests demand less subjective judgment from the teacher when grading.

There is also a risk that the establishment of an independent school coincides with other events that are important for educational outcomes, and if this is the case, there is a risk that a potential treatment effect found in the analysis is not attributable to the opening of the independent school. One such issue is that independent school location choices and conservative municipal governments are correlated. Therefore, if the opening of an independent school coincides with a shift in power, the true treatment could be the shift in power, rather than the opening of the school. This concern is controlled for, and among the municipalities studied in this paper, none have a coinciding shift of power.

Further, the independent school reform coincided with the decentralization and free school choice reforms, which raises the concern of whether the effect from the independent school or a combination of all reforms is captured. But, thanks to the considerable time elapsed since the reforms and the time window the municipalities studied got independent schools, the municipalities should have reached a new post-reform steady state before the arrival of the first independent school. Thus, the effect on the actual municipality compared to the synthetic municipality should be driven by the independent school establishment, including indirect effects.

Lastly, there is a risk that the municipalities in the donor pool have in effect already been treated if another municipality nearby has an independent school. This effect is expected to have the largest impact on donors which are highly integrated with municipalities with an independent school. If such municipalities are given high weights in the construction of the synthetic control, the identifying assumption that the synthetic control is a credible counterfactual to the true municipality is threatened. To evaluate this issue, we identify the municipalities in the donor pool who share an urban area with a municipality with an independent school and find eleven municipalities. For these we check how large share of the population that lives in the shared urban area, how large share of the school children that attends a school in another municipality and how large donor weights the respective municipalities contributes with. We conclude that there are two donor municipalities which might be problematic to include in the donor pool, namely Burlöv and Eslöv. These two have a relatively high degree of integration with municipalities with independent schools and are also given substantial weights when constructing the synthetic control for some of the treated municipalities. However, after running the leave-one-out robustness test (presented in section 5.2.5.2) we find that the synthetic controls are robust to the exclusion of these municipalities and are hence confident that the inclusion of these municipalities in the donor pool does not have any major impact on the findings. Please refer to table C.1 in appendix C for an overview of all affected municipalities and their respective shares and weights.

## 4.3 The equality analysis

For the second research question, regarding equality of educational quality, we are unable to construct a satisfactory counterfactual using the synthetic control method. Instead, we employ a fixed effects regression framework to test whether between school results variability has changed. Because results do not reflect differences in the socio-economic composition of the school, the analysis is complemented with a value-added based inequality measure which isolates variation between schools from their respective student body effects.

To construct the value-added, we estimate model (4) on the full school-level sample with covariates share of students with Swedish background ($Swedish$), average educational level of parents ($Education$), and share of boys ($Boys$), in addition to time fixed effects $\phi$. Preferably one would also like to control for individual student's previous grades (Sahlgren & Jordahl, 2016), but this is not possible given the data at hand. Then results are predicted for each school and residuals are obtained, which are the value added for each school. This substantially follows the calculation of SALSA-residuals, which is the value-added measure of the School Authority (Skolverket, 2017). Afterwards, the student count weighted standard deviation of value added is calculated within each municipality and year, which we define as the inequality measure

$$Results_{it} = \beta_0 + \beta_1 Eduaction_{it} + \beta_2 Swedish_{it} + \beta_3 Boys_{it} + \phi_t + \epsilon_{it} \tag{4}$$

Firstly, it is tested whether the inequality variable and results variability are significantly different after the establishment of an independent school in each of the municipalities of interest, using a t-test. Secondly, in a fixed effects regression framework, it is tested whether the treated municipalities together have experienced rising inequality compared to all municipalities that have never had an independent school, by estimating model (5). Where $Treatment$ indicates whether the municipality is treated or not in period $t$, $TreatmentGroup$ which indicates whether the municipality is ever treated or not, $\phi$ are period fixed effects and $\gamma$ are municipality fixed effects. The later framework is more credible for identifying the effect on inequality from independent schools as it also considers the development in the control group municipalities.

$$Inequality_{it} = \beta_0 + \beta_1 Treatment_{it} + \beta_2 TreatmentGroup_i + \phi_t + \gamma_i + \epsilon_{it} \quad (5)$$

The estimates of model (5) are robust to constant differences across municipalities, common trajectories across time and, for the inequality measure, student body effects. However, differential municipal specific trajectories cannot be ruled out. There is a risk that this introduces bias to the analysis. For example, it could be the case that such differential trajectories reflect unobservable characteristics which correlate with more or less suitable environments for independent schools to locate in, and hence introduce selection bias. As hypothetical scenarios along the lines of this example cannot be ruled out, causality cannot be claimed in this section.

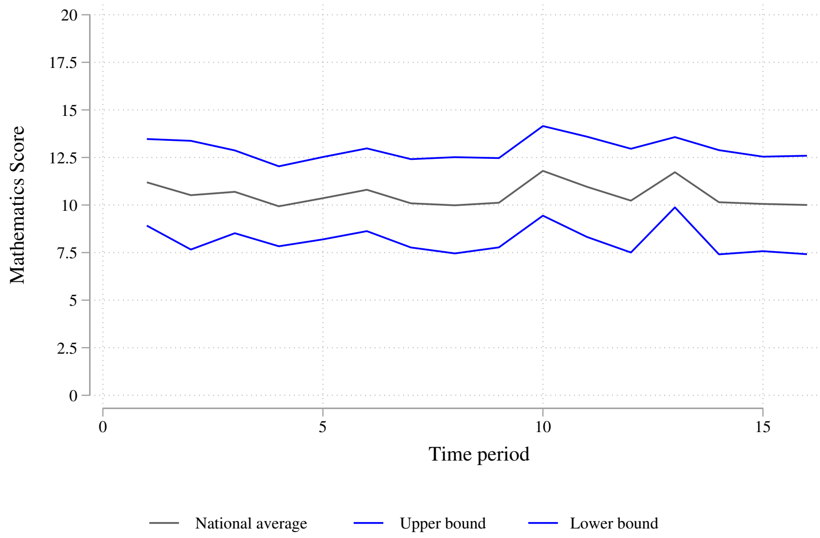
As mentioned, the school-level dataset does not contain independent schools until 2013. Thus, the analysis can only be conducted on municipalities treated afterwards, namely Hässleholm, Katrineholm, Klippan, and Älmhult. This data constraint also means that the model used to obtain the value-added residuals is regressed on a sample of municipalities with only municipal schools and the municipalities of interest.

# 5 Empirical findings

## 5.1 Descriptive statistics

In this section we present descriptive statistics on the nationwide mathematics national test results and distribution (Graph 1), share of students in independent schools over the years in Sweden (Graph 2), and the share of 9th grade students in independent schools in the treatment municipalities (Table 1). The graphs infer that the national test scores in mathematics have remained constant over the years, the share of students in independent schools have a positive trend, and the share of students in independent schools in the municipalities studied are mainly consistent with the national average, except for in Ljungby, Gislaved, and Söderhamn which have lower shares.

Graph 1: 9th grade mathematics national test scores 95% confidence interval on municipal level between 2004-2019.
Source: Data collected from Skolverket




Graph 2: Share of students in independent in the country between 1992-2021.
Source: Data collected from Skolverket

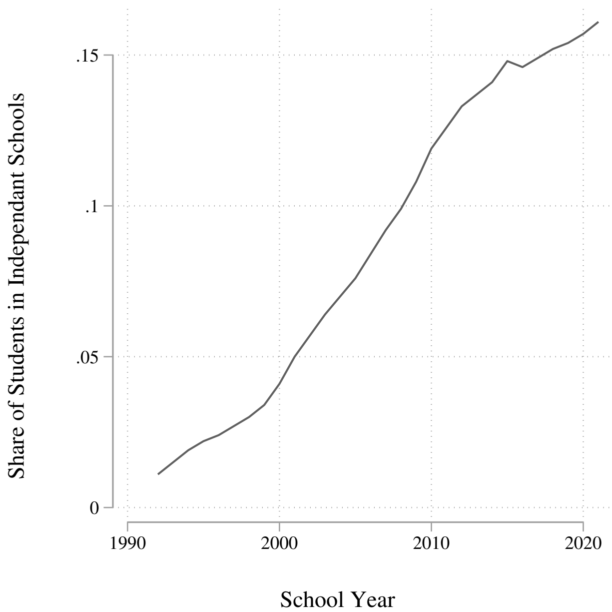


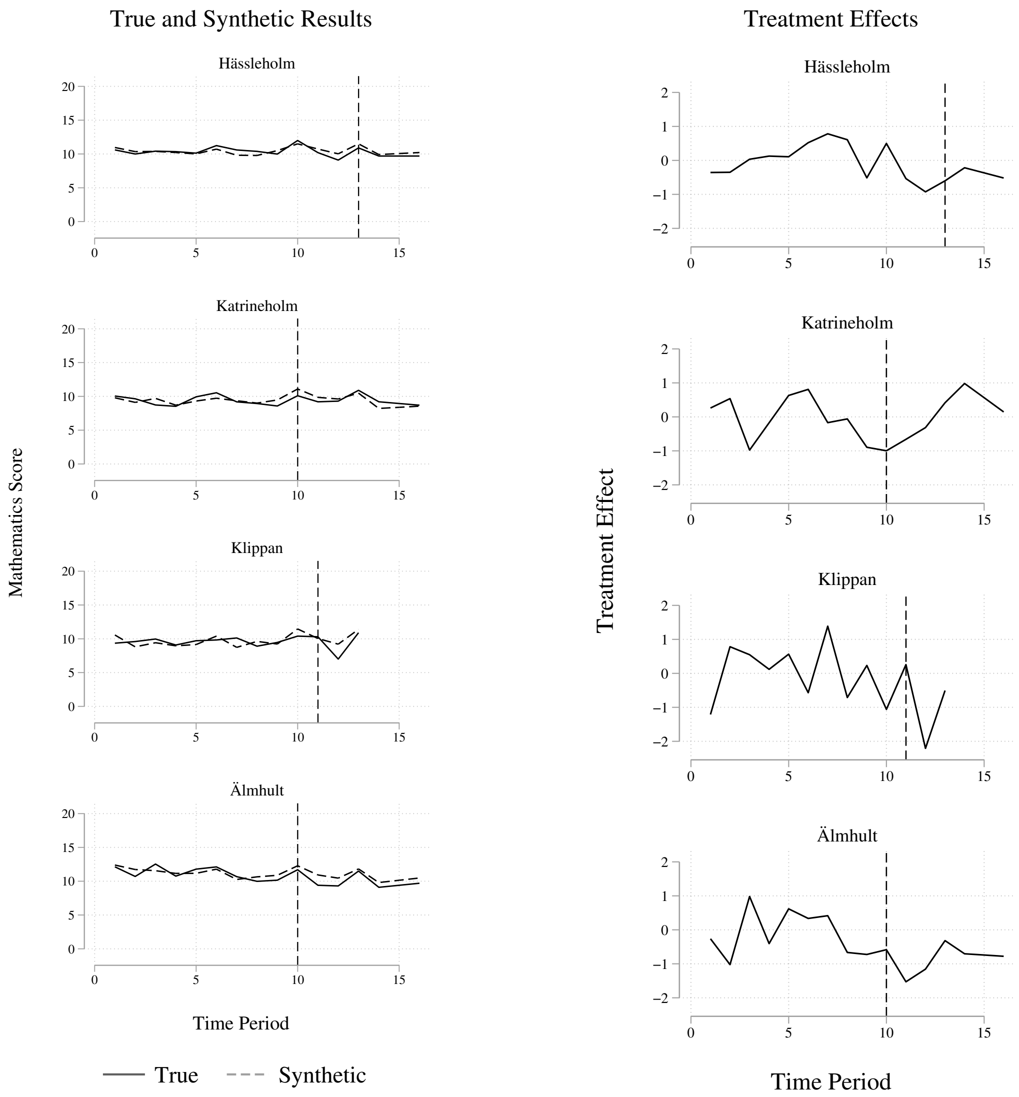
**Table 1:** Share of 9th grade students in independent and municipal schools in the studied municipalities in year 2019.
Source: Data collected from Skolverket

| Municipality | Independent Schools | Municipal Schools |
|---|---|---|
| **Hässleholm** | 12% | 88% |
| **Katrineholm** | 20% | 80% |
| **Klippan** | 14% | 86% |
| **Älmhult** | 24% | 76% |
| **Ljungby** | 11% | 89% |
| **Gislaved** | 2% | 98% |
| **Hultsfred** | 23% | 77% |
| **Mariestad** | 23% | 77% |
| **Söderhamn** | 2% | 98% |
| **Falkenberg** | 18% | 82% |

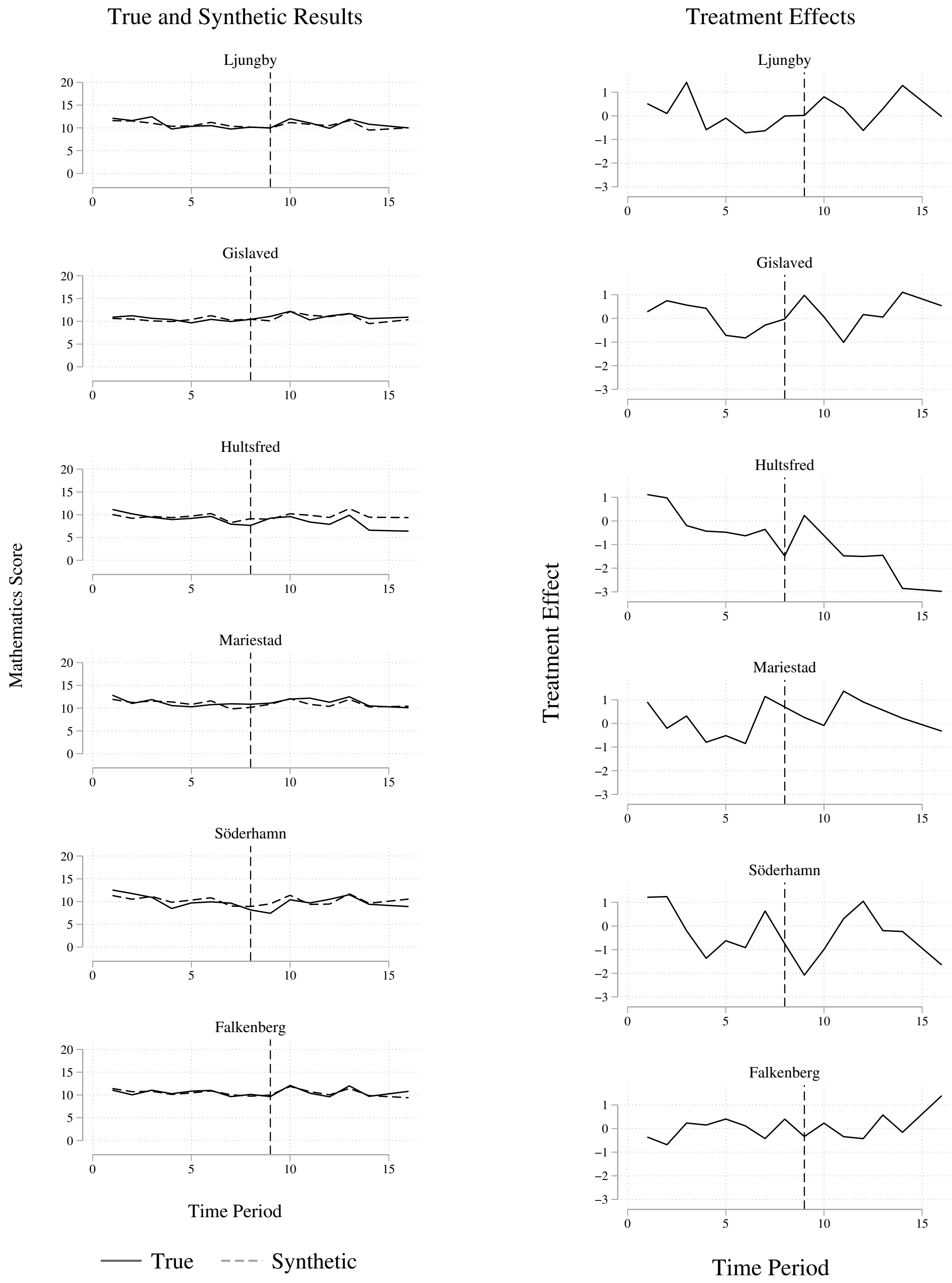
## 5.2 Empirical findings: Efficiency analysis

The results from the synthetic control model are presented in Panel 1. The left part plots the synthetic and true municipality, and the right part plots their difference, that is the treatment effect. The results are interpreted in the following sections: Firstly, the pre-treatment fit is evaluated, and the donor and predictor weights are reviewed. Then, the post-treatment results are analyzed along with tests for statistical inference. Finally, the robustness of the results is tested.

Panel 1: Main results of the synthetic control model. Left side plots the true and synthetic municipality mathematics scores and the right side plots their difference (treatment effect) between 2004-2019.



Panel 1 cont.


True and Synthetic Results
Treatment Effects
Ljungby
Gislaved
Hultsfred
Mariestad
Söderhamn
Falkenberg
Mathematics Score
Treatment Effect
Time Period
True
Synthetic

### 5.2.1 Pre-treatment fit and predictor balance

The statistical power to detect a potential treatment effect is directly related to how well the synthetic municipality approximates the true municipality in the pre-treatment period. The smaller the deviation in the pre-treatment period, the larger is the power to detect a small treatment effect. Informally the "fit" can be evaluated by graphically comparing the trajectories of the synthetic and true municipality. For most municipalities the trajectories are close, but for Hultsfred the synthetic control does not seem to fully capture a negative trend in the pre-treatment period. Formally, how closely the synthetic municipality resembles the true municipality is evaluated using the root mean square prediction error (RMSPE), which can be interpreted as the deviation in results between the synthetic and real municipality. The RMSPE for the treated municipalities are presented in Table 2 and ranges between about 0.39 to 0.97 points, corresponding to a percentage deviation of approximately 3-9%.

**Table 2:** Pre-Treatment RMSPE for the municipalities studied

| **Municipality** | **Hässleholm** | **Katrineholm** | **Klippan** | **Älmhult** | **Ljungby** |
|---|---|---|---|---|---|
| **RMSPE** | 0.52 | 0.60 | 0.82 | 0.65 | 0.67 |
| **Municipality** | **Gislaved** | **Hultsfred** | **Mariestad** | **Söderhamn** | **Falkenberg** |
| **RMSPE** | 0.59 | 0.67 | 0.75 | 0.97 | 0.39 |

The above analysis is complemented by an evaluation of the predictor balance, that is comparing the predictor means of the synthetic and true municipality over the pre-treatment period. The predictor balance is satisfactory for most predictors and municipalities, as presented in Table 3. Close predictor balance combined with a relatively small RMSPE, increases confidence in the identifying assumption that the synthetic municipality is a credible counterfactual to the true one, given that they are similar in both outcome and underlying predictors. While the resulting power of these tests are ultimately determined by the in-space-placebo test, the balance and trajectory fit are close enough to consider the synthetic municipality a credible approximation of the treated municipality. In the next section the building blocks underlying these synthetic controls are analyzed, that is the donor and predictor weights.

**Table 3:** Prediction and Outcome balance. The table presents the pre-treatment averages for the predictors and outcome variable for the true and synthetic municipality for each municipality studied.

| **Variables** | **Hässleholm** | | **Katrineholm** | | **Klippan** | | **Älmhult** | | **Ljungby** | |
|---|---|---|---|---|---|---|---|---|---|---|
| | **Tr.** | **Syn.** | **Tr.** | **Syn.** | **Tr.** | **Syn.** | **Tr.** | **Syn.** | **Tr.** | **Syn.** |
| **Mathematics Score** | 10.4 | 10.4 | 9.4 | 9.4 | 9.6 | 9.6 | 11.2 | 11.3 | 10.8 | 10.8 |
| **Swedish Background** | 78.7 | 78.6 | 75.8 | 76.4 | 80.5 | 80.4 | 79.8 | 80.4 | 87.6 | 87.7 |
| **Education Parents** | 42.3 | 42.3 | 41.9 | 41.8 | 30.4 | 30.5 | 40.6 | 40.9 | 41.4 | 41.4 |
| **Education Municipality** | 17.4 | 17.4 | 15.3 | 15.3 | 12.7 | 12.7 | 16.2 | 16.3 | 15.2 | 15.2 |
| **Urbanization** | 79.1 | 79.1 | 81.5 | 81.4 | 74.2 | 73.6 | 69.3 | 69.9 | 70.6 | 71.5 |
| **Cost per Student** | 83755 | 83751 | 86186 | 85373 | 79203 | 79284 | 81157 | 81819 | 78180 | 78253 |
| **Teacher Density** | 11.3 | 11.3 | 11.4 | 11.4 | 11.8 | 11.8 | 11.7 | 11.8 | 11.5 | 11.6 |

| **Variables** | **Gislaved** | | **Hultsfred** | | **Mariestad** | | **Söderhamn** | | **Falkenberg** | |
|---|---|---|---|---|---|---|---|---|---|---|
| | **Tr.** | **Syn.** | **Tr.** | **Syn.** | **Tr.** | **Syn.** | **Tr.** | **Syn.** | **Tr.** | **Syn.** |
| **Mathematics Score** | 10.5 | 10.4 | 9.5 | 9.5 | 11.2 | 11.2 | 10.4 | 10.4 | 10.5 | 10.5 |
| **Swedish Background** | 79.6 | 84.9 | 90.4 | 90.5 | 92.5 | 92.2 | 94.3 | 93.9 | 87.3 | 87.5 |
| **Education Parents** | 31.2 | 33.4 | 30.7 | 30.6 | 39.0 | 39.2 | 35.6 | 35.6 | 36.7 | 36.8 |
| **Education Municipality** | 11.3 | 12.8 | 11.2 | 11.3 | 15.4 | 15.4 | 13.1 | 13.1 | 14.6 | 14.6 |
| **Urbanization** | 80.8 | 78.6 | 81.1 | 79.9 | 75.8 | 74.6 | 76.4 | 75.8 | 73.7 | 73.8 |
| **Cost per Student** | 79809 | 79831 | 75100 | 76010 | 71293 | 72797 | 73350 | 73443 | 75177 | 75334 |
| **Teacher Density** | 11.9 | 11.4 | 11.8 | 11.9 | 12.2 | 12.2 | 12.8 | 12.7 | 12.5 | 12.5 |

### 5.2.2 Donors and predictor weights

The weights allocated to each municipality in the donor pool, that is the W-matrix (see table C.2 in appendix), are sparse for all municipalities except for Älmhult and Falkenberg. Nevertheless, the weights for those two are concentrated to particular municipalities, which alleviates the transparency concern associated with non-sparse controls. Also, for all municipalities except Klippan the optimal synthetic control has more than four municipalities with nonzero weights, which means that results are more likely not contingent on certain municipalities. Synthetic Gislaved and Katrineholm, while constructed using multiple municipalities, have weights concentrated in Burlöv and Köping respectively. Klippan however, has only two donors, namely Sollefteå and Tjörn. Whether this concentration is influencing and biasing the results is analyzed in the leave-one-out test presented later in this paper.

The weights given to the respective predictors, the V-matrix (see Table C.3 in appendix), are in line with previous research. Mathematics score is given the largest weight, about 70-85%, followed by the share of parents with more than high school education, the educational level in the municipality, the average educational spending, the share of parents with Swedish background, urbanization and number of teachers per child. The weights given to the predictors are consistent over all municipalities, indicating that the model captures an underlying relationship between the predictors and the mathematics score.

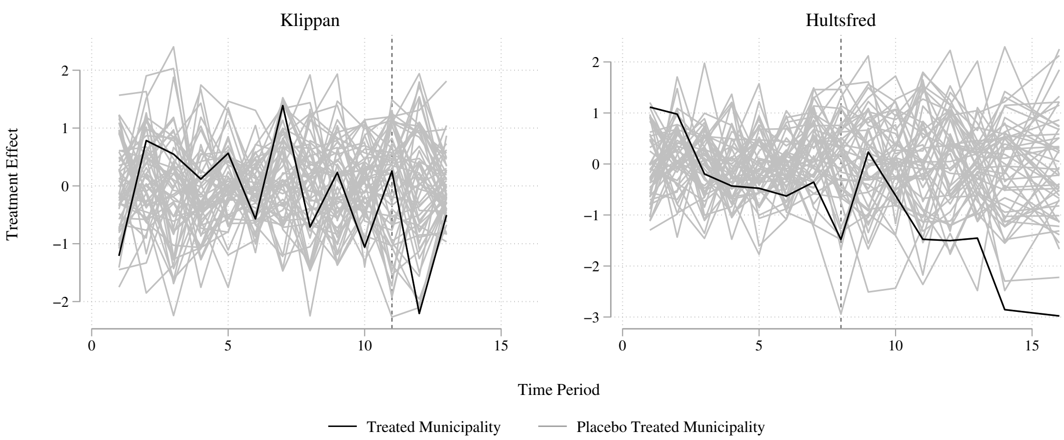
### 5.2.3 Treatment effects

In the post-treatment period, if a sufficiently large effect exists, the treated municipality is expected to deviate from the synthetic municipality. For all municipalities, except for Hultsfred and perhaps for Klippan, there are no post-treatment effects distinguishable from noise in the pre-treatment period. This is the most important result of this analysis. Hultsfred exhibits persistent negative treatment effects, with a decrease of 1,5 to 3 grade points. Klippan exhibits a decrease of 2 grade points in one period. Since the post-treatment period is limited, these results should be interpreted as short to medium term effects rather than long term effects. Next, the statistical significance of these results is tested.

### 5.2.4 Statistical inference

To perform statistical inference, the in-space-placebo test is executed by falsely assigning treatment to each of municipalities in the donor pool, constructing a synthetic control, and obtaining the treatment effect for each placebo-treatment. If a treatment effect exists, the true treated municipality should diverge in the post-treatment period relative to the placebo-treated municipalities. The result of the analysis is found in Graph 3 for Hultsfred and Klippan, and in panel C.1 in the appendix for the other municipalities. There are no divergences in the treated municipalities compared to the placebo treated municipalities, except for Hultsfred. Thus, all municipalities, including Klippan, except for Hultsfred, are null results. These test-results can be translated to a value analogous to conventional p-values, which is performed in the next section.

Graph 3: In-Space-Placebo analysis of Klippan and Hultsfred. The graphs plot the treatment effects for the truly treated municipality and the placebo-treated donor pool municipalities between 2004-2019. The other municipalities can be found in panel C.1 in appendix C.




From the in-space-placebo results the root mean squared prediction error (RMSPE) distributions are obtained (presented in panel C.2 in appendix C). The distribution is constructed by calculating the ratio between the pre- and post-treatment RMSPE and ranking the ratios in descending order. Following Abadie (2021), we also exclude placebo-treated municipalities that have a pre-treatment RMSPE $\sqrt{2}$

times larger than the true synthetic control, to not mechanically inflate the distribution with controls with low RMSPE-ratios. A true treatment effect shall be supported by a high ratio relative to that of the placebo treated municipalities, that is the ratio should be extreme relative to the distribution. From the distribution, a value analog to a p-value can be constructed as follows.

$$p = \frac{1}{J+1} \sum_{j=1}^{J+1} I_+(r_j - r_1) \tag{6}$$

Where J is the number of placebo treated municipalities, $r$ is the RMSPE ratio, $I_+$ is a function which equals one if its argument is nonnegative and zero otherwise. The analogous p-values can be found in Table 4. Hultsfred is the only municipality near any conventional significance level, with a p-value of 6%. All other municipalities have null results. However, this result is still to be subject to further robustness checks in the next section, and we are hence cautious not to draw any definitive conclusions from the result at this stage.

**Table 4:** P-value analogs for the municipalities studied.

| **Municipality** | **P-value** |
|---|---|
| Falkenberg | 69% |
| Söderhamn | 67% |
| Mariestad | 85% |
| Hultsfred | 6% |
| Gislaved | 63% |
| Ljungby | 68% |
| Älmhult | 33% |
| Klippan | 20% |
| Katrineholm | 55% |
| Hässleholm | 60% |

### 5.2.5 Robustness tests

Following Abadie (2021) we use two statistical robustness tests to asses the validity of the synthetic controls, namely the "In-time-placebo" and the "Leave-one-out". The tests are presented for all municipalities in appendix C.

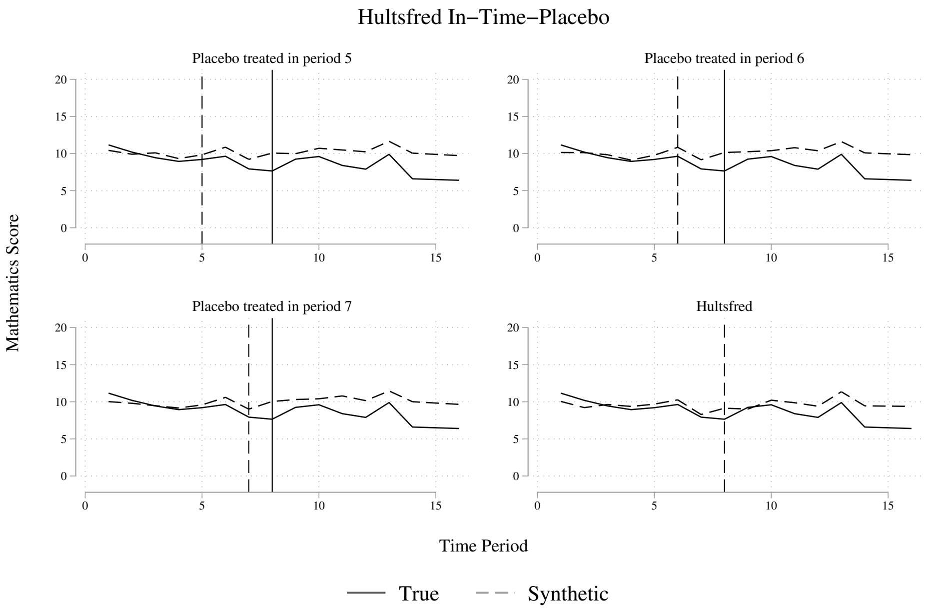
#### 5.2.5.1 In time placebo test

The in-time-placebo test placebo assigns treatment to a period earlier than actual treatment. We choose to placebo treat the municipalities one, two and three time periods prior to the actual treatment to also test for alternative treatment definitions. This paper defines the treatment period as the first period in which an independent school has registered 9th grade national tests. Alternatively, the opening year of an independent lower secondary school in the municipality could be defined as treatment. If this is the case, there should be a divergence between the synthetic and treated municipality already in that year and this divergence should not occur in later placebo or true treatments. Upon inspection, there is no support for this alternative treatment definition[6].

The primary reason for doing the in-time-placebo test, however, is to ensure that a potential treatment effect is a true effect, rather than a result of overfitting the synthetic control in the pretreatment period. If the synthetic control is a good approximation of the counterfactual development in the municipality had it not received an independent school, then the synthetic control should closely follow the actual development in the municipality up until the actual treatment period, even though the model was only allowed to fit the synthetic control up until an earlier time period. The results of the tests are found in panel 2 for Hultsfred (and panel C.3 in appendix C for the rest), in which the dashed and solid vertical lines are the false and true treatment period respectively. The synthetic controls are similar regardless of the assigned treatment period, for all municipalities except for Hultsfred. Hence, overfitting is not a problem for most municipalities. For Hultsfred, the synthetic control changes and a divergence is found at placebo treatment already three years before true treatment. This implies that the treatment effect found in the previous analysis likely is a result of overfitting. This test invalidates the prior results for Hultsfred.

[6]Please refer to table C.4 in appendix for an overview of opening years

Panel 2: In-time-placebo test for Hultsfred. The graphs plot the true and synthetic outcomes for the placebo treatment years and the true treatment year between 2004-2019. The other municipalities are found in panel C.3 in appendix C.




### 5.2.5.2 Leave-one-out test

The leave one out test is performed by reiterating the synthetic control model, leaving out one municipality at the time from the donor pool. This tests whether the optimal synthetic control is overly dependent on the inclusion of a particular municipality in the donor pool. In essence, if results are overly contingent on a particular municipality, then the control is exposed to idiosyncratic shocks in that municipality, and therefore the control cannot be interpreted as a credible counterfactual. If the trajectory of the synthetic control is different from the leave-one-out controls, then the results are not robust. This is of particular interest as the synthetic control assigned large weights to few donors in the analysis of Katrineholm, Klippan and Gislaved. Results from the leave one out analysis is presented in in panel C.4 in the appendix and shows that the actual synthetic controls follow the leave-one-out controls closely for all municipalities. Thus, we are reassured that the results are not driven by the synthetic control being overly dependent on a particular municipality.

## 5.3 Empirical findings: Equality analysis

While there is no effect on the aggregate mathematics scores in the municipalities, there might be distributional consequences affecting equality. One way to approach equality is to study changes in between school results variability. However, this measure is flawed as it also reflects the evident increased school segregation on characteristics that are in turn correlated with school results. Therefore all analysis will primarily use a value-added variability measure, which this paper defines as inequality, and be complemented with results variability.

### 5.3.1 Inequality variable estimation

To construct the inequality variable, model (4) is estimated on a sample of municipalities with only municipal schools as well as the municipalities of interest. The results show that there is a negative association between the share of boys in the school and results. This is expected, as boys are less mature than girls in year 9 (Buchmann et al., 2008). Further, there is a positive association between parents' education, share of students with Swedish background and results. This is in line with research presented earlier in this paper (see for example Holmlund et al. (2014)). All the coefficient estimates are significant on a 1% level or lower. Table 5 summarizes the results of the model estimations. Inequality is then calculated as the within municipality student count weighted standard error of the residuals from the estimated model.

**Table 5:** Estimation results model (4). Boys is a variable measuring the share of boys in each school and year. Education measures the average educational level among parents in the school and ranges between 1-3. Swedish measures the share of students with Swedish background.

| **Mathematics Score** | **Coefficent Estimate** | **Standard Error** |
|---|---|---|
| Boys | -0.0128*** | (0.00324) |
| Education | 3.694*** | (0.200) |
| Swedish | 0.0129*** | (0.00286) |
| Constant | 2.930*** | (0.465) |
| | | |
| Period fixed effects | Yes | |
| Observations | 3,877 | |
| R-squared | 0.234 | |

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

### 5.3.2 Comparison within municipalities over time

Results variability and the inequality variable are presented over time for each municipality in Panel 3. Next, the averages for both measures in the pre-treatment period are compared to the post-treatment period averages. The results of this double-sided student t-test is found in Table 6. All municipalities, but Älmhult, have higher inequality and results variability in the post-treatment period, but the differences are not statistically significant on any meaningful significance level. Hence, neither have significantly increased in the respective municipalities.

Panel 3: Inequality and results variability between 2004-2019

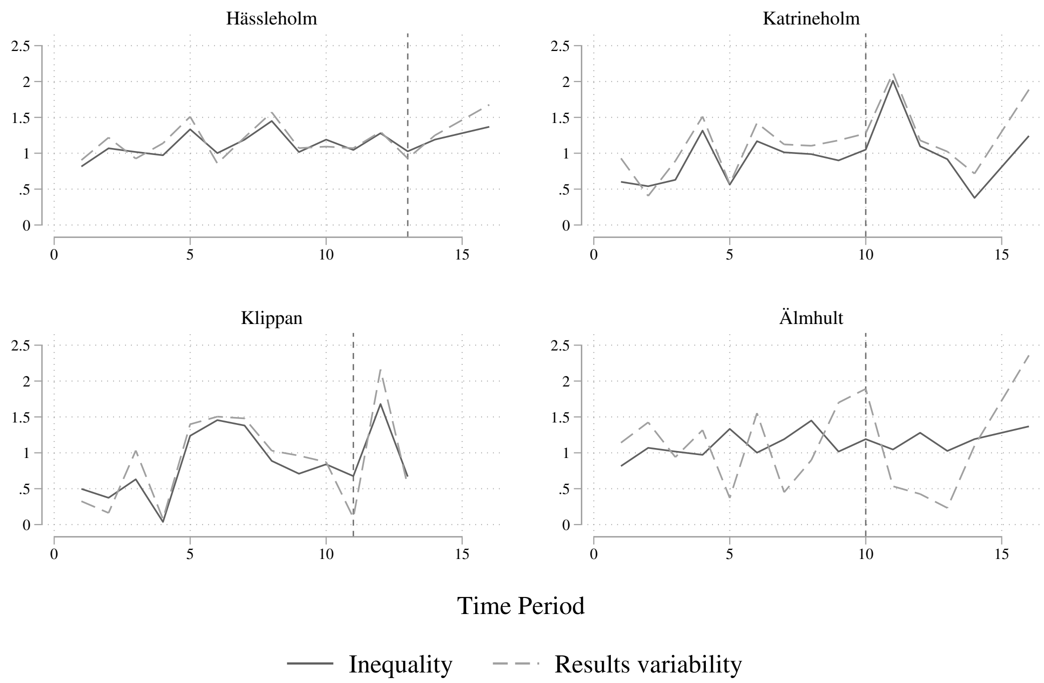



**Table 6:** Double sided t-test on inequality and results variability for equality of pre- and post-treatment averages for the municipalities studied

| | **(1) Inequality** | | **(2) Results variability** | |
|---|---|---|---|---|
| | **Difference** | **P-value** | **Difference** | **P-value** |
| **Hässleholm** | 0.08012 | 0.521 | 0.127618 | 0.6212 |
| **Katrineholm** | 0.258127 | 0.3114 | 0.351187 | 0.1955 |
| **Klippan** | 0.2034012 | 0.6195 | 0.0473682 | 0.9478 |
| **Älmhult** | 0.041139 | 0.928 | -0.00119 | 0.998 |

### 5.3.3 Comparison between municipalities within time

Finally, the four municipalities of interest are compared against the municipalities that have never had an independent school in a fixed effects regression framework. This is done by estimating model (5) for both the constructed (1) inequality measure and (2) results variability and the results are presented in Table 7. The coefficient estimate on the treatment indicator is positive for both outcome variables, moderate in magnitude and statistically significant. Thus, in this framework, the establishment of an independent school is associated with increased inequality and results variability. The different conclusion from the previous analysis could either be a result from higher statistical power to detect effects or because this framework considers the development in the control group. Further, the treatment group indicator is positive and significant on a 10% level or lower for inequality and 5% level or lower for results variability. Implying that the municipalities in the sample that received an independent school had larger variation even before the independent school opened. However, as discussed in the methodology section, selection bias cannot be ruled out in the fixed effect framework and these results should be interpreted as associations.

**Table 7:** Estimation results of model (5) on inequality and results variability. Treatment is a dummy variable indicating whether the municipality has an independent lower secondary school or not at time t. Treatment group is whether the municipality received an independent school in the sample years. Both estimations include period and municipal fixed effects.

| **Variables** | **(1) Inequality** | **(2) Results variability** |
|---|---|---|
| Treatment | 0.2047318*** | 0.1981976** |
| | (0.0722577) | (0.0946424) |
| Treatment group | 0.2047318* | 0.1639798** |
| | (0.0765179) | (0.0701133) |
| | | |
| Period fixed effects | yes | yes |
| Municipal fixed effects | yes | yes |
| Observations | 798 | 798 |
| Municipalities | 54 | 54 |
| Within R-squared | 0.0614 | 0.0523 |

Cluster robust standard errors in parenthesis

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

# 6 Discussion and Conclusion

## 6.1 Generalizability of results

The approach of this thesis has been to study individual municipalities. Primarily, to shed light on the heterogeneity among both independent schools and municipalities. This implies that the above results hold only, in a strict sense, for the ten municipalities studied. But, as the number of case studies with the same findings increases, so does the external applicability to other municipalities with the same characteristics. The results are, however, not generally applicable, because municipalities that are different from the ones studied here might experience other dynamics, and hence respond differently to treatment. For example, large municipalities could have more diversified offerings of municipal schools and the independent school benefit from improved matching between student and school is therefore not large. In this paper, we study ten municipalities, of which nine have null results and one has inconclusive results. As all results point in the same direction, our confidence in the results external validity is strengthened. We conclude that the null results in this study are indicative for at least small- to medium sized municipalities. Furthermore, these results hold for the subject of mathematics, and hence, we cannot make inference about the effect on other subjects. While mathematics is considered to be the most appropriate proxy for educational quality, the effects on performance in other subjects might be different.

## 6.2 The results in relation to previous research

The results in this paper differ in an interesting way from findings in previous research. We cannot find support for either the earlier research, finding positive effects, or the later research, finding negative effects. There are a few potential reasons for this. Firstly, as discussed in the previous section, the effects might vary between different municipalities, and the effect may be negative or positive when studying a broader set of municipalities. Secondly, previous research finding positive effects could suffer from unsolved selection biases arising when using between municipality variation, something this paper has tried to tackle by instead employing the synthetic control method. Thirdly, this paper does not consider how large a share of the student body attends independent schools, and the positive effects in earlier results might require higher penetration. Fourthly, we study the aggregate effects on a municipality, rather than the differences between independent and municipal schools as recent research finding negative effects has done. Thus, we capture the indirect effects on the school system rather than on the individuals that choose to attend an independent school. Lastly, prior papers study a wider range of subjects which, as discussed in the previous section, could lead to different conclusions.

## 6.3 Policy implications

Independent schools and whether they positively or negatively contribute to students learning, is heavily discussed in political debate. In the light of the declining results in international assessments it is popular to blame independent schools, while the other reforms, implemented around the same time, are often overlooked. The result of this paper calls for a shift of focus for educational policy makers, away from the question of ownership towards other aspects of the independent school reform and the other coinciding reforms. One hypothesis is that the theoretical predictions of improved educational outcomes from independent schools can be realized if the market failures are addressed. Specifically, reducing the information asymmetries making comparisons between schools based solely on true educational quality possible. In addition to the other reforms, further studies of the educational content, for example the grading system, curriculum and teacher's education would benefit from more attention in future research. The reason is that the Swedish national test in mathematics results have remained relatively constant, while performance in the PISA and TIMSS have deteriorated, which leads to the hypothesis that the educational content is to blame rather than the industrial organization. This is also supported in an IFAU report, which dismisses the industrial organization explanation in favor of changes to educational content (Holmlund et al., 2014).

This study finds a tentative association between the establishment of independent schools and inequality, and while the exact causal relationships remain ambiguous, the independent school reform could have increased inequality. Addressing the institutions surrounding independent schools that might increase inequality could be fruitful for policy makers aiming to improve the equality of outcomes. We suggest, with support from the theory presented in this paper, that policy makers should consider the opt-in design of school choice. One might also consider creating institutions decreasing the information asymmetry and ensuring that everyone, especially socio-economic weak groups, are given equal opportunity to make an informed school choice. Furthermore, as with efficiency, qualitative aspects of the school system might also be considered, such as the curriculum, pedagogical philosophy and teachers' education.

## 6.4 Conclusion

This paper has examined the effect of lower secondary independent schools on efficiency and equality of educational outcomes, by studying experiences in Swedish municipalities. For the efficiency analysis, among the ten municipalities studied, none of them have short to medium term treatment effects that are significantly distinguishable from noise. Therefore, we find no evidence of independent schools having an effect on aggregate educational results in the municipalities they locate in. These results imply that if policy makers want to improve academic achievement of Swedish students, they should dedicate less effort to the question of ownership, and more to educational content and the other 90's reforms. For the equality analysis, we cannot draw causal conclusions, but find a positive association between inequality and municipalities receiving an independent school. This implies, tentatively, that policy makers could consider certain aspects of the independent school reform to improve the equality of educational outcomes.

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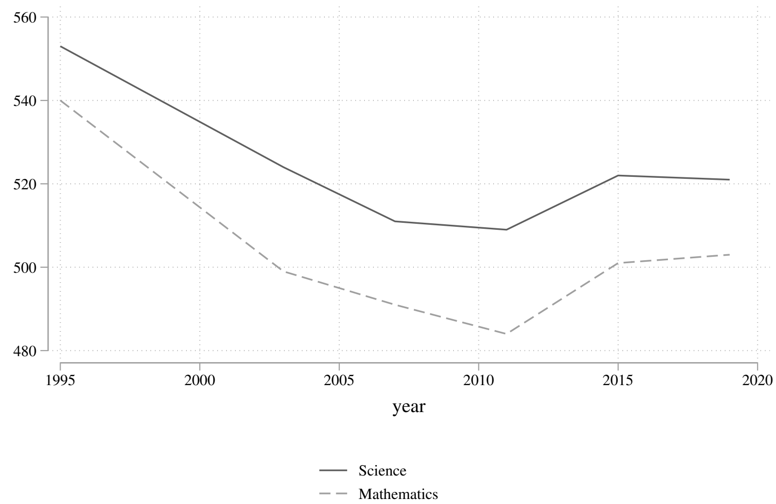
# Appendices

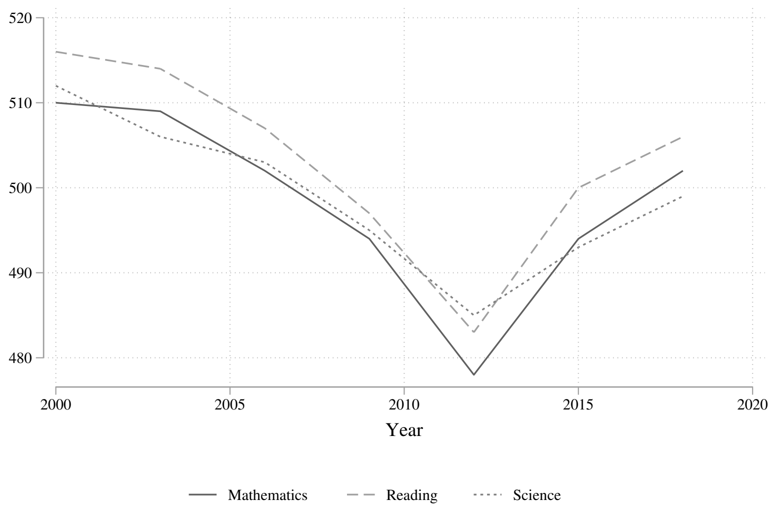
## A Swedish performance in international comparisons

As referred to in the introduction, Swedish performance in international comparisons has been declining over the years. Below we present the Swedish scores in TIMSS (scince and mathematics) and PISA (scince, mathematics and reading). Note that the results for 2018 in the PISA test should be interpreted cautiously, as a large amount of students with low proficiency in Swedish was excluded from the test.

Graph A.1: Swedish students in the 8th grade TIMSS results between 1995-2019.
Source: Skolverket (2022)



Graph A.2: Swedish students in the 8th grade PISA results between 2000-2018.
Source: Skolverket (2021)




# B Description of variables and data sources

Complementary information regarding the data set and its sources is found in the table below, stating which data set the variable belong to, a description of the variable, and the source from which it is collected.

Table B.1: Description of variables and data sources. Data set available upon request.

| **Data set** | **Variable** | **Description** | **Source** |
|---|---|---|---|
| Efficiency | Mathematics Score | Municipal average score on national tests in mathematics taken in the ninth grade, ranges from 0-20 | Skolverket |
| Efficiency | Swedish Background | Share of students in the municipality with Swedish background, meaning that both parents are born in Sweden | Skolverket |
| Efficiency | Education Parents | Share of students parents with some teritary education in the municipality | Skolverket |
| Efficiency | Education Municipality | Share of municipal population aged 25-64 with completed teritary education | Kolada |
| Efficiency | Urbanization | Share of population living in urban areas | Kolada |
| Efficiency | Cost per Student | Expenses on education in the primary school per student | Kolada |
| Efficiency | Teacher Density | Teachers per student in the primary school | Kolada |
| Equality | Mathematics Score | School average score on national tests in mathematics taken in the ninth grade, ranges from 0-20 | Skolverket/ SCB |
| Equality | Swedish | Share of students in the school with Swedish background, meaning that both parents are born in Sweden | Skolverket |
| Equality | Education | Average educational attainment of parents to students in the school, ranges between 1-3 | Skolverket |
| Equality | Boys | Share of boys among students in the school | Skolverket |

# C The efficiency analysis

Below is a table presenting the municipalities in the donor pool that is having some degree of integration with a municipality with an independent school. We present the donors, the fraction of citizens living in an urban area that is shared with a municipality with an independent school, the share of independent school commuters and the weight given to the donor in the respective synthetic controls.

Table C.1: Municipal integration with municipalities with an independent school and donor weights. The table includes donors that have a shared urban area with a municipality with an independent school. It presents the population in such shared urban areas, share of students registered in the donor municipality commuting to independent schools, and the weight it has been allocated in the synthetic controls.

| Donor | Shared Urban Area | School Commuters | Donor Weight |
|---|---|---|---|
| Östhammar | 0,66% | 3% | <1% of synthetic Ljungby<br>1% of synthetic Falkenberg |
| Vingåker | 8,01% | 13% | <1% of synthetic Falkenberg |
| Trosa | 0% | 2% | 14% of synthetic Älmhult |
| Gnosjö | 0,03% | 3% | 3% of synthetic Älmhult |
| Burlöv | 94,76% | 20% | 25% of synthetic Hässleholm<br>16% of synthetic Katrineholm<br>7% of synthetic Älmhult<br>20% of synthetic Ljungby<br>44% of synthetic Gislaved<br>2% of synthetic Falkenberg |
| Habo | 1,08% | 7% | 31% of synthetic Hässleholm<br>25% of synthetic Älmhult<br>34% of synthetic Ljungby<br>36% of synthetic Mariestad<br><1% of synthetic Falkenberg |
| Eslöv | 3,75% | 7% | 10% of synthetic Älmhult<br>4% of synthetic Mariestad<br>32% of synthetic Falkenberg |
| Svedala | 0% | 4% | 8% of synthetic Hässleholm |
| Skurup | 0,73% | 9% | 18% of synthetic Klippan<br>20% of synthetic Falkenberg |
| Gagnef | 3,99% | 2% | <1% of syntehtic Älmult<br><1% of synthetic Falkenberg |
| Krokom | 7,49% | 4% | <1% of syntehtic Älmult<br>2% of synthetic Ljungby<br><1% of synthetic Falkenberg |

The W-matix is presented below, showing the weights given to each donor in the respective synthetic municipalities.

Table C.2: Donor Weights, W-Matrix. Presents the optimal weights allocated to each municipality in the donor pool for each municipality studied in the construction of the synthetic control.

| **Donor** | **Municipality** | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | Hässleholm | Katrineholm | Klippan | Älmhult | Ljungby | Gislaved | Hultsfred | Mariestad | Söderhamn | Falkenberg |
| Alvesta | | | | | | | | | | 1% |
| Arboga | | | | 0% | | | 1% | | | 0% |
| Askersund | | | | 0% | | | | | | |
| Avesta | | | | 0% | | | | | | 0% |
| Burlöv | 25% | 16% | | 7% | 20% | 44% | | | | 2% |
| Degerfors | | 4% | 14% | | | | 35% | | | 0% |
| Emmaboda | | | | 18% | 10% | | | | | 0% |
| Eslöv | | | | 10% | | | | 4% | | 32% |
| Forshaga | | | | 0% | | | | | | 0% |
| Gagnef | | | | 0% | | | | | | 0% |
| Gnosjö | | | | 3% | | | 10% | | | 5% |
| Götene | | | | 0% | | | | | | 1% |
| Habo | 31% | | | 25% | 34% | | | 36% | | 0% |
| Hallsberg | | | | 0% | | | | | | 0% |
| Hallstahammar | | | | 0% | | | | | | 0% |
| Haparanda | | | | | | | | | | |
| Heby | | | | | | 21% | 0% | | | 0% |
| Herrljunga | | | | 0% | | | | | | 1% |
| Hällefors | | | | | | | | | | |
| Högsby | | | | 0% | | | | | | |
| Karlskoga | | | | 0% | | | | 13% | | 1% |
| Kil | | | | 0% | | | | | | 0% |
| Kramfors | | | | 0% | | | | | | 0% |
| Krokom | | | | 0% | 2% | | | | | 0% |
| Kumla | | | | 0% | | | | | | 1% |
| Köping | | 40% | | 0% | | | | | | 0% |
| Laholm | | | | 0% | | | | | | 3% |
| Lessebo | | | | 0% | | | | | | 1% |
| Lindesberg | 24% | | | | | | | | | 0% |
| Ludvika | | | | 0% | | | | | | 0% |

Table C.2 cont.

| **Donor** | | | | | **Municipality** | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | Hässleholm | Katrineholm | Klippan | Älmhult | Ljungby | Gislaved | Hultsfred | Mariestad | Söderhamn | Falkenberg |
| Lysekil | | | | 0% | | | | | | 0% |
| Malung | | | 1% | 0% | | 21% | | | 1% | 0% |
| Mellerud | | | | 0% | 30% | | | | | 9% |
| Mjölby | | | | 0% | | | | | | 1% |
| Mora | | | | 0% | | | | | | 0% |
| Mönsterås | | | | 0% | | | | | | 0% |
| Nora | | 27% | | 0% | 2% | | | | | 0% |
| Nybro | | | | 0% | | | | | | 0% |
| Nykvarn | | | | | | | | | | 1% |
| Nynäshamn | | | | 0% | | | | | | 0% |
| Oskarshamn | 4% | | | 0% | | | | | | 0% |
| Ovanåker | | | 12% | 0% | | | | | 26% | 1% |
| Perstorp | | | 26% | 0% | | | | | | |
| Piteå | | | | 0% | | | | | 4% | 0% |
| Rättvik | | | | 0% | | | | | 3% | 0% |
| Sala | | | | 0% | | | | | | 0% |
| Skurup | | | 18% | 0% | | | | | | 20% |
| Smedjebacken | 5% | 7% | 13% | | | | | | | 0% |
| Sollefteå | | 5% | | 0% | | | | | | 0% |
| Sotenäs | | | | 0% | | | | | | |
| Surahammar | | | | 0% | | | 16% | | | 0% |
| Svedala | 8% | | | 0% | | | | | | 0% |
| Tanum | | | | 20% | | | | | | 0% |
| Tibro | | | | 0% | | | 15% | 17% | 24% | 6% |
| Tidaholm | | | | 0% | | | 23% | | | 1% |
| Tierp | | | | 0% | | | | | | 1% |
| Timrå | | | | 0% | | | | | 36% | 1% |
| Tjörn | 3% | | | | | | | | | 0% |
| Tranemo | | | | 0% | | | | | | 0% |
| Trosa | | | | 14% | | | | 16% | 6% | |
| Töreboda | | | | 0% | | | | | | 1% |
| Ulricehamn | | | | 0% | | | | | | 0% |
| Vaggeryd | | | | 0% | 2% | | | | | 1% |
| Vara | | | | 0% | | 15% | | 15% | | 3% |
| Vimmerby | | | 16% | 0% | | | | | | 0% |
| Vingåker | | | | | | | | | | 0% |
| Åmål | | | | | | | | | | |
| Östhammar | | | | 0% | | | | | | 1% |

Note: The weights are presented without decimals. This results in multiple weights for Älmhult and Falkenberg being presented as zero, even though they are in reality > 0,5%. A true zero-weight is presented as a blank space.

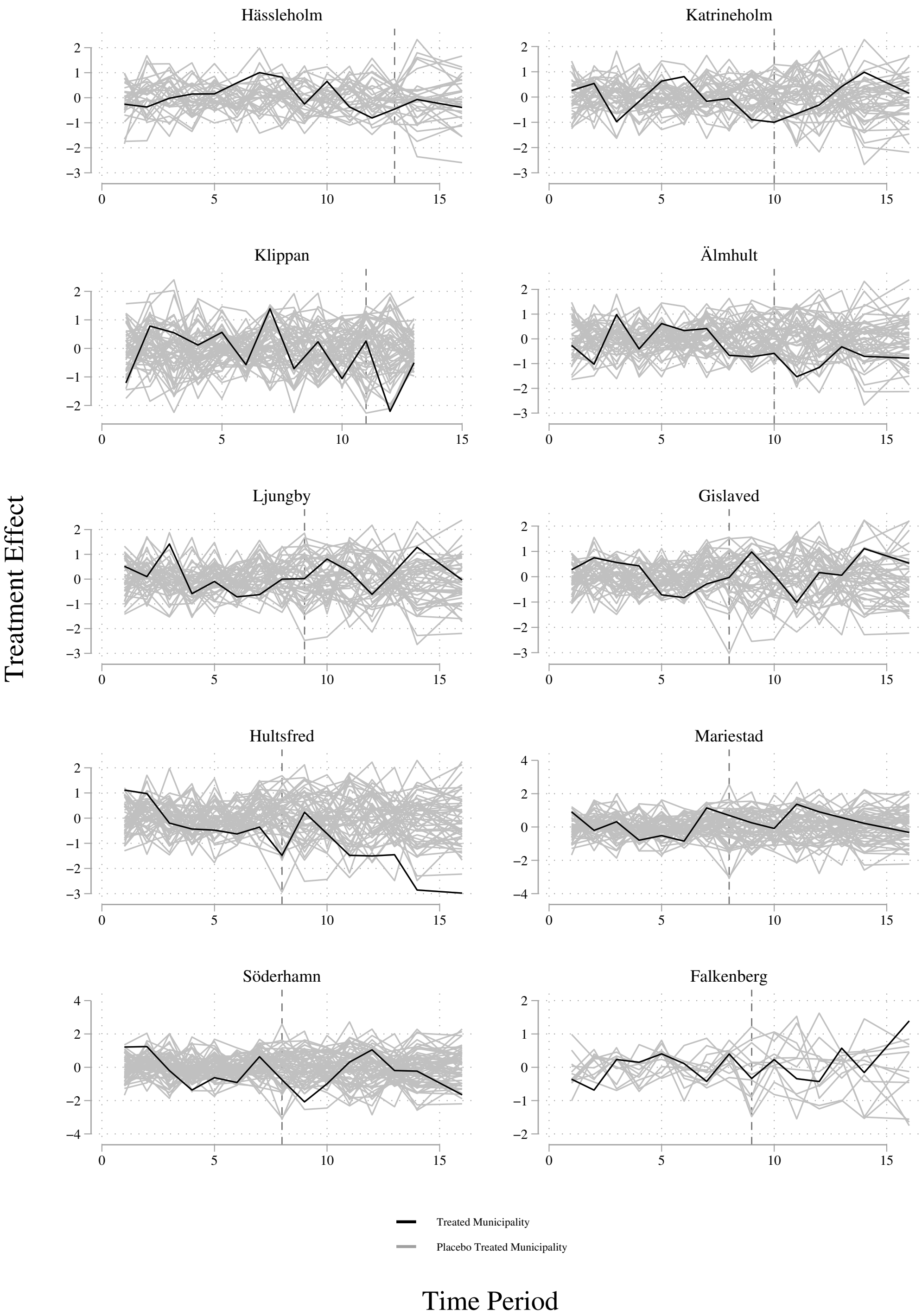
Predictor weights, or the V-matrix, is presented below. Here we present the weight given to each predictor when constructing the respective synthetic controls.

Table C.3: Predictor Weights V-Matrix. It displays the optimal predictor weights given for each municipality studied.

| Municipality | Math Score | Swedish Back. | Ed. Parents | Ed. Mun. | Urb. | Cost/Student | Teach. Dens. |
|---|---|---|---|---|---|---|---|
| **Hässleholm** | 77% | 1% | 10% | 8% | 1% | 3% | 1% |
| **Katrineholm** | 74% | 2% | 10% | 10% | 1% | 3% | 0% |
| **Klippan** | 73% | 2% | 11% | 10% | 1% | 3% | 0% |
| **Älmhult** | 74% | 2% | 10% | 10% | 1% | 3% | 0% |
| **Ljungby** | 80% | 1% | 9% | 7% | 0% | 3% | 0% |
| **Gislaved** | 84% | 2% | 6% | 5% | 1% | 1% | 0% |
| **Hultsfred** | 85% | 1% | 6% | 4% | 1% | 1% | 1% |
| **Mariestad** | 87% | 1% | 6% | 4% | 1% | 1% | 0% |
| **Söderhamn** | 85% | 1% | 6% | 5% | 1% | 1% | 0% |
| **Falkenberg** | 80% | 1% | 9% | 7% | 1% | 3% | 0% |

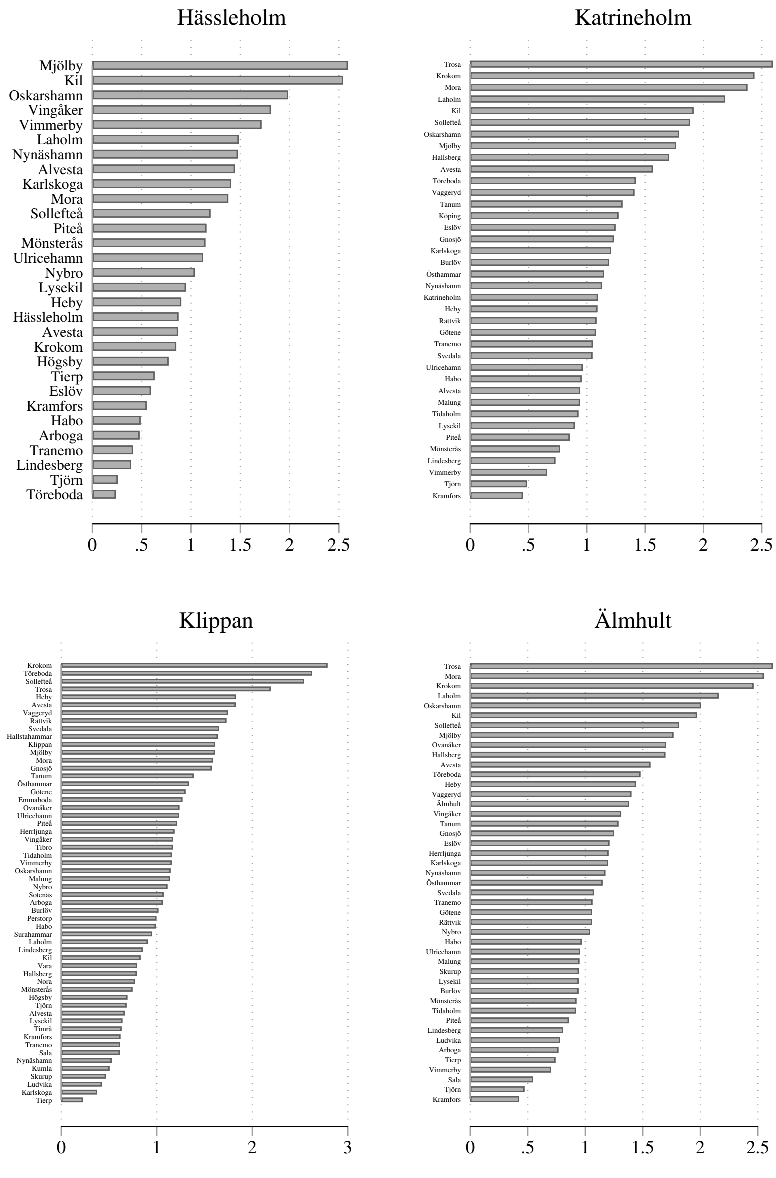
The results of the In-Space-Placebo is found below. Here we placebo-treat each municipality in the donor pool for each of the municipalities analysed.

Panel C.1: In-Space-Placebo. The graphs plot the treatment effects for the truly treated municipality and the placebo-treated donor pool municipalities between 2004-2019.

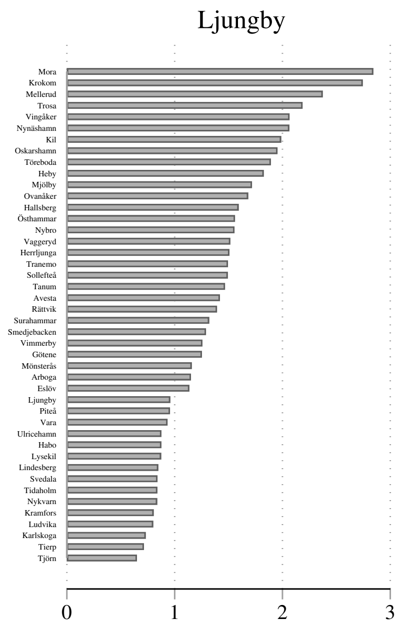


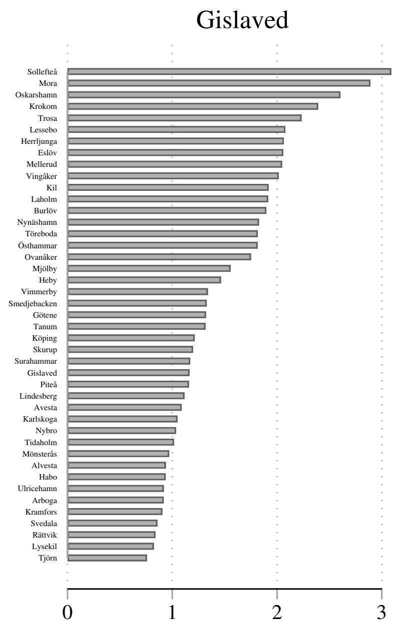
Below we present the RMSPE ratio distribution, which display the ratio between post-and pre-treatment RMSPE for each of the placebo treated donors.

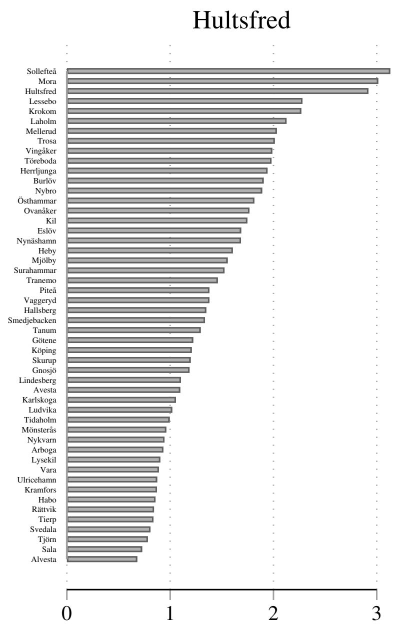
Panel C.2: RMSPE Ratio of placebo-treated donors, from the in-space-placebo test, for each municipality studied ranked in descending order

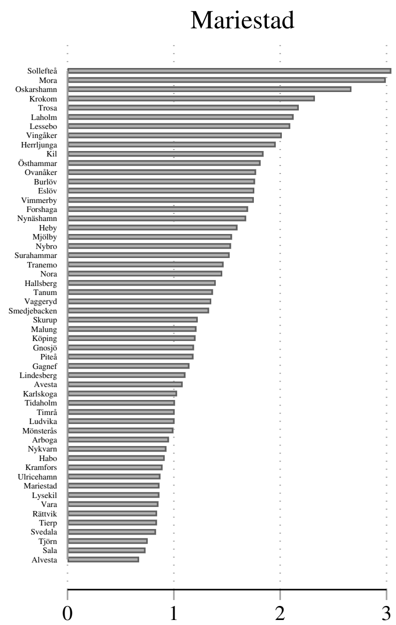


Panel C.2 cont.: RMSPE Ratio

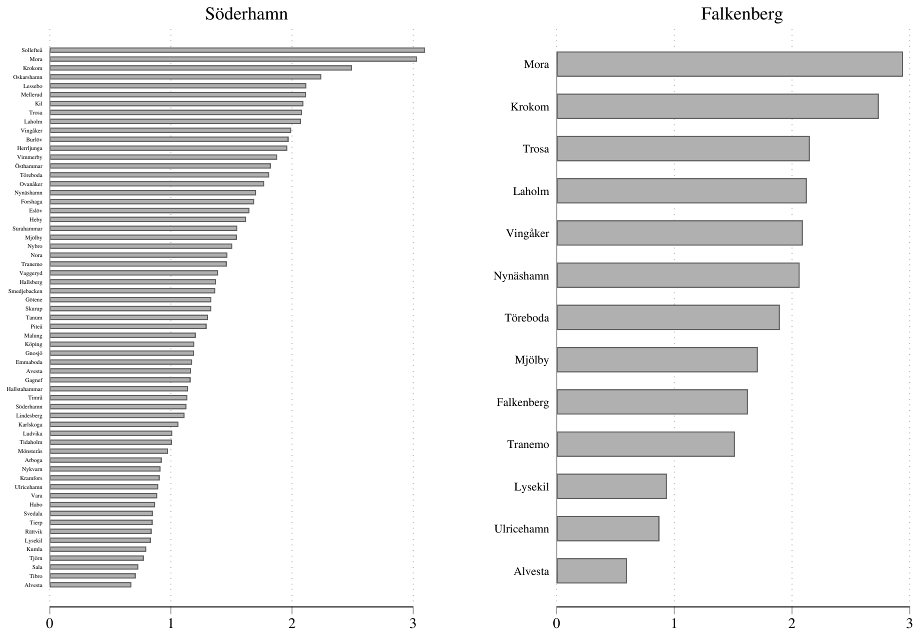


Panel C.2 cont.: RMSPE Ratio



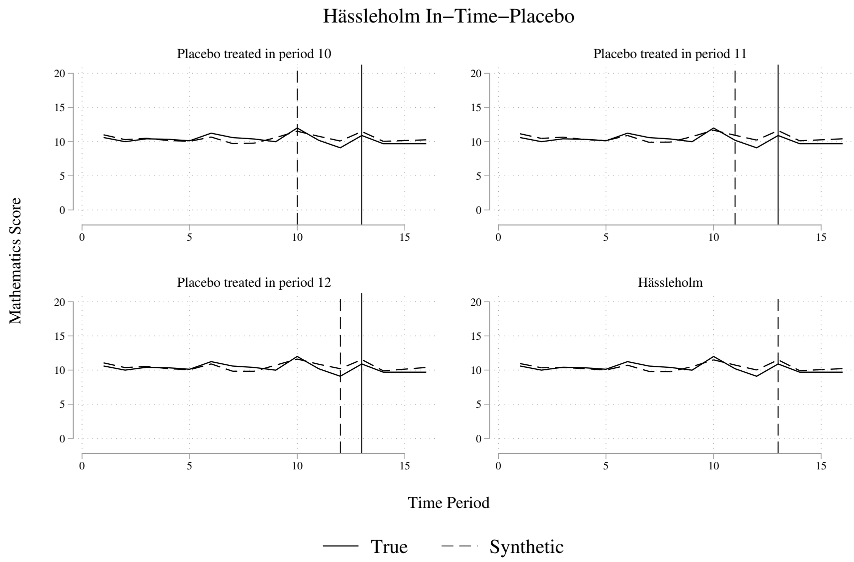

´ Below we present a table of our treatment definition and alterantive defintions and their corresponding treatment period.

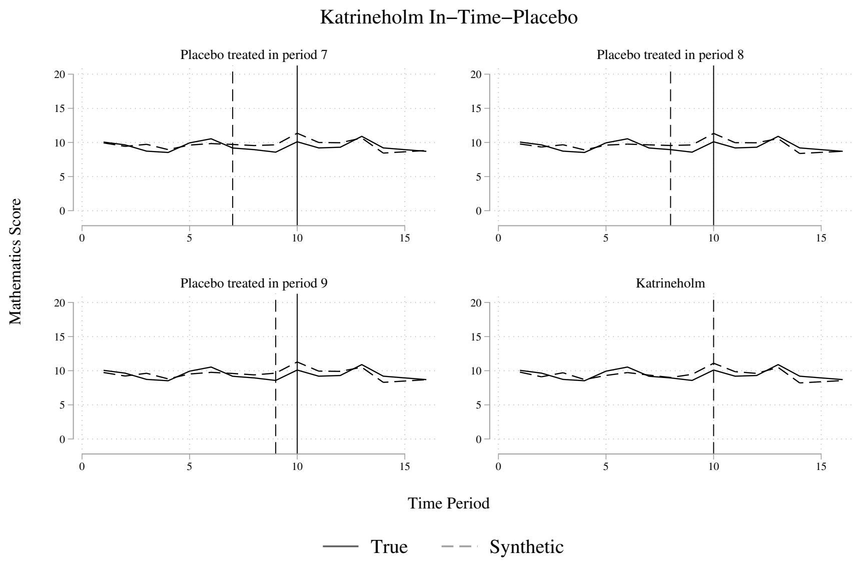
Table C.4: Treatment definition and subsequent treatment period. National test result is the main definition and Indpendent lower secondary school is the alternative definition

| **Municipality** | **Independent lower secondary school** | **National test results** |
|---|---|---|
| Hässleholm | 12 | 13 |
| Katrineholm | 9 | 10 |
| Klippan | 10 | 11 |
| Älmhult | 9 | 10 |
| Ljungby | 8 | 9 |
| Gislaved | 6 | 8 |
| Hultsfred | 8 | 8 |
| Mariestad | 8 | 8 |
| Söderhamn | 8 | 8 |
| Falkenberg | 7 | 9 |

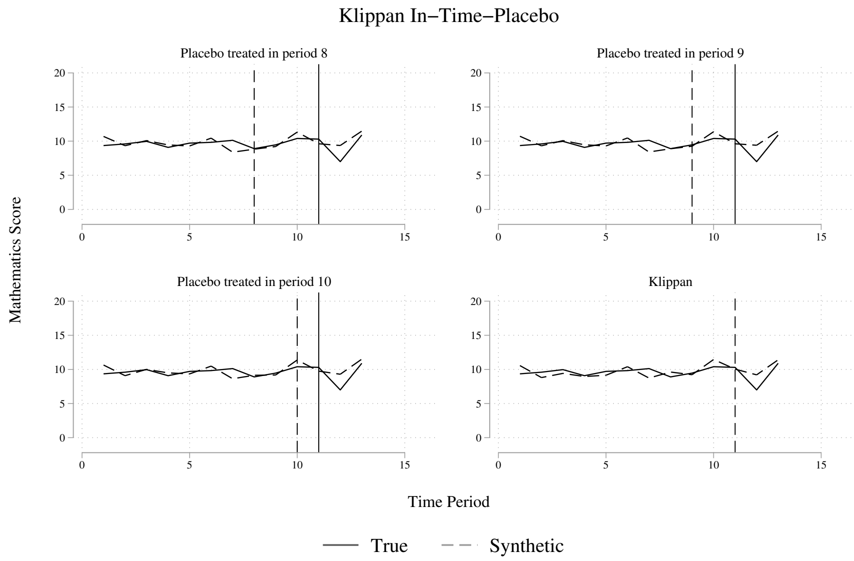
Below we present the In-Time-Placebo test, where we placebo treat the respective treated municipalities one, two and three periods before the actual treatment occurred.

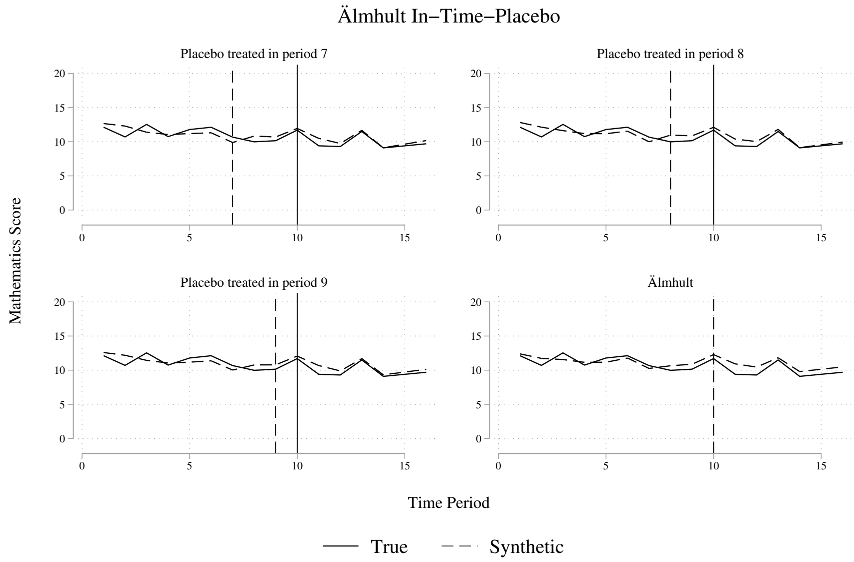
Panel C.3: In-Time-Placebo. The graphs plot the true and synthetic outcomes for the placebo treatment years and the true treatment year between 2004-2019.



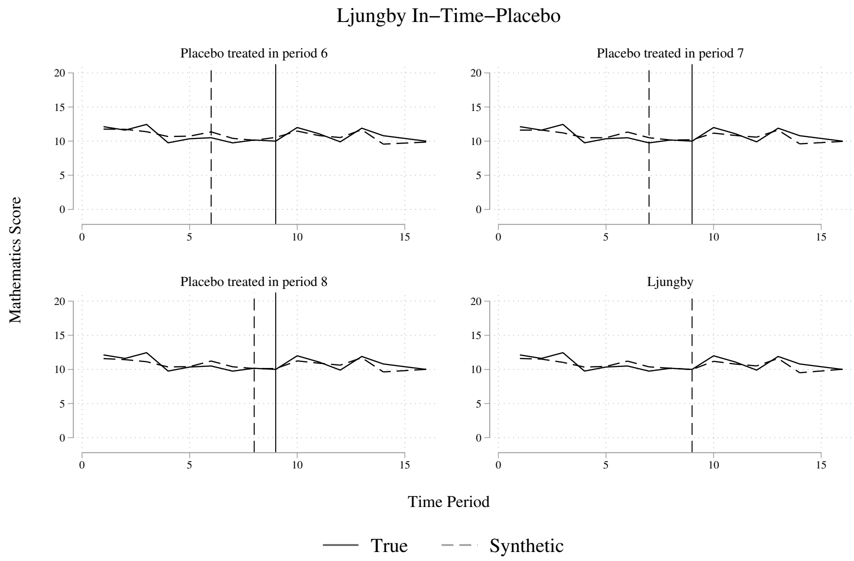



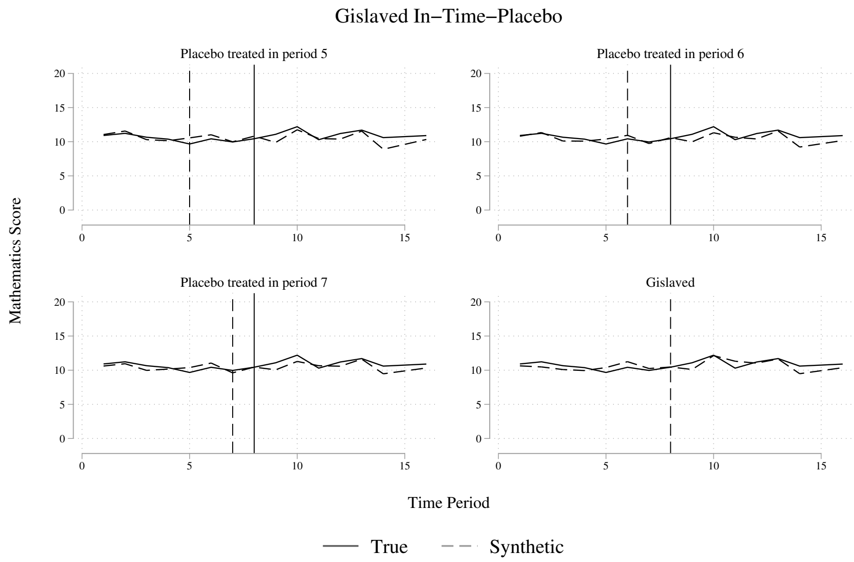
Panel C.3 cont.



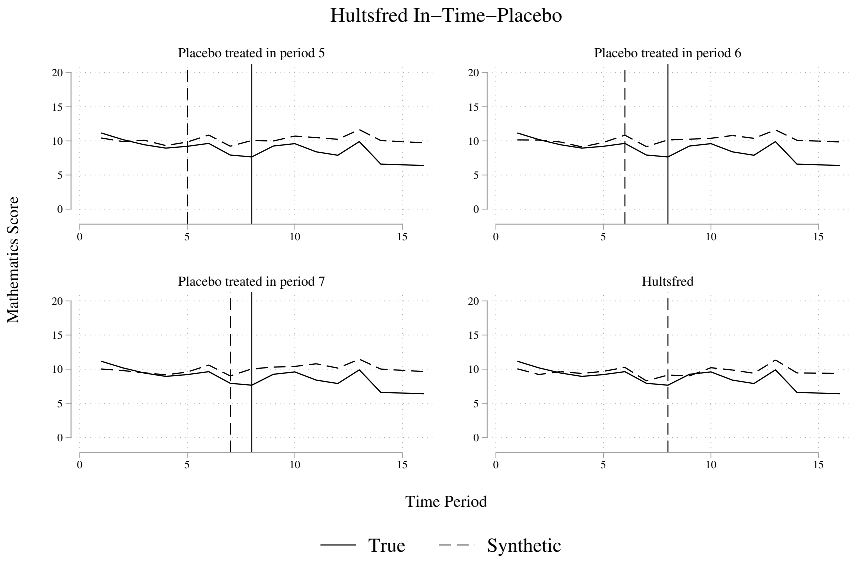



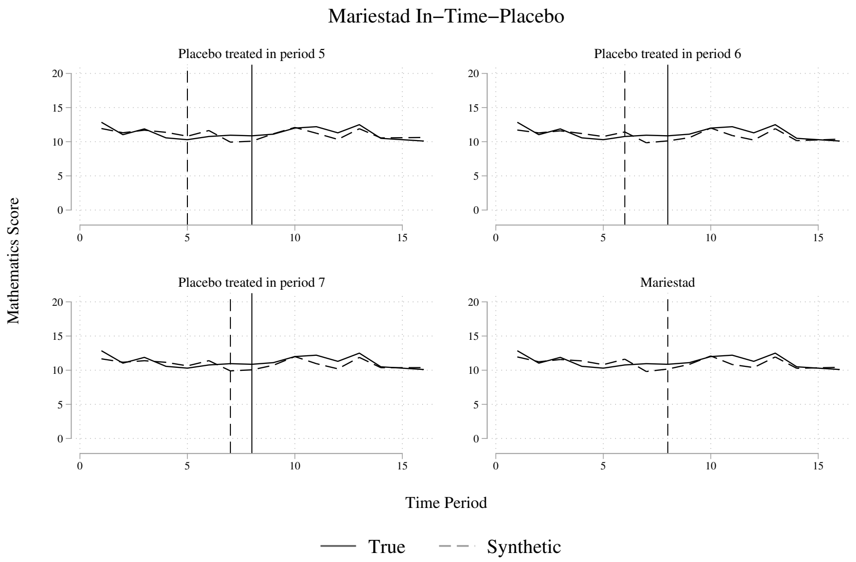
Panel C.3 cont.



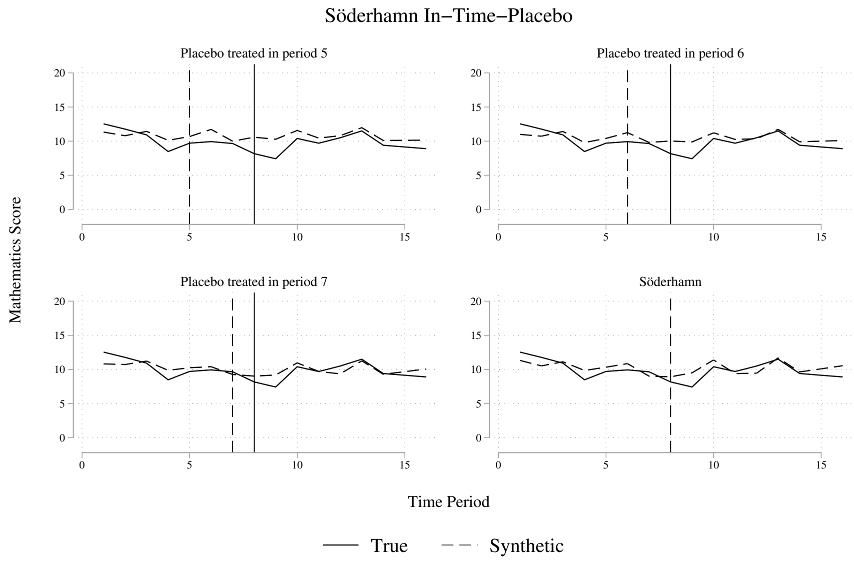



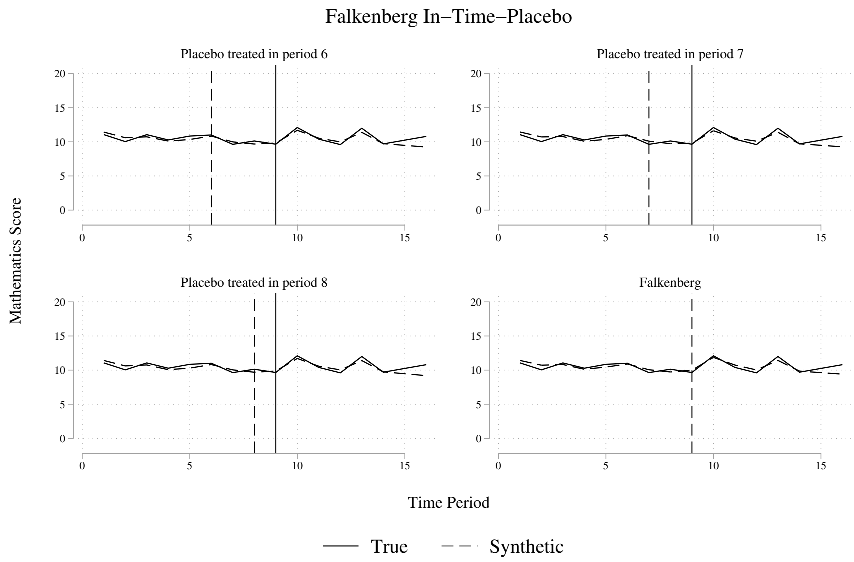
Panel C.3 cont.



Panel C.3 cont.




Below we present the leave one out test, where we re-run the synthetic control analysis while iteratively dropping one municipality from the donor pool.

Panel C.4: Leave-One-Out. The graphs display the optimal synthetic control and the leave-one-out controls between 2004-2019.