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Educational level segregation and the first year of high school

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Abstract

What is the role of bridge-year programs in educational level segregation? The Dutch education system, characterized by early stratification, has been criticized for reinforcing socioeconomic disparities. Bridge-year programs, which delay this stratification by combining students of various educational levels in the first year of high school, are proposed as a potential solution to reduce these disparities. Utilizing panel data from 162 Dutch municipalities over the period 2015-2020, this study employs pooled Ordinary Least Squares and fixed-effects regression models to analyze the relationship between the prevalence of bridge-year programs and educational level segregation. The results indicate that the pooled-OLS model suggests a reduction in segregation with an increase in bridge-year programs. However, these findings are not robust when controlling for unobserved confounding through fixed effects models. The fixed-effects analysis reveals no significant impact of bridge-year programs on educational level segregation, highlighting the influence of time-invariant unobserved confounding factors. This research underscores the need for further investigation into educational policies that promote integration and equal opportunities. Future studies should consider a broader dataset and the effects of recent policy changes promoting bridge-year programs to provide more definitive conclusions.

1 Introduction

With the prospect of the European Parliament elections of 2024, a group of 24 European education advocacy groups has called for desegregation of schools in Europe through a jointly written manifest (Scholen, n.d.). In this manifest, they argue that the segregation of schools poses a threat to the structure of our society and democracy. These groups insist that all member-states actively pursue anti-segregation policy to minimize the effect of socioeconomic status on educational performance. From the start of high-school, the Dutch educational system is highly stratified and selective. Some argue this causes harmful segregation by socioeconomic status. Therefore, some political parties and the Dutch board of education propose a broader and longer period for students to be combined in one classroom until they split into 'high' and 'low' educational levels (Onderwijsraad, 2023).

There is a Dutch term for this: brugklas, which we will loosely translate to bridge-year. The term bridge-year typically refers to the first year of secondary education in the Netherlands. During this year, students are not yet divided into specific educational tracks. The bridge-year serves as a transitional period, aiming to give students more flexibility and support before they are separated into different educational tracks, usually the following year. A more detailed explanation of bridge-years can be found in the data section. These bridge-years have, according to some, the potential to bridge different social classes and provide students with low socioeconomic status but high potential more time to develop. However, there are doubts about these policy ideas. Some argue that long and broad bridge-years conflict with parental preferences, are unrealistic to implement, and actually cause more inequality (Scheerens and Kirschner, 2021). Additionally, bridge-years can be seen as a proxy for desegregation of schools through having more high-schools that combine educational levels under one roof. Since a high-school that only houses one educational level is unable to have a bridge-year, municipalities that have more bridge-years likely have a broader school system. Therefore, it is valuable to research this question more precisely in a data-driven analysis.

To quantify the relationship between bridge-years and minimizing the effect of socioeconomic status on educational performance, we introduce a more specific dependent variable: educational level segregation. We measure this in terms of a segregation score (Central Bureau for Statistics, 2023). This score measures to what extent groups from a certain educational level live separately from other groups with a different educational level.

In this research, we pose the following question: what is the relationship between the per capita number of high-schools with a bridge-year and educational level segregation in Dutch municipalities? We answer this question by combining three panel data sets on Dutch municipalities. These panel data sets contain the educational level segregation, the number of bridge-years, and various control variables for every Dutch municipality over the years 2015 to 2020. We analyse the relationship between these variables by performing a pooled-OLS and a time and municipality fixed effects regression on the panel data.

We start by reviewing previous literature on the subject of educational segregation and equality of opportunity, to which we aim to contribute. In this literature review, we motivate the relevance of our research, define key subjects used in our analysis, explore possible control variables that should be included and compare the Dutch situation to international examples. Next, we precisely explain the data-sources we use for our research. We use three different data sets: one for our dependent variable, one for our treatment variable, and one for our control variables. The concept of educational level segregation and bridge-year will require specific explanation. Following this, we explain our model and methodology. We discuss the general model and the differences for the pooled-OLS and the municipality and time fixed effects. After explaining the methods used on our data we will discuss the assumptions and limitations associated with these methods and we perform several specification tests. Next, we discuss our results. In our analysis, we find no clear significant coefficient for the relationship between the number of bridge-years and educational level segregation. Only the pooled-OLS shows a significant, negative, correlation between the two. We interpret this as an increase of 4.2 % in the diversity of the average person's network when there is one more bridge-year in a municipality. However, all variations of our fixed effects model show no significant coefficient. We find a significant positive coefficient for the number of non-Western migrants and educational level segregation. However, it must be noted that these results do not imply any causal claims. Inference using fixed effects models requires specific assumptions, such as the independence of variables from the error term and the presence of time-variant regressors. Even if the assumptions hold, these models have limitations including low statistical power, limited external validity, restricted time periods, measurement errors, time invariance, undefined variables, and unobserved confounding (Hill et al., 2020).

2 Literature Review

This thesis is motivated by the existing literature related to the role of segregation in education and equality of opportunity. Primarily, our research is driven by a report by SEOR and Oberon, which analyzes, through simulations, policy measures designed to counteract educational segregation (Hek et al., 2024). One of the policy measures explored in this report is having multiple educational levels under one high-school roof. The report concludes that this measure can be considered the most effective in decreasing educational segregation and increasing equality of opportunity. The underlying idea is that increased interaction between students can have beneficial effects through the exchange of knowledge, values, and network. Another study builds upon this same idea by organizing meetings between pupils from different school and backgrounds, which significantly decreased educational segregation (Walraven and Peters, 2012). Additionally, recent research in American schools demonstrates that diversity can positively impact academic performance, specifically showing that the presence of immigrants positively affects US-born students through knowledge exchange (Figlio et al., 2024).

This study is closely related to the educational tracking debate. The age into which students are placed into different tracks, educational tracking, varies between countries. Some countries track students into differing-ability schools by age 10, others keep their entire secondary-school system comprehensive (Hanushek and Wößmann, 2006). This has motivated several studies to perform cross-country comparisons. Generally this research suggests that early tracking has a negative effect on student performances, especially on the literacy rates of already low achieving students (Lavrijsen and Nicaise, 2016, and equality of opportunity (Bol and Werfhorst, 2013, Hanushek and Wößmann, 2006).

More broadly, this research is motivated by the belief that social resources, or knowing people who know people, significantly enhance an individual's economic success in life. In the Moving to Opportunity experiment, in which randomly selected families in high poverty neighbourhood are given a financial incentive to move to low-poverty neighbourhoods, it was found that the exposure to better environments during childhood is an important determinant of children's long-term outcomes like college attendance and earnings (Chetty, Hendren, and Katz, 2016). Additionally, it was found that moving to a low-poverty area improved subjective well-being, mental health, physical health and family safety (Ludwig et al., 2013). One of the mechanisms behind these results could be the increased contact with individuals from higher social classes in a better neighbourhood. The more separated different groups in society are, the more they grow apart. This issue becomes particularly concerning when the affluent exclusively benefit one another, thereby excluding other lower social classes from their sphere. Literature related to social resources generally confirms that informal capital has a positive effect on career success (Zhang et al., 2010). However, other studies suggest that informal resources do not necessarily lead to higher occupational prestige and income (Graaf and Flap, 1988).

Segregation is defined as the separation or isolation of a proportion of the community (Press, n.d.). Segregation can refer to factors such as sex, age, income, language, comparative advantage and historical location (Schelling, 1969). In the case of comparative advantage, this segregation could simply be seen as a distribution corresponding to a socially efficient satisfaction of individual preferences (Schelling, 1969). However, in most other cases, policymakers view segregation as an undesirable phenomenon. In our case we are interested in segregation between educational levels in society. We focus on the matter in which adults have a diverse network in terms of educational level. Policymakers generally focus on addressing segregation in areas highly concentrated with poor households (Musterd, Ostendorf, and De Vos, 2003). Much of the literature on segregation suggests that large cities are the source of the most highly segregated groups and that these groups disperse and assimilate over time and with distance from the city (Peach, 1996). As shown in Figure 1 in our data section, educational level segregation in Dutch municipalities is also higher in more urbanized areas. Educational level segregation specifically concerns a proportion of the community isolated by educational level. In our research, as mentioned in our data section, we measure this by the similarity in educational level of the people in someone's social circle. Literature on the economics of social networks discusses that individuals are more likely to form ties with others who are similar to themselves (Jackson, 2009, Greenwood et al., 2014). Additionally, it is shown that educational level plays an important role in the formation of social networks and later economic outcomes (Mani and Riley, 2021). It is thus very unsurprising, yet highly unfavourable, that educational levels naturally segregate.

Based on previous literature, there are several control variables that should be included to minimize omitted variable bias. One study finds, based on data on school composition, evidence that non-western migrants are particularly affected by segregation (Burgess and Wilson, 2005). This suggests that areas with a high number of non-Western migrants can be expected to have a higher level of educational level segregation, which is why we include the number of non-Western migrants in our regression. Another variable of interest could be the average distance to schools. If a municipality offers more bridge-years it is likely there are more high-school in this municipality. The more high-schools there are, the more

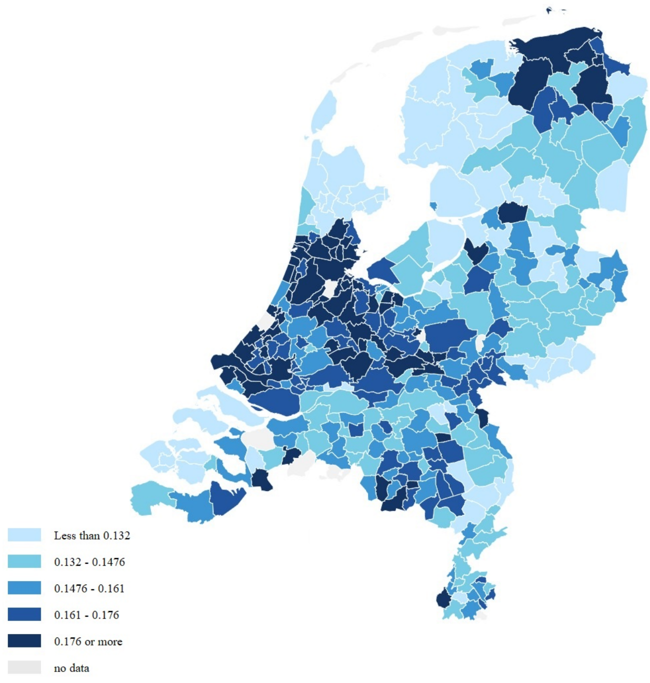

Figure 1: Educational level segregation 2020. Source: (Central Bureau for Statistics, 2023)

likely some of them offer a bridge-year. If the number of high-schools in a municipality increases, the average distance to schools will decline. Average distance to schools is thus by definition correlated with the number of bridge-years offered in a municipality and has been proven to influence academic success and could thus be expected to correlate with educational level segregation (Garza and Fullerton, 2018). Additionally, we should consider the general characteristics of the job-market and housing market, such as labor pressure, the disposable income of private households, social benefit recipients and more. In our data section we motivate our control variables in more detail

The Netherlands is not the only country that has debated the concept of a bridge-year in high-schools to mitigate social disparities in educational participation. In the 1970s, the German state of Lower Saxony introduced the Orientieringstufe, an intermediary level between primary and secondary school. This concept is highly comparable to the Dutch bridge-years. Similar to the Netherlands, students in Lower-Saxony were provided an additional two years to develop before being selected into different secondary education levels. Research examining this orientation level generally concludes this project did not meet its expectations (Schuchart, 2003, Schuchart and Weishaupt, 2004). Due to conservative recommendations, the orientation level actually had a obstructing effect on the transition to high-school. The main mechanisms identified in the literature that could have lead to the disappointing outcomes of the orientation level include the system's failure to provide accurate prognoses for students, a lack of reflection and transparency, and parental influences.

Another German concept that is based on the same principle as the Dutch bridge-years is the Gesamtschule. This is essentially a broad comprehensive school in which different educational courses are offered at the same institution. As discussed, bridge-years can be seen as a proxy for these broad schools. International research on these broad schools show differentiated results (OECD, 2004). Two countries with one of the best educational systems in the world are Finland (Morgan, 2014) and Hong Kong (Marsh and Lee, 2014). Hong Kong has a highly layered school system while in Finland all educational levels are combined. Therefore, broad school systems do not seem to provide guaranteed positive, or negative, results relating to educational success. However, the main reason why countries implement desegregation policies in education is to reduce inequality of opportunity (Giorgis, 2012).

Reverse causality in our study would manifest if educational level segregation influences the number of bridge-years offered by high-schools in a municipality. This could occur, for example, when an increase in educational level segregation prompts the government to subsidize high-schools offering bridge-years. This would require the government to be convinced that the introduction of bridge-years could decrease educational level segregation. In 2021 the Dutch government introduced a subsidy for high-schools that start offering heterogeneous bridge-years (Ministerie van Justitie en Veiligheid, 2021). Over the past three years, 68 million euros in subsidies have been requested by high-schools. This policy was mainly motivated by the COVID-19 pandemic in an attempt to stabilize the educational system. The government has highlighted two primary objectives of this policy: enhancing equal opportunities and reducing segregation (Dienst Uitvoering Subsidies aan Instellingen, n.d.). Therefore, it seems plausible that governments may introduce policies to expand bridge-year programs in response to growing societal concerns about educational segregation.. Thus, the possibility of reverse causality requires consideration.

3 Data

In our analysis we combine three different data sets. One for our dependent variable, one for our treatment variable, and one for the control variables. All these datasets consist of yearly cross-sectional panel data at the municipality level. After combining these three datasets and excluding municipalities with incomplete data, we are left with yearly data on 162 Dutch municipalities. Table 1 shows the summary statistics of our data set. The average municipality offers 7.1 bridge-years, thus over the 162 municipalities there are 1200 high-schools with a bridge-year. In 2024 there are 1633 high-schools in the Netherlands (DUO, 2024). This means that for every high-school in the Netherlands 0.73 bridge-years are offered. Since some bridge-years could offer multiple bridge-years, for example combining VMBO and HAVO aswell as HAVO and VWO, the percentage of high-schools that offer a bridge year should be lower than 73 percent. In Table 1 we find that the average distance for VMBO high schools is on average lower compared to HAVO and VWO schools. Moreover, we find that there are large differences between municipalities in for example it's population, the number of jobs or the number of social benefit recipients.

Table 1: Summary statistics

	Mean	SD	Min	Max	N
Segregation	170.46	21.48	117.80	241.60	826.00
Bridge-years	7.10	11.80	0.00	97.00	844.00
Population	77153.68	1.0e+05	919.00	8.7e+05	844.00
Labour pressure	73.81	9.62	44.80	113.00	844.00
Non-western migrants	9.26	6.97	1.50	38.90	844.00
Population density	1182.43	1259.73	23.00	6620.00	844.00
Students in secondary education	4249.60	4962.72	33.00	38094.00	844.00
Number of jobs	40.80	71.82	0.50	654.90	844.00
Private households	30.71	4.30	23.50	60.30	844.00
Social benefit recipients	20405.47	25492.05	240.00	2.2e+05	844.00
Distance to vmbo school	2.46	1.25	0.40	8.00	844.00
Distance to havo/vwo school	3.81	4.33	1.10	35.00	844.00

3.1 Dependent variable

For our dependent variable, educational level segregation, we use a dataset provided in a dashboard by the Dutch Central Agency for Statistics, often abbreviated to CBS (Central Bureau for Statistics, 2023). CBS collects high quality statistical information as part of its legal obligation to the Dutch government. This dashboard provides insights into the educational level segregation of individuals aged 25 to 55 years from the years 2009 to 2020 in Dutch municipalities. The dataset includes only individuals aged 25 to 55 since younger individuals often have not finished their education by 25, and those over 55 may not have accurate information on their education. CBS defines educational level segregation by composing a segregation score, which divides the population into four groups based on educational level: people with a master's degree, people with a bachelor's degree, people with a high school diploma, and people without any of these degrees. This information is based on various registers and a labour force survey, a large-scale panel study conducted by CBS. By using multiple sources, CBS achieved an impressive coverage rate of 11 million in 2015, which is still increasing every year.

The score measures, for each educational level and for the total population, the extent to which groups of a certain educational level live separately from groups with a different educational level. A low segregation score indicates a low level of segregation between groups. This score ranges from zero, meaning no segregation, to one, indicating complete segregation. In our data, the score varies from 0.117 until 0.241, as shown in Table 1. The score employs a so-called ego-network, where individuals more central in someone's network are assigned a larger weight. This network consist of five layers: Housemates, Family, Neighbours, Colleagues and Classmates. In this ego-network, housemates get assigned a higher weight than family, family gets assigned a higher weight than neighbours, and so on. These relationships are based on administrative data and it is unknown how often, if at all, these individuals actually interact with each other. CBS notes that this network does not include social relationships such as acquaintances through the church, the sport club or online. The network layer Roommates includes all individuals that are registered at the same address, or at the same institution for individuals that for example live in jail or in a nursing home. This data is derived from the Dutch population register. The network layer Family includes legal and biological family like partners, nuclear family and in-laws. This information is derived from the Dutch population register. The network layer Neighbours is derived from the Dutch population register and location data of buildings. This layer includes all individuals that live in the 10 addresses closest to the individual, regardless of the distance. Additionally it includes twenty randomized addresses within a radius of 200 meters of the individual's home. This second measure can be more appropriate for sparsely populated areas. The network layer Colleagues includes all individuals that work at the same company. If a individual has more than 100 colleagues only the 100 that live closest to the individual are included. This information is derived from the insurance administration of the Dutch tax agency together with the Dutch population register. The Classmate network layer is derived from educational registers provided by DUO. This includes all students that study at the same school in the same grade. If there are multiple classrooms in a school for one grade, we do not have the data to separate this.

The segregation score is measured relative to what can be expected based on the composition of a municipality's population. For example, if a municipality consists of 10 percent of people with a bachelor's degree as their highest education, people with a bachelor's degree will be considered segregated if more than 10 percent of this group is in their network. Figure 1 shows the average level of educational level segregation for each municipality in the Netherlands for the year 2020. It is important to separate the two concepts educational level segregation and educational segregation. The first measures the segregation in educational level for adults after they finished school, while educational segregation is an academic concept that concerns the segregation within schools. Our dependent variable is educational level segregation.

3.2 Treatment variable

For our treatment variable, the number of bridge-years offered by high schools in a municipality, we use a dataset provided by DUO, an agency controlled by the Dutch Ministry of Education. This dataset contains the number of bridge-years, or brugklassen in Dutch, offered by high schools for each municipality from 2015 to 2020 (Dienst Uitvoering Onderwijs, 2022). In our analysis, we divide the number of bridge-years by the population of the municipality.

Dutch high-school education lasts four to six years and is quite structured. The system is generally divided into three types of secondary school: VMBO, HAVO and VWO (Velden, Büchner, and Traag, 2014). At the age of twelve children are expected to enroll in secondary education and are admitted to a track based on teacher recommendations and standardised test scores. VMBO is a pre-vocational track that combines general education with voca-

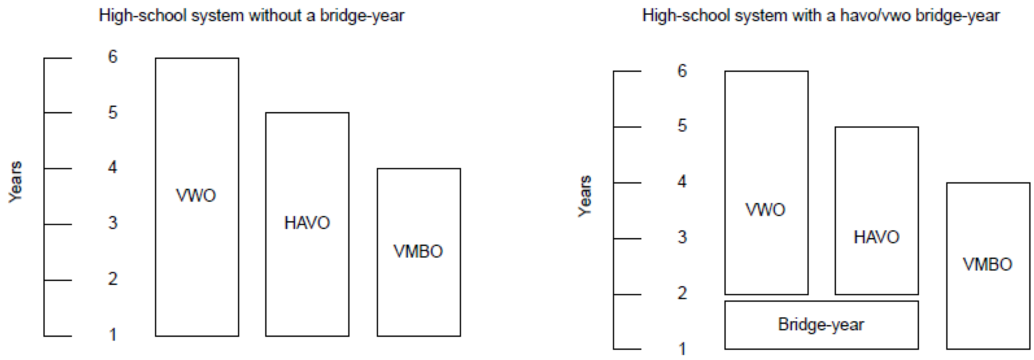

Figure 2: Visualisation of a bridge-year

tional training and takes four years. HAVO, a pre-college track, takes five years and prepares students for higher professional education, comparable to a university of applied sciences. HAVO has higher enrollment requirements compared to VMBO. Recently, there have been calls to avoid hierarchical language that gives pre-vocational education a negative image (Trouw, n.d.). VWO is the most academically oriented track and prepares students for university, taking six years. Figure 6 in appendix A provides a more detailed explanation of the structure of the Dutch educational system.

The basic format of the Dutch educational system requires students to be split into three different educational tracks around the age of 12. From this point on, classrooms, and often schools, are separated by these educational levels. The concept of a bridge-year is to postpone this separation. For example, a bridge-year that combines HAVO and VWO students does not separate them between these educational levels until the end of the year. A visualisation of this example can be seen in figure 2. A bridge-year is thus a general year in secondary education in which the student has not yet chosen a specific type of education (Statistiek, 2024).

The data differentiate between different types of bridge-years: homogeneous, heterogeneous. A homogeneous bridge-year is defined as a category where educational levels are still separated but does allow students flexibility to switch educational levels at the end of the year. This means that students are already separated into the three educational tracks but the school does allow flexibility for students to switch tracks. A homogeneous bridge-year is thus, in most characteristics, simply a regular first-year of high-school and would in international academic literature not be called a bridge-year since it does not offer any extra orientation period for the students or extra interaction between students from different backgrounds. Since we are only interested in classrooms where educational levels are combined, we exclude the homogeneous types from our analysis. After excluding the homogeneous type, we sum the remaining bridge-years for each municipality for each year. DUO obtains this data from student registration data in the ROD, Register of Educational Participants, and institutional data from BRIN, Basic Register of Institutions. Schools are obligated to provide accurate information to these registers. DUO acknowledges that the actual numbers may slightly differ from those registered by institutions due to, for example, human error.

A big limitation in this study is that we only have data on bridge-years for six years. Children generally start high-school around the age of 12, and educational segregation is not measured for individuals under the age of 25. This makes it difficult to measure the long-term effect that bridge-years could have on educational level segregation. In our discussion we discuss this limitation in more detail.

3.3 Control Variables

Finally, for our control variables, we use another CBS database on regional key figures in the Netherlands (Central Bureau for Statistic, 2024). This database collects extensive information on various topics, such as education, income, industry, and organizes this regionally. For our analysis we focus on the following variables at the municipality level: total population, labour pressure, non-western immigrants; population density; number of students in secondary education; total number of jobs in the municipalities economy; private households, excluding students; number of social benefit recipients; and the average road distance to the closest high school for the whole population, categorized by educational level. The total population variable measures only the individuals registered in the Dutch population register. Labour pressure is defined as the ratio between the population within the workforce and the population outside of the workforce. Non-western immigrants is measured as a percentage of the total population and includes all immigrants from the countries in Africa, Latin-America and Asia, excluding Japan and Indonesia. The number of students in secondary education includes all students enrolled in educational tracks: VMBO, HAVO or VWO. Private income measures the average disposable income for private households, The number of jobs shows the number of jobs of employees at companies and institutions in a municipality. The number of social benefit recipients shows the number of people that receive benefit through the Dutch unemployment act, the social assistance act or through other welfare related law.

4 Methodology

4.1 Model

In our data we observe the same 162 municipalities over six years, which constitutes panel data. Educational level segregation, our outcome variable, depends on several factors, some of which are unobserved. Unobserved variables correlated with our treatment variable, the number of bridge-years, can bias the treatment effect. However, under certain conditions, a correlation between the treatment variable and the outcome variable may suggest an unbiased causal effect. This is conditional on the omitted variables remaining constant over time. This means that even with heterogeneous variables across units, panel data estimators can help us to come closer to an accurate causal estimate of our treatment variable. However, due to the assumptions and limitations mentioned later in this section our results cannot be considered accurate estimates of the causal effect we aim to find. In our analysis, we will use two different panel data estimators: pooled ordinary least squares, POLS, and fixed effects.

For a cross-sectional unit i our model can be written as

$$Y_{it} = \beta_0 + \beta_1 D_{it} + \mu_i + \varepsilon_{it}; t = 1, 2, ..., T$$

Where we observe a sample of $i = 1, 2, ..., 162$ cross-sectional municipalities for the years $t = 1, 2, ..., 6$. We let D denote all observable variables, and β_1 be the treatment effect. We let μ_i represent the sum of all time-invariant unobserved characteristics of a municipality, the unobserved confounding. Finally, ε_{it} is the error term, which captures the time-varying unobserved characteristics, the idiosyncratic error. In our analysis we use heteroskedasticity-consistent robust standard errors (White, 1980), this will be motivated by the results of the Breusch-Pegan test, discussed in the specification section.

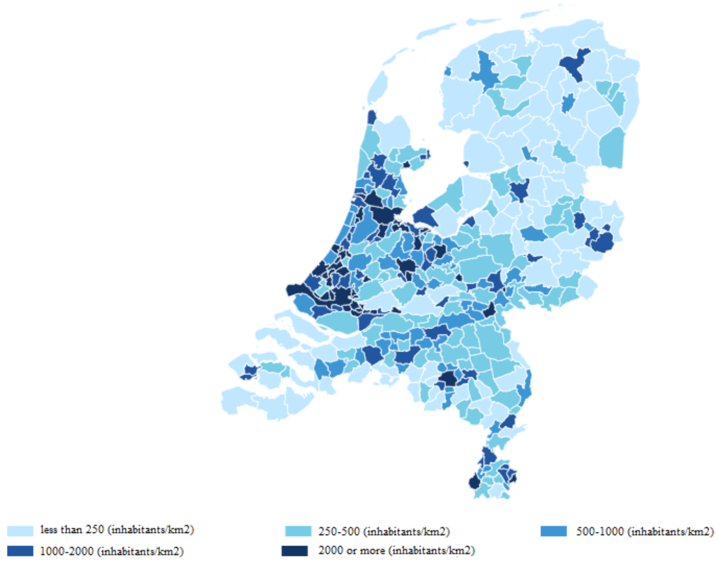

Figure 3: Population density in the Netherlands 2024. (Centraal Bureau voor de Statistiek, 2024)

4.2 Control variables

Trying to accurately estimate our treatment effect poses several challenges. One of these challenges is the problem of endogeneity. This occurs when unmeasured variables influence both the outcome and the treatment variable (Wright, 2015). To capture some of this bias we include control variables that we suspect to be a source of endogeneity. However, even after including our control variables there is likely still unmeasured confounding that causes endogeneity, since there are many variables we do not have the data for.

We include the control variables population and population density. Municipalities with a higher population or population density likely have a higher educational level segregation score. This is confirmed when we compare Figure 1 with Figure 3. The pattern of educational level segregation is clearly similar to the pattern of population density. Additionally, we expect the population and population density to influence the number of bridge-years in a municipality. Even though we use bridge-years per capita as our treatment variable, the absolute size of a municipalities population could still affect the number of bridge-years due to economies of scale, differences in infrastructure and availability of resources. Population density influences various socio-economic outcomes which could influence the demand for bridge-years within a municipality.

Next, labour pressure, defined as the ratio between the population within the workforce and the population outside the workforce, is included as a control variable in our analysis. We argue that an increase in labour pressure can increase the demand for labour which can increase the demand for immigration. If there is more demand for immigration it seems likely that the integration of the immigrants will be more successful since they have more contact with society through their job and the inhabitants will be more willing to receive them. Additionally, migrants' housing demand could increase local labour demand which could in turn again increase acceptance of working immigrants (Howard, 2020). Increased acceptance of immigrants could help decrease educational level segregation. This reasoning also motivates the inclusion of non-western migrants. Both non-western migrants and labour pressure could possibly influence the number of bridge-years in a municipality. Both variables influence the socioeconomic factors of a municipality. As shown in our literature review, promoting bridge-years is often motivated by certain socioeconomic factors such as inequality.

Our next control variable holds the number of students in secondary education in a municipality. The number of students in secondary education can directly impact educational level segregation through the size of the schools. Students at larger schools are more likely to form more diverse connections, which could lead to a reduction in educational segregation later in life. Additionally, if a municipality has less students there could be less possibilities to combine classrooms in to bridge-years, which will lead to less bridge-years per capita in a municipality.

The average disposable income of private household, our next control variable, can reflect the characteristics of the infrastructure and housing patterns within a municipality. Areas with richer private households could be more affluent neighbourhoods, which could influence social interactions and outcomes later in life (Chetty, Hendren, and Katz, 2016). The same reasoning can apply to the number of social benefit recipients in a municipality. Areas with more social benefit recipients could have differences in diversity of the neighbourhood which could in turn influence the educational level segregation in a municipality. Additionally municipalities with higher income households or more social benefit recipients might have more or less resources for well funded schools that can offer programs such as bridge-years. The number of jobs in a municipality is included as a control variable because it reflects the size of the economy of the municipality. Areas with more jobs may attract families seeking better employment opportunities, potentially altering the demographic composition of students in high schools. Which could affect both the educational level segregation and the need for bridge-years. Additionally a municipality with a well developed job-market could influence the perceived value of education, potentially affecting the distribution of students across different educational tracks.
Finally, the effect of the average distance to high-schools, differentiated in VMBO and HAVO/VWO, is controlled for. In areas where the average distance to schools is high, students might interact less with their classmates because they live further apart which could increase educational segregation. Additionally, if the average distance to schools is higher schools are likely to be further apart. This could increase the need for schools that offer multiple educational levels under one roof which could increase the number of bridge-years in a municipality.

4.2.1 Bad Controls

Bad controls arise when control variables are included in a regression that introduce unintended discrepancies between the estimated coefficient and the effect this coefficient is supposed to measure (Cinelli, Forney, and Pearl, 2020). Bad controls are variables that are themselves outcome variables and thus could be affected by the treatment. Bad controls can either simply be a outcome variable or proxy-control, where the included variable partially controls for omitted factors but is partially affected by the variable of interest (Hanck, Angrist, and Pischke, 2011). Variables that could possibly be affected by the number of bridge-years in a municipality are the number of jobs per capita, students in secondary education per capita, and the number of social benefit recipients per capita. If a municipality offers more bridge-years this could move students to keep studying longer which would increase the number of students in secondary education at a given moment. Additionally, assuming that bridge-years has an effect, positive or negative, on the quality of education, it does not seem unlikely that this could influence the number of jobs or the number of social benefit recipients in a municipality. However, due to our limited time-frame, it seems unlikely these effects would have manifested this early. Nevertheless, we will exclude these variables as a robustness check to see how this impacts our results.

4.3 Interaction effects

For all of our estimation methods it may be beneficial to include interaction effects. An interaction effect measures how the effect of one independent variable depends on the value of another independent variable (Cox, 1984). Suppose we have only have 2 control variables, our model including an interaction term would be written as

$$Y_t = \beta_0 + \beta_1 D_{1t} + \beta_2 D_{2t} + \beta_3 (D_{1t} * D_{2t}) + \mu_i + \varepsilon_{it}; t = 1, 2, ..., T$$

In our model, we choose to include interaction effects between bridge-years and population density, bridge-years and non-western migrants, and labour pressure and non-western migrants. For example, if the coefficient for interaction between bridge-years and population density is significantly positive, it would indicate that the effect of the number of bridge years on educational level segregation is larger in more densely populated municipalities.

There are several reasons one might want to include interaction effects in their model. One is to identify sub-groups in our sample in which our treatment might have a larger effect (VanderWeele and Knol, 2014). In our case this would help us identify which municipalities would benefit most from more bridge-years. This is why we include interaction effects between bridge-years and population density. It could be interesting to find out whether densely populated municipalities such as Amsterdam or Rotterdam benefit more, or less, from bridge years compared to more sparsely populated municipalities. The interaction effects between bridge-years and non-western migrants can help us understand if municipalities with a high number of non-western migrants per capita benefit more, or less, from the availability of bridge-years. Another reason to include interaction effects is that it could provide insights into the mechanisms of the outcome (VanderWeele and Knol, 2014)). This is our motivation to include the interaction effect between labour pressure and non-western migrants. We are interested in how educational segregation reacts differently to migration in municipalities with a high labour pressure compared to municipalities with a low labour pressure.

4.4 Pooled OLS

Our first estimator will be the pooled ordinary least squared regression. Here, we ignore the panel structure and pool all the data into one large cross-section before performing a standard OLS regression. One advantage of pooling the data is that it increases the sample size, thereby enhancing the power and accuracy of the analysis. Additionally, this method allows us to eliminate annual dummy variables to account for macroeconomic variables. However, the pooled OLS regression does not account for the confounding between the units and the time-variant effects of the data (Wooldridge, 2002). Ignoring the panel structure, the model becomes:

$$Y_{it} = \beta_0 + \beta_1 D_{it} + \eta_{it}; t = 1, 2, ..., T$$

Where the error term $\eta_i t = c_i + \varepsilon_{it}$ captures both the unobserved confounding and the idiosyncratic error. The main assumption for an accurate estimate is that the unobserved confounding c_i is uncorrelated with the treatment variable D_{it}. In our research, this would mean the number of bridge-years per capita in a municipality would be unrelated to certain unobserved background characteristics. This assumption is not credible. Unobserved background characteristics, such as the quality of schools, socioeconomic status, political priorities, or historical development patterns, can simultaneously influence both the number of bridge-years and the educational level segregation. Because these unobserved factors cannot be included in the analysis, there will be omitted variable bias. Nevertheless, this method is included in our research as a baseline comparison, and it can help us understand the mechanisms by identifying potential variables of interest.

4.5 Fixed effects

In an attempt to reduce the unobserved confounding, we will use a fixed effects model. The fixed effects model controls for time-invariant unmeasured confounding. However, it is unable to control for other biases such as reverse causality and time-varying unmeasured confounding (Gunasekara, 2014). In our fixed effects model we reintroduce the panel structure of our data, which brings our regression equation back to:

$$Y_{it} = \beta_0 + \beta_1 D_{it} + \mu_i + \varepsilon_{it}; t = 1, 2, ..., T$$

Next, we create time-demeaned variables by subtracting the mean from the individual municipality:

$$\ddot{Y}_{it} = Y_{it} - \bar{Y}_i$$
$$\ddot{D}_{it} = D_{it} - \bar{D}_i$$

When we do this, μ_i, the sum of all time-invariant unobserved characteristics of a municipality, cancels out, since it is time-invariant and the mean should be equal to the individual levels. For this to hold, D_{it} cannot be correlated with the error term at any time point. This does allow D_{it} to be correlated to μ_i, contrary to the random effects model. Since all time-invariant variables vanish, we cannot include any observed time-invariant control variables in our regression due to perfect collinearity. With the time-invariant unobserved confounding eliminated, our regression equation becomes:

$$\ddot{Y}_{it} = \beta_0 + \beta_1 \ddot{D}_{it} + \ddot{\varepsilon}_{it}; t = 1, 2, ..., T$$

The standard application of the fixed effects model is at the individual level, which in our case means the municipality level. Intuitively, this is equivalent to creating a dummy indicator for each municipality, fitting one regression line for each. Additionally, we could also apply the fixed effects model at the individual and time level. Here, we add a dummy for each time-period, allowing us to control for variables that are fixed across municipalities for each period, but change over time. Examples of such variables in our case could be related to educational policies or economic trends that effect the nation as a whole, such as inflation. Including time fixed effects our regression equation becomes:

$$\ddot{Y}_{it} = \beta_0 + \beta_1 \ddot{D}_{it} + \lambda_t + \ddot{\varepsilon}_{it}; \quad t = 1, 2, \ldots, T$$

Where λ_t represent the time fixed effects dummy's. Here only $T - 1$ dummy's are included since the model includes an intercept. The combined model eliminates bias from both unobserved factors that change over time but are constant over municipalities and it controls for factors that differ across municipalities but are constant over time.

4.5.1 Assumptions and limitations

There are several important identifying assumptions for the fixed effects model. First, the independent variables must be uncorrelated with the error term at all time periods, conditional on the unobserved effect. Second, regressors must vary over time to avoid collinearity. Another crucial assumption is the absence of serial correlation between time periods. Finally our data should have no large outliers.

To test these assumptions, we can start by performing a Durbin-Wu-Hausman test (Hausman, 1978), which detects endogenous regressors in a regression model based on the difference between the fixed effects and random effects estimators. If the test rejects its null hypothesis, it suggests that the regressors are correlated with the individual effects, supporting the use of fixed effects. Next we can include lagged variables and calculate the Durbin-Watson statistic to test for auto-correlation in lag 1 in the residuals (Durbin and Watson, 1950). To check for large outliers in our data we can calculate the kurtosis value of our data (Groeneveld and Meeden, 1984).

Another caveat in our data is the potential for reverse causality. Reverse causality occurs when the dependent variable influences the independent variable. In our research, we must consider whether the educational level segregation in a municipality could influence the number of bridge-years offered by high schools. A plausible mechanism would be policy-makers recognizing that bridge-years might decrease educational level segregation and thus increasing the number of bridge-years in municipalities with high segregation scores. Like discussed in our literature review, this is a realistic mechanism. However, if this policy were effective in the long run, it would decrease segregation scores due to the higher number of bridge-years, making the reverse effect ambiguous. Nevertheless, it is valuable to statistically test for reverse causality in our data. We can do this by including lagged values of our independent variables in our model. The idea is that past values are less likely to be influenced by current values of the dependent variable. We thus assume that past values of our independent variable influence the current dependent variable, but not vice versa. If the results of this regression are similar to those of our regression without the lagged values, it suggests that reverse causality is not present.

A significant limitation of the fixed effects model is its inability to address time-variant unobserved confounding. If omitted variables are time-variant, the fixed effects model simply incorporates these into the error term, complicating causal inference. Even with our control variables, there is likely still time-variant unobserved confounding that influences both educational level segregation and the number of bridge-years in a municipality. Potential sources of this confounding are likely related to policy changes and economic trends. If these sources of confounding impact municipalities equally at any given year, we can still control for them by including time fixed effects. However, municipality-specific sources of confounding, such as local government initiatives, cannot be controlled for. Therefore, it is prudent to be conservative in drawing causal conclusions and essential to perform robustness and sensitivity analyses.

4.6 Specification tests

Before discussing our results we start by performing several specification tests. We start by testing for multi-collinearity by running a correlation matrix as seen in Table 2. We use a threshold of 0.9 to determine problematic multi-collinearity (Franke, 2010).This analysis reveals that the population variable is particularly problematic, with correlations above 0.97 with the variables: students in secondary education, number of jobs and social benefit recipients. Additionally, these last three variables are also highly correlated with each other. This why we chose to divide these variables by the total population of each municipality. Table 3 shows that this removes all problematic collinearity relating to these variables. Next, we test the heterogeneity across years. As mentioned, our fixed effects model assumes that unobserved municipality effects are constant over time. If there are significant changes in heterogeneity across years, this assumption could be violated. Figure 4 shows that the heterogeneity is fairly constant across years, indicating that the assumption holds. This result supports the use of the fixed effects model

We will perform a Hausman-test to detect endogenous regressors in our regression model based on the difference between the fixed effects model and the random effects model (Hausman, 1978). Table 4 shows the results of this test. With a p-value of 0.00, the null hypothesis of no correlation between the explanatory variables and the error term can be rejected. This indicates that the regressors are correlated with the individual effects, which supports the use of the fixed effects model instead of the random effects model.

	1	2	3	4	5	6	7	8	9	10	11	12
Segregation (1)	1											
Bridge-years (2)	-0.122	1										
Population (3)	0.410	0.155	1									
Labour pressure (4)	-0.220	-0.196	-0.608	1								
Non-western migrants (5)	0.436	0.221	0.747	-0.648	1.0000							
Population density (6)	0.492	0.167	0.567	-0.536	0.740	1						
Students in secondary education (7)	0.407	0.166	0.989	-0.591	0.763	0.563	1					
Number of jobs (8)	0.390	0.145	0.978	-0.608	0.697	0.525	0.955	1				
Private households (9)	0.409	0.154	0.998	-0.597	0.736	0.564	0.980	0.980	1			
Social benefit recipients (10)	0.399	0.172	0.990	-0.590	0.741	0.552	0.978	0.957	0.989	1		
Distance to vmbo school (11)	-0.383	-0.247	-0.299	0.379	-0.559	-0.556	-0.302	-0.302	-0.297	-0.297	1	
Distance to havo/vwo school (12)	-0.222	-0.255	-0.2897	0.3864	-0.5276	-0.5101	-0.3042	-0.2846	-0.2823	-0.2858	0.7293	1.0000

Table 2: Correlation matrix of variables

	1	2	3	4	5	6	7	8	9	10	11	12
Segregation(1)	1											
Bridge-years(2)	-0.122	1										
Population(3)	0.410	0.155	1									
Labour pressure(4)	-0.220	-0.196	-0.608	1								
Non-western migrants(5)	0.436	0.221	0.747	-0.648	1							
Population density(6)	0.492	0.167	0.567	-0.536	0.740	1						
Students in secondary education(7)	-0.080	-0.024	-0.315	0.535	-0.285	-0.286	1					
Number of jobs (8)	0.054	0.308	0.427	-0.546	0.417	0.266	-0.230	1				
Private households (9)	0.409	0.154	-0.135	-0.597	0.736	0.564	-0.327	0.417	1			
Social benefit recipients (10)	-0.093	-0.004	-0.223	0.322	-0.241	-0.209	-0.336	-0.246	-0.198	1		
Distance to vmbo school (11)	-0.383	-0.247	-0.299	0.379	-0.559	-0.556	0.138	-0.374	-0.297	0.132	1	
Distance to havo/vwo school (12)	-0.222	-0.255	-0.290	0.386	-0.528	-0.510	0.022	-0.382	-0.282	0.208	0.729	1

Table 3: Correlation matrix after dividing by population

Next we will perform a Breusch-Pegan test to determine whether the variance of the errors are dependent on the values of our independent variables (Breusch and Pagan, 1979). If this is the case we speak of heteroskedasticity. In the presence of heteroskedasticity, the assumption of constant variance of the error terms is violated. Which can lead to inconsistent and biased estimates of the standard errors, which could lead to incorrect inference of the results. In table 5 the results of the Breusch-Pegan test are shown. The resulting P-values is 0.000 which means we an very soundly reject the null hypothesis of homoskedasticity. The variance of the residuals increases as a function of at least on of our independent variables and possibly more than one of these independent variables. The most common strategy for dealing with heteroskedasticity is the use of heteroskedasticity-consistent robust standard errors (White, 1980). This method allows for the fitting of a model that contains heteroskedatic residuals. We can thus conclude that the results of this test support the use of heteroskedasticity-consistent robust standard errors.

Statistic	Value
Chi-squared	28.84
Degrees of Freedom	7
P-value	0.00

Table 4: Hausman Test Results

Statistic	Value
N	805
Chi-squared	1457.97
P-value	0.00

Table 5: Breusch-Pegan Test Results

Next we perform the Durbin-Watson test as a measure of serial correlation in the error

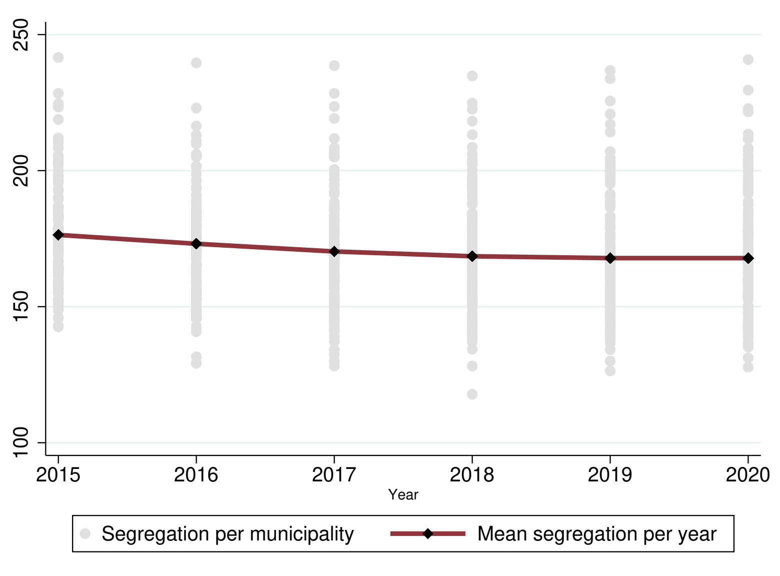

Figure 4: Heterogeneity across years

terms (Durbin and Watson, 1950). After performing this test we are left with a Durbin-Watson statistic of 0.053. This suggests a strong positive auto correlation. Generally test statistics under one are considered to be a genuine cause of concern (Field, 2009). However, the inclusion of robust standard errors and time-fixed effects, could reduce the interpretation of these test results.

Finally we calculate the kurtosis value to test for large outliers in our data. If there are no heavy tails or possible outliers in our data the kurtosis value should be around 3 (Groeneveld and Meeden, 1984). We find that most of our variables are between the range of 2 and 4, suggesting that outliers is not a problem in our data.

5 Results

When determining the statistical significance of our results we us the common threshold of a 95 percent confidence interval (Fisher, 1925). Since the stakes in the field of education are high, and the interpretation of our results could imply drastic policy changes, we do not think it is appropriate to lower this interval.

5.1 Pooled-OLS results

The first column of table 6 shows the results of our pooled ordinary least squares regression. Here, we included all of our control variables and ignored the panel structure of our data. As shown in the table, the coefficient for the number of bridge-years in a municipality per thousand inhabitants is negative and significant at a 99 percent confidence interval. To speak in terms of our model, β_1 is estimated to be -84.01. This means that we estimate the educational level segregation score to decrease by around 0.084 points, on a scale from 0 to 1, for every additional bridge-year offered per 1000 inhabitants. Notice that the actual range of educational segregation is between 0.1178 and 0.2416, see Table 1. For a municipality with 41 thousand inhabitants, which is the average for a Dutch municipality, we divide the effect by 41 compared to the 1000 inhabitants, and thus we estimate the segregation score to drop by 0.0021 points for every additional high-school that offers a bridge-year.

Figure 5: Distribution of the average high educated individuals' network during a drop in the segregation score in our model municipality

To fully understand the significance of these results, we need to revisit the interpretation of the educational level segregation score as discussed in our data section. This score measures the extent to which people live separately from different educational groups relative to what can be expected based on the composition of the population. To illustrate, say a municipality of one thousand people is composed of fifty percent highly educated people and fifty percent low-educated. If the average person's network exactly replicates this distribution, the educational level segregation score will be zero. If the average highly educated individual has zero low-educated individuals in their network, and vice versa, the segregation score will be one, indicating full segregation. In reality, the segregation score does not fall outside the range of 0.117 to 0.241, as seen in Table 1. So what would a segregation score drop of 0.084 points mean in our fully segregated fifty-fifty example? This would mean the average individual moves 8.4 percent from fully segregated, and thus 0 percent of the other educational group in their network, to zero segregation, and thus 50 percent of the other educational group in their notwork. In other words, a drop of 0.084 point in educational segregation entails, in our model municipality, an increase of 4.2 percent in people from another educational group in the average persons network. Figure 5 shows a visualisation of the effect of this drop in educational segregation on the distribution of the network of the average high educated individual in our model municipality. Furthermore we find a significant coefficient for the control-variables population density, students in secondary education per capita, number of jobs per capita, average distance to VMBO schools, and the average distance to HAVO/VWO schools. We find that educational segregation positively correlates with population density, the number of students in secondary education per capita and the average distance to HAVO/VWO schools. We find that educational segregation negatively correlates with the number of jobs per capita and the average distance to VMBO schools. These findings confirm the necessity to include these variables.

Of course, as discussed, the POLS method has many limitations, and we should be careful with the causal interpretation of these results. Even though we included many control variables, there is still likely to be a correlation between the unobserved confounding and our treatment variable that this method does not control for. Unobserved background characteristics are likely still biasing our results. Many of these background characteristics are fixed for each municipality or over time, which makes it interesting to include municipality and time fixed effects.

5.2 Fixed effects results

The second column of Table 6 presents the results of our municipality and time fixed effects analysis. Here we reintroduce the panel structure of our data and create time demeaned variables by subtracting the mean from the individual municipality. Additionally we include a dummy for each year, allowing us to control for variables that are fixed across municipalities but change over time. Table 6 indicates that the estimated coefficient is not significant at any of the confidence intervals. This suggests that the number of bridge-years offered in a municipality does not have a significant effect on the educational level segregation. Interestingly, we find a significant positive coefficient for the number of non-Western migrants, suggesting a positive relationship between educational level segregation and the number of non-western migrants in a municipality.
A possible interpretation of this finding is that an increase in non-Western migrants heightens polarization between social classes, as individuals may feel threatened by high levels of migration and wish to maintain their cultural identity. Another interpretation could be that migrants generally have fewer connections within the country, having had less time to develop them, which could increase segregation. The stark difference in results between our pooled-OLS and the municipality and time fixed effects models suggests that in the pooled regression, there is considerable variation in time-invariant unobserved confounding factors between municipalities that are not accounted for. These confounding factors could be due to municipality-specific differences in history, location, culture and more, which are not measured in our data.

Subsequently, it is logical to replace our treatment variable with lagged versions of our bridge-years variable. Intuitively, it is reasonable to assume that it takes some years for the potential effects of bridge-years on educational level segregation to manifest. Ideally, we would have data on the number of bridge-years spanning more than twenty years. In such a case, we could observe the long-term effects of students exposed to bridge-years on educational level segregation as adults. However, our dataset is limited, as it only includes observations from six consecutive years. Thus to avoid reducing our sample size to much, we only include op to three years of lags. Table 7 presents the results of these lagged fixed effects models, showing the one-year, two-year, and three-year lags for the municipality and time fixed effects model. As seen in Table 7, we find no significant coefficient for any of our lags. Additionally, since the past values of our independent variable do not appear to have a different relationship with our outcome variable, this suggests that there is no indication of reverse causality.

5.3 Interaction effects

In our model, we include interaction effects between bridge-years and population density, bridge-years and non-Western migrants, and labour pressure and non-Western migrants. These interaction effects are incorporated into both the pooled OLS and the municipality and time fixed effects. The results of these interaction effects are presented in Table 8. Here, we find no convincing evidence for existing interaction effects. We only find a coefficient of 0.0306 for the interaction effect between bridge-years and population density, which is merely significant at a 90% confidence interval. This suggest that the relationship between bridge-years and educational level segregation is less negative when a municipality has a higher population density. This would imply that introducing bridge-years is more effective in municipalities that are sparsely populated. However, the already weak significance of this interaction term does not hold up when we switch to the fixed effect models. This makes the results unreliable since they are not robust to changes, and the pooled OLS model is subject, to many, aforementioned, limitations.

	(1)	(2)
VARIABLES	Pooled OLS	Fixed effects
Bridge-years	-84.01***	2.985
	(10.63)	(16.89)
Population	0.000170**	-0.000678**
	(7.73e-05)	(0.000318)
Labour pressure	0.0503	-0.392
	(0.134)	(0.366)
Non-western migrants	0.0565	1.698***
	(0.162)	(0.598)
Population density	0.00560***	-0.000409
	(0.000744)	(0.00240)
Students in secondary education	384.6***	289.1
	(126.0)	(181.3)
Number of jobs	-15,662***	-37.747**
	(5,333)	(14.726)
Private households	-0.221	1.328**
	(0.152)	(0.596)
Social benefit recipients	52.24**	-126.5*
	(23.82)	(75.34)
Distance to vmbo school	-6.460***	0.298
	(0.722)	(1.629)
Distance to havo/vwo school	1.931***	-0.110
	(0.510)	(0.227)
Constant	140.3***	230.6***
	(15.27)	(36.91)
Observations	805	805
R-squared	0.397	0.337
Number of municipalities		162
Municipality FE		YES
Year FE		YES

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 6: Regression results

VARIABLES	(1) Fixed effects
Bidge-years (t = -1)	6.082
	(19.04)
Bridge-years (t = -2)	21.17
	(17.93)
Bridge-years (t = -3)	-28.13
	(19.08)
Population	0.000522
	(0.000587)
Labour pressure	-0.926
	(0.681)
Non-western migrants	2.295*
	(1.346)
Population density	-0.00355
	(0.00633)
Students in secondary education	418.4*
	(212.4)
Number of jobs	-24,676
	(18,833)
Private households	-1.029
	(1.119)
Social benefit recipients	-67.36
	(104.9)
Distance to vmbo school	-1.761
	(2.133)
Distance to havo/vwo school	-9.292**
	(3.852)
Constant	251.9***
	(60.37)
Observations	342
Number of municipalities	126
R-squared	0.139
Year FE	YES

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 7: Results with lagged bridge-years

VARIABLES	(1) Pooled OLS	(2) Fixed effects
Bridge-years	-120.0***	-16.35
	(18.87)	(28.49)
Bridge-years ∗ Population density	0.0306*	0.00298
	(0.0178)	(0.0231)
Bridge-years ∗ Non-western migrants	0.255	1.884
	(2.860)	(3.867)
Labour pressure ∗ Non-western migrants	-0.0165	-0.00814
	(0.0168)	(0.0307)
Population	0.000236***	-0.000610*
	(7.95e-05)	(0.000340)
Labour pressure	0.230	-0.330
	(0.190)	(0.515)
Non-western migrants	1.208	2.171
	(1.205)	(2.615)
Population density	0.00281*	-0.00101
	(0.00161)	(0.00409)
Students in secondary education	363.5***	291.3
	(124.8)	(182.6)
Number of jobs	-15,023***	-38,164***
	(5,421)	(14,573)
Private households	-0.380**	1.185*
	(0.165)	(0.645)
Social benefit recipients	62.14**	-133.1*
	(24.29)	(74.72)
Distance to vmbo school	-6.607***	0.206
	(0.736)	(1.666)
Distance to havo/vwo school	1.751***	-0.112
	(0.504)	(0.233)
Constant	129.3***	229.0***
	(18.14)	(51.16)
Observations	805	805
R-squared	0.405	0.338
Number of municipalities		162
Municipality FE		YES
Year FE		YES

Robust standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 8: Regressions including interaction effects

5.4 Robustness checks

For our pooled-OLS, we can perform a sensitivity analysis to better understand the impact of the omitted variables in our regression models (Cinelli, Forney, and Pearl, 2020). Table 9 shows the results of this analysis. $R^2_{Y\sim D|\mathbf{X}}$ indicates that in an extreme scenario, even if there is a confounder completely independent of all control variables in our model, and this confounder explains all of the residual variance in the outcome, then this confounder would need to explain at least 7.03 percent of the residual variance of the treatment to fully account for the observed estimated effect. $RV_{q=1}$, the robustness value, shows that only unobserved confounders, independent of the covariates, that explain more than 23.98 percent of the residual variance of both the treatment and the outcome are strong enough to bring the estimate to zero. $RV_{q=1,\alpha=0.05}$, shows that these unobserved confounders need to explain more than 18.53 percent of the residual variance of both the treatment and the outcome to bring the estimate to a range where it is no longer statistically significant within a 95 percent confidence interval. It is difficult to estimate whether our unobserved confounding would exceed these robustness thresholds. We can, however, bound the strength of our unobserved covariates within the same strength of one of our observed control variables: the number of jobs per capita. Table 9 shows that a confounder as strong as the number of jobs per capita can at most explain 4.91 percent of the residual variation of the outcome and 1 percent of the residual variation of the treatment. This means that our estimate would be robust to a unobserved confounding as strong as our jobs control variable.

Outcome: *Educational segregation*						
Treatment:	Est.	S.E.	t-value	$R^2_{Y\sim D\|\mathbf{X}}$	$RV_{q=1}$	$RV_{q=1,\alpha=0.05}$
Bridge-years	-84.0062	10.85	-7.746	7.03%	23.98%	18.53%
df = 793		*Bound (1x Amount of jobs)*: $R^2_{Y\sim Z\|\mathbf{X},D}$ =4.91%, $R^2_{D\sim Z\|\mathbf{X}}$ = 1.26%				

Table 9: Sensitivity analysis

Table 10 shows the results after dropping the control variables: the number of jobs per capita, students in secondary education per capita, and the number of social benefits per capita. We drop these variables because we suspect them to possibly be affected by the number of bridge-years in a municipality, and thus be bad controls. Comparing the results in Table 6 to the results in Table 10 shows that the coefficient for our variables of interest, bridge-years, remains stable for both the pooled OLS and the fixed effects regression. This indicates that the inclusion of these potential bad controls did not significantly bias the estimators and suggests that our results are robust. Additionally, we find that the coefficient for non-Western migrants remains significant and positive after excluding the possible bad controls.

In our next robustness check, we will divide the sample into two, urban and rural, to see if our results hold up in both groups. We divide these two groups by the threshold of 690 inhabitants per square kilometer, which is the median of all the municipalities in our data. Table 11 shows the results of these divided regression. Comparing column one and three of Table 11 shows that the coefficient for our rural sub-sample is more negative compared to our urban sub-sample. Comparing column two and four shows that the insignificant coefficient goes from positive in the urban sample to negative in the rural sample. However, the statistical interpretation of our results, after splitting up the sample, does not change compared to our main results in Table 6. Additionally, our R-squared across the sub-samples remains very stable for the fixed effects model and less stable for the pooled-OLS. These findings support the robustness of of our results, particularly those of the fixed effects model.

VARIABLES	(1) Pooled OLS	(2) Fixed effects
Bridge-years	-85.54***	3.531
	(10.45)	(17.26)
Population	0.000171**	-0.000493
	(6.75e-05)	(0.000331)
Labour pressure	0.424***	-0.443
	(0.132)	(0.360)
Non-western migrants	0.124	2.428***
	(0.151)	(0.685)
Population density	0.00570***	-0.000772
	(0.000689)	(0.00228)
Private households	-0.235*	1.147*
	(0.130)	(0.606)
Distance to vmbo school	-5.981***	0.866
	(0.721)	(1.834)
Distance to havo/vwo school	1.796***	-0.0883
	(0.494)	(0.261)
Constant	141.6***	184.7***
	(10.41)	(28.47)
Observations	805	805
R-squared	0.381	0.319
Number of municipalities		162
Municipality FE		YES
Year FE		YES

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 10: Results after dropping possible bad control variables

VARIABLES	(1) Pooled OLS (Urban)	(2) Fixed effects (Urban)	(3) Pooled OLS (Rural)	(4) Fixed effects (Rural)
Bridge-years	-68.52***	9.870	-97.63***	-15.65
	(15.91)	(24.19)	(13.90)	(20.36)
Population	0.000128	-0.000627*	0.00209***	-0.00143
	(9.02e-05)	(0.000329)	(0.000447)	(0.00179)
Labour pressure	-0.0837	0.366	0.323*	-1.861***
	(0.175)	(0.508)	(0.165)	(0.620)
Non-western migrants	-0.247	2.652**	0.682**	2.224**
	(0.179)	(1.036)	(0.301)	(1.116)
Population density	0.00447***	0.000638	0.0679***	0.0351
	(0.000824)	(0.00246)	(0.00660)	(0.133)
Students in secondary education	245.2	250.3	342.7**	405.9
	(174.6)	(153.5)	(152.1)	(332.6)
Number of jobs	-6,200	-3,017	-41,715***	-111,697***
	(6,440)	(12,907)	(6,915)	(28,654)
Private households	-0.143	1.364**	-4.786***	2.485
	(0.177)	(0.613)	(1.025)	(3.635)
Social benefit recipients	-13.25	-153.9*	206.4***	-67.36
	(33.00)	(91.45)	(39.68)	(170.0)
DIstance to vmbo school	-8.377***	0.904	-2.556***	-0.322
	(2.045)	(2.353)	(0.799)	(1.829)
Distance to havo/vwo school	-1.017	-0.929	2.760***	-0.0156
	(1.309)	(1.355)	(0.544)	(0.253)
Constant	185.5***	152.2***	49.26***	344.4***
	(18.94)	(39.72)	(18.07)	(84.36)
Observations	411	411	394	394
R-squared	0.384	0.372	0.550	0.373
Number of municipality_id		79		85
Municipality FE		YES		YES
Year FE		YES		YES

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 11: Results after splitting the sample in urban and rural

6 Discussion

The outcomes of this research have provided valuable insights into the mechanisms behind educational level segregation and the role high-schools can play in this. The findings of this paper indicate that there is no statistically significant relationship observed between the number of bridge-years offered by a municipality and the educational level segregation. Additionally, we find suggestive evidence for a significant positive relationship between the number of non-Western migrants and educational level segregation. However, interpretation of the results, especially causal interpretations, must be done with caution. This research suffers from several limitations, which will be discussed below.

6.1 Narrow time-frame

A first and obvious limitation of our research is the narrow time-frame of our data. Children in the first-year of high school are generally around the age of twelve. Educational level segregation is measured in adults between the ages of 25 and 55. Ideally, we would thus have data on bridge-years for at least twenty years. Unfortunately, we only have data on six years. The mechanisms behind the hypothesized relationship between bridge-years and educational level segregation can be split into two. First, there is the lagged effect. Children enrolled in a bridge-year can be expected to have more interaction with children that will end up in a different educational level. Later in life, these children could be expected to have a more educationally diverse network compared to children who have not been in a bridge-year. For this mechanism to show in our results, we would need a broader time-frame than we have in this research. The second mechanism will be called the direct effect. This could be caused by, for example, parents having children in a bridge-year which could cause parents to become friends with their children's friends' parents. Or more generally, the existence of bridge-years can be seen as a proxy for the existence of schools with multiple educational levels under one roof. After all, there cannot be a bridge-year if the school only offers one educational level. If a municipality has more diverse schools, one could argue that this creates a better climate in general for more interaction between social classes, which could decrease educational level segregation. For capturing this direct effect, our narrow data set is more suitable.

6.2 External validity

A second issue is the weak external validity of our results. Our external validity is questionable in two ways. Our dependent and treatment variables are both very specific concepts, which makes it hard to extrapolate our results to broader lessons. Educational level segregation may sound like a general term, but in our case, it is a score based on very specific and original research by the Dutch statistical agency (Central Bureau for Statistics, 2023). This makes it hard to compare our results with previous literature, as this previous literature does not use the same measure of educational level segregation. This, in turn, makes it hard to gauge the significance of our results in settings outside of the Netherlands. However, this does not mean our results cannot be be valuable outside of the Netherlands. As substantiated in our literature review, educational level segregation is closely tied to the pursuit of equality of opportunity, a widely acknowledged concern across nations. Another limitation of the segregation score is that it measures the relationships based on administrative data. This means that it is unknown whether these relationships actually exist, and if they do, how strong they are.

Additionally, the term bridge-years or 'brugklassen', is a very specific concept to the Dutch educational system. As mentioned in our literature review, only Germany is found to have a similar system . We argue that bridge-years should be seen as a proxy for the broader concept of diversity in schools and specifically the combination of educational levels under one roof. As mentioned before, for a school to have a bridge-year it will by definition have at least two educational levels under one roof. However, the inverse does not hold. If a school has multiple educational levels under one roof, this does not mean this school offers a bridge-year. The proxy is thus not perfect. One must, therefore, be careful when extrapolating these results to broader concepts or different regions.

7 Conclusion

In this paper, we provide an insight into the mechanisms behind education segregation by analysing a specific characteristic of Dutch high schools. Our analysis is motivated by previous literature that suggests promoting broad and diverse schools positively impacts segregation and equality of opportunity. Our research narrows this down by focusing on so-called bridge-years, which combine multiple educational levels into one classroom for the first year of high-school. We use this bridge-year as a proxy for the broader concept of diversity in education. However, one should keep in mind that there is not a one to one relationship between the effect, or the absence of it, of bridge-years on educational level segregation and the effect of diversity on educational level segregation. Our research specifically focuses on the relationship between the number of bridge-years offered by high-school within Dutch municipalities and the educational level segregation, as measured by the Dutch statistical agency, within each municipality, over the years 2015-2020. The outcome and treatment variables in our analysis are both very specific concepts, and one should thus be careful with extrapolation to broader concepts or different regions.

In our complete analysis, there are only two instances in which we find a, doubtful, significant result. First, we find a significant coefficient in our pooled-OLS Here, we find the educational level segregation to drop by 0.084 points when the number of bridge-years increases by one. In a municipality of one thousand people that is composed of fifty percent highly-educated and fifty percent low-educated individuals, these results suggest that the average individual increases the diversity of their network by 4.2 percent. However, due to unobserved background characteristics, these results are likely biased. Based on the limitations of the pooled-OLS, we cannot reject the null hypothesis of no effect. These results do arouse curiosity aboput what would happen with more extensive data over a longer period. All other coefficients we find for bridge-years are insignificant, meaning we cannot reject the null hypothesis of no effect of the number of bridge years on educational level segregation. Through our control variables, we obtain some other, unintended, significant results. A recurring finding in all our different versions of the fixed effects model is that there is a significant coefficient for the number of non-Western migrants. Here, the number of non-Western migrants as a percentage of the total population seem to increase educational level segregation.

Due to the limitations of this research, we cannot provide any trustworthy policy implications based on our results. One should rather see this paper as an incentive for asking further questions. Further research could more precisely analyze the relationship between bridge-years and educational level segregation by obtaining a larger data set over a larger period. Additionally, the introduction of subsidies that promote Dutch high-schools to offer bridge-years in 2021, as mentioned in our literature review, could be an interesting intervention to analyze. Ideally, future research could work with a government to randomly assign subsidies for bridge-years to municipalities. This randomized control trial design could, in the long run provide a clearer causal answer to our research. However, this research could be difficult to implement and might introduce ethical concerns. Moreover, our research could motivate interest in the relationship between migrants and educational level segregation.

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A The Dutch educational system

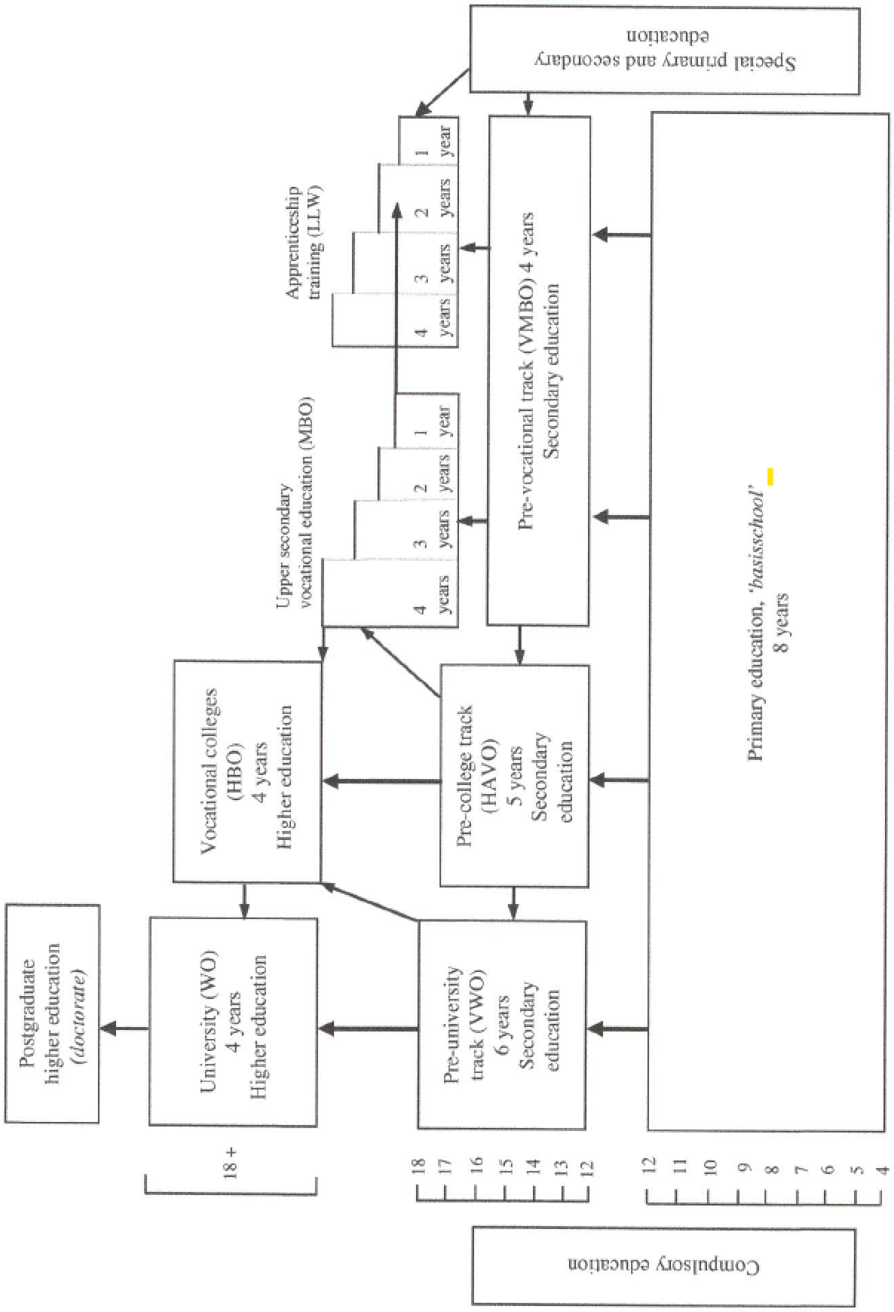

Figure 6: Box-graph of the dutch educatonal system (Velden, Büchner, and Traag, 2014)