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**Empirical Essays on Education and
Health Policy Evaluation**

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Introduction

Estimating the causal effect of a policy is a fundamental challenge to policy evaluation. It is vital to separate the causal effect of a policy from changes driven by other covariates in order for policy makers to understand the costs and benefits related to a certain policy. This thesis contains three standalone papers that demonstrate the use of econometrics models to estimate the causal effects of education and health policies. These studies share the common theme in two dimensions. First of all, regarding the topics, they are striving to understand how an individual's human capital are affected by education and health policies. Second, in terms of methodology, the empirical results of these studies are produced by the match of identification of quasi-random variation in observational data, unique datasets and appropriate choices of econometrics models. They are all aligned to the same goal to provide a better understanding of common labour economics topics.

The first chapter estimates how the quasi-random variation in the proportion of school peers from the same Region-of-Origin (ROO) affects the probability for an individual to have a partner from the same ROO (endogamy) and the proportion of colleagues from the same ROO in the same workplace later in life (work segregation). This is answered by a fixed effect regression model, together with a unique dataset that merges different register databases from Sweden. The dataset includes ROO background, education, labour market outcomes and multigenerational linkages of the universe of ninth graders from the school years 1988 to 2000. Main findings show both statistically and economically significant results for immigrants: one standard deviation increase in the proportion of same-ROO peers increases the probability of an immigrant to have a partner from the same ROO by over 7% and increases the probability of having same-ROO colleagues by over 12%.

The second chapter is the heterogeneous causal impact of a nationwide information campaign of salt iodisation in 1936 on individuals with different socioeconomic status (SES) at birth, using a difference-in-differences regression model. This is made possible by using a novel dataset that merges the Swedish population registers with two unique historical data sources of pre-intervention iodine deficiency prevalence and SES at a highly disaggregated level, thereby being able to trace the educational attainment and labour market outcomes of these individuals during their adulthood. The results of this study, strikingly, show that while the intervention increased human capital for individuals from families with high SES, those from families with

low SES did not benefit. While the intervention led to an 8% increase in an individual's probability of having a high-skill occupation, modest effects are found for an individual's education. The social gradients shown by these results are critical for governments who use salt iodisation to improve human capital in the population.

The third chapter exploits a sharp policy change in Hong Kong when half of the secondary schools were mandated to change the teaching language from English to Chinese from the school year 1998-1999 onward. The policy impact is identified with regression discontinuity design and the main dataset is the census data in 2011. The results show that mother tongue education increases an individual's unemployment rate and decreases his or her likelihood of having high-paid occupation. However, due to limitation of the dataset, the study finds insignificant but imprecise estimates of the differential impact on the likelihood of university attendance between individuals with different socioeconomic status, which has been a controversial topic in the society in Hong Kong. The first set of results, nevertheless, warrants a discussion of whether mother tongue education enhances learning or worsens an individual's labour market outcomes.

Chapter 1

Impact of School Peers from the Same Region-of-Origin on Endogamy and Work Segregation: Evidence from Sweden*

Debbie Lau[†]

Abstract

This study estimates how the quasi-random variation in the proportion of school peers from the same Region-of-Origin (ROO) affects the probability for an individual to have a partner from the same ROO (endogamy) and the proportion of colleagues from the same ROO in the same workplace later in life (work segregation). This is answered by a fixed effect regression model, together with a unique dataset that merges different register databases from Sweden. The dataset includes ROO background, education, labour market outcomes and multigenerational linkages of the universe of ninth graders from the school years 1988 to 2000. Main findings show both statistically and economically significant results for immigrants: one standard deviation increase in the proportion of same-ROO peers increases the probability of an immigrant to have a partner from the same ROO by over 7% and increases the probability of having same-ROO colleagues by over 12%.

Keywords: policy evaluation, causal inference, register data, segregation, endogamy

JEL Classification: I21, J12, J15, N34

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1 Introduction

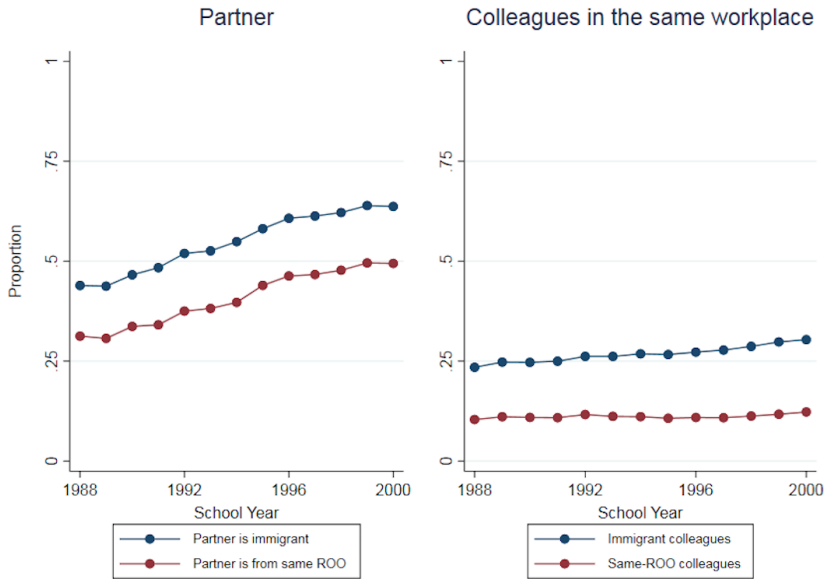
A growing body of literature has found negative impacts of segregation. Ample evidence has shown that segregation faced by immigrants in different aspects of their lives can affect their labour market outcomes¹ (Celikaksoy, Nekby, & Rashid, 2010; Elwert, 2018; Elwert & Tegunimataka, 2016; Åslund & Skans, 2010). This has been suggested as the cause for income inequality between immigrants and native-born individuals (Elliott & Lindley, 2008; Greenwood, Guner, Kocharkov, & Santos, 2014; Åslund & Skans, 2010). Even worse is that these negative impacts can be passed on to the next generation. Segregation is on the rise in Sweden, especially in terms of choosing a mate. Figure 1 shows the segregation in terms of partner and work faced by immigrant ninth graders after graduation, by school year. The graph on the left shows that the probability for immigrants to have an immigrant partner has been markedly increasing across school cohorts. Within 15 years of graduation, 55.8% of these immigrant ninth graders had an immigrant partner, and 41.6% had an immigrant partner coming from the same region-of-origin (ROO) as themselves. While the graph is flatter for workplace segregation, immigrants tend to be employed in workplaces with twice as many immigrants on average as the workplaces where natives are employed. This indicates for the need to understand the root cause of segregation.

For decades, decreasing school segregation has been a top agenda of educational policy makers (Coleman, 1968). Recent research has shown the connection between school segregation and segregation later in life (Merlino, Steinhardt, & Liam, 2019). Even though the exact mechanism is still not clear, one possible explanation is that early exposure to peers with immigration backgrounds can change one's attitude toward and acceptance of immigrants. Researchers, however, face challenges in evaluating the impact of school diversity on segregation faced by immigrants later in life. First of all, it is difficult to measure the degree of segregation both in school and later in life. Self-reported friendships and interracial relationships have been common measures of segregation (Baker, Mayer, & Puller, 2011; Cargargo, Stinebrickner, & Stinebrickner, 2010; Marmaros & Sacerdote, 2006; Merlino et al., 2019), but they are subject to measurement errors and manipulation. The second empirical challenge is endogeneity of the possible measures of school segregation. Above all, a common problem in most of the related studies is that they use survey data and cannot evaluate how

¹A recent study in Sweden by Böhlmark and Willén (2020), however, finds mixed results on the effects of ethnic residential segregation on education and labor market outcomes of immigrants and natives.

the immigrants are affected because of small sample size. They usually focus on how the natives are affected instead, which does not provide policy makers with sufficient understanding of the impacts on immigrants in order to pass beneficial policies.

Figure 1: Segregation that immigrant ninth graders experience after graduation, by school year



Note: These two graphs use the main sample in this study which will be further illustrated in the data section. The main sample includes all ninth graders in each school year who have a partner or work record within 15 years after the ninth grade.

This study attempts to contribute to the literature by helping to fill these three gaps. I compose a unique data-set by merging different register databases from Sweden, including ROOs background, education, labour market outcomes and multigeneration databases to study: how the quasi-random variation in the proportion of school peers from the same ROO affects the probability for an individual to have a partner from the same ROO (endogamy) and the proportion of colleagues from the same ROO in the same workplace later in life (work segregation). This dataset captures the universe of ninth graders from the school years 1988 to 2000, which makes it possible to have enough statistical power to study individuals with different immigration backgrounds in a way that other studies could not accomplish. Second, because the endogamy and work segregation measures are calculated with register data, this circumvents the measurement errors

and manipulation problem. Moreover, I exploit the variation in the proportion of school peers from the same ROO in the same school and across school years by controlling for the school-specific time trend. It is reasonable to assume that this variation is exogenous, as parents choose schools but not a particular school year to enroll their children. Last but not least, thanks to this comprehensive dataset, this study is able to investigate the impacts of school segregation on endogamy and work segregation in the same group of individuals, which enables me to examine school segregation as a policy instrument across different spectrums of segregation faced by immigrants later in life.

My main findings are that immigrants with more school peers from the same ROO tend to have partners from the same ROO and work in a workplace with more colleagues from the same ROO, while the natives are not affected by the proportions of immigrants and natives among school cohorts. The magnitude of the effects on immigrants are both statistically and economically significant: one standard deviation increase in the proportion of same-ROO peers increases the probability of an immigrant to have a partner from the same ROO by over 7% and increases the probability of having same-ROO colleagues by over 12%. This suggests that school segregation can lead to long-term effects of segregation on immigrants. My results also show that whether school peers from the same ROO have the same or opposite gender as an individual, equally large effects on immigrants occur. Therefore, having same-ROO school peers affects individuals through altering both friend networks and the pool of potential partners. Furthermore, by analysing the differential impacts on different groups of individuals, I have found that (1) better adaptation to the host country does not mean that an immigrant student will be less affected by having school peers from the same ROO; (2) the differences in appearance of non-native individuals do not drive the results; and (3) family environment is a possible reason behind the impacts on immigrants. These findings provide valuable insights for policy makers on how immigrants define their social network as opposed to the traditional view of considering immigrants from different ROOs as a group, how segregation at school continues into adulthood in different important aspects of life, and the mechanisms behind these impacts.

The remaining sections are organized as follows: section 2 summaries related literature; section 3 describes the identification strategy; section 4 explains how the merged databases were used to construct different components in the identification strategy; section 5 presents the empirical results and investigates the mechanisms behind the results; and section 6

concludes.

2 Literature Review

This section summarises different strands of literature relevant to the current study. First, I present the main findings of studies on the effects of interracial relationships on various outcomes to illustrate the importance of understanding the determinants of interracial relationships. One of the main outcomes of the current study is endogamy in terms of ROOs. However, the usual outcome in the literature is the opposite, interracial relationships. Second, among possible determinants of interracial relationships, I continue to discuss the effects of school peers' race on one's probability of having an interracial relationship and related outcomes by economists. They mostly exploit quasi-random variation in the school peers' race, which provides understanding on the causal impacts of school peers on one's relationship and related outcomes.

Endogamy, a term for the romantic union of individuals with the same characteristics, has always been prevalent across cultures. This can be explained by individuals' inherent preference to choose those with similar characteristics (Kalmijn, 1998) for romantic relationships. Two sociological theories, social exchange theory and contact hypothesis, attempt to explain the circumstances under which interracial relationships happen (Allport, 1954; Davis, 1941; Fryer Jr., 2007; Merton, 1941). Social exchange theory suggests that interracial relationships occur when minorities possess superior characteristics that enable them to find native spouses with lower socioeconomic status. For instance, a minority individual who has attained a high level of education may be paired up with a native with a lower level of education. The contact hypothesis holds that more exposure of the natives to the minorities and vice versa can alter the attitudes of both groups toward each other. By reducing prejudice between groups, interracial relationships become more probable. This theory underlies the policy of increasing diversity in schools to enhance assimilation of minorities.

Interracial relationships play an important role in assimilation. These relationships can enhance the minority spouses' human capital in the local labour market and institutions (Meng & Gregory, 2005; Nekby, 2010), such as through improving the minority spouses' knowledge of the local language and customs, and as well as expansion of their social networks through the native spouses' contacts. It is empirically challenging to estimate the causal impacts of interracial relationships because they are endogenous, and simultaneous bias can exist if the outcomes are measured before in-

terracial relationships are formed. Despite these challenges, studies have attempted to estimate the causal impacts of interracial relationships. These studies focus on the effects on immigrants or minorities. While many of these studies based on U.S. data have found that being in an interracial relationship affects one's future regarding fertility, family size, and labour market outcomes (Angrist, 2002; Bleakley & Chin, 2010; Meng & Gregory, 2005; Ponomareva, Chou, & Alex, 2018), a study based on Swedish data shows that the intermarriage premiums for immigrants are largely due to selection (Nekby, 2010). A relevant stream of research has found a link between education and income inequality in assortative mating (Cancian & Reed, 1998; Greenwood et al., 2014; Schwartz, 2010).

Economics studies have found mixed evidence on the causal impacts of school peers' race on one's relationship and related family outcomes. The most relevant study to this one is that by Merlino et al. (2019), which exploits the idiosyncratic variation in the proportion of black peers in the same school across years to study this question. This is an identification strategy originally adopted by Hoxby (2000) to study the effect of school peers. Regarding external validity, one advantage of using this type of idiosyncratic variation is that it is common and not driven by a specific policy or immigrant shock that triggers the proportion of minorities in schools in a given period and place. Merlino et al. (2019) show that in the United States, having more black peers of the same gender increases the probability that whites will have relationships with blacks later in life. They also find that the results are mediated by the change of whites' attitude toward blacks, but not by an increase in meeting opportunities. Also in the United States, Gordon and Reber (2018) and Shen (2018) use the quasi-random variation in the proportion of black peers in school resulting from court-ordered school desegregation, which was carried out gradually in different counties, to study the change in proportion of mixed-race births among all births at the county level. However, both studies found inconclusive results for the causal impacts of school peers' race.

Three related outcomes that economists have been investigating in relation to the effects of school peers' race are one's attitude toward minorities, friend networks and labour market outcomes. These outcomes are possible mechanisms through which school peers' race affects the probability of interracial relationships. Such studies often look at random assignment of one's roommates at the start of college or other types of education. While both Boisjoly, Duncan, Kremer, Levy, and Eccles (2006) and Carrell, Hoekstra, and West (2015) find that the quantity of black peers affects whites' acceptance of blacks, one additional finding of Carrell et al. (2015) is that

the quality of black peers also significantly affects white males' acceptance of others from minority groups. Similar positive results are found regarding the effects of having black peers in school on increasing the probability that whites will have blacks in their self-reported friend networks (Baker et al., 2011; Camargo et al., 2010; Marmaros & Sacerdote, 2006). Billings, Deming, and Rockoff (2012) also study the effects of school peers' race on crime and other labour market outcomes in relation to changes in U.S. school segregation policy. One caveat is that these studies and others on interracial relationships and birth outcomes use a variety of measurements of different degrees of assimilation (Merlino et al., 2019). For example, an effect on self-reported attitude toward blacks by a white person does not necessarily mean that the white individual will choose to marry a black person later. Therefore, these results should be interpreted with care when using them to infer anything about minorities' assimilation in a society.

In conclusion, there is mixed evidence on the causal effects of interracial relationships on one's labour market outcomes and to what degree school peers' race is a factor in interracial relationships. Among the sparse evidence, the majority of studies use U.S. data, which mostly define black people as the single minority group, and thus the results cannot be applied directly to situations involving other minority groups and in other countries. Moreover, because of the small sample size for the minority groups, most studies are able to draw conclusions only on the effects on natives but not on minorities. The current study attempts to help fill these gaps in the literature.

3 Identification Strategy

For individual i with ROO j who was in the ninth grade at school s in year t , equation (1) is estimated:

$$y_{ijst} = \beta_0 + \beta_1 * \text{SameROO}_{jst} + \gamma_s + \lambda_t + \mathbf{x}_{ist} + \gamma_s * t + e_{ijst} \quad (1)$$

There are four main dependent variables: $\text{HavePartner}_{ijst}$, Endogamy_{ijst} , HaveWork_{ijst} and Colleague_{ijst} . The first outcome, $\text{HavePartner}_{ijst}$, is a dummy variable equal to one if an individual has at least one partner within 15 years after ninth grade. Endogamy_{ijst} is a dummy variable equal to one if an individual's first partner has the same ROO as the individual. Details on the ROO information are provided in section 4. Having a partner refers

to both marriage and cohabitation as recorded in the register data².

HaveWork_{ijst} is a dummy variable indicating whether an individual has at least one work record within 15 years after graduating from ninth grade. Colleague_{ijst} is the proportion of colleagues from the same ROO as individual i in a workplace. A workplace is defined as a specific work location for a particular company. For both Endogamy_{ijst} and Colleague_{ijst} as dependent variables, I restrict the sample to be those either having had at least one partner or one work record. This is equivalent to studying the pair of outcomes of whether one is employed and, conditional on employment, how much income one has, for instance.

In the closely related literature using interracial relationships or mixed-race births as dependent variables, Gordon and Reber (2018), Merlino et al. (2019), and Shen (2018) do not make a distinction as to whether their dependent variables are conditional on whether an individual has a relationship or children. These studies assume that the independent variable does not affect the probability of an individual to have a relationship or birth, which is not reasonable, as one mechanism through which school peers' composition can affect an individual's partner outcome is by altering the potential pool of partners. Therefore, I explicitly use both $\text{HavePartner}_{ijst}$ and Endogamy_{ijst} to understand whether the independent variable affects an individual's probability of having a relationship and, conditional on this, how the partner characteristics are affected. The same holds true for the pair of outcomes for HaveWork_{ijst} and Colleague_{ijst} .

SameROO_{jst} is the "leave-me-out" proportion of peers from the same ROO as individual i in ninth grade, as given below. Referring to the denominator of the proportion below, peers are defined as all ninth graders in the same school in the same year, excluding the individual him- or herself. This proportion is constant for students from the same ROO in the same school in the same year. Therefore, the subscript for SameROO is jst . This variable ranges from zero to one. The main dependent and independent variables are defined using register data on each individual's partner, colleagues, and school peers, as well as all of their ROOs. This is in contrast to the literature, which more often than not uses survey data with self-reported information on partners, work colleagues, and school peers. This is one merit of this study over similar literature and more of these advantages will be discussed in section 4.

²Same-sex marriage in Sweden was legalized on 1 May 2009. Relationships of same-sex couples have been always registered, even before the legalization, and thus both are captured by this variable (Statistics Sweden, 2016).

Proportion of peers from same ROO:

$$\frac{\text{Size of cohort from same ROO} - 1}{\text{Size of cohort} - 1}$$

In another specification, shown in equation (2), the proportion of immigrant peers in ninth grade is used as the main independent variable. It is interesting to compare the results with these two main independent variables because if immigrant students consider immigrant peers as a close social circle, the impact of having additional immigrant peers should be similar to the impact of having additional immigrant peers coming from the same ROO. The difference in the results can thus indicate whether immigrants differentiate immigrants from the same ROO from other immigrants for inclusion in their social networks.

$$y_{ijst} = \alpha_0 + \alpha_1 * \text{Immigrant}_{jst} + \gamma_s + \lambda_t + \mathbf{x}_{ist} + \gamma_s * t + u_{ijst} \quad (2)$$

In both equation (1) and (2), γ_s is the school fixed effects, which control for time-invariant characteristics of each school. At the same time, unobservable parental characteristics that affect their school choices based on these time-invariant school characteristics and also the outcomes of individual i will not bias the estimation of β_1 for equation (1) and α_1 for equation (2). Additionally, the school fixed effects act as controls for different geographic areas, and thus the socioeconomic backgrounds of families. λ_t is the year fixed effects, which control for common shocks for those studying in different schools in the same year. \mathbf{x}_{ist} includes gender, Female_i and the total number of ninth graders at school s in year t , GradeSize_{st} . $\gamma_s * t$ is the school-specific linear time (school year) trend³. In addition, ROO fixed effects are included for estimation of equation (1) and (2) for immigrants. This model assumes that even though parents may choose to let their children to attend schools with more (or fewer) students from the same ROO, they are not able to take into account the idiosyncratic variation in the proportion of students from a ROO within a school across different school

³This component is not added for estimation of the subsamples of natives and non-adoptees because the computation does not work for such large sample sizes. Note, however, that the addition of this component does not change the results significantly for other subsamples.

years.

$\hat{\beta}_1$ in equation (1) is the estimated effect of having a higher proportion of peers in ninth grade from the same ROO on one’s relationship and work outcomes. Equivalently for equation (2), $\hat{\alpha}_1$ is the estimated effect of immigrant peers. Because of the fact that the main independent variable, SameROO_{jst} , ranges from zero to one, it is not meaningful to interpret $\hat{\beta}_1$ directly as the effect of changing SameROO_{jst} from zero to one, as an individual would not likely experience a change from having no peers from the same ROO to having all peers from the same ROO. Alternatively, $\hat{\beta}_1$ is interpreted in terms of effect in percentage points, as below, which represents the effect of one standard deviation increase in the within-school proportion of peers from the same ROO. Furthermore, the effect size will be calculated for better understanding of the magnitude of the coefficients and is defined as the effect in percentage points divided by the mean of y_{ijst} , also shown below. These are also valid for $\hat{\alpha}_1$ in equation (2).

$$\text{Effect in percentage points} = \hat{\beta}_1 * 100 * \text{Within school s.d.}$$

$$\text{Effect size} = \frac{\text{Effect in percentage points}}{\text{Mean of } y_{ijst} \text{ in percentage points}}$$

4 Data

4.1 Datasets

The base sample is composed of all ninth graders in Sweden from 1988 to 2000 recorded in the register of graduation from compulsory school (*årskurs 9 elevregistret*)⁴. More on the choice of years selected will be provided at the end of this section. Without grade skipping or repetition, ninth graders are 16 years old. In total, there are 1,588 schools and 1,310,330 ninth graders. The average cohort size, which is the number of ninth graders in a school in a year, is 121 students. This dataset is merged with the total population register (*registret över totalbefolkningen*), which contains information on

⁴The register data also include enrollment and test score records at the high school level (*gymnasial*). However, the timeframe is much shorter for the high school data from which a reasonably sized school cohort can be identified. It should be noted that the age at which school peers are identified using the ninth graders’ data is comparable to the age studied by others, such as Merlino et al. (2019).

immigrant status and the ROOs of all students. This allows me to calculate the proportion of peers from the same ROO for individual i . section 4.2 presents details on the classification by immigrant status and ROO. For students who repeated ninth grade, the peers they were exposed to when they were in ninth grade for the first time are used to construct the main independent variable. The base sample is also linked to the integrated database for labour market research (longitudinell integrationsdatabas för sjukförsäkrings och arbetsmarknadsstudier, or LISA) from 1990 to 2015 to obtain information on partner and workplaces. For each individual with more than one partner, the ROO information for the first partner is used to define the relationship outcome. For each individual who has more than one work record, the first workplace is used to define the workplace outcome.

Forming the base sample with these register datasets gives this study advantages over other studies discussed in section 2. First of all, the ROOs used in this study were not self-reported. The other studies generally used ROOs that were provided by the individuals via surveys. Their answers may not be true, however, and often individuals provided multiple ROOs, which means that the researchers had to make decisions about which ROO to select from those provided by an individual. Another advantage of this study is that individuals' partners and school peers have been identified from the datasets, whereas for studies relying on surveys, the data may have been inaccurate, as individuals could have incorrectly remembered or manipulated the information about their partners and school peers. Furthermore, cohabitation is recorded in the Swedish data if the couples have children, allowing this study to measure relationship outcomes more precisely. However, these data are incomplete because they do not include cohabitating couples without children or relationships without cohabitation. Last but not least, the base sample is sufficiently large for precisely estimating the impact on immigrants of peers from the same ROO. The sizes of the subsamples of immigrants in other studies are often too small to produce precise estimations of this impact. Because of this, most studies focus on how the proportion of immigrants among school peers can affect relationship outcomes for the natives. However, it is also important to understand the differential impacts on the natives and the immigrants. The large sample size of this study allows insights into the differential impacts on first- and second-generation immigrants, as well as adoptees.

I restrict my sample to include only ninth graders up to the school year 2000, because students who recently attended ninth grade are too young to have a registered relationship or a work record. According to Statistics

Sweden (2019b) and Statistics Sweden (2013), the average age at which Swedish individuals have a registered relationship is around 30 years old. My sample is quite consistent with the current dataset. Figure A.1 in the Appendix shows the distribution of everyone in the ninth grade register who has at least one relationship. The red line indicates the school year 2000. The later an individual attended ninth grade, the lower the probability that he or she has a relationship. Figure A.2 in the Appendix shows the distribution of everyone in the ninth grade register who has at least one work record. The standard age of students graduating from high school is 18 years old. The red line indicates the school year 2012, which corresponds to the last school cohort that become 18 years old within the data timeframe. It can also be observed that those who are younger have less probability of having a work record. In the descriptive statistics and estimation that follows, I restrict the relationship and work outcomes to those observed within 15 years after ninth grade so that different school cohorts can have the same data timeframe.

4.2 Region of Origin (ROO)

For the purposes of this study, both first- and second-generation immigrants are defined as immigrants. First-generation immigrants are individuals born outside of Sweden, with both parents born outside of Sweden as well. Second-generation immigrants are individuals born in Sweden, but with both parents born outside of Sweden. For first-generation immigrants, their places of birth are used as their ROOs, while for the second-generation immigrants, their fathers' places of birth are used as their ROOs. In the latter case, the multigenerational register (multigenerationsregistret) provides the link between individuals and their parents. The immigrant status and ROOs of an individual's partner and colleagues are defined in the same way. Adoptees whose adoptive fathers were born overseas are defined as second-generation immigrants. However, most of the adoptees are defined as natives because their adoptive fathers were born in Sweden.

To define the ROOs, countries are grouped into regions of birth from the raw data. The main dataset includes nine regions of birth. Some regions of birth were reported for only a small number of individuals, so I consolidated them into five ROO categories for immigrants. The ROO category for natives is Sweden. Table 1 shows which countries belong to each ROO category. The right-hand column lists the countries most immigrants come from for each ROO. For instance, for Africa, the dominating country is Ethiopia, with 37% of African immigrants coming from this country. The country percentages are calculated based on statistics of first generation

immigrants in the 1990s, because most of the first-generation immigrants in my main dataset came to Sweden during the 1980s and 1990s. There is not a great difference between the statistics from the 1980s and the 1990s. It can be seen that countries with similar development levels and cultures fall into the same ROO category.

Table 1: Categories of Immigrants' ROOs

ROO	Countries
Africa	Ethiopia (37%), Morocco (10%), Tunisia (7%), Egypt (5%), Somalia (5%), Uganda (5%), Gambia (5%), Algeria (4%), South Africa (3%), Kenya (2%)
Asia	Iran (32%), Lebanon (13%), Iraq (8%), India (7%), South Korea (7%), Vietnam (5%), Syria (5%), Thailand (4%), Sri Lanka (4%), China (3%)
Eastern Europe	Yugoslavia (55%), Turkey (32%), Soviet Union (10%) Switzerland (2%)
Scandinavia without Sweden	Finland (68%), Norway (17%), Denmark (14%), Iceland (2%)
South America	Chile (62%), Colombia (11%), Uruguay (5%), Argentina (5%), Brazil (5%), Bolivia (4%), Peru (4%), Ecuador (1%), Venezuela (1%)
Other Western countries	Germany (19%), Poland (18%), Hungary (8%), Greece (7%), U.S. (7%), Estonia (6%), UK (6%), Romania (4%), Czechoslovakia (4%), Austria (3%)

Source: Statistics Sweden (2019a).

Note: The percentages in parentheses represent the proportions of first-generation immigrants coming from each country among those listed for a ROO for using 1990s data. The percentages are rounded to the nearest whole number. Countries in each ROO category are listed in descending order by percentage. For ROOs with more than 10 countries where the proportions are available and are at least 1% after rounding, the first 10 countries are listed.

Table 2 presents the characteristics of the entire sample. The sample size is 1,310,330, of which 48.8% are female. About 89.5% of the ninth graders are native and 10.5% are immigrants, including 5.7% first-generation and 4.8% second-generation immigrants. The lower panel of table 2 shows the immigrants' ROOs. The largest proportion of immigrants comes from Scandinavia without Sweden, followed by Eastern Europe and Asia.

Table 2: Characteristics of the Sample

Variable	Obs.	Mean	Std. Dev.
Female	1,310,330	.488	.5
Native	1,310,330	.895	.307
Immigrant	1,310,330	.105	.307
First-generation	1,310,330	.057	.232
Second-generation	1,310,330	.048	.214
Adoptee	1,310,330	.021	.142
ROOs of immigrants			
Africa	138,073	.055	.229
Asia	138,073	.21	.407
Eastern Europe	138,073	.217	.412
Scandinavia without Sweden	138,073	.26	.439
South America	138,073	.07	.256
Other Western countries	138,073	.188	.39

4.3 Main Variables

Summary statistics of the main dependent and independent variables are provided separately for natives and for immigrants from different ROOs as a whole in table 3 and for adoptee and nonadoptee natives in table 4. I further divide immigrants into first- and second-generation immigrants in the lower panel of table 3. Differential impacts on each of these subgroups will be studied. For the summary statistics on partners and colleagues, I restrict the sample to those having at least one partner or work record within 15 years after ninth grade. This is reflected by the smaller sample sizes of these variables. Also, when calculating the effect in percentage points, I use the within-school standard deviation of the peers variable for the subsample used for estimation for different regressions. However, these within school standard deviation is very similar to that reported here for the entire sample.

Among the natives who have a partner within 15 years of ninth grade, 91% have a native partner as their first partner and 9.2% have an immigrant partner. Immigrants have a much greater probability of having an immigrant partner within 15 years of ninth grade, at 55.8%. It is worth noting that for these immigrants having immigrant partners, their partners tend to come from the same ROO as they do. Table 3 shows that 41.6% of immigrants with partners have partners who come from the same ROO meaning that 74.6% ($\approx \frac{41.6\%}{55.8\%}$) of the immigrant-immigrant relationships are between immigrants from the same ROO. However, because of data limitations, it

is not possible to determine whether these same-ROO partners are from the same *countries*. Notwithstanding this limitation, one important insight from the information given in table 3 is that for immigrants, the smaller subsets of those coming from the same ROO form the pool of partners, rather than all immigrants as a whole. Regarding work segregation, immigrants are twice as likely as natives to have immigrant colleagues at their workplace.

Referring to the lower panel of table 3, first generation immigrants have a much higher probability of having immigrant or same-ROO partners than second-generation immigrants. For those having partners, 65.9% of first-generation immigrants have immigrant partners, and 78.8% of these immigrant partners come from the same ROO. Even though the probability is lower for second-generation immigrants, they still have a much high probability of having immigrant partners than natives. There is, however, no significant difference in the proportion of immigrant colleagues at a workplace between these two types of immigrants. Interestingly, table 4 shows that there is no remarkable difference between nonadoptive natives and individuals adopted by natives, even though they have discernible differences in appearance.

Regarding the main independent variable, for native ninth graders, on average, 91.4% of their peers are natives, regardless of gender. Gender is also not a factor in considering peers for immigrant ninth graders. Therefore, tables 3 and 4 only report data on peers of both genders combined. The standard deviation of the proportion of peers of both genders from same ROO is 14.6 percentage points, resulting from the variations across schools. The within-school standard deviation accounts for the variations within a school across different years. It is comparatively small, at around 3.6 percentage points for the natives. For immigrants, on average, around 8% of their peers are from the same ROO as they are. The between-school standard deviation of this proportion is 9.3 percentage points, while the within-school standard deviation is 5.9 percentage points. Worth noting is that the within-school standard deviation is much larger than those found in the closely related study, Merlino et al. (2019).

Table 3: Summary Statistics of Dependent and Main Independent Variables by Immigration Status

	Mean	Between-school s.d.	Within-school s.d.	N
Native				
Have at least one partner	.532	.100	.496	1,172,257
Partner is immigrant	.0922	.0935	.287	623,745
Partner is from same ROO	.908	.0935	.287	623,745
Have at least one work record	.972	.085	.164	1,172,257
Colleagues are immigrants	.114	.06	.123	1,139,737
Colleagues from same ROO	.886	.06	.123	1,139,737
Immigrant peers, both genders	.0861	.146	.0363	623,745
Peers from same ROO, both genders	.914	.146	.0363	623,745
Immigrant				
Have at least one partner	.503	.190	.491	138,073
Partner is immigrant	.558	.261	.459	69,444
Partner is from same ROO	.416	.234	.464	69,444
Have at least one work record	.910	.122	.281	138,073
Colleagues are immigrants	.27	.105	.237	125,579
Colleagues from same ROO	.112	.0688	.201	125,579
Immigrant peers, both genders	.261	.168	.071	69,444
Peers from same ROO, both genders	.0804	.0927	.0591	69,444
First generation immigrant				
Have at least one partner	.510	.224	.488	75,049
Partner is immigrant	.659	.295	.439	38,283
Partner is from same ROO	.519	.277	.469	38,283
Have at least one work record	.890	.149	.304	75,049
Colleagues are immigrants	.276	.114	.239	66,792
Colleagues from same ROO	.105	.0771	.2	66,792
Immigrant peers, both genders	.264	.164	.0728	38,283
Peers from same ROO, both genders	.0745	.0823	.0592	38,283
Second generation immigrant				
Have at least one partner	.494	.226	.488	63,024
Partner is immigrant	.433	.272	.441	31,161
Partner is from same ROO	.29	.214	.414	31,161
Have at least one work record	.933	.141	.245	63,024
Colleagues are immigrants	.263	.119	.231	58,787
Colleagues from same ROO	.119	.0886	.199	58,787
Immigrant peers, both genders	.258	.162	.0653	31,161
Peers from same ROO, both genders	.0876	.0827	.0541	31,161

Note: The summary statistics of relationship and work outcomes use the subsample of individuals who have had at least one partner or work record within 15 years of ninth grade. The summary statistics of the independent variables use the subsample of individuals who have had at least one partner. The summary statistics of the independent variables for the subsample of individuals who have at least one work record are similar, and thus not reported here.

Table 4: Summary Statistics of Dependent and Main Independent Variables by Adoption Status

	Mean	Between-school s.d.	Within-school s.d.	N
Nonadoptee				
Have at least one partner	.721	.135	.444	1,147,083
Partner is immigrant	.1	.092	.298	827,065
Partner is from same ROO	.9	.092	.298	827,065
Have at least one work record	.978	.084	.144	1,147,083
Colleagues are immigrants	.114	.0603	.123	1,122,400
Colleagues from same ROO	.886	.0603	.123	1,122,400
Immigrant peers, both genders	.085	.144	.0356	827,065
Peers from same ROO, both gender	.915	.144	.0356	827,065
Adoptee				
Have at least one partner	.481	.229	.482	25,174
Partner is immigrant	.113	.169	.295	12,103
Partner is from same ROO	.887	.169	.295	12,103
Have at least one work record	.958	.127	.192	25,174
Colleagues are immigrants	.127	.081	.127	24,119
Colleagues from same ROO	.873	.081	.127	24,119
Immigrant peers, both genders	.0847	.112	.0323	12,103
Peers from same ROO, both genders	.915	.112	.0323	12,103

Note: Only adoptees who are natives are included here. Those who were adopted by immigrant fathers are defined as second-generation immigrants. The summary statistics of relationship and work outcomes use the subsample of individuals who have had at least one partner or work record within 15 years of ninth grade. The summary statistics of the independent variables uses the subsample of individuals who have had at least one partner. The summary statistics of the independent variables for the subsample of individuals who have at least one work record are similar, and thus not reported here.

5 Empirical Results

5.1 Main Findings

Tables 5 and 6 present the estimated β_1 of equation (1) and α_1 of equation (2) for the subsamples of natives and immigrants. In each table, columns (1) to (3) shows the results for relationship outcomes, while the remaining column shows the results for work-related outcomes. The first row provides the results when the proportion of immigrant peers is used as the main independent variable. For the results for immigrants, there is an additional independent variable where the proportion of same-ROO peers is

used, and these results are shown in the second row. For columns (2) and (3), only individuals who have had at least one partner within 15 years of ninth grade are included in the estimation. Similarly, for columns (5) and (6), only individuals who have at least one work record are included in the estimation. This is to separate the impacts of school peers on the extensive and intensive margins for both relationship and work outcomes. As similar results are found for males and females for both natives and immigrants, only the pooled results for both genders are presented here.

In table 5, the estimated result shows that one standard deviation increase in the proportion of immigrant peers in ninth grade is associated with a decrease of 0.01 percentage points in the probability that natives have at least one partner within 15 years of ninth grade. This translates to a decrease of 0.02% of the same outcome. The estimate is insignificant even at the 10% significance level. Column (2) shows that for natives who have a partner, one standard deviation increase in the proportion of immigrant peers in ninth grade is associated with an increase of 0.31 percentage points in the probability that natives have an immigrant partner. This translates to an increase of 3.39% of the same outcome. The estimate is significant at the 1% significance level. Similar results are found for the work outcome. These findings imply that having immigrant school peers does not affect the likelihood that natives have partners or jobs. This is particularly clear for the work outcome, where a precisely estimated close-to-zero coefficient is found. For those who have partners or jobs, having immigrant school peers only exerts limited impacts on the likelihood that natives have immigrant partners or colleagues.

For immigrants, the results are similar to those for the natives at the extensive margins for relationship and work outcomes. For instance, referring to the first column of table 6, one standard deviation increase in the proportion of same-ROO peers in ninth grade is associated with an increase of 0.72 percentage points in the probability that immigrants have a partner. This translates into an increase of 1.43% of the same outcome. Having immigrant school peers, however, affects relationship and work outcomes for immigrants at the intensive margin to a larger extent, especially when the immigrant school peers come from the same ROO as the individual. Column (2) of table 6 shows that while one standard deviation increase in the proportion of immigrant peers in ninth grade is associated with an increase of 0.89 percentage points in the probability that immigrants have a partner, conditional on having a partner, one standard deviation increase in the proportion of same-ROO peers increases the same outcome by 1.46 percentage points. The fact that the coefficients are larger in mag-

nitude for same-ROO peers than for immigrants in general as school peers implies that immigrants are affected to a larger extent by new peers from the same ROO than by peers from other ROOs. Furthermore, column (3) shows that one standard deviation increase in the proportion of same-ROO peers increases the probability that an immigrant has a partner from the same ROO by 3.23 percentage points, which is equivalent to an increase of 7.76% of the same outcome. Similar results are found for work outcomes. Column (6) shows that one standard deviation increase in the proportion of same-ROO peers in ninth grade is associated with an increase of 1.36 percentage points in the proportion of colleagues from the same ROO as the individual. This translates to an increase of 12.14% of the same outcome.

In summary, my main findings show that the proportion of immigrants among school peers in ninth grade does not affect natives' chances of having a partner or job within 15 years. For natives who have a partner or job, there are small effects on whether they have an immigrant partner or a job with a higher proportion of immigrant colleagues. The presence of other immigrants among school peers has greater effects on immigrants, not in their chances of having a partner or job, but by increasing the likelihood that they have an immigrant partner or a job in a workplace with more immigrant colleagues. This is especially true if one considers immigrant school peers from the same ROO. The results suggest that immigrants consider other immigrants from the same ROO as more important peers than other immigrants in general.

Table 5: Impacts of School Peers on Natives

	Relationship Outcome			Work Outcome		
	(1) Have partner	(2) Partner is immigrant	(3) Partner is from same ROO	(4) Have work	(5) Proportion of immigrant colleagues	(6) Proportion of same-ROO colleagues
Proportion of immigrant peers	0.003 (0.015)	0.086*** (0.014)	-0.086*** (0.014)	-0.024*** (0.006)	0.044*** (0.005)	-0.044*** (0.005)
Observations	1,172,257	623,745	623,745	1,172,257	1,139,737	1,139,737

Note: Each column presents the estimation results for different dependent variables. Each column shows the estimated α_1 of equation (2) for the main independent variable, where school peers refer to peers of both genders. Robust standard errors clustered at school-level are presented in parentheses. * $p < 0.10$, ** $p < 0.05$ and *** $p < 0.01$.

Table 6: Impacts of School Peers on Immigrants

	Relationship outcome			Work outcome		
	(1) Have partner	(2) Partner is immigrant	(3) Partner is from same ROO	(4) Have work	(5) Proportion of immigrant colleagues	(6) Proportion of same-ROO colleagues
Proportion of immigrant peers	-0.004 (0.035)	0.126* (0.050)	0.127* (0.053)	0.001 (0.021)	0.027 (0.019)	0.025 (0.017)
Proportion of same-ROO peers	0.121** (0.044)	0.247*** (0.045)	0.548*** (0.055)	0.084*** (0.022)	0.101*** (0.023)	0.238*** (0.021)
Observations	138,073	69,444	69,444	138,073	125,579	125,579

Note: Each column presents the estimation results for different dependent variables. Each column shows the estimated β_1 of equation (1) and α_1 of equation (2) for two definitions of the independent variables. For both definitions, school peers refer to peers of both genders. Robust standard errors clustered at school-level are presented in parentheses. * $p < 0.10$, ** $p < 0.05$ and *** $p < 0.01$.

5.2 Mechanisms behind Endogamy and Work Segregation

Here I estimate variations of equation (1) by defining peers in different ways, following Merlino et al. (2019). Peers are defined as ninth graders in the same school in the same year either of the same gender as individual i , excluding individual i , or of the opposite gender from individual i . Using the latter definition, one should not be deducted from either the nominator or the denominator because individual i is not counted in the number of students of the opposite-gender in the same school and year. Also, the independent variable now becomes SameROO $_{ijst}$ or Immigrant $_{ijst}$ (for equation (2)) with the subscript $ijst$, since this variable now varies at the individual level depending on the gender of the individual.

These two variables are both meaningful in the sense that the same-gender cohort tends to be the main potential pool for friendships, while the opposite-gender cohort forms the potential pool for the majority of relationships. The former may affect the relationship outcomes indirectly through the composition of one's social circle. These two variants of the independent variables are calculated as shown below. The definitions for the proportions of immigrant peers of the same gender as or opposite gender from individual i are calculated in the same way, by replacing same-gender cohort with immigrant cohort.

Proportion of peers from same ROO, same gender:

$$\frac{\text{Size of same-gender cohort from same ROO} - 1}{\text{Size of same-gender cohort} - 1}$$

Proportion of peers from same ROO, opposite gender:

$$\frac{\text{Size of opposite-gender cohort from same ROO}}{\text{Size of opposite-gender cohort}}$$

Table 7 shows the different estimated coefficients of β_1 in equation (1) for the subsample of immigrants when the dependent variable is the probability of having a same-ROO partner, conditional on having a partner, and the independent variables are the proportions of same-ROO peers of both genders, the same gender, or the opposite gender. The similarity of the coefficients across different definitions of the independent variables shown in the table implies that in Sweden under the data time frame, the influence of school peers of the same gender as or opposite gender from individual i has indistinguishable impacts on individual i . This differs from the results of Merlino et al. (2019), who find that relationship outcome is affected only

by proportions of school peers with the same gender in the United States, which implies that school peers affect an individual's relationship outcome through altering the individual's social circle, but not the potential pools of partners. However, my finding shows that this does not hold in Sweden. This similarity of the coefficients is found with the natives, with other outcome variables, and when the proportion of immigrant peers is used as the independent variable.

Table 7: The Impacts of School Peers on Immigrants (extracted results for only one outcome)

	(1)	(2)	(3)
	Partner is from same ROO		
Proportion of same-ROO peers of both genders	0.548*** (0.055)		
Proportion of same-ROO peers of same gender		0.441*** (0.045)	
Proportion of same-ROO peers of opposite gender			0.397*** (0.041)
Observations	69,444	69,444	69,444

Note: Each column presents the estimation results for different independent variables and shows the estimated β_1 of equation (1). Robust standard errors clustered at school level are presented in parentheses.

* $p < 0.10$, ** $p < 0.05$ and *** $p < 0.01$.

Moreover, the distinctive background characteristics of different groups of individuals together with the available data to identify these groups have provided the opportunity to further shed light on the mechanisms behind the influence of immigrant school peers on individual's relationship and work outcomes. First, immigrants are divided into first- and second-generation immigrants. Neither of these groups of immigrants have natives as parents. However, the second-generation immigrants were born in Sweden, and there is a greater chance that their parents settled in Sweden earlier than the parents of first-generation immigrants and before children

were born. The second-generation immigrants should be better adapted to the local culture and have better skills in the local language. Therefore, my hypothesis is that second-generation immigrants are affected less than first-generation immigrants by their same-ROO or other immigrant peers.

Tables 8 and 9, however, show mixed results. Equally small effects are found for the impacts on the extensive margin. A comparison of columns (1) and (4) in both tables shows that the magnitude of the impacts on the probabilities of having a partner or a job is slightly larger for first-generation immigrants. For instance, for first-generation immigrants, one standard deviation increase in the proportion of same-ROO school peers increases the probability of having a partner by 0.65 percentage points, whereas the probability increases by 0.78 percentage points for second-generation immigrants. These are equivalent to an increase of 1.27% and 1.58% of the outcome, respectively.

For second-generation immigrants, however, the proportion of same-ROO school peers has a greater effect on the relationship outcome. For them, one standard deviation increase in the proportion of same-ROO peers increases the probability of having a same-ROO partner by 3.34 percentage points, which is equivalent to an increase of 11.52%. For first-generation immigrants, the corresponding effect is an increase of 5.04% of the outcome. This does not hold for working outcomes, where one standard deviation increase in the proportion of same-ROO peers increases the proportion of same-ROO colleagues by 11.33% for first-generation immigrants and 11.95% for second-generation immigrants, which are very similar. These results show that the higher level of adaptation to the culture and better local language skills do not lessen the response of second-generation immigrants to new school peers sharing the same ROO, as compared with first-generation immigrants. In fact, they are affected to a larger extent regarding relationship outcome.

While both groups of natives, adoptees and nonadoptees, were raised by native parents, they differ at least in appearance. Any differential impacts on these two groups of natives can provide hints as to whether differences in appearance are a possible mechanism driving the impacts of having immigrant school peers. Tables 10 and 11 present the results for these two groups. Both sets of results are very similar for all variables, and they are all small in magnitude. This suggests that the differences in appearance between these two groups do not alter the extent to which immigrant school peers can affect their relationship and work outcomes. Alternatively, comparing adoptees and second-generation immigrants, who are both dif-

ferent in appearance from the natives and differ from each other in terms of whether they were raised by native parents, second-generation immigrants respond to a much larger extent to new school peers coming from the same ROO as they do. Therefore, whether an individual is raised by native parents is a highly possible mechanism whereby the individual is affected by same-ROO school peers. However, there is not sufficient information to understand whether those raised by immigrant parents have a higher preference for same-ROO partners and work colleagues, or whether they are discriminated against in forming relationships and seeking jobs in the labour market.

Table 12 presents additional specifications as robustness checks for immigrants and to aid in understanding the mechanisms behind the empirical results. The top panel of the table shows the relevant previous results for easier reference for readers. Except for the model represented in column (3), all dependent variables of the new specifications are the same as in the old models. The additional changes made for the new specifications are shown in the last three rows of the table. In column (3), a new dependent variable is used. The results for further subgroups, such as first- and second-generation immigrants, are not shown because I find similar changes between the old and new specifications for immigrants as a whole and for these subgroups.

Columns (1) and (2) replicate the results of columns (2) and (3) in table 6, when the subsample of those married and not having children are dropped. It means that the new sample contains only those who are married or cohabitating and have children. For both definitions of the independent variables, the absolute values of the coefficients have decreased significantly. This means that my main results can capture the impacts on fertility together with the tendency to have partners from similar immigration backgrounds. However, one must keep in mind that, as noted in section 4, the Swedish dataset does not include cohabitating couples without children or relationships without cohabitation. It is also important to note that the impact on fertility does not dominate the empirical results of my main findings. In column (3), I define the dependent variable as a dummy equal to one when an individual has a partner who both comes from the same ROO and studies at the same school in ninth grade as the individual. The values of the estimated coefficients drop drastically, while still statistically significant at the 1% significance level, which indicates precisely estimated zero impacts. This reflects the fact that the impact of having school peers with similar immigration backgrounds does not cause individuals to form relationships with partners with similar immigration backgrounds from the same school.

Thus, the impacts extend to the social network of an individual beyond the circle of classmates in ninth grade and continue to affect the individual in the long term after graduating from school. Last but not least, columns (4) and (5) shows the results of having colleagues with similar immigration backgrounds when further work-related controls are added, including number of employees in a workplace, number of employees in a company as a whole, and industry indicators. The differences in the results are not noticeable between specifications with and without the work-related controls. This reflects that the results can be generalized to different types of work.

Table 8: Impacts of School Peers on First-Generation Immigrants

	Relationship Outcome			Work Outcome		
	(1) Have partner	(2) Partner is immigrant	(3) Partner is from same ROO	(4) Have work	(5) Proportion of immigrant colleagues	(6) Proportion of same-ROO colleagues
Proportion of immigrant peers	0.038 (0.047)	0.071 (0.060)	0.084 (0.068)	-0.001 (0.031)	0.001 (0.030)	0.008 (0.023)
Proportion of same-ROO peers	0.110* (0.045)	0.092 (0.054)	0.442*** (0.070)	0.123*** (0.032)	0.030 (0.036)	0.201*** (0.032)
Observations	75,049	38,283	38,283	75,049	66,792	66,792

Note: Each column presents the estimation results for different dependent variables and shows the estimated β_1 of equation (1) and α_1 of equation (2) for two definitions of the independent variables. For both definitions, school peers include both genders. Robust standard errors clustered at the school level are presented in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 9: Impacts of School Peers on Second-Generation Immigrants

	Relationship Outcome			Work Outcome		
	(1) Have partner	(2) Partner is immigrant	(3) Partner is from same ROO	(4) Have work	(5) Proportion of immigrant colleagues	(6) Proportion of same-ROO colleagues
Proportion of immigrant peers	-0.079 (0.048)	0.165* (0.076)	0.108 (0.076)	0.006 (0.029)	0.049 (0.028)	0.042 (0.025)
Proportion of same-ROO peers	0.145* (0.072)	0.424*** (0.072)	0.618*** (0.068)	0.052* (0.026)	0.162*** (0.027)	0.263*** (0.027)
Observations	63,024	31,161	31,161	63,024	58,787	58,787

Note: Each column presents the estimation results for different dependent variables and shows the estimated β_1 of equation (1) and α_1 of equation (2) for two definitions of the independent variables. For both definitions, school peers include both genders. Robust standard errors clustered at the school level are presented in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 10: Impacts of School Peers on Adoptees

	Relationship Outcome			Work Outcome		
	(1) Have partner	(2) Partner is immigrant	(3) Partner is from same ROO	(4) Have work	(5) Proportion of immigrant colleagues	(6) Proportion of same-ROO colleagues
Proportion of immigrant peers	0.019 (0.144)	-0.020 (0.160)	0.020 (0.160)	-0.119 (0.062)	0.042 (0.045)	-0.041 (0.045)
Observations	25,174	12,103	12,103	25,174	24,119	24,119

Note: Each column presents the estimation results for different dependent variables and shows the estimated α_1 of equation (2) for two definitions of the independent variables. For both definitions, school peers include both genders. Robust standard errors clustered at the school level are presented in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 11: Impacts of School Peers on Nonadoptees

	Relationship Outcome			Work Outcome		
	(1) Have partner	(2) Partner is immigrant	(3) Partner is from same ROO	(4) Have work	(5) Proportion of immigrant colleagues	(6) Proportion of same-ROO colleagues
Proportion of immigrant peers	0.004 (0.015)	0.085*** (0.014)	-0.085*** (0.014)	-0.023*** (0.006)	0.044*** (0.005)	-0.044*** (0.005)
Observations	1,147,083	611,642	611,642	1,147,083	1,115,618	1,115,618

Note: Each column presents the estimation results for different dependent variables and shows the estimated α_1 of equation (2) for two definitions of the independent variables. For both definitions, school peers include both genders. Robust standard errors clustered at the school level are presented in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 12: Summary of Further Specifications on Immigrants

Dependent Variable	Relationship Outcome			Work Outcome	
	(1) Partner is immigrant	(2) Partner is from same ROO	(3) Partner is from same ROO and same school	(4) Proportion of immigrant colleagues	(5) Proportion of same-ROO colleagues
Previous results					
Proportion of immigrant peers	0.126* (0.050)	0.127* (0.053)	— —	0.027 (0.019)	0.025 (0.017)
Proportion of same-ROO peers	0.247*** (0.045)	0.548*** (0.055)	— —	0.101*** (0.023)	0.238*** (0.021)
Observations	69444	69444	—	125579	125579
New results					
Proportion of immigrant peers	0.107 (0.059)	0.070 (0.071)	0.014 (0.013)	0.009 (0.021)	0.020 (0.019)
Proportion of same-ROO peers	0.025 (0.054)	0.369*** (0.061)	0.072*** (0.013)	0.118*** (0.020)	0.257*** (0.021)
Observations	31600	31600	69444	90871	90871
Without those married and not having children	Y	Y			
New dependent variable			Y		
Work-related controls added				Y	Y

Note: Each column presents the estimation results for different dependent variables and heterogeneity results and shows the estimated β_1 of equation (1) and α_1 of equation (2) for two definitions of the independent variables. The top panel includes the previous results for comparison with the new specifications. The last three rows show the additional changes made to the new models. For both definitions, school peers include both genders. Robust standard errors clustered at the school level are presented in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

6 Conclusion

This study explores the effects of having school peers from the same ROO on an individual's probability of having a partner from the same ROO and on the proportion of same-ROO colleagues in a workplace within 15 years of ninth grade. It does so by exploiting the quasi-random variation in school peers from the same ROO as an individual in ninth grade. I attempt to contribute to the literature through defining partners and colleagues with register data and investigating the mechanisms behind the impacts of immigrant school peers.

The empirical results have revealed mixed results between natives and immigrants. On the one hand, for natives, there are trivial effects on the probability of having had at least one partner within 15 years of ninth grade; the probability of having at least one work record; the probability of having an immigrant partner, conditional on having had at least one partner; and the proportion of immigrant colleagues in the first workplace, conditional on having at least one work record. On the other hand, for immigrants, one standard deviation increase in the proportion of school peers from the same ROO leads to a greater than 7% increase in the probability of having a partner from the same ROO, conditional on having had at least one partner, and a greater than 12% increase in the proportion of colleagues from the same ROO for the first workplace, conditional on having at least one work record. Additional results also show that the effects on the probability of having had at least one partner or work record are trivial.

These results indicate the need for diversity in the school environment, especially for the immigrants. My main findings contrast with those of the most closely related study, that by Merlino et al. (2019), in that they find that having more immigrants, which means fewer natives, among school peers affects natives' tendency to have interracial relationship after graduation. However, my study finds that the composition of school peers in terms of ROO does not affect the relationship outcome of the natives. This study complements theirs by also examining the effects on the immigrants, who are at the center of the assimilation discussion.

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Appendix

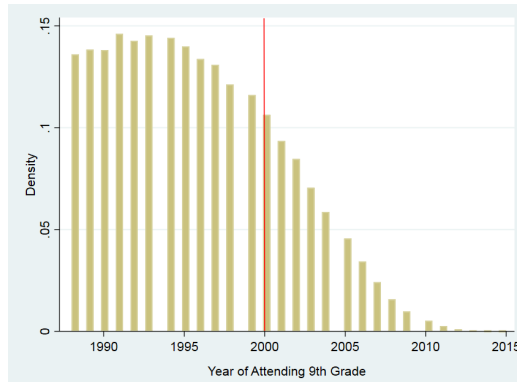


Figure A.1: Proportion of individuals having had at least one partner within 15 years of ninth grade

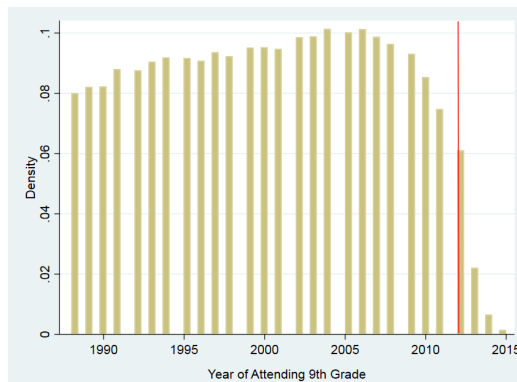


Figure A.2: Proportion of individuals having at least one work record within 15 years of ninth grade

Chapter 2

Impact of Salt Iodisation on Human Capital: Evidence from Sweden*

Debbie Lau[†]

Abstract

The focus of this study is the heterogeneous causal impact of a nationwide information campaign of salt iodisation in 1936 on individuals with different socioeconomic status (SES) at birth, using a difference-in-differences regression model. This is made possible by using a novel dataset that merges the Swedish population registers with two unique historical data sources of pre-intervention iodine deficiency prevalence and SES at a highly disaggregated level, thereby being able to trace the educational attainment and labour market outcomes of these individuals during their adulthood. The results of this study, strikingly, show that while the intervention increased human capital for individuals from families with high SES, those from families with low SES did not benefit. While the intervention led to an 8% increase in an individual's probability of having a high-skill occupation, modest effects are found for an individual's education. The social gradients shown by these results are critical for governments who use salt iodisation to improve human capital in the population.

Keywords: causal inference, register data, iodine deficiency, information campaign

JEL Classifications: I12, I15, I18, J24, J62, N34

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1 Introduction

Malnutrition is one of the greatest threats to global public health. A lack of proper nutrition during an individual's early years of life inhibits the body's growth and development (WHO, 2018) and has been shown to be highly associated with adult diseases (Barker, 1990), indicating the importance of eradicating this public health problem. Moreover, a growing body of economics literature suggests that health capital during early life is essential for the development of other types of human capital (Heckman, 2007), such as cognitive ability¹. Studies have found that nutritional supplementation during early life improves test scores, educational attainment, and labour market outcomes measured later in life². Iodine is a particularly important nutrient for cognitive function because it plays a vital role in brain development (Delange, 1994; Eastman & Zimmermann, 2018; Zimmermann, 2008, 2010). A well-cited meta-analysis finds that individuals from an area of widespread iodine deficiency (ID) have one standard deviation lower intelligence quotient (IQ) scores than those from an area without widespread ID (Bleichrodt & Born, 1994).

According to the most recent international survey conducted by the World Health Organization (WHO, 2018)³, one-third of the world's population suffers from disorders due to ID. While it is most common in developing countries in Southeast Asia and Africa, ID has recently been reappearing in developed countries, such as Australia and the UK (Andersson, Karumbunathan, & Zimmermann, 2012; Charlton & Skeaff, 2011). Even though there is a consensus that salt iodisation is the cheapest method of eradicating ID, less is understood on the effectiveness of different ways to implement salt iodisation. The most notable difference among countries with a salt iodisation policy is that while some prohibit the sale of non-iodised salt, others only recommend an appropriate concentration level of iodine in salt. The former is usually referred to as mandatory salt iodisation while the latter is called voluntary iodisation. Both practices are common around the world. For instance, out of 29 European countries with a salt iodisation policy, 16 of them, including Belgium, France, Germany, and the UK, follow the latter practice (Andersson, de Benoist, Darnton-Hill, & Delange, 2007). Given that both practices are common, surprisingly little is known

¹The evolving theoretical literature on the human capital production function suggests the dynamic complementarity of human capital (i.e. future investment is more productive for those who currently have higher human capital) and it is self-producing (i.e. human capital produces further human capital).

²See a recent summary by Almond, Currie, and Duque (2017)

³This is the latest survey where the indicators of ID are comparable across different countries. It uses ID prevalence among school-aged children.

about how they differ in terms of their benefits and the recipients of these benefits. This gap needs to be filled with better policy evaluation.

Findings from the economics literature on the effectiveness of information campaigns, such as those related to poverty reduction, schooling, and health (Ravallion, 2016) also underline the need to study the social gradients of voluntary salt iodisation policy. For instance, one study has found that individuals with higher educational levels are more responsive to information campaigns on HIV prevention (de Walque, 2007)⁴. To the best of my knowledge, this is the first study that is able to identify the causal impact of salt iodisation policy on a voluntary basis. More specifically, this study reveals the social gradients related to such policy. I explore an intervention that promotes iodised salt consumption through a nationwide information campaign in Sweden, where the government issued a recommended level of iodine content in salt and urged all physicians in public hospitals to promote the use of iodised salt. My difference-in-differences model estimates the differential causal impact of such policy by individuals' socioeconomic status (SES) at birth, using the variation of pre-intervention ID prevalence and the proxy of individuals' SES at birth at a highly disaggregated level.

Recent economics studies have found surprisingly large to zero effects of mandatory salt iodisation policies (Adhvaryu, Bednar, Molina, Nguyen, & Nyshadham, 2018; Bengtsson, Peterson, & Sävje, 2013; Deng & Lindboom, 2018; Feyrer, Weil, & Politi, 2017; Field, Robles, & Torero, 2009; Politi, 2014, 2015). Even studies on exactly the same policy in a single country have had mixed results when using different data sources (Bengtsson et al., 2013; Field et al., 2009). This highlights the fact that high-quality data are needed to answer this research question. Above all, there has to be information on the intensity of ID an individual experienced during early childhood. Another challenge is that in order to identify the likely differential impacts of a voluntary salt iodisation policy, one needs to know the pre-intervention SES of an individual.

To overcome these empirical challenges, I have collected the data of pre-intervention ID prevalence from a chapter in a medical journal, *Kropfstudien V. Die Verbreitung Des Endemischen Kropfes in Schweden*, written in the 1930s by a physician, J.A. Höijer, and the SES proxy from the hard-copy of tables from the municipality yearly statistics describing the usage

⁴Oreopoulos and Salvanes (2011) summarize a few more examples on the higher responsiveness of individuals with more years of schooling to health information campaigns. Kremer, Rao, and Schilbach (2019) provide an updated summary of the theories and empirical evidence on how information alters one's decision making.

of poverty assistance at municipality level also in the 1930s. Due to the fact that these historical data were measured at different geographical units, I have used documents from the Swedish National Archives to map these data. A novel dataset is thus created by merging these two unique historical data sources with the Swedish population registers, which includes the educational attainment and labour market outcomes for all individuals born in Sweden in the 1930s, as well as the approximation of ID prevalence and SES of their places of birth before the intervention is implemented. My results, strikingly, show that while the intervention increased human capital for individuals from families with high SES, those from families with low SES did not benefit. While the intervention led to an 8.04% increase in an individual's probability of having a high-skill occupation, which is comparable to the magnitude of the effects found in the literature, modest effects are found for an individual's education. These findings imply that this intervention unintentionally widened inequality, which is undesirable for policy makers, and it was less effective than the mandatory policy. These implications are critical for governments who are using or plan to use salt iodisation to improve human capital in the population.

This paper is organised as follows. Section 2 summarises theoretical and empirical evidence on the link between ID and human capital; section 3 describes the intervention I exploit and the intuition of my identification strategy; section 4 illustrates how the identification strategy is incorporated into my regression model; section 5 presents the outcome and background variables used; section 6 shows my estimates and interprets the main findings; section 7 discusses the magnitude and mechanism of the policy effects and the threats to identification; and section 8 concludes.

2 Literature Review

This section begins with the theoretical link between iodine and cognitive development, then presents the descriptive and causal empirical evidence that supports this link. Iodine plays a vital role in brain development during early life. The thyroid gland uses iodine to produce thyroid hormones which are necessary for brain development (Delange, 1994; Zimmermann, 2008, 2010). Insufficient intake of iodine can result in a disorder called goiter, in which the thyroid gland is enlarged and can be observed as a swelling on the neck⁵. This happens because the thyroid becomes enlarged to maximize its capacity to store iodine and produce thyroid hormones.

⁵Figure 1 in section 3 shows patients with goiter in Sweden.

Because of its visibility and the fact that ID is endemic geographically⁶, goiter prevalence has been a common proxy for ID in a population⁷. The worst consequence of ID, however, is irreversible mental retardation. Severe ID in utero and during early childhood leads to impaired mental function because the brain undergoes rapid growth during this period. However, ongoing studies on the effects of mild and moderate ID on fetuses and infants have found mixed results (Eastman & Zimmermann, 2018; WHO, 2007).

Bleichrodt and Born (1994) conducted a meta-analysis of international studies on the effect sizes of iodine provision on IQ. They conclude that ID led to a decrease of 13.5 IQ points which is equivalent to one standard deviation of the IQ distribution. However, this meta-analysis mixes both experimental studies of the provision of iodine supplementation and descriptive studies of the difference in IQs between individuals living in iodine-sufficient and iodine-deficient places. A more recent meta-analysis by Qian et al. (2005) summarizes the effect sizes for different types of studies on the Chinese population. They find that in the absence of iodine supplementation, children from iodine-deficient places, on average, have IQs 12.45 points lower than those from iodine-sufficient places. Furthermore, in iodine-deficient places, the average IQ increases by 8.7 points for children whose mothers received iodine supplementation during pregnancy. While these meta-analyses provide straightforward summaries of the effect sizes of iodine provision, their inclusion of observational studies can lead to bias. Furthermore, the combination of experimental studies with different treatment intensities and underlying population characteristics also makes it more difficult to interpret these summarised effect sizes.

Several recent economics studies use quasi-experimental designs to estimate the causal effects of related policies. Table 1 summarises the literature studying the effects of the spread of iodised salt on human capital development of the population. These interventions are similar to the one that is studied in the current paper, where the governments either prohibited the sale of noniodised salt or required the use of iodised salt, including a sudden increase in iodised salt consumption. They all use difference-in-differences regression to estimate the difference in the change of outcomes

⁶This means that individuals living in the same area often have the same risk of having ID. Medical geologists have found that ID, and goiter as a type of ID disorder, often occurs in areas with low iodine content in soil and drinking water. These places are usually located either away from ocean or where iodine in soils has been washed out by flooding or intense glaciation, such as high mountain ranges, rain shadow areas and central continental regions (Fuge, 2013; Karlsson, 1993; Kelly & Snedden, 1958).

⁷Other methods of detecting ID, such as checking the iodine level in urine, have not been available until recently.

between areas with and without endemic goiter for those conceived after the widespread use of iodised salt. They use measures of human capital development at different ages, providing a spectrum of results observed at different stages of life. I sort studies in descending order of the treatment intensity of the policies to provide a better comparison of effects. These studies (Adhvaryu et al., 2018; Deng & Lindeboom, 2018; Feyrer et al., 2017; Politi, 2014, 2015; Tafesse, 2018) have found sizable effects on test scores observed in childhood and teenage years, educational attainment, and labour market outcomes observed in adulthood. In particular, effect sizes found by Deng and Lindeboom (2018) are very close to the effect sizes shown in the meta-analysis of Chinese studies by Qian et al. (2005). Moreover, most of these results show that females benefit more from this type of intervention than males.

Almost all of the literature used quasi-experimental design to study policies implemented in the early 20th century, and there were limited data to exploit in investigating the differential policy effects with respect to family backgrounds. This is, however, an important aspect of health policy to be evaluated. Deng and Lindeboom (2018) and Tafesse (2018) were able to shed light on a related issue. Deng and Lindeboom (2018) look at heterogeneity of the policy effects with respect to parents' preference for sons at the village level ⁸. Their results show that girls from villages with a higher preference for son benefit more from an iodised salt policy than do girls in other villages. Girls in the former group received less parental investment from beginning. Following a similar strategy, Tafesse (2018) finds that *boys* from communities with stronger preference for son benefit more from the policy than boys in other communities. This is possibly due to parental reinforcement investment subject to observable cognitive endowments. However, how family SES and the intervention interact with each other still needs to be studied.

Last but not least, another economics study exploits a slightly different policy, an iodine oil capsule supplementation program in Tanzania. Field et al. (2009) find that this program increases educational attainment by 0.35 years on average for those exposed to iodine supplementation in the first trimester of gestation, and the impact is larger and more robust for girls. These results are surprising for the large impact of iodine fortification it shows, and this study has been widely cited among studies on the causal impact of iodine fortification. However, a recent study of Bengtsson et al. (2013) provides empirical evidence against the well-cited results by

⁸In this study, parents refers to individuals aged 30 to 50 which is the likely age range of parents.

Field et al. (2009) by exploiting a dataset with more samples. This, again, strongly suggests a lack of sufficient empirical evidence on the impacts of iodine fortification and highlights the need for more comprehensive studies.

Table 1: Literature with Quasi-experimental Design

Country	Study	Treatment Intensity ¹	First Stage ²	Reduced-Form Estimates ³
China	Deng and Lindeboom (2018)	i) 12pp; ii) 40mg/kg	15pp	Outcomes observed at early teenage: 6% increase in educational attainment; 15% increase in math test scores; 14% increase in verbal test scores; 12% increase in years of schooling
Switzerland	Politi (2014, 2015)	i) authors only mention a place with goiter rate > 11.7% is defined as endemic; ii) 3.75 mg/kg	45pp	Outcomes observed in adulthood: 10% increase in the likelihood of having higher paying occupation for males; 7% increase in likelihood of attaining secondary education for females 8% increase in attaining tertiary education for females;
Sweden	Current study	i) 12pp; ii) 10 mg/kg	NA	Outcomes observed in adulthood: Years of schooling; P(attained over compulsory-level education); P(have high-skill occupation)
United States	Feyrer et al. (2017)	i) 0.7pp; ii) 100mg/kg ⁴	Both studies only mention use of iodised salt spread quickly across the country	Outcomes observed for young males: 29% increase in the likelihood of being selected into air force = 1 sd increase of IQ, or 15 IQ points

Continued on next page

Table 1 – continued from previous page

Country	Study	Treatment Intensity ¹	First Stage ²	Reduced-Form Estimates ³
	Adhvaryu et al. (2018)			Outcomes observed in adulthood: Not significant and low magnitude for males; Very small increase of yrs of schooling for females, i.e. around 2 weeks; 2.3% increase in employment for females; 24% increase in income for females
India	Tafesse (2018)	i) No info on definition of endemics; 15mg/kg ii)	30pp	Outcomes observed in childhood: 5% increase in numeracy skill for girls; 3% increase in numeracy skill for boys; 6% increase in language skill for girls; 3% increase in language skill for girls

¹ Studies are sorted according to treatment intensity of the policies. Treatment intensity refers to two types of information: i) the difference in percentage points (pp) of the average goiter rate between endemic and nonendemic areas defined by the authors; and ii) the concentration of iodised salt (how many milligrams of iodine were added to 1 kilogram of salt).

² The column shows the increase in percentage points of iodised salt consumption induced by the policies. Some of these numbers were extracted from the discussion of the first stage of the policies; where the studies do not explicitly discuss the first stage, the numbers were taken from anecdotal evidence in the studies.

³ All studies except Politi (2014, 2015) explicitly have first-stage and reduced form regression. Other studies show only reduced-form estimates. Effects in percentage are either taken directly from the studies or calculated from the information they provide.

⁴ The iodine content is cited from Zimmermann (2008). It is not available in either Feyrer et al. (2017) or Adhvaryu et al. (2018).

3 The Intervention

3.1 Background

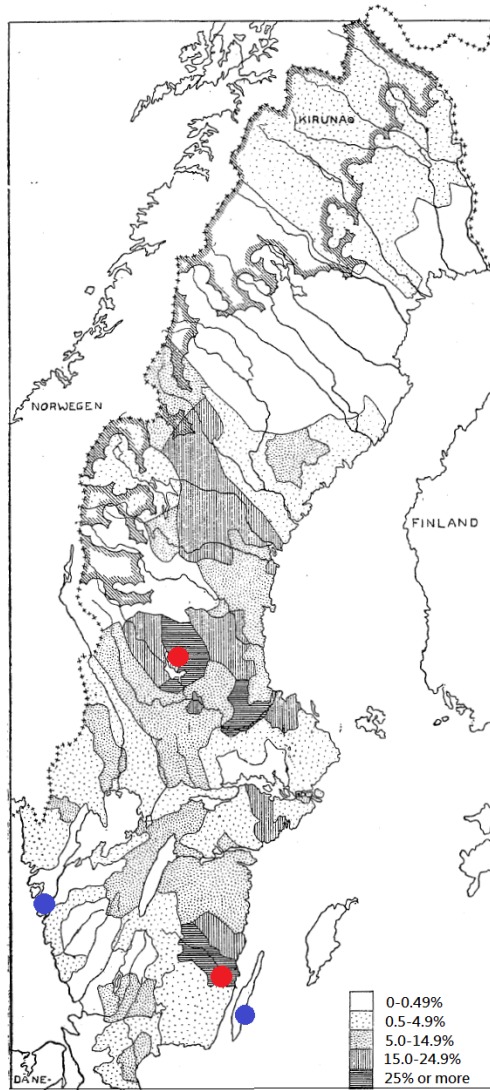
The intervention was induced by the study of goiter by Höijer (1931), which showed the severity of the problem. Goiter was first observed in the early 19th century in Sweden, at the same time that the consumption of iodine-rich salted herring plummeted (Karlsson, 1993). Since then, a numbers of studies were conducted on goiter prevalence at different localities. However, none of them are able to give a full picture of the prevalence throughout Sweden. Höijer (1931) published his study on the prevalence of goiter among conscripted men in 1929 and collected data on goiter prevalence among women himself in order to validate how representative the conscription data were. Figure 1 includes photographs of goiter patients in some localities visited by Höijer. As shown in figure 2, Höijer (1931) summarised different data sources and found that a goiter belt extended from Väster-norrland County in northern Sweden to Dalarna and Gävleborgs Counties in middle Sweden, and then to Småland County in the south⁹. Moreover, there was high variation in goiter prevalence within Sweden. While Oskarshamn and Mora (red dots on the map) had goiter prevalence over 25%, there were places with no occurrence, such as Gothenburg and Öland (blue dots).

Figure 1: Goiter patients from different parts of Sweden



⁹A goiter belt refers to an area with endemic goiter.

Figure 2: Goiter prevalence in Sweden



Source: author's note upon map from Højjer (1931). Shaded area at military district level.

In this study, goiter prevalence is used as the proxy of pre-intervention severity of ID in a local population. More specifically, it is the percentage of 20 year-old men diagnosed with goiter during conscription for each military district (Højjer, 1931)¹⁰. Sweden was divided into 75 military districts for conscription purpose. The data were measured during conscription registration in 1929 and it is the only year the conscription physicians recorded the presence or absence of goiter for everyone. In total, about 60,000 20

¹⁰These military districts were called Rullföringsområden in Swedish.

year-old men were investigated in year. The timing of these data is good as it is close to the year of intervention and serves as an excellent approximation of goiter prevalence when the intervention started. These are the only nationwide data on goiter or ID before the intervention. There are limited data on goiter prevalence among conscripts in 1928 for some military districts. Goiter prevalence in 1928 and 1929 is highly correlated for these districts.

The conscription data was validated by a survey by Höijer in 180 places across Sweden from 1929 to 1930. Höijer collected goiter data for both men and women in different age groups in each place. He even collected data on cretinism and animals' goiter prevalence¹¹. He divided these places into five groups according to their goiter prevalence levels based on information he had collected. These levels are shown in Table 2¹². He found that goiter prevalence was generally lower among men than women in a single place. Furthermore, while the exact goiter rates could be different from the conscription data, the ranking of places by goiter prevalence based on each data source is consistent. He also observed that in Sweden, mountainous areas or plains close to steep slopes or around larger rivers had higher goiter prevalence. These are exactly the kinds of places prone to low iodine content in the soil according to findings from medical geology¹³.

Table 2: Different Levels of Goiter Prevalence in Places Surveyed

Level	Men	Women
1	0%	0%-3%
2	0%-4%	2%-13%
3	4%-7%	14%-20%
4	8%-15%	21%-33%
5	more than 15%	more than 33%

Note: Data from Höijer (1931)

Höijer defined places belonging to levels 3 to 5 as having endemic goiter. In these places, goiter prevalence was at least 14% among women and at least 4% among men. As the target group in my study is pregnant women, and WHO defines a place as having endemic goiter if the goiter prevalence is above 10%, I also define places belonging to levels 3, 4, and 5 as having

¹¹Cretinism is a more severe ID disorder in which the patient suffers from both stunted physical and mental growth.

¹²Some ranges overlap with each other in the original study, perhaps because of typos. I present the original, uncorrected numbers here.

¹³Refer to the discussion in section 2

endemic goiter. However, I base my study on the conscription data, because these data cover the entire country of Sweden. Therefore, I consider military districts with goiter prevalence of at least 5% to be endemic goiter districts, as this is consistent with Højjer's definition of levels 3 to 5¹⁴. Classifying districts as either endemic or nonendemic also alleviates measurement errors of treatment intensity as compared with using the goiter prevalence for each district (Politi, 2014, 2015). I use the Swedish National Archives (SFS, 1901) to match information from parishes (*församling*) with military districts in order to assign individuals' parishes of birth to the appropriate military districts of birth. Individuals were conscripted according to parish of residence. Therefore, I assume that individuals did not move between birth and conscription.

3.2 Information Campaign

In response to the evidence of widespread goiter from Højjer (1931), the National Board of Health introduced an information campaign on 27 April 1936 in which it sent out a letter to all physicians in public hospitals and clinics to promote the use of iodised salt¹⁵. A copy of the letter appears in figures A.1 in the Appendix. In the letter, the National Board of Health informed physicians about the prevalence of goiter all over the country, urged them to promote iodine consumption because it can cure goiter, suggested that iodised salt was one of the best sources of iodine, and recommended that the appropriate concentration was 10mg iodine per 1 kg of salt. Furthermore, the authority emphasised that pregnant women and young children were particularly prone to ID and thus needed special attention. At the end of the letter, it requested that all counties with endemic goiter report how they would address this issue.

Ideally, there would be information on the decrease of goiter, as a proxy for ID, among pregnant women and young children following the intervention. This could have been evidence for the first-stage effect of the intervention. Unfortunately, such information is not available. Instead, there is anecdotal evidence of the active response of the population to the intervention in some counties. This is most obvious in Östergötland which had the highest goiter prevalence before the intervention (Højjer, 1931). The county government sent out a letter to households about the information physicians had received from the central government so as to promote iodine consumption among households¹⁶. As shown in figure 3, the goiter preva-

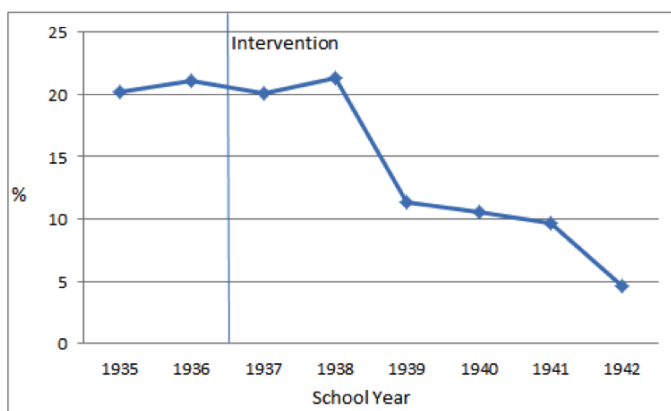
¹⁴This is the 75th percentile of goiter rate at the district level. Politi (2014, 2015) also use the same threshold for defining high and low goiter places.

¹⁵The Swedish authority was Medicinalstyrelsen during that period of time.

¹⁶See figure A.2 in the Appendix.

lence among students in a school in this county dropped drastically—by half—after the intervention. There are two reasons why this drop did not happen immediately after the intervention: first, goiter does not respond immediately to a change of the iodine level in the body, but rather it is alleviated after sufficient intake of iodine for months to years; and second, it took time for households to adjust their salt consumption behaviour. In short, because of the centralization of the healthcare system in Sweden, it is highly possible that all physicians took this information seriously and paid special attention to ensure sufficient intake of iodine by pregnant women and young children after the intervention.

Figure 3: Goiter Prevalence among Students in Östergötland



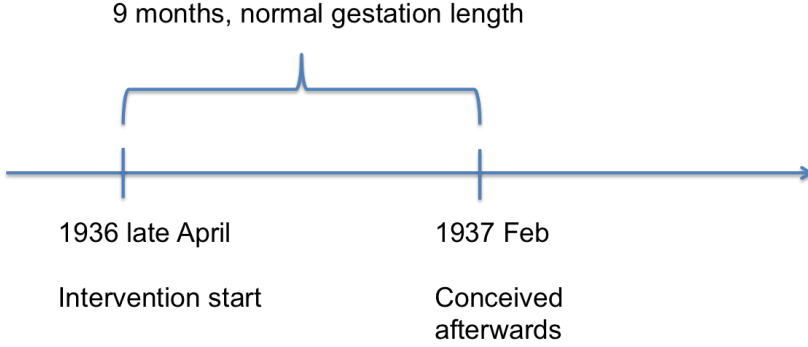
Source: Gullstörm (1943)

3.3 The Treated Individuals

Both times and places of birth have to be considered to define an individual's treatment status related to the intervention. In places without endemic ID, those conceived before and after the intervention should not have had ID and did not gain from the intervention, whereas in places with endemic ID, those conceived after the intervention should have been treated and benefited from the intervention. These two dimensions of treatment status constitute the main components of the difference-in-differences design in my identification strategy. First of all, referring to the timeline in figure 4, assuming that everyone has a gestational length of nine months, all born in or after February 1937 were conceived after the intervention. These individuals were fully exposed to the policy both in utero and during early childhood. Therefore, I expect that in places with endemic ID, individuals born in or after February 1937 would experience improvement

in human capital development.

Figure 4: Timeline



4 Difference-in-Differences Model

4.1 Baseline Model

This section illustrates how the nature of the intervention and available goiter measurement data are incorporated into my regression models. Following the identification strategy of the literature on similar policies (Adhvaryu et al., 2018; Deng & Lindeboom, 2018; Feyrer et al., 2017; Politi, 2014, 2015; Tafesse, 2018), I estimate the effects of the policy with a difference-in-differences design. Equation (1) is my baseline model:

$$y_{idt} = \beta_0 + \beta_1 * (After_t * Endemic_d) + \alpha_d + \lambda_t + \alpha_d * t + \alpha_d * t^2 + \epsilon_{idt} \quad (1)$$

In equation (1), y_{idt} is an outcome of interest of individual i who was born in military district d (hereafter ‘district’) in year-month period t . $After_t$ is a dummy variable indicating whether an individual was conceived after the intervention. Assuming that each birth has a gestation period of nine months¹⁷, $After_t$ equals one for those who were born in or after February 1937. $Endemic_d$ is another dummy variable, indicating whether an individual was born in a district with high goiter prevalence at the time. It is a proxy of the prevalence of ID in the population of a specific district.

¹⁷There is no information on the gestation length for individuals in the data. Therefore, I assume there were no preterm births. The violation of this assumption causes underestimation of the policy effects as some treated cohorts will be misclassified in the control group.

α_d is the district of birth fixed effects and controls for the time-invariant characteristics of each district of birth. It can control for pre-intervention differences between endemic and nonendemic districts shown in the descriptive statistics in table 5, in section 5, supposing that these differences are fixed.

Because of changes in the population’s educational level, driven by educational reform, and subsequent changes in its occupational pattern, it is vital to control for the trend of the outcomes carefully. λ_t is the cohort of birth fixed effects. A cohort of birth refers to those who were born in a particular year and month. It controls for common shocks to outcomes for each cohort of birth. $\alpha_d * t$ is the district-specific linear trend (monthly trend). This controls for different linear trends of the outcome variables across districts, which can confound the estimation of β_1 , the coefficient of interest for the effects of the intervention. In addition, I control for the district-specific quadratic trend, achieved by adding $\alpha_d * t^2$ into the model. Standard errors are clustered at the district level to account for correlated errors within a district.

$\hat{\beta}_1$ is the intention to treat policy effect. More specifically, the policy effect refers to the difference in the change in outcome between those who were conceived before and after the intervention and between those who were born in endemic and non-endemic districts. This does not require the assumption that the intervention induced no improvement of human capital for the nonendemic districts. If some individuals in nonendemic districts had only mild iodine deficiency and thus did not have goiter symptoms before the intervention, they could still possibly have benefited from the intervention. However, $\hat{\beta}_1$ estimates the *additional* gains for endemic districts over nonendemic districts from the intervention. Ideally, there are data for estimating the first stage of the policy effect, which is the increase in the proportion of individuals consuming iodised salt. It is because these are the compliers who drive the reduced-form results.

4.2 Dynamic Specification

Equation (2) is my second specification. The only difference from equation (1) is that equation (2) interacts the dummy variable for birth year and the dummy variable for being born in endemic districts. Therefore, there is one estimate of the policy effects for each birth year, except the reference year. The birth years are included in equation (2) for simpler illustration. In fact, the reference group refers to those born between May 1936 and

January 1937. Year 1935 refers to those born between May 1935 and April 1936. Year 1937 refers to those born between February 1937 and January 1938. Other years follow the same logic.

$$y_{idt} = b_0 + \sum_{t=1932, t \neq 1936}^{1943} b_t * (Year_t * Endemic_d) + \gamma_d + \pi_t + \gamma_d * t + \gamma_d * t^2 + e_{idt} \quad (2)$$

In table 3 below, the reference group is the second birth cohort in the third column of the table. Other birth cohorts are classified into two groups, born before or after the reference group. Only individuals born in endemic districts in or after February 1937 were exposed to the intervention fully, both in utero and during early childhood, and thus, they are free from ID. Those in the reference group were born during the first nine months of the intervention. They were fully exposed to the intervention fully during early childhood but only partially in utero. For example, an individual born in September 1936 was conceived in December 1935. He or she had been in utero for five months when the intervention started. Unlike the previous group, they were not exposed to the intervention at the beginning of the in utero period. However, referring to the second column, those born before May 1936 were worse off as they were not exposed to the intervention until after birth.

Table 3: Classification of Birth Cohorts for Dynamic Specification

	Born in/before Apr 1936	Born between May 1936 to Jan 1937	Born in/after Feb 1937
Born in non-endemic areas	No ID	No ID	No ID
Born in endemic areas	ID	Less ID	No ID

Coefficients on year 1937 and after estimates the effect of being exposed to the intervention fully in utero. These will be positive if exposure early in utero matters. I also expect that the coefficients for later years are bigger for the gradual spread of information and change of consumption behaviour. For coefficients on year 1935 and before, they estimates the effect of starting to be exposed to the intervention later during early childhood. These

will be negative if exposure during early childhood matters. I expect the coefficients to be more negative for earlier years. Therefore, this second specification both allows for different intervention effects for individuals born in different years and possible lags of the intervention effects due to the more realistic nature of the intervention.

5 Outcomes and Background Variables

5.1 Variables

The base sample comes from Sweden’s register of the total population and includes all births in Sweden starting in 1932 for those who were alive as of 1961. This study makes use of those born in Sweden between 1932 and 1943. The register provides year-month and parish of birth of all individuals in Sweden. For the birth cohorts used for this study, Sweden was divided into about 2,000 parishes. Parish of birth data are missing for around 3 percent of individuals. For those with data on parish of birth, around 10 percent of them belong to parishes that cannot be found in the document I used to match information from parishes with military districts (SFS, 1901), which means that they are not included in my sample for analysis. Furthermore, I restrict the sample to individuals for whom the parish of birth poverty level is available for all analysis so that results of the baseline model and dynamic specification are comparable.

5.1.1 Educational Attainment

The cohorts I study had seven years of compulsory education, which was called *folkskola*¹⁸. Students could attend lower secondary school, called *realskola*, instead of completing all seven years at the compulsory school. Those who left compulsory school after the fourth year would attend a four-year lower secondary school, and those leaving after the sixth year would attend a three-year lower secondary school. The lower secondary school was academically oriented and prepared students for admission to high school, called *gymnasial*, which was a prerequisite for university entrance. Those who did not attend lower secondary school but stayed in compulsory school for all seven years could not enter university. Therefore, whether an individual had attained admission to high school (i.e., attained

¹⁸Information on the educational system in Sweden is from Costas and Mårten (2005), Fischer, Karlsson, and Nilsson (2013) and Fischer, Karlsson, Nilsson, and Schwarz (2016).

over a compulsory-level education) was a benchmark of having a higher educational level. During that period of time, promotion to high school was highly selective. Less than half of my sample did attain a higher level than the compulsory education¹⁹. The compulsory education had previously been lengthened from six to seven years and later was further extended to nine years. However, these educational reforms had little effect on the cohorts in this study²⁰.

The outcome variables have been created from information from the 1970 Swedish census, when the cohorts in the sample were 27 to 38 years old and should have completed their education. The 1970 census records individuals' highest educational attainment in seven categories, which I then transformed into respective years of schooling and added a dummy variable representing whether an individual ever attained beyond compulsory schooling²¹.

5.1.2 Occupational Status

The 1970 census record individuals' occupations using the three-digit Nordic Occupational Classification (NYK)²². This scale includes codes for 294 types of occupations. These occupation types are further grouped into 9 broader categories. The second column in Table 4 lists the descriptions of these 9 categories. The matching between the original 294 occupation codes and these 9 broader categories was developed by Statistics Sweden²³. The third column in the same table provides examples of occupations in each category. For the outcome variables, I further group these broader categories into low-, middle- and high-skill occupations, shown in the fourth column.

5.1.3 Socioeconomic Status at Birth

The family backgrounds of individuals while in utero and during early childhood are needed to study the differential impacts of the intervention. My measure of family background (SES) is based on the percentage of individuals living in poverty for each parish calculated from information published

¹⁹There is a downward trend for this proportion but without any discontinuity for my studied cohorts.

²⁰This is discussed further in section 7.

²¹This is equivalent to *Attained over compulsory-level education*.

²²NYK is loosely based on the International Standard Classification of Occupations (ISCO) for 1958, according to the codebook of the census.

²³Socioeconomic grouping (Socioekonomisk Indelning) in 1982.

Table 4: Classification of Occupations

Category	Description	Example	Skill Group
1	Unskilled Employees in Goods Production	Agricultural worker,	Low-skill
2	Unskilled Employees in Service Production	Hotel receptionist	
3	Skilled Employees in Goods Production	TV repairman,	Middle-skill
4	Skilled Employees in Service Production	Attendant in psychiatric care,	
5	Assistant Non-manual Employees, Lower-Level	Secretary	
6	Assistant Non-manual Employees, Higher-Level	Dental nurse,	High-skill
7	Intermediate Non-manual Employees	Surveyor,	
8	Professionals & Other Higher Non-manual Employees	Dentist,	
9	Upper-Level Executives	Managing director	

Note: Two categories of occupations have been omitted here: Farmers and Self-Employed Other Than Professionals and Farmers. They constitute around 2.5% of individuals in the sample.

in the Yearbook for Sweden’s Municipalities in 1936 (SCB, 1936)²⁴. There are around 2,000 parishes for the studied cohorts. As mentioned previously, the goiter prevalence data is available at the level of the 75 military districts. This SES measure allows variation of SES within a military district with the same goiter prevalence. It is essential for investigating the heterogeneity of policy effects with respect to SES. For each parish, it is equal to the following:

$$\frac{\text{No. of Recipients of Public Assistance (Fattigvården) in 1934}}{\text{Population as of 1 January 1936}}$$

Individuals born in a parish having a poverty rate less than the median, 8%, are defined as having families with high SES at birth. Another advantage of using this SES measurement is that it is based on data from shortly before the intervention. First, because the timing is very close to the intervention, the data can provide a good approximation of poverty levels of parishes at the time of the intervention. Second, because the SES is measured before the intervention, it is free from endogeneity that would arise if this measurement were affected by the intervention.

5.2 Preintervention Characteristics

Table 5 shows the characteristics for cohorts conceived before the intervention, giving a brief description of the difference between endemic and nonendemic districts before the intervention. The last column shows the difference in mean of endemic over nonendemic districts. The average goiter rate in endemic districts was 13.63%, while it was 1.43% in nonendemic districts. The difference in the average goiter rates between these two types of districts is comparable to what has typically been reported in the literature (refer to table 2), which makes it easier to compare my estimates with those from other studies. Individuals born in nonendemic districts have attained a higher education level and have a higher likelihood of having middle- or high-skill occupations (conditional on being employed). These differences are also statistically significant. However, pre-intervention differences between endemic and nonendemic districts do not bias my difference-in-differences estimates. The district of birth fixed effects, α_d , in equations (1) and (2) controls for these differences as long as they are constant.

²⁴At that time, the number of parishes(*församlingar*) and municipalities(*kommuner*) in Sweden was almost the same, around 2,000. However, not all municipalities are included in these statistics.

Table 5: Descriptive Statistics
for Cohorts Conceived before Intervention

	Endemic		Nonendemic		Difference in Mean
	Mean	SD	Mean	SD	
Goiter rate	13.627	8.917	1.434	1.340	12.193***
Dead	0.448	0.497	0.443	0.497	0.005**
Age of Death	69.474	11.060	69.352	11.100	0.122
Female	0.493	0.500	0.494	0.500	-0.001
Outcomes					
Years of schooling	8.730	2.493	8.915	2.583	-0.184***
Educational attainment					
Above compulsory school (folkskola)	0.378	0.485	0.413	0.492	-0.036***
Above lower secondary school (grunskola/realskola)	0.315	0.465	0.346	0.476	-0.052***
Above highschool (short gymnasial)	0.138	0.349	0.154	0.361	-0.031***
Occupation status					
Middle- and highskill	0.546	0.498	0.568	0.495	-0.022***
High-skill	0.311	0.463	0.333	0.471	-0.022***
SES at Birth					
High SES	0.427	0.495	0.693	0.461	-0.265***
Observations	56,763	–	163,711	–	–

Note: The table reports summary statistics using only cohorts conceived before the intervention (i.e. born before February 1937). The last two columns show the difference in means between endemic and nonendemic districts. The number of observation refers to the size of the subsample regardless of different numbers of missing observations for individual variables. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

6 Empirical Results

6.1 Pooled Results

Table 6 shows the estimation results of equation (1) for four different outcomes. The coefficient for the interaction term, *After* x *Endemic*, is the estimated policy effect. For each outcome, the result for the simplest model without any fixed effects or trends is provided first. Then I show estimates when fixed effects and trends are added in the model gradually. In columns (1), (5), (9), and (13), the estimated coefficients of *After* are positive and statistically significant at the 1% significance level, while those of *Endemic* are negative but also statistically significant at the 5% significance level. This is consistent with the upward trend of outcomes and the fact that there is a pretreatment difference between endemic and nonendemic districts, as shown in table 5 in the previous section. Columns (4), (8), (12), and (16) provide the estimates of the full model in equation (1), which is the preferred one, considering the trend of the outcomes driven by educational reforms and thus changing the occupational pattern of the population²⁵.

For all four outcomes, the estimates are similar across specifications in terms of both magnitude and statistical significance. Furthermore, the results are similar between males and females. Therefore, separate results by gender will not be shown. Referring to the full model in columns (4), (8), (12), and (16), the intervention had statistically insignificant effects on all four outcomes. For individuals conceived after the intervention in endemic districts, the intervention led to an increase of 0.014 years of education which is equivalent to 0.73 weeks of schooling; a decrease of 0.1 percentage points in the probability of attaining over compulsory-level education; and an increase of 0.9 percentage points in the probability of having middle- or highskill occupation and the probability of having highskill occupations.

²⁵As a robustness check, I also conducted my analysis using the interaction between the raw goiter rate and the indicator variable of being conceived in or after February 1937 as the main independent variable for the calculation in sections 6.1 and 6.2. I multiply the coefficients by 12, which is the difference in average goiter rate between endemic and nonendemic districts, for easier comparison with my main results. These results show similar patterns to my main ones, with coefficients that are very close to zero and statistically insignificant without considering SES. For the heterogeneous results, the coefficients for the high-SES group are larger than those for the low-SES group. However, the magnitude of all coefficients is smaller than the magnitude in my main results. This is consistent with the fact that measurement error tends to bias the estimates toward zero, as well as that Højjer's validation suggests that while the ranking of goiter rates among districts is correct, the goiter rate is in general lower than what he observed (see the discussion in section 3). Therefore, my main results with Højjer's classification of endemic districts as the base is preferred and it should be noted that even the robustness check shows the obvious difference in coefficients between the two SES groups.

Apart from being statistically insignificant, these effects are also economically insignificant because of their small magnitude. The magnitude will be discussed further in section 7. In short, these results provide evidence that the intervention had zero policy effect as far as the whole population is concerned.

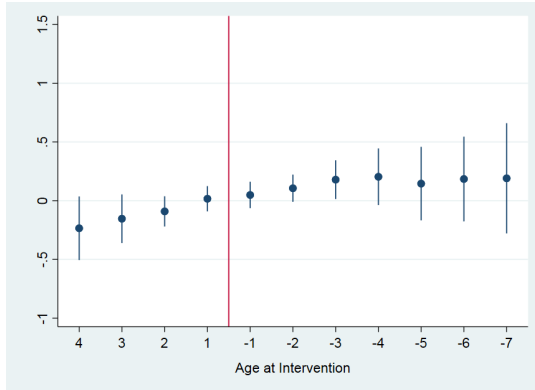
Table 6: Results of Baseline Model

	Years of Schooling				Probability of Attaining Compulsory-Level Education				Probability of Having Middle- or High-Skill Occupation				Probability of Having High-Skill Occupation			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
<i>After X Endemic</i>	-0.103 (0.062)	-0.043 (0.031)	0.014 (0.033)	0.014 (0.035)	-0.018 (0.011)	-0.006 (0.006)	-0.001 (0.006)	-0.001 (0.006)	-0.010 (0.009)	-0.002 (0.005)	0.007 (0.009)	0.009 (0.009)	-0.005 (0.009)	0.003 (0.006)	0.012 (0.007)	0.009 (0.008)
<i>After Endemic</i>	0.554*** (0.057)				0.125*** (0.010)				0.044*** (0.008)				0.022*** (0.008)			
	-0.184** (0.076)				-0.036** (0.014)				-0.022** (0.011)				-0.022** (0.010)			
District fixed effect	No	Yes	Yes	Yes	No	Yes	Yes	Yes	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Cohort fixed effect	No	Yes	Yes	Yes	No	Yes	Yes	Yes	No	Yes	Yes	Yes	No	Yes	Yes	Yes
District-specific linear trend	No	No	Yes	No	No	No	Yes	No	No	No	Yes	No	No	No	Yes	No
District-specific quadratic trend	No	No	No	Yes	No	No	No	Yes	No	No	No	Yes	No	No	No	Yes
Observations	500031	500,031	500,031	500,031	500,031	500,031	500,031	500,031	365,954	365,954	365,954	365,954	365,954	365,954	365,954	365,954
Adj. R^2	0.012	0.034	0.035	0.035	0.016	0.039	0.040	0.040	0.002	0.012	0.012	0.012	0.001	0.010	0.011	0.011

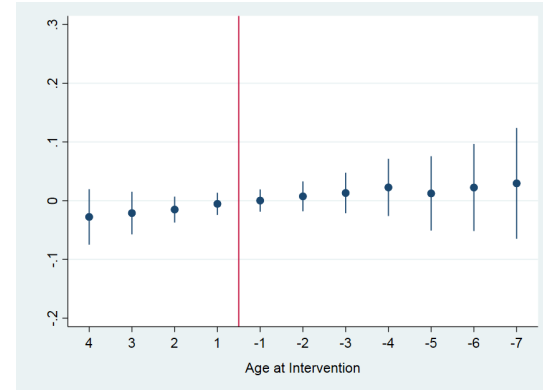
Note: Robust standard errors clustered at district-level are presented in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure 5 visualizes the estimated coefficients \hat{b}_t in equation (2) for each outcome. On the x-axis, the birth year t is replaced by the age of intervention, with year as the unit. The reference group has the age of zero at the intervention. Each dot on the figure represents a point estimate, while the vertical line passing through the dot is the 95% confidence interval for the estimate. In plain language, each coefficient is the estimated difference in outcome between those born in year t and the reference group, which is additional for those born in endemic districts. The vertical red line indicates the start of the intervention. Compared with the reference group, birth cohorts to the right of the vertical red line had longer exposure to the intervention in utero. Therefore, estimates for these cohorts explain whether in utero exposure to the intervention led to better outcomes. By contrast, birth cohorts to the left of the vertical red line began to be exposed to the intervention later in childhood than the reference group. Therefore, estimates for these cohorts illustrate whether exposure to the intervention later in childhood led to worse outcomes. For each outcome, the scale of the y-axis here is the same as in the graphs showing the results of dynamic specification by SES in figure 6 for easier comparison.

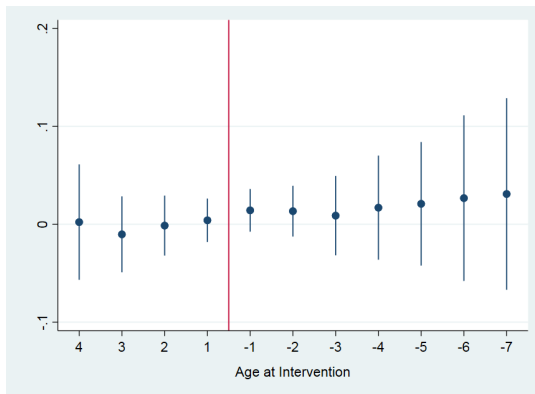
Consistent with the baseline results, all coefficients are statistically insignificant. Let us consider the two coefficients with the greatest absolute values for explanatory purposes. The results of years of schooling show that individuals who began to be exposed to the intervention when they were four years old had 12 weeks less schooling than those exposed to the intervention since birth, while those conceived four years after the intervention stayed in school 11 weeks more than the reference group with partial exposure to the intervention in utero. Even though this is consistent with the prediction that those older than the reference group would have worse outcomes and those younger would have better outcomes, the magnitudes are, again, economically insignificant. Similar patterns can be observed in the other three outcomes shown in figure 5. These results provide evidence for zero effect of the intervention on the whole population even if a more flexible model with different effects is allowed for each birth cohort.



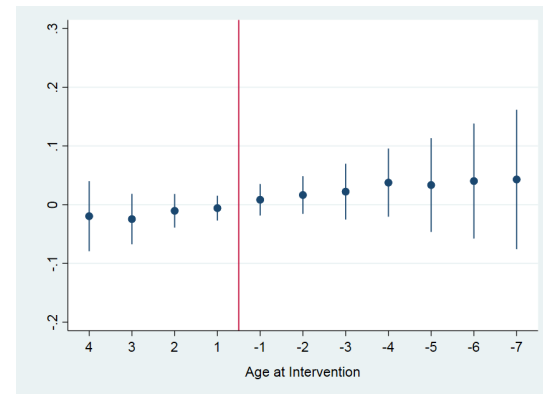
(a) Years of Schooling



(b) Probability of Attaining over Compulsory-Level Education



(c) Probability of Having Middle- to High-Skill Occupation



(d) Probability of Having High-Skill Occupation

Figure 5: Results of Dynamic Specification

6.2 Differential Impacts by SES

My second hypothesis is whether the policy effects of the intervention were different between individuals having parents with high and low SES backgrounds at birth. The parish's poverty level is used to proxy an individual's SES background²⁶. Table 7 shows the estimation results of equation (1) for two subsamples, those born in poor parishes and others. Panel A shows the results for individuals with high SES at birth while panel B provides results for the low-SES subsample. The results reveal heterogeneous policy effects for individuals with high- and low-SES backgrounds. This is especially true for the occupational outcomes. The point estimates are very close to zero for individuals with low SES.

By contrast, for the high-SES group, the intervention increased the probability of having high-skill occupations by 2.5 percentage points. The point estimate is statistically significant at the 1% significance level. The result is similar if I consider both middle- and high-skill occupations, but the point estimate is not statistically significant because the standard error almost doubles the size of the former result. This is equivalent to an 8.04 percent increase of the probability of having high-skill occupations. A similar result is found for *years of schooling*. However, the magnitude of policy effects is less profound. The result shows that for individuals with high SES, the intervention led to an increase of 0.073 years of schooling which can be translated into 3.80 weeks of schooling or a 0.84 percent increase.

²⁶I also used father's occupation from the 1960 census as a second measurement of SES. However, this measurement suffers from several problems, including sample selection and endogeneity. The results are also imprecise, and the standard errors of point estimates are double compared with the results using my primary measurement. Considering the problems with this second measurement, the result using it will not be presented here. However, they are available upon request.

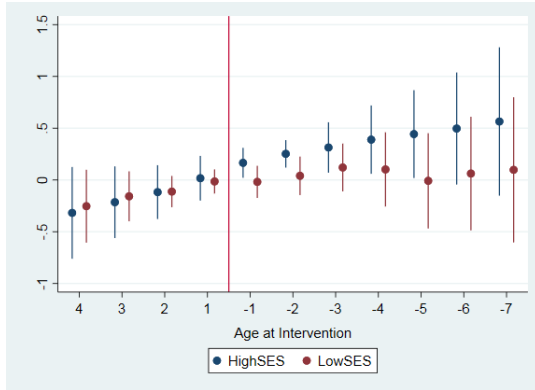
Table 7: Results by SES

	Years of Schooling	Probability of Attaining over Compulsory-level Education	Probability of Having Middle- or High-Skill Occupation	Probability of Having High-Skill Occupation
	(1)	(2)	(3)	(4)
Panel A: High SES				
<i>After X Endemic</i>	0.073*	0.008	0.024	0.025***
	(0.042)	(0.008)	(0.015)	(0.008)
Observations	304,558	304,558	222,424	222,424
Panel B: Low SES				
<i>After X Endemic</i>	-0.030	-0.009	-0.006	-0.003
	(0.051)	(0.008)	(0.011)	(0.013)
Observations	195,473	195,473	143,530	143,530

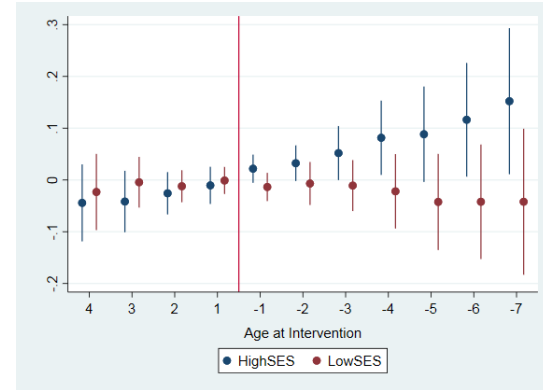
Note: Each coefficient represents the estimated result of equation (1) using a subsample with specific family backgrounds. Robust standard errors clustered at district level are presented in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The estimation results of the dynamic specification separately for individuals with high- and low-SES backgrounds provide further support for the heterogeneous policy effects for these two groups. Figure 6 presents the results. The blue dots represent the point estimates for the high-SES group, and the red dots for the low-SES group. For the outcomes, *years of schooling* and *probability of attaining over compulsory-level education*, it is clear that cohorts born earlier than the reference group had worse outcomes compared with the reference group. Importantly, these cohorts suffered to the same extent regardless of their SES background, because the point estimates almost overlap each other for these cohorts.

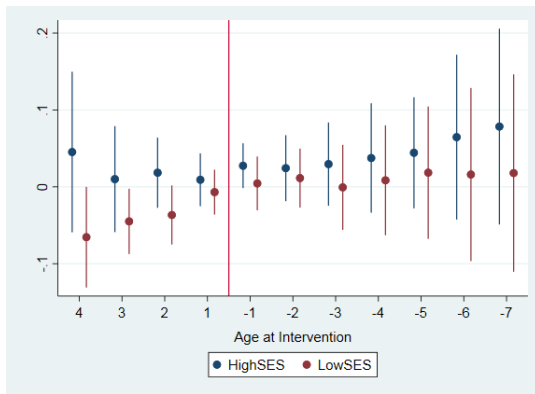
On the contrary, we can observe a clear divergence of the high- and low-SES groups for cohorts conceived after the intervention, which implies that the policy only affected those with high-SES backgrounds. The divergence is also consistent with the fact that the information spread gradually to households. More specifically, for the high-SES group, in utero exposure to the intervention increased schooling by 8.58 to 29.38 weeks, which is equivalent to 1.89 to 6.47 percent. This also increases the probability of attaining over compulsory-level education by 0.02 to 0.15 percentage points which can be translated into a 5.82 to 40.21 percent increase. The results for *probability of having high-skill occupation* also shows that there is a divergence between the SES groups after the intervention. However, as there were different trends preintervention, the results do not provide as strong evidence as the educational outcomes for the effects of being exposed to the intervention in utero.



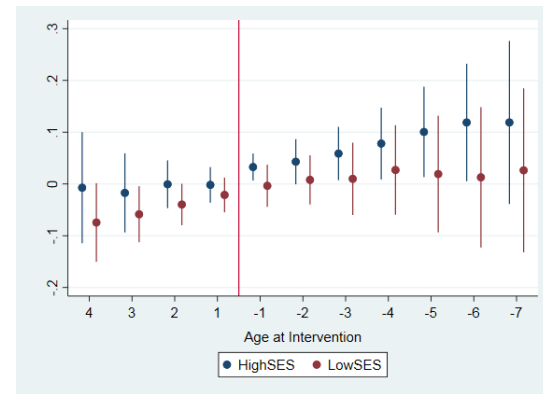
(a) Years of Schooling



(b) Probability of Attaining over Compulsory-level Education



(c) Probability of Having Middle- or High-Skill Occupation



(d) Probability of Having High-Skill Occupation

Figure 6: Results of Dynamic Specification by SES

7 Effects of the Policy

7.1 Magnitude of Results

Table 8 shows a simple comparison of my results for the low-SES group (*Low SES* column) and the high-SES group (*High SES* column) and the results in the literature (*Literature* column). They all show the results in percentage changes. I compare three outcomes that are available in my study and studies that also have similar variables. *Years of Schooling* is defined exactly the same in these studies, while two other variables are defined differently. Definitions of the variables in the literature can be found in table 2. It is hard to find outcome variables that are exactly the same in different studies because of the different institutions and data available in different countries. The two studies that have similar variables, by coincidence, study policies that also have similar treatment intensity, making the chosen literature more comparable. In the *Low SES* column, it can be seen that the policy has negative effects on all three outcomes, but the magnitudes of the percentage decreases are all not comparable to what is found in the literature. In the *High SES* column, while the effects on occupational outcome are comparable to the effects found in the other studies, the effects on the educational outcomes are also positive but much smaller in magnitude. This suggests that the intervention in Sweden benefited only the high-SES group, but the benefits were much smaller in magnitude than those resulting from similar policies in other countries.

Table 8: Comparison of Policy Effects, in Percentage Changes

	Low SES	High SES	Literature
Years of schooling	-0.34%	0.84%	12% (Deng & Lindeboom, 2018)
Educational attainment	-2.38%	2.12%	6%-8% (Deng & Lindeboom, 2018) (Politi, 2014)
Probability of having good occupation	-0.96%	8.04%	10% (Politi, 2015)
Source	Section 6.1	Section 6.2	Table 2

7.2 Mechanism

Compared with similar policies studied by the block of literature in table 2, the treatment magnitude of this Swedish policy is close to that found in studies of China's and Switzerland's policies. The difference in the goiter prevalence rate between endemic and nonendemic areas is 12 percentage points in China's study, while in this study, the difference is also 12 percentage points. However, the iodine content added to salt was 40mg/kg

in China, but in Sweden, the government only *recommended* that an iodine level of 10mg/kg be added to salt. Food stores could continue to sell noniodised salt or iodised salt with iodine contents lower than the recommended level²⁷. Advertisements from newspaper archives, such as the one in figure 7, also show that iodised salt was already available before the intervention. Therefore, the channel through which this intervention could improve human capital must not have been the increase in the availability of iodised salt, but rather the spread of information that changed households' consumption behaviour.

Thus, the explanation of my results is related to the question of who were the recipients of the information. There are two hypotheses regarding the effects of the intervention, based on the content of the policy and anecdotal evidence on the reaction to this intervention. First, I predict that pregnant women from endemic places benefited more than those from nonendemic places. Thus, their children should have benefited more in terms of human capital improvement. Second, families with higher-SES backgrounds were more likely to react to this intervention, and their children should have been able to gain more from this intervention as well. This hypothesis is based on the fact that these families had better access to prenatal advice. For instance, as reported by Bhalotra, Karlsson, and Nilsson (2017), in the 1930s in Sweden, only about 30 percent of births took place in hospitals. Therefore, children born to low SES families at that time might not have had access to prenatal care and thus may not have gotten this information from physicians that iodine consumption benefits children's growth²⁸. These findings and the possible explanations for them suggest that voluntary salt iodisation is inefficient compared with the mandatory iodisation and it can unintentionally widen inequality.

7.3 Possible Confounding Events

Educational reforms in the first half of the 1900s and World War II both possibly confound the findings of this study. Therefore, it is worth taking a closer look at these events. The first educational reform my studied cohorts might have experienced was the increase in duration of compulsory school from six to seven years starting in 1936. This was gradually implemented

²⁷This point was confirmed through email correspondence with the Food Authority (Livsmedelsverket).

²⁸Iodised salt was around 1.5 times more expensive than noniodised salt, according to information from the central archive and newspaper archive. However, because of the low cost of salt in general, this price difference may not have been influential in creating different consumption patterns of iodised salt between high- and low-SES households.

Figure 7: Clipping from the 6 March 1930 issue of the newspaper *Dagens Nyheter*



across school districts over 12 years, from 1936 to 1948. The second reform was a further increase to nine years, which was gradually implemented in 1950s to 1960s with all school districts mandated to have made the change by 1969 (Costas & Mårten, 2005; Fischer et al., 2013, 2016). While almost all of my studied cohorts should have experienced the first reform, the second should not have affected so many of them as to influence my main findings. Another event occurring during the time period of my study was World War II, which started in September 1939. While Sweden remained neutral during the entire war, its economy and food supply were affected by the blockage of transportation between Sweden and other European countries during the war. Empirical evidence, however, has found that there were no statistically significant changes in the height and weight of school-aged children in Sweden during that period (Angell-Andersen et al., 2004). Furthermore, my results in the dynamic specification in figure 6 shows a clear pattern in the outcome variables, except *probability of having middle- to highskill occupation*, with the coefficients starting to be positive and statistically significant for those born in and after February 1937 and from high-SES families²⁹. This is also the point in time when the divergence of outcomes between the high- and low-SES groups started in endemic

²⁹The detailed regression results for the dynamic specifications are not presented here to save space. However, they are available upon request.

districts. Given all this, the educational reforms and World War II should not be the driving forces for the main findings in this study.

8 Conclusion

Information campaigns are ubiquitous policy instruments, not least with respect to poverty reduction, schooling and health-related behaviour. Information also influences the compliance rate of an intervention even if it is not originally an information campaign (Ravallion, 2016). However, researchers are still striving to understand what roles information plays in helping policy makers achieve their goals. At present, we understand that the way information is spread and the responsiveness of individuals to information are vital determinants of the success of an information campaign and even other types of intervention (Kremer et al., 2019). Further studies are needed, however, to add to our limited understanding of how these interventions work.

In the context of salt iodisation policy, this paper provides the first piece of evidence of the unequal and limited benefits resulting from an information campaign promoting the consumption of iodised salt. Making use of the endemic characteristics of iodine deficiency (ID) and the combination of the Swedish population registers with two unique historical data sources of preintervention ID prevalence and SES, I have found that this information campaign only benefited individuals with high SES at birth, while the magnitude of the benefits gained by this group of individuals was still small compared with the benefits from similar policies in other countries where salt iodisation was made mandatory or the availability of iodised salt was increased drastically by an intervention. These findings contribute to our knowledge of the impact of iodine consumption on human capital development and the effectiveness of information campaigns. Social planners should take account of these findings when designing policies to improve human capital development of the population.

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Appendix

Figure A.1: Information from the National Board of Health, 1936

angående förebyggande åtgärder mot endemisk struma;

den 27 april 1936.

Struma förekommer som bekant i vårt land liksom i många andra länder i endemisk form. Den endemiska strumans utbredning hos oss har nyligen närmare studerats. En skildring av dessa undersökningar återfinnes i Svenska Läkaresällskapets handlingar, årgång 1931, band 57. Å sid. 53 i denna avhandling finnes en karta, som ungefärligen anger gränserna för den endemiska strumans utbredning.

Den endemiska struman inverkar ofördelaktigt på befolkningens kroppsliga och själsliga utveckling. Dess profylax är därför föremål för livligt intresse. Även om etiologien ännu icke är klarlagd, synas starka skäl tala för åsikten, att en väl ordnad s. k. jodprofylax i trakter med endemisk struma medför stora fördelar. Medicinalstyrelsen har haft hithörande frågor under övervägande och vill härigenom uppmana tjänsteläkare i trakter med endemisk struma att verka för genomförande av en dylik profylax.

Denna kan anordnas på flera sätt. Sälunda kan i trakter, där ständig tillgång till saltsjöfisk förefinnes, avsedd jodtillförsel vinnas genom regelbundet användande av dylik fisk en till två gånger i veckan. Därvid bör icke blott köttet utan även körtlar, framför allt levern, användas. På vissa håll utomlands har regelbunden jodtillförsel även erhållits genom att korna utfodrats med jodhaltigt foder. Jod övergår nämligen i mjölken. Vidare få barn, som dagligen taga en liten dos fiskolja, sitt jodbehov säkrat.

De tvenne metoder, som i utlandet mest brukas och vilka båda redan i Sverige prövats på flera håll, äro dels jodtillsats till det koksalt, som användes i hushållet, s. k. »koksaltprofylax», och dels utdelande till skolbarnen genom lärarpersonalen av avpassad mängd jodsalt i lämplig form, s. k. »skolprofylax». Denna har först prövats i vårt land i Sandviken (Gibson och Backman; Svensk Läkartidning 1924, sid. 113 och 1927 sid. 1317), koksaltprofylaxen i Falun (Hollström, Nordisk Hygienisk Tidskrift, sid. 219).

Koksaltprofylaxen

tillgår så, att sedvanligt koksalt för hushållets bruk utbytes mot ett koksalt, till vilket satts en ringa mängd jodsalt, exempelvis ett gram jodsalt till 100 kilo koksalt. Sådant »joderat koksalt» kan för närvarande i vårt land erhållas såväl importerat som berett inom landet.

Lämpligen bör tjänsteläkare övertyga sig om att såsom joderat koksalt utbjuden vara verkligen är tillräckligt jodhaltig. Prov kan utföras med följande reagens, som dock ej är hållbart mer än en dag.

5 % stärkelselösning, 10 cm³ (ej den svenska farmakopéns med kvicksilverjodid konserverade)

Acid. sulf. dilut. (S. F.), 5 droppar

0.5 % natriumnitritlösning, 4 droppar.

Vid reaktionens utförande packas saltet samman i en sked eller skål, så att en jämn yta bildas, och på denna drypes en droppe av reagenset. Vid tillfredsställande jodhalt, minst 0,1 milligram jod på 10 gram koksalt, blir färgen djupblå. Vid svagare jodhalt blir den blåa färgen allt mindre uttalad, vid 0,02 mil-

ligram jod på 10 gram koksalt knappt synlig. Är saltet orent, kan färgtonen bli violett.

Skolprofylaxen

består däri, att lärarepersonalen ombesörjer att skolbarnen med regelbunden mellantid intaga en viss mängd jod. Vid de första försöken i Sandviken ordnades så, att barnen höst och vår under tio dagar i sträck dagligen erhöles en lakritspastill innehållande 0,2 gram jodnatrium. I enstaka fall inträder härvid en lindrig jodism. I Sverige har intet fall av Morbus Basedow i samband med skolprofylax påvisats. I Schweiz, Tyrolen m. fl. länder erhålla barnen numera oftast en gång i veckan, på sina håll dagligen, under hela skolåret en karamell, i vilken ingår en mindre dos jod. Denna dos var först 10 milligram i veckan eller mer, men har sedan minskats till hälften eller mindre; ända ned till 1 milligram jod i veckan. Vid sådant framgångssätt riskeras icke fall av jodism. En annan fördel är, att dessa jodkarameller med små doser jod tagas hellre än sådana med jod i större doser.

Val av metod.

Skolprofylaxen har den fördelen, att den kommer praktiskt taget alla barn i skolåldern till godo. Den lider av den svagheten, att den icke tillgodoser jodtillförseln under den viktiga utvecklingstiden dessförinnan, vilken är att beakta särskilt med hänsyn till att vårt land rymmer ej så litet kretinism. Där denna form av jodprofylax kommer till användning, bör man därför på annat sätt tillgodose den havande kvinnans, det späda barnets och koltbarnets behov, liksom man bör förena skolprofylaxen med undervisning om fortsatt försiktig jodprofylax efter skolåldern.

Tillhandahållandet av joderat koksalt för frivilligt bruk har den fördelen, att den kan komma alla åldrar till godo, men den nackdelen, att en stor del av befolkningen länge ställer sig utanför varje jodprofylax. Intet hinder förefinnes för ett samtidigt införande i en trakt av både koksaltprofylax och skolprofylax, då helst i dess schweiziska form. En sådan kombination är tvärtom att anbefalla.

Befolkningen bör uttryckligen varnas för intagande av okontrollerade jodpreparater, vilka i trakter med endemisk struma medföra stora risker (Basedow). Ovan anbefallda form av jodprofylax är däremot enligt hittills samlad erfarenhet riskfri. Den tillför ej heller kroppen större jodmängd än den, som vid havskusten ingår i varje människas kost.

Slutligen vill medicinalstyrelsen framhålla att, även om omförmälda förebyggande åtgärder mot endemisk struma kan anses ofarlig för personer, som lida av struma, behandlarna av dessa sjukdomstillstånd icke avses i detta cirkulär.

Samtliga tjänsteläkare i trakter med endemisk struma böra ägna denna fråga sitt intresse. Förste provinsialläkare böra i sina årsberättelser omförmäla i vilka distrikt jodprofylax blivit anordnad, vilka metoder, som kommit till användning, och vad annat i detta samband kan vara av intresse.

Stockholm den 27 april 1936.

Figure A.2: Letter from Östergötland County Government, 1937

Förste provinsialläkarens råd angående
förebyggande åtgärder mot struma.

Till Husmödrarna i Östergötland.

Som bekant är struma vanligt förekommande inom Östergötland. Struman (förstoring av sköldkörteln) ger sig till känna genom ansvällning å främre delen av halsen. Denna åkomma behöver visserligen icke i regel jämföras med sjukdom, men det oaktat kan struma ha menlig inverkan på hälsotillståndet. Det är därför av stor betydelse ur folkhälsans synpunkt att uppkomsten av struma förebygges.

Erfarenheten har lärt, att en regelbunden jodtillsats till födan motverkar uppkomsten av struma. En praktisk metod i detta avseende är att begagna ett hushållssalt, tillsatt med en ringa mängd jod. Detta förfarande har med god framgång tillämpats inom vissa andra länd. Även inom vårt land begagnas dylikt hushållssalt i allt större utsträckning. Det finnes för närvarande att köpa i handeln under olika beteckningar, såsom John Bull, Union eller Falksalt och betingar ett pris av 25 å 30 öre kg.

Förste provinsialläkaren får härmed fästa uppmärksamheten på vikten av att ett hushållssalt av angiven typ måtte komma till allmänt bruk i våra hem i Östergötland. Saltet bör användas året om, såväl till matlagningen som bordssalt. Det är av vikt, att barnen redan från småbarnsåldern få denna jodtillsats till sin näring, ävensom att havande och ammande kvinnor använda detsamma i dagligt bruk.

Linköping den 16 januari 1937.

Chapter 3

Impact of Mother Tongue Education on Labour Market Outcomes and Educational Inequality*

Debbie Lau[†]

Abstract

This study exploits a sharp policy change in Hong Kong when half of the secondary schools were mandated to change the teaching language from English to Chinese from the school year 1998-1999 onward. The policy impact is identified with regression discontinuity design and the main dataset is the census data in 2011. The results show that mother tongue education increases an individual's unemployment rate and decreases his or her likelihood of having high-paid occupation. However, due to limitation of the dataset, the study finds insignificant but imprecise estimates of the differential impact on the likelihood of university attendance between individuals with different socioeconomic status, which has been a controversial topic in the society in Hong Kong. The first set of results, nevertheless, warrants a discussion of whether mother tongue education enhances learning or worsens an individual's labour market outcomes.

Keywords: policy evaluation, causal inference, mother tongue education, inequality

JEL Classification: I21, I24, I26, J24, J62

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1 Introduction

Sizable returns to years of education have been found in terms of both monetary rewards in the labour market and other nonpecuniary benefits (Oreopoulos & Salvanes, 2011). Besides *quantity* of education (i.e., years of schooling), what is even more influential is the *quality* of those years of education. Without a doubt, the medium of instruction (MOI), which refers to the language used to teach, is one of the most important inputs into the education production function because it directly affects the productivity of other school inputs. In simple terms, teaching students in a language that they do not understand well will be less effective even if there are good teachers and relatively small classes. Unfortunately, this is exactly the problem faced by as much as 40% of the global population, who do not have access to education in their mother tongues (Walter & Benson, 2012). For instance, 38% in East Asia and the Pacific do not have access to education in their mother tongues, and the corresponding percentage in Sub-Saharan Africa reaches 87% (United Nations Development Programme, 2004). Most countries in these regions were historically colonized, and the former colonial language often remains the official language and has become the dominant language in the labour market.

While mother tongue education seems to be a straightforward solution, a growing strand of economics literature supports the existence of premiums in the labour market for skills in the dominant language (Angrist, Chin, & Godoy, 2008; Angrist & Lavy, 1997; Azam, Chin, & Prakash, 2013; Chakraborty & Bakshi, 2016; Godoy et al., 2007; Levinsohn, 2004; Munshi & Rosenzweig, 2006; Shastry, 2012). Policy makers often face the dilemma of either allowing schools to continue using the former colonial language as the MOI or having them switch to teaching in the students' mother tongue. On the one hand, students may struggle to understand lessons taught in the former colonial language as the MOI, and their parents often cannot help them. On the other hand, using the mother tongue as the MOI can deprive an individual of the chance to learn the dominant language. Currently, this stream of literature is represented almost exclusively by studies that estimate how the returns to English skills evolved during India's transition to trade liberalization. Other studies also mostly focus on developing countries, and thus there is scarce evidence on this issue in developed countries. The biggest empirical challenge to studying this topic is that mother tongue education is not random. Only a handful of studies have attempted to study the causal effect of this policy by exploring national reforms of the MOI (Angrist et al., 2008; Angrist & Lavy, 1997; Chakraborty & Bakshi, 2016). These studies so far have found mixed results regarding the effects

of mother tongue education on labour market outcomes.

This study contributes to the sparse empirical evidence on the impact of mother tongue education in a developed region by examining the effects of an MOI reform in Hong Kong (HK) whereby over 50% of secondary schools were mandated to change their MOI from English to Chinese. The advantage of studying this reform is that it applies only to students entering secondary school from the school year 1998–1999 onward. The government also requires strict compliance with compulsory school law that determines the timing of school entrance based on one’s age. Therefore, month of birth is used as a running variable in a regression discontinuity design (RDD) to define those who were educated under the new system. This allows me to study how mother tongue education causally affects labour market outcomes of individuals. This study uses HK census data, which include month of birth and extensive information on educational attainment and labour market outcomes of a random sample of the population. I also investigate whether students from families with lower socioeconomic status (SES) have less chance of entering university after the reform. English proficiency is a prerequisite of entering university and after the reform, the remaining schools that were permitted to keep using English as the MOI were mostly located in school districts with higher SES. This raises the question of whether mother tongue education becomes an inferior track if some schools are still allowed to teach in the dominant language of the labour market, and these schools are not equally accessible to students with different SES backgrounds.

My main findings show that mother tongue education significantly increases the unemployment rate by over 40%, conditional on the fact that an individual participates in the labour market. This is both statistically and economically significant. Conditional on being employed, an individual exposed to the reform is less likely to have a high-rank occupation. This is consistent with the empirical evidence from developing countries that mother tongue education decreases one’s likelihood of having a high-rank occupation. However, due to limitation of the dataset, the study finds insignificant but imprecise estimates of the differential impact on the likelihood of university attendance between individuals with different socioeconomic status, which has been a controversial topic in the society in Hong Kong. The first set of results, nevertheless, warrants a discussion of whether mother tongue education enhances learning or worsens an individual’s labour market outcomes.

The remainder of this paper is organized as follows: section 2 summarizes

researchers' current understanding of language skill premiums; section 3 details the structure of education in Hong Kong and the reform; section 4 illustrates my identification strategy; section 5 describes the datasets used in this study; section 6 presents the empirical results; and section 7 concludes.

2 Literature Review

Empirical evidence about how language skill affects one's productivity is limited, because data on individuals' language skills are not easily found. There is some descriptive evidence showing that one's skill in the language dominant in the labour market is highly associated with better earnings. Levinsohn (2004) finds that in India, individuals speaking English at home earn around 20 percentage points more in monthly wages than those speaking other languages at home. Azam et al. (2013) also shows economically significant results for individuals speaking fluent English in India. Moreover, the results reveal that the language skill premiums are cut by half for individuals speaking only a little English, and that these language skill premiums are correlated with one's educational level. Godoy et al. (2007) find that in Bolivia, in an autarkic society with hunters and farmers, Spanish skill premiums raise earnings through better access to credit and use of modern production technologies. While this empirical evidence is invaluable to motivate better understanding of the returns to language skills, they all suffer from omitted variable bias because individuals' unobservable characteristics are correlated with both language skill and earnings.

A few economics studies attempt to estimate the causal effect of language skills by exploiting language-related educational reforms, under the assumption that either using a language as the MOI or teaching it as a subject improve one's ability in that language, with mixed results. Chakraborty and Bakshi (2016) examine the effects of a policy in India whereby English as a subject was abolished in West Bengal primary schools. They find that weekly wages decreased by 26% for the cohorts exposed to the policy. Angrist and Lavy (1997) study another type of policy, which changed the MOI from French to Arabic in Moroccan secondary schools. They observe a sharp reduction in French writing skills among students attending secondary schools after the reform. Their results also show that the decrease in French skills caused earnings to drop by more than 17%. On the other hand, Angrist et al. (2008) find that the change of MOI from English to Spanish in secondary schools in Puerto Rico has little effect on self-reported English speaking skills, which is contrary to policy makers' presumption that using English as the MOI would improve students' English skills.

An interesting feature of the strand of economics literature on language skill premiums is the focus on how these premiums evolve during globalization in developing countries, contributing to the enormous literature on skill-biased economic development. These studies, however, look almost exclusively at India. Levinsohn (2004) uses two cross-sectional datasets, recorded before and after the start of trade liberalization in India and finds that the calculated returns to English language skills increase by 39% between these two time points. Munshi and Rosenzweig (2006) reveal that the interplay between social network and gender determined the evolution of returns to English during globalization in Bombay city. While lower-caste girls are increasingly likely to enter schools with English as the MOI, boys from the same caste are influenced by the working-class social network of their caste and continue to study in local language schools in preparation for joining the workforce. Shastry (2012) provides a surprising example in which skill-biased development does not lead to widening inequality. He claims that people in India with mother tongues having a greater linguistic distance from Hindi have a lower cost of learning English. He finds that after trade liberalization, information technology companies are more likely to set up offices in districts where these people reside. At the same time, school enrollment grows at a faster pace in these districts. As a result, the English skill premiums increase much more slowly in these districts than in the others.

Given the potential skill premiums of the dominant language in the labour market, an educational policy that distinguishes the accessibility of education of the dominant language for different students collides with the notion of tracking. Pekkarinen, Uusitalo, and Kerr (2009) and Malamud and Pop-Eleches (2011) study tracking of students into vocational and academic tracks and find a link between tracking and educational inequality. Pekkarinen et al. (2009) observe that postponing the age when students can enter vocational schools in Finland reduces intergenerational elasticity of income. This is the only study that is able to address the effect of tracking directly on intergenerational mobility of income. Malamud and Pop-Eleches (2011), however, show mixed results. They find that a similar policy in Romania increases the probability that disadvantaged students will finish high school in the academic track but not enter university. They argue that postponing tracking without university expansion cannot increase university entrance for disadvantaged students. Guyon, Maurin, and McNally (2012) study another type of tracking policy: tracking students by ability into elite and nonelite schools. They exploit a policy change in Northern Ireland that generated a 15% increase in enrollment in elite schools. They find that this

policy increased the number of students who passed subsequent high-stakes standardized exams by about 4-7%. If mother tongue education is carried out in a way that only a limited amount of schools use the labour market dominant language, and students with higher SES have better access to these schools, this raises the question of whether students are tracked by their SES for education in the dominant language, which affects their labour market outcomes. This unexpected potential consequence of MOI policy remains unexplored in the literature.

3 Institutions and Reform

3.1 Structure of Education

Figure 1 shows the structure of education in HK¹. Before 1998 (left panel), primary and lower secondary education were free and compulsory. The school year starts on 1 September. Children enter primary schools the first September after they turn six years old. As HK was a British colony, both Chinese and English have been official languages². However, each of these languages is dominant in different situations. Regarding education, while most disciplines have been taught in English in university, except for a few such as Chinese language and Chinese literature, most primary schools teach in Chinese in all subjects except English³.

After six years of primary education, students are allocated into lower secondary schools through the Secondary School Places Allocation system (Education Commission, 1997). The entire HK is divided into 18 school districts. Within each district, all students in the sixth year of primary school are divided into five equal-size ability groups called bands, with the first band representing students with the best school performance. School performance refers to a weighted average of in-school exam results from the last two years of primary school and the result of the standardized Academic Aptitude Test⁴. Band 1 students have priority to select lower

¹The education system was overhauled beginning in the school year 2009–2010. More details can be found on the website of the Education Bureau (<http://www.edb.gov.hk/en>). However, the school cohorts in this study are hardly affected by this new system.

²HK was a British colony for 154 years from 1843 to 1997, after the opium war (Hsu, 2000). English was the sole official language until the Official Languages Ordinance was enacted in 1974, after which Chinese has been recognized as another official language of HK.

³In the school years 1996–1997, 1997–1998 and 1998–1999, 99.5% of primary schools taught in Chinese. This figure was announced in the government’s press release on 7 July 1999 (see www.info.gov.hk).

⁴This exam tests students’ language and logic skills, which theoretically do not require

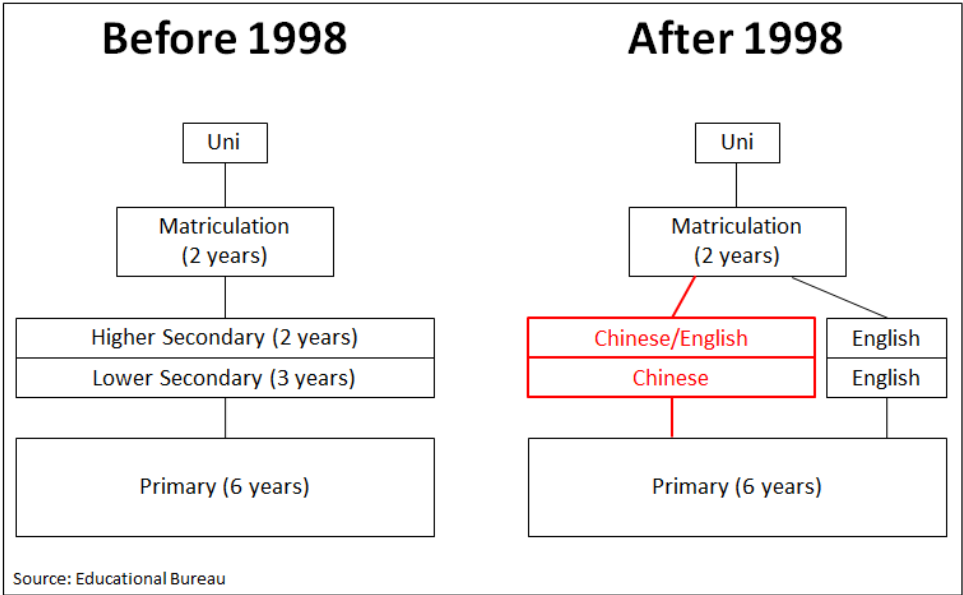


Figure 1: Structure of education in HK

secondary schools in their district. Students can be promoted to the higher secondary level in the same schools after three years at the lower secondary level.

The first high-stakes standardized exam comes afterward. After two years of higher secondary education, all students sit for the HK Certificate of Education Examination and compete for places at the matriculation level. On average across school years, only about one-third of higher secondary students are promoted to the matriculation level. Students prepare for university admission at the matriculation level. Then they take the HK Advanced Level Examination for university admission. This is another competitive high-stakes standardized exam, and only about one-third to one-half of the matriculation students can enter university every year.

3.2 MOI Mandate

The HK government started to encourage secondary schools to teach in Chinese beginning in 1984⁵, but it did not mandate the MOI of individ-

subject knowledge in courses in primary schools.

⁵The year 1984 is the year when the Sino-British Joint Declaration was signed. This document confirmed the handover of Hong Kong back to China from 1 July 1997.

ual schools until the school year 1998–1999. In 1996, the government announced that it would issue guidelines to all schools on their MOI in 1997, which would take effect starting in the school year 1998–1999 (Education Bureau, 1997). The government promoted this policy with the rationale that mother tongue education can enhance students’ learning. The initial plan was to have all secondary schools use Chinese as the MOI at the lower secondary level. However, this plan was refined as a result of the opposition from schools. In the end, the government agreed to allow a limited number of schools to keep English as the MOI based on three criteria: performance of their intakes in the prior three years, quality of the teachers, and the schools’ strategy to support teaching in English. In April 1998, the government also published a list of schools with information on their mandated MOI.

As shown in figure 1 after the reform in 1998 (right panel), lower secondary schools are divided between Chinese and English schools. This policy applies only to new intakes into public lower secondary schools after the reform. At the higher secondary level, Chinese schools can choose to teach in English under some conditions set by the government. This policy created a sharp increase in the proportion of Chinese schools among public secondary schools. The proportion of enrollment in private schools is low in HK⁶. As figure 2 shows, before the school year 1998–1999, about 19% of public secondary schools were Chinese schools. The percentage jumped to around 73% from the school year 1998–1999 onward⁷. At student level, in the school year 1998–1999, the proportion of students entering Chinese schools was 73.08% (Tsang, 1998), which is consistent with the numbers at school level in the figure⁸. This policy has been controversial, and students, parents, and principles of Chinese schools had been protesting for an overhaul of the policy because of the negative labels imposed on their schools. As a result, starting in the school year 2010–2011, secondary schools are no longer mandated to be Chinese or English schools (Education Bureau, 2010). The MOI reform, however, is the only significant change of education experienced by the cohorts I study.

One noteworthy thing is that English schools are not distributed evenly across districts. The magnitude of the change varies across districts: it is

⁶According to the Annual Digest of Statistics, published by the HK Census and Statistics Department, the proportion of secondary school students enrolled in private schools was around 11% in 1995–1996 and trended down to around 7% in 2001–2002.

⁷The number of schools has been stable at around 400 before and after the reform.

⁸The proportion at student level is only available for only one year. Therefore, I use the figures at school level to visualize the change brought about by the policy across school years.

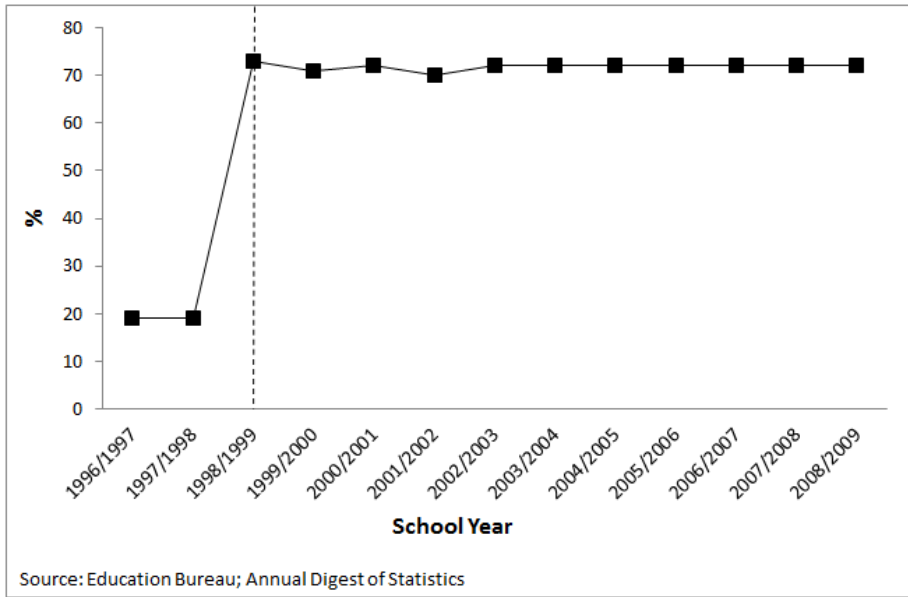


Figure 2: Proportion of Chinese schools in HK

larger in districts with low average SES of households. In figure 3⁹, I use the proportion of university degree holders in each district in early 1998 as a proxy for SES. The darker the colour a district, the higher the proportion of its residents who hold degrees. The numbers indicate the proportion of enrollments into Chinese schools at the lower secondary level in each district after the policy change. The leftmost selected district has the lowest SES; the two selected districts at the bottom are where the central business area is located and have the highest and second- highest SES. The proportion of enrollments into Chinese schools is above the HK average (73%) in the lowest SES district, while the proportions for the other two districts are far below the HK average.

Fierce debate over this school-based language streaming policy, as the media termed it, continued throughout the school years when the policy was in effect. The intense discussion centered on two questions: Does teaching in Chinese worsen students' future productivity? And does this reform hinder students with low SES from entering university? Education researchers have found mixed results, leaving the questions without clear answers. Using a stratified sample of students with types of schools as strata, Tsang (1998) finds that although students in Chinese schools perform better in

⁹I drop one district in figure 3 because a lot of highly educated expatriates live there and it has fewer school-age children.

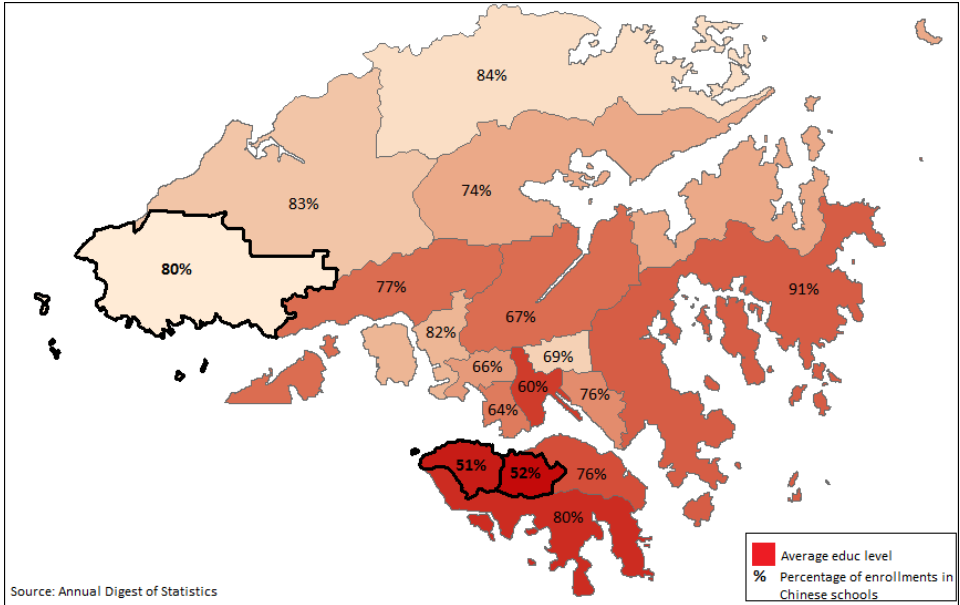


Figure 3: Proportion of enrollments into Chinese schools after the reform by districts

subjects other than English, they do worse in English in the two high-stakes exams, which lowers their chance of entering university. Conversely, Ho and Man (2007) using test results from the Programme for International Student Assessment (PISA) find that students in Chinese schools do not perform worse than their counterparts in English schools. There are also mixed results regarding whether the reform widened differences between schools and created greater inequality (Ho & Man, 2007; Tsang et al., 2004).

4 Identification Strategy

4.1 Regression Discontinuity Design

Equation (1) is the reduced-form regression to study the policy impact on labour market outcomes. Labour mkt outcomes_{*i*} are several outcomes including educational attainment, unemployment, and type of occupation. $I[\text{school cohort} \geq 1 \text{ Jan } 1986]_i$ is an indicator function that equals to one if individual *i* was born on or after 1 January 1986 and zero otherwise. These school cohorts enter lower secondary schools under the new system. Supporting evidence for the validity of this cutoff is provided in section 4.2. $f(\text{year-month of birth}_i)$ is a polynomial function of the year and month

of birth combination of individuals, which is the running variable. It is used to isolate the trend of outcomes. α_m is the fixed effect of calendar month of birth that captures the unobserved and similar characteristics of individuals born in the same calendar month. \mathbf{X}_i is a vector of individual covariates that are invariant with time. It includes gender, immigrant status, district of residence five years earlier, usual language spoken at home, and other approximations of individual i 's SES. Error terms are clustered at the *year-month-of-birth* level.

$$\text{Labour mkt outcomes}_i = \gamma_0 + \gamma_1 * I[\text{school cohort} \geq 1 \text{ Jan 1986}]_i + f(\text{year-month of birth}_i) + \alpha_m + \mathbf{X}_i + e_i \quad (1)$$

where $\hat{\gamma}_1$ is the RDD estimate of the effect of mother tongue education on labour market outcomes.

Date of birth has been widely used as the running variable in RDD settings when a change in educational policy for everyone begins in a new school year. For example, in recent literature, this design has been employed to study the postponement of tracking (Malamud & Pop-Eleches, 2010, 2011), extension of compulsory schools (Clark & Royer, 2013; Devereux & Hart, 2010; Grenet, 2013; Oreopoulos, 2006; Ou, 2013) and provision of universal prekindergarten (Fitzpatrick, 2010) starting in a particular school year, to name just a few. In particular, similarly to my identification strategy, Ou (2013) uses the HK census data and RDD to study the effects of introducing compulsory primary schools in HK in 1971. Because the outcomes in my study were recorded when the studied cohorts entered the labour market, any sudden change in the labour market conditions that affected all those in the same cohort equally during their entrance can bias the RDD estimate. However, individuals from the same birth cohort enter the labour market in different years, because only a small portion of students can be promoted to matriculation and university, as explained in the previous section. Section 7 in the appendix discusses more in depth why a change in labour market conditions should not explain the effect estimated by $\hat{\gamma}_1$.

Equation (2) is the reduced-form regression to study the policy impact on the SES composition of those who attained university (hereafter ‘university attainers’). It is estimated with the subsample of individuals who ever attended university. The dependent variable, SES_i , is the SES level of an individual at age 12 when he or she enters lower secondary school. SES_i is

approximated in several ways using individual i 's immigration background and his or her parental characteristics. For the latter, the SES approximation is available only for individuals living with their parents during the census. By using a dummy variable indicating whether an individual is living with parents as the dependent variable in equation (2), I find that there is no sudden change in the portion of individuals living with parents around the cutoff. This means that the estimation result of equation (2) will not be biased by a change in the likelihood of an individual living with parents brought about by the reform. These approximations will be further discussed in section 5. $\hat{\theta}_1$ is the RDD estimate of the change in the SES composition of university attainers brought about by mother tongue education.

$$\begin{aligned} \text{SES}_i = & \theta_0 + \theta_1 * I[\text{school cohort} \geq 1 \text{ Jan } 1986]_i \\ & + f(\text{year-month of birth}_i) + \alpha_m + u_i \end{aligned} \quad (2)$$

4.2 Argument for Using *year-month of birth* as the Running Variable

The treatment in this study is defined as starting lower secondary school under the new system, beginning in the school year 1998–1999. For the interpretation of the RDD estimates from the reduced-form regression to be meaningful, *year-month of birth*, which is the running variable, should be able to predict in which school year an individual enters lower secondary school. According to the compulsory school law in HK, children must start primary school the first September after they turn six years old. At the same time, students cannot start primary school before they are five years and eight months old. Since the school year starts on 1 September every year, theoretically, the cohort that started lower secondary school in 1998–1999 consisted of those who were born on or after 1 January 1986. Therefore, I set this date as the cutoff to distinguish the control and treatment groups.

Ideally, the study should use individual-level data on the school year in which each individual entered lower secondary school. However, this information is not available in the census data from 2011, the main dataset. Instead, I use a supplementary dataset, the Trends in International Mathematics and Science Study 1995 (TIMSS 1995), to determine whether *month of birth* is a good running variable from which to infer whether an individual started lower secondary school under the new system. TIMSS 1995 draws a representative sample of students from the third and fourth years

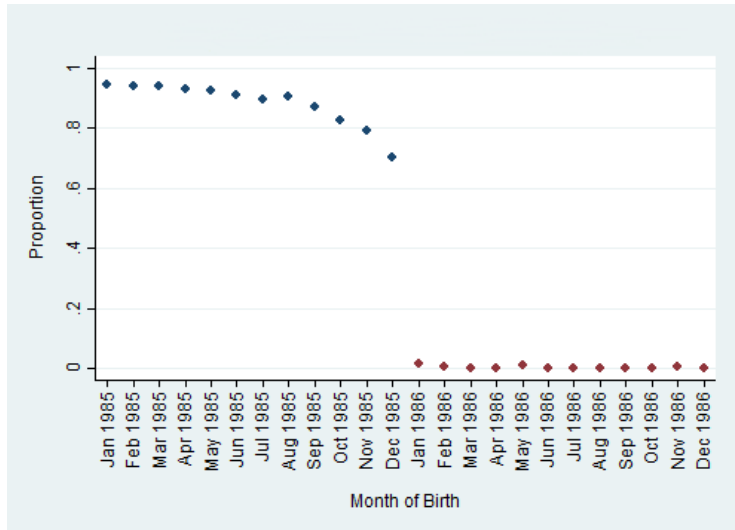
of primary schools in HK, including students born on and around the cut-off date, 1 January 1986. The dataset provides information on *year-month of birth* and grade level for each student when he or she took this test. Therefore, I can check whether 1 January 1986, the theoretical cutoff date, can empirically predict the grade levels. Although I cannot check whether individuals born on or after the cutoff date were more likely to enter lower secondary school in or after 1998–1999, I can use this dataset to check whether a subsample of the same birth cohorts I am studying attended the expected grade level according to their *year-month of birth* in 1995.

Figure 4a and 4b are plotted using the TIMSS 1995 dataset. They include a portion of the same birth cohorts of interest using the main dataset, those who were born between 1 January 1985 and 31 December 1986, as shown on the x-axes. The cutoff date of my identification strategy, 1 January 1986, is thus included in this range. Figure 4b shows the proportion of these students who were in the third year of primary school when they took the TIMSS test in 1995. Figure 4a shows the proportion who were in the fourth year of primary school at the same time. Each dot represents the proportion of students at the particular grade level among those born in each year and month cell.

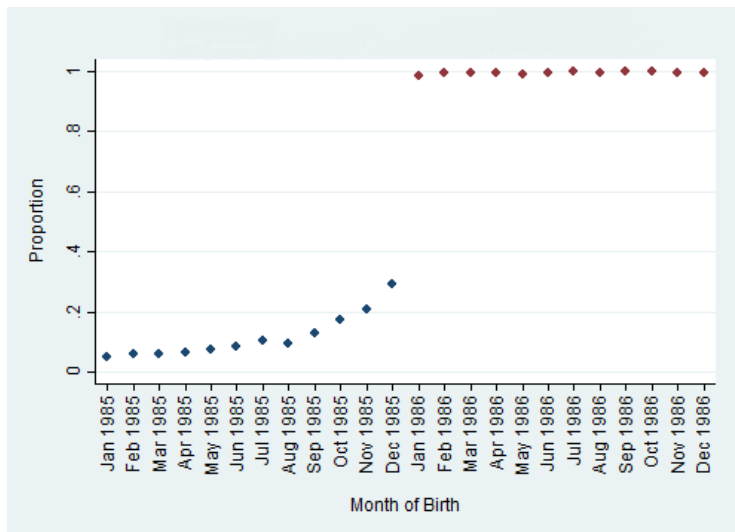
We can see that those students born between January and December in the same year tend to have been studying at the same grade level. According to the compulsory school law in HK, assuming that students did not skip or repeat grades¹⁰, those born on or before 31 August 1985 should have been in the fourth year of primary school when the TIMSS test took place. Also, those born on or after 1 January 1986 should have been in the third year of primary school. Those born between 1 September 1985 and 31 December 1985 had the flexibility to start primary school with either of the two cohorts. Consistent with this rule, we can see that over 80% of students in the first cohort were in the fourth year of primary school, while almost all of the students in the second cohort were one grade lower. Those born between these two cohorts were more variable. This pattern indicates that students complied with the compulsory school law in HK, which means that we can infer when an individual started compulsory schooling and entered lower secondary schools according to his or her year and month of birth¹¹.

¹⁰In HK, there is no regular system for students to skip grades. Proportions of students enrolled in primary and secondary schools before the standard entrance ages are very low, around 4% across school years. The grade repetition rate is also very low, around 0.4% for primary education and 3% for lower secondary education (Census & Statistics Department, 2002)

¹¹Similarly, I use the PISA 2000+ data to check whether the same cohorts were in the expected grade levels based on the compulsory school law. However, because PISA



(a) Proportion of fourth graders by month of birth



(b) Proportion of third graders by month of birth

Figure 4: Transition from the third to fourth year of primary school by the cohorts of interest

Using the TIMSS 1995 dataset, I am able to estimate the first-stage regression, equation (3). It has the same specification as the reduced-form regression, equation (2), except that the outcome variable is replaced by a dummy variable indicating whether the students were in the third year of primary school when they took the TIMSS test. Table 1 shows the regression results of equation (3). The treatment on the table refers to the indicator function indicating whether a student was born on or after 1 January 1986. The regression results are consistent with the patterns seen in figure 4b to 4a. This suggests that the students born around the cutoff had been transitioning between grades in a regular way that can be inferred from their month of birth, which supports the use of *month-of-birth* for treatment assignment for the reduced-form regression.

$$\begin{aligned} \text{Primary } 3_i = & \beta_0 + \beta_1 * I[\text{school cohort} \geq 1 \text{ Jan } 1986]_i \\ & + f(\text{year-month of birth}_i) + \alpha_m + \varepsilon_i \end{aligned} \quad (3)$$

used a different sampling strategy than TIMSS, this dataset includes students born in a narrower month-of-birth window ranging from 1 October 1985 to 30 September 1986. According to the compulsory school law, those born on or after 1 January 1986 should have been in the fourth year of secondary school when the PISA test took place, while those born between 1 October 1985 and 31 December 1985 could have been in either the fourth or fifth year of secondary school. We can see a pattern consistent with this in figures A.1 and A.2. The PISA dataset is not used for further analysis of the first stage and robustness of the identification assumption because calendar month fixed effect cannot be included, as the PISA sample only includes students born within a 12-month period.

Table 1: First-Stage Results:
Using the *Month-of-Birth* Cutoff to Predict Grade Level in Primary
School

Dataset	TIMSS 1995
Dependent Variables	Attending Primary 3 in 1995
Born after the cutoff	0.762*** (0.124)
Calendar month fixed effect	Y
Observations	6,905
<i>R</i> -squared	0.782

Note: This table shows the estimation results of the first-stage regression, equation (3) using the TIMSS 1995 dataset. Clustered standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

5 Data

5.1 Census Data in 2011

This study uses a sample of the 2011 census data of HK that covers 5% of the population. The HK government conducts a census every 10 years, and a by-census between censuses. In each census, a portion of individuals are interviewed with long forms. More detailed household and personal information are obtained from these interviewees. In 2011, 10% of the population was interviewed with long forms. This dataset includes half of these observations. It is a cross-sectional dataset of around 350,000 individuals, before appropriate sample limitation. There are around 300-400 observations per each month-of-birth cell. The data were collected by the government in June to August 2011. This means that individuals born on the cutoff date, 1 January 1986, were 25 years old when they were interviewed by census officers. This dataset includes each individual's month and year of birth and detailed educational and demographic information.

The census dataset indicates individuals living in the same household and their relationship with each other. For example, if an individual lives with his or her parent, I can also observe the parent as another individual in the dataset. The same is true for an individual's spouse. However, for an individual living with a parent, the dataset indicates only one parent, which will be the mother except for individuals living with a single father. Therefore, if an individual is living with both parents, the father is iden-

tified as the spouse of the mother. Individuals with missing data in year or month of birth are dropped from the sample, as it is necessary to construct the running variable. Immigrants who had not moved to HK in the policy year are also excluded from the sample. I use observations within a 48-month window of the running variables – that is, individuals born 24 months before and after 1 January 1986¹².

For equation (1) on labour market outcomes, I look at an individual’s educational attainment, employment status and type of occupation. An individual’s educational attainment is measured by a dummy variable indicating whether he or she has ever attained university/above. This refers to two dummy outcomes: whether an individual is a manual worker, and whether an individual has a high-rank occupation. For equation (2) on SES composition of university attainers, I proxy SES of individuals at 12 years old in four ways: (1) whether the individual is an immigrant from mainland China; (2) whether the father’s highest educational attainment is lower secondary level or below; (3) whether the father is a manual worker; and (4) whether the father is unemployed.

All these are contemporary variables in 2011 because there is no information on parental characteristics in the past. Malamud and Pop-Eleches (2011) use similar information from the census data of Romania to proxy whether an individual was disadvantaged in his or her early teenage years. Among these variables, an individual’s immigrant status and the father’s education and occupation are more invariant than the father’s employment status. Therefore, they are better proxies of the SES of individuals in the past. There are no missing data for educational levels, so for proxy (2)

¹²The optimal bandwidth calculation by Calonico, Cattaneo, and Titiunik (2014) ranges from 11 to 25 for different outcomes. For simplicity, in the regression results tables, I show the estimates using a bandwidth of 24 but also show how the estimates change when I vary the bandwidth. Also, individuals born from January to December form a school cohort and are expected to attend the same grade in the same year. Therefore, it is meaningful to include observations with the cohort level instead of the month level. RDD estimates are in general sensitive to the bandwidth choices. There is no agreed-upon method for this calculation. However, several methods are commonly used by RDD studies. Most literature calculates bandwidths with the cross-validation method (adopted in Ludwig and Miller (2007)) and asymptotic mean squared error (AMSE) minimizing method (developed in Imbens and Kalyanaraman (2012)). More recent literature, such as Card, Johnston, Leung, Mas, and Pei (2015), Carpenter, Dobkin, and Warman (2016) and Cook and Kang (2016), also uses a modified version of the AMSE minimizing method, which was developed by Calonico et al. (2014). Calonico et al. (2014) correct for the problem in Imbens and Kalyanaraman (2012) that the asymptotic variance of the RDD estimator does not converge to zero. I use the Stata program, *rdbwselect* (version 2016), to calculate the optimal bandwidth. This program was developed by the same authors who developed the method.

above, missing data refers to those who cannot be linked to their fathers. For proxies (3) and (4), missing data refers to those who cannot be linked to their fathers and also those whose fathers are not working (3) or whose fathers are not in the labour force.

5.2 Descriptive Statistics

The descriptive statistics of the finalized samples, after appropriate sample exclusion as described above, are in table 2, which shows the characteristics of individuals born within the 48-month window of the cutoff date. Table 2 shows that 16,050 individuals were born within the window. The standard age to graduate from university is 22 years old if an individual does not skip or repeat grades. The individuals in our sample are between 23 and 27 years old. Only 9% of them are full-time students, and 8% are married. Thus, they are less likely to live in a university dormitory (which is counted as a household in the census) or to have moved out of their parents' households to live with their partners.

Table 2 shows the characteristics of individuals who can be linked to their parents. They have characteristics similar to the full-sample of individuals shown in table 2. Looking at the highest educational attainment of parents in table 2, we can see that around 69% of mothers and 66% of fathers attained lower secondary level or below. I define this as having a low level of education when I proxy the SES level. Only 49% of mothers are in the labour market. This means that mothers' employment is not the major source of income for these households. Moreover, it is difficult to discern whether a nonworking mother implies low SES of a household. Therefore, the father's characteristics are better measures of the SES level of a household.

Table 2: Characteristics of Individuals within the 48-Month Window

Variable	Obs	Mean	Std. Dev.
Age	16,050	25.01	1.22
Male	16,050	0.51	0.50
Immigrant from mainland China	16,050	0.15	0.36
Married	16,050	0.08	0.28
Full-time student	16,050	0.09	0.28
Highest educational attainment			
Lower secondary/below	16,050	0.08	0.27
Higher secondary	16,050	0.37	0.48
Matriculation	16,050	0.14	0.35
University/above	16,050	0.41	0.49
Years of schooling	16,050	13.24	2.89
Working population	16,050	0.87	0.34
Employed	13,930	0.93	0.26
Unemployed	13,930	0.07	.26
Manual workers, among employed	12,929	0.31	0.46

Table 3: Characteristics of Individuals Living with Parents and of Their Parents within the 48-Month Window

	Individuals			Mothers			Fathers		
	Mean	SD	N	Mean	SD	N	Mean	SD	N
Age	24.96	1.22	13,621	53.17	4.66	13,162	57,10	6,06	11,141
Male	0.52	0.50	13,621	-	-	-	-	-	-
Immigrant from mainland China	0.16	0.36	13,621	0.45	0.50	13,162	0.48	0.50	11,141
Married	0.04	0.18	13,621	0.84	0.36	13,162	0.97	0.18	11,141
Full-time student	0.09	0.28	13,621	-	-	-	-	-	-
Highest educational attainment									
Lower secondary/below	0.06	0.25	13,621	0.69	0.46	13,162	0.66	0.47	11,141
Higher secondary	0.36	0.48	13,621	0.22	0.42	13,162	0.21	0.41	11,141
Matriculation	0.14	0.35	13,621	0.06	0.24	13,162	0.07	0.26	11,141
University/above	0.43	0.50	13,621	0.03	0.17	13,162	0.06	0.23	11,141
Years of schooling	13.37	2.82	13,621	7.71	3.65	13,162	8.15	3.84	11,141
Working population	0.89	0.32	13,621	0.49	0.50	13,162	0.72	0.45	11,141
Employed	0.93	0.26	12,059	0.64	0.96	6,440	0.96	0.20	8,008
Unemployed	0.08	0.26	12,059	0.04	0.20	6,440	0.04	0.20	8,008
Manual workers, among employed	0.30	0.46	11,152	0.70	0.46	6,180	0.70	0.46	7,674

6 Empirical Results

6.1 Effect of the Reform on Labour Market Outcomes

In this section, I show the reduced-form estimation of the effect of the reform on labour market outcomes. First, I plot the outcomes by month of birth (figure 5) and show the estimation results of equation (1) (table 4) which are complementary to the plots. For those plots, more specifically, I first estimate equation (4) below. Outcome_{*i*} is the same outcome variable as in the reduced-form regression. M_i represents the dummy variables of month of birth ranging from February to December. π_M are the coefficients for respective month of birth. After estimating equation (4) below, I obtain the summation of residuals and the estimated constant terms, which is $\hat{\pi}_1 + \hat{\mu}_i$. Then, I take the averages of these terms for individuals born in the same month of birth and plot them in the figure below. Therefore, each open circle represents the average outcome for individuals born in each *month-of-birth* cell, after controlling for calendar month fixed effects. In table 4, for each outcome, I estimate equation (1) with linear and quadratic polynomials of the running variable. The results are shown in panel A and panel B, respectively.

$$\text{Outcome}_i = \pi_1 + \sum_{M=2}^{12} \pi_M * M_i + \mu_i \quad (4)$$

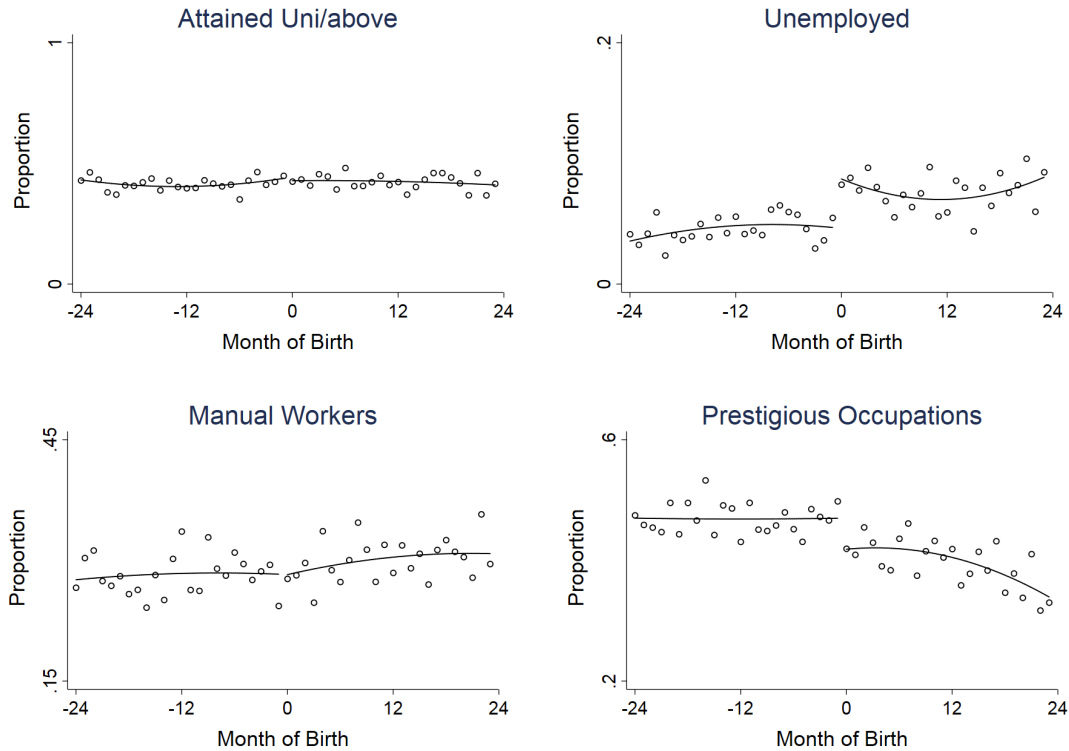
First of all, on the plot, there is no jump in the probability of attaining university/above around the cutoff. Consistent with the plot, in column (1) of table 4, the effects of the reform are statistically insignificant. This agrees with the fact that the number of university places does not increase sharply around the cutoff, which also supports my second hypothesis because the pool of university attainers does not change in size, and my study tests the change in composition of this pool. For other outcomes, we can detect a clear discontinuity around the cutoff for *unemployed* and *prestigious occupations*, which echoes the estimates presented in table 4.

For *unemployed*, the result with linear polynomial shows that the reform increases the probability of being unemployed by 3.5 percentage points, which is equivalent to an increase of the probability by as high as 48.62%. The coefficient is significant at the 5% significance level. The quadratic specification only increases the magnitude and significance level of the estimates. For *prestigious occupations*, the result with linear polynomial shows that the reform decreases the probability of having high-rank occupations

by 2.6 percentage points. This is equivalent to a decrease of the probability by 6.30%, which is economically significant. The coefficient is, however, not statistically significant. The quadratic specification increases the magnitude but not the statistical significance level of the estimates. For other outcomes, the estimates are of small magnitude and statistically insignificant.

The MOI policy in HK mandates that a large proportion of lower secondary schools must teach in Chinese, but it allows schools to switch back to English at higher secondary level under some conditions upon approval by the government. This can be translated to at least 3 and up to 5 years' decrease in exposure to education with English as the MOI. The effects are comparable to those resulting from the abolishment of teaching English as a single subject in West Bengal, India, from primary school (Chakraborty & Bakshi, 2016), which decreased the probability of having high-rank occupations by over 4%. More specifically, this reform in India decreased the time spent learning English as a subject from 10 to 5 years for the affected cohorts.

Table 5 presents the estimates with different bandwidths and also with linear and quadratic polynomials of the running variable. It can be seen that for *unemployed* (column (2)) and *prestigious occupations* (column (4)), the magnitude of the estimates remains stable across specifications, including the change of bandwidths and polynomial specifications, except that the significance levels do change. For other outcomes, the magnitudes of the coefficients change across specifications, but they remain statistically insignificant throughout all specifications. Also, it should be noted that both choices of bandwidths in the robustness check are less desirable than the bandwidth choice in the main result. First, the 12-month bandwidth specification is not able to control for calendar *month-of-birth* fixed effects. Second, while the 36-month bandwidth specifications yield smaller standard errors of coefficients, expanding the bandwidth risks the increase of possible biases, which weakens the advantage of RDD estimation. Therefore, based on the main results and supports of robustness check, the reform does increase the probability of being unemployed and decrease the chance of having high-rank occupation conditional on employment. This suggests that mother tongue education has an adverse effect on the labour market outcomes in general. Tables A.1 and A.2 show further sensitivity of the results when individuals within the six-month window of the cutoff are excluded.



Note: The solid lines are quadratic fitted values from regressions of the outcome variables on month of birth. The open circles indicate the average of the outcome variables by month of birth, after controlling for calendar month of birth fixed effects.

Figure 5: Labour market outcomes in 2011

Table 4: Effects of the Reform on Labour Market Outcomes in 2011:
Reduced-Form Results

	(1)	(2)	(3)	(4)
	Attained Uni/ Above	Unemployed	Manual Workers	Prestigious Occupations
Panel A: Linear				
	0.033 (0.023)	0.035** (0.013)	-0.006 (0.018)	-0.026 (0.024)
Panel B: Quadratic				
	-0.014 (0.028)	0.079*** (0.017)	-0.006 (0.021)	-0.043 (0.025)
Observations	7,674	6,868	6,360	6,360
Pretreatment mean	0.412	0.072	0.308	0.413

Note: Clustered standard errors in parentheses. Calendar month fixed effect and controls for gender, immigration status, district of residence 5 years earlier, and usual language spoken at home included in all regressions. The bandwidth presented here is 24 months. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Robustness Check:
Labour Market Outcomes in 2011

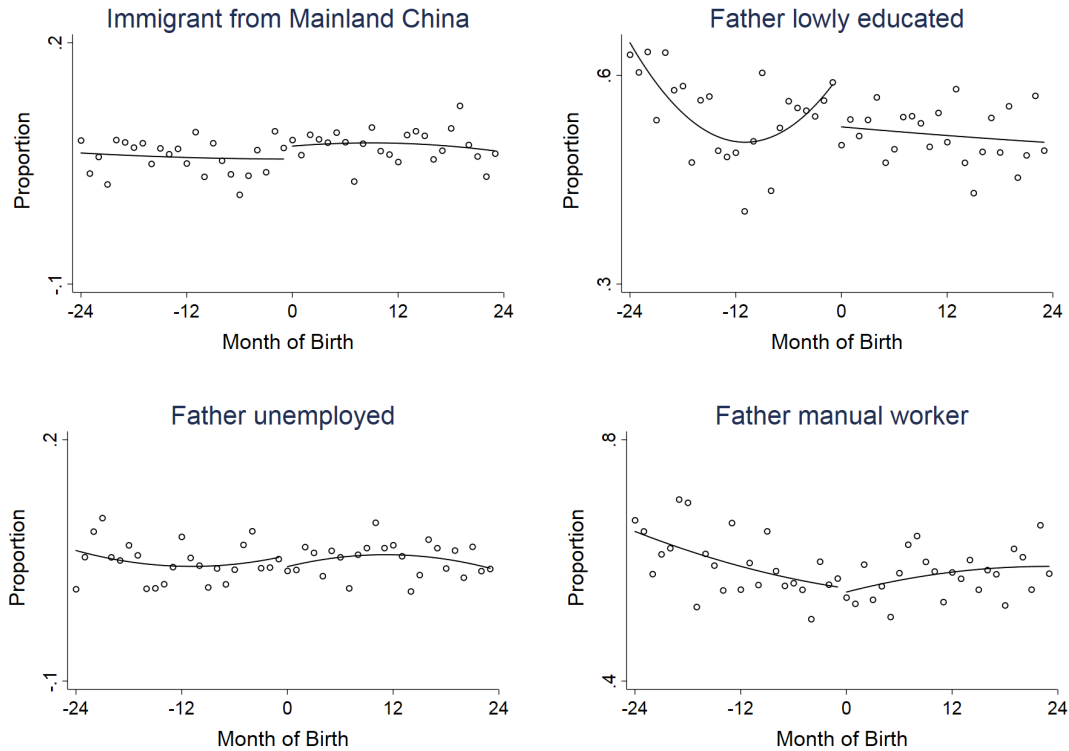
	(1)	(2)	(3)	(4)
	Attained Uni/ Above	Unemployed	Manual Workers	Prestigious Occupations
12 months				
Panel A: Linear	0.007 (0.029)	0.062** (0.018)	-0.006 (0.027)	-0.031 (0.028)
Panel B: Quadratic	-0.048 (0.027)	0.076*** (0.027)	0.013 (0.028)	-0.077* (0.028)
36 months				
Panel A: Linear	0.007 (0.018)	0.007 (0.010)	-0.008 (0.015)	-0.031 (0.019)
Panel B: Quadratic	0.023 (0.026)	0.033* (0.015)	-0.003 (0.019)	-0.021 (0.024)

Note: Clustered standard errors in parentheses. Calendar month fixed effect included in regressions with 36 months bandwidth. Controls for gender, immigration status, district of residence 5 years earlier, and usual language spoken at home included in all regressions. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

6.2 Effect of the Reform on Composition of University Students

Similarly to figure 5, figure 6 below shows regression-adjusted means of various SES proxies at age 12 for the subsample of individuals who have ever attained university or above, by their months of birth. As the estimation result of the previous subsection confirms, the reform does not alter the size of the pool of university attainers for those born very close to the cutoff. Therefore, what this subsection tests is purely whether the SES composition of this pool changes. The data are more jumpy around their means, especially for *father is lowly educated* and *father is manual worker*. This is because the samples for each SES proxy are smaller than the full sample. But still, from simply eyeballing, it is difficult to detect any sudden jump of the SES proxies around the cutoff.

The estimation results of equation (2) in table 6 agree with the conclusion from figure 6 in the sense that the estimates of the reform effects are largely statistically insignificant. However, it should be noted that the estimates are of quite large magnitudes. For example, in column (1) with linear specification, the reform leads to an increase in the proportion of immigrant students from mainland China who attained university by 21.57%. This is in contrast to my hypothesis. Even when we look at the lower bound of the confidence interval, the model cannot rule out economically significant effects of the decrease in low SES students in the pool of university attainers. For instance, the lower bound of the decrease is -3.40% for *immigrants from mainland China*, -7.36% for *father is lowly educated*, and -4.18% for *father is manual worker*. Table 7 also shows that the estimates are not robust to changes in the bandwidths and polynomial specifications. Therefore, no change in the SES composition of university attainers can be detected in this study. This means the results are inconclusive. Tables A.3 and A.4 shows further sensitivity of the results when individuals within the six-month window of the cutoff are excluded.



Note: The solid lines are quadratic fitted values from regressions of the outcome variables on month of birth. The open circles indicated the average of the outcome variables by month of birth, after controlling for calendar month of birth fixed effects.

Figure 6: Composition of university attainers

Table 6: Effect of the Reform on the Composition of University Students:
Reduced-Form Results

	(1)	(2)	(3)	(4)
	Immigrant from Mainland China	Father lowly educated	Father manual worker	Father Unemployed
Panel A: Linear	0.022 (0.013)	0.018 (0.029)	0.023 (0.024)	0.006 (0.014)
Panel B: Quadratic	0.016 (0.016)	-0.069*** (0.024)	0.002 (0.029)	-0.017 (0.017)
Observations	6,605	4,913	3,433	3,574
Pre-treatment mean	0.102	0.528	0.576	0.039

Note: Clustered standard errors in parentheses. Calendar month fixed effect included in all regressions. The bandwidth presented is 24 months. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 7: Robustness Check:
Composition of University Students

	(1)	(2)	(3)	(4)
	Immigrant from Mainland China	Father lowly educated	Father Manual Worker	Father Unemployed
12 months				
Panel A: Linear	-0.055*	-0.096***	-0.023	-0.009
	(0.021)	(0.023)	(0.028)	(0.017)
Panel B: Quadratic	-0.096**	-0.093***	-0.053	-0.013
	(0.033)	(0.026)	(0.038)	(0.020)
36 months				
Panel A: Linear	0.016	0.017	0.013	0.007
	(0.012)	(0.023)	(0.021)	(0.011)
Panel B: Quadratic	-0.005	-0.015	0.012	-0.009
	(0.015)	(0.029)	(0.032)	(0.014)

Note: Clustered standard errors in parentheses. Calendar month fixed effect included in regressions with 36 months bandwidth. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

6.3 Fulfillment of Assumption of RDD

For the above RDD estimates to credibly measure the effects of the MOI policy on the outcomes, two assumptions of the RDD model must be fulfilled¹³. First of all, students and parents should not be able to manipulate their treatment status. Even though parents may time births and there is variation in the numbers of individuals in each value of the running variable, *month-of-birth*, there should not be bunching of the distribution of the running variable around the cutoff. It is also not reasonable to believe that parents timed births with response to this policy, because the mandate was made about a year before students at the cutoff entered lower secondary school. A related point is that parents should not be able to choose the school year in which their children enter lower secondary school. As shown in the first-stage results, students were promoted through different grades

¹³This section is written using the information from Imbens and Lemieux (2008), and Lee and Lemieux (2010).

according to their month of birth. Therefore, these points should not pose a problem to my identification.

Notwithstanding these arguments supporting that this assumption is fulfilled, I have conducted two tests to empirically support my assumption. First, I conduct a density test of the running variable according to the method of Cattaneo, Jansson, and Ma (2017). The test fails to reject the null hypothesis that there is no bunching of the running variable around the cutoff. Figure 7 illustrates that there is no bunching of the smoothed distribution of the running variable using the method of Cattaneo et al. (2017). Table 8 shows the estimation results of running the reduced-form regression when I use student characteristics in grade 3 or 4 of primary school as the dependent variables. These data are from TIMSS 1995 datasets and were gathered before the reform. The results show that the treated cohorts do not have characteristics statistically significantly different from those of the control cohorts. The lower panel of table 8 shows the results of the same covariates test with the main dataset.

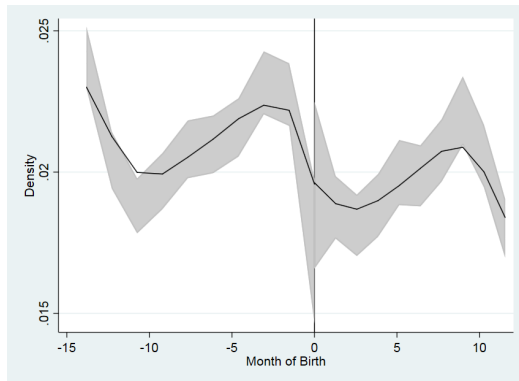


Figure 7: Density Test of Running Variable

Second, potential outcomes both without and under the new system should be continuous in *month of birth*. This implies that no other policies or events should create a jump in potential outcomes. Otherwise, the RDD estimates will capture these other effects and cannot represent the effect of this reform. In the academic year 1998–99, the government did not implement any educational policies other than the MOI policy that would possibly affect the outcomes of individuals. One possible concern is that because many lower secondary school teachers who used to teach in English now had to teach in Chinese after the implementation of the policy, this could possibly have affected teaching quality and thus the outcomes of

individuals. A related concern is about the quality of textbooks. However, the provision of Chinese textbooks should not have been a problem, as some lower secondary schools were teaching in Chinese even before the policy change. Since the early 1990s, the government has been gradually increasing places of university, but there has not been a large jump in the number at any point. This trend of university expansion can be controlled for by the polynomial functions of the running variable.

Table 8: Covariates Test

Covariates	(1) Female	(2) Immigrant	(3) Home Possess Computer, Desk & Dictionary	(4) Home Possess 100 Books or More
Treat	0.0582 (0.244)	-0.143 (0.157)	-0.0873 (0.236)	0.0282 (0.210)
Calendar month fe	Y	Y	Y	Y
Date source	TIMSS	TIMSS	TIMSS	TIMSS
Observations	6,905	6,905	6,905	6,905

Note: Clustered standard errors in parentheses

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

Covariates	(5) Female	(6) Immigrant from Mainland China	(7) Father can speak English	(8) Father lowly educated	(9) Father is manual worker
Treat	0.006 (0.018)	0.007 (0.016)	0.033 (0.022)	-0.040 (0.025)	-0.020 (0.034)
Calendar month fe	Y	Y	Y	Y	Y
Date source	Census	Census	Census	Census	Census
Observations	16,050	16,050	11,141	11,141	7,674

6.4 Discussion of Policy Effects

As discussed earlier, the MOI policy in HK decreased the amount of exposure to education in English by at least 3 years for the affected students. This reflects a substantial loss of exposure to English, because all subjects except the single subject of English are now taught in Chinese. The affected students do not experience hearing English spoken as the lecture language, reading English teaching materials, or answering exam questions in English, which has impacts on all aspects of English ability, including listening, reading, speaking and writing. Because students were grouped into bands for school choices even before the reform, it is reasonable to believe that the reform did not alter the peer composition within the same secondary schools, and the only change was that of the MOI. Moreover, it should be noted that almost all primary schools in HK already used Chinese as the MOI, so lower secondary school was the start of intensive English training for all HK children before the reform.

As children enter lower secondary schools at the age of 12, this timing coincides with the empirical evidence provided by Bleakley and Chin (2010) that immigrant children arriving in the United States after the age of 11 often have a more difficult time integrating as a result of lower English proficiency. Since Chinese is widely used as the language of communication at home and in HK society, students may not have any other way to obtain intensive training in English without the exposure in lower secondary. Therefore, it is not surprising to find that this reform has increased the unemployment rate for the treated cohort to a large extent, even if the quantity of education was not affected (refer to column (1) in table 4 for the reduced-form estimates on the probability of attaining higher education). This highlights the important role of acquiring language skills at this critical age.

Regarding the second hypothesis, one possible explanation of the insignificant results could be that only the best students can enter university in HK. The government provides subsidies for only a limited number of students to enter university, which is around 18% of each birth cohort every year (see the discussion about this in section 3). Therefore, under this elite education system, most students who were able to enter university both before and after the reform were likely from the limited numbers of English schools that were not affected by the reform at all. This echoes the detracking policy in Romania, where Malamud and Pop-Eleches (2011) find that even through the treated cohorts perform better in the labour market as the proportion of students continuing their academic education increases,

low SES students do not have a higher chance of entering university, as the environment in university is limited. This can reconcile the results of the two hypotheses.

7 Conclusion

This paper examines the effects of a policy reform in Hong Kong (HK) mandating that more than half of all secondary schools must change their MOI from English to Chinese. First, since English is the dominant language in the labour market in HK, as in many other countries in the world, this leads to the hypothesis that the treated cohorts are less competitive in the labour market, as these students have received less training in English. Second, as a student's English level is a vital determinant for university admission, graduates from English schools have an edge over graduates from Chinese schools in competing for a limited places in university. Because the small number of remaining English schools are unequally distributed in HK, with most being located in high SES school districts, this gives rise to the question whether the reform hinders students from low SES families from entering university, which is my second hypothesis.

My main findings show that the reform does have a statistically and economically significant negative impact on the labour market outcomes for the treated cohorts. However, my empirical results regarding the second hypothesis are inconclusive due to the quality of the data. While adding to the economics literature on language skill premiums in developed countries, this study also exemplifies the use of econometric models to improve policy evaluation. Last but not least, future economics research in the field of education should account for the different aspects of quality of education apart from studying the returns to quantity of education, i.e. years of schooling. Regarding the MOI as an aspect of the quality of education that students are exposed to, it is invaluable to obtain information on the MOI used to teach student and the students' subsequent language ability so that researchers can observe how the MOI affects labour market outcomes mediated through the change in their language ability¹⁴.

¹⁴The study by Azam et al. (2013) differs from other studies in India. Unfortunately, their study is not able to estimate the causal effect of language ability on labour market outcomes.

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Appendix

Use of PISA Datasets for the First-Stage

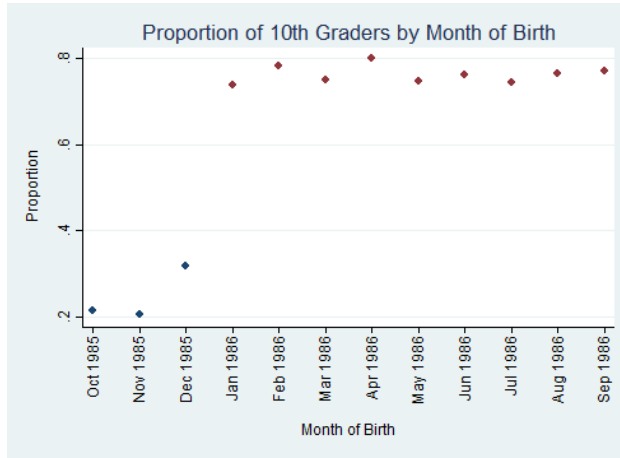


Figure A.1: Transition from the fourth to fifth year of secondary schools of our cohort of interest

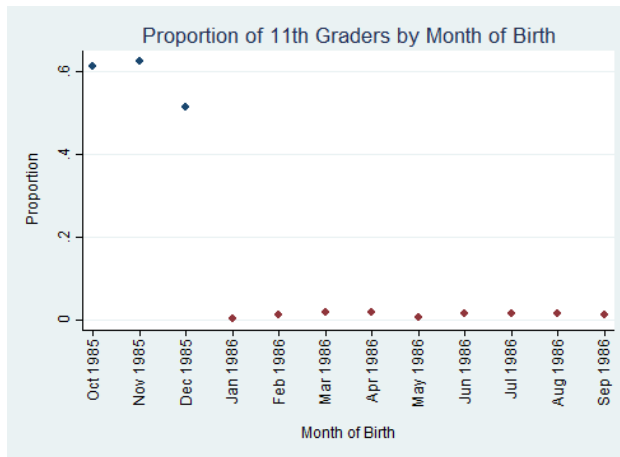


Figure A.2: Transition from the fourth to fifth year of secondary schools of our cohort of interest

Different Times of Labour Market Entrance for Before and After Cohorts

Differences in labour market conditions when leaving school for cohorts before and after the policy reform can give rise to bias of the estimated policy effects. This subsection provides evidence that there was no big jump in the unemployment rate experienced by the after cohorts when they left school. As mentioned earlier, after higher secondary school, only around 30% of students can be promoted to matriculation level (Hong Kong Examinations and Assessment Authority, 2007, 2009; University Grants Committee, 2017). Afterward, only around 40% of students can be promoted to university. Duration of most university programmes is three years. Therefore, the expected time of labour market entrance for students even in the same cohorts is different.

Figures A.3, A.4 and A.5 show the unemployment rate (Census & Statistics Department, 2019) for corresponding age groups when individuals with a certain educational attainment are expected to enter the labour market. The shaded area in figure A.3 indicate months when individuals from the first treated cohort and the previous cohort are expected to enter the labour market if they started to work after higher secondary school, which is after the first public examination. The red line shows the unemployment rate for those 15 to 19 years old. Individuals are 17 years old when they finished higher secondary school without skipping grade or repeating a grade. Around 70% of individuals cannot continue to matriculation and above. It can be seen that there is no big jump in the unemployment rate between the times when large proportions of the before and after cohorts are expected to leave school.

Figure A.4 also shows that the before and after cohorts experienced similar labour market conditions for those enter the labour market after matriculation when they are 19 years old. However, for those finishing university and leaving school at the age of 22, figure A.5 shows that the unemployment rate for the age group 20 to 29 started to climb. Therefore, the after cohort indeed experienced a tougher labour market conditions. However, it should be noted that only around 18% of the cohorts attained up to university, and most of the before and after cohorts entered the labour market before this point in time. As a result, the bias driven by this group of university attainers should not be the main factor in the estimated policy effects.

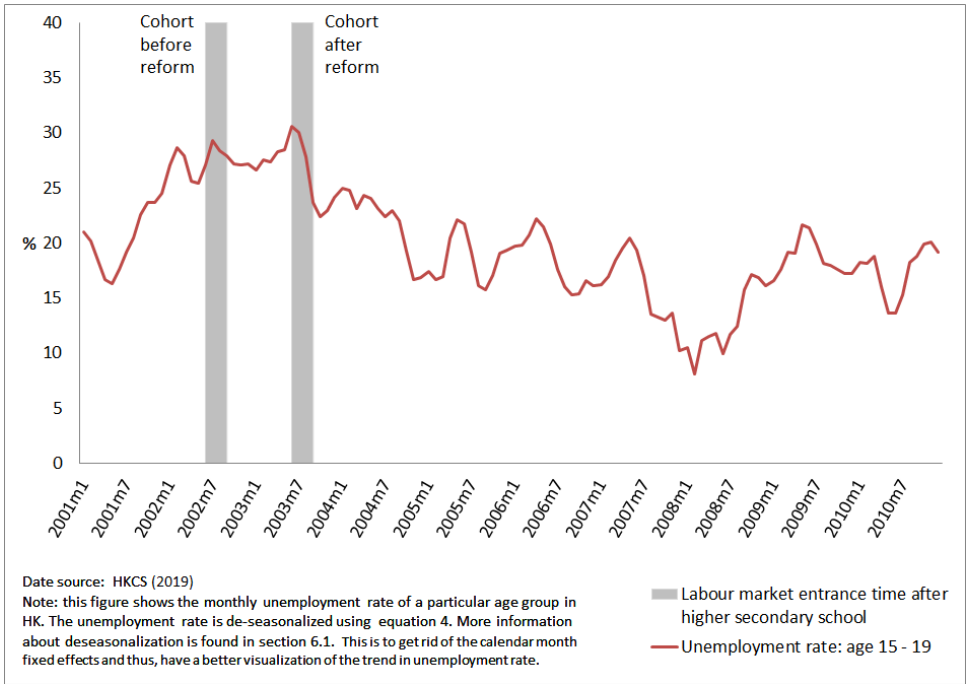


Figure A.3: Unemployment rate for those entering the labour market after higher secondary school

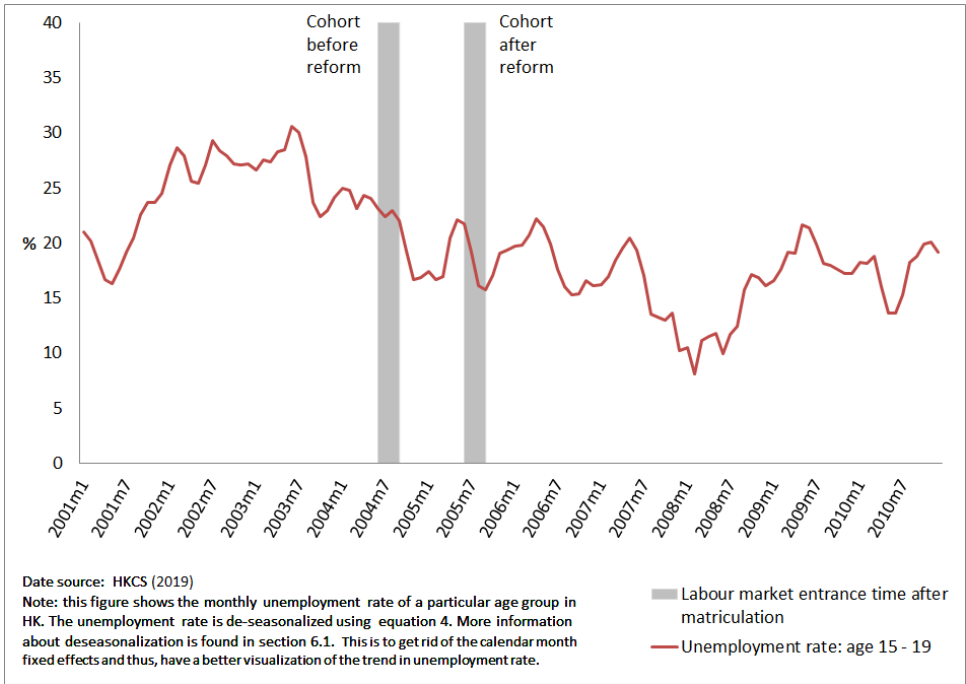


Figure A.4: Unemployment rate for those entering the labour market after matriculation

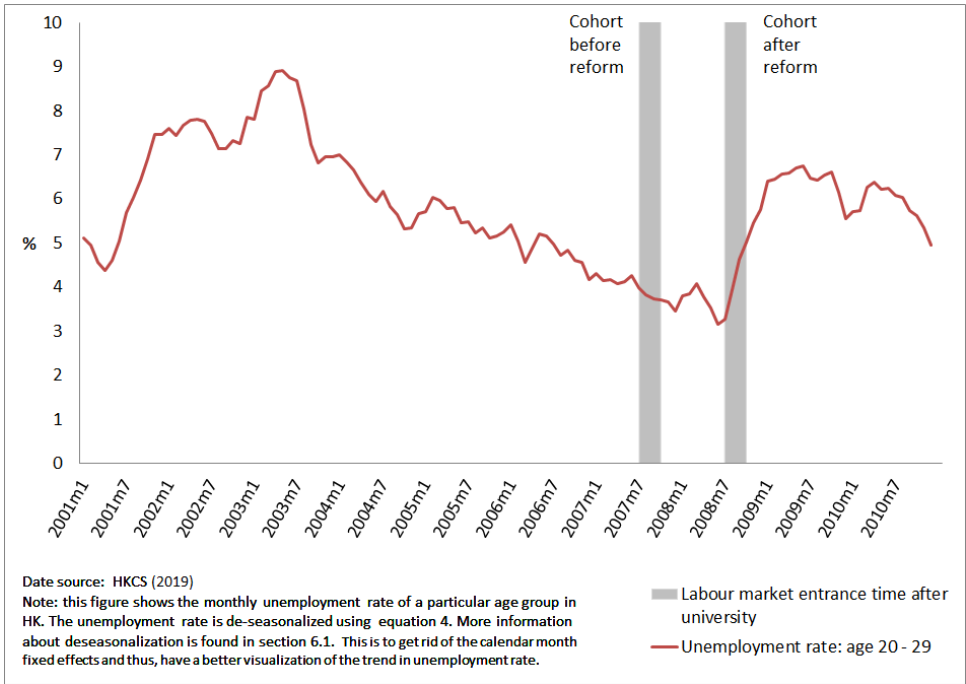


Figure A.5: Unemployment rate for those entering the labour market after university

Donut Sample

Table A.1: Effect of the Reform on Labour Market Outcomes in 2011:
Reduced-Form Results

	(1) Attained Uni/ Above	(2) Unemployed	(3) Manual Workers	(4) Prestigious Occupations
Panel A: Linear	0.054 (0.032)	0.004 (0.013)	-0.004 (0.025)	-0.020 (0.036)
Panel B: Quadratic	-0.015 (0.068)	0.066** (0.022)	-0.004 (0.040)	-0.044 (0.074)
Observations	6,650	5,950	5,500	5,500
Pretreatment mean	0.410	0.072	0.310	0.409

Note: Clustered standard errors in parentheses. Calendar month fixed effect and controls for gender, immigration status, district of residence 5 years earlier, and usual language spoken at home included in all regressions. The bandwidth presented here is 24 months with the individuals with 3 months close to the cut-off not included. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.2: Robustness Check:
Labour Market Outcomes in 2011

	(1)	(2)	(3)	(4)
	Attained Uni/ Above	Unemployed	Manual Workers	Prestigious Occupations
12 months				
Panel A: Linear	0.027 (0.074)	0.048 (0.028)	0.007 (0.034)	0.027 (0.065)
Panel B: Quadratic	-0.179 (0.138)	0.095 (0.088)	-0.038 (0.144)	0.076 (0.205)
36 months				
Panel A: Linear	0.013 (0.024)	-0.011 (0.011)	-0.009 (0.019)	-0.030 (0.025)
Panel B: Quadratic	0.068 (0.054)	0.005 (0.019)	0.000 (0.035)	-0.004 (0.051)

Note: Clustered standard errors in parentheses. Calendar month fixed effect included in regressions with 36 months bandwidth. Controls for gender, immigration status, district of residence 5 years earlier, and usual language spoken at home included in all regressions. Individuals born 3 months close to the cut-off are not included. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.3: Effect of the Reform on the Composition of University Students:
Reduced-Form Results

	(1)	(2)	(3)	(4)
	Immigrant from Mainland China	Father lowly educated	Father manual worker	Father Unemployed
Panel A: Linear	0.032* (0.016)	0.059 (0.037)	0.045 (0.031)	0.006 (0.019)
Panel B: Quadratic	0.049 (0.031)	-0.086 (0.056)	0.054 (0.059)	-0.059 (0.037)
Observations	5,713	4,233	2,974	3,099
Pre-treatment mean	0.100	0.524	0.580	0.040

Note: Clustered standard errors in parentheses. Calendar month fixed effect included in all regressions. The bandwidth presented here is 24 months with the individuals with 3 months close to the cut-off not included. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.4: Robustness Check:
Composition of University Students

	(1)	(2)	(3)	(4)
	Immigrant from Mainland China	Father lowly educated	Father Manual Worker	Father Unemployed
12 months				
Panel A: Linear	-0.010 (0.026)	-0.106 (0.060)	0.039 (0.046)	-0.009 (0.017)
Panel B: Quadratic	-0.032 (0.073)	-0.047 (0.158)	0.144* (0.068)	-0.127* (0.053)
36 months				
Panel A: Linear	0.029* (0.014)	0.044 (0.027)	0.025 (0.027)	0.009 (0.013)
Panel B: Quadratic	0.017 (0.021)	0.046 (0.049)	0.057 (0.047)	-0.017 (0.024)

Note: Clustered standard errors in parentheses. Calendar month fixed effect included in regressions with 36 months bandwidth. Individuals born 3 months close to the cut-off are not included. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

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