IFAU – INSTITUTE FOR LABOUR MARKET POLICY EVALUATION

Essays in labor and demographic economics

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Abstract

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This thesis consists of four self-contained essays.

Essay 1: (with Olof Åslund) We study the impact of family size on intermediate and long-term outcomes using twin births as an exogenous source of variation in family size in an unusually rich dataset. Similar to recent studies, we find no evidence of a causal effect on long-term outcomes and show that not taking selection effects into account will likely overstate the effects. We do, however, find a small but significant negative impact of family size on grades in compulsory and secondary school among children who are likely to be vulnerable to further restrictions on parental investments.

Essay 2: This essay investigates the consequences of a series of Swedish policy changes beginning in 1989 where different regions started subsidizing the birth control pill. The reforms were significant and applied to all types of oral contraceptives. My identification strategy takes advantage of the fact that the reforms were implemented successively over time and targeted specific cohorts of young women, in particular teenagers. This generates plausibly exogenous variation in access to the subsidy. I first demonstrate that access significantly increased pill use. Using regional, temporal, and cohort variation in access, I then go on to examine the impact on abortions. The estimates show that the subsidy significantly decreased the abortion rate by about 8 percent. Furthermore, the results indicate that long-term access decreases the likelihood of teenage childbearing by about 20 percent. However, there is no significant effect on labor supply, marriage, educational attainment or welfare take-up.

Essay 3: (with Olof Åslund, Per-Anders Edin and Peter Fredriksson) We study peer effects in compulsory school performance among immigrant youth in Sweden. The empirical analysis exploits a governmental refugee placement policy that provides exogenous variation in the initial place of residence in Sweden; and it is based on tightly defined neighborhoods. There is tentative evidence that the share of immigrants in the neighborhood has a negative effect on GPA. But the main result is that, for a given share of immigrants in a neighborhood, the presence of highly educated peers of the same ethnicity has a positive effect on school grades. The results suggest that a standard deviation increase in the fraction of highly educated adults in the assigned neighborhood increases the compulsory school GPA by 0.9 percentile ranks. This magnitude corresponds roughly to a tenth of the gap in student performance between refugee immigrant and native born children.

Essay 4: This essay investigates the consequences of residential segregation for immigrants' health. To this end, I make use of a rich dataset covering the entire Swedish working-age population from 1987 to 2004. The dataset contains annual information on the exact diagnosis for all individuals admitted to Swedish hospitals, as well as a wide range of individual background characteristics. This allows me to investigate some of the mechanisms through which segregation could affect health, e.g. income and stress. It is however difficult to identify the causal link between segregation and health since individuals might sort across residential areas based on unobserved characteristics related to health. To deal with this methodological problem I exploit a governmental refugee placement policy which provides plausibly exogenous variation in segregation. The OLS estimates show a statistically significant positive correlation between segregation and the probability of hospitalization. Estimates that account for omitted variables are however in general statistically insignificant.

In memory of my grandfather

Stig Ström

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I believe that the Department of Economics at Uppsala University together with the Institute for Labour Market Policy Evaluation (IFAU) offers a great research environment. I would like to express my gratitude to some people at these places to whom I am particularly grateful. First, I would like to mention Per Johansson. I have always considered Per as my "unofficial" advisor. Per has helped me in several ways throughout my studies and always been generous with his time. My interest in applied microeconometrics can to a large extent be attributed to him. I would also like to thank Peter Fredriksson who, together with P-A and Olof, is a co-author of one of the papers in this thesis. Cooperating with Peter, Olof and P-A has indeed been very valuable and taught me a lot of the art and practice of doing research. Matz Dahlberg and Bertil Holmlund have also helped me in various ways during my years at the department.

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Not only do the Department and IFAU host many skilled economists but also offer a nice and friendly atmosphere. Much of this can be attributed to the wonderful staff, always providing swift and excellent assistance in a kind and patient way. Of the people at these places, I would in particular like to thank Katarina Grönvall, Ann-Sofie Wettergren Djerf, Åke Qvarfort, and Monica Ekström.

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Last but not least, I would like to thank my family for always supporting and believing in me.

A wonderful and sunny winter day in Uppsala, December 2008

Hans Grönqvist

[1] Other likely suspects are Captain Kirk and Hammarby IF.

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Introduction

This thesis consists of four self-contained essays, broadly belonging to the field of labor and demographic economics. Their main common denominator is the focus on analyzing various social policies and problems with respect to its consequences for labor markets, human capital formation, and health.

Ever since the seminal work of Gary Becker (e.g. Becker 1976), economists have been applying the economic toolbox to explore a wide range of social issues including crime, discrimination, racial and gender differences, inequality, family structure, social interactions, and intergenerational mobility. In many cases, these questions had already been studied by other social sciences. The economic approach outlined by Becker and others however turned out to provide a useful framework for analyzing social issues and thereby contributing to the overall knowledge.[1] My thesis can be seen as building on this work.

The essays in this thesis are empirical and study questions related to fertility and residential segregation with a special focus on the relationship between early life experiences and child/youth outcomes. A large number of studies have highlighted that childhood experiences may have long lasting impacts, and that these effects often are stronger among disadvantaged children (e.g. Cunha and Heckman 2007; Currie 2001; Haveman and Wolfe 1995). The thesis also contributes to this literature.

A central theme in the thesis is distinguishing between causation and correlation. Determining cause and effect is one of the oldest questions in the social sciences, where data generated by controlled randomized experiments are rare. There are basically two dimensions to this problem. First, the relationship between two variables could be driven by some other unobserved variable. Second, the variables could directly influence each other. In both cases, it will be difficult to claim that one variable causally affects the other.

To illustrate these problems, consider the question of estimating the effect of unemployment on crime (e.g. Freeman 1999). An observed positive correlation between unemployment and crime could either be due to a causal effect, i.e. that unemployment causes crime, or be spuriously

[1] For a discussion on economists' contribution to the literature, see Lazear (2000).

driven by omitted variables and/or simultaneity. As an example, both variables could be correlated with local school quality, meaning that if school quality is not accounted for in the analysis, the researcher may erroneously attribute a rise in crime to an increase in unemployment. Alternatively, companies might choose to move away from areas with high crime rates, causing the unemployment level to rise. From a policy perspective, understanding causality is imperative in making correct policy decisions. For instance, if the relationship between unemployment and crime is actually driven by school quality, a crime preventive policy focusing on reducing the unemployment rate might not be very efficient.

To deal with these methodological problems, I make use of various quasi-experiments, generating natural treatment and control groups similar in all characteristics (except for the treatment received). These "experiments" are typically in the form of major policy changes. Since such policies often are "exogenously" imposed on the individuals, omitted variables and simultaneity become less of a concern.

Another major obstacle when analyzing these kinds of questions is the limited availability of high quality data. An additional contribution of this thesis is to exploit Sweden's extensive population micro data. Very few datasets contain information linking individuals' records to family characteristics from early childhood to adulthood. The fact that I have access to precisely such rich data is advantageous since it minimizes problems with small and unrepresentative samples and implies less scope for measurement error.

Family size and child outcomes

The first essay (co-written with Olof Åslund) deals with the relationship between family size and children's outcomes. Economists' interest in the topic stems from theoretical work proposing a "quantity-quality trade-off" in parental decisions on family size (e.g. Becker and Lewis 1973). In order to increase the quantity of children, these theories suggest, parents are forced to decrease the investments in their children, given the family budget constraint, which leads to lower "quality" of the offspring (e.g. less education or worse labor market outcomes). A vast body of empirical work supports the theoretical view that large families keep living standards low. In fact, these findings together with the theoretical predictions have been used as arguments for introducing policies aimed at restraining family size in several developing countries.

In order to properly analyze this question it is however necessary to take into account potential omitted variables. Parents make decisions on the number of children to have based on many factors which often are unobserved and possibly affect their children's well-being. To examine this question we therefore exploit the incidence of twin births. Because twin births essentially are randomly determined they are unrelated to omitted variables. Similar to recent studies, we find no evidence of a causal effect on long-term outcomes and show that not taking omitted variables into account will likely overstate the effects. We do, however, find a small but significant negative impact of family size on grades in compulsory and secondary school among children who are likely to be vulnerable to further restrictions on parental investments.

The consequences of subsidized contraception

Also the second essay deals with fertility and youth outcomes. Unintended childbearing is both frequent and widespread. For instance, in the U.S. almost 60 percent of all pregnancies are unplanned; a rate that is even higher among young women. The social and economic costs of unintended childbearing are potentially large since these births are associated with poor socioeconomic and health outcomes of both mothers and children. In addition, unwanted pregnancies account for approximately 1.5 million abortions annually in the U.S. alone (Institute of Medicine 1995). These concerns have motivated policy makers to instigate a wide range of family planning programs. Despite the vast interest in such interventions there is very scarce evidence on the efficiency of different policies. The reason for this is that most policies have been introduced simultaneously for all women. This makes it difficult to find proper comparison groups to the women affected by the policy, which would make it possible to answer the counterfactual question: what would have happened to these women had the policy *not* been introduced.

In the essay I explore a series of Swedish policy changes where different regions beginning in 1989 started subsidizing the birth control pill for teenagers. The reforms were significant and applied to all types of oral contraceptives. The main argument for subsidizing the birth control pill for teenagers is that young women in particular may lack stable income sources, and therefore are more likely to prematurely end or delay the course of the treatment. I examine whether access to the subsidy affected teenagers long-term outcomes in terms of abortions, fertility, labor supply and educational attainment.

There are many arguments for why easier access to oral contraceptives could matter for these outcomes. If women substitute between the ”pill” and other not as effective contraceptive methods in order to avoid unwanted births, a subsidy that changes the relative price between these technologies can potentially affect the abortion rate. Socioeconomic outcomes could be affected through, e.g., delayed childbearing, smaller families, reduced risk of shot-gun marriage, or increased returns to education and work.

The main concern in the analysis is that the introduction of the subsidy could be correlated to unobserved variables related to women's outcomes. To address this issue I use a special feature of the reforms: that the subsidy was implemented successively over time and targeted specific cohorts of young women. This makes it possible to control for permanent unobserved regional and cohort characteristics, as well as common temporal shocks. The results show that access to the subsidy significantly decreased abortions and reduced the likelihood of teenage childbearing. I find no significant effect on socioeconomic outcomes.

Segregation and minorities' outcomes

The last two essays study issues related to segregation. Racial and ethnic disparities in socioeconomic and health outcomes are large and well documented. For instance, in Sweden the difference in the compulsory school grade point average between immigrant and native students is roughly of the same size as the gap between boys and girls. Moreover, the incidence of heart disease is in many immigrant groups up to 50 percent higher than that of natives. The fact that some of these differences remain even after adjusting for individual background characteristics has motivated social scientists to look for possible explanations. To date, a large body of research has demonstrated that residential segregation adversely affects the social and economic well-being of the segregated minority group (e.g. Coleman 1966 or Wilson 1987). The purpose of the third essay (co-written with Olof Åslund, Per-Anders Edin and Peter Fredriksson) is to examine the role of ethnic concentration among immigrant youth in compulsory school performance, while the fourth essay focuses on the relationship between segregation and immigrants' health.

Identifying the causal link between segregation and individuals' outcomes is difficult since residential location is a choice variable. If individuals sort across residential areas based on unobserved characteristics related to the outcome of interest the estimates will be biased. Most previous studies attempt to deal with this issue by controlling for potential confounders but it is far from certain whether this approach really controls for all variables that could matter. This problem is addressed using a Swedish refugee placement policy where authorities between the years

1987–1991 assigned newly arrived refugees to their initial location of residence. The policy was implemented in a way that makes initial location independent of unobserved individual characteristics. In this sense, the policy can be thought of as representing an experiment where initial level of segregation is randomized to individuals thereby accounting for omitted factors (after controlling for observed characteristics).

The results suggest that a standard deviation increase in the fraction of highly educated peers in the assigned neighborhood increases compulsory school GPA by 0.9 percentile ranks; a corresponding increase in the size of the ethnic community in the assigned neighborhood has about the same effect, but is less precisely estimated. Peer influences are larger among those who arrived before age seven than for those who arrive at an older age.

In the last essay, the OLS estimates show statistically significant evidence of an adverse correlation between segregation at the parish level and the risk of being hospitalized. For instance, a one standard deviation increase in segregation is associated with a rise in the likelihood of an immigrant being admitted to hospital by about 6 percent. Similar results are documented for different subgroups of the population. In contrast to most previous studies, estimates that account for omitted variables are however in general not statistically significant.

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Essay 1: Family size and child outcomes: Is there really no trade-off?*

Co-authored with Olof Åslund

Introduction

Social scientists have for a long time been interested in how early experiences determine children's long-term welfare (e.g. Haveman and Wolfe 1995). One example is the relationship between family size and the outcomes of children, where theory proposes a "quantity-quality trade-off": when increasing the quantity of children parents are forced to decrease their investments per child (e.g. Becker and Lewis 1973; Willis 1973; Becker and Tomes 1976).[1] The seemingly robust empirical finding that increased family size adversely affects children's outcomes (e.g. Björklund et al 2004, Hanushek 1992, or Holmlund 1988) has however recently been questioned by studies arguing that more complex empirical strategies are needed to identify causal effects of family size.

* We are grateful to Peter Fredriksson, Magnus Gustavsson, Rafael Lalive, Eva Mörk, Peter Nilsson, Oskar Nordström-Skans, and Kjell Salvanes for valuable comments and discussions, and to Björn Öckert for sharing his data. We thank Louise Kennerberg for preparing the data. This essay has benefited from comments by audiences at the 2007 Annual Meetings of the European Economic Association (Budapest), the 2007 Nordic Summer Institute in Labor Economics (Helsinki), Uppsala University/IFAU, Stockholm University (SOFI) and Växjö University (CAFO).

[1] The original model considers parental investments in their children as being subject to financial constraints. The model has later been extended to take into account time constraints (Lundholm and Ohlsson 2002). Models of spillover effects have also been used to explain the observed negative relationship between family size and children's attainments (e.g. Zajonc 1976). In short, these models suggest that adding siblings decreases the average human capital level within the family because young children do not have the same intellectual level as older family members. The hypothesis is that this will hurt the outcomes of children from large families.

We follow the approach by Black et al (2005) who used twin births as an exogenous source of variation in family size and found no effect of family size on the amount of education completed. In addition to replicating their findings, we analyze a broader set of outcomes ranging from childhood to adulthood using high quality data on entire Swedish birth cohorts. Intermediate outcomes (such as grades) are interesting as indicators on performance and well-being during adolescence. They also provide a supplementary test of the quantity-quality trade-off hypothesis.

Needless to say, the potential trade-off differs depending on economic circumstances. In developing countries with fertility rates of about six births per woman, malnutrition may be a consequence of sibship size, which could affect long-term economic outcomes. In industrialized countries with fertility rates between one and two, nutrition is in most cases not the issue. Still, parents in richer countries act under a budget constraint (at least in terms of hours available), which may decrease the resources available for each child as family size increases. Even though the effects of family size may work through different mechanisms in different parts of the world, the basic theories suggest there to be universal signs of the trade-off.

Still, it is not hard to come up with explanations as to why the effects may actually go in the other direction. Children may stabilize marriages or keep parents at home, which some presume to be beneficial for the upbringing of children. One could also argue that siblings act as role models or inspire each other to progress at school or in other arenas.

The net effects of family size must therefore be determined empirically. As already mentioned, recent work questions the conclusions from previous studies. The first objection is methodological: the observed correlation may not reflect causation. For instance, parents with preferences for small families might also be the ones who emphasize education and labor market success for their children. The second objection concerns the quality of data used: most studies are plagued by problems generated by small and often unrepresentative samples, and/or by poor child-parent match rates, making the estimates both imprecise and less reliable.

We use detailed Swedish population micro data covering the entire birth cohorts 1972–79 (843,333 individuals) and twin births to address both of these problems. Because twin births are essentially randomly determined they provide an exogenous source of variation in family size that can be used to distinguish causation from correlation.[2] Our data come from administrative records and include a wide range of educational and labor market outcomes: grades in all subjects ever taken,

[2] Rosenzweig and Wolpin (1980) were the first to use twin births as an instrument for family size.

GPA in compulsory and secondary school, transitions to higher education, highest degree attained, years of schooling, earnings, employment status, welfare dependence etc. We document effects through the educational system and then later in the labor market. Also, there is rich information on parental characteristics that makes it possible for us to directly investigate whether the effect of family size is stronger for parents with limited resources, as suggested by the seminal work by Becker and others.

Judging from recent empirical work, it seems that the jury is still out. Angrist et al (2006) combine several instrumentation strategies on Israeli data and state that the results are "remarkably stable in showing no evidence of a quantity-quality trade-off".[3] Black et al (2007a) find negative effects of sibship size on IQ in Norway. Qian (2006) argues that the family size effect on school enrolment varies with birth order in China, and Caceres (2006) finds inconclusive evidence on a number of outcomes in the US. Rosenzweig and Zhang (2006) find negative effects on parental investments in education in China. Grawe (2008) finds evidence of a trade-off between family size and several child outcomes including achievement scores.

Similar to Black et al (2005) and Angrist et al (2006) we find no effect of family size on long-term educational attainment or labor market outcomes. The analysis also shows that one risks overstating the impact of family size unless endogeneity is handled; OLS estimations suggest a substantial correlation between sibship size and all the outcomes considered. There is, however, some evidence that family size affects grades in groups that are likely to be vulnerable to reductions in parental investments: in large hosts of siblings, at higher parities and for children to low-educated parents. Furthermore, we find clearer impacts on subjects where parental investments are more likely to be influential.

Data

Our data come from the IFAU database, which builds on population-wide registers from Statistics Sweden. Combining information from several registers gives standard individual characteristics (earnings, place of residence, etc) as well as detailed information on performance in the educa-

[3] Another instrument that has been used in recent studies is sibling sex composition (e.g. Lee 2006, or Conley and Glauber 2006, Angrist et al. 2006) The argument for this approach is that parental preferences for mixed sex of their children encourage parents to have another child if their preferences are not satisfied at the latest attempt. However, the instrument has been criticized since research has shown that sex composition may have a direct effect on child outcomes (e.g. Butcher and Case 1994).

tional system. A "multi-generation" register provides links between children and their biological parents, and thereby to their siblings. Below we describe the sampling strategy and the information used. [4]

The sample consists of all individuals born in the years 1972–79. This means that we have information on 8 cohorts containing a total of 843,333 individuals. As described below, we use various subsamples of these individuals in the empirical analysis. The reason for choosing these cohorts is that we can observe their final grades in compulsory school; the educational registers start in 1988 and people typically graduate at age 16. Individuals who are not alive or not living in Sweden at age 16 are not included in the data. The data end in 2004 and thus the youngest cohort is followed to age 25.

We link each of these individuals to their biological parents and siblings through a unique parental identification number. We use the mother to link siblings to each other, but also connect each child to his/her biological father. In the register it is possible to observe the mother's total number of children up to and including 2004. Considering the cohorts studied it is likely that the observed number of children in 2004 is also the completed family size. The register contains information on year and month of birth, which makes it possible to identify twins. We also have information on the exact birth order of each child. It is important to note that the information on birth order and number of children is not conditional on having found the siblings in the other parts of the dataset (restricted to the population age 16–65 in the years 1985–2004). This information is directly recorded for each mother. Thus, we avoid the problem of poor match rates inherent in many previous studies.

Our instrument is a dummy variable set to unity for twin births at the nth birth (n={2,3,4}) and zero otherwise.[5] We restrict the sample to families with at least n births and study the outcomes of children born before the nth birth. Separate estimations are thus performed for kids from families with (potential) twin births at the second, third, and fourth birth respectively. We use twins only to construct the instrument and exclude all twins from the empirical analysis. The reason for not studying the outcomes of these children is that twin births are often premature resulting in e.g. low birth weight, which is known to affect children later in life (e.g. Black et al 2007b).

Parental variables can first be measured in 1985, and then annually through 2004. For two reasons we measure parental education in 1991: (i) there was a quality update based on the 1990 census; (ii) later observa-

[4] All registers are not available in all years, as discussed below. *Table A 1* presents all variables and which primary register they are taken from.

[5] Triplets and quadruplets are excluded from the analysis because they constitute extremely rare and unusual events.

tion makes it more likely that education is completed.[6] About 96.5 percent of the mothers are present in the data from 1991. For fathers, the corresponding figure is 92 percent. Those not in the data are older than 65, have emigrated or deceased. We include these parents and control for missing data in the regressions.[7] We also create measures of parental "permanent" income calculated as annual earnings (measured in 1985 prices) averaged over the observation years. Permanent income better captures parents' ability to invest in their children and current income has been shown to be a poor proxy of life-time income, especially at young ages (e.g. Böhlmark and Lindquist 2006, or Haider and Solon 2006).[8]

Table 1 displays the distribution of family sizes (number of children) for all mothers who gave birth at least once from 1972 through 1979. We see that somewhat more than half of the mothers give birth to one or two children, whereas having more than five births is quite uncommon.

Table 1 Distribution of mother's number of children

Number of children	Number of observations	Percentage	Cumulative distribution
1	70,851	11.57	11.57
2	277,157	45.26	56.83
3	175,584	28.67	85.50
4	59,210	9.67	95.17
5	18,505	3.02	98.19
6	6,510	1.06	99.25
7	2,578	0.42	99.67
8	1,072	0.18	99.85
9	465	0.08	99.92
≥ 10	462	0.07	100
Total:	612,394	100	

Table 2 gives some descriptive statistics on the children included in the estimations. The first two columns show means and standard deviations for first-born in families with two or more children. We see that the average child in this sample has about 13 years of schooling, and that as much as 92 percent has a high school degree. The university enrolment rate of 47 percent further signals that this is not a completely representa-

[6] Our results are not sensitive to the inclusion of this variable.
[7] Note that we have complete information on demographic characteristics for all parents and children (e.g. number of children and year of birth) from the multi-generation register. Thus, missing data is only an issue for the information on parents' socioeconomic status.
[8] This variable is defined both separately for each parent and combined as family permanent income. Note, though, that we do not condition on parental earnings in the main analysis, but use it to investigate the potentially heterogeneous effects of family size and to check whether parental characteristics are related to twin births.

tive sample of Swedish youth.[9] Educational attainment is relatively high, which is not so surprising given that first-born typically perform better than other children (see e.g. Black et al 2005). This is also clear when we compare the three samples. All measures of educational attainment decrease as we go from sample (i) to (iii): GPAs are lower, fewer graduate from high school and go on to university, and the total amount of schooling is lower in samples where family size and average birth order is higher. Similar patterns are also visible for labor market outcomes. Not surprisingly, the mothers of many children are also less educated on average, which also seems to be true for the fathers.

Table 2 Summary statistics for samples used in the analysis

Sample:	(i) First child in families with at least two births		(ii) First two children in families with at least three births		(iii) First three children in families with at least four births	
	(1) Mean	(2) Std. dev.	(3) Mean	(4) Std. dev.	(5) Mean	(6) Std. dev.
Individual characteristics						
GPA compulsory school	51.70	28.66	47.65	28.99	42.06	28.98
Graduated sec. school	.92	.28	.89	.31	.84	.36
GPA secondary school	51.37	29.02	49.49	29.09	46.18	29.24
Years of schooling	12.90	2.10	12.63	2.12	12.19	2.13
Enrolled in university	.47	.50	.42	.49	.34	.47
Welfare dependence	.06	.23	.07	.25	.10	.30
log(earnings)	7.20	1.16	7.14	1.17	7.06	1.21
Non-employed	.21	.41	.23	.42	.26	.44
Female	.49	.50	.49	.50	.49	.50
Age (in 2004)	28.68	2.28	28.51	2.29	28.41	2.31
Mother's characteristics						
Age (in 2004)	52.75	4.34	52.84	4.34	53.00	4.81
Education: Compulsory school	.23	.41	.27	.44	.36	.48
High school ≤ 2 years	.39	.49	.38	.49	.36	.48
High school >2 years	.09	.29	.08	.27	.06	.24
University ≤ 2 years	.15	.36	.14	.35	.11	.32
University >2 years	.14	.34	.13	.34	.10	.31
Father's characteristics						
Age (in 2004)	55.57	4.75	55.73	4.74	56.08	5.30
Education: Compulsory school	.29	.46	.32	.47	.37	.48
High school ≤ 2 years	.29	.45	.28	.45	.30	.46
High school >2 years	.16	.37	.15	.35	.12	.33
University ≤ 2 years	.11	.31	.10	.29	.08	.27
University >2 years	.16	.36	.16	.36	.13	.33
Family permanent income (in 1985 years prices)	206,021	104,964	191,437	106,545	161,766	101,793
Observations	291,467		232,495		93,463	
Pr(twins at nth birth)	.008		.010		.010	

Notes: The samples consist of children born 1972–79. Summary statistics for parental education and income is conditional on having found the parent in the employment register. For a description of the variables, see Table A.1.

[9] Further details on our measures of educational attainment are given below in the description of the institutional background and in Table A1 presenting the contents of the variables.

Institutional background

Sweden's educational system

This brief description of the Swedish schooling system draws primarily on Björklund et al (2005). We refer to that publication for further details on the education system in general, and for information on the reforms that took place in the 1990s.[10] For the cohorts considered here, practically everybody started their nine years of compulsory education at age 7, and followed a common curriculum determined by the central government. After the 9th grade, a vast majority moved on to upper-secondary education. In the mid-1980s, the transition rate was about 80 percent, but grew to as much as 97–98 percent in the mid-1990s (Landellet al 2000). The transition is still, however, voluntary, and also includes a choice between a number of vocational training programs on the one hand, and on the other a collection of programs preparing for further studies. Over time, the vocational programs have been reformed so to give eligibility for pursuing higher education. This involved a gradual change from two-year to three-year programs (which was the length of the preparatory programs throughout the observation period). In practice, however, university enrolment is still low after completion of the vocational programs. Furthermore, the possibility of "correcting" one's choice by adding grades for specific subjects was present for all the cohorts considered here.

After finishing upper-secondary school—typically at age 19—an increasing number of youth move on to college/university, although many times not immediately following graduation. Swedish universities are with few exceptions public, and there is a centralized admission system. There is of course heterogeneity in terms of the length of the university studies, both because programs differ and because students take additional programs/courses to a varying extent. A typical program leading to a Master's degree lasts 4–5 years.

Most grades used in our analysis come from the "old" system in which grades were on a scale from 1 to 5, where 5 was the highest. These grades were "relative" so that the national average for each cohort was to be 3.0.[11] The GPA used here is simply the mean of the individual's grades, rounded to one decimal. Since nobody has an average below 1, we have 40 steps in the GPA for these years. In the late 1990s the grading system

[10] Note that throughout the empirical analysis we include cohort fixed effects to capture effects of changes in the educational system (as well as other variations over time).

[11] In practice, the national average may vary slightly across cohorts since grades were not synchronized.

was replaced by an "absolute" scale with 80 steps in the observed GPA distribution.[12] Since there are institutional changes in the grading system and also a debate on increasing grade inflation in the new system, we: (i) use the by-cohort percentile ranking of the individual grade; (ii) include cohort dummies in all estimations.

Family policy in Sweden

One could argue that Sweden is not the first place to look for trade-off effects on children. The welfare state encompasses a number of measures to assist children and their parents; from health care, via child care, to financial aid (see Björklund 2006 or Hoem 1990 for details). Health care is free for all children, and until school start kids attend regular check-ups to monitor health and the development of physical and psychological skills. There is also a (more or less) mandatory vaccination program. Schools then take over the responsibility for following the children during their adolescence.

There are extensive earnings-related parental leave benefits, and also a "speed premium" which makes it possible to maintain benefit levels provided child spacing is sufficiently low (Andersson et al 2005). Public child care was rapidly expanded during the 1970s. Compulsory pre-school from age 6 had been implemented nationally by the late 1970s. An increasing majority of the children attend child care at a much younger age than 6; local governments are obliged to provide care to cover the time the parents spend on market work, job search or studies. Child care is heavily subsidized, and the fees are means-tested. Dismissal due to pregnancy, delivery or marriage has been illegal sine 1939, and since 1979 parents have the right to reduce work hours to 75 percent. There is also a flat rate child allowance, which is not means-tested. The amount has been changed over the years, and since 1982 there is a bigger allowance for the third child and beyond.

Abortion was legalized in 1975. If there are selective abortions due to twin pregnancies, the instrument may be invalid.[13] However, selective abortion of twins is extremely rare in Sweden and it is highly unlikely

[12] Each subject gives one of the following points: 0 (fail), 10 (pass), 15 (pass with distinction), or 20 (pass with special distinction). The GPA is then computed as the sum of the best 16 grades. The maximum score in the compulsory school GPA is 320, and the lowest score observed is 0. The secondary school GPA weights the subjects by the length of the courses taken, so that a long course affects the GPA more than a short course.

[13] A selective abortion is defined as one where the pregnancy is wanted and the motive for having an abortion is that the fetal is believed to have some unwanted characteristics. This is opposed to a general abortion where the motive is not the fetus but rather the pregnancy in itself.

that is constitutes a problem for our analysis.[14] Another potential concern is the use of fertility treatments, which can increase the probability of twin births and thereby cause a selection problem in twin births similar to that in family size in general. However, frequent use of fertility drugs and assisted conceptions is a quite recent phenomenon. For example, the first successful assisted conception in Sweden took place in 1982. Even though there are (negative and positive) trends in twin births over time, data suggest that a sharp rise Sweden did not occur before 1990 (Hoem and Strandberg 2004). Thus, since most of the siblings to our subjects (90.1 percent) were born prior to this year we do not think that this issue is likely to be a major concern. Note also that we use potential twin births at 2nd, 3rd and 4th whereas fertility treatments are arguably more common at lower parities.

Empirical strategy

We follow Black et al (2005) and Angrist et al (2006) and study the older siblings to potential twins, meaning that we compare e.g. first-born from families where the second birth was a twin birth to first-born from families where the second birth was a singleton. The advantage of this approach is that we avoid the potential problem that parents who choose to have another child after the occurrence of the twin birth possibly represents a selected sample. Also, restricting the sample to families with at least n births ensures that, ex ante, preferences for family size in families experiencing a twin birth or a singleton at the nth birth are the same.

To see the problems associated with estimating the causal effect of family size on child outcomes, consider the following regression model

$$Y_i = \gamma_0 + S_i\gamma_1 + \mathbf{P_i}'\boldsymbol{\gamma_2} + \mathbf{X_i}'\boldsymbol{\gamma_3} + u_i \qquad (1)$$

where Y_i is some measure of human capital indexed for individual i; S_i denotes family size; $\mathbf{P_i}$ is a vector of parental characteristics; $\mathbf{X_i}$ is a vector of individual characteristics; u_i is an individual specific error term. Equation (1) represents the standard model that has been used in previous literature (see e.g. Guo and VanWey 1999). Typically, these studies conclude that family size is adversely related to several outcomes (education, earnings, teen pregnancies etc).

[14] In 1999, 31,000 abortions were performed, out of which only 375 were classified as selective. Virtually all of these were performed due to illnesses or defects of the fetus.

The main concern with this model is that family size may be correlated with the error term, i.e. $\mathrm{E}[S_i u_i] \neq 0$. For instance, parents with low resources in some (unobserved) dimension might choose to have large families and also invest less in their children. If a negative shock, like unemployment, increases the likelihood of having another child (to feel needed or to qualify for economic benefits) and at the same time affects the outcomes of the children, we have a similar problem. Another potential source of bias is from simultaneity. Parents might adjust their perceptions of the optimal number of children depending on the quality of previous children. If their last child is of high quality, parents may feel no need to have another child, and vice versa (Behrman and Taubman 1986). One can also imagine an opposite situation where parents have babies until they find that they are unable to devote as much resources to the last one as they wish; Black et al (2005) interpret their finding of a "last child" effect in this way.

Given that twin births are determined by nature—and unrelated to parental characteristics— they can be used as an instrument for family size to get rid of bias originating from omitted variables and simultaneity. The first-stage in our 2SLS model can be written as

$$S_i = \pi_0 + T_i \pi_1 + \mathbf{P_i}' \pi_2 + \mathbf{X_i}' \pi_3 + v_i \qquad (2)$$

The instrument denoted by T_i is a dummy variable set to unity for the nth birth being twin and zero otherwise. Of course, for this approach to make sense, twin births must be correlated with family size, i.e. $\mathrm{E}[T_i S_i] \neq 0$. Furthermore, the standard exclusion restriction must hold: the instrument must not have an independent effect on the outcome, and must not be correlated with any unobserved factors affecting the outcome.

We have investigated this last issue by regressing the instrument on parental characteristics (see Table A2). Parental socioeconomic status is not found to be correlated with the instrument. This is expected, since twin births are essentially randomly determined. The fact that observed characteristics are not related to the probability of having a twin birth supports the assumption that neither are there unobserved characteristics influencing this probability.[15] It is however well-known that the probability of twinning increases with the mother's age (confirmed in a separate

[15] Remember that unobserved variables affecting twin births are only a problem if they are also related to the outcome variable, and if this correlation is not captured by the covariates included in the model.

analysis available upon request), which emphasizes the need to control for the mother's age when giving birth.

The second potential problem is harder to disregard: having younger siblings who are twins may affect you through other ways than the mere increase in family size. Some studies have shown evidence of an association between birth-spacing and children's attainments (e.g. Petterson Lidbom and Skogman Thoursie 2007). If this is the case, then twin births potentially affect older siblings through its effect on spacing. Also, twins have lower average birth weight, and may therefore require more of the family's resources than other kids (Rosenzweig and Zhang 2006). One way to investigate whether variation in family size given by multiple births is equivalent to variation coming from other sources is to ask whether there are effects beyond the increase immediately caused by the twin birth. The data used here contain indications on the existence of such effects: Åslund and Grönqvist (2007) show that e.g. the probability of having four children is higher among mothers experiencing twin births at the second birth. As noted by Angrist et al (2006), this could be explained by the fact that a twin birth effectively increases the available time for child-bearing.

What is, however, appealing about the twin strategy is that the reduced form—i.e. the impact of a twin birth on the outcomes of older siblings—is in itself interesting to estimate since it carries some policy relevance. If older siblings are affected, policy makers may want devote special attention to older siblings in families who for some reason have one more child than planned, or who have younger children with extra needs.

The second condition for the approach to be valid is that twin births affect family size, i.e. that the first stage regressions of the 2SLS models have explanatory power. As is evident from Table A3, this is clearly the case. Having a twin birth at the second birth increases family size by about 0.75 children. For twin births at higher birth-orders, the effects are even bigger. One could imagine different mechanisms behind this effect. Obviously, for many parents having twins at the second, third or fourth birth directly means one more child than planned. If there are other parents whose preferences are not so much concerning the number of children, but rather on having children during a sequence of years, these parents may still opt to have kids after the twin birth even if this results in a larger offspring than what they originally planned for. The fact that the compliance rates are high is encouraging since this implies that our 2SLS estimates come close to the average treatment effect of family size rather than a LATE (i.e. the impact for families who are induced to have another child because they had multiple births, see Angrist 2004). The F-statistics (corresponding to the null hypothesis that the coefficient on the instrument in the first stage regression is zero) take on values in the order of 886–3,904, suggesting that weak instruments are not a concern.[16]

Results

In this section we present the results from our empirical analysis of the impact of family size on child outcomes. The next sub-section presents the main results using twin births as an instrument for family size. We then provide results from robustness checks.

Before proceeding to the analysis of the causal impact of family size, let us look at Figure 1 showing correlations between sibship size and educational and labor market outcomes. The graphs are based on regressions of the respective outcome variable on a set of dummies for the number of children in the family. The reference group is children from one-child families. The differences in outcomes are quite small when the number of children is in the order of 1–3. For larger families there is however a sharp decline in the average outcomes. Kids with four brothers and/or sisters have as much as ten percentiles lower GPA in compulsory school, almost a year less of schooling, and earn about 12 percentage points less compared to single kids. [17] Previous studies have demonstrated that it is very easy to jump to conclusions regarding the effects of family size, given the strong correlations in the data. Clearly, this holds also for Sweden.

[16] These values are considerably larger than the values suggested by Staiger and Stock (1997) as being the lower limits that ensures that weak instruments cause no major problem.

[17] It is worth noting that the "effects of sibship size" consider the impact on a given individual. Provided that there are birth order effects, increases in family size means a change in *average* child quality in the family, even though the outcomes of the individuals are unaffected.

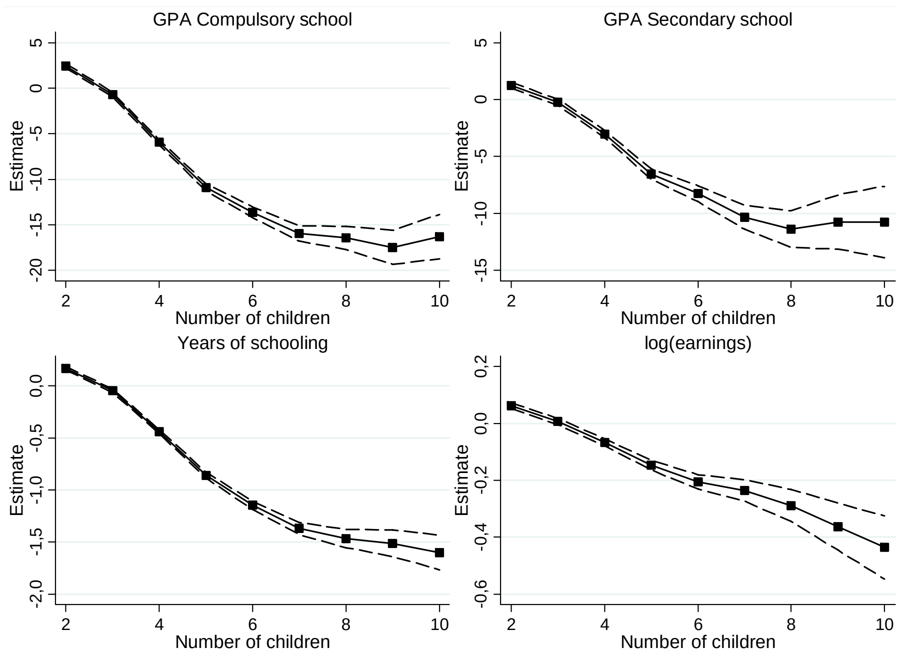

Figure 1 Correlation between family size (number of children) and various education and labor market outcomes

Notes: The graphs are based on regressions of the respective individual outcome variable on a set of dummies for the number of children in the family. Dashed lines represent 95% confidence intervals. No other covariates are included in the regressions. The omitted category is children from one-child families.

The baseline results

Table 3 presents results from separate regressions for an array of outcomes in different samples. Note that each cell in the table represents a unique regression. The models include fixed effects for birth order[18], gender, the individual's and his/her parents' birth cohorts, mothers' age at the nth birth (i.e. the potential twin birth), parental education (5 levels), and for missing parental data. Given the number of estimates included in the table we do not show the coefficients for other covariates—full results are available upon request. Let us, though, mention that these estimates show an expected and stable pattern: females perform better than males in school, highly educated parents mean better outcomes, and higher birth order implies worse outcomes.

[18] While birth order effects are indeed interesting, we choose to focus solely on family size in the presentation. One reason for this is that there appears to be less uncertainty regarding the effects of birth order (e.g. Black et al 2005, or Booth and Kee 2005), another is too avoid an exceedingly long paper.

The first row of results in panel A is for GPA in compulsory school. As we go down the table, the dependent variables become more long-term, ending in the panel B using labor market outcomes in 2004. There are three samples used in this analysis, all constructed in a similar way: we study effects on the n–1 first siblings in families with at least n births, using twin births at the nth birth as an instrument for family size. In other words: In the first sample we include first-born in families with at least two children, where the instrument is whether the second birth was a twin birth or not. For each sample there are three sets of estimates: OLS, Reduced form (RF) and 2SLS. In the OLS models we simply include family size among the regressors. These estimates are not to be interpreted as causal even though the samples are more homogenous compared to those used in Figure 1. The twin birth dummy is included directly among the regressors in the reduced form models. In the 2SLS models it is used as an instrument for family size. Provided that the underlying assumptions hold, these two models capture a causal link between the regressors and the dependent variables.

The OLS estimates consistently show a negative correlation between sibship size and outcomes: grades are lower, transitions to higher education less frequent, years of schooling fewer, non-employment more prevalent, earnings lower and welfare dependence more common. To get to the causal estimates, assume for now that the only reason that a twin birth influences the outcomes of older siblings is that it increases family size, which says that the 2SLS estimates are the ones to focus on. By contrast, these show no significant impact on any of the outcomes for samples (i) and (ii). For sample (iii), the results suggest a negative GPA impact in compulsory and secondary school of 2–4 percentiles.[19] Since also the GPA effects are small, statistical uncertainty is a problem. Åslund and Grönqvist (2007) use a larger number of cohorts (1972–87) and find significant GPA effects in both samples (ii) and (iii).

There is little doubt that one consequence of having twins is that family size increases. But it also means closer spacing of the offspring, which could mean harder restrictions on the families' resources, but also potentially economies of scale in e.g. homework assistance. Twins are also different in the sense that they can be expected to generate—but also divert—attention. In other words: it is quite possible that there are several mechanisms at work here, all of which reflect circumstances during childhood. Believers in this hypothesis would argue that the reduced form estimates are the

[19] The estimate for compulsory school is only borderline significant. It is somewhat puzzling that the GPA OLS estimates are smaller in absolute terms (although not significantly different from the 2SLS and RF specifications). Heterogeneous responses (cf. Angrist, 2004) are less likely to be the cause, considering the large RF estimates. One could therefore suspect that twin births are particularly influential in the short term compared to the average effect of increased family size. We also note that Black et al (2007a) find a similar pattern for IQ.

ones illuminating the causal effect of interest. As is clear from Table 3, the impression does not differ very much whether we look at the reduced form or at the 2SLS estimates. A high degree of similarity is also expected given the strong first stage estimates.

Table 3 OLS, Reduced Form (RF), and 2SLS estimates of the relationship between family size and child outcomes

Sample:	(i) First child in families with at least two births			(ii) First two children in families with at least three births			(iii) First three children in families with at least four births		
	(1) OLS	(2) RF	(3) 2SLS	(4) OLS	(5) RF	(6) 2SLS	(7) OLS	(8) RF	(9) 2SLS
Panel A. Education									
GPA compulsory	–1.189	.651	.844	–1.257	–.725	–.846	–.726	–1.493	–1.921
school	(.060)	(.505)	(.655)	(.082)	(.559)	(.651)	(.123)	(.915)	(1.171)
Graduated sec. school	–.016	.002	.002	–.019	–.002	–.003	–.014	–.002	–.003
	(.001)	(.005)	(.007)	(.001)	(.007)	(.008)	(.002)	(.012)	(.015)
GPA sec. school	–.625	.755	.969	–.884	–.470	–.549	–.470	–2.749	–3.531
	(.072)	(.581)	(.746)	(.096)	(.641)	(.748)	(.150)	(1.054)	(1.344)
Enrolled in university	–.013	.004	.005	–.016	–.003	–.003	–.012	–.014	–.018
	(.001)	(.009)	(.011)	(.001)	(.009)	(.011)	(.002)	(.014)	(.019)
Years of schooling	–.113	.017	.022	–.132	–.038	–.043	–.100	–.033	–.042
	(.004)	(.038)	(.048)	(.006)	(.042)	(.048)	(.009)	(.065)	(.083)
Panel B. Labor market									
Non-employment	.019	–.005	–.007	.023	.000	.000	.019	–.026	–.033
	(.001)	(.009)	(.010)	(.001)	(.009)	(.010)	(.002)	(.014)	(.018)
log(earnings)	–.049	.034	.044	–.056	–.022	–.025	–.039	.016	.021
	(.003)	(.024)	(.030)	(.004)	(.026)	(.030)	(.006)	(.041)	(.053)
Welfare dependence	.016	.001	.001	.020	.004	.005	.018	–.001	–.001
	(.001)	(.005)	(.006)	(.001)	(.005)	(.006)	(.002)	(.010)	(.013)

Notes: Each cell represents the coefficient on the Number of children variable in unique regressions. The sample consists of children born 1972–79. All regressions include fixed effects for birth order, gender, the individual's and his/her parents' birth cohorts, mothers' age at the nth birth, parental education (5 levels), and missing parental data. The instrument is a dummy for twin births at the nth birth. For definitions of the variables, see Table A.1. Standard errors robust for intra- family correlation are reported in parentheses.

Robustness checks

We have performed a number of robustness checks to investigate whether our results are sensitive to changes in sample composition or to the choice of covariates. Due to the large number of estimates involved in this exercise we do not report the results but provide a discussion of the most important findings.[20]

There is some evidence in the literature that the probability of having twin births differs across ethnicities (Myrianthopoulos 1970). This might be a concern since ethnicity is likely to be correlated with the error term

[20] All estimates are available upon request.

in the outcome equations. To deal with this issue we re-estimated our models including fixed effects for the mother's region of birth (27 strata aggregated by Statistics Sweden). The estimates are not sensitive to this inclusion.[21] We also tried dropping the covariates for parental education and found that the estimates are practically invariant, which strengthens our belief that omitted variables are not a problem.

Because we observe family size in 2004 it is possible that our estimates are not capturing the impact of twin births on completed family size. To examine this we imposed the restriction that the mothers must be at least 40 years old in 2004 (very few mothers in previous cohorts have children after this age). This restriction does not affect our estimates. Also, excluding very large families (>6 children) does not change our estimates.

Exploiting parental preferences for mixed sibling sex composition is an alternative instrumentation strategy. As noted by Angrist et al (2006), the twin instrument and the sex composition instrument identify different parameters, and there is substantial evidence that the sibling sex composition may affect children (e.g. Butcher and Case 1994) and parents (e.g. Dahl and Moretti 2004, or Johansson 2007) through other channels than family size. Åslund and Grönqvist (2007) develops these arguments and presents an analysis based on sibling sex composition, similar to the one in Angrist et al (2006). The analysis basically suggests that the effects of family size are very small, if existing.

Alternative intermediate outcomes

Since Table 3 suggests that there are some effects on GPA, we also experimented with some other intermediate outcomes. We found no impact of sibship size on delayed graduation from high school or on the probability to graduate from a preparatory high school program (as opposed to a vocational); see Table A4. Furthermore, the analysis showed that the secondary school GPA estimates do not change when the percentile ranking is performed by high school program and year (as opposed to year only in Table 3).

If the potential impact of family size goes through parental investments per child, one could argue that we would expect bigger grade effects in subjects where parental efforts—e.g. homework assistance—are more likely to matter. Table A5 presents estimates for grades in specific (groups of) subjects, which give some support to this idea even though standard errors are large. Family size appears to have no impact on per-

[21] One interesting variation would be to estimate separate models for children with foreign-born mothers. However, the number of twin births would be too low to get reasonable precision in the estimates.

formance in sports, but there are indications on effects on grades in Swedish, science and social science in samples (ii) and (iii).

Differential effects

We have investigated differential effects along several dimensions: gender, parental education, family income, and birth order (see Tables A6 and A7).[22] There is no evidence on differential impacts on boys and girls, but clear signs that the impact is stronger on children to low-educated parents.[23] We also split the samples by family permanent income; the patterns vary across samples and the estimates are in most cases not significantly different from each other. It therefore seems as if parental education matters due to other channels than providing higher income. This is perhaps not so surprising considering Sweden's compressed earnings distribution and extensive welfare state.

Table 3 suggests that the effects are only present for larger family sizes, which raises the question of whether the effects vary with birth order. It is indeed interesting to see that the most negative GPA estimates are for 3rd children in families with at least four births; in other words those who are closest in age to the potential twins with parents whose time constraint is most likely to be binding. We also investigated the possibility that the impact depends on one´s age at the potential twin birth. There was a tendency to larger negative effects for those below the median age in samples (ii) and (iii). In sample (i), there is actually a significant positive impact on those above the median age of 3, but no effect on the younger individuals. One (speculative) interpretation is that for this subsample the twin birth meant parents staying at home during a formative period, i.e. ages 4–6.

Concluding remarks

This paper investigates the effect of family size on children's educational and labor market outcomes in Sweden. As in other countries there is a strong correlation between family size and intermediate as well as long-run individual outcomes. Similar to other recent studies (Black et al 2005,

[22] The (mostly insignificant) estimates for the other outcomes in the Table A6 dimensions are included in Åslund and Grönqvist (2007).

[23] Note that we have taken a conservative approach in these regressions and excluded all individuals with *any* parent having missing information on education to avoid misclassification errors. This leads to samples with somewhat fewer observations than those used in the main analysis. An alternative strategy is to classify parents with missing data as non-academic; the estimates from the two approaches are very similar.

Angrist et al 2006), we find that most of the correlations do not have a causal interpretation. There is, however, some evidence that family size affects grades in compulsory and secondary education. The results give some support to the trade-off hypothesis: the impact is larger among children in more exposed positions (large sibships, high birth order, low-educated parents). Also, family size seems to be more influential in subjects where homework assistance is more important. But taken together, the evidence presented in this paper suggests that family size only plays a minor role in determining children's outcomes.

Our data are very rich, both in terms of the number of observations and concerning the variety of outcome variables available. Since the effects appear to be relatively small and only present for certain types of outcomes, detecting them may require large datasets of high quality. Our results are roughly in line with the results on IQ presented in Black et al (2007a).

The idea of parents having constrained resources for each child when the family becomes big enough is plausible, and it seems strange that it would have no impact on the children. On the other hand, the period during which children require the most attention is relatively short. One possible interpretation of the findings is therefore that while an unplanned increase in family size may imply restrictions that affect the older siblings negatively at some point during adolescence (causing lower grades), there is still time for parents, children and society to correct this behavior so that there are no clear long-term traces of family size.

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Appendix

Table A 1 Definitions of included variables (Statistics Sweden register in parentheses)

Variable	Definition
GPA comp. school	The percentile rank of compulsory school GPA (computed by year of graduation) (Grade 9 student register)
GPA sec. school	The percentile rank of upper secondary school GPA (computed by year of graduation) (Register of high school graduates)
Graduated sec. school	Indicator variable = 1 if completed upper secondary school no later than 2004; 0 otherwise (Employment register)
Enrolled in university	Indicator variable = 1 if enrolled in university no later than 2004; 0 otherwise (University register)
Years of schooling	Completed level of education translated into years of schooling according to the International Standard Classification of Education 1997 (ISCED97) (Employment register)
Welfare dependence	Indicator variable = 1 for the incidence of welfare in 2004; 0 otherwise (LOUISE)
log(earnings)	The natural logarithm of (annual) labor related income in 2004 (including self-employment) measured in hundreds of SEK (Employment register)
Non-employed	Indicator variable = 1 for not employment status "not employed" on November 1, 2004 (Employment register)
Delayed comp. (secondary) school	Indicator variable = 1 if graduated after age 16 (19); 0 otherwise (Register of high school graduates)
Preparatory program	Indicator variable = 1 if attended a theoretical/preparatory program in upper secondary school; 0 otherwise (Register of high school graduates)
Female	Indicator variable = 1 if female; 0 otherwise (Multi-generation register)
Parental characteristics	
Number of children	Mother's recorded number of children (Multi-generation register)
Education	Indicator variable = 1 for highest completed level of education; 0 otherwise (5 levels: compulsory school, high school ≤ 2 years, high school > 2 years, university ≤ 2 years, university > 2 years) (Employment register)
Permanent income	Annual labor related income (including self-employment) measured in 1985 prices and averaged over observation years. (Employment register)

Table A 2 Correlation between twin births and parental characteristics

Instrument:	(1) Pr(Twins at 2nd birth)		(2) Pr(Twins at 3rd birth)		(3) Pr(Twins at 4th birth)	
	Estimate	Std. error	Estimate	Std. error	Estimate	Std. error
Mother's characteristics						
log(permanent income)	–.0005	.0004	–.0007	.0004	–.0006	.0006
Compulsory school	Ref.		Ref.		Ref.	
High school ≤ 2 years	–.0015	.0011	–.0002	.0011	–.0020	.0016
High school >2 years	–.0025	.0014	–.0015	.0015	.0000	.0027
University ≤ 2 years	–.0013	.0013	.0021	.0014	–.0033	.0020
University >2 years	–.0016	.0015	.0002	.0016	–.0017	.0026
Father's characteristics						
log(permanent income)	.0002	.0003	–.0005	.0004	–.0003	.0005
Compulsory school	Ref.		Ref.		Ref.	
High school ≤ 2 years	–.0007	.0010	–.0015	.0011	–.0008	.0017
High school >2 years	–.0009	.0012	–.0019	.0014	–.0035	.0020
University ≤ 2 years	–.0002	.0013	–.0015	.0015	.0003	.0024
University >2 years	–.0012	.0012	–.0013	.0014	.0008	.0022
Number of observations	105,022		90,129		33,854	

Notes: The table reports estimates, together with robust standard errors, from regressions of dummies for twin births (at the nth birth) on parental characteristics. Each column represents a separate regression. Education is measured in 1985. The sample restricted to parents born before 1961 who experienced their nth birth (conditional on having at least n children) later than 1985. All regressions include fixed effects for birth cohort, year of the potential twin birth, and missing value on education. For definitions of the variables, see Table A.1. Value of F-statistic [p-value] corresponding to the null hypothesis that the coefficients on mother's {father's} characteristics are jointly equal to zero: column (1) 1.13 [0.34] {0.29 [0.92]}; column (2) 1.83 [0.11] {0.98 [0.43]}; column (3) 1.05 [0.39] {0.78 [0.57]}.

Table A 3 Family size explained by twin births

Sample:	(i) First child in families with at least two births		(ii) First two children in families with at least three births		(iii) First three children in families with at least four births	
Outcome	Estimate	F-statistic	Estimate	F-statistic	Estimate	F-statistic
GPA comp. school	.772	3,904	.857	3,155	.777	1,020
Graduated sec. school	.779	3,419	.872	2,597	.780	871
GPA sec. school	.779	3,586	.856	3085	.778	1143
Enrolled in university	.779	3,458	.872	2,598	.778	886
Years of schooling	.780	3,419	.871	2,629	.780	870
Unemployed	.779	3,419	.872	2,597	.780	872
log(earnings)	.778	3,242	.866	2,794	.777	950
Welfare	.779	3,458	.872	2,598	.778	886

Notes: The table displays first stage estimates by outcome. The F-statistic corresponds to the null hypothesis that the coefficient on the instrument (twin births) is zero. The sample consists of children born 1972–79. All regressions include fixed effects for birth order, gender, the individual's and his/her parents' birth cohorts, mothers' age at the nth birth, parental education (5 levels), and for missing parental data. For definitions of the variables, see Table A.1. Standard errors robust for within family correlation are reported in parentheses.

Table A 4 OLS and 2SLS estimates of the relationship between family size and alternative intermediate (instrument: twin births)

Sample:	(i) First child in families with at least two births			(ii) First two children in families with at least three births			(iii) First three children in families with at least four births		
	(1) OLS	(2) 2SLS	(3) Mean	(4) OLS	(5) 2SLS	(6) Mean	(7) OLS	(8) 2SLS	(9) Mean
Delayed comp. school	.011	–.002	.033	.013	.010	.041	.012	–.013	.061
	(.001)	(.004)		(.001)	(.005)		(.001)	(.009)	
Delayed sec. school	.013	.001	.128	.015	.001	.135	.014	–.018	.159
	(.001)	(.009)		(.001)	(.010)		(.002)	(.017)	
Preparatory program	–.017	–.016	.553	–.014	.003	.495	–.009	–.003	.424
	(.001)	(.013)		(.002)	(.013)		(.002)	(.023)	
GPA by	–.568	1.298	51.00	–.868	–.269	50.05	–.498	–3.980	47.82
year×program	(.076)	(.774)		(.100)	(.785)		(.155)	(1.390)	

Notes: Each cell represents the coefficient on the Number of children variable in unique regressions. The sample consists of children born 1972–79. All regressions include fixed effects for birth order, gender, the individual's and his/her parents' birth cohorts, mothers' age at the nth birth, parental education (5 levels), and for missing parental data. For definitions of the variables, see Table A.1. Standard errors robust for within family correlation are reported in parentheses.

Table A 5 2SLS estimates of the effect of family size on grades in single subjects/group of subjects in compulsory school using twin births as an instrument for family size

Sample:	(i) First child in families with at least two births		(ii) First two children in families with at least three births		(iii) First three children in families with at least four births	
	(1) OLS	(2) 2SLS	(3) OLS	(4) 2SLS	(5) OLS	(6) 2SLS
Swedish	–.575	.105	–.624	–.950	–.427	–2.577
	(.059)	(.610)	(.080)	(.616)	(.122)	(1.100)
Science	–1.251	.829	–1.428	–1.158	–1.019	–1.870
	(.062)	(.668)	(.083)	(.667)	(.127)	(1.191)
Social science	–1.223	.279	–1.226	–.707	–.815	–1.346
	(.061)	(.655)	(.082)	(.652)	(.124)	(1.197)
Sports	–1.343	.069	–1.651	.598	–.857	.184
	(.063)	(.664)	(.085)	(.685)	(.128)	(1.317)

Notes: Each cell represents the coefficient on the Number of children variable in unique regressions. The sample consists of children born 1972–79. All regressions include fixed effects for birth order, gender, the individual's and his/her parents' birth cohorts, mothers' age at the nth birth, parental education (5 levels) and for missing parental data. The dependent variable is the percentile rank of the (mean of the) grade(s) of the respective subject(s). Subjects included in Science are: physics, chemistry, biology, technology. Subjects included in Social science are: social science, history, geography, religion. For definitions of the variables, see Table A.1.. Standard errors robust for within family correlation are reported in parentheses.

Table A 6 2SLS estimates of the effect of family size on grades in different subpopulations using twin births as an instrument for family size

Sample:	(i) First child in families with at least two births		(ii) First two children in families with at least three births		(iii) First three children in families with at least four births	
	GPA comp.	GPA sec.	GPA comp.	GPA sec.	GPA comp.	GPA sec.
Estimate as in Table 3	.844	.969	–.846	–.549	–1.921	–3.531
	(.655)	(.746)	(.651)	(.748)	(1.171)	(1.344)
By gender						
Girl	.233	-.123	-1.744	-.030	-2.730	-3.497
	(.889)	(1.003)	(.902)	(1.021)	(1.656)	(1.929)
Boy	1.552	2.206	-.046	-1.084	-1.017	-3.483
	(.965)	(1.112)	(.883)	(1.027)	(1.483)	(1.784)
By parental education						
Academic parents	1.204	1.683	.507	.476	-.183	-1.994
	(.876)	(.991)	(.956)	(1.022)	(1.878)	(2.107)
Non-academic parents	.406	.178	-2.469	-2.040	-2.635	-4.421
	(1.011)	(1.151)	(.937)	(1.154)	(1.496)	(1.716)
By pos. in fam. permanent income distribution						
Lower third	1.101	1.717	-1.638	-2.533	-.707	-.291
	(1.222)	(1.461)	(1.248)	(1.500)	(2.404)	(2.960)
Middle third	.638	2.204	-.059	.828	-3.885	-7.422
	(1.243)	(1.361)	(1.163)	(1.364)	(1.855)	(2.153)
Upper third	1.174	-.125	-.562	-.161	-1.1054	-2.437
	(.996)	(1.147)	(1.008)	(1.112)	(1.829)	(2.055)
By age at the nth birth						
Below median	.094	–.087	–.970	–1.324	–2.792	–4.802
	(1.003)	(1.123)	(.950)	(1.089)	(2.011)	(2.083)
Above median	1.570	1.963	–.675	.169	–1.316	–2.596
	(.866)	(1.004)	(.856)	(1.000)	(1.354)	(1.710)

Notes: Each cell represents the coefficient on the Number of children variable in unique regressions. The sample consists of children born 1972–79. All regressions include fixed effects for birth order, gender (where appropriate), the individual's and his/her parents' birth cohorts, mothers' age at the nth birth, parental education (5 levels) and for missing parental data (where appropriate). The median age of the children in sample (i) [ii] {iii} is (3) [6] {8}. For definitions of the variables, see Table A.1. Standard errors robust to within family correlation are reported in parentheses.

Table A 7 2SLS estimates of the effect of family size on child outcomes by birth order using twin births as an instrument for family size

Outcome:	GPA comp.	GPA sec.	Grad. sec.	Enrolled univ.	Yrs school.	Non-emp.	log (earn.)	Welf. dep.
Families with at least three births								
Estimate as in Table 3	–.846	–.549	–.003	–.003	–.043	.000	–.025	.005
	(.651)	(.748)	(.008)	(.011)	(.048)	(.010)	(.030)	(.006)
1st child	–.800	–.336	.004	.008	.016	–.015	–.061	.009
	(.849)	(1.016)	(.010)	(.014)	(.062)	(.013)	(.043)	(.009)
2nd child	–.893	–.743	–.011	–.014	–.106	.017	.014	.001
	(.833)	(.976)	(.010)	(.014)	(.061)	(.014)	(.040)	(.008)
Families with at least four births								
Estimate as in Table 3	–1.921	–3.531	–.003	–.003	–.043	.000	–.025	.005
	(1.171	(1.344	(.015	(.011)	(.048)	(.010)	(.030)	(.006)
1st child	–.515	–2.397	.031	.002	.102	–.004	–.069	–.012
	(1.605)	(1.998)	(.022)	(.026)	(.117)	(.029)	(.087)	(.019)
2nd child	–1.057	–.936	–.055	–.022	–.238	–.025	.035	.025
	(1.861)	(2.265)	(.027)	(.029)	(.138)	(.030)	(.090)	(.022)
3rd child	–4.618	–7.630	.013	–.040	.009	–.080	.111	–.022
	(1.926)	(2.131)	(.026)	(.032)	(.141)	(.031)	(.096)	(.019)

Notes: Each cell represents the 2SLS coefficient on the Number of children variable in unique regressions. The sample consists of children born 1972–79. All regressions include fixed effects for gender, the individual's and his/her parents' birth cohorts, mothers' age at the nth birth, parental education (5 levels), and for missing parental data. For definitions of the variables, see Table A.1. Standard errors robust to within family correlation are reported in parentheses.

Essay 2: Putting teenagers on the pill: The consequences of subsidized contraception**

Introduction

Unintended childbearing is both frequent and widespread. For instance, in the U.S. almost 60 percent of all pregnancies are unplanned; a rate that is even higher among young women (Institute of Medicine 1995). The social and economic costs of unintended childbearing are potentially large since these births are associated with poor socioeconomic and health outcomes of both mothers and children. In addition, unwanted pregnancies account for approximately 1.5 million abortions annually in the U.S. alone (Institute of Medicine 1995). These concerns have motivated policy makers to instigate a wide range of family planning programs.[1] Despite the vast interest in such interventions there is however very scarce evidence on the efficiency of different policies.

This paper investigates the consequences of a series of Swedish policy changes beginning in 1989 where different regions started subsidizing the birth control pill. The reforms were significant and applied to all types of oral contraceptives. The subsidy rate was on average 75 percent. My identification strategy takes advantage of the fact that the reforms were

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[1] The Institute of Medicine (1995) reports that there are more than 200 local programs operating in the U.S. that in some way address unintended pregnancy.

implemented successively over time and targeted specific cohorts of young women, in particular teenagers. This generates plausibly exogenous variation in access to the subsidy, which is used to investigate the impact on abortions, fertility, marriage, educational attainment, and labor supply.

The main argument for subsidizing the birth control pill for teenagers is that young women may lack stable income sources, and therefore are more likely to prematurely end or delay the course of the treatment. Since the timing of the treatment is crucial for its success even short interruptions from the programme increases the risk of an unintended pregnancy. Still, it is not obvious that the demand for contraception is price elastic. Women who consider the cost of pregnancy as very high may either choose to completely abstain from sex or always pay the cost of getting the pill. Thus, it is not certain that subsidizing the pill will lead to a behavioral response. Furthermore, having access to inexpensive contraceptives could mean that women raise their level of sexual activity, increasing the likelihood of a pregnancy. This makes the net effect on fertility ambiguous. If women substitute between the ”pill” and other not as effective contraceptive methods in order to avoid unwanted births, a subsidy that changes the relative price between these technologies can potentially also affect the abortion rate.

There are several reasons for why easier access to oral contraceptives could matter for socioeconomic outcomes as well. The most obvious mechanisms are: delayed childbearing, smaller families or reduced risk of shot-gun marriages.[2] Additionally, it has been suggested that oral contraceptives may raise the returns to investments in education and work by reducing uncertainty about future interruptions from the labor market and school (Bailey 2006; Goldin and Katz 2002; Weiss 1986; Mincer and Polachek 1974). This means that a subsidy can have a direct effect on socioeconomic outcomes. A similar story is provided by Chiappori and Oreffice (2007) who propose that access to oral contraceptives may improve the woman’s bargaining position within a couple, leading to an increased share of the household’s resources; something that potentially could reduce female labor supply through a standard income effect.

The topic of this paper is related to a series of recent studies highlighting the role of the birth control pill for women’s well-being. Ananat and Hungerman (2007), Bailey (2006), Goldin and Katz (2002), and Guldi (2007) exploit cross-state and cross-time variation in different groups’ access to the birth control pill in the U.S. in the 1960s and 1970s. The

[2] Studies of the link between fertility, marriage, and socioeconomic outcomes include: Ashcraft and Lang (2007); Åslund and Grönqvist (2007); Holmlund (2005); Hotz, Mullins and Sanders (1997); Kearney and Levine (2007); Klepinger, Lundberg and Plotnick (1999); Maynard (1996); Stevenson and Wolfers (2007).

results suggest that access to the pill increased labor supply, lead to later age at first marriage, delayed childbearing, and reduced the abortion rate. Bailey (2007) takes advantage of variation in state laws regulating contraceptive sales from 1873 to 1965 (Comstock laws) and shows that access to the pill accelerated the reduction in U.S. fertility rates. More closely related to my paper is Kearney and Levine (2008) who examine the consequences of state-level Medicaid policy changes that expanded eligibility for family planning services to higher income women and to Medicaid clients whose benefits would expire otherwise. The results indicate that the reforms led to a nine percent decrease in births to eligible women age 20–44; a finding that is attributed to greater contraceptive use.[3]

My paper adds to this literature in several ways. First and foremost, it is the first to evaluate the social and economic consequences of subsidized oral contraceptives. As already suggested, this is a question of great interest for policy makers. The fact that the subsidy focused on a group of individuals often targeted in various preventive programs makes the policy relevance even clearer. Second, the impact of a recent subsidy is arguably more relevant for the contemporary debate over contraception since most countries already have introduced the birth control pill. Third, the rich data used makes it possible to study a wide variety of different outcomes, and to examine differential effects with respect to socioeconomic background.

I begin the empirical analysis by exploiting county level panel data to examine the relationship between the subsidy and the sales of oral contraceptives. The results suggest that the subsidy increased sales by on average 5–7 percent, and there is suggestive evidence that this effect is bigger for teenagers. I go on to study the impact on abortions. Using regional, temporal and cohort variation in access I find that the subsidy reduced the abortion rate by about 8 percent. There is also tentative evidence of an effect on the birth rate, although the estimates are insignificant at the 5 percent level. The estimates are robust to several sensitivity checks.

The last part of the paper uses population micro data to examine the effects on fertility, labor supply, educational attainment, and marriage. The results show that women with long-term access to the subsidy (>4.5 years) are 20 percent less likely to have a child before age 21. Consistent with the notion that access to inexpensive contraceptives matters more

[3] In a broader context, my paper is related to studies on the impact of abortion policies on women's outcomes and to a large literature on the relationship between birth control programmes and fertility in developing countries; see e.g. Gruber, Ananat and Levine (2007), Gruber, Ananat, Levine and Staiger (2006), Gertler and Molyneaux (1994), Miller (2005), Prichett (1994).

for financially constrained individuals this effect is found to be significantly stronger for women from poor socioeconomic background. However, I find no statistically significant effect on number of children, marriage, educational attainment, or labor supply; although some of the coefficients are relatively imprecisely estimated.

Background

Since its introduction in 1964 the birth control pill has grown to become the leading contraceptive method among young Swedish women (Santow and Bracher 1999).[4] The aim of this section is to describe the institutional setting surrounding the birth control pill. I then investigate whether the subsidy affected women's use of the pill.

Institutional setting[5]

In Sweden, oral contraceptives are sold by prescription from a doctor or midwife. The typical procedure for a young woman wishing to use the pill is to schedule an appointment at a youth clinic to meet with a physician. Youth clinics are health centers for teenagers that offer free consultation about contraception as well as associated medical examinations. Virtually all municipalities have at least one clinic. Individuals are also free to visit any private or public health care institution, but the process is still the same. If the physician deems oral contraceptives appropriate she prescribes the drug and the girl can then collect it at the state pharmacy. Parental consent to the treatment is not required. The physician is bound by the professional secrecy and if a girl does not want her parents to know about the treatment the physician cannot contact them. It is however common practice that the doctor or midwife in these cases tries to convince the girl to tell her parents.

The question of providing financial support for oral contraceptives targeted to young women was raised in the late 1980s. The Swedish government had since 1974 been directing large resources towards various family planning policies, including a national subsidy on oral contraceptives for all women. However, in 1984 the discount was abolished and the price of the pill quadrupled. The new policy also required users to renew prescriptions no later than every 3 months, instead of once a year,

[4] Almost 60 percent of Swedish women age 18–24 regularly use oral contraceptives (National Board of Health and Welfare, 2001).

[5] This section primarily draws on detailed descriptions of the reforms outlined in: Csillag (1993), National Board of Health and Welfare (1994, 2001, 2005) and Västragötalandsregionen (2000).

which of course meant that using the pill would call for more planning. Immediately after the policy change the sales of oral contraceptives started to fall and many youth clinics reported that teenage girls had begun to interrupt their treatment. Following a period of decreasing teenage abortion rates, abortions started to increase.[6] These events seem to have been what motivated the new reforms.

As the first region, the municipality of Gävle started subsidizing oral contraceptives for teenagers in 1989. The reform was evaluated by the local authorities and the results showed that the consumption of oral contraceptives among teenagers increased from 42 to 60 percent after subsidy was introduced.[7] Moreover, the teenage abortion rate fell by almost 50 percent. The experiment was considered as a success and in the following years other regions therefore launched policies based on the same principle as in Gävle, meaning that the subsidy targeted specific cohorts of young women. The subsidy rate was on average 75 percent and applied to all types of oral contraceptives (National Board of Health and Welfare 1994).[8] When introduced the policy temporarily received large attention from the local media and posters with information were printed and highlighted at the youth clinics.

Table 1 contains a description of the reforms up to 1993, which is the last year for which this information is available. Note that most of the regions that introduced the subsidy are counties, but some municipalities also participated. By the end of 1993 eight counties had still not implemented the reform.[9] From Table 1 it is clear that both the starting dates and targeted cohorts vary across regions and that only two areas provided the subsidy to women older than 20. In this context it is worth mentioning that the reforms did not overlap with other major changes in Swedish family policy (Björklund 2006).

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[6] Abortions have been allowed in Sweden on demand and basically free of charge since 1975 (Santow and Bracher, 1999).

[7] The evaluation consisted of a simple before and after analysis.

[8] Unfortunately, I do not have access to information about the regional specific subsidy rates.

[9] The fact that some regions may have implemented a subsidy after 1993 introduces some complications for my analysis; an issue I will return to later in the paper.

Table 1 The implementation of the subsidy

Regions which introduced the subsidy before 1994	Starting date	Eligible cohorts
Gävle (municipality)	Nov 01, 1989	$\leq 19^{*}$
Sandviken (municipality)	Nov 30, 1989	$\leq 19^{*}$
Partille (municipality)	Jan 01, 1990	≤ 20
Hofors (municipality) and Ockelbo (municipality)	Mar 31, 1990	$\leq 19^{*}$
Örebro (county)	Jun 01, 1990	$\leq 18^{*}$
Kristianstad (county)	Nov 29, 1990	$\leq 18^{*}$
Kronoberg (county)	Jan 01, 1991	≤ 19
Blekinge (county)	Mar 01, 1991	≤ 19
Solna (municipality)	Sep 01, 1991	≤ 22
Gotland (county)	Oct 01, 1991	$\leq 20^{*}$
Södermanland (county)	Jan 01, 1992	$\leq 19^{*}$
Malmöhus (county) (except Malmö municipality), Västernorrland (county), Älvsborg (county), Västmanland (county), Kopparberg (county)	Jan 01, 1992	≤ 19
Värmland (county)	Mar 01, 1992	$\leq 24^{*}$
Jämtland (county)	Apr 01, 1992	≤ 24
Göteborg (county) and Bohuslän (county) (except for Partille and Göteborg municipalities)	Jul 01, 1992	≤ 20
Gävleborg (county) (except for Gävle, Sandviken, Hofors and Ockelbo municipalities)	Nov 09, 1992	$\leq 19^{*}$
Uppsala (county)	Mar 01, 1993	≤ 19
Malmö (municipality)	Mar 26, 1993	≤ 18
Halland (county)	Jul 01, 1993	≤ 19
Regions which did not introduce the subsidy before 1994		
Stockholm (county) (except for Solna municipality); Östergötaland (county); Jönköping (county); Kalmar (county); Göteborg (municipality); Skaraborg (county); Västerbotten (county); Norrbottens (county);		

* Individuals are eligible for the subsidy until the calendar year they turn this age.

Prior to the reforms, a full year's supply of the birth control pill sold for just below USD 100 (in 2008 year's price level). [10] Although the price might seem fairly low, for young teenage girls without own incomes the costs of obtaining oral contraceptives could very well amount to a large fraction of their budget. This situation is especially likely to be problematic for girls that for some reason can not ask their parents for money to get the pill, and is worsened by the strong regularity requirements surrounding the treatment programme. In order for oral contraceptives to provide maximum protection against pregnancy the treatment must proceed for 21 days followed by a seven day recess. If these conditions are not fulfilled, protection is immediately endangered. In fact, anecdotal evidence from youth clinics prior to the reforms suggests that many unintended pregnant girls stated that they had not been able to start a new treatment because they could not afford the pill at the day the program was scheduled to begin and therefore had been forced to postpone it (National Board of Health and Welfare, 1994).

[10] The price varied slightly depending on the type of product but there was no regional variation in prices prior to the reforms.

The impact on sales and consumption

Did the subsidy really increase the use of the pill? To answer this question I use information from the state pharmacy (Apoteket) on the total sales of oral contraceptives in each county and year starting in 1980. The state pharmacy is the sole provider of prescriptive drugs in Sweden, so sales should provide a good proxy for consumption. Sales are reported in terms of the annual number of (defined) daily dosages sold per woman age 15–44.[11]

Before proceeding to the econometric analysis it is useful to start the examination by graphically illustrating how sales have evolved over time. Figure 1 plots sales by year from 1980 through 2000. We can see that sales increase up until 1984, after which there is a sharp decline. This decline coincides perfectly with the abolishment of the major nationwide subsidy of oral contraceptives described earlier. The vertical line marks the starting year of the new reforms and we can see that sales starts to rise in precisely this year.

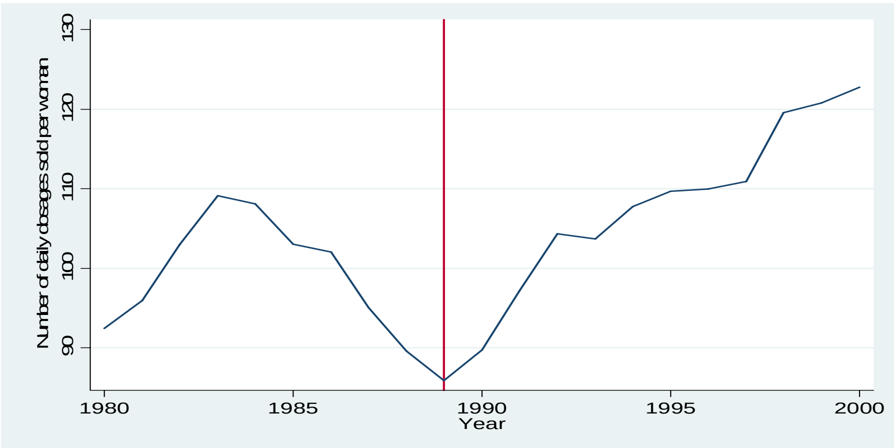

Figure 1 Number of (defined) daily dosages sold per woman by year
Note: Vertical line marks the starting year of the reforms.

Although suggestive, one cannot be certain from the graphical evidence that no unobserved factors affecting sales occurred simultaneously as the reforms. One such factor could be increased awareness of the risks associated with HIV/AIDS. To rule out potential confounders I turn to a more formal analysis by estimating regressions of the following form

$$Sales_{ct} = \beta Policy_{ct} + \mu_c + \mu_t + \rho(\mu_c \times t) + \varepsilon_{ct} \qquad (1)$$

[11] The measure takes into account varying content of hormones in different products.

where $Sales_{ct}$ is the (log) number of dosages sold per woman age 15–44 living in county c in year t. $Policy_{ct}$ is a dummy for the county having implemented the subsidy; μ_c is a set of county fixed effects; μ_t is a set of year fixed effects; $\mu_c \times t$ is a set of linear county trends.

This is a standard difference-in-differences specification where the county-specific fixed effects take into account all persistent county characteristics affecting sales, such as permanent differences in fertility, access to family planning services, etc. Similarly, year fixed effects control for all time-varying factors that affect sales in different counties in the same way, e.g. changes in the national family policy. Linear trends control for smoothly evolving factors within each county. The model assumes that no unobserved regional-specific events affecting sales happened at the same time as the introduction of the subsidy. A total of 19 counties observed from 1980 through 1993 are included in the analysis.[12]

The results from the regressions can be found in Table 2. Column (1) presents estimates excluding linear county trends, i.e. $\rho = 0$. The reported standard errors are robust to serial correlation at the county level. The coefficient suggests that the subsidy increased sales by just below 7 percent. The estimate is highly significant. Nevertheless, one should bear in mind that, because there are rather few counties, the standard errors may understate the standard deviation of the estimator (Bertrand, Duflo and Mullainathan 2004). Column (2) shows that the coefficient is robust to including linear county trends.

One potential concern is that regions which introduced the subsidy even in its absence would have experienced increased sales. To investigate this I have run regressions exploring the relationship between future subsidies (t+2 years) and current sales. If causality runs from the subsidy to sales then one should find that future subsidies do not affect current sales, conditional on current policy.[13] The results are displayed in column (3). As can be seen, the coefficient on current policy is still significant and the estimate virtually unchanged. In contrast, the coefficient on future policy is insignificant.

[12] The following counties are excluded from the analysis due to limited availability of data: Älvsborgs län, Bohuslän, Kristianstads län, Malmöhus län, Skaraborgs län. Note also that I cannot use information for later years since some regions may have introduced the subsidy after 1993.

[13] To be specific, $Policy_{(t+2)}$ is a dummy that switches from zero to one two years before the implementation of the subsidy and stays on. This "falsification" test has previously been used by Lochner and Moretti (2004), Black, Devereux and Salvanes (2004) and Dahl (2005) to investigate the exogeneity of compulsory schooling laws. Note that the results are similar also when using higher leads.

Table 2 OLS estimates of the effect of the subsidy on the sales of oral contraceptives

	Dependent variable: Log(Number of dosages sold per woman)		
	(1)	(2)	(3)
Policy	.068 (.025)	.047 (.018)	.044 (.017)
$\text{Policy}_{(t+2)}$	-	-	.025 (.021)
County fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Linear county trends	No	Yes	Yes
N	266	266	266

Notes: Standard errors robust to heteroscedasticity and serial correlation at the county level (19 cells) in parenthesis. The sample consists of a panel of all Swedish counties (except Älvsborgs län, Bohuslän, Kristianstads län, Malmöhus län, Skaraborgs län), observed from 1980 through 1993.

Note that these regressions estimate the average effect of the subsidy across all cohorts and therefore cannot tell how much of the effect is due increased pill use among teenagers. To perform a cohort specific analysis I make use of a survey called ULF (Undersökningen av Levnadsförhållanden). The survey asks women age 16–84 whether they have consumed oral contraceptives within the last two weeks prior to the survey date. The question was asked in one round before the reforms and one round "after" (1980/81 and 1996/97). The survey consists of a (cross-sectional) random sample of about 3,500 Swedish women and the sample size net of attrition is sufficiently large to disaggregate the data by age cohort.[14] [15] Statistics Sweden compiled the data on my behalf.

In the first round, 25.8 percent of 16–20 year olds stated that they had consumed oral contraceptives within the last two weeks. The same figure for 21–24 year olds is 35.8 percent, and 25.2 percent for 25–30 year olds. All cohorts increased their use of the pill up to the second round where the corresponding numbers were: 35, 45.9 and 30.6 percent. This means that consumption grew by 36 percent for the youngest cohort, 28 percent for individuals age 21–24, and 21 percent for 25–30 year olds. Thus, the increase in consumption use was indeed largest in the eligible cohorts.[16]

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[14] Attrition in ULF is generally around 25 percent.

[15] Unfortunately, sample size restrictions, in combination with the fact that some regions may have implemented the reforms after 1993, prevents me from disaggregating the data by region.

[16] Of course, this can be due to a range of different factors not related to the reforms and the results should therefore be interpreted with caution. The most obvious concern is that the Swedish women may have brought forward their sexual debut. However, the average age at first intercourse has been stable around age 16 since the 1960s (Forsberg 2005).

Taken together, I believe that the results presented in this sub-section provide credible evidence that the subsidy actually did increase the use of oral contraceptives among young women.

The impact on abortions and birth rate

Having established that the subsidy increased pill use I now investigate whether access to it affected the likelihood that a woman had an abortion, as well as the consequences for the birth rate. The analysis makes use of publicly available data on all legal abortions performed in Swedish counties from 1985 and onwards. This information was obtained from the National Board of Health and Welfare (Socialstyrelsen).[17] The only related study in any field that I am aware of is Ananat and Hungerman (2007) who use cross-state and cross-time variation in the pill's diffusion at the time when it was introduced in the U.S. to explore whether access to the pill affected the risk of a young unmarried woman having an abortion. When analyzing the birth rate I use aggregated data from the IFAU-database.[18] My baseline estimates are obtained from the following model

$$Outcome_{cat} = \gamma Policy_{cat} + \theta_c + \theta_a + \theta_t + \theta_{ca} + \theta_{ct} + \theta_{at} + v_{cat} \qquad (2)$$

where c, a, and t denote county, age cohort (five year intervals) and year, respectively. The outcome is either the (log) abortion rate or the (log) birth rate. The θ's represents fixed effects for county, age cohort, year, and all of their interactions.

This model is very flexible and takes into account most potential confounders. The fixed effects control for nationwide changes in the outcome over time, time-invariant county characteristics, permanent differences across cohorts, within-county and within-cohort shocks, as well as the fact that permanent county-specific differences could matter more for different cohorts. The identifying assumption is that there should be no unobserved county specific shocks occurring simultaneously as the introduction of the subsidy which also affect the *relative* outcomes between the cohorts.

The results are shown in Table 3. As a benchmark, I start by providing evidence on the impact of the subsidy on *teenage* abortions, relying only on cross-county and cross-time variation in the introduction of the policies. The specification is analogous to equation (1) and to the model used by Ananat and Hungerman (2007), who find that access to the pill low-

[17] The data can be found on the following web-page: http://www.socialstyrelsen.se/
[18] The database is described in detail in Section 4.

ered the teenage abortion rate from 27 abortions per every 1000 women to 22, implying a decrease of about 18 percent. Column (1) shows that the subsidy decreased the number of teenage abortions by about 6 percent.

The fact that my estimates are smaller in magnitude than those presented by Ananat and Hungerman (2007) is natural since the introduction of the birth control pill in the US in the 1960s and 1970s is likely to have had larger consequences for the use of oral contraceptives. For instance, compared to my result showing that access to the subsidy increased sales by about 7 percent (cf. Table 2), Goldin and Katz (2002) finds that more lenient state regulations regarding minors was associated with 33–40 percent greater pill use by young unmarried women.

As already mentioned, column (1) assumes that no other events affecting the outcome occurred in the same year as the subsidy was introduced. Column (2) relaxes this assumption by using older not eligible cohorts as control groups. Even if an unobserved shock occurred simultaneously as the subsidy this will not bias the estimates as long as it does not also affect the relative abortion rate between different cohorts. Column (2) shows that the estimates are similar to those in column (1). The estimate is statistically significant and the coefficient suggests that the abortion rate is reduced by about 8 percent.

Last, by the same argument as earlier, column (3) tests the exogeneity of the subsidy by investigating the relationship between future subsidies and the current abortion rate. As can be seen, the coefficient on future policy is close to zero and insignificant, suggesting that the policies indeed were exogenous.

Table 3 OLS estimates of the effect of the subsidy on the abortion rate

	Dependent variable: Log(Teenage abortion rate) (1)	Log(Abortion rate) (2)	Log(Abortion rate) (3)
Policy	–.060 (.031)	–.080 (.033)	–.077 (.036)
$Policy_{(t+2)}$	-	-	–.007 (.032)
County fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Cohort fixed effects	-	Yes	Yes
N	171	684	684

Notes: Robust standard errors in parenthesis. Additionally, the standard errors in column (1) are robust to serial correlation at the county level. The sample consists of a panel of all Swedish counties (except Älvsborgs län, Bohuslän, Kristianstads län, Malmöhus län, Skaraborgs län), observed from 1985 through 1993. The regressions in column (1) cover the teenage abortion rate and the unit of observation are all teenagers age 15–19, in a given county and year. The regressions in column (2) cover the abortion rate for all women in the following age cohorts: 15–19, 20–24. 25–29, 30–34, and the unit of observation are all women in each cell. In addition to county, year and cohort fixed effects, columns (2) and (3) include all interactions between these variables.

Table 4 examines the consequences of the subsidy for the birth rate.[19] The empirical approach is identical to the one used when analyzing the abortion rate. Column (1) shows results for the teenage birth rate. We can see that the subsidy decreased the teenage birth rate by about 7.5 percent. The estimate is however not significant at the 10 percent level. Column (2) uses older cohorts as control groups within each county-by-year cell. The estimate is basically identical to that in column (1) and the precision has increased; a finding that is natural since the number of observations has increased. Still, the coefficient is insignificant at the 5 percent level (p-value .075). The Column (3) tests the exogeneity of the policy by adding a dummy for future policy. As earlier, this coefficient is close to zero and insignificant.

[19] For comparison purposes I include the same set of counties and cohorts in the analysis as in Table 3. Note however the results are virtually identical to using all counties and cohorts.

Table 4 OLS estimates of the effect of the subsidy on the birth rate

	Dependent variable:		
	Log(Teenage birth rate) (1)	Log(Birth rate) (2)	Log(Birth rate) (3)
Policy	–.076 (.054)	–.075 (.042)	–.090 (.045)
$\text{Policy}_{(t+2)}$	-	-	.033 (.040)
County fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Cohort fixed effects	-	Yes	Yes
N	171	684	684

Notes: Robust standard errors in parenthesis. Additionally, the standard errors in column (1) are robust to serial correlation at the county level. The sample consists of a panel of all Swedish counties (except Älvsborgs län, Bohuslän, Kristianstads län, Malmöhus län, Skaraborgs län), observed from 1985 through 1993. The regressions in column (1) cover the teenage birth rate and the unit of observation are all teenagers age 16–19, in a given county and year. The regressions in column (2) cover the birth rate for all women in the following age cohorts: 16–19, 20–24. 25–29, 30–34, and the unit of observation are all women in each cell. In addition to county, year and cohort fixed effects, columns (2) and (3) include all interactions between these variables.

Consequences for socioeconomic outcomes, fertility and marriage

This section examines the effect on women's socioeconomic outcomes, fertility and marital status. The analysis exploits rich micro data covering the entire Swedish population age 16–65 during the period 1985–2004.[20] One part of the database includes annual information on standard individual characteristics (earnings, place of residence, etc). It also contains several registers with educational information, as well as a "multi-generation" register linking children to their biological parents.

My sample consists of all Swedish women born during the period 1965–1975. The reason for this restriction is that including older cohorts increases the likelihood that some individuals may have left their homes at the time when I observe them, enhancing the risk of both measurement error and selective sorting. Furthermore, younger cohorts cannot be used since I only have detailed knowledge about the reforms up until 1993 and wish to avoid the possibility that later cohorts in the control regions may have been exposed.[21] For most cohorts, region of residence is defined

[20] The information is taken from the IFAU-database and was originally collected by Statistics Sweden.
[21] I know that some regions did in fact introduce the subsidy after 1993, although I have no information on what cohorts were eligible or the exact starting date.

according to where the girl lived at age 16. Individuals born 1965–1968 are assigned a residential area depending on where they lived in 1985.

All subjects are linked to their biological parents and information is added on each parent's education and earnings in 1985. I then add information on the birth dates of the subjects' children.[22] Using place of residence in combination with the subject's birth date I construct a variable measuring the cumulative length of exposure to the subsidy, starting at age 14 and ending when she no longer is eligible.

The empirical analysis focuses on several types of outcomes: fertility, marriage, educational attainment, and labor market status. Teenage childbearing is defined as having the first child no later than age 20. I also study whether the woman has completed university or high school. My data contain information on a wide range of labor market and income variables as well: annual earnings, employment status, welfare take-up, and disposable income.

All outcomes are recorded in 2004 when the subjects are 29–39 years old. This avoids the possibility that some individuals may not have completed their education.[23] Table A.1 contains a detailed description of how the variables have been constructed and from which registers the information has been collected. Table A.2 displays summary statistics.

Main results

The empirical strategy takes advantage of cross-regional and cross-cohort variation in access to the subsidy to identify the parameters of interest. I estimate regression models of the following form

$$Outcome_{ibm} = \alpha_0 + Exposure_{bm}\alpha_1 + X_i'\alpha_2 + \lambda_b + \lambda_m + \delta(\lambda_m \times b) + v_{ibm}$$

where the outcome is indexed for individual *i* in birth cohort *b* from municipality *m*; $Exposure_{bm}$ is a measure of the cumulative exposure to the subsidy; X_i is a vector of background characteristics; λ_b and λ_m represents year of birth and municipality specific fixed effects, respectively; $\lambda_m \times b$ represents municipality specific trends. The model ignores regional and cohort differences which are absorbed by the fixed effects. Thus, the identifying assumption is that once that I condition on region,

[22] Note that the multi-generation register contains information on the woman's number of children and her children's birth dates even though the children themselves may be too young to be included in the population sample of the database.

[23] The age restrictions imply that the estimates will not capture the impact of the subsidy on completed fertility.

cohort, and possibly also background characteristics, exposure should not be correlated to the error term, i.e., $E[v_{ibm} | Exposure_{bm}, X_i, \lambda_b, \lambda_m, (\lambda_m \times b)] = E[v_{ibm} | X_i, \lambda_b, \lambda_m, (\lambda_m \times b)]$.

The key variables of interest are four dummies indicating the cumulative exposure to the subsidy. The reference group is individuals with no exposure. I also present results from models where exposure is defined linearly. All regressions include fixed effects for municipality of residence and year of birth. In addition, I control for each parent's earnings and age, with dummies for each parent's highest completed level of education (five levels), missing information on education or earnings, municipality specific linear trends, and immigrant status. All standard errors are clustered at the municipality level to take into account possible serial correlation (286 cells).[24]

To conserve space, I do not report estimates for the control variables, but it is worth mentioning that these are all significant and display expected signs: having highly educated, as well as older parents, means a lower probability of becoming a teenage mother, fewer children, more schooling, higher earnings, a lower probability of being non-employed and receiving welfare, and higher disposable incomes. The same is true for children to high income parents.

Table 5 contains results for fertility and marital status. The specification used for fertility is different than in Table 4 in the sense that the focus is now on exposure length. I start by asking whether the subsidy affected family size. We can see in column (1) that the coefficients are monotonically decreasing in exposure length, suggesting a dose-response relationship. Still, the F-statistic which tests the null hypothesis that the coefficients on exposure are jointly equal to zero is insignificant.

Column (2) displays results for the probability of becoming a teenage mother. We can see that women exposed to the subsidy for more than 54 months are on average about 20 percent (–.013/.067) less likely to become teenage mothers, although the effect of shorter exposure is more moderate. Also for this outcome there are clear indications of a dose-response relationship. The F-statistic rejects the null hypothesis of no joint effect. Similar conclusions can be drawn from the linear measure of exposure in Panel B. On average, each additional year of exposure reduces the probability of teenage motherhood by .3 percentage points. This implies that exposure for 5 years lowers the probability of teenage childbearing by 22 percent ((.003*5)/.067). These estimates are comparable to the results presented by Bailey (2006) who finds that the probabil-

[24] I have also experimented with accounting for intra-group correlation at the municipality×cohort level with similar results (cf. Moulton 1990).

ity of experiencing the first birth by age 22 fell by 16 percent in states that had relaxed restrictions on older teens' eligibility to the pill.

The impact of long-term access is significant and it is relevant to ask whether the results make sense. In this context it is worth mentioning that these regressions cannot separate between age at first exposure and length of exposure: a cohort that experienced long-term exposure is also a cohort where the subjects were exposed early in life. If easier access to contraceptives is more important in the early teens this could potentially explain the relatively large effects.

Last, column (3) examines the impact on the probability of marriage. This effect is ex ante ambiguous since better planned births may both decrease the likelihood of (shot-gun) marriages as well as improve the quality of later marriage. The F-statistic in column (3) shows no significant effect of exposure to the subsidy on the probability of being currently married, although the coefficient on exposure for 37–54 months is significantly negative.

Table 5 OLS estimates of the effect of the subsidy on fertility and marital status

	Dependent variable:		
	Nbr. of children (1)	Pr(Teenmother) (2)	Pr(Married) (3)
Panel A			
Exposed 1–18 months	.004 (.009)	.003 (.002)	–.005 (.004)
Exposed 19–36 months	–.010 (.011)	–.003 (.003)	–.008 (.005)
Exposed 37–54 months	–.015 (.013)	–.006 (.003)	–.013 (.006)
Exposed > 54 months	–.017 (.024)	–.013 (.005)	–.013 (.009)
F-statistic [p-value]	0.93 [.447]	5.85 [.000]	1.30 [.269]
Panel B			
Years of exposure	–.005 (.004)	–.003 (.001)	–.003 (.002)
Municipality fixed effects	Yes	Yes	Yes
Year of birth fixed effects	Yes	Yes	Yes
Mean of dependent variable	1.452	.067	.391
N	588,367	588,367	588,367

Notes: The sample consists of all women born 1965–l975 All regressions controls for each parent's earnings and age, and with dummies for each parent's education (five levels), for missing information on education and earnings, and immigrant status. The regressions include linear municipality trends. The outcomes are observed in 2004. Parental characteristics are measured in 1985. Standard errors robust to heteroscedasticity and serial correlation at the municipality level (286 cells) are shown in parenthesis. The omitted category in Panel A is women with no exposure to the subsidy. Reported F-statistic tests the null hypothesis that the coefficients on exposure are jointly zero. See Table A.1. for definitions of the included variables.

Next, I examine the impact of the subsidy on socioeconomic outcomes. Columns (1) and (2) in Table 6 provide results for educational attainment. We can see that exposure to the subsidy is neither statistically significantly related to the probability of graduating from high school, nor to the likelihood of completing university. The F-statistics, as well as the individual coefficients, are all insignificant. I have also run regressions using (imputed) years of schooling as dependent variable with similar results. This conclusion holds for labor supply as well: columns (3) – (6) find no statistically significant effect on the probability of being non-employed, annual earnings, the probability of receiving welfare, or disposable income; however a few point estimates are just marginally insignificant for disposable income.

Given that I find a negative impact on the probability of teenage childbearing, it might at first glance seem surprising that there is no significant effect on socioeconomic outcomes. Furthermore, Bailey (2006) demonstrates that access to the pill before age 22, at the time when it was introduced in the US, raised the number of hours worked. Still, the results should be interpreted having in mind that some of the coefficients are fairly imprecisely estimated.

It is also relevant to ask how the estimates reconcile with past research on the consequences of early childbearing. To answer this question, consider the following thought experiment: if the entire (potential) effect of access to the pill on education is via its effect on teenage childbearing, what would the results in previous studies imply for my estimates? The most credible Swedish study to date is Holmlund (2005) who uses within-family variation in childbearing decisions and shows that teenage motherhood decreases the average length of schooling by .59 years. Observe that my analysis is based on comparing outcomes across cohorts, while Holmlund's analysis is executed at the individual level. Taken together, this paper and Holmlund's results show that 3 out of 1000 individuals in the total population potentially prolonged their education by .59 years (cf. Table 5, Panel B, Column 2). This is not a particularly big effect, and one that probably would be difficult to detect in the data.

To summarize, the results suggest that exposure to the subsidy significantly lowers the probability of teenage motherhood. However, I find no statistically significant effect on number of children, marriage, educational attainment, labor supply, or welfare take-up. Next, I assess the robustness of the estimates.

Table 6 OLS estimates of the effect of the subsidy on socioeconomic outcomes

	Dependent variable:					
	Pr(High sch. grad.) (1)	Pr(Univ. grad.) (2)	Pr(Non-employed) (3)	Log(earnings) (4)	Pr(Welf.) (5)	Log(Disp. inc.) (6)
Panel A						
Exposed 1–18 months	.001 (.003)	.001 (.004)	.001 (.003)	–.003 (.011)	.001 (.002)	.001 (.004)
Exposed 19–36 months	.001 (.003)	–.001 (.005)	–.000 (.004)	.007 (.012)	.001 (.002)	.005 (.004)
Exposed 37–54 months	–.001 (.003)	.001 (.007)	–.002 (.005)	.008 (.014)	.002 (.002)	.008 (.005)
Exposed > 54 months	.002 (.007)	–.009 (.013)	–.007 (.009)	.013 (.027)	.008 (.004)	.018 (.011)
F-statistic [p-value]	0.27 [.894]	0.60 [.662]	0.26 [.906]	0.32 [.867]	1.49 [.205]	0.87 [.483]
Panel B						
Years of exposure	–.000 (.001)	–.000 (.002)	–.001 (.001)	.003 (.004)	.000 (.001)	.002 (.001)
Municipality FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year of birth FEs	Yes	Yes	Yes	Yes	Yes	Yes
Mean of dept. var.	.931	.425	.176	7.120	.089	7.084
N	587,503	587,503	587,503	517,733	587,503	585,744

Notes: The sample consists of all women born 1965–1975. All regressions controls for each parent's earnings and age, and with dummies for each parent's education (five levels), for missing information on education and earnings, and immigrant status. The regressions include linear municipality trends. The outcomes are observed in 2004. Parental characteristics are measured in 1985. Standard errors robust to heteroscedasticity and serial correlation at the municipality level (286 cells) are shown in parenthesis. The omitted category in Panel A is women with no exposure to the subsidy. Reported F-statistic tests the null hypothesis that the coefficients on exposure duration are jointly zero. See Table A.1. for definitions of the included variables.

Robustness checks

My identification strategy is based on several assumptions. First, the timing of the reforms should not be correlated with regional trends. Second, individuals should not respond to the policy by selectively moving. Although it is unlikely that families would change their residential area because of the subsidy I provide some evidence on this issue by investigating what happens to the estimates when removing some key covariates. Parents' education and earnings is perhaps the variables most likely to be associated with selective relocation. If unobserved factors are at least equally as important as these observed characteristics, dropping the latter can provide insights as to whether omitted factors may be driving the results. If I find the estimates sensitive to removing covariates, then one might suspect that omitted variables are important as well. By the same argument, removing the municipality-specific trends can give information on the likelihood of differential trends biasing the estimates.

Table 7 presents results where I successively remove covariates. To conserve space I only report estimates for the linear measure of exposure, but the results are similar when using dummies. Reassuring is that the coefficients are not sensitive to removing controls for parents' education and earnings. In a few cases the results are somewhat sensitive to dropping linear trends. For instance, for the probability of graduating from university and logged disposable income the coefficients become almost significant at the 5 percent level, and for number of children, the coefficient switches sign but stay insignificant. For the other outcomes, the coefficients are however quite stable and the overall conclusions still hold. The results highlight the importance to control for trends to account for slow-moving economic and demographic changes in each region.

Table7 Consequences of removing covariates

	Change in specification:			
Dependent variable:	Estimate as in Tables 5 and 6 (1)	+Removing controls for each parent's education (2)	+Removing controls for each parent's earnings (3)	+Removing municipality trends (4)
Number of Children	–.005	–.005	–.005	.001
	(.004)	(.003)	(.003)	(.002)
Pr(Teenage mother)	–.003	–.003	–.003	–.001
	(.001)	(.001)	(.001)	(.000)
Pr(Currently married)	–.003	–.003	–.003	–.001
	(.002)	(.002)	(.002)	(.001)
Pr(High school graduate)	–.000	–.000	–.000	–.000
	(.001)	(.001)	(.001)	(.000)
Pr(University graduate)	–.000	–.000	–.000	.003
	(.002)	(.002)	(.002)	(.001)
Pr(Non-employed)	–.001	–.000	–.001	–.001
	(.002)	(.001)	(.001)	(.001)
Log(earnings)	.003	.003	.003	.002
	(.004)	(.004)	(.004)	(.002)
Pr(Welfare)	.000	.000	.001	–.000
	(.001)	(.001)	(.001)	(.000)
Log(Disposable income)	.002	.002	.003	.003
	(.001)	(.001)	(.002)	(.001)

Notes: The table reports the coefficient on "Years of exposure". The sample consists of women born 1965–l975. All regressions control for municipality and year of birth fixed effects, each parent's age, and immigrant status. All outcomes are observed in 2004. Parental characteristics are measured in 1985. Standard errors robust to heteroscedasticity and serial correlation at the municipal level (286 cells) are shown in parenthesis. See Table A.1. for definitions of the included variables.

Differential effects

Since there are no strong indications that the results are driven by omitted factors I continue the analysis by examining whether the effect varies by family background characteristics. Table 8 displays estimates for the linear measure of exposure; but the results are not sensitive to how exposure is defined. Each cell represents a separate regression. The focus is on education and earnings. "Academic family" is defined as having at least one parent having completed at least theoretical/preparatory high school. "Non-Academic family" is defined as both parents having completed at most vocational high school education. Similarly, "High-income family" is defined as having at least one parent above the median in the earnings distribution (defined separately for mothers and fathers). A "Low-income family" is a family where both parents are below the median in their respective earnings distribution.

We can see that the effect of exposure to the subsidy on teenage childbearing is significantly more negative for women from "Non-Academic" families, and there is also a tendency for stronger effects in "Low-income" families. These findings are consistent with the idea that access to inexpensive contraceptives may have more profound effects for financially constrained individuals. There are also indications of a negative effect on the likelihood of marriage for women from less educated families. However, I do not find any evidence of differential effects for the other outcomes.

Table 8 Differential effects with respect to family background

	Change in sample:				
Dependent variable:	Estimate as in Tables 5 and 6 (1)	Academic Family (2)	Non-Academic Family (3)	High-Income Family (4)	Low-Income Family (5)
Number of Children	–.005	–.004	–.004	–.004	–.007
	(.004)	(.004)	(.005)	(.004)	(.007)
Pr(Teenage mother)	–.003	–.001	–.004	–.002	–.004
	(.001)	(.001)	(.001)	(.001)	(.002)
Pr(Currently married)	–.003	–.002	–.005	–.002	–.004
	(.002)	(.002)	(.002)	(.002)	(.003)
Pr(University grad.)	–.000	.001	–.003	–.001	.003
	(.002)	(.002)	(.003)	(.002)	(.003)
Pr(High school grad.)	–.000	.000	.000	–.000	–.000
	(.001)	(.000)	(.002)	(.001)	(.002)
Pr(Non-employed)	–.001	.000	–.001	–.002	.000
	(.001)	(.001)	(.002)	(.001)	(.002)
Log(earnings)	.003	.006	.005	.008	.001
	(.004)	(.005)	(.005)	(.004)	(.007)
Pr(Welfare)	–.004	–.000	–.000	–.000	.001
	(.003)	(.001)	(.001)	(.001)	(.001)
Log(Disposable income)	.002	.003	.003	.003	.001
	(.001)	(.002)	(.002)	(.002)	(.003)

Table 8 cont'd

Notes: Standard errors robust to heteroscedasticity and serial correlation at the municipality level (286 cells) in parenthesis. The table reports the coefficient on "Years of exposure". The sample consists of women born 1965–l975. Wherever appropriate, the regressions controls (linearly) for each parent's earnings and age, with dummies for each parent's education (five levels), for missing information on education and earnings, and immigrant status. All regressions include municipality and year of birth fixed effects, and linear municipality trends. All outcomes are observed in 2004. Parental characteristics are measured in 1985. "Academic family" is defined as having at least one parent who has completed at least theoretical/preparatory high school. "High income family" is defined as having at least one parent above the median in each parent's earnings distribution. See Table A.1. for definitions of the included variables.

Concluding Remarks

While most countries are committed to reducing unintended childbearing, and thereby improve the well-being of both mothers and children, there is little consensus on the efficiency of different policies.

This paper examines the consequences a series of Swedish policy changes beginning in 1989 where different regions started subsidizing the birth control pill. The reforms were significant and applied to all types of oral contraceptives. My identification strategy takes advantage of temporal, regional and cohort variation in the implementation of the subsidy, generating plausibly exogenous variation in access.

Using county level panel data I find that the subsidy increased sales by some 5–7 percent and reduced the abortion rate by about 8 percent. There is also tentative evidence of an effect on the birth rate, although the coefficient is insignificant at the 5 percent level. The estimates are robust to several sensitivity checks. Using rich population micro data I also examine the effect on socioeconomic outcomes, fertility, and marital status. The results show that women with long-term access to the subsidy are 20 percent less likely to have a child before age 21; an effect that is significantly stronger for women from poor socioeconomic background. I find no statistically significant effect on number of children, marriage, educational attainment, or labor supply, although some of the estimates are rather imprecise.

From a policy perspective, the results suggest that subsidizing oral contraceptives may be a fruitful way both to reduce abortions as well as lower the incidence of teenage childbearing. In this context, it is relevant to ask whether the results in this paper can be extended to other settings. Sweden is well-known for its extensive welfare state which encompasses a number of measures to assist children and their parents (Björklund 2006). Child care is heavily subsidized and local governments are obliged to provide care to cover the time the parents spend on market work and education. There are extensive earnings-related parental leave benefits and parents have the right to reduce work hours to 75 percent. There is also a flat rate child allowance.

With these policies in mind it is perhaps not so surprising to find no significant effects on long-term socioeconomic outcomes. It is conceivable that Sweden’s generous family policy compensates women for the potential detrimental effects of having an unplanned child. Thus, the consequences of introducing a similar reform as the one studied in this paper could be more far-reaching in other countries.

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Appendix

Table A.1 Definitions of key variables and data sources

Variable	Definition	Data source
Teenage mother	Indicator = 1 for having first the child no later than age 20; 0 otherwise.	Multigeneration register
Number of children		Multigeneration register
High school	Indicator variable = 1 for highest completed level of education being high school; 0 otherwise.	Employment register
University	Indicator variable = 1 for highest completed level of education being university; 0 otherwise.	Employment register
Non-employed	Indicator variable = 1 for employment status “not employed” on November 1, 2004.	Employment register
Earnings	Labor related income (including self-employment) in hundreds of SEK.	Employment register
Welfare	Indicator variable = 1 for the incidence of welfare; 0 otherwise.	LOUISE
Disposable income	After tax income plus all transfers recieved.	LOUISE
Currently married	Indicator variable = 1 for currently married; 0 other-wise.	LOUISE
Parental characteristics		
Education	Indicator variable = 1 for highest completed level of education; 0 otherwise (5 levels: compulsory school, high school ≤ 2 years, high school > 2 years, university ≤ 2 years, university > 2 years).	Employment register
Earnings	Labor related income (including self-employment) measured in hundreds of SEK.	Employment register

Notes: Parental characteristics are observed in 1985. All other variables are observed in 2004, if not indicated otherwise.

Table A.2 Summary statistics

Variable	Mean	Standard deviation
Teenage mother	.067	.250
Number of children	1.452	1.151
High school	.931	.254
University	.423	.494
Non-employed	.176	.381
Log(earnings)	7.120	1.161
Welfare	.089	.285
Log(Disposable income)	7.084	.463
Currently married	.391	.488
Exposed 1–18 months	.072	.258
Exposed 19–36 months	.072	.259
Exposed 37–54 months	.049	.256
Exposed > 54 months	.023	.216
Years of exposure	.650	1.483
Mother		
Age (in 1985)	41.581	6.148
Compulsory school	.419	.493
High school ≤ 2 years	.344	.475
High school > 2 years	.052	.222
University ≤ 2 years	.090	.286
University > 2 years	.094	.292
Earnings	595.61	406.965
Father		
Age (in 1985)	44.591	7.080
Compulsory school	.416	.493
High school ≤ 2 years	.249	.433
High school > 2 years	.153	.360
University ≤ 2 years	.068	.252
University > 2 years	.114	.318
Earnings	1079.96	746.11

Essay 3: Peers, neighborhoods, and immigrant student achievement: Evidence from a placement policy***

Co-authored with Olof Åslund, Per-Anders Edin and Peter Fredriksson

Introduction

In most Western countries the inflow of immigrants has risen substantially over the past decades.[1] The recently arrived individuals tend to settle in close proximity to people sharing their ethnic background, thereby reinforcing the growth of "ethnic enclaves" (Stark, 1991). There is a large literature on the impact of residential segregation on outcomes of minorities in general,[2] including some studies that have explicitly considered the impact on recent adult migrants (Edin et al. 2003, Gould et al. 2004, Åslund and Fredriksson 2008). The effect of immigrant concentration on the educational achievement of child migrants is equally interesting but has so far received relatively little scientific attention. This is perhaps somewhat surprising given the recent literature arguing that the early environment plays an important role for children's skill formation and long-term economic outcomes, and that the impact of the environment is more pronounced in disadvantaged families (e.g., Cunha and Heckman 2007). The purpose of this paper is to empirically examine the role of ethnic concentration among migrant youth in compulsory school performance.

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[1] For a summary of the OECD experience, see e.g. Friedberg and Hunt (1995).

[2] See, e.g., Cutler and Glaeser (1997) Bertrand et al. (2000), Grönqvist (2006), and Goel and Lang (2007) for recent contributions.

Theoretical research gives no clear predictions on how ethnic concentration per se will affect minority students. Ethnic peers may be beneficial if they, e.g., provide information on the workings of the educational system, but detrimental if residential concentration hampers proficiency in the host country's language. Several studies also point out that the effects are likely to vary with the quality of the contacts. Well-established and educated peers may act as role models, but living among people with poor socioeconomic status and performance may have a negative influence on youth (cf. Cutler and Glaeser 1997). Peer pressure can also generate incentives to perform poorly at school to gain status in a disadvantaged group (cf. the "acting white" phenomenon analyzed by, e.g., Austen-Smith and Fryer 2005).

There is a growing body of—largely US—research studying the effects of racial composition within schools or neighborhoods on students' academic performance (see e.g., Angrist and Lang 2004; Boozer et al. 1992; Card and Rothstein 2007; Grogger 1996; Guryan 2004; Hanushek et al. 2002; Hoxby 2000; and Rivkin 2000). In general, the results from these studies suggest that minority students who attend schools with a large fraction of ethnic peers, or are in other ways exposed to a disproportional share of minority peers, perform worse academically than other minority students.

As noted above, the issue of peer effects among child migrants has received little attention in the academic community. There are several reasons for focusing on immigrants in particular. First, the group typically performs substantially worse than other students in industrialized countries (OECD 2006). Second, many governments run various types of policies aimed at influencing where new immigrants settle (Edin et al. 2004); thus, knowledge on the importance of peer characteristics is highly policy relevant. Third, it seems reasonable that peers can exert particularly strong influences on young migrants striving to find their place in the new country.

Cortes (2006) is one of the few studies examining whether ethnic concentration affects the school performance of immigrants.[3] She studies the effect of age at arrival and attending an enclave school on the test scores of a sample of first and second generation immigrants residing in the cities of Miami and San Diego in the U.S. The results suggest that attend-

[3] See Bygren and Szulkin (2007) for a related study using Swedish data. Their analysis suggests that peer effects are channeled through compulsory school performance, since long-term correlations between peers and educational attainment disappear when compulsory school grades are accounted for. Jensen and Rasmussen (2008) have examined whether student outcomes are related to immigrant concentration using Danish data. Their estimates suggest a negative impact of immigrant concentration on student performance, but the study does in practice not handle residential self-selection.

ing an enclave school (defined as one where above 25 percent are foreign-born) has no effect on students' test scores.

In many ways, Borjas (1995) is the study which is most similar to the present one. He found that immigrants who grew up in ethnic communities with an abundance of human capital did better on the labor market. However, as for many other studies of contextual effects, one could worry that selection problems bias the estimates in Cortes (2006) and Borjas (1995). This is mainly because a student's neighborhood or school is a family choice variable. If parents choose neighborhoods/schools based on unobserved characteristics that also affect learning outcomes, the estimates will be biased and cannot be interpreted causally.

Some recent studies have relied on placement policies generating exogenous variation in the initial residential distribution. We have previously used this approach to study economic outcomes among adult migrants (Edin et al. 2003, Åslund and Fredriksson 2008, Åslund et al. 2006, and Åslund and Rooth 2007). Between 1987–1991 Swedish authorities assigned refugees to their initial location. Since individuals were not free to choose, we argue that the initial location was independent of (unobserved) individual characteristics, an issue we will obviously return to below.[4]

Our strategy is quite demanding on data availability. We have access to administrative records containing detailed information on all students graduating from Swedish compulsory schools during 1988–2003. The data also contain rich individual information on the population age 16–65 from 1985 and onwards, and provide the opportunity to link children to their parents. This means that we can identify when the individual arrived, where he or she initially resided, the characteristics of his/her parents, and also the properties of the neighborhood peers at different points in time.

The results suggest that a one standard deviation increase in the fraction of highly educated peers in the assigned neighborhood raises compulsory school GPA by 0.9 percentile ranks; a corresponding increase in the size of the ethnic community in the assigned neighborhood has about the same effect, but the effect is less precisely estimated. Peer influences are larger among those who arrived before age seven than for those who arrive at an older age.

[4] Gould et al. (2004) use a similar placement policy where Ethiopian refugees were distributed across Israeli municipalities to identify the causal effect of school quality on students' high school grades. In a sensitivity analysis they include the fraction of Ethiopian children in the class as a covariate, and thus touch on the question of ethnic peer effects. The estimate turns out to be insignificant. There are also papers exploiting similar policies in Denmark; see e.g. Damm (2005, 2007).

Had we not accounted for residential self-selection using the placement policy our conclusions regarding the impact of ethnic concentration would have been very different. Auxiliary regressions suggest that disadvantaged children (in the unobserved sense) are sorted into neighborhoods with a high share of members from their own ethnic group. The sorting bias is so severe that the size of the ethnic community at the time of graduation is negatively related to student outcomes. Sorting bias does not plague the estimate on the educational composition of the ethnic group, however.

The analysis also shows that the effects of the educational composition of peers do not vary across the population of child migrants. However, the size of the ethnic community is more important for boys and for children whose parents are less-educated, two groups that have the poorest school outcomes. These results shed light on the sorting bias alluded to above. Having a less-educated family background, for example, is arguably negatively correlated with the unobserved determinants of school outcomes. The results on heterogeneous effects thus suggest that it is rational for students from weak backgrounds to sort themselves into ethnic communities, which, again, is the sorting pattern we observe in our data.

The above results are obtained by holding the overall population of immigrants constant. In auxiliary regressions, imposing more restrictive assumptions, we also report evidence on how school performance is affected by the size of the total immigrant community. These tentative results suggest that immigrant concentration is detrimental for school performance, but that the positive effects of ethnic concentration prevails.

Background

Immigration and residential concentration in Sweden

Sweden has a large immigrant population: 12 percent (out of a population of 9 million) are foreign-born. Even though Sweden has received net migration since the 1930s, the larger inflows began in the 1950s and 1960s as workers were recruited not only from neighboring countries such as Finland, but also from Central and Southern Europe and Turkey. Starting in the 1970s, labor migrants were gradually replaced by refugees and family reunification migrants, a development that accelerated in the 1980s and 1990s. The large refugee inflows have changed the source country composition of the immigrant population dramatically. Parallel to the demographic changes there has been a decline in the economic performance of migrants. Today, Sweden stands out as one of the countries with the largest immigrant-native differentials in the labor market (OECD 2007).

As in other Western countries, the immigrant population is concentrated to certain regions and neighborhoods. Greater Stockholm, Göteborg and Malmö host about one third of the overall population but as much as half of the foreign-born. Within larger regions, immigrants tend to be concentrated to particular areas, usually situated in the suburbs (Åslund et al. 2006). The residential concentration is also reflected in the immigrant share of the neighborhoods populated by the foreign-born.[5] The typical immigrant lives in an area where a quarter of the working-age population is foreign-born, which can be compared to the national average of 12 percent.

Previous studies show that the typical immigrant-dense neighborhood contains a mix of ethnic groups—they are primary united by a shortage of natives (Andersson 2000). Still, different groups are relatively concentrated in different areas; e.g. Iranians constitute a substantially larger share of the foreign-born in Göteborg than in Sweden's other major cities. Also at the finest geographic level this segregation is evident; people have substantially more country-of-origin peers living in their SAMS area than what can be explained by regional sorting or by a division of immigrants and natives in general. We will return to this issue in the description of our sample of child migrants.

Immigrants in Swedish compulsory education

Compulsory education is 9 years in Sweden and starts at age 7; the typical age at graduation is thus 16.[6] There is a national curriculum that all compulsory schools follow. After compulsory school a vast majority go on to secondary education (the fraction grew from 80 percent in the mid-1980s to 97–98 percent in the mid-1990s; see Landell et al. 2000), even though secondary education is voluntary.

We study cohorts graduating the nine-year compulsory school between 1988 and 2003. Within this time-frame, the grading system was reformed. Up until 1998, grades given at graduation were on a scale from 1 to 5 and relative in the sense that the national average for each graduating cohort was to be 3.0. We use the GPA (i.e. the mean of the individual's grades), rounded to one decimal. Given that there are no observations with GPA below 1, there are 40 steps in the GPA for these years. From

[5] As described in the data section we use SAMS (Small Area Market Statistics) areas to define neighborhoods.
[6] See Björklund et al. (2005) for further details on the Swedish education system.

1998, grades are on an "absolute" scale, which is to be based on performance only and not related to the achievement of others. Each subject gives one of the following points: 0 (fail), 10 (pass), 15 (pass with distinction), or 20 (pass with special distinction), and the GPA is defined as the sum of the best 16 grades. The maximum score is thus 320, the minimum is 0, and the distribution contains 80 observed steps. Given the differences in the grading system over time, and the fact that there is evidence of grade inflation in the new system (e.g., Cliffordson, 2004), we use the by-cohort percentile ranking of the individual grade and include cohort dummies in all estimations.

Of special interest for our study are the rules for allocating students to schools. Up until 1991, the Swedish compulsory school system assigned students to the school nearest situated to their residential area. This residence principle is still the leading rule on how to allocate students to schools. However, in 1992, the central government introduced a school choice reform, where parents in principle are free to choose their children's school within the municipality. It is important to note, however, that parental preferences are severely constrained by space limitations, and priority is always given to kids residing close to the school. Thus, the assignment of refugee children to neighborhoods to a very large degree determined which schools they attended. Also, since there are far more neighborhoods than schools, controlling for area of residence effectively also means controlling for schools.

There is ample evidence that immigrant children perform poorly in the Swedish school system.[7] According to PISA 2003, the gap between the Swedish-born and the foreign-born at age 15 amounts 70–80 score points (which corresponds to 0.7–0.8 standard deviations of the PISA score distribution) in math, reading and science. The gap between the native-born and immigrants is about twice as large as the gender difference in reading. Within the immigrant group, there are big differences depending on time spent in Sweden: those who arrive after age 7 perform substantially worse than those who migrate before age 7 (see Böhlmark 2008).

The refugee placement policy[8]

In 1985, the Swedish Immigration Board was given the task of assigning newly arrived refugee immigrants to an initial municipality of residence.

[7] See Lundh et al. (2002) and Björklund et al. (2005) for discussions of previous research.

[8] Several previous studies have used the placement policy as a source of exogenous variation in studies of adult migrants (Edin et al. 2003; Åslund and Fredriksson 2008, Åslund and Rooth 2007; and Åslund et al. 2006). Since the underlying assumptions are the same in our analysis of child migrants, we refer to Edin et al. (2003) for a detailed description of the placement policy.

The policy was introduced in response to complaints from cities that had experienced a rise in immigration and perceived this as a burden on local public budgets. By placing asylum seekers in municipalities that had suitable characteristics for reception the government hoped to improve the reception process.

Because of the large inflow of asylum seekers in the late 1980s, the number of receiving municipalities was increased from 60 to include 277 of Sweden's 284 municipalities in 1989. Available housing essentially determined the placement. The policy was formally running 1985–1994, but the implementation was strictest between 1987 and 1991. During this period, the placement rate was around 90 percent, and the individuals involved were given very little room to choose the initial municipality of residence. Therefore, we focus our analysis on the 1987–91 period.

Asylum seekers were placed in refugee centers pending a decision from the immigration authorities. The centers were located all over Sweden, and center assignment was independent of port of entry to Sweden. The mean duration between entry into Sweden and the receipt of a permit varied between three and twelve months during 1987–1991. After receiving the permit, municipal placement occurred within a much shorter period of time, partly because there were explicit goals for reducing the time span between receipt of the residence permit and placement. Refugee preferences were considered in the municipal assignment, but individuals applied for residence in the largest cities where there were few vacancies because of the economic boom. Assigning a refugee to a municipality was conditional on having found a vacant apartment within that particular municipality. (Since individuals were assigned to an apartment, they were in practice assigned to a SAMS area.) After having been assigned to an apartment, refugees were basically free to move. The only "cost" of moving, apart from direct moving costs, was delayed enrolment in language courses.

Placement as a policy experiment

The a priori arguments for considering placement as exogenous with respect to the unobserved characteristics of the individual are the following: (i) the individual could not choose his/her first place of residence due the institutional setup, the practical limitations imposed by scarce housing, and the short time frame between the receipt of residence permit and placement; (ii) there was no direct interaction between local placement officers and individual refugees, meaning that any selection must have occurred on observed characteristics.

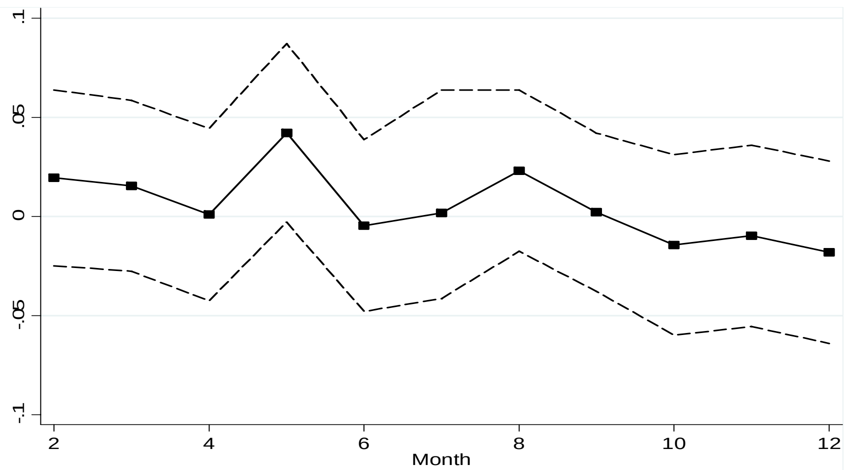
With respect to the first point, note that the timing of the receipt of the residence permit must coincide with the arrival of a housing vacancy in the preferred location, if the immigrant was to realize his or her most preferred option. The joint probability of these two events happening at the same time must be considered extremely low.[9]

Previous work substantiates the argument that the placement policy did create a geographic distribution which was independent of unobserved individual characteristics. Edin et al. (2003) show that the overall geographic distribution of those subjected to the placement policy differed from the location choices made by migrants arriving from the same regions shortly before the reform. Åslund et al. (2006) show that the initial characteristics of the assigned locations differed pre/post reform, but that after 9–10 years in Sweden the sorting pattern of those who arrived under the placement policy came to resemble that of other migrants. We take this as evidence that people were not able to realize their preferred option.

A strict test of our assumption that placement is exogenous conditional on observables is hard to come by since it requires a characteristic which was not exploited by placement officers but correlated with the unobserved ability of the individual. Nevertheless, we have examined whether the share of highly educated in the ethnic community ("ethnic human capital") in the assigned location is correlated with month of birth—which is related to various outcomes (Bound et al. 2000). Figure 2 presents the regression coefficients on dummies for month of birth, along with a 95-percent confidence interval, holding constant the other individual characteristics which potentially influenced placement. There is no systematic relationship between ethnic human capital and month of birth. One of the individual coefficients is close to being significant. But this is not surprising: even if ethnic human capital and birth month are randomly associated we would expect 1 of the 11 coefficients to be significant at the 9 percent level.

[9] Oreopoulos (2003) uses a similar argument to motivate why assignment to a public housing project can be considered exogenous for new recipients of welfare payments in Toronto.

Figure 2 Ethnic human capital in assigned location by month of birth



Notes: The figure shows estimates (solid line, 95 percent confidence interval given by dashed lines) from a linear regression of the share of highly educated in the ethnic community in the assigned location on a set of dummies for month of birth. The model also controls for gender, age at immigration, age of the mother, the educational attainment of the mother as well as the father, family size, country of birth fixed effects, neighborhood fixed effects, immigration year fixed effects, and graduation year fixed effects.

Given the institutional setting and the information documented in previous work, we think it is valid to assume that the assignment location is exogenous to the child, conditional on his/her observed characteristics. Note that this assumption is less strict than in, e.g., Edin et al. (2003), since child and parental ability are not perfectly correlated.[10]

Data

We use data from the IFAU-database (IFAU – Institute for Labour Market Policy Evaluation). The database contains detailed micro data covering the entire Swedish population aged 16–65 for each year during 1985–2004. The data originate from administrative registers maintained by Statistics Sweden.[11] The database contains information on, e.g., labor market status, educational history, income, taxes, and various demographic variables. An important feature of the data is that we can link each student to his/her parents and we are thereby able to include infor-

[10] Estimates of the intergenerational earnings correlation are typically much lower in Sweden than in the U.S. Corak (2006) reports "preferred" estimates for different countries: the estimate for Sweden is 0.27 compared to 0.47 for the U.S.

[11] The key registers are the income tax registers (*Inkomst- och taxeringsstatistiken*), population registers (*Registret för totalbefolkningen*), the register on educational attainment (*Utbildningsregistret*), the grade-9 register (*Årskurs-9 registret*), and the multi-generational register (*Flergenerationsregistret*).

mation on several parental characteristics. We define parental characteristics separately for each parent. From 1988 and onwards there is information on all final grades for students graduating from Swedish compulsory school.

Our main sample consists of the children of refugees whose parents obtained their residence permit between the years 1987 to 1991. These children may have graduated from compulsory school between 1988 and 2003. In general, the individuals were between 0 and 15 years of age at migration. We identify refugee immigrants by region of origin and exclude children who did not arrive together with the parent who first came to Sweden. The motivation for excluding these individuals is that they are likely to have immigrated because of family reunification reasons, and these immigrants were exempted from the placement policy.

In this paper we use SAMS (Small Area Market Statistics) areas to capture neighborhoods. SAMS areas are defined as homogenous areas in certain respects; it may be a homogenous area with certain types of buildings—high-rise buildings, owner-occupied housing, or business complexes, for instance. The SAMS are the smallest geographic unit available in Swedish data. Sweden has about 9,000 SAMS areas, which gives an average of 1,000 residents (of which about 600 are of working age). However, the average individual lives in an area with 1,849 inhabitants aged 16–65. Since the foreign-born are concentrated to urban areas it is not surprising to find that the average immigrant lives in a somewhat more populated area; the average immigrant lived in a SAMS area with 2,498 inhabitants aged 16–65.

Since individuals do not enter the database before age 16, we use the assignment location of the parent(s) who arrived together with the child to get information on the first SAMS area. We also measure the characteristics of the location observed in the individual's year of graduation. A potential problem is that we only observe the region of residence at the end of the year. If the observed initial location differ from the actual initial placement due to internal migration, this creates a measurement error in initial placement. This issue has been thoroughly investigated in Edin et al. (2003) where a weighting scheme based on aggregate data on municipal refugee reception from the Immigration Board was used. The estimates from the weighted regressions were very similar to the non-weighted ones, suggesting that this measurement error is not a big concern.

We have in mind a model where immigrant student outcomes are influenced by ethnic peers. Some peers may be more important than others, either because of their characteristics or because they are more likely to act/interact in relevant settings related to education. In the baseline model we include three variables: (i) (the log of) the number of "countrymen"

25–65 living in the SAMS area; (ii) (the log of) the fraction of the countrymen in the area with at least three years of upper secondary education; and (iii) (the log of) the fraction of the highly-educated countrymen in the area who are parents. The first variable is intended to measure the quantity of contacts, whereas the two other variables measure the “quality” or properties of these contacts. The main reason for defining peers in terms of the characteristics of the neighborhood rather than the characteristics of fellow students at school is that we do not observe school catchment area. Even though location determines school to a large degree (as discussed above), we would have to invoke some assumptions in order to determine with whom immigrant children would go to school. While we uniquely identify the initial neighborhood, it is clearly possible that, e.g., 75 percent of the children in that neighborhood attend one particular school while 25 percent attend another. Furthermore, to estimate the relevant fractions in one school or another we would have to use the behavior in preceding cohorts, which creates additional measurement difficulties. All in all, we decided not to pursue this strategy, mainly because the estimates based on neighborhood characteristics will capture the same phenomena.[12]

There are of course countless ways to measure peer variables. We have chosen a specification where the results are invariant to the precise segregation measure used; see Bertrand et al. (2000) on this point. We will focus on the size and characteristics of the individual’s own ethnic group as these effects are identified under a weaker set of assumptions than characteristics that vary only across neighborhoods. Nevertheless, we will also provide auxiliary estimates of the impact of immigrant concentration.

The outcome studied in this paper is the percentile rank (by graduation year) of the compulsory school GPA. Although not perfect, the GPA is the best widely available summary measure of compulsory school performance. Furthermore, it is the basis for admission and selection to upper secondary school.

A description of the sample

Table A.1 and Table A.2 provide some general descriptive statistics of the estimation sample, containing a total of 20,039 individuals. Not unexpectedly, outcomes are quite poor; the average percentile rank of the GPA is 40. The typical child migrant in the sample was 8 years of age

[12] There are roughly 1,500 schools from which the individuals could graduate, and 9,000 neighborhoods (SAMS areas). Thus, on average a school aggregates 6 neighborhoods. Studying the relative importance of peer influences at school and in the neighborhood is indeed an interesting topic, which we leave for future studies.

when he/she arrived in Sweden. There are slightly more boys in the sample (53–47) and mean sibship size is close to 3, which is relatively high by Swedish standards.

A fair share (16.5 percent) of the fathers is not present in the data. Among those observed, educational information is unavailable for about 11 (7.6) percent of the fathers (mothers). The observed distribution of education shows that about half the parents have only compulsory education. Thirty percent have some short or long high school, and approximately 20 percent have obtained education at the university level.

It is also clear that there is variation in region of origin. Iranians are the largest group, making up about a quarter of the sample. 16 percent originate in Northern Africa, 12 percent in Chile. About 20 percent of the individuals have arrived from different parts of Eastern Europe and the former USSR.

The descriptive statistics also show residential concentration among the studied refugees. There is substantial variation in the size of the SAMS population in the sample, but the average is higher than what is observed in the overall population, which is consistent with concentration to larger cities. The immigrant share in the neighborhood (at the time of graduation) is as high as 31 percent, which is much higher than in the overall population (12 percent). Concentration in the "ethnic" dimension is even stronger: on (a weighted) average, the groups studied constitute 0.6 percent of the working-age population, yet the average "ethnic" share in the neighborhood is 3.2 percent at the time of graduation.

Empirical results

We begin this section by presenting and outlining the empirical strategy. Then we turn to the baseline results relating outcomes at graduation to the characteristics of the assignment location. We present the average effects as well as separate estimates by certain observed characteristics (gender, parental education, and age at arrival). Finally, we discuss some robustness checks where we vary the specification of the baseline regression.

The empirical strategy

The purpose of the analysis is to study peer effects on school performance among refugee children. Our basic strategy is to use the exogenous variation in neighborhood properties induced by the placement policy and estimate variations of a model with the following structure:

$$y_{icst_1} = \beta_1 \ln N_{cst_0} + \beta_2 \ln n^e_{cst_0} + \beta_3 \ln n^{ek}_{cst_0} + \lambda_c + \lambda_s + \lambda_{t_0} + \lambda_{t_1} + \varepsilon_{icst_1}$$

where y_{icst_1} denotes the outcome (the percentile ranked GPA in this case) for individual i, originating from country c, who was assigned to SAMS area s, graduating from compulsory school at time t_1. We include fixed effects for country of origin, neighborhood, time of arrival (t_0) and time of graduation. The key neighborhood characteristics are denoted N_{cst_0}, $n^e_{cst_0}$, and $n^{ek}_{cst_0}$. N_{cst_0} is the number of adult individuals (excluding the parent(s) of the individual) from a particular source country residing in the neighborhood at the time of arrival. $n^e_{cst_0} = (N^e_{cst_0} / N_{cst_0})$ is the fraction of the adults who are highly educated; "high education" is defined as having at least three years of university-preparatory upper-secondary education. $n^{ek}_{cst_0} = (N^{ek}_{cst_0} / N^e_{cst_0})$ is the fraction of the highly-educated who have kids. For simplicity, individual background characteristics have been suppressed.

This specification provides a convenient test of what (and to some extent why) neighborhood characteristics are important. If $\beta_1 = \beta_2 = \beta_3$, then it is the number of highly educated parents that have an impact on student performance. A priori, this seems like a reasonable starting point, since then it is the educational background of the kids with whom the immigrant (potentially) interacts which is of importance. The configuration $\beta_3 = 0, \beta_1 = \beta_2$ may suggest that the neighborhood is important because all adults act as role models. In this case, it is the number of highly educated in the entire ethnic community that matters; there is no additional effect coming from the human capital of the parents. In general, β_2 measures the impact of increasing the human capital of the community while holding size constant, while β_1 gives the effect of increasing the size of the community (contact availability) while holding the educational composition constant. Notice that all neighborhood characteristics are dated at the time of immigration since this is the only time point when our conditional independence assumption holds.

This specification can be seen as a way of estimating the impact of the assignment location with a minimum of assumptions about the mechanisms at work. This specification can also be seen as a reduced form of a structural model where school performance is affected by cumulated peer influences since immigration (see Åslund and Fredriksson 2008 for further discussion).

However, even though the estimates of β_1, β_2, and β_3 are free of bias due to self-selection, it is likely that there are other properties of the assigned location besides those included explicitly in our model. To this end, we use a strategy initiated by Bertrand et al. (2000) and later used by Edin et al. (2003) and Åslund and Fredriksson (2008). Since the included characteristics vary across origin groups within locations (or conversely across locations within groups), we are able to include fixed effects for each SAMS area and for each region of origin. Thus, any common factor affecting the average performance in a neighborhood is accounted for in the analysis, and so are differences in average performance depending on region of origin. The specification thus implies that the estimate on, e.g., the size of the ethnic community should be interpreted as giving the effect of increasing the number of fellow countrymen while holding the immigrant share constant. Since there are potentially 9,000 neighborhood fixed effects, the specification is very flexible and the scope for within-neighborhood variation in omitted variables is limited. The vector of characteristics includes the subject's age at immigration, the mother's age, mother's and father's level of education, gender and family size.

A final word regarding the specification is that, since the neighborhood variables are entered in logs, we encounter some problems when there are no other fellow countrymen in the community. We deal with this issue by assigning an arbitrary low value for, e.g., the size of the ethnic community and then include a dummy variable that indicates no other fellow countrymen. Note that the inclusion of the dummy variable implies that the procedure of assigning arbitrary values to empty cells will not affect the estimate on the neighborhood characteristics. Further, the estimate on, e.g., the size of the community gives the effect of increasing the size of the community conditional on there being at least one person from one's own ethnic group in the neighborhood.

Baseline estimates

Table 1 presents the baseline results relating compulsory school GPA to neighborhood characteristics. The table only reports the results of main interest; the estimates on the other included characteristics are presented in Table A.3. These additional covariates exhibit the expected impact. Girls outperform boys by about 8 percentile ranks on average. Parental education has a substantial impact on outcomes: a university educated mother increases the percentile rank by over 11 points relative to a mother with compulsory education (the estimates on father's education have a similar flavor). There are substantial performance differences across birth regions and also patterns suggestive of worse outcomes in larger families, even though these patterns are weaker than what is sometimes found in the literature (Åslund and Grönqvist 2007).

Let us now turn to the estimates of the upper panel of Table 1, where school performance is related to the characteristics of the assigned neighborhood. Both the size and the educational attainment of the ethnic community have a positive impact on performance. There is no additional effect coming from the human capital of the parents. The latter result may be somewhat surprising, but can perhaps be interpreted as saying that the human capital of the ethnic community is important mainly because adults act as role-models.

The magnitudes involved suggest that a given change in the educational attainment of the ethnic community is almost twice as important as the size of the community. However, if the estimates are evaluated at the typical variation in the data they are about as important: one standard deviation changes in quality (education) and quantity (size of community) improves student performance by 0.9 percentile ranks. The effect of quantity is less precisely estimated (it is significant at the 10 percent level).

Since the human capital of the parents has no additional effect on student performance, we move on to the more parsimonious specification in column (2). The size of the coefficients is reduced somewhat but the level of human capital in the ethnic community remains statistically significant at the 10 percent level.

The interaction between quantity and quality may also matter, i.e., it may be more (or less) important to have high quality peers in a sizable community. Column (3) adds the interaction of the two variables to the specification. The point estimate on the interaction is insignificant, and therefore we drop this specification from here on.

The basic argument for approaching the issue of peer influences in the way we do instead of simply relating peer characteristics to outcomes in the overall population of graduates (possibly belonging to certain groups) is that we believe that residential sorting may bias the estimates. To illustrate the importance of sorting bias, Panel B of Table 1 presents results from models where peer characteristics are measured at the time of graduation. The results show that sorting bias is a concern for the estimate on the number of peers: the estimate is statistically significant and has the opposite sign compared to the corresponding one in Panel A. Sorting bias does not appear to affect the estimate on the educational composition of the ethnic community.

Table 1 The relationship between neighborhood characteristics and compulsory school grades

	Dependent variable: Percentile ranked GPA		
	(1)	(2)	(3)
Panel A. Year of arrival			
Size of ethnic community	.647*	.488	.491
	(.352)	(.332)	(.333)
Share with high education	1.141**	.987*	1.015*
	(.546)	(.533)	(.563)
Share of high-educated who are parents	–.209	--	--
	(.713)		
Interaction (size and share high-educated)	--	--	–.044
			(.309)
Panel B. Year of graduation			
Size of ethnic community	–.522**	–.532**	–.536**
	(.228)	(.210)	(.209)
Share with high education	1.256**	1.237**	1.244**
	(.566)	(.554)	(.554)
Share of high-educated who are parents	.295	--	--
	(.565)		
Interaction (size and share high-educated)	--	--	–.230
			(.290)
(Initial) SAMS FE:s	Yes	Yes	Yes
Ethnic group FE:s	Yes	Yes	Yes
Year of arrival FE:s	Yes	Yes	Yes
Year of graduation FE:s	Yes	Yes	Yes
Number of observations	20,039	20,039	20,039

Notes: The sample consists of refugee immigrants whose parents arrived during the period 1987−1991 and who completed compulsory school not later than 2003. Panel A displays estimates of neighborhood characteristics measured at the year of arrival. Panel B shows the corresponding estimates for the year of graduation. All regressions control linearly for the subject's and the mother's age, with dummies for each parent's educational attainment (five levels), family size, gender and missing values. Column (2) presents estimates where the coefficients are evaluated at the mean of the other variable. Standard errors robust for clustering at the SAMS*ethnic group level (5947 cells) in parentheses. ** = significant at 5 % level; * = significant at 10 % level.

We noted in the previous section that the studied refugees became more concentrated with time in Sweden. The size of the ethnic community in the neighborhood doubles between the time of arrival and the time of graduation. The results in Table 1 imply that it is primarily less-skilled families (in the unobserved sense) that relocate to neighborhoods where ethnic concentration is higher. This pattern is similar to the findings of Edin et al (2003), who also conclude that sorting inflicts a negative bias on the estimate on the number of peer contacts. Note that we arrive at this conclusion despite having very flexible controls for neighborhood and region of origin.

Analyses by subgroups

We have re-estimated the baseline model of column (2) in Table 1 for some demographic subgroups; the results are presented in Table 2. The first division examines if the effects vary by gender. According to the estimates, boys (who perform poorly in school) are significantly influenced by the number of peers, whereas girls are not.

A similar pattern is available in columns (3) and (4), where the size of the community has a positive and significant for children from “non-academic” families (who perform poorly in school). The effects of the human capital of the ethnic community do not vary by gender and educational background.

The differential effects of the size of the peer group are interesting and shed some light on the sorting pattern in our data. Boys and children with a less-educated family background perform worse than average in school. The observed determinants of school outcomes are, arguably, positively associated with the unobserved factors determining school performance. The results presented in columns (1) to (4) thus suggest that it may be beneficial for students from weak backgrounds to sort themselves into ethnic communities, which is also the sorting pattern implied by the results in Table 1.

In columns (5) and (6) the sample is split by age at migration. The assignment neighborhood characteristics are only important for children arriving before age seven. This could be interpreted in two ways. First, it could be that skills are shaped at low ages (cf. Cunha and Heckman, 2007). And, second, the estimates could reflect a cumulative effect of peer contacts. Arriving at a young age arguably means longer exposure to the environment captured by the included variable, and thereby a higher treatment dose.

Table 2 Differential effects with respect to background characteristics.

	By gender		By parental education		By age at immigration	
	Boy	Girl	Academic family	Non-Academic family	Up until age seven	After age seven
	(1)	(2)	(3)	(4)	(5)	(6)
Size of ethnic community	1.279**	–.441	–.121	.946*	1.284**	–.543
	(.441)	(.507)	(.530)	(.499)	(.503)	(.457)
Share high educated	1.358**	1.091	1.521	1.169	1.903**	–.514
	(.690)	(.786)	(.999)	(.757)	(.819)	(.719)
(Initial) SAMS FE:s	Yes	Yes	Yes	Yes	Yes	Yes
Ethnic group FE:s	Yes	Yes	Yes	Yes	Yes	Yes
Year of arrival FE:s	Yes	Yes	Yes	Yes	Yes	Yes
Year of graduation FE:s	Yes	Yes	Yes	Yes	Yes	Yes
Mean (sd) of the dependent variable	36.60	44.78	48.13	33.67	44.08	37.01
	(26.86)	(28.54)	(28.52)	(25.63)	(28.27)	(27.22)
Number of observations	10,598	9,441	9,407	10,632	9,767	10,272

Notes: The sample consists of refugee immigrants who arrived during the period 1987−1991 and completed compulsory school not later than 2003. Where appropriate, the regressions control linearly for the subject's and the mother's age, with dummies for each parent's educational attainment (five levels), family size, gender and missing values. Standard errors robust for clustering at the SAMS*ethnic group level (5,947 cells) in parentheses. "Academic family" is defined as having at least one parent who has completed at least university preparatory upper-secondary school. ** = significant at 5 % level; * = significant at 10 % level.

Robustness checks

We have performed a number of robustness checks to investigate whether our results are sensitive to changes in sample composition, specification or outcome measure. In this section we discuss the results from these exercises.

One concern is that neighborhood effects may be non-linear. For instance, the effect of living in an ethnic enclave might matter more for individuals residing in very highly segregated areas. To examine this we ran regressions including quadratic terms for our key variables of interest. It turns out that the estimates on the non-linear terms are not significantly different from zero.

Another concern is that small source countries have been aggregated for confidentiality reasons in our data. Treating such regions as a single "country" obviously introduces measurement error in our analysis. We therefore re-estimated our models for individuals for whom we can uniquely identify country of origin. The coefficients are of the same magnitude but somewhat less precisely estimated. This is to be expected considering that our sample size is reduced by almost 50 percent.

We also experimented with alternative outcome variables. One relevant question is whether segregation influences host country language skills. We have therefore run regressions where the outcome is grade in

Swedish.[13] The results (not reported) suggest that the effects of ethnic peers are similar for Swedish grades as for grades in general.

Finally, we have investigated to what extent ethnic concentration affects the probability to finish school on schedule. In fact, 22 percent of our sample finish 9th grade later than "normal". It turns out that these estimates are very imprecise. There is no strong evidence that peer characteristics influence the probability to graduate in time.

The impact of the overall immigrant population

The neighborhood fixed effects included in the baseline model are important to isolate the impact of ethnic peers. A disadvantage is however that they exclude the possibility to study the impact of e.g. the neighborhood immigrant share or the educational level of natives in the area. Table 3 is an attempt to shed some light on the broader question on the importance of the characteristics of the immigrant (and native) community. For purposes of identification, we replace the neighborhood fixed effects by municipality fixed effects, implying that identification of the characteristics of the immigrant community comes from comparisons across neighborhoods within municipality.

We proceed in steps: first we introduce the size of the ethnic community using SAMS fixed effects (column 2); then we add the size of the immigrant community while holding the overall population size of the neighborhood constant (column 3). Thirdly, we add the educational composition of each community respectively (column 4). For ease of reference, column (1) reproduces the results of column (2) from Table 1.

[13] These estimates should be interpreted cautiously since immigrant students are allowed to choose between two different tracks: a standard track and a special track for immigrants. This introduces a potential selection problem; however, we find no evidence suggesting that the ethnic network affects the choice of track.

Table 3 The impact of the characteristics of the overall immigrant community

	Dependent variable: Percentile ranked GPA			
Year of arrival	(1)	(2)	(3)	(4)
Ethnic community				
Size of community	.488	.570*	.646**	.620**
	(.332)	(.271)	(.247)	(.282)
Share high educated	.987*	--	--	.484
	(.533)			(.460)
Immigrant community				
Size of community		--	−1.034**	−.725
			(.524)	(.596)
Share high educated			--	−.640
				(.899)
Native community				
Share high educated		--	--	1.470*
				(.864)
Overall population size			.879	.715
			(.554)	(.597)
(Initial) SAMS FE:s	Yes	Yes	No	No
(Initial) Municipality FE:s	No	No	Yes	Yes
Ethnic group FE:s	Yes	Yes	Yes	Yes
Year of arrival FE:s	Yes	Yes	Yes	Yes
Year of graduation FE:s	Yes	Yes	Yes	Yes
Number of observations	20,039	20,039	20,039	20,039

Notes: The sample consists of refugee immigrants whose parents arrived during the period 1987−1991 and completed compulsory school not later than 2003. All regressions control for the subject's age at immigration and the mother's age, dummies for each parent's educational attainment (five levels), family size, gender and missing values. Standard errors are robust for clustering at the SAMS*ethnic group level (5,947 cells) in parentheses. ** = significant at 5 % level; * = significant at 10 % level.

The interpretation of the estimates relies on the assumption that we are able to include everything that is relevant about the neighborhood. The fact that the coefficient on ethnic human capital in column (4) is half that of the estimate in (1), signals that caution is called for. Note, on the other hand, that the point estimate on the size of the ethnic community is positive and of similar magnitude in all three columns, suggesting that we do handle at least part of the bias due to residential sorting.

Taken at face value, columns (2) and (3) suggest a positive impact a larger ethnic community, similar to the baseline estimates. By contrast, according to column (3) there is a negative effect of expanding the immigrant community. The quantity parameters in column (4) are quite similar to those in column (3). The "quality" parameters suggest that a generally well-educated environment (as seen in the estimate on the fraction of highly educated natives) is more important than the educational level in one's ethnic group.

Concluding remarks

This paper studies peer effects in compulsory school performance among immigrant children in Sweden. To handle self-sorting in the residential market, the analysis uses a governmental refugee placement policy in place in the late 1980s and early 1990s.

The results show that peers matter. A higher level of education among fellow countrymen in the assigned neighborhood affects grades in a positive direction. A one standard deviation increase in the fraction of highly educated peers raises student performance by 0.9 percentile ranks. A standard deviation increase in the size of the ethnic community has about the same effect, but the effect is less precisely estimated.

Is this a small or large magnitude? At first glance, it may seem small relative to the importance of individual or family characteristics. For instance, it corresponds roughly to a tenth of the grade difference between refugee immigrants and the native-born in our data. But we think it would be a mistake to conclude that the characteristics of the neighborhood are largely irrelevant.

Whether the magnitudes involved should be interpreted as small or large depends on the "true" structural model relating student performance to neighborhood or peer characteristics. Any human capital model would imply that the entire history of peer characteristics is relevant. In our setting, the majority of the families (some 75 percent) escaped "treatment" by moving out of the assigned neighborhoods. Under reasonable assumptions, this implies that our estimates on initial neighborhood characteristics are lower bounds on the true effects in the structural human capital model.

We have also presented some evidence on the importance of handling the problems associated with residential sorting in studies relating contextual variables to individual outcomes. Like some previous studies on adult migrants (Edin et al. 2003, Åslund and Fredriksson 2008), we find that one is likely to infer—erroneously—that the number of peer contacts has a negative effect on school performance if sorting bias is not addressed appropriately. Our analysis of heterogeneous effects reveals an interesting pattern. Disadvantaged students/families gain more by having many peers around than other students/families. And it is also these families that move to ethnically concentrated areas. The sorting pattern thus appears to be rational from the point of view of the disadvantaged groups.

Our baseline estimates answer questions concerning the impact of varying the size and characteristics of one's own ethnic group holding the other characteristics of the neighborhood constant. We have also presented an attempt to study the broader issue of immigrant segregation and contextual influences on academic performance. These estimates could be plagued by omitted variable bias; taken at face value, however, they indicate the complexity of the mechanisms at work. An immigrant-dense environment may have a negative impact on student performance – since, e.g., it may be negative for learning the host country language – but, given the size of the immigrant community, positive forces may arise through the links in the ethnic community. Also, the general education level of one's surrounding may be more important than the presence of highly educated ethnic peers. While this analysis is tentative it raises interesting questions. Establishing what lies behind these estimate is an important area for further study.

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Appendix

Table A.1 Summary statistics

Variable	Mean	Standard deviation
Subject:		
GPA (percentile rank)	40.45	27.96
Age (in 2003)	21.95	3.84
Age at immigration	8.00	3.8
Female	.47	.50
Sibship size	2.99	1.56
Mother:		
Age (in 2003)	47.38	6.39
Education: Compulsory school	.50	.50
Upper secondary school ≤ 2 years	.14	.34
Upper secondary school > 2 years	.17	.38
University ≤ 2 years	.11	.31
University > 2 years	.08	.28
Father:		
Age (in 2003)	51.48	6.99
Education: Compulsory school	.42	.49
Upper secondary school ≤ 2 years	.14	.35
Upper secondary school > 2 years,	.17	.38
University ≤ 2 years	.12	.33
University > 2 years	.15	.35
Regional characteristics: Year of arrival		
Share high-educated in own group	34%	
Share high-educated in immigrant group	31%	
"Ethnic" concentration	1.6%	
Immigrant concentration	19%	
Population size	1528	
ln(share high-educated in own group)	–.778	.758
ln(size of ethnic community)	2.142	1.445
Regional characteristics: Year of graduation		
Share high-educated in own group	39%	
Share high-educated in immigrant group	38%	
"Ethnic" concentration	3.2%	
Immigrant concentration	31%	
Population size	2012	

Notes: The regional characteristics are defined with respect to the adult population aged 25-65. Summary statistics for each parent's educational attainment is conditional on having found this information in the records.

Table A.2 Region of birth

Region of birth	Percent of sample
1. Former Yugoslavia	5.2
2. Poland	5.5
3. The Baltic states (Estonia, Latvia, Lithuania)	0.3
4. Eastern Europe 1 (Rumania, The former USSR, Bulgaria, Albania)	6.0
5. Eastern Europe 2 (Hungary, The former Czechoslovakia)	2.4
6. Mexico and Central America (El Salvador, Mexico Other countries)	1.6
7. Chile	13.3
8. Other South America (Peru, Brazil, Colombia, Argentina, Uruguay, Other countries)	2.0
9. African Horn (Ethiopia, Somalia, Sudan, Djibouti)	5.0
10. North Africa (Arabic countries) and Middle East (Lebanon, Syria, Morocco, Tunisia, Egypt, Algeria, Israel, Palestine, Jordan, Other countries)	17.8
11. Other Africa (Gambia, Uganda, Zaire Ghana, Other countries)	1.1
12. Iran	25.5
13. Iraq	4.8
14. Turkey	3.8
15. South East Asia (Vietnam, Thailand, the Philippines, Malaysia, Laos Other countries)	3.9
16. Other Asia (Sri Lanka, Bangladesh, India, Afghanistan, Pakistan)	1.7
Total	100

Table A.3 Estimates on individual characteristics for specification in Table 1, column (1)

	Dependent variable: Percentile ranked GPA
Individual characteristics:	
Female	8.136**
	(.396)
Age at immigration	–4.698**
	(.458)
Mother characteristics: Age	.124**
	(.043)
Education: Compulsory school	--
High school ≤ 2 years	4.687**
	(.854)
High school > 2 years	5.830**
	(.783)
University ≤ 2 years	11.295**
	(.958)
University > 2 years	13.519**
	(1.110)
Missing education	.688
	(1.004)
Father characteristics: Missing father	1.237
	(1.057)
Education: Compulsory school	--
High school ≤ 2 years	3.494**
	(.907)
High school > 2 years	3.460**
	(.848)
University ≤ 2 years	8.077**
	(.941)
University > 2 years	11.705**
	(0.967)
Missing education	-1.850*
	(.997)
Number of observations	20,039
R-squared	0.336

Notes: Estimates on individual characteristics for the specification in Table 1, column (1). The sample consists of refugee immigrants whose parents arrived during the period 1987−1991 and completed compulsory school not later than 2003. The regressions control with dummies for family size, (initial) SAMS, Ethnic group, Year of arrival, Year of graduation. The regression also controls for the regional characteristics listed in Table 1, column (1) and indicator variables controlling for the SAMS*(ethnic group) "cell" having no observations. Standard errors are robust for clustering at the SAMS*ethnic group level (5947 cells) in parentheses. ** = significant at 5 % level; * = significant at 10 % level.

Essay 4: Residential segregation and minority health: Evidence from population micro data[****]

Introduction

Racial and ethnic disparities in health are large and well documented (e.g. Loue 1998). In the US, African-Americans are twice as likely as white Americans to die from heart disease and 34 percent more likely to die from cancer. In Sweden, the incidence of heart disease is in many immigrant groups up to 50 percent higher than that of natives, and immigrants are 27 percent more likely to suffer from mental disorders (Swedish National Institute of Public Health 2002). The fact that some of these differences remain even after adjusting for individual background characteristics has motivated social scientists to look elsewhere for possible explanations. Several recent studies claim that residential segregation could be one reason and indeed show empirical support of an adverse relationship between segregation and health (e.g. Acevado-Garcia and Lochner 2001; Chang 2006; Eschbach et al. 2004; Gould 2000; LeClere 1997; Mellor and Milyor 2004). In fact, Williams and Collins (2001) go as far as to state that residential segregation is "a fundamental cause of racial disparities in health".

The purpose of this paper is to investigate the consequences of residential segregation for immigrants' health. To this end, I make use of a rich longitudinal dataset collected from administrative registers covering the entire Swedish working-age population. The dataset contains annual information on the exact diagnosis for all individuals admitted to Swedish hospitals from 1987 to 2004, as well as a wide range of standard individual characteristics.

There are several arguments for why segregation can affect health. For instance, segregation could reduce the cost of information sharing, and

[****] I am grateful to Per Johansson for generously sharing his data and to Staffan Khan for data preparations. I acknowledge helpful comments by Olof Åslund, Janet Currie, Per-Anders Edin, Mikael Elinder, Erik Glans, Bertil Holmlund, Emilia Simeonova, Roope Uusitalo and audiences at the American Economic Association Annual Meetings 2009 (San Francisco), the Annual Swedish Integration Research Network Conference (Växjö), and Uppsala University.

thereby facilitate individuals' ability to invest in health. Segregation may also affect health investments through its potential impact on income and prices. There is also a discussion that social interactions within a spatially concentrated network can influence health related attitudes and norms, e.g. the value of medical check-ups. Since many of the mechanisms can work in either direction, the net effect of segregation on health is an empirical question.

Identifying the causal link between segregation and health is difficult since residential location is a choice variable. If individuals sort across residential areas based on unobserved characteristics related to health, the estimates will be biased. Most previous studies attempt to deal with this issue by controlling for potential confounders but it is far from certain whether this approach really renders a consistent estimate of the parameter of interest.[1]

I address the selection problem using a Swedish refugee placement policy where authorities during the years 1987–1991 assigned newly arrived refugees to their initial location of residence. The policy was implemented in a way that makes initial location independent of unobserved individual characteristics. The arguments for considering placement as exogenous with respect to the unobserved characteristics of the individual are the following: (i) the individual could not choose his/her first place of residence due the institutional setup and to the practical limitations imposed by scarce housing; (ii) there was no direct interaction between local placement officers and individual refugees, meaning that any selection must have occurred on observed characteristics. This plausibly exogenous source of variation in location is exploited by estimating models relating health to initial segregation and by instrumenting for individuals' long-term exposure to segregation.

The paper makes several contributions to the literature. First, it represents one of the first attempts to investigate the effect of segregation on immigrants' health. The exceptionally rich dataset in combination with plausibly exogenous variation in segregation makes it possible to explore the question in much greater detail than what previously has been feasible. The dataset also makes it possible to examine some of the mechanisms through which segregation can affect health, e.g. income and stress. Since many countries have implemented similar policies aimed at influencing the settlement decisions of newly arrived immigrants, knowl-

[1] One exception is Gould (2000) who studies the consequences of racial segregation for birth-weight using government structure at the metropolitan level as instruments for segregation. She finds that increased levels of segregation leads to lower birth-weight of children to black mothers.

edge of the relationship between residential location and health becomes highly policy relevant.[2]

The results can briefly be summarized as follows. The OLS estimates show statistically significant evidence of an adverse correlation between segregation (at the parish level) and the risk of being hospitalized. For instance, a one standard deviation increase in segregation is associated with a rise in the likelihood of an immigrant being admitted to hospital by about 6 percent. Similar results are documented for different subgroups of the population. In contrast to most previous studies, estimates that account for omitted variable bias are however in general not statistically significant. The results are robust to several sensitivity checks.

Background

Why segregation can affect health

Theory suggests several reasons for why residential segregation can influence the health outcomes of minorities.[3] One of the most common arguments is that segregated areas are more likely to be located in regions with poor access to health care, and/or offers health care of lower quality (e.g. Chandra and Skinner 2004). This potential channel is however not a direct effect of segregation and merely states that segregation and the quality of health care can be correlated.

Other possible mechanisms work through health investments. Grossman (2000) provides a simple framework for understanding this channel. In the model, individuals value their own health as well as their family members. Each individual has an initial endowment of health which increases through investments (e.g. buying healthier food) and depreciates in the absence of investments. Individuals are also assumed to consume other goods not related to health and are subject to their budget constraints.

The model suggests two potential mechanisms through which segregation could influence health: incomes and prices. To date, a large body of research has demonstrated that residential segregation adversely affect earnings, welfare dependence, unemployment, and educational attainment (e.g., Bertrand, Luttmer and Mullainathan 2000; Borjas 2000; Cut-

[2] Similar policies are currently active (or have recently been) in, e.g., the US, Denmark, Germany and the Netherlands (Edin, Fredriksson and Åslund 2004).

[3] Currie (2008) provides a general discussion of the relationship between health and residential location.

ler and Glaeser 1997; Wilson 1987).[4] Obviously, an individual's incomes directly restrict the amount of health inputs he/she can purchase. Additionally, education could affect individuals' ability to combine various inputs to produce health (cf. Cutler and Lleras-Muney 2008). Segregation potentially also lowers the prices of certain "ethnic goods" by facilitating trade in goods and services (e.g. Lazear 1999).

The literature also discusses the role of peers and social networks. The idea is that interactions within one's social network can influence behavior through attitudes, values and norms, and that this influence grows with the size of the network (cf. Currie and Aizer 2004; Ichino and Maggi 2000). Examples include attitudes towards smoking and alcohol habits, vitamin consumption, medical check-ups, and physical activity. Furthermore, since the cost of information sharing arguably is lower when interacting with linguistically and culturally similar individuals, the network potentially serves to distribute information about e.g. the health care system. Last, it has been proposed that social interactions within a disadvantaged network could cause stress, which in the long run deteriorates health. This is has been called the theory of allostatic load (McEven and Stellar 1993).

Related studies

The relationship between segregation and health has received sparse attention in the empirical literature. Most of the existing studies are by epidemiologists and focus on the link between racial segregation and mortality. One of the earliest examples is Yankauer (1950) who finds that black and white infant mortality rates were higher in the more segregated residential areas of New York City. Several subsequent studies document similar results (e.g. LaVeist 1989; Polednak 1996). Adult mortality has been examined as well, and the evidence shows that segregation is positively correlated to black mortality rates (Collins and Williams 1999; Hart, Kunitz, Sell, and Mukamel 1998; Leclere, Rogers, and Peters 1997). A few papers also find an adverse relationship between segregation and blacks' self-rated health (e.g. Subramanian, Acevedo-Garcia, and Osypuk, 2005).

Evidence on the consequences of residential segregation for immigrants' health is even scarcer. Huie, Hummer and Rogers (2002) and Lee and Ferraro (2002) find that segregation seems to lower the mortality

[4] Even though most studies find a negative relationship between segregation and minorities' socioeconomic outcomes it should be noted that the evidence is mixed. For instance, Edin et al. (2003) show that living in an ethnic enclave improves labor market outcomes of less skilled immigrants.

rates of Puerto Rican and Mexican American adults, and improve the self-rated health of Mexican immigrants.

Although the abovementioned studies provide suggestive evidence on a link between segregation and health it should be noted that most studies suffer from problems generated by omitted variables as well as small and unrepresentative samples.

Related to segregation is a large literature on general neighborhood effects showing that neighborhood characteristics in many cases significantly predict health outcomes. For instance, Kling and Vortuba (2004) find that the mortality rates of male youth participating in the Gautreaux Assisted Housing Program operating in Chicago from 1978 to 1998 are significantly negatively correlated to neighborhood characteristics related to human capital and work. Kling, Liebman and Katz (2007) investigate the consequences of randomly assigning housing voucher offers (the Moving to Opportunity Program) to mainly disadvantaged families living in high poverty public housing. They find evidence of mental health benefits of the voucher offers for adults and for female youth. Beneficial effects for female youth on physical health were, however, offset by adverse effects for male youth.

Migration to Sweden and the settlement policy [5]

Sweden's immigrant population is quite large. In 2004, 13 percent of the 9 million residents were foreign-born. This number can be compared to the US where about 12 percent of the population is foreign-born. Since the 1970s the majority of migrants arriving are either refugees or family reunification immigrants. The economic performance of the migrants has been trending downwards over the past decades. In fact, among the OECD countries, Sweden now has one of the largest immigrant-native differentials in the labor market (OECD 2007).

As in most industrialized countries, there is a stark geographic concentration of immigrants to the urban areas. The largest Swedish cities of Stockholm, Göteborg and Malmö host about one third of the overall population and almost fifty percent of the foreign-born population. There is also a tendency for immigrants to cluster spatially within the urban areas, usually to the suburbs.

In order to reduce the geographic concentration of immigrants, the Swedish government gave in 1985 the Immigration Board the task of assigning new refugee immigrants to an initial municipality of residence. The decision was a reaction to complaints from cities which had experienced a rise in immigration. The cities argued that the increased immigra-

[5] This section draws heavily on Åslund et al. (2008).

tion was a burden on local public services. By placing asylum seekers in municipalities that had suitable characteristics for reception the government intended to improve the reception process.

Because of the large inflow of asylum seekers in the late 1980s, the number of receiving municipalities was increased from 60 to include 277 of Sweden's 284 municipalities in 1989. The original idea was to put people in locations with good opportunities for providing work or education. However, during the period the housing market was booming so available housing essentially determined the placement. The policy was formally active 1985–1994, but implementation was strictest between 1987 and 1991. During this period the placement rate was close to 90 percent, and since the individuals involved were given very little room to choose the initial municipality of residence I focus my analysis on this period.

After having arrived, asylum seekers were placed in refugee centers, while waiting for the authority's decision. The centers were distributed all over Sweden, and there was no connection between the port of entry to Sweden and the location of the center. On average, the wait for a permit varied between three and twelve months. After admission, municipal placement usually occurred within a shorter time period. Refugee preferences were considered in the municipal assignment, but most individuals applied for residence in the largest cities, where vacancies were very few, implying that there was in practice very little room for individual preferences. Assigning a refugee to a municipality was conditional on having found a vacant apartment within that particular municipality. Thus, assignment was in practice to a neighborhood within the municipality. Subsequent to having been assigned to an apartment, immigrants were free to move. Aside from relocation costs, moving only implied delayed enrolment in language courses.

The Swedish health care system[6]

In Sweden, the local county councils and municipalities serve as the major financiers and providers of health care and the vast majority of hospitals are public. Each county council is obliged to give its residents equal access to high quality health services and medical care. Counties and municipalities contract private providers but health care is mostly financed through local taxes. The county councils set their own patient fees but there is a uniform national ceiling on the total amount that a patient pays during a 12-month period (out-of-pocket). Thus, patient fees only

[6] This brief outline of the Swedish health care system draws on the Swedish Association of Local Authorities and Regions (2005).

account for about 3 percent of the total revenues. In general, the daily fee for staying in a hospital is about USD 12.

There is free choice of provider but referral is required in some cases, particularly in the cases of patients seeking highly specialized care, or if the patient chooses care in another county. In their contacts with health care providers, immigrants have the right to an interpreter free of charge.

Empirical strategy

The purpose of this paper is to estimate the effect of residential segregation on immigrants' health. To illustrate the methodological problems associated with this question consider the following model

$$Health_{ijk} = Segr_{jk}\beta_1 + X_i'\beta_2 + v_{ijk} \quad (1)$$

where the health status of subject i from ethnic group j residing in region k is a function of some measure of segregation, $Segr_{jk}$, and a vector of standard individual characteristics X_i. This specification is typically used in previous studies (e.g. Chang 2006). The main concern with this model is that segregation is likely to be correlated to a range of different variables that also related to health, e.g. socioeconomic background; something that will lead to biased estimates of β_1. Although it is possible to condition out some of these variables in rich datasets it is far from certain whether this approach really controls for all factors that could matter.

To address this concern I exploit the Swedish refugee placement policy, described earlier, where authorities assigned newly arrived refugees to their initial location of residence. Several studies have used the same identification strategy in examining the relationship between neighborhoods and immigrants' economic outcomes (e.g. Edin, Fredriksson and Åslund 2003; Åslund and Fredriksson 2008; Åslund and Rooth 2007; Dahlberg and Edmark 2008; Åslund, Edin, Fredriksson and Grönqvist 2008). Åslund and Fredriksson (2008) provide the perhaps most comprehensive description of the policy.

The rationale for considering placement as exogenous with respect to the unobserved characteristics of the individual are the following: (i) the individual could not choose his/her first place of residence due to the institutional setup and to the practical limitations imposed by scarce housing; (ii) there was no direct interaction between local placement officers and individual refugees, meaning that any selection must have occurred on observed characteristics.

Previous studies have documented the policy actually did create a geographic distribution that was independent of unobserved individual characteristics. Edin et al. (2003) show that the overall geographic distribution of those subjected to the placement policy differed from the location choices made by migrants arriving from the same regions shortly before the reform. Additionally, Åslund, Östh and Zenou (2006) show that the initial characteristics of the assigned locations differed pre/post reform, but that after 9–10 years in Sweden the sorting pattern of those who arrived under the placement policy came to resemble that of other migrants. Similar results can also be found in Åslund et al. (2008). Taken together these findings clearly indicate that people were not able to realize their preferred option. [7] [8] Based on the institutional setting, the information documented in previous work and the observations made in different data sets, I find it rational to treat the assignment location as exogenous, conditional on observed characteristics.

My identification strategy takes advantage of the exogenous variation in segregation in the assigned parish by estimating the following baseline equation

$$Health_{ijkt} = Segr_{jkt_0}\gamma_1 + X_i'\gamma_2 + \lambda_j + \lambda_k + \lambda_{t_0} + m_{ijkt} \qquad (2)$$

where the λ's represent fixed effects for ethnic origin (j), parish (k), and year of arrival (t_0). X_i is a vector of individual characteristics controlling for: age, gender, marital status, educational attainment (6 levels), and number of children. $Segr_{jkt_0}$ is dated at the time of immigration since this is the only point in time when segregation is (conditionally) independently determined. By using the fact that segregation varies across origin groups within parishes, or conversely across parishes within origin groups, the model controls for the possibility that there may be other properties of the initial location that matters for immigrants' health.[9] Hence, any common factors affecting the average health in a parish are accounted for in the analysis, e.g. the quality of the local health care, and

[7] Note that the timing of the receipt of the residence permit must coincide perfectly with the arrival of a housing vacancy in the preferred location, if the immigrant was to realize his or her most preferred option. The joint probability of these two events happening at the same time must be considered extremely low (cf. Oreopoulus 2003).

[8] Stricter tests of conditional independence are difficult since they require that there is a characteristic unobserved or at least unexploited by placement officers but correlated with the unobserved ability of the individual. In an attempt along these lines, Åslund and Fredriksson (2007) and Åslund et al. (2008) study whether month of birth—which is often claimed to be related to outcomes in several dimensions (Bound et al 2000)—correlated with the properties of the assignment location. They find no such association.

[9] This approach was initiated by Bertrand et al. (2000) and later used by Edin et al. (2003).

so are ethnic health differentials. All outcomes are observed in 2004. The specification can thus be considered as a way of capturing the long-term consequences of segregation on health, where initial segregation proxy for individuals' actual exposure.

Data and sample selections

The empirical analysis uses micro data from administrative registers. The dataset, collected by Statistics Sweden, covers the entire Swedish population age 16–65 (16–74) during the period 1987–2000 (2001–2004), and contains annual information on a wide range of labor market, educational and demographic characteristics. Information on diagnoses has been added from the National Board of Health and Welfare's registers and covers all completed admissions to public hospitals from 1987 to 1996. From 1997 the register also includes admissions to private health care. In order for a patient to be included in the register he/she must have been admitted to the hospital, meaning that the data do not cover medical treatments occurring in direct connection to a visit to a clinic. However, from 2002 the data also cover outpatient medical contacts in the specialized care, i.e. shorter visits (not requiring admission) to doctors that provide specialized care. Because of this reason I choose to focus my analysis on the latest years.

Diagnoses are classified according to the World Health Organization's International Statistical Classification of Diseases and Related Health Problems (ICD). ICD is a four digit coding of diseases and signs, symptoms, abnormal findings, complaints, and external causes of injury or diseases. The underreporting (conditional on having been admitted to hospital) is very low and is estimated to be less than one percent per year.

The analysis focuses on several of the most common diseases: cancer, heart disease, mental health problems, respiratory diseases, cerebrovascular diseases (stroke), illnesses related to pregnancy, depression, and diabetes. This classification is largely taken from Kuhn, Lalive and Zweimüller (2007) and is based on the 9th round of ICD (ICD-9). Table A.1. displays the different types of diagnoses and the way they have been aggregated.

My main sample contains immigrants arriving from refugee sending source countries during the period 1987 to 1991. For this sample, I include standard individual background characteristics such as: highest completed level of education, family size, gender, marital status, age, and year of immigration. Refugees are identified by "country" of origin. Table A.2. lists the included source countries. Note that the smallest countries have been aggregated due to confidentiality reasons.[10]

There are many ways to measure segregation and ethnic concentration. I choose one of the most commonly used measures: the Relative Clustering Index (RCI) (e.g. Bertrand et al. 2000; Borjas 2000; Massey and Denton 1988).[11] The index can be written as

$$\mathrm{RCI}_{\mathrm{jkt}} = \left(\frac{Group_{kt}^{j}}{Pop_{kt}}\right) \Big/ \left(\frac{Group_{t}^{j}}{Pop_{t}}\right)$$

where $Group_{kt}^{j}$ is the number of individuals from ethnic group *j* living in parish *k* in year *t*; Pop_{kt} is the total population in the same parish-by-year cell; $Group_{t}^{j}$ denotes the total number of co-ethnics in the overall country in year *t*; Pop_{t} is the total population residing in Sweden year *t*. Thus, for a given year, the index relates the proportion of co-ethnics in the parish to the proportion of co-ethnics in the country and consequently represents a measure of the relative concentration of co-ethnics. The major advantage of using this measure instead of simply taking proportions is that it does not underweight ethnic groups which are small in the overall country and therefore would never constitute a large fraction of any parish.[12] In terms of population size, the average parish contains about 4,500 individuals, making it comparable to the US census tracts. Table A.3. displays descriptive statistics for this and other selected variables.

Using hospitalizations as a measure of health

One potential problem is that diagnoses (in some cases) only are recorded for hospitalized individuals. Even though there is increased coverage in the data of outpatient medical contacts at the time when I observe the outcomes one might still be worried that the likelihood of being admitted

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[10] One concern is that aggregating dissimilar countries introduces measurement error in segregation. As a robustness check in the subsequent analysis, I only include uniquely identified source countries and show that the results are insensitive to the aggregation.

[11] As argued by Massey and Deanton (1988) segregation can be measured along several dimensions. This analysis focuses on ethnic concentration. In practice, different segregation indices tend to be strongly correlated (cf. Echenique and Fryer 2007).

[12] A possible concern is that the observed initial location (recorded in the end of the year) may differ from the actual initial placement because of internal migration. Edin et al. (2003) examine this issue carefully by using a weighting scheme based on aggregate data on municipal refugee reception from the Immigration Board. Their weighted regressions show no evidence that measurement error in initial location leads to biased estimates.

to hospital, conditional on an individual's true health status, can be correlated to segregation. In this case, diagnoses based on hospitalizations may not provide an accurate measure of health. In addition, within the pool of equally sick individuals there may be a selection of who actually seeks medical care. If this process if correlated with segregation, there is a similar problem.

There are two arguments against that this is a first-order problem. First, the Swedish health care policy calls for local county councils to provide its residents with equal access to inexpensive high quality health services and medical care, something that is likely to weaken the incentives for differential selection into medical care. Second, the fixed effect estimation will account for any differences in the average quality of the local health care, as well as the possibility that the health care system at a national level might favor different ethnic groups, or the fact that the propensity of seeking medical care can vary across ethnicities and localities.[13]

In the empirical analysis I present several pieces of evidence supporting the view that differential selection into medical care is not likely to be a problem.

Empirical analysis

This section examines the effect of segregation on immigrants' health. The baseline specification, given by equation (2), relates health status in 2004 to segregation in the assigned parish. As previously mentioned, this specification can be interpreted as a way of capturing the long-term consequences of segregation on health.

The dependent variable used in the estimations is a dummy taking the value 1 if the individual has been admitted to hospital and 0 otherwise.[14] Note however that a probit model produces coefficients which (when evaluated at mean value) are very close to the OLS estimates. I have also run regression using number of admissions as dependent variable with similar results.

The regressions control for age (third-order polynomial), number of children, gender, year of immigration, marital status, educational attain-

[13] Using US data Simeonova (2007) shows that: (i) the quality of clinics or doctors is not the underlying reason for racial differences in black and white mortality; (ii) that minorities and whites have access to similar physician quality; (iii) and that doctors treat patient similarly regardless of race.

[14] To be correct, since the data cover outpatient encounters with the medical system in the specialized care, the outcome is actually the probability of having received a diagnosis. But as the majority of diagnoses are based on hospitalizations I choose to refer to the outcome as the probability of hospitalization.

ment (six levels), and missing information on education. To conserve space I do not report the estimates for the control variables (available on request). In general, these show a reduced probability of hospitalization for highly educated individuals, as well as for individuals with more children, married people, younger individuals, and males. If not indicated otherwise, segregation is defined as the (log) RCI measured in the year of arrival. [15]

The next sub-section provides the main results. Estimates are presented both for different types of diagnoses and for different subgroups of the population. I then present results from robustness checks and gives some extensions. The last sub-section provides OLS and IV estimates from models relating health to individuals' actual long-term exposure to segregation.

Main results

Table 1 shows OLS estimates of the effect of segregation on the probability of being hospitalized. Each cell represents a separate regression. Estimates are shown both for the main sample as well as for different subgroups of the population stratified according to: gender, educational attainment and age. To take into account the grouped nature of the data the standard errors are clustered at the ethnic group-by-parish level (cf. Moulton 1990).

Column (1) shows results for the main sample. We can see that the estimate is close to zero and insignificant. A 95 percent confidence interval suggests that it is possible to rule out that a one standard deviation increase in segregation raises the probability of hospitalization by more than .44 percentage points ((.0008+.1.96*.0013)*1.322), corresponding to an increase in the likelihood of being admitted by about 5 percent. Columns (2)–(7) display estimates by population subgroup. I start by stratifying the sample by gender. From the estimates shown in columns (2) and (3) it is clear that there is no evidence of differential gender effects. The point estimate for males is larger in magnitude but as for females it is not statistically significant. Columns (4) and (5) present estimates by educational attainment. Again, there is no significant effect. Last, columns (6) and (7) provide results by age at immigration. The reason for choosing age 30 as the limit is that this is the median age at immigration in my main sample. I have also experimented with other divisions but the overall conclusion still holds: there is no evidence of a differential effect by age at immigration.

[15] The log specification takes into account that a unit increase in segregation from a low base is proportionately larger than one from a high base.

Table 1. The effect of segregation on the probability of hospitalization

	Main sample	By gender: Males	Females	By educational attainment: At most short high school	At least long high school	By age: <30 years old at immigration	≥30 years old at immigration
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
log(RCI)	.0008	.0024	.0001	.0012	.0001	.0011	.0028
	(.0013)	(.0017)	(.0021)	(.0015)	(.0026)	(.0017)	(.0021)
Mean of dept. var.	.0813	.0644	.0988	.0856	.0700	.0763	.8770
Obs.	73,431	37,271	36,160	53,531	19,794	40,915	32,516
Year of imm. FE:s	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Parish FE:s	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Ethnic group FE:s	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: If not indicated otherwise, the table shows OLS estimates of the coefficient on (log) RCI measured in the year of arrival. The outcome is observed in 2004. Each cell represents a separate regression. Standard errors reported in parenthesis are clustered at the group-by-parish level. The sample consists of refugee immigrants who arrived to Sweden in the period 1987–1991. Wherever appropriate, the regressions controls for age (third-order polynomial), number of children, gender, marital status, educational attainment (six levels), and for missing information on education. */** denote significance at the 10/5 percent level.

Table 2 shows estimates by type of diagnosis. Most of the diagnoses represent common chronic illnesses. The rationale for studying depression is that this disorder has been shown to be linked to stress, thus providing a (rough) way of investigating one of the mechanisms through which segregation has been proposed to affect health (e.g. Artinian et al. 2004). We can see that the coefficients are close to zero; and, as earlier, they are also insignificant.

Table 2. The effect of segregation on the probability of hospitalization by type of diagnosis.

	Stroke (1)	Respiratory (2)	Mental (3)	Cancer (4)	Heart (5)	Depression (6)	Diabetes (7)
log(RCI)	–.0002	.0003	–.0001	–.0003	.0003	.0001	.0000
	(.0002)	(.0003)	(.0004)	(.0003)	(.0003)	(.0002)	(.0002)
Mean of dept. var.	.0012	.0034	.0066	.0050	.0046	.0017	.0011
Obs.	73,431	73,431	73,431	73,431	73,431	73,431	73,431
Year of imm. FE:s	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Parish FE:s	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Ethnic group FE:s	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: If not indicated otherwise, the table shows OLS estimates of the coefficient on (log) RCI measured in the year of arrival. The outcome variables are observed in 2004. Each cell represents a separate regression. Standard errors reported in parenthesis are clustered at the group-by-parish level. The sample consists of refugee immigrants who arrived to Sweden in the period 1987–1991. Wherever appropriate, the regressions controls for age (third-order polynomial), number of children, gender, marital status, educational attainment (six levels), and for missing information on education. */** denote significance at the 10/5 percent level.

Tables A.4–A.6 replicate the analysis in Table 2 for the different subgroups. Table A.4 shows results by gender. Starting with males, we can see that there is a significant positive effect of segregation on males' probability of contracting respiratory diseases. The coefficient suggests that a one standard deviation increase in segregation raises the likelihood of being admitted by about .1 percentage point (.0008*1.322). Compared to the mean of the dependent variable this translates to an increase of about 30 percent ((.0008*1.322)/.0035). Still, since the mean of the dependent variable is quite low, caution is warranted when interpreting the magnitude of the estimate. The result is suggestive, especially since in many groups men are more likely to smoke than females. Thus, it is possible that the finding could reflect peer group influences in substance use. In column (7) we find that segregation increases the probability of being diagnosed for depression. There is no evidence of an effect on the remaining outcomes. Looking at Panel B we can see that there is actually a statistically significant negative effect on females' risk of being admitted to hospital for mental illnesses; although only at the 10 percent level. This finding can be compared with the results presented in Kling, Liebman and Katz (2007) who find beneficial effects on the mental health of females in families who received housing voucher offers in the Moving to Opportunity Program. The other coefficients are all close to zero and insignificant.

Table A.5 shows estimates by educational attainment. In Panel A we can see that none of the coefficients for less educated individuals are significant. For persons with higher education, there is a significant negative effect of segregation on the probability of being hospitalized for cancer; cf. column (4). There is no significant effect on the remaining outcomes. The tendency of insignificant estimates continues in Table A.6 displaying results by age at immigration. The only exception is that segregation significantly raises the likelihood of being admitted to hospital for heart problems for individuals who were under the age of 30 when immigrating.

To summarize, with a few exceptions, the results in this sub-section show no statistically significant effect of segregation on the probability of being hospitalized. This result holds also when studying different subgroups of the population and for various types of diagnoses. There is however a significant positive effect on males' risk of being admitted for respiratory diseases and depression, as well as on the probability of hospitalization for heart diseases for persons who were younger than age 30 at immigration. Conversely, there is a significant *negative* effect on females' risk of being admitted for mental disorders and highly educated individuals likelihood of being hospitalized for cancer. Still, the fact that a few estimates are found to be significant is hardly surprising since multiple related hypotheses are tested.[16] Thus, the overall conclusion is that there are no clear (or at least very weak) indications that segregation influences immigrants' health.

Robustness checks and extensions

Table 3 examines how sensitive the main results are to changes in the choice of segregation index, functional form, and sample selections. I start by asking whether there is a non-linear relationship between segregation and health; i.e. that the impact is stronger or weaker for higher levels of segregation. To investigate this I have run regressions including the square of the (log) RCI. As can be seen in Panel A, the point estimate for the squared term is insignificant, suggesting that there is no evidence of non-linear effects.

[16] For instance, at the 10 percent significance level one probabilistically expects to find one out of ten estimates significant. See e.g. Kling and Liebman (2004) for a discussion on statistical inference in the case of multiple related outcomes.

Table 3. Sensitivity analysis of the main results.

	Dependent variable: Pr(Hospitalization)
	(1)
A.	
log(RCI)	.0015
	(.0014)
$\log(\text{RCI})^2$	.0006
	(.0006)
B. RCI	.0008
	(.0005)
C. log(Size of ethnic community)	.0011
	(.0013)
D. log(Dissimilarity index)	.0001
	(.0012)
E. Uniquely identified source countries	.0010
	(.0019)
Year of imm. FE:s	Yes
Parish FE:s	Yes
Ethnic group FE:s	Yes

Notes: If not indicated otherwise, the table shows OLS estimates of the coefficient on (log) RCI measured in the year of arrival. The outcome variable is observed in 2004. Each panel represents a separate regression. Standard errors reported in parenthesis are clustered at the group-by-parish level. The sample consists of refugee immigrants who arrived to Sweden in the period 1987–1991. The regressions controls for age (third-order polynomial), number of children, gender, marital status, educational attainment (six levels), and for missing information on education. */** denote significance at the 10/5 percent level.

The next question is whether the results are sensitive to how segregation is defined. From a theoretical perspective, there are no reasons to prefer a logarithmic measure; it could very well be that it is absolute changes in segregation that matters. To examine this I have estimated models introducing the RCI in a non-logarithmic form. We can see that the results from the regressions are qualitatively similar. Additionally, the results could be sensitive to the specific type of segregation index used. Panel C shows results for the (log) number of co-ethnics in the parish. In contrast to the RCI, this variable represents a measure of the quantity of contacts. We can see that the estimate is insignificant. Panel D examines the perhaps most frequently used segregation index – the dissimilarity index. [17] Also for this variable is the estimate insignificant.

[17] Using previous notation, the dissimilarity index is formally defined as $DI = \frac{1}{2}\sum_{k=1}^{K}\left|\frac{Group_{kt}^{j}}{Pop_{t}^{j}} - \frac{Group_{kt}^{(-j)}}{Pop_{t}^{(-j)}}\right|$, where (–j) denotes non-group members.

To investigate whether the results are robust to the way Statistics Sweden has aggregated the small source countries I have re-estimated the models only including the uniquely identified countries. The sample size to fall from 73,431 to 39,962 but the estimates are virtually identical.

Table 4 presents results for alternative outcome variables. The purpose is twofold: first, the outcomes are in themselves of interest to examine; second, they provide further robustness checks of the results already presented. I start by investigating the relationship between segregation and mortality. As previously mentioned, most past studies that have focused on this outcome generally find a significant adverse relationship. The variable is defined as the probability of dying during the observation period (up until 2004). The estimate is presented in column (1). As can be seen, it is insignificant.[18]

As discussed earlier, there is a possibility that segregation could affect either the likelihood of being admitted to hospital, or the propensity of seeking medical care. Although the ethnic group and parish fixed effects will control for such differences there is still a chance that this process varies systematically at the ethnic group-by-parish level. The fact that the estimate in column (1) is consistent with the previous results showing no overall effect of segregation on health is encouraging since this variable is not plagued by the potential problem of differential selection into medical care. The result from another "test" of this issue is presented in column (2). The test is based on the idea that acute illness is less likely to be influenced by selection into hospitalization: in cases of acute sickness, individuals simply must seek medical care, and doctors have less scope for not admitting them. The analysis is made possible by the fact that the data contains information on whether the hospital visit was planned or not. Consequently, the dependent variable in column (2) is the probability of unplanned care. The estimate is very similar to that presented in Table 1, supporting the view that differential selection into medical care is not causing bias to the estimates. Column (3) presents even further evidence on this question by examining how segregation is related to the probability of taking out sick leave. Although not perfect, sick leave can be considered as a measure of health status.[19] In Sweden, a doctor's certificate

[18] The fact that some individuals die and thereby fall out of the sample highlights a potentially important question: what would the effect of segregation on health have been had the deceased individuals not died? Although the insignificant estimate for mortality makes this question less vital I have investigated the issue using methods to deal with sample attrition (see Little and Rubin 1987). The approach is to first estimate a probit model where the probability of deceasing is related to the observed set of covariates, and then in a second stage reweight the sample using the (inverse) predicted probability of dying. It turns out that the estimates obtained from this methodology are practically identical to the previous estimates.

[19] See Hesselius, Johansson and Nilsson (2008), and Hesselius, Johansson and Vikström (2008) for evidence on how social interactions influence the likelihood of taking out sick leave.

is not required when reporting sick until the seventh day of absence. Thus, since sick leave is not directly influenced by the health care system there is less scope for differential selection into medical care. Consistent with previous results, the estimate in column (3) shows an insignificant effect of segregation on the probability of reporting sick.

Table 4. Alternative outcome variables

	Pr(Deceased)	Pr(Unplanned hosp.)	Pr(Sick leave)
	(1)	(2)	(3)
log(RCI)	.0002	.0007	.0005
	(.0006)	(.0011)	(.0008)
Mean of dept. var.	.0219	.0512	.1926
Obs.	88,895	73,431	73,431
Year of imm. FE:s	Yes	Yes	Yes
Parish FE:s	Yes	Yes	Yes
Ethnic group FE:s	Yes	Yes	Yes

Notes: If not indicated otherwise, the table shows OLS estimates of the coefficient on (log) RCI measured in the year of arrival. The outcome variable in column (1) is defined as the probability of having deceased up until 2004. The outcome variables in columns (2)–(5) are observed in 2004. Each panel represents a separate regression. Standard errors reported in parenthesis are clustered at the group-by-parish level. The sample consists of refugee immigrants who arrived to Sweden in the period 1987–1991. The regressions controls for age (third-order polynomial), number of children, gender, marital status, educational attainment (six levels), and for missing information on education. */** denote significance at the 10/5 percent level.

Last, since I have information on annual earnings it is possible to investigate whether the relationship between segregation and health is mediated through income, as suggested by theory. To examine this I have run regressions controlling for log earnings in 2004. In these models I include individuals with zero earnings by adding an arbitrary low value to earnings and controlling for this in the regressions. Note that controlling for log earnings is not straightforward since this variable is likely to be endogenous. The results are shown in Table A.7. We can see that conditioning on log earnings matters very little for the estimates.

The consequences of long-term exposure to segregation

As discussed earlier, the vast majority of past studies find a significant adverse relationship between segregation and health. To reconcile the results in this paper with the previous literature this sub-section asks what the results would have been if omitted variables had not been accounted for. This is done by relating the probability of hospitalization to individuals' average exposure to segregation during the entire observation period. As a comparison, I then use the assigned level of segregation to instrument for average exposure to segregation. Since initial segregation can be seen as a proxy for actual exposure to segregation, the estimates presented in the previous subsections can be interpreted as "reduced form" estimates. Still, because of residential relocations and changes in population net-inflows over time, an IV approach provides a more direct way to examine this question. Some caution is however warranted when interpreting the IV estimates. It is possible that people randomly assigned to very segregated neighborhoods moved more quickly to better neighborhoods with better health services. In this case, the instrument could have an effect on health other than through the effect on average segregation. Similarly, one could argue that health is an accumulative process where inputs in the health production function at different points in time are complementary, which also would invalidate the instrument.

The first stage of the IV model can be written as

$$(3) \qquad \overline{Segr}_{ijk} = Segr_{jkt_0}\pi_1 + X_i'\pi_2 + \pi_j + \pi_k + \pi_{t_0} + w_{ijk}$$

where the dependent variable is segregation averaged over the observation period (excluding the year of immigration), i.e., $\overline{Segr}_{ijk} = \sum_{t_0+1}^{T} Segr_{itjk} / T$. Assuming that there is no direct effect of initial segregation on subsequent health, apart from for the one operating through $\overline{Segr}_{ijk}$, and that the error terms in the first stage and outcome equations are not correlated, the IV estimator consistently estimates the effect of segregation on health.

Technically speaking, the OLS estimates provide the average effect of segregation in the population. These estimates are not to be interpreted as causal but as showing the correlation between segregation and health. If the underlying assumptions hold, the IV estimates should be seen as providing the average causal impact for those individuals who were induced to stay because they were placed in a municipality with a given level of segregation and who otherwise would have moved.[20]

[20] Angrist, Imbens and Rubin (1996) provide a framework for interpreting IV estimates when the responses are heterogeneous. The key assumption underlying this interpretation is that the response to the instrument should be monotonic. In this setting, monotonicity means that either refugees placed in an parish with segregation level n in the coming years are exposed to an average level of segregation that is at least as large as for refugees placed in a parish where the segregation level was n-1. Note that the consequences of violations of this assumption need not be serious if there are relatively few individuals for which monotonicity does not apply. This is because the IV estimate is a weighted average of the effect of those individuals who are shifted by the instrument and those whom the instrument moves in the opposite direction, where the latter group receives negative weights.

Table 5 replicates the main results in Table 1 using actual exposure to segregation. Consistent with previous studies there are clear indications of a positive correlation between segregation on the probability of becoming hospitalized. For the main sample, column (1) suggests that a standard deviation increase in segregation is associated with a rise in the probability of being hospitalized by about 5.7 percent ((.004*1.168/.0831). Similar results are found in all subgroups of the population. The only exception is for highly educated persons where the estimate is insignificant.

Panel B displays the IV estimates. The first stage relationship between initial segregation and average segregation in column (1) is .442 (.010), suggesting that the instrument is strong (cf. Staiger and Stock 1997).[21] The fact that the compliance rate is relatively high means that the IV estimates may come close to the average effect of segregation in the population.

As always in IV analysis, the estimates are less precise than the OLS estimates, and in this case none of the IV estimates are statistically significant. Observe however that the size on many of the coefficients is reduced to less than half of the size of the OLS estimates.

[21] The first stage coefficient is stable across the different population subgroups.

Table 9. The effect of actual exposure to segregation on the probability of hospitalization by individual background characteristics

		By gender		By educational attainment		By age	
	Main sample	Men	Females	At most short high school	At least long high school	Less than 30 years old at imm.	At least 30 years old at imm.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
A. OLS	.0040**	.0049**	.0031*	.0057**	–.0002	.0045**	.0051**
	(.0011)	(.0014)	(.0018)	(.0014)	(.0021)	(.0015)	(.0019)
B. IV	.0019	.0060	.0001	.0026	.0003	.0025	.0060
	(.0029)	(.0042)	(.0042)	(.0034)	(.0060)	(.0040)	(.0046)
Mean of dept. var.	.0813	.0644	.0988	.0856	.0700	.0763	.877
Obs.	73,431	37,271	36,160	53,531	19,794	40,915	32,516
Year of imm. FE:s	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Parish FE:s	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Ethnic group FE:s	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The table shows estimates of the coefficient on (log) RCI. The endogenous variable is (log) RCI averaged over the observation period, excluding the year of immigration, and the instrument is (log) RCI measured in the year of immigration. The outcome variable is observed in 2004. Each cell represents a separate regression. Standard errors reported in parenthesis are clustered at the group-by-parish level. The sample consists of refugee immigrants who arrived to Sweden in the period 1987–1991. Wherever appropriate, the regressions controls for age (third-order polynomial), number of children, gender, marital status, educational attainment (six levels), and for missing information on education. */** denote significance at the 10/5 percent level.

Concluding remarks

This paper asks whether segregation affect immigrants' health. An exceptionally rich dataset in combination with plausibly exogenous variation in segregation provides an opportunity to investigate the question in much greater detail than what has been possible in the previous literature.

In contrast to most previous studies, the results suggest that there is no statistically significant effect of segregation on the overall probability of being hospitalized. I have also examined the impact of segregation on different types of diagnoses and for different subgroup populations. The fact that a few point estimates are found to be significant is not surprising since multiple related hypotheses are tested. The results are robust to several sensitivity checks.

The last part of the paper asks what would the results have been had endogeneous sorting not been taken into account. The OLS estimates suggest a strong positive correlation between segregation and the risk of being hospitalized. The estimates are significant both in statistical terms, as well as in magnitude. By contrast, instrumenting for individuals' average exposure to segregation using initial segregation shows no significant effect. Taken together, the overall conclusion is that there is no, or at least very weak evidence that segregation affects individuals' long-term health.

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Table A.1. Groups of diagnoses used in the empirical analysis (based on ICD 9)

Overall	Any cause of admission
Cerebrovascular diseases	I60–I69
Respiratory diseases	J00–J99
Mental diseases	F00–F99
Cancer	C00–D48
Pregnancy	O00–O99
Heart	I01, I05–I09, I11, I13, I20–I25, I30–I52
Depression	F30–F39
Diabetes	E10–E14

Table A.2. Region of birth

	Percent of the sample
1. Former Yugoslavia	5.77
2. Poland	6.16
3. The Baltic states (Estonia, Latvia, Lithuania)	0.26
4. Eastern Europe 1 (Rumania, The former USSR, Bulgaria, Albania)	7.34
5. Eastern Europe 2 (Hungary, The former Czechoslovakia)	2.30
6. Mexico and Central America (El Salvador, Mexico Other countries)	1.42
7. Chile	8.45
8. Other South America (Peru, Brazil, Colombia, Argentina, Uruguay, Other countries)	2.47
9. African Horn (Ethiopia, Somalia, Sudan, Djibouti)	8.34
10. North Africa (Arabic countries) and Middle East (Lebanon, Syria, Morocco, Tunisia, Egypt, Algeria, Israel, Palestine, Jordan, Other countries)	14.70
11. Other Africa (Gambia, Uganda, Zaire Ghana, Other countries)	2.50
12. Iran	20.53
13. Iraq	5.89
14. Turkey	5.17
15. South East Asia (Vietnam, Thailand, the Philippines, Malaysia, Laos Other countries)	5.90
16. Other Asia (Sri Lanka, Bangladesh, India, Afghanistan, Pakistan)	2.80

Notes: The total number of observations is 73,431.

Table A.3. Summary statistics for selected variables

Variable	Mean	Standard deviation	Min	Max
Admitted to hospital	.081	.273	0	1
log RCI	–.366	1.322	–11.689	4.063
Female	.492	.500	0	1
Married	.629	.483	0	1
Age at immigration	29.86	8.94	16	65
Compulsory school	.425	.494	0	1
Short high school	.199	.399	0	1
Long high school	.194	.395	0	1
Short university	.103	.304	0	1
Long university	.073	.260	0	1
PhD	.003	.063	0	1

Notes: The sample consists of refugee immigrants who arrived to Sweden in the period 1987–1991. Hospital admissions are recorded in 2004. Remaining variables are observed in the year of immigration. Summary statistics on education is conditional on that the information is available.

Table A.4. The effect of segregation on the probability of hospitalization by type of diagnosis and gender

	Stroke (1)	Respiratory (2)	Mental (3)	Cancer (4)	Pregnancy (5)	Heart (6)	Depression (7)	Diabetes (8)
A. Males	–.0005	.0008**	.0007	–.0006	--	.0003	.0004*	.0001
	(.0003)	(.0004)	(.0006)	(.0004)		(.0005)	(.0002)	(.0003)
Mean of dept. var.	.0015	.0035	.0073	.0028		.0066	.0013	.0014
Obs.	37,271	37,271	37,271	37,271		37,271	37,271	37,271
B. Females	.0002	–.0003	–.0010*	.0000	–.0003	.0005	–.0003	–.0001
	(.0002)	(.0004)	(.0006)	(.0006)	(.0011)	(.0004)	(.0003)	(.0002)
Mean of dept. var.	.0009	.0033	.0059	.0072	.0271	.0028	.0020	.0008
Obs.	36,160	36,160	36,160	36,160	36,160	36,160	36,160	36,160
Year of imm. FE:s	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Parish FE:s	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Ethnic group FE:s	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: If not indicated otherwise, the table shows OLS estimates of the coefficient on (log) RCI measured in the year of arrival. The outcome variables are observed in 2004. Each cell represents a separate regression. Standard errors reported in parenthesis are clustered at the group-by-parish level. The sample consists of refugee immigrants who arrived to Sweden in the period 1987–1991. Wherever appropriate, the regressions controls for age (third-order polynomial), number of children, gender, marital status, educational attainment (six levels), and for missing information on education. */** denote significance at the 10/5 percent level.

Table A.5. The effect of segregation on the probability of hospitalization by type of diagnosis and educational attainment

	Stroke (1)	Respiratory (2)	Mental (3)	Cancer (4)	Heart (5)	Depression (6)	Diabetes (7)
A. At most short high school	–.0003	.0002	.0001	.0000	.0005	.0002	–.0000
	(.0003)	(.0003)	(.0005)	(.0004)	(.0004)	(.0002)	(.0002)
Mean of dept. var.	.0013	.0038	.0071	.0048	.0048	.0017	.0013
Obs.	53,531	53,531	53,531	53,531	53,531	53,531	53,531
B. At least long high school	.0001	.0006	–.0004	–.0014*	–.0001	–.0005	–.0002
	(.0004)	(.0005)	(.0008)	(.0008)	(.0007)	(.0005)	(.0002)
Mean of dept. var.	.0010	.0024	.0053	.0055	.0039	.0016	.0005
Obs.	19,794	19,794	19,794	19,794	19,794	19,794	19,794
Year of imm. FE:s	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Parish FE:s	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Ethnic group FE:s	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: If not indicated otherwise, the table shows OLS estimates of the coefficient on (log) RCI measured in the year of arrival. The outcome variables are observed in 2004. Each cell represents a separate regression. Standard errors reported in parenthesis are clustered at the group-by-parish level. The sample consists of refugee immigrants who arrived to Sweden in the period 1987–1991. Wherever appropriate, the regressions controls for age (third-order polynomial), number of children, gender, marital status, educational attainment (six levels), and for missing information on education. */** denote significance at the 10/5 percent level.

Table A.6 The effect of segregation on the probability of hospitalization type of diagnosis and age

	Stroke	Respiratory	Mental	Cancer	Heart	Depression	Diabetes
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
A. Less than 30 years old at immigration	.0000	.0003	–.0006	–.0001	.0003*	–.0000	.0001
	(.0001)	(.0003)	(.0006)	(.0003)	(.0002)	(.0003)	(.0002)
Mean of dept. var.	.0005	.0026	.0080	.0031	.0010	.0018	.0007
Obs.	40,915	40,915	40,915	40,915	40,915	40,915	40,915
B. At least 30 years old at immigration	–.0005	.0007	.0005	–.0007	.0007	.0003	–.0001
	(.0005)	(.0005)	(.0005)	(.0007)	(.0007)	(.0003)	(.0003)
Mean of dept. var.	.0021	.0044	.0049	.0074	.0091	.0015	.0016
Obs.	32,516	32,516	32,516	32,516	32,516	32,516	32,516
Year of imm. FE:s	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Parish FE:s	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Ethnic group FE:s	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: If not indicated otherwise, the table shows OLS estimates of the coefficient on (log) RCI measured in the year of arrival. The outcome variables are observed in 2004. Each cell represents a separate regression. Standard errors reported in parenthesis are clustered at the group-by-parish level. The sample consists of refugee immigrants who arrived to Sweden in the period 1987–1991. Wherever appropriate, the regressions controls for age (third-order polynomial), number of children, gender, marital status, educational attainment (six levels), and for missing information on education. */** denote significance at the 10/5 percent level.

Table A.7. The effect of segregation on the probability of hospitalization by type of diagnosis conditional on earnings

	Overall	Stroke	Respira-tory	Mental	Cancer	Heart	Depres-sion	Diabe-tes
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
log(RCI)	.0015	–.0001	.0003	–.0001	–.0001	.0004	.0001	.0001
	(.0011)	(.0002)	(.0003)	(.0004)	(.0003)	(.0003)	(.0002)	(.0002)
Mean of dept. var.	.0813	.0012	.0034	.0066	.0050	.0046	.0017	.0011
Obs.	73,431	73,431	73,431	73,431	73,431	73,431	73,431	73,431
Year of imm. FE:s	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Parish FE:s	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Ethnic group FE:s	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: If not indicated otherwise, the table shows OLS estimates of the coefficient on (log) RCI measured in the year of arrival. The outcome variables are observed in 2004. Each cell represents a separate regression. Standard errors reported in parenthesis are clustered at the group-by-parish level. The sample consists of refugee immigrants who arrived to Sweden in the period 1987–1991. Wherever appropriate, the regressions controls for age (third-order polynomial), number of children, gender, marital status, educational attainment (six levels), missing information on education, and zero earnings. */** denote significance at the 10/5 percent level.

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