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ABSTRACT

The Value of Earning for Learning: Performance Bonuses in Immigrant Language Training*

We study the effects of performance bonuses in immigrant language training for adults. A Swedish policy pilot conducted in 2009-2010 gave a randomly assigned group of municipalities the right to grant substantial cash bonuses to recently arrived migrants. The results suggest substantial effects on average student achievement. But these were fully driven by metropolitan areas; in other parts of Sweden performance was unaffected. The relative effects were larger for younger students but similar for men and women, and present for migrants from different parts of the world. The bonus had a less clear impact on enrollment, but there are indications that it may have increased the probability of progressing to bonus-awarding courses in metropolitan areas.

JEL Classification: J08, J15, I24

Keywords: immigration, language training, performance bonus

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1 Introduction

A central idea in economics is that we act on economic incentives: increasing the rewards for a certain type of behavior will make us more likely to behave in that way. Despite the theoretical predictions, the empirical evidence from education research is mixed. While a number of randomized trials suggest that financial incentives do improve student achievement (e.g., Angrist, Lang and Oreopoulos 2009, Angrist et al. 2002, Kremer, Miguel and Thornton 2009, Angrist and Lavy 2009, Dearden et al. 2009, Dee 2009 and Pallais 2009) there is also evidence of more limited or no effects (e.g., Angrist, Oreopoulos and Williams 2010, Fryer 2010, Bettinger 2008 and Sharma 2010).

This paper evaluates a pay for performance scheme from a setting not studied before: performance bonuses in language training for immigrants. The experiment is unique in the sense that it tests how financial rewards affect basic human capital accumulation among adults and because it is an incentive program targeted at immigrants. The lack of economic and social integration among the immigrant population is a major concern for policy makers throughout most of the industrialized world and host country language proficiency is generally considered a key factor in promoting economic as well as political and social inclusion. There is plenty of evidence that the labor market rewards such skills[1]. Many countries therefore spend substantial resources on language training for immigrants. Whether costs are considered too high or the perceived benefits are seen as too low is hard to tell, but it is a fact that many immigrants never come to master the host country language (Tubergen and Kalmijn 2005 and Rooth and Åslund, 2006).[2]

Our data come from a Swedish policy pilot implemented within the already existing Swedish language tuition program for immigrants in 2009–2010. Municipalities which had expressed an interest in participating in the pilot were matched into pairs, and then allocated to treatment and control by pair-wise randomization. In treated municipalities, migrants passing a bonus rewarding course within fifteen months after immigration but no later than a year after the course start were awarded up to SEK 12,000 (about 1,350 Euros). The data used include all immigrants since 2006 and we are able to investigate both enrollment and performance effects, as well as heterogeneous effects in terms of

[1] See, for example, Bleakley and Chin (2004), Chiswick and Miller (2002), Carnevale and Lowell (2001), Chiswick and Miller (2010), and Carliner (1999) for the US, Dustmann and Fabbri (2003) for the UK, Dustmann (1994) and Dustmann and Van Soest (2002) for Germany, Chiswick and Miller (1995) for Australia, Chiswick (1998) for Israel. The list is not exhaustive. Early work includes McManus, Gould and Welch (1983), Borjas (1984), Carliner (1980), Tainer (1988) and Kossoudji (1988). Swedish evidence is given in Rooth and Åslund (2006).
[2] This is also true for migrant youth; see for example OECD (2012)

student characteristics. The design of the experiment also allows us to evaluate the effect of the extra funds that were paid out to the municipalities that participated in the pilot evaluation.

The analysis shows that the introduction of performance bonuses had a less clear effect on overall enrollment but a substantial positive average effect on student achievement. However, the effects are fully driven by metropolitan areas in the sample. Thus, performance in the other participating municipalities was not affected. In the metropolitan areas, student achievement improved both for courses qualifying directly for a bonus, i.e., continuation courses, and for beginner's courses, even though the relative impact was higher for the bonus-awarding courses. The effects were similar for men and women but the relative effect was greater among the young. It also tends to be stronger for people with no more than secondary schooling. All in all, the results indicate that performance schemes may play a role for improving student achievement for immigrant adults, but that the effects seem to depend on institutional features that require some further understanding.

The paper proceeds as follows: in Section 2 we discuss theoretical arguments and expected impacts, empirical evidence on financial incentives in education, and the link between economic integration of immigrants and language skills. Section 3 describes the Swedish tuition system for immigrants in which the experiment was conducted. In Section 4 we present the policy pilot and in Section 5 we discuss the data and some initial descriptive statistics. Section 6 first presents the empirical strategy and specification, and then turns to the empirical results and robustness checks. In Section 7 we conclude.

2 Theoretical considerations and previous evidence

2.1 Financial awards in education – motivations and expectations

In a stylized world, one could argue that there are essentially two reasons for policy makers to consider influencing individual decisions and actions: (i) people do not know their own good (i.e., paternalism); (ii) there are externalities that are not internalized by individual decisions.

Regarding the first argument, people may underinvest in education, including language skills, if they have time inconsistent preferences or misperceive education costs to be too high or the returns to be too low (Rodriguez-Planas 2010). Under such circumstances, influencing an individual to invest more in education will increase his/her utility.

The second argument considers the utilities of others, or society at large. There are numerous possible spillover effects from having a more well-educated population. For example, it has been suggested that it positively affects economic growth, innovation, democratic stability, and so forth (e.g., Moretti 2004 and Krueger and Lindahl 2001). Given such effects, it may be optimal for society to stimulate investment in education. In a welfare state such as Sweden’s, there are of course also more mundane external effects; immigrants who learn the host country language are more likely to be employed and pay taxes and less likely to depend on social benefits (more on this link below).

Whether behavior is actually influenced by the introduction of new forms of economic incentives, depends on the comparison between costs and gains, which is likely to vary across individuals and groups. A potential drawback of performance bonuses is that there may be deadweight losses, if not only those who change their behavior earn the bonus. Both these arguments consider the possibilities of designing the bonus scheme in an appropriate way to maximize the incentive effect while minimizing deadweight costs. There may also be directly negative performance effects of a bonus, if it, for example, causes stress or affects people to opt for more risky study strategies. It has, for example, been suggested that rewards crowd out self-motivation and curiosity among other things (e.g., Deci et al., 2001)[3].

So far the arguments circle around individual behavior and decisions. But it can also be argued that implementing a bonus in one setting and (as in our example) for some participants can have positive or negative effects on others. Positive peer effects can arise if students aiming for a bonus encourage their peers to also work harder. Opposite effects can arise, for example, due to jealousy or crowding-out of teaching resources. If the bonus is seen as unfair, it is also possible that it would trigger negative sentiments at an aggregate level.

[3] There is a long debate in psychology on whether extrinsic motivation crowds out intrinsic motivation (see, for example, Deci et al., 2001 and Cameron and Pierce, 2002, for opposing views) but also Rodríguez-Planas 2010 for an overview of mechanisms. Morover, see Leuven et al. (2010) for evidence of financial awards crowding out intrinsic motivation for weak student groups.

2.2 Empirical evidence on economic incentives in education

A number of randomized trials have been carried out to test whether financial awards alone or in combination with educational support services can improve student achievement or lower dropout rates.[4] The collected evidence is mixed.

Positive effects on student achievement and/or dropout rates of introducing financial awards to students in elementary schools, secondary schools and colleges are found in studies from various countries (e.g., Angrist, Lang and Oreopoulos, 2009, Angrist et al., 2002, Kremer, Miguel and Thornton, 2009, Angrist and Lavy, 2009, Dearden et al., 2009, Dee, 2009, Jackson 2010, and Pallais, 2009). For example, in an attempt to improve student performance at a Canadian college, academic support services and/or financial incentives was assigned by randomization to new students (Angrist, Lang and Oreopoulos, 2009). The results show that the take-up for services was higher for women and that the combination of economic rewards and support services had a positive effect on study results for this group. Furthermore, the effects were not limited to the year of the intervention.

In contrast, there is also evidence from randomized experiments from the US, Canada and elsewhere that suggest that financial incentives play a limited role or are not effective (Angrist, Oreopoulos and Williams 2010, Fryer 2010, Bettinger 2008, and Sharma 2010). Probably the largest experiment up to date was carried out on more than 200 elementary schools in the three different metropolitan areas in the US (see Fryer, 2010). Interventions were randomized on the school level and included financial incentives for reading books, improved classroom grades and awards for interim assessments. Overall, the study gives little support for this type of interventions.[5]

Thus, what to expect a priori from the experiment analyzed in this paper is not obvious. Furthermore, none of the experiments discussed in this section were directed towards newly arrived immigrants nor did they focus on language proficiency. Financial incentives appear to have an effect on achievement in some settings, but the evidence also suggests that it is no panacea working in all cases.

[4] For a more extensive overview see Rodriguez-Planas (2010)

[5] In an attempt to summarize the literature Angrist, Oreopoulos and Williams (2010) draw the conclusion that if incentives work they appear to have larger effects in elementary and secondary schools than on the university level.

2.3 Immigrant language skills and labor market outcomes

A prime reason for the political interest in promoting host country language acquisition is its expected impact on labor market outcomes. Like many other OECD countries, Sweden exhibits major native-immigrant differences in the labor market (e.g. Sébastien et al. 2010). In 2009 about 62.5 percent of the foreign-born Swedish population was employed. This could be compared to 75.7 percent among natives (Eriksson, 2011). There are naturally also big discrepancies within the group of foreign-born. In general, immigrants arriving for humanitarian reasons and succeeding family members perform substantively worse than immigrants arriving from OECD countries.[6]

Language proficiency is arguably a key component of the skill acquisition which is often assumed to explain much of the relatively sharp increase in earnings among recently arrived migrants (see e.g. Borjas 1999, LaLonde and Topel 1997 for overviews, or Friedberg 2000, Bratsberg and Ragan 2002, and Berman, Lang and Siniver 2003). The literature contains abundant evidence that immigrants that master the dominant language of the destination country have higher earnings than immigrants that lack such skills. Moreover, destination country language skills has been suggested to be associated with lower unemployment rates and higher employment levels as well as decreasing consumption costs (Chiswick and Miller, 1998). Better language skills could, for example, lower the search cost for housing or other particular goods.

An inherent problem in the literature is to establish whether the acquisition of host country language skills has an effect on labor market outcomes or whether it is merely an association. If language proficiency is correlated with individual ability this unobserved heterogeneity is likely to bias the estimates of simple correlation studies. It has also been pointed out that measurement errors are common when measuring language skills (e.g., Dustmann and van Soest, 2001 and 2002). The more recent literature tries to address these issues (e.g., Bleakley and Chin, 2004, Dustmann and Fabbri, 2003, and Dustmann and Van Soest, 2002) and to no surprise the overall

[6] The inflow of immigrants to Sweden has been relatively large the last decades and in the end of 2011 about fifteen percent of the Swedish population was born abroad. This corresponds to an increase of four percentage points (423,000 individuals) since the year 2000 (SCB, 2012). The five largest source countries among immigrants arriving in 2011 were Iraq, Poland, Afghanistan, Somalia and China (SCB, 2012). During the last decade, on average, 15 percent of all residence permits were granted to refugees, 36 percent to family members of earlier immigrants or Swedish born residents and 37 percent to labor immigrants including immigrants from the European Economic Area (EEA) and the rest of the world (Migrationsverket, 2012).

message stands, i.e., the acquisition of destination country language skills is of importance for the labor market success of immigrants[7].

Despite the potential benefits many immigrants never become proficient in language of the host country (e.g. Tubergen and Kalmijn 2005 and Rooth and Åslund, 2006, (for results on Sweden)). Another factor that may be important for fluency is exposure to the language of the host country (e.g., Chiswick and Miller, 1995 and Chiswick and Miller, 1998). Some studies find that host country language proficiency is inversely related to the size of the linguistic community in the area of residence (Lazear, 1999). A third factor is efficiency, which refers to the process of translating exposure into actual skills (Chiswick and Miller, 1998). For example, age at the time of migration is of importance. Young individuals are more likely to become more fluent in a second language (Long, 1990). The educational background of immigrants similarly appears to affect the possibilities of acquiring language skills. In general, immigrants with more schooling pick up language skills more easily (Chiswick and Miller, 1998). The linguistic distance between the native language and the dominant language probably also affects the pace at which language skills are acquired.

3 The Swedish language tuition system for immigrants

The bonus scheme under study was implemented within the Swedish tuition system for immigrants and in this section we describe some of the relevant features of the program. Immigrants to Sweden have been offered Swedish tuition in one form or another since the 1960s (Kennerberg and Sibbmark, 2005). The aim of this educational program is to provide adult immigrants with basic Swedish language skills (Skollag 2010:800)[8]. The scheme known as Sfi (Swedish for immigrants) is free of charge and also aims to provide basic reading and writing training to immigrants lacking such skills.

It is an ambitious program. In 2011 around 102,400 people were enrolled. About 66 percent of the long-term migrants between 1994 and 2003 aged 20 to 55 years old started Sfi within a year after immigration excluding immigrants from Norway, Denmark and Finland (Kennerberg and Åslund 2010). The number of immigrants

[7] One exception is Hayfron (2001) that do not find a link between language training for Thirld world immigrants in Norway and earnings. See footnote 1 for addtional references.

[8] This law from 2010 replaced Skollag 1985:1100 that had been in place since 1985 but the purpose of the scheme however remained the same.

enrolling into Sfi for the first time is naturally related to the number of immigrants to Sweden a particular year.

It is the responsibility of the municipalities to provide language training programs but the municipalities can contract other providers. In 2010 about 35 percent of the students were enrolled in courses offered by private institutions, adult educational associations (studieförbund) and folk high schools (folkhögskolor) (Swedish National Agency for Education, 2010A).[9] The municipalities finance the basic language training either through grants from the state or through local taxes. The state compensates municipalities that receive refugees and this compensation normally covers the cost of providing language courses among other things. For other groups of immigrants the municipalities finance the program through the tax system (Kennerberg and Sibbmark, 2005). Since 2007 municipalities also receive a lump sum from the central government for refugees that has either passed a course within Sfi within 12 months after immigration or if a refugee has worked or had an internship for at least 5 months during the first 12 months in Sweden (SFS 2007 and SFS 2009).[10]

Immigrants interested in enrolling into Sfi should be offered a place within three months after fulfilling the requirements to participate. Apart from lacking basic Swedish skills the only additional criteria for admission is that the immigrant should be registered as a resident in a municipality and be at least sixteen years old.[11] In general immigrants that apply to participate can be divided into two groups: refugees and other immigrants. About one third belong to the former group (Swedish Schools Inspectorate, 2010) and this group is often assigned to language training through the introduction programs organized for refugees. The second group is more heterogeneous; some students are directed to Swedish tuition through the social insurance system or the employment services, others enroll voluntarily.

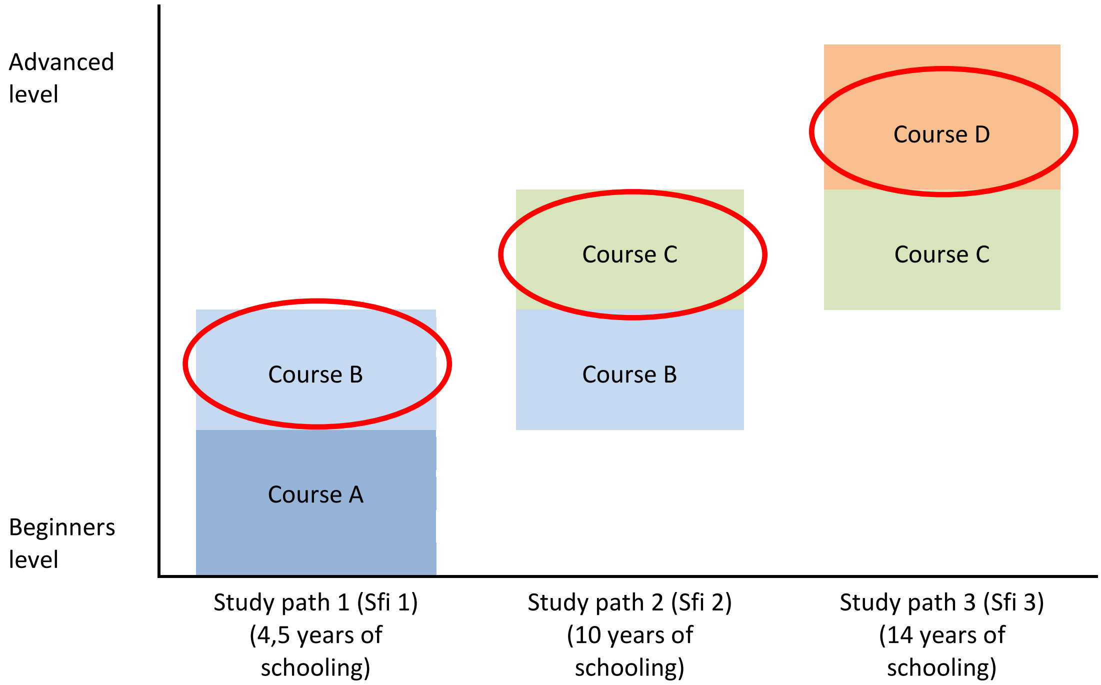
The bonus program was accordingly implemented in a setting where there are already strong incentives to participate (in addition to those provided by the expected gains from learning the Swedish language). The conditions vary depending on immigrant category and individual characteristics and situation, but for a large share of

[9] All providers participated in the experiment.
[10] In principle, this would be an interesting topic for an(other) evaluation.
[11] A few exceptions exist. Norwegian and Danish citizens are in general not eligible to the scheme. Furthermore, Finnish citizens residing in Finland but working in Sweden are under certain criteria eligible to language training programs.

the immigrants going to the language courses can be considered mandatory for receiving financial and other forms of support from society.

The Swedish tuition system for immigrants is regulated in more detail by SKOLFS 2009:2 (replaced in 2012 by SKOLFS 2012:13). It is a regulation that describes the purpose and aim of the educational program as well as the structure of the training programs. Detailed goals for the courses are also specified. The structure of the educational scheme is roughly sketched in Figure 1. It contains three study paths, study path 1 (Sfi1), study path 2 (Sfi2) and study path 3 (Sfi3). The different study paths are targeted to groups that differ in their educational background. On average the language courses should include at least 15 class hours per week. The length of a course could vary depending on the educational background of the participants, but there is a target (although not a limit) of 525 hours. Standardized tests are used as a tool for grading on course B, C and D indifferent of the study path and the tests are given throughout the year.

Figure 1 The Swedish language tuition system for immigrants, schematic overview



Note: Mean years of schooling in the home country within parentheses. The average is calculated for beginners of Sfi between 2009-07-01 and 2010-06-30. A circle indicates that the course entitles for a bonus payment under certain criteria (see Section 4.2).

The student group is heterogeneous, which is partly reflected by the differences in schooling from the home country. For example, the average years of schooling for students following path 1 is only 4.5 years which could be compared to fourteen years for students on study path 3 (Figure 1). Each study path contains two courses. A course is either a beginner's course or a more advanced course depending on the study path. One example is course B in study path 1 and 2 that has the same goals indifferent of the study path, but different structures to better fit the educational background of the students and their previous knowledge of Swedish. After finishing a course the student can make progress by starting a new course. All students have the right to progress up to the most advanced course, i.e., course D on study path 3.

3.1 Previous studies of the Sfi tuition system

Deficiencies of the tuition system have been stressed in a number of reports (e.g., Statskontoret 2009, Riksrevisionen 2008 and Eriksson 2007, Swedish Schools Inspectorate, 2010). For example, the share of dropouts and the number of students still enrolled after three years in the system has been highlighted as potential problems (Eriksson 2007 and Statskontoret 2009). Official statistics show that of those enrolling in 2009, 60 percent had completed at least one course by 2011, 29 percent had dropped out temporarily or permanently, and 11 percent were still enrolled. Outcomes were somewhat better among women than among men, and the recent figures compare favorably to statistics from earlier years (Swedish National Agency for Education 2012).

Whether the results of Sfi are satisfying or not does not only depend on the actual pass rate of the courses but also on the alternatives to taking the courses. If a person drops out because he or she finds a job it is questionable whether that should be considered a failure of the system. Despite the scope of the program the labor market effects of the program have received little attention. Indirect evidence suggests that non-participants work to a larger extent than participants. On the other hand there is a considerable group that has not enrolled into Sfi that has a rather weak position on the labor market or depend on social assistance (Kennerberg 2009).[12]

[12] One reason why there is a lack of studies on labor market effects is the inherent problem of finding a suitable comparison group to the participants of Sfi. Kennerberg and Åslund (2010) studied the correlation between participation in Sfi and the later success on the labor market by the use of a matching procedure. They concluded that immigrants who enrolled into Sfi initially had lower employment rates and earnings relative to "comparable" non-

4 The design of the policy pilot

The intention of the policy pilot was to test whether providing economic incentives improve student achievement and/or attract more immigrants to language training courses. Increased language skills were in turn hoped to ease the transition to the labor market (Prop. 2008/09:156). To this end a performance bonus (in Sweden known as Sfi-bonus) was introduced in a limited number of municipalities. The policy pilot was run by the central government, which cooperated with the Institute for Evaluation of Labour Market and Education Policy (IFAU) in designing the experiment.

A selected group of fifty municipalities, based on a "sufficiently" high number of students in the municipality (given the even number of inquiries one may suspect that the cut-off was somewhat arbitrary), were inquired about their willingness to participate in the policy pilot by the Central Government Offices. The municipalities were informed that participants would receive additional funds for quality improvements within Sfi and that a maximum of fifteen municipalities would be entitled the right to pay out performance bonuses. Indicating an interest the municipality agreed to participate, regardless of whether it was assigned to the treatment or to the control group. Thirty-five of the fifty municipalities that received the proposal indicated their interest to participate.

Given this, there were five types of municipalities: (i) municipalities that would be able to pay out performance bonuses and receive additional funds for quality improvements; (ii) municipalities that would receive additional funds; (iii) municipalities that had shown interest in participating, but were not included in the pilot; (iv) municipalities that rejected the proposal; and (v) municipalities that were not offered to participate. By comparing group (i) and (ii) it is possible to evaluate the effect of the bonus. This is the main purpose of the evaluation. Moreover, by comparing outcomes in (ii) with (iii) and (iv) the effect of additional funds can also be evaluated to some extent (however, without the random component).

The next step was to divide the 35 municipalities that had accepted the proposal to participate into group (i), (ii) or (iii). Five broad criteria were used. Firstly, a maximum of 15 municipalities were allowed to pay out performance bonuses. Secondly, group (i)

participants, but that earnings converged after ten years in the country and that employment rates were surpassed by around five percentage points. A strict causal interpretation of the results is not possible however due to methodological constraints. A thorough discussion and various approaches to an empirical analysis can also be found in Riksrevisionen (2008).

and (ii) should include a high and similar number of expected participants. Thirdly, group (i) and (ii) should be roughly comparable in terms of population size, labor market conditions and geography. Fourthly, the whole country should be represented and lastly, the major cities of Stockholm, Göteborg and Malmö (who were all interested in the pilot) should be represented in each of (i), (ii) and (iii).

Following these criteria, pools of pairs of municipalities were constructed. One exception to the sample criteria was that the municipality pair including Stockholm and Göteborg was balanced by adding Uppsala and Södertälje (who both belong to the Stockholm local labor market region) to Göteborg. To meet the criteria of the large cities being represented in group (i) – (iii), Malmö was chosen not to be included in the pilot. Thus, 13 pools of municipalities were constructed to not exceed the maximum limit of 15 municipalities getting the right to pay out the performance bonus. Out of the pool of the 35 municipalities that showed interest to participate, seven municipalities were assigned to group (iii).[13] Finally, within the pairs treatment was assigned randomly (the outcome of the randomization process is discussed below).

4.1 The performance bonus

The municipalities belonging to group (i), the experimental group, were permitted to pay out a performance bonus to students within the Swedish tuition system for immigrants under certain conditions. To be eligible for a performance bonus the following rules applied:

1. The immigrant must have registered as a resident in any municipality for the first time between the 1st of July 2009 and 30th of June 2010 and should have revived a residence permit in accordance with Aliens Act, Chapter 5, paragraph 1, 2, 3, 3a, 4, 5 or 6.
2. The participant in the Swedish tuition system for immigrants should be between 18 and 64 years old.
3. The participant should have received a pass or a pass with distinction on one of the following courses: study path 1, course B; study path 2, course C; or study path 3, course D in one of the treated municipalities.

[13] These municipalities were excluded as no suitable municipality pair was found and because the Government had set the maximum number of municipalities that could implement the bonus scheme to fifteen.

4. The grade should have been received within 12 months after the course start but no later than 15 months after immigration.

The types of residence permits that were required include, in general, permits granted to refugees and family immigrants. Thus, immigrants with work permits and guest students were not eligible. Consequently most immigrants from EEA/EU were not eligible while the majority of the immigrants from other parts of the world fulfilled the requirement[14]. Municipalities that introduced the bonus system were also required to inform newly arrived immigrants about the conditions and requirements surrounding the bonus system.

Regarding the time frame, the average number of weeks between the course start and the date of completion varies across study paths and courses. For immigrants enrolling into Sfi for the first time in 2008 the average number of weeks before receiving at least a pass on any course was 59 weeks. For beginners on the bonus courses, i.e., course B, study path 1, the average was 57 weeks; course C, study path 2, the average was 50 weeks; and for course D, study path 3, 19 weeks (Swedish National Agency for Education 2010B). Note that these averages are not restricted to newly arrived immigrants.

If fulfilling these requirements an immigrant could apply for a bonus payment from the municipality.[15] The application should have been handed in no later than three months after the completion of the course. The size of the performance bonus depended on the course. Bonus courses are highlighted by a circle in Figure 1. Course B, study path 1, yielded a bonus of SEK 6,000; course C, study path 2, gave SEK 8,000; and course D, study path 3, gave SEK 12,000. As a student had the right to progress until course D on study path 3, more than one bonus payment is possible per student. The total amount that a student could receive was however set to SEK 12,000.

4.2 Limitations of the design

From a scientific perspective, there are endless ways of designing a bonus experiment. For practical implementation, however, it has to be politically feasible. While the bonus

[14] Within the group of immigrants from EEA/EU, migrants granted residence permits were eligible, including family migrants applying for permits following the national legislation, but not immigrants with the "right of residence" (uppehållsrätt) that follow EU rules. The latter group dominates the group.
[15] The municipalities were reimbursed by the Swedish National Agency for Education for the payment of the bonuses and its related administrative costs.

scheme provided a rare opportunity of a controlled large-scale design in a politically important and sensitive issue, it had its limitations.

In the process of setting up the pilot, a common objection was that it was unfair to offer the bonus in some locations but not in others. For this reason and for practical purposes, varying treatment across individuals in the same location was not an option. While the relatively few municipalities inquired covered a very large share of the Sfi students, it also means that randomization will only be across a limited number of units. The within-pair randomization was an attempt to make sure that the treatment and control group did not end up being too different. But while the ambition was to create similar matches with respect to the number of Sfi participants, geography and labor market characteristics, the small number of alternatives necessarily made some matches rather poor.

In section 6 we will discuss what the design did and did not bring, and how this affected the choice of empirical specification. First we turn to our data sources.

5 Data sources and descriptive statistics

5.1 Data on the foreign-born population and the participants in language training

The database used for the evaluation contains a rich set of individual level demographic variables as well as information on earnings, employment, and other labor market related indicators. It covers the total population of individuals between 18 and 64 years old during 2006 to 2010 and the content was mainly collected by Statistics Sweden (SCB). In addition, information on country of birth is taken from a population register in which some countries of birth are grouped. We also make use of a register that contain information on participants in Swedish tuition for immigrants (Sfi). This register contains, among other things, individual information on enrollment into courses, course starts, the completion date, reasons for dropping out, and grades received. Finally, a table containing the date of immigration of all individuals that have immigrated since 1985 is used. All registers are linked with a personal identifier.

The sample used in the main empirical analysis is restricted to immigrants who arrived to municipalities that were affected by the reform, i.e., introduced the bonus scheme, or belonged to the control group. We include all foreign-born that immigrated between 2006-07-01 and 2010-06-30, with a few exceptions. Following the criteria for receiving a bonus we restrict the sample to those 18-64 years old. Immigrants from Norway, Finland and Denmark are also excluded as they in general are not eligible for language training courses.[16] The immigration date restriction is set by the first unique immigration date in the table INUT that contains the universe of immigration dates since 1985.

The Stockholm–Göteborg/Södertälje/Uppsala group dominates the sample, making up about sixty percent of the total number of observations. We will therefore present three sets of estimates throughout: estimates for the group containing Stockholm (hereafter referred to as metropolitan areas), other municipalities, and all municipalities. In some instances we will also split the sample in other dimensions, for example, region of origin, gender, and age. All demographic and labor market related characteristics are measured the year of arrival.

Table 1 presents some characteristics of immigrants to the treated and non-treated areas.[17] The statistics refer to the pre-reform period to give a picture of how comparable the groups were prior to the intervention. In metropolitan areas, the immigrants were on average 31 years old, slightly more than half were men, half were married, and a little bit less than a quarter had children under 18 living at home. These demographics are well-balanced across treatment and control. People in the latter group, however, had a weaker economic position upon arrival: a larger fraction received social assistance, and earnings were lower on average. In terms of region-of-origin there are also differences. For example, immigrants from the Horn of Africa and Sudan are overrepresented in the treated areas and immigrants from Iraq are overrepresented in the control group (see columns 2 and 3).

Regarding "other municipalities" the differences between the group that implemented the bonus scheme and the control municipalities are small in terms of demographics and labor market outcomes (columns 4 and 5).

[16] Immigrants from Iceland are also excluded as they are grouped together with Norwegians for confidentiality reasons (see discussion above). This is likely to be a small concern as this is small group.
[17] The full regression sample is found in Table A1 in the appendix.

Table 1 Pre-reform characteristics

	Metropolitan areas		Other municipalities		All municipalities	
	Treated	Control	Treated	Control	Treated	Control
Age	31.26	31.06	31.68	30.91	31.41	30.99
Gender (0=women)	0.54	0.54	0.53	0.51	0.53	0.53
Married or partner	0.46	0.48	0.53	0.52	0.48	0.50
Children under 18 in household	0.21	0.23	0.28	0.26	0.24	0.24
Social assistance	0.13	0.26	0.27	0.27	0.18	0.27
Annual earnings (100's of SEK)	430.27	268.68	284.87	299.47	377.92	282.34
Bosnia and Herzegovina	0.00	0.01	0.01	0.01	0.01	0.01
Former Yugoslavia	0.01	0.03	0.05	0.05	0.03	0.04
Poland	0.10	0.08	0.11	0.10	0.11	0.09
Ireland and UK	0.03	0.02	0.02	0.02	0.03	0.02
Germany	0.00	0.00	0.00	0.00	0.00	0.00
Mediterranean countries	0.05	0.03	0.02	0.03	0.04	0.03
The Baltics	0.03	0.02	0.03	0.02	0.03	0.02
Eastern Europe and former Soviet	0.09	0.08	0.09	0.07	0.09	0.08
Central Europe	0.01	0.02	0.02	0.02	0.02	0.02
France and Benelux	0.04	0.04	0.02	0.02	0.03	0.03
US and Canada	0.02	0.02	0.01	0.01	0.02	0.02
Central America	0.01	0.01	0.01	0.01	0.01	0.01
Chile	0.01	0.01	0.01	0.01	0.01	0.01
South America	0.04	0.02	0.03	0.03	0.04	0.02
Horn of Africa and Sudan	0.08	0.05	0.05	0.06	0.07	0.06
North Africa and Middle East	0.06	0.06	0.06	0.06	0.06	0.06
Sub-Saharan Africa and Egypt	0.03	0.04	0.03	0.03	0.03	0.04
Iran	0.03	0.05	0.04	0.03	0.03	0.04
Iraq	0.08	0.18	0.16	0.16	0.11	0.17
Turkey	0.02	0.02	0.03	0.03	0.02	0.03
East Asia	0.06	0.07	0.04	0.07	0.05	0.07
South East Asia	0.04	0.04	0.06	0.06	0.05	0.05
South Asia	0.08	0.06	0.07	0.05	0.08	0.06
Australia and the Pacific	0.01	0.01	0.00	0.01	0.01	0.01
Not classified	0.00	0.00	0.00	0.00	0.00	0.00
Course start within 3 months	0.29	0.21	0.29	0.25	0.29	0.23
Course start within 6 months	0.41	0.39	0.44	0.43	0.42	0.41
Course start within 12 monts	0.48	0.51	0.54	0.53	0.50	0.51
Passed any course	0.16	0.20	0.23	0.23	0.18	0.21
Passed a bonus course	0.06	0.10	0.09	0.11	0.07	0.10
Passed other course	0.14	0.19	0.21	0.20	0.17	0.19
N	24910	21218	14016	16916	38926	38134

Note: The sample includes all immigrants to Sweden that arrived to Sweden between 2006-07-01 and 2009-06-30, i.e., before the reform, aged 18-64 years old residing in a municipality that implemented the bonus scheme or a control municipality. Immigrants from Norway, Finland and Denmark are excluded as they normally are not eligible for language training programs. All demographic and labor market characteristics are measured the year of arrival. The outcome variables course start within 3/6/12 months refers to time before the first course start within Sfi after immigration. The outcome variables passed any course/a bonus course/other course are set to unity if an individual have completed a course within 15 months after immigration but no longer than 12 months after immigration, i.e., the bonus requirement was fulfilled.

In the empirical analysis we focus on essentially two types of outcomes: the probability of starting a course and the probability of completing a course (varying some of the criteria, for example, time frames and types of courses). The statistics shown in Table 1 reveal that prior to the bonus pilot there was a substantial difference between the treatment and the control group in the fraction starting a course rapidly in metropolitan areas. Six months after immigration, however, the numbers had evened (see also figures A1-A3 in the appendix for a graphical presentation of this pattern). As for the study outcomes, for metropolitan areas, there are differences between the treatment and control group in favor of the control group. In the next section we will address how these differences affect the empirical analysis. For other municipalities the differences in outcomes are small.

While the data are quite rich, they lack two components of interest: (i) residence permit classification that would enable us to more accurately identify bonus eligibility at the individual level; (ii) information on whether the individual applied for and received a bonus. The first restriction means that to the extent that we wish to identify only those eligible, we would need to use some proxy based on country of birth and other variables (in addition to immigration date). This is not a great concern as the main purpose of the evaluation is explore the overall effect of the reform on enrollment and performance, i.e. allowing for crowding-out and spillover effects. The second restriction mostly matters for descriptive purposes and is handled through another data source not linked to the main data (see immediately below).

5.2 Bonus payments

For descriptive purposes information from a database constructed for the experiment and maintained by the Swedish National Agency for Education is used. It contains information on all the performance bonuses that has been paid out since the start of the experiment and a limited set of individual characteristics. A total of 1,005 bonuses were awarded to students fulfilling the criteria for the performance bonus (Table 2). More than half of the bonuses were paid out to students on course D on study path 3, i.e., the most advanced course within the tuition system (see Figure 1).

Table 2 Characteristics of the recipients of the bonus

Municipality	Number of bonuses	Gender (0=women)	Age	Study Path 1, Course B	Study Path 2, Course C	Study Path 3, Course D
Borås	81	0.58	28.43	0.16	0.46	0.38
Halmstad	52	0.48	28.13	0.02	0.17	0.81
Huddinge	57	0.33	30.09	0.02	0.16	0.82
Karlstad	55	0.49	28.78	0.11	0.16	0.73
Katrineholm	27	0.56	30.04	0.00	0.00	1.00
Nacka	51	0.49	28.08	0.04	0.18	0.78
Sandviken	20	0.35	29.35	0.10	0.05	0.85
Sollentuna	93	0.51	30.66	0.25	0.44	0.31
Stockholm	361	0.50	29.86	0.03	0.25	0.72
Trelleborg	9	0.78	25.44	0.00	0.00	1.00
Uddevalla	18	0.39	29.44	0.17	0.06	0.78
Växjö	148	0.49	28.39	0.33	0.44	0.23
Örnsköldsvik	33	0.64	29.73	0.00	0.09	0.91
Total	1005	0.50	29.32	0.11	0.27	0.62

Note: Sample includes all migrants that received a bonus payment and that immigrated between 2009-07-01 and 2010-06-30. Source: Skolverkets Sfi-bonusdatabas, own tabulations.

The distribution of grades across courses shows that a pass was more common than a pass with distinction on all bonus courses (not presented in the table). The mean age of the individuals receiving a bonus was 30 years and the gender distribution was fairly equal. The table also shows that the number of bonus payments varies considerably across municipalities.

6 Empirical analysis

Below we first discuss the choice of empirical strategy based on the design of the pilot and descriptive statistics. This leads to an econometric specification used to retrieve the baseline estimates on enrollment and student achievement. After presenting these results together with an investigation of heterogeneous impacts, we discuss a number of specification checks. Finally, the section contains a tentative analysis of the effects of the extra funds given to the participating (treatment as well as control) municipalities.

6.1 Choosing an empirical strategy

The design of the pilot in combination with the data available gives several options for the empirical strategy. The aim of this section is to outline the arguments guiding this choice.

In a randomized experiment with a sufficiently large number of observation units over which randomization is done, one could simply compare the mean outcomes of interest in the treatment and control groups to get the treatment effect. Table 3 presents statistics for student achievement in the municipal pairs studied (see Table A2 in the appendix for a corresponding presentation of course starts). The pair-wise comparisons in the pilot period show significantly positive differences in four cases, negative in three cases, and no significant difference in six of the municipal pairs. The population-weighted difference presented in the top row of the table suggests no difference in the outcomes.

Table 3 Student achievement, t-test

	Pilot period				Before pilot			
	Mean pass rate treated group	Mean pass rate control group	Diff (Treated-Control)	p-value	Mean pass rate treated group	Mean pass rate control group	Diff (Treated-Control)	Diff=0 (p-value)
All treated-all untreated	.203	.211	–.008	.110	.185	.214	–.029	.000
Stockholm-Göteborg	.180	.191	–.012	.055	.157	.200	–.043	.000
Huddinge-Haninge	.171	.219	–.048	.011	.195	.233	–.037	.001
Borås-Jönköping	.297	.320	–.023	.398	.270	.159	.111	.000
Växjö-Kalmar	.276	.155	.121	.000	.250	.187	.063	.001
Sandviken-Gävle	.298	.252	.046	.227	.351	.325	.026	.281
Nacka-Täby	.088	.177	–.089	.000	.101	.189	–.088	.000
Sollentuna-Solna	.279	.119	.159	.000	.186	.122	.064	.000
Halmstad-Helsingborg	.266	.215	.050	.042	.294	.281	.013	.405
Karlstad-Västerås	.225	.372	–.147	.000	.306	.322	–.016	.405
Trelleborg-Landskrona	.144	.111	.032	.265	.127	.133	–.006	.742
Örnsköldsvik-Härnösand	.421	.429	–.007	.917	.416	.307	.109	.003
Uddevalla-Trollhättan	.453	.289	.164	.000	.380	.247	.133	.000
Katrineholm-Nyköping	.506	.412	.094	.059	.223	.405	–.182	.000

Note: The sample includes all immigrants to Sweden that arrived to Sweden between 2006-07-01 and 2010-06-30, aged 18-64 years old residing in a municipality that implemented the bonus scheme or a control municipality. Immigrants from Norway, Finland and Denmark are excluded as they normally are not eligible for language training programs. The outcome is defined as having passed a course within 15 months after immigration but no longer than 12 months after the course start, i.e., the requirement for receiving a bonus.

If one believed strongly in the outcome of the randomization, this could be it. But making the same comparison in the pre-period casts doubt on such a belief: outcomes were in fact significantly better in the control group prior to the reform. As for the pairs, some of the differences we saw in the pilot period existed already before. There are also examples of substantial changes within control municipalities over time, suggesting that outcomes in Sfi are affected by several factors outside the reform, and probably also includes substantial random variation.

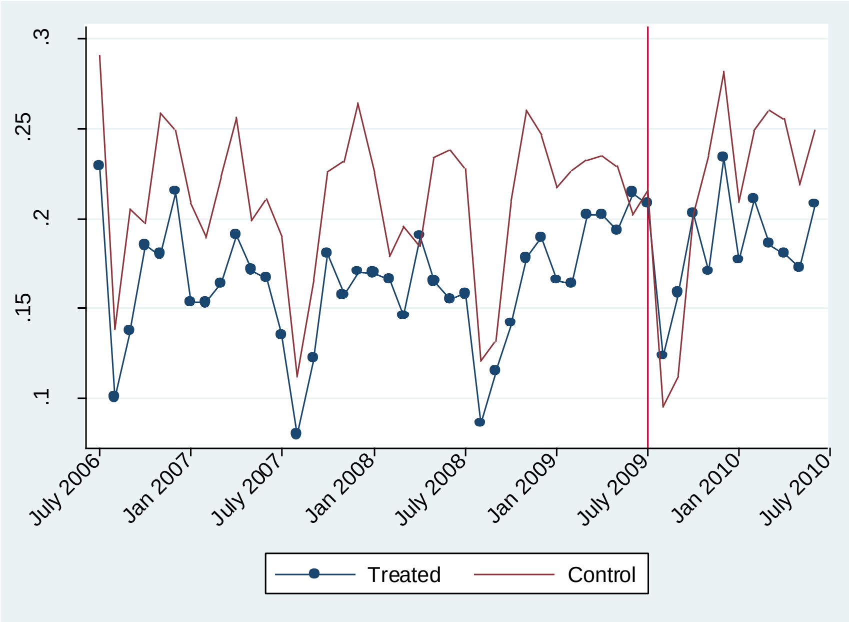
A second alternative is thus to use a regression-discontinuity approach (see Lee and Lemieux 2010), exploiting the fact that treatment (in the sense of being eligible for a bonus) switches from one day to another based on the date of registration. This would mean largely ignoring the control group (and the randomization) and focusing on the potential shift in the treated regions just around the introduction of the bonus. While there may also be principal caveats to such an analysis (treatment effects could, for example, be gradual, and there are possible spillover effects or general equilibrium effects affecting also those arriving shortly prior to the reform), inspection of the data suggests that important conditions for an RD analysis are not met. In our context, the "running variable" along which the RD uses a discontinuity in treatment would be time. From Figure 2 and Figure 3 below it is clear that there is a lot of seasonal variation in the outcome variable along this dimension (this is true also for course starts, se figures A1-A3 in the appendix)[18].

A natural alternative is then to consider a difference-in-differences (DD) approach, i.e., assuming that in absence of the bonus, the average development of outcomes over time is expected to be similar in the two groups. The identifying assumption is then that absent the reform, the development over time around the reform would have been the same in the treated and non-treated areas. While treatment is simply a before-after, the approach allows for controlling for general time effects in a flexible manner (for example, dummies for month of immigration). Given the patterns of Figure 2 and Figure 3, this is important. As will be discussed below, we will investigate the plausibility of the assumptions of the DD approach using several model specifications and specification checks.[19]

[18] Immigrants arriving during the summer months on average have poorer outcomes. This is likely to depend on both the composition of the arriving migrants as well on institutional features. More labor migrants arrive during the summer for seasonal work. This group is less likely to be interested in starting Sfi. Similarly, migrants arriving during the summer holidays are more likely to miss the course starts in autumn as it takes time to screen and place new migrants into Sfi. Furthermore, there is a sharp discontinuity starting in August, which for 2009 means the second month of treatment. Further inspection shows that the number of immigrants as well as their characteristics (gender, age, children) appear to change non-smoothly around the discontinuity. There is a peak in the number of residence permits that are granted in August/September in comparison with June and immigrants arriving the former months are on average younger and a larger share is male. Even though there are techniques for handling some of these problems, our interpretation is that the setting is not appropriate for an RD analysis.

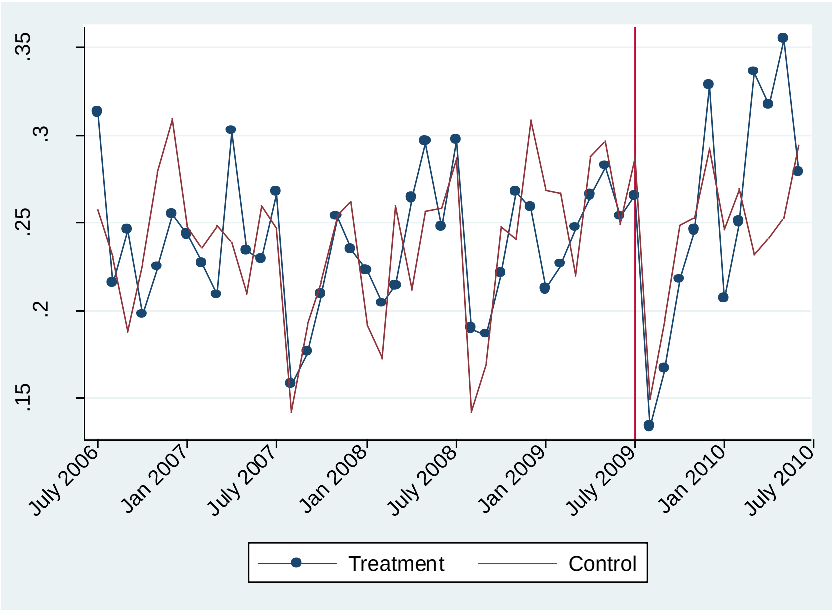
[19] Note that since there is by definition no observed history for the studied population (the clock starts ticking when they register in Sweden for the first time), we are unable to use many of the more flexible estimators (e.g. conditional difference-in-differences) discussed in the literature (e.g. Heckman et al. 1999, and Bergemann et al. 2009).

Figure 2 Metropolitan areas



Note: The figure shows the fraction of the immigrant population arriving between 2006-07-01 and 2010-06-30 that completes any course within 15 months from immigration but no more than 12 months after the course start unconditional on enrollment. The vertical line represents the introduction of the bonus scheme that occurred on the 1st of July 2009.

Figure 3 Other municipalities



Note: See note Figure 2.

Given these considerations it is relevant to ask what the randomization process brings in terms of benefits for the empirical evaluation. First, a major advantage is that selection into the pilot was similar in the treatment and the comparison group; all the municipalities stated their willingness to participate in the trial under the same expectations. Second, within this group it was a random draw that decided treatment status – it was not the most interested among the interested that eventually got to try the bonus. Thus, even though the number of municipalities was not large enough to perfectly balance pre-reform outcomes and covariates, we may arguably be less concerned of selection on potential future outcomes; by definition, there is no self-selection.

6.2 Empirical specification

Based on the discussion above, we specify the baseline model in the following way:

$$\boldsymbol{y_{ijt}} = \boldsymbol{a} + \boldsymbol{X_{it}\beta} + \emptyset_{\boldsymbol{t}} + \boldsymbol{\theta_j} + \boldsymbol{D_{jt}\gamma} + \boldsymbol{\varepsilon_{ijt}} \qquad (\mathbf{1})$$

$\boldsymbol{y_{ijt}}$ is the outcome of interest (course starts or completion of a course with at least a pass). $\boldsymbol{X_{it}}$ is a vector of control variables including age, age squared, gender (0 if a woman), civil status, the presence of children in the household, and country or region of birth. $\emptyset_{\boldsymbol{t}}$ is a vector of immigration month fixed effects, $\boldsymbol{\theta_j}$ is a set of municipality fixed effects. Finally $\boldsymbol{D_{jt}}$ is an indicator taking the value one for immigrants settling after on or after July 1, 2009, in municipalities that were affected by the reform. $\boldsymbol{\gamma}$ is thus the average treatment effect of the reform.

An always-present question in this type of analysis is whether and how to cluster the standard errors. For essentially two reasons, we choose not to cluster in the baseline specification: (i) we do not find it entirely implausible to regard the observations as independent at the individual level; (ii) as discussed below we will to some extent lean on "placebo" regressions (see further discussion below), where a procedure resulting in larger standard errors means a higher risk of disregarding problematic pre-reform patterns. But one could clearly argue that it is more reasonable to assume independence at an aggregated level. We therefore present in the appendix (Table A3 and A6) two alternatives for clustering: (i) municipality; (ii) municipality interacted by immigration month. Alternative (i) captures the units over which randomization is done and is a common choice in the literature (e.g. Angrist and Lavy 2009). Alternative (ii) is a level at which there are reasons to believe more strongly in dependence, e.g. due to people being in the same class and meeting the same labor market opportunities. [20]

In section 6.5 we discuss several robustness checks, including alternative definitions of outcomes and covariates, and restrictions of the sample.

6.3 Effects on enrollment

Table 4 below shows the baseline estimates for course starts. We examine respectively the probability of starting a course within 3, 6, and 12 months after immigration. The estimates are all positive and significant for metropolitan areas, but insignificant and closer to zero for the other municipalities. Through the dominance of the metropolitan areas, the estimates for the overall sample are positive. Taken at face value these estimates indicate a non-trivial positive impact on enrollment in the metropolitan areas.

However, this interpretation is questioned by several robustness checks. A somewhat formal way of testing the plausibility of the model is to run "placebo" regressions pretending that the bonus scheme was implemented on July 1, 2008, i.e., one year prior to the actual reform.[21] The idea is that if we see "effects" where there should be none, one should be cautious when interpreting the main estimates. For metropolitan areas there are indications of an on average more positive development over time than in the comparison areas, while the opposite holds true for other municipalities (see Table A4 in the appendix), which casts doubt on the baseline enrollment estimates. Furthermore, including a group-specific linear time trend (thus allowing for a gradual divergence starting before the reform and assumed to be continuing after), removes also the significance and the size of the estimates for Stockholm (see Table A5).

All in all, these findings suggest that we need to be very cautious in interpreting the enrollment estimates as causal effects of the reform. In fact, there is only one enrollment estimate that comes out of the sensitivity checks more or less unscattered: the positive impact on starting a bonus course within 12 months. It makes sense that an impact at this level is delayed, given that most people do not take bonus courses as their first course. It also seems reasonable that the possibility of earning a bonus may have

[20] In alternative (i) the number of clusters (28) is then smaller than the level recommended by e.g. Angrist and Pischke (2009) to achieve the asymptotic properties. This problem becomes acute when we run separate regressions for metropolitan areas (4 clusters). With (i), the main estimates are marginally significant, with (ii) the level of significance is essentially unaffected.

[21] We have also run placebo regressions pretending that treatment occurred on July 1st 2007, i.e., two year before the real reform limiting the observation window to +/- 12 months around the fictive reform The results are in line with the placebo estimates discussed here.

affected some students to progress in the system. In sum, it is hard to argue strongly that the implementation of the bonus increased or speeded up overall enrollment. At the same time the overall estimates contain too much uncertainty to fully rule out an impact on enrollment.

Table 4 Effects on course starts

	Metropolitan areas		Other municipalities		All municipalities	
	ATE	(S.E.)	ATE	(S.E.)	ATE	(S.E.)
Started within:			Any course			
3 months	0.035***	(0.008)	-0.009	(0.009)	0.020***	(0.006)
6 months	0.026**	(0.008)	0.012	(0.010)	0.021***	(0.006)
12 months	0.032***	(0.009)	-0.011	(0.010)	0.015*	(0.007)
			Bonus course			
3 months	0.001	(0.002)	-0.004	(0.004)	-0.000	(0.002)
6 months	0.006	(0.004)	-0.006	(0.006)	0.002	(0.003)
12 months	0.020**	(0.007)	0.002	(0.009)	0.012*	(0.005)
			Other course			
3 months	0.039***	(0.008)	0.002	(0.009)	0.026***	(0.006)
6 months	0.035***	(0.008)	0.017	(0.010)	0.028***	(0.006)
12 months	0.041***	(0.009)	-0.009	(0.010)	0.021**	(0.007)
N	62589		41918		104507	
			Enrollment rate any course			
3 months	0.26		0.26		0.26	
6 months	0.40		0.43		0.41	
12 months	0.50		0.52		0.51	
			Enrollment rate bonus courses			
3 months	0.02		0.04		0.03	
6 months	0.05		0.08		0.07	
12 months	0.16		0.21		0.18	
			Enrollment rate other courses			
3 months	0.27		0.27		0.27	
6 months	0.38		0.39		0.39	
12 months	0.46		0.49		0.47	

Note: Robust standard errors within parentheses (see Table A3 for clustered standard errors). Sample includes all immigrants to Sweden that arrived to Sweden between 2006-07-01 and 2010-06-30 aged 18-64 years old residing in a municipality that implemented the bonus scheme or a control municipality. Immigrants from Norway, Finland and Denmark are excluded as they normally are not eligible for language training programs. Treatment is defined as the interaction between residing in a bonus-municipality and the immigration period (pre- or post-2009-07-01, i.e., before or after the reform). The outcomes are defined as either having started a course within a quarter of a year (91 days) /half a year (182 days) /a year (365 days) after arrival to Sweden. Controls include the immigration month fixed effects, age, age squared, gender, civil status, children under 18 in household and country of birth. Each cell represent separate regressions.
* <0.05 ** <0.01 *** <0.001.

6.4 Effects on student achievement

We now turn to investigate the effects on student achievement. Our baseline empirical setup is chosen to allow for the reform to affect outcomes through any channel. By starting at immigration, we allow for the possibility that outcomes improve because more/other people register in the courses (although we find no strong support for the latter notion). We also include all immigrants since also the non-eligible may be positively or negatively affected by the existence of a bonus. The performance criterion for receiving a bonus was that one finished a bonus-granting course no more than 15 months after immigration and within a year from the course start.

Table 5 Effects on student achievement

	Metropolitan areas		Other municipalities		All municipalities	
Passed the following course:	ATE	(S.E.)	ATE	(S.E.)	ATE	(S.E.)
Any course (1A-3D)	0.032***	(0.007)	0.005	(0.009)	0.020***	(0.005)
Bonus courses (1B, 2C, 3D)	0.034***	(0.005)	0.007	(0.007)	0.022***	(0.004)
Other courses (1A, 2B, 3C)	0.031***	(0.007)	0.005	(0.009)	0.019***	(0.005)
N	62589		41918		104507	
			Mean pass rates			
Any course	0.18		0.23		0.20	
Bonus courses	0.08		0.10		0.09	
Other courses	0.16		0.21		0.18	

Note: Robust standard errors within parentheses (see Table A6 for clustered standard errors). Outcome defined as having passed a course within 15 months after immigration and 12 months after the course start. See also note Table 4.
* <0.05 ** <0.01 *** <0.001.

Since the introduction of the bonus may have influenced also other courses (e.g. through people being motivated by the bonus to advance faster within the system), Table 5 presents the effect of the bonus system on all courses, bonus-granting courses only and other courses separately. The estimates suggest that the overall effect on student achievement was statistically and economically significant in the metropolitan areas, but literally zero in other treated areas. Since metropolitan areas dominate the sample, the average effect for the overall treatment group is also positive and significant. The fact that courses not qualifying for the bonus were affected is likely to be because most individuals start out in non-qualifying courses; working for the prize of a bonus then more or less requires passing the first course. It could however also be taken to indicate spillover through peer effects or through overall changes in teaching or local institutions. The effect on bonus courses is however twice as large as the effect on other courses evaluated at their respective pass rates.

Table A7 in the appendix shows that for student achievement, the model works quite well. The placebo regressions display that only a few estimates are statistically significant, and overall the magnitude is clearly smaller than in the actual analysis. If anything, the negative placebo estimates outside the metropolitan areas could be indicating that we would underestimate the impact of the bonus. Furthermore, the achievement results are not affected by the inclusion of linear trends (Table A8).

It is quite likely that the effects of a bonus may vary depending on individual characteristics. Table 6 shows estimates for subgroups defined by region of origin, age, gender, income and receipt of social benefits. Splitting the sample by broad region of origin (EEA vs Non-EEA) provides a crude indicator of eligibility as well as expected socioeconomic position; most in the former are not in a residence permit category covered by the bonus, most in the latter are.[22] As can be seen in the table below, the estimated effects for the two origin groups are similar in metropolitan areas. For the other municipalities there is a tendency to an effect for the EEA immigrants, but the estimate is not statistically significant. The mean of the dependent variable is twice as high among the Non-EEA migrants, making effects smaller in the relative sense. This may be surprising considering that a greater fraction in this group were eligible. The placebo estimate for EEA however suggests that we should be somewhat cautious in interpreting these differences. On the other hand, it can be argued that EEA immigrants have on average lower costs for learning the language (for example, a native language closer to Swedish, see also the discussion in section 2.1). Furthermore, the most common type of residence permits for those eligible in this group is based on family ties and it is likely that this group has a particular advantage as they immigrate to someone already living in the country. Worth noting is also that the share of this group in the total sample is relatively small (25 percent of the full sample) and even smaller when

[22] Due to the fact that some countries are grouped together in the data our definition of EEA does not follow the actual definition. The following countries are included: Poland, Ireland, the UK, Germany, Greece, Italy, Malta, Monaco, Portugal, San Marino, Spain, Estonia, Latvia, Lithuania, Slovak Republic, Czech Republic, Hungary, Andorra, Belgium, France, Liechtenstein, Luxemburg, the Netherlands, Switzerland, Austria.

considering only those that have enrolled in Sfi (about 15 percent of all enrolled students).[23]

Table 6 Student achievement – Heterogeneous effects

	Metropolitan areas		Other municipalities		All municipalities	
Passed any course:	ATE	(S.E.)	ATE	(S.E.)	ATE	(S.E.)
EEA	0.034**	(0.012)	0.024	(0.016)	0.031***	(0.009)
Non-EEA	0.029***	(0.008)	-0.000	(0.011)	0.016*	(0.007)
Women	0.033**	(0.011)	-0.001	(0.014)	0.019*	(0.009)
Men	0.031***	(0.008)	0.011	(0.011)	0.021**	(0.007)
18-29 years old	0.045***	(0.009)	0.002	(0.012)	0.026***	(0.007)
30-64 years old	0.017	(0.010)	0.004	(0.013)	0.012	(0.008)
Controlling for income	0.032***	(0.007)	0.004	(0.009)	0.020***	(0.005)
Household on SA	0.039	(0.021)	0.025	(0.022)	0.035*	(0.015)
Household not on SA	0.029***	(0.007)	0.002	(0.009)	0.017**	(0.006)
			Mean pass rates			
EEA	0.10		0.13		0.11	
Non-EEA	0.21		0.27		0.23	
Women	0.22		0.28		0.25	
Men	0.14		0.19		0.16	
18-29 years old	0.19		0.24		0.21	
30-64 years old	0.17		0.22		0.20	
Controlling for income	0.18		0.23		0.20	
Household on SA	0.31		0.37		0.34	
Household not on SA	0.15		0.19		0.16	

Note: Robust standard errors within parentheses. Outcome defined as having passed a course within 15 months after immigration and 12 months after the course start. See also note Table 4.
* <0.05 ** <0.01 *** <0.001.

Table 6 further shows that the impact is larger among younger migrants in metropolitan areas. Notably, these are not on average more likely to complete courses, but appear to be more responsive to the bonus.[24] For men and women, the results are very similar (the estimated relative effect is however greater for men). As we pointed out there are notable differences in labor market outcomes measured the year of arrival between the treatment and control group in metropolitan areas. Splitting the sample depending on whether a household is a recipient of social assistance (SA) shows that the point estimate is larger for individuals that receives social benefits. In relative terms the

[23] Dividing the sample into finer regions of origin (or even separate source countries) largely confirms the baseline picture of a positive impact in Stockholm but not in the other areas. It should however be noted that statistical precision becomes a concern and that there are examples of substantial negative but insignificant point estimates.
[24] In our baseline sample we include immigrants aged 18-64 years old, i.e., the age group that was affected by the implementation of the bonus scheme. Some municipalities however require that participants in Sfi should be at least 20 years old. Otherwise, they are offered language training within high school education programs. To this end we have re-run our analysis setting the age requirement to at least 20 years at time of immigration. This exercise has little effect on our results.

size of the effect of the bonus is similar in the two groups when evaluated at their respective pass rates. Including a control for income from labor; this did not have any effect on the results[25].

6.5 Variations and robustness checks

6.5.1 Time frame for completion

It is interesting to ask whether students only moved the completion date to just before the time limit for eligibility or whether effects are present also for more long-run outcomes. Changing the outcome to a pass or pass with distinction within 18 months after arrival show support for the latter notion (see Table A10), i.e., that it was not just a matter of changing the completion date to fulfill the bonus requirement (18 months is the maximum follow-up for all cohorts studied).

6.5.2 Time-varying effects

We have also investigated the possibility that the impact varies over time. One hypothesis is that the effect would increase with time since implementation as the bonus becomes more known. An opposite idea would say that the immediate impact is bigger than the long-run, as the existence of the bonus becomes an established part of the system, not receiving that much attention. Table A11 shows estimates where we allow the estimate to vary by month of immigration. The somewhat puzzling estimates suggest that the impact in the metropolitan areas is only present in the first four months following the reform, thus lending support to the latter explanation. On the other hand, for the other treatment areas, where the baseline estimates are zero, there are substantial positive estimates for a number of succeeding months later in the pilot period.

One way to view these estimates is to say that since there is no impact in all of the months, we cannot believe in the baseline estimate. On the other hand, if there was no treatment effect and the significant estimates were just outcomes of major but random swings over time, we would expect to see significant differences going in the other direction as well. But we do not.

If one instead is willing to accept the existence of an impact, the question of how to explain the observed time pattern arises. This necessarily becomes speculative. It could be that what we see in the metropolitan areas is an effect of some people responding to

[25] Placebo estimates are found in Table A9 in the appendix.

the news of the bonus and possibly also to feeling "chosen" relative to their peers being in the same classes but non-eligible due to their earlier arrival. To reconcile the pattern in the other locations one could think of a situation where information and institutions respond slower, perhaps in combination with peer effects spurring performance in some narrowly defined cohorts but not in others.

Our reading of the time-varying results is that they underscore the fact that even though there appears to be some effect of the bonus on study achievement, there is a lot we do not understand regarding the mechanisms.

6.5.3 Achievement conditional on enrollment

There are good reasons for focusing on the overall population of recent immigrants, and not just on those enrolling in Sfi. First, the policy aimed to increase language learning in general, which partly could be achieved through increased enrollment. Second, conditioning on enrollment may create sample selection problems if the reform affects who enrolls. However, a conditional analysis has the merit of excluding those who under no circumstances are interested in Sfi.

Table A12 shows that the basic conclusions from above are not altered by restricting the sample to those who actually participated. Point estimates are somewhat larger in Stockholm, and particularly so for bonus courses, but there are no effects in the other regions as in the unconditional analysis. Performing the conditional analysis by subgroups reveals some patterns worth noting (Table A13). The estimated positive impact is more marked among EEA immigrants who actually enrolled, compared to the effect among non-EEA students. This pattern signals the potential importance of how feasible the bonus threshold appears considering the individual's characteristics.

An objection to the bonus scheme has been that it favors the highly educated and those who for other reasons may find it easier to accomplish the goals of the bonus program. If not generally, so at least in terms of the higher amounts given for more advanced courses. Table A13, however, does not suggest that the impact was greater among the highest educated.[26] The point estimates suggest similar effects for people with 0-6 and 7–12 years of schooling, but no significant impact on those with 13 or more years of education. Even though this does not necessarily say anything about

[26] Data on individual education for all cohorts under study are only available through the Sfi register and can thus not be used in the unconditional analysis.

fairness, it indicates that it was not the case that only the most educated were affected by the policy. It should however be noted that some of the placebo estimates suggest that one should be very cautious in drawing firm conclusions in this part of the analysis. It is also quite possible that the selection into participation varies by level of education.

Throughout the analysis, the results for metropolitan areas differ substantially from other participating municipalities. One could speculate that Stockholm's comparatively quick course starts make bonuses more feasible. One way of investigating this possibility is to include controls for time until course start in a conditional analysis. Doing so strongly confirms the idea that an early start is important for completing in time for a (possible) bonus; the pass rates fall sharply month-by-month. Thus, the regulations can be seen as quite strict. The point estimate on treatment for Stockholm is however more or less unchanged for all courses (Table A14). Thus, it is not the case that the effects can be explained by people in Stockholm enrolling quicker after the reform compared to before. To the extent that the shorter enrollment times in Stockholm do play a role, it is more likely to be as a time-constant institutional feature promoting the impact of the bonus.

6.5.4 Strategic behavior – relocation and course choice

The setup of the bonus aimed at granting eligibility to people immigrating to some municipalities but not to others. Only the participating municipalities were instructed to inform about the bonus, and the information stated the requirements similar to section 4.2. Yet, the fine prints of the regulations opened up for gaming the system in the sense that those who moved to a course in a participating municipality were not to be excluded from the bonus. We have therefore checked whether changing to a participating municipality became more common with the reform, which it did not. Neither do the data suggest that people chose to immigrate to bonus municipalities to a greater extent (results available on request).

Another possibility is that the bonus affected course choice. It is hard to tell to what extent students can affect this decision. Many municipalities use tests or conversations to classify the students, but in other cases the procedure is unclear and one cannot rule out the possibility that some individuals acted strategically. To the extent that our outcome variables capture the aims of the bonus in a reasonable way (and thus we ignore the possibility that completing some courses may not be as valuable as completing others), one could argue that the baseline approach is robust to such compositional effects since it includes the entire population of migrants and studies impacts at all types of courses.

6.5.5 Additional specification checks

Even though we control for background characteristics, one could worry that if some groups are more affected by general time effects, imbalances across treatment and coontrol may create a false impression of a treatment effect. This should to some extent show up in the placebo analysis, but we have nevertheless tried re-weighting the sample according to country of origin. The estimates confirmed the baseline results (which are in line with the above-mentioned analysis on finer subgroups based on region of origin) (see Table A15).

Another test performed to confirm our results is to re-run our analysis by excluding covariates altogether. This is a common variation, based on the idea that if the results are unaffected this would to some extent confirm how successful the randomization was. The outcome of this exercise indicates that there are limited imbalances in covariates as the point estimates become slightly smaller, but that the qualitative results are not affected (results available on request). One should, however, note that this type of sensitivity check is less needed as well as less informative, given the randomization that by definition excludes self-selection on unobservables. The fact that pre-reform outcomes and covariates are not perfectly balanced through the randomized allocation does in our setting not raise a concern that, for example, locations which are more ambitious in raising performance are also more likely to exert effort which make them more likely to participate.

6.6 Effects of extra funds

The performance bonus was the major component of the pilot, but the extra funds that were paid out to attract municipalities to participate may also have affected the outcomes. A total of 44 million SEK was paid out by the Swedish National Agency for Education to the municipalities that introduced the performance scheme, in addition to the compensation for administrative costs associated with the bonus system, and to the municipalities in the control group (Swedish National Agency for Education, 2011). The amount that municipalities received was conditional on the number students that were enrolled the period prior to the introduction of the bonus system. The subsidy was intended for quality improvement and the municipalities were free to spend it on whatever they prioritized. Thus, there is variation across municipalities. 65 percent of the extra funds were spent on the requirement of new staff, 20 percent on teaching material and teaching aids, and the rest on various things (Swedish National Agency for Education, 2011).

Table 7 Effects of extra funds on course starts and student achievement

Panel A. Effects on course starts				
	Baseline results		Placebo	
Started any course within:	ATE	(S.E.)	ATE	(S.E.)
3 months	0.039***	(0.005)	0.017*	(0.007)
6 months	0.022***	(0.006)	0.013	(0.008)
12 months	0.007	(0.006)	0.003	(0.008)
Panel B. Effects on student achievement				
Passed any course within:	Baseline		Placebo	
15 months after immigration and 12 months after the course start	0.006	(0.005)	0.016*	(0.007)
N	108094		54803	

Note: The sample include all immigrants to Sweden that arrived between 2006-07-01 and 2010-06-30 aged 18-64 years that resides municipalities that received extra funds, i.e., the control group in the above analysis, and municipalities that declined the offer to participate or were excluded despite an interest to participate. Migrants from Norway, Iceland, Finland and Denmark are excluded. Treatment is defined as the interaction between residing in a municipality that received extra funds and the immigration period (pre- or post-2009-07-01, i.e., before or after the reform). Controls include age, age squared, gender, civil status, children in household, immigration cohort based on the immigration date and country of birth. In the placebo regressions treatment is defined to take place on July 1st 2008 and the sample is restricted to +/- 12 months around this date.
* <0.05 ** <0.01 *** <0.001.

To investigate whether the extra funds had an effect, we ran regressions following the same structure as above, but using the control group as the treatment group. As control group we use all municipalities inquired about participation but not included in the trial. Note, though, that this analysis does not benefit from the randomized trial used in the main analysis. Panel A of Table 7 shows that extra funds appear to be positively correlated with the number of course starts. There is however no evidence of the extra funds having affected student achievement. The placebo estimates are however also significant in some cases (but clearly smaller in the case of enrollment), emphasizing the need for caution in interpreting the results. The results are not sensitive to including linear trends. A tentative conclusion is that there are indications that the extra funds made some municipalities able to receive students at a somewhat higher pace.

7 Conclusions

This paper evaluates the effects of a pay-for-performance scheme within the Swedish language tuition system for adult immigrants. The use of economic incentives to improve student achievement has become increasingly popular among policy makers, practitioners and researchers around the world but this is the first time financial awards are used in the type of setting studied here. A policy pilot run in 2009–2010 gave a randomly assigned group of municipalities the right to grant substantial cash bonuses to recently arrived immigrants meeting certain performance criteria. In short, to qualify for the bonus a student should have passed a bonus qualifying language course within 15 months after arriving to Sweden but no longer than a year after the course start.

The average estimated impact on student achievement is substantial, but driven alone by the metropolitan areas included in the pilot. Thus, the other municipalities were not affected. For the metropolitan areas the effect of the bonus scheme appears to have had a comparable effect across regions of origin. In relative terms the effect is however larger for younger students and for men. The relative estimated impact of the bonus scheme is also greater for immigrants from the EEA/EU in comparison with immigrants born elsewhere. Similar effects are present for groups of different socioeconomic status. A large number of specification tests and robustness checks, which by and large support the baseline conclusion, are also discussed.

We also studied the impact of the performance bonus on enrollment in language courses. The analysis shows no clear impact; the only finding surviving our robustness checks is an increase in the probability of starting bonus courses in a somewhat long-term perspective. These effects are limited to metropolitan areas and consistent with the observation that effects on student achievement were limited to these areas.

A very important issue is why we see a positive impact in Stockholm but not elsewhere. It is possible that there are institutional advantages in larger cities, making it possible to provide teaching and classes in a manner suitable for the bonus requirements. For example, Stockholm has a record of people starting more quickly and a quick start is strongly associated with the probability of completing in time. While it is not the case that the impact can be explained by an increase in early starts seen in Stockholm, it could be that quick enrollment is a factor promoting the impact of the bonus. One should however acknowledge the fact that the risk of there being something else than the bonus scheme driving the result is higher than if we had seen a similar pattern in all the regions. A similar concern is that the impact only seems to have been present in some periods. Taken together there are clearly still much to be understood regarding the mechanisms at work.

On the other hand, such puzzles are perhaps not that surprising given the very mixed empirical evidence in previous studies. Since this is also the first study of performance bonuses for adult education and also for the immigrant population, it is hard to tell what to expect. Our estimates point to potentially very large effects in relative terms. But this is partly because outcomes are not very good to begin with. It does not seem entirely unlikely that the potential of gaining about Euro 1,350 would make three or four out of a hundred students complete their studies faster.

Taking the baseline estimates at face value, a back-of-the-envelope calculation arrives at a cost of about SEK 14,500 (Euro 1,600) per added course completion in Stockholm.[27] We do not have a good estimate of the exact value of completing Sfi, neither in terms of actual language skills nor on its labor market value. However, if one attaches any value to it in terms of shortening welfare dependence and promoting labor market integration (which, e.g., the results in Kennerberg and Åslund 2010 tend to do), the order of magnitude of the cost is relatively modest. This suggests that one would at least like to further understand the mechanisms at work before dismissing the bonus as an irrelevant policy tool.

[27] The number of people entering Stockholm in the pilot period times the estimated increase in the probability: (33690-24910)*.032 = 280.96. The total paid bonuses plus administrative fees is 4,082,800, which divided by 280.96 yields a cost of 14,532 per added completion.

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Appendix

Table A1 Regression sample

	Metropolitan areas		Other municipalities		All municipalities	
	Treated	Control	Treated	Control	Treated	Control
Age	31.28	30.85	31.53	30.86	31.37	30.86
Gender (0=women)	0.54	0.54	0.53	0.51	0.53	0.53
Married or partner	0.46	0.47	0.52	0.51	0.48	0.49
Children under 18 in household	0.21	0.22	0.27	0.25	0.23	0.23
Social assistance	0.13	0.25	0.25	0.26	0.17	0.25
Annual earnings (100's of SEK)	434.05	261.20	274.28	295.51	376.24	276.34
Bosnia and Herzegovina	0.00	0.01	0.01	0.01	0.01	0.01
Former Yugoslavia	0.02	0.03	0.05	0.05	0.03	0.04
Poland	0.09	0.07	0.10	0.09	0.10	0.08
Ireland and UK	0.03	0.02	0.02	0.02	0.03	0.02
Germany	0.00	0.00	0.00	0.00	0.00	0.00
Mediterranean countries	0.05	0.03	0.02	0.03	0.04	0.03
The Baltics	0.03	0.02	0.03	0.03	0.03	0.02
Eastern Europe and former Soviet	0.09	0.08	0.09	0.07	0.09	0.08
Central Europe	0.01	0.02	0.02	0.02	0.01	0.02
France and Benelux	0.04	0.03	0.02	0.02	0.03	0.03
US and Canada	0.03	0.02	0.01	0.02	0.02	0.02
Central America	0.01	0.01	0.01	0.01	0.01	0.01
Chile	0.01	0.01	0.01	0.00	0.01	0.01
South America	0.04	0.02	0.03	0.03	0.04	0.02
Horn of Africa and Sudan	0.09	0.06	0.07	0.07	0.08	0.07
North Africa and Middle East	0.06	0.06	0.06	0.06	0.06	0.06
Sub-Saharan Africa and Egypt	0.03	0.04	0.03	0.03	0.03	0.04
Iran	0.03	0.05	0.04	0.03	0.04	0.04
Iraq	0.07	0.16	0.14	0.14	0.10	0.15
Turkey	0.03	0.03	0.03	0.03	0.03	0.03
East Asia	0.07	0.07	0.04	0.07	0.06	0.07
South East Asia	0.04	0.04	0.06	0.06	0.05	0.05
South Asia	0.08	0.06	0.08	0.06	0.08	0.06
Australia and the Pacific	0.01	0.01	0.00	0.01	0.01	0.01
Not classified	0.00	0.00	0.00	0.00	0.00	0.00
Course start within 3 months	0.30	0.21	0.28	0.25	0.29	0.23
Course start within 6 months	0.42	0.39	0.43	0.42	0.42	0.40
Course start within 12 months	0.49	0.50	0.53	0.52	0.50	0.51
Passed any course	0.16	0.20	0.24	0.23	0.19	0.21
Passed a bonus course	0.07	0.09	0.10	0.11	0.08	0.10
Passed other course	0.15	0.18	0.21	0.20	0.17	0.19
N	33690	28899	19105	22813	52795	51712

Note: Sample includes all immigrants to Sweden that arrived to Sweden between 2006-07-01 and 2010-06-30, i.e., the full regression sample, aged 18-64 years old residing in a municipality that implemented the bonus scheme or a control municipality. Immigrants from Norway, Iceland, Finland and Denmark are excluded. All demographic and labor market characteristics are measured the year of arrival. The outcome variables course start within 3/6/12 months refers to time before the first course start within Sfi. The outcome variables passed any course/a bonus course/other course are set to unity if an individual have completed a course within 15 months after immigration but no longer than 12 months after immigration, i.e., the bonus requirement was fulfilled.

Table A2 Course starts within 6 months after immigration, t-test

	Pilot period				Before Pilot			
	Mean pass rate treated group	Mean pass rate control group	Diff (Treated-Control)	Diff=0 (p-value)	Mean pass rate treated group	Mean pass rate control group	Diff (Treated-Control)	Diff=0 (p-value)
All treated-all untreated	0.421	0.386	0.035	0.000	0.422	0.410	0.012	0.001
Stockholm-Göteborg	0.427	0.383	0.043	0.000	0.411	0.392	0.020	0.000
Huddinge-Haninge	0.393	0.418	-0.025	0.291	0.443	0.415	0.029	0.045
Borås-Jönköping	0.413	0.472	-0.059	0.039	0.394	0.440	-0.046	0.004
Växjö-Kalmar	0.297	0.235	0.062	0.085	0.378	0.404	-0.026	0.228
Sandviken-Gävle	0.444	0.406	0.039	0.365	0.597	0.568	0.029	0.252
Nacka-Täby	0.245	0.430	-0.185	0.000	0.282	0.345	-0.063	0.002
Sollentuna-Solna	0.475	0.293	0.182	0.000	0.414	0.282	0.132	0.000
Halmstad-Helsingborg	0.522	0.339	0.184	0.000	0.525	0.483	0.042	0.015
Karlstad-Västerås	0.406	0.504	-0.098	0.002	0.473	0.505	-0.032	0.111
Trelleborg-Landskrona	0.270	0.216	0.054	0.151	0.321	0.333	-0.012	0.628
Örnsköldsvik-Härnösand	0.600	0.655	-0.055	0.416	0.687	0.593	0.094	0.010
Uddevalla-Trollhättan	0.553	0.405	0.148	0.002	0.595	0.429	0.165	0.000
Katrineholm-Nyköping	0.794	0.566	0.228	0.000	0.692	0.560	0.132	0.000

Note: The outcome is defined as having started a course within 6 months after immigration. The sample includes all immigrants to Sweden that arrived to Sweden between 2006-07-01 and 2010-06-30 aged 18-64 years old residing in a municipality that implemented the bonus scheme or a control municipality excluding immigrants from Norway, Iceland, Finland and Denmark.

Table A3 Effects on course starts – clustered standard errors

	Metropolitan areas			Other municipalities			All municipalities		
	ATE	(S.E.)	[S.E]	ATE	(S.E.)	[S.E.]	ATE	(S.E.)	[S.E]
Started within:					Any course				
3 months	0.035	(0.025)	[0.013]	-0.009	(0.031)	[0.014]	0.020	(0.024)	[0.011]
6 months	0.026	(0.045)	[0.018]	0.012	(0.026)	[0.015]	0.021	(0.026)	[0.013]
12 months	0.032	(0.022)	[0.013]	-0.011	(0.025)	[0.015]	0.015	(0.020)	[0.011]
					Bonus course				
3 months	0.001	(0.008)	[0.003]	-0.004	(0.011)	[0.005)	-0.000	(0.007)	[0.003]
6 months	0.006	(0.018)	[0.006]	-0.006	(0.017)	[0.008]	0.002	(0.012)	[0.005]
12 months	0.020	(0.024)	[0.009]	0.002	(0.022)	[0.012]	0.012	(0.016)	[0.008]
					Other course				
3 months	0.039	(0.018)	[0.014]	0.002	(0.029)	[0.013]	0.026	(0.021)	[0.011]
6 months	0.035	(0.030)	[0.017]	0.017	(0.027)	[0.014]	0.028	(0.021)	[0.012]
12 months	0.041	(0.012)	[0.012]	-0.009	(0.028)	[0.015]	0.021	(0.019)	[0.011]
N	62589			41918			104507		

Note: Robust standard errors clustered on municipalities in parentheses and standard errors clustered within municipalities and immigration month in brackets. See also note Table 4.

Table A4 Placebo regressions – course starts

	Metropolitan areas		Other municipalities		All municipalities	
	ATE	(S.E.)	ATE	(S.E.)	ATE	(S.E.)
Started within:			Any course			
3 months	0.028**	(0.009)	-0.019	(0.011)	0.013	(0.007)
6 months	0.008	(0.010)	-0.032*	(0.012)	-0.006	(0.008)
12 months	0.046***	(0.010)	-0.011	(0.012)	0.023**	(0.008)
			Bonus course			
3 months	-0.001	(0.003)	0.012*	(0.005)	0.005	(0.003)
6 months	-0.004	(0.005)	0.006	(0.007)	0.000	(0.004)
12 months	-0.008	(0.008)	0.004	(0.011)	-0.004	(0.006)
			Other course			
3 months	0.035***	(0.009)	-0.018	(0.011)	0.016*	(0.007)
6 months	0.017	(0.010)	-0.038**	(0.012)	-0.003	(0.008)
12 months	0.048***	(0.011)	-0.011	(0.012)	0.024**	(0.008)
N	31284		21294		52578	
			Enrollment rate any course			
3 months	0.26		0.27		0.26	
6 months	0.39		0.42		0.41	
12 months	0.48		0.52		0.50	
			Enrollment rate bonus courses			
3 months	0.02		0.04		0.02	
6 months	0.05		0.08		0.06	
12 months	0.16		0.21		0.18	
			Enrollment rate other courses			
3 months	0.26		0.28		0.27	
6 months	0.37		0.39		0.38	
12 months	0.46		0.48		0.47	

Note: Robust standard errors within parentheses. Sample includes all immigrants to Sweden that arrived to Sweden between 2007-07-01 and 2009-06-30. Treatment is defined to take place on 2008-07-01. See also note Table 4.
* <0.05 ** <0.01 *** <0.001.

Table A5 Effects on course starts – adding linear time trends

	Metropolitan areas		Other municipalities		All municipalities	
	ATE	(S.E.)	ATE	(S.E.)	ATE	(S.E.)
Started within:			Any course			
3 months	-0.014	(0.012)	0.005	(0.014)	-0.008	(0.009)
6 months	0.001	(0.013)	0.014	(0.016)	0.004	(0.010)
12 months	0.002	(0.013)	-0.023	(0.015)	-0.009	(0.010)
			Bonus course			
3 months	-0.003	(0.004)	0.021***	(0.006)	0.006	(0.003)
6 months	0.001	(0.006)	0.029**	(0.009)	0.010*	(0.005)
12 months	0.026*	(0.010)	0.026	(0.013)	0.024**	(0.008)
			Other course			
3 months	-0.019	(0.012)	-0.006	(0.014)	-0.015	(0.009)
6 months	0.004	(0.013)	-0.001	(0.015)	-0.000	(0.010)
12 months	0.008	(0.013)	-0.039*	(0.016)	-0.012	(0.010)
N	62589		41918		104507	

Note: Robust standard errors within parentheses. See also note Table 4.
* <0.05 ** <0.01 *** <0.001.

Table A6 Effects on student achievement – clustered standard errors

	Metropolitan areas			Other municipalities			All municipalities		
Passed the following course:	ATE	(S.E.)	[S.E.]	ATE	(S.E.)	[S.E]	ATE	(S.E.)	[S.E]
Any course (1A-3D)	0.032	(0.021)	[0.010]	0.005	(0.025)	[0.012]	0.020	(0.016)	[0.008]
Bonus courses (1B, 2C, 3D)	0.034	(0.011)	[0.006]	0.007	(0.019)	[0.009]	0.022	(0.011)	[0.005]
Other courses (1A, 2B, 3C)	0.031	(0.018)	[0.009)	0.005	(0.026)	[0.012]	0.019	(0.015)	[0.008]

Note: Robust standard errors clustered on municipalities in parentheses and standard errors clustered within municipalities and immigration month in brackets. See also note Table 4.

Table A7 Placebo regressions – student achievement

	Metropolitan areas		Other municipalities		All municipalities	
Passed the following course:	ATE	(S.E.)	ATE	(S.E.)	ATE	(S.E.)
Any course (1A-3D)	0.007	(0.008)	-0.012	(0.011)	-0.002	(0.007)
Bonus courses (1B, 2C, 3D)	-0.003	(0.006)	-0.027***	(0.008)	-0.013**	(0.005)
Other courses (1A, 2B, 3C)	0.003	(0.008)	-0.005	(0.011)	-0.001	(0.006)
N	31284		21294		52578	
			Mean pass rates			
Any course	0.17		0.23		0.20	
Bonus courses	0.08		0.10		0.08	
Other courses	0.16		0.20		0.18	

Note: Includes migrants that arrived between 2007-07-01 and 2009-06-30. Treatment defined to take place 1 of July 2008. Outcome defined as having passed a course within 15 months after immigration and 12 months after the course start. See also note Table 4.
* <0.05 ** <0.01 *** <0.001.

Table A8 Effects on student achievement – adding linear time trends

	Metropolitan areas		Other municipalities		All municipalities	
Passed the following course:	ATE	(S.E.)	ATE	(S.E.)	ATE	(S.E.)
Any course (1A-3D)	0.034**	(0.011)	0.003	(0.014)	0.020*	(0.008)
Bonus courses (1B, 2C, 3D)	0.024**	(0.008)	0.025*	(0.010)	0.024***	(0.006)
Other courses (1A, 2B, 3C)	0.042***	(0.010)	-0.002	(0.013)	0.023**	(0.008)
N	62589		41918		104507	
			Mean pass rates			
Any course	0.18		0.23		0.20	
Bonus courses	0.08		0.10		0.09	
Other courses	0.16		0.21		0.18	

Note: Robust standard errors within parentheses. Outcome defined as having passed a course within 15 months after immigration and 12 months after the course start. See also note Table 4.
* <0.05 ** <0.01 *** <0.001.

Table A9 Student achievement – Heterogeneous effects – Placebo regressions

	Metropolitan areas		Other municipalities		All municipalities	
Passed any course:	ATE	(S.E.)	ATE	(S.E.)	ATE	(S.E.)
EEA	0.020	(0.013)	-0.019	(0.018)	0.004	(0.011)
Non-EEA	0.001	(0.010)	-0.009	(0.013)	-0.005	(0.008)
Women	-0.008	(0.014)	-0.024	(0.017)	-0.016	(0.011)
Men	0.017	(0.010)	0.003	(0.014)	0.010	(0.008)
18-29 years old	0.008	(0.011)	-0.031*	(0.015)	-0.009	(0.009)
30-64 years old	0.007	(0.012)	0.008	(0.016)	0.007	(0.010)
Controlling for income	0.006	(0.008)	-0.012	(0.011)	-0.002	(0.007)
Household on SA	-0.001	(0.025)	-0.058*	(0.026)	-0.026	(0.018)
Household not on SA	0.005	(0.009)	0.007	(0.012)	0.004	(0.007)
			Mean pass rates			
EEA	0.10		0.13		0.11	
Non-EEA	0.20		0.26		0.23	
Women	0.22		0.28		0.24	
Men	0.13		0.18		0.15	
18-29 years old	0.18		0.23		0.20	
30-64 years old	0.17		0.23		0.19	
Controlling for income	0.17		0.23		0.20	
Household on SA	0.30		0.36		0.33	
Household not on SA	0.14		0.18		0.16	

Note: Robust standard errors within parentheses. Outcome defined as having passed any course within 15 months after immigration and 12 months after the course start. Treatment is defined to take place on 1st of July 2008. Immigrants immigrating between 2007-07-01 – 2009-06-30 included. See also note Table 4.
* <0.05 ** <0.01 *** <0.001.

Table A10 Effects on student achievement – pass within 18 months after immigration

	Metropolitan areas		Other municipalities		All municipalities	
Passed the following course:	ATE	(S.E.)	ATE	(S.E.)	ATE	(S.E.)
Any course (1A-3D)	0.032***	(0.007)	0.001	(0.009)	0.018**	(0.006)
Bonus courses (1B, 2C, 3D)	0.031***	(0.006)	0.007	(0.008)	0.020***	(0.004)
Other courses (1A, 2B, 3C)	0.034***	(0.007)	0.001	(0.009)	0.020***	(0.006)
N	62589		41918		104507	
			Mean pass rates			
Any course	0.21		0.27		0.24	
Bonus courses	0.10		0.13		0.12	
Other courses	0.19		0.24		0.21	

Note: Robust standard errors within parentheses. Outcome defined as having passed a course within 18 months after immigration. See also note Table 4.
* <0.05 ** <0.01 *** <0.001.

Table A11 Time-Varying effects on student achievement

	Metropolitan areas		Other municipalities		All	
Passed:	ATE	(S.E.)	ATE	(S.E.)	ATE	(S.E.)
Any course (1A-3D)						
July 2009	0.038	(0.023)	-0.026	(0.029)	0.010	(0.018)
August 2009	0.074***	(0.012)	-0.031	(0.017)	0.034***	(0.010)
September 2009	0.088***	(0.015)	-0.020	(0.022)	0.045***	(0.013)
October 2009	0.056*	(0.022)	-0.019	(0.028)	0.028	(0.017)
November 2009	-0.014	(0.023)	-0.006	(0.029)	-0.011	(0.018)
December 2009	0.001	(0.028)	0.031	(0.033)	0.014	(0.021)
January 2010	0.021	(0.023)	-0.028	(0.027)	-0.001	(0.018)
February 2010	0.014	(0.024)	-0.002	(0.031)	0.009	(0.019)
March 2010	-0.010	(0.024)	0.096**	(0.030)	0.032	(0.018)
April 2010	-0.015	(0.024)	0.074*	(0.032)	0.019	(0.019)
May 2010	-0.003	(0.023)	0.070*	(0.031)	0.023	(0.018)
June 2010	0.015	(0.026)	-0.020	(0.032)	-0.000	(0.020)
	62589		41918		104507	

Note: Robust standard errors within parentheses. Outcome defined as having passed a course within 15 months after immigration and 12 months after the course start. See also note Table 4.
* <0.05 ** <0.01 *** <0.001.

Table A12 Effect on student achievement conditional on enrollment within 6 months and placebo

	Baseline estimates			Placebo estimates		
	Metropolitan areas	Other municipalities	All municipalities	Metropolitan areas	Other municipalities	All municipalities
Passed the following course:	ATE (S.E.)	ATE (S.E.)	ATE (S.E.)	ATE (S.E.)	ATE (S.E.)	ATE (S.E.)
Any course (1A-3D)	0.045**	-0.005	0.020	0.002	0.015	0.002
	(0.014)	(0.017)	(0.011)	(0.018)	(0.021)	(0.013)
Bonus courses (1B, 2C, 3D)	0.096***	0.028	0.059***	-0.003	-0.052*	-0.026
	(0.018)	(0.020)	(0.013)	(0.020)	(0.022)	(0.015)
Other courses (1A, 2B, 3C)	0.036*	-0.006	0.014	-0.005	0.028	0.004
	(0.014)	(0.018)	(0.011)	(0.018)	(0.021)	(0.014)
	Mean pass rate			Mean pass rate		
Any course	0.40	0.49	0.44	0.39	0.48	0.43
Bonus courses	0.18	0.22	0.20	0.18	0.21	0.19
Other courses	0.36	0.44	0.40	0.36	0.42	0.39

Note: Robust standard errors within parentheses. Outcome defined as having passed a course within 15 months after immigration and 12 months after the course start. In the placebo regressions migrants immigrating between 2007-07-01 – 2009-06-30 are included and the reform is defined to take place on the 2008-07-01. In the regressions we include dummies indicating time until first course start, defined as months until start. See also note Table 4.

* <0.05 ** <0.01 *** <0.001.

Table A13. Heterogeneous results – conditional on enrollment within 6 months after immigration and placebo estimates

	Metropolitan areas		Other municipalities		All municipalities	
Passed any course:	ATE	(S.E.)	ATE	(S.E.)	ATE	(S.E.)
EEA	0.056	(0.037)	0.031	(0.047)	0.040	(0.028)
Non-EEA	0.039**	(0.015)	-0.006	(0.018)	0.015	(0.011)
Women	0.035	(0.019)	-0.010	(0.023)	0.013	(0.015)
Men	0.057**	(0.020)	0.011	(0.025)	0.031*	(0.015)
18-29 years old	0.058**	(0.019)	-0.007	(0.023)	0.026	(0.015)
30-64 years old	0.032	(0.020)	0.002	(0.025)	0.015	(0.015)
Controlling for income	0.046***	(0.014)	-0.006	(0.017)	0.020	(0.011)
Household on SA	0.068*	(0.027)	0.008	(0.028)	0.044*	(0.019)
Household not on SA	0.033*	(0.016)	-0.008	(0.021)	0.013	(0.013)
0-6 years of education	0.058	(0.034)	0.016	(0.038)	0.033	(0.025)
7-12 years of education	0.065**	(0.023)	0.025	(0.026)	0.044*	(0.017)
13+ years of education	0.002	(0.020)	-0.015	(0.028)	-0.007	(0.016)
			Placebo estimates			
EEA	0.064	(0.045)	-0.014	(0.058)	0.036	(0.035)
Non-EEA	-0.008	(0.019)	0.018	(0.022)	-0.003	(0.014)
Women	-0.011	(0.025)	0.016	(0.028)	-0.002	(0.018)
Men	0.020	(0.025)	0.017	(0.031)	0.010	(0.019)
18-29 years old	0.007	(0.024)	-0.001	(0.030)	-0.002	(0.019)
30-64 years old	-0.001	(0.025)	0.023	(0.029)	0.006	(0.019)
20 years old and older	0.004	(0.018)	0.017	(0.021)	0.005	(0.013)
Controlling for income	0.002	(0.018)	0.017	(0.021)	0.003	(0.013)
Household on SA	0.031	(0.033)	-0.040	(0.033)	-0.005	(0.023)
Household not on SA	-0.008	(0.021)	0.053*	(0.027)	0.011	(0.016)
0-6 years of education	0.113**	(0.042)	0.010	(0.049)	0.065*	(0.031)
7-12 years of education	-0.034	(0.028)	0.033	(0.031)	-0.006	(0.021)
13+ years of education	-0.015	(0.026)	-0.008	(0.033)	-0.014	(0.020)
			Mean pass rates (full period)			
EEA	0.39		0.45		0.42	
Non-EEA	0.40		0.49		0.44	
Women	0.44		0.52		0.47	
Men	0.35		0.45		0.39	
18-29 years old	0.42		0.52		0.46	
30-64 years old	0.38		0.46		0.41	
Controlling for income	0.40		0.49		0.44	
Household on SA	0.43		0.50		0.46	
Household not on SA	0.39		0.48		0.42	
0-6 years of education	0.42		0.41		0.42	
7-12 years of education	0.36		0.48		0.41	
13+ years of education	0.42		0.54		0.46	

Note: Robust standard errors within parentheses. Outcome defined as having passed a course within 15 months after immigration and 12 months after the course start. In the placebo regressions treatment is defined to take place on 1st of July 2008 and migrants immigrating between 2007-07-01 – 2009-06-30 are included. See also note Table 4.
* <0.05 ** <0.01 *** <0.001.

Table A14 Effect on student achievement conditional on enrollment within 6 months – controlling for time before course start and placebo

	Baseline estimates			Placebo estimates		
	Metropolitan areas	Other municipalities	All municipalities	Metropolitan areas	Other municipalities	All municipalities
	ATE (S.E.)	ATE (S.E.)	ATE (S.E.)	ATE (S.E.)	ATE (S.E.)	ATE (S.E.)
Passed any course (1A-3D)	0.039**	-0.005	0.016	-0.006	0.017	-0.001
	(0.014)	(0.017)	(0.011)	(0.017)	(0.021)	(0.013)
Started a course within (ref <1 month):						
1-2 month	-0.062***	-0.020	-0.042***	-0.057***	0.003	-0.029*
	(0.012)	(0.013)	(0.009)	(0.016)	(0.018)	(0.012)
2-3 months	-0.097***	-0.049***	-0.073***	-0.112***	-0.026	-0.072***
	(0.012)	(0.013)	(0.009)	(0.017)	(0.019)	(0.013)
3-4 months	-0.130***	-0.090***	-0.109***	-0.152***	-0.071***	-0.113***
	(0.013)	(0.014)	(0.010)	(0.018)	(0.020)	(0.013)
4-5 months	-0.188***	-0.127***	-0.157***	-0.211***	-0.114***	-0.166***
	(0.014)	(0.015)	(0.010)	(0.019)	(0.021)	(0.014)
5+ months	-0.249***	-0.177***	-0.214***	-0.261***	-0.151***	-0.210***
	(0.014)	(0.016)	(0.011)	(0.020)	(0.023)	(0.015)
	Mean pass rate			Mean pass rate		
Any course	0.40	0.49	0.44	0.39	0.48	0.43

Note: Robust standard errors within parentheses. Outcome defined as having passed a course within 15 months after immigration and 12 months after the course start. In the placebo regressions migrants immigrating between 2007-07-01 – 2009-06-30 are included and the reform is defined to take place on the 2008-07-01. See also note Table 4.
* <0.05 ** <0.01 *** <0.001.

Table A15 Effects on student achievement – weights defined by country/region of birth

	Metropolitan areas		Other municipalities		All municipalities	
Passed the following course:	ATE	(S.E.)	ATE	(S.E.)	ATE	(S.E.)
Any course (1A-3D)	0.028***	(0.007)	0.006	(0.009)	0.017**	(0.005)
Bonus courses (1B, 2C, 3D)	0.033***	(0.005)	0.008	(0.007)	0.020***	(0.004)
Other courses (1A, 2B, 3C)	0.027***	(0.007)	0.006	(0.009)	0.017**	(0.005)
N	62589		41918		104507	
			Mean pass rates			
Any course	0.18		0.23		0.20	
Bonus courses	0.08		0.10		0.09	
Other courses	0.16		0.21		0.18	

Note: Robust standard errors within parentheses. Outcome defined as having passed a course within 15 months after immigration and 12 months after the course start. See also note Table 4.
* <0.05 ** <0.01 *** <0.001.

Figure A1 Course starts within 3 months

Note: The figure shows the share of migrants starting any course within 3 months after immigration.

Figure A2 Course starts within 6 months

Note: The figure shows the share of migrants starting any course within 6 months after immigration.

Figure A3 Course starts within 12 months

Note: The figure shows the share of migrants starting any course within 12 months after immigration.