AKF Working paper

Gabriel Pons Rotger

Contribution of Enforced Language Training to the Labour Market Participation of Family Reunited Migrants

AKF's publication *Contribution of Enforced Language Training to the Labour Market Participation of Family Reunited Migrants* is available at www.akf.dk

AKF, Danish Institute of Governmental Research
Købmagergade 22, DK-1150 Copenhagen K
Phone: +45 43 33 34 00
Fax: +45 43 33 34 01
E-mail: akf@akf.dk
Internet http://www.akf.dk

Publisher: AKF
ISBN: 978-87-7509-993-1
I:\08 SEKRETARIAT\FORLAGET\GPR\2847\2847_ENFORCED_LANGUAGE_TRAINING.DOCX
December 2011

AKF, Danish Institute of Governmental Research
Carries out and reports social science research of interest to the public sector and in particular to regions and local governments.

Gabriel Pons Rotger

Contribution of Enforced Language Training to the Labour Market Participation of Family Reunited Migrants

AKF, Danish Institute of Governmental Research
2011

Preface

Financial support for this research was provided by the Danish Council for Independent Research – Social Sciences (FSE). I am grateful to Helena Skyt Nielsen, Michael Svarer, Beatrice Schindler Rangvid, Nicolai Kristensen, Hans Hummelgaard, Eskil Heinesen and Karl Fritjof Krassel for their comments.

Gabriel Pons Rotger
December 2011

Contents

Summary **7**

1 Introduction **8**

2 Language Training for Family Reunited Migrants **12**

3 Data **15**

4 Econometric Approach **23**

4.1 Identification 23

4.2 Estimation 24

5 Results **28**

6 Sensitivity Analysis **34**

7 Conclusion **38**

References **39**

Appendix: Figures and Table **43**

Dansk sammenfatning **47**

Summary

This paper analyses to what extent non-western family reunited migrants have incentives to adapt their home country skills to the labour market of a receiving developed country. In order to shed light on the strength of such incentives, the paper estimates the causal effect of enforced language training with respect to voluntary training on the assimilation of non-western family reunited migrants in the Danish labour market up to nine years after their arrival. To do so, the paper compares the labour participation and taxable income of two similar cohorts who obtained residence permit around the introduction of compulsory language training, in 1999, by means of the panel regression discontinuity design method.

The paper concludes that family reunited migrants have incentives to transfer their home skills to the destination country since enforced language training does not increase labour participation in the long run, the main challenge of the policy reform, and has a moderate positive effect on the earnings of family reunited migrants.

1 Introduction

Integration of non-western migrants is an important challenge faced by western governments who see the immigrant workforce as a potential remedy to soaring dependency ratios (Bauer, Lofstrom & Zimmermann 2000). However, employment gaps between natives and non-western immigrants are substantial and, in order to enhance participation, many western countries implemented from the late 1990s and 2000s introduction programmes with a more or less mandatory character.

From 1999, Denmark, one of the pioneer countries in this type of policy, applies a programme for all adult non-western newcomers who in this country are mainly family reunited migrants or humanitarian migrants. The introduction programme encompasses brief information on the Danish society, a three-year-long Danish language training and active labour-market policies. Family reunited migrants are, different from refugees, excluded from the active labour-market policies (Liebig 2007). The responsibility for the implementation of the introduction programme is in the hands of the municipalities.

Denmark, like the Netherlands and Finland in the late 1990s, or other European countries during the 2000s, adopted a hard immigration policy approach. Concretely, assistance with language training was required in order to achieve permanent residence in the future or to maintain a standard level of social assistance.[1,2] Family reunited migrants are mostly affected by the future insecure juridical status in Denmark,[3] which is an important threat since the spouses residing in Denmark have permanent residence and economic self-sufficiency upon the arrival of the foreign born partner.

Due to their difficulties to transfer skills and possibly by the foregone activity due to post-migration human capital investment, non-western immigrants are characterised by much lower earnings and employment than comparable natives during their first years in Denmark.

The Danish language is a fundamental barrier for newcomers' participation, even for those characterised with similar education and applicable work experience, such that language acquisition is a necessary condition in order to gain self-sufficiency in the Danish labour market.

There are arguments in favour and against forced integration, and this paper explores empirically whether enforced language training contributes to the assimilation of family re-

---

1 See section 2.

2 In the Netherlands, as in Denmark, active participation is required for granting permanent residence and for the right to a nationality (Euwals et al. 2007). In Finland, participation is "motivated" only by possible reduction or withdrawal of integration benefits in case of absenteeism (Sarvimäki & Hämäläinen 2009). In the 2000s the enforced approach was adopted by Austria, Germany, the UK and Sweden (Entzinger & Biezeveld 2003; Carrera 2006). France's programme allows voluntary enrolment, but participation is mandatory once the foreign born resident has signed the integration contract. Belgium implements voluntary language and introductory courses to all non-Belgium nationals (Carrera 2006).

3 This financial threat primarily affects refugees, who to a higher extent are none self-supported.

united migrants to the Danish labour market with respect to the hypothetical situation where family reunited migrants voluntarily decide on the level of participation.

The theory of adverse ability selection of Borjas (Borjas 1987, 1990, 1992a, 1992b) suggests that immigrants coming from countries with greater income inequality than the destination country are selected from the lower tail of the ability distribution in their sending countries. In this case, enforcing language training is necessary, since otherwise newcomers do not have incentives to make the necessary steps to adapt their skills to the labour market.

In contrast to the ability argument, Chiswick's theory of skills' transferability (Chiswick 1978) claims that limited opportunities in less-developed countries make it worthwhile for individuals to migrate to developed countries due to low opportunity costs of not doing so. Non-western migrants are not necessarily adversely selected, and because they have gone through a process of acquiring human capital in their home countries, they highly benefit from investing in destination country skills, even if these are different from the home skills (Duleep & Regets 1997). This theory predicts that due to high returns to such an investment, non-western foreign born will experience greater employment growth afterwards than comparable natives.

The family investment theory argues, in case of simultaneous migration of partners, that due to liquidity constrains one spouse might take up unskilled employment to finance the family's consumption and the other partner's acquisition of host-country skills (Long 1980; Duleep & Sanders 1993; Baker & Benjamin 1997; Basilio, Bauer & Sinning 2009). In this case, a mandatory programme like the Danish one might be necessary for the partner who takes up the dead-end job. However, the Danish legislation requires that one of the partners has permanent residence in this country in advance, which implies that there is a minimum time lag of six years between the arrivals of both partners. Another important point against the liquidity constrain argument is the fact that the course fee is covered entirely by the municipalities, such that the only family cost of language acquisition is the foregone earnings due to participation.

Finally, different from economic migrants, family reunited migrants are migrants with quite restricted out-migration since their spouses are financially and juridically established in the host country,[4] a situation that might enhance newcomers' incentives to learn the language of the country where they expect to reside permanently (Cortes 2004).[5]

The research question of this paper is therefore to what extent family reunited migrants have incentives to adapt their home country education and work experience to Denmark. Evidence on compulsory training improves participation and earnings will be taken as evidence on lack of incentives, while the absence of relevant effects on labour performance will provide evidence on skills' transferability investment behaviour, since family reunited migrants vol-

4 Given the eligibility conditions for issuing a family reunited migrant permit, one of the spouses must have lived in Denmark for at least six years. In our sample, foreign born spouses had resided an average of 13 years in Denmark when the spouses were reunited. This suggests that in many cases family reunion occurs when the spouse residing in Denmark becomes financially stable (Duleep & Sanders 1993).

5 Family reunited migrants in Denmark present very low out-migration rates (Jensen & Pedersen 2007). In our sample, after nine years in the host country, only about 10% of newcomers have left the country.

untarily invest in language and therefore enforcement is superfluous to this type of migration. In addition, the paper determines whether enforcement increases out-migration in order to determine whether the empirical approach needs to take into account selective out-migration. In case that enforcement increases out-migration, this will indicate that the mandatory character of the programme interferes with the optimal investment plan of newcomers imposing additional effort to learn the language. As far as I know this is the first attempt to study immigrants' incentives through the effect of enforced human capital investment.

In order to shed light on family reunited migrants' incentives, the paper compares participation and taxable income in the host country, for two similar cohorts, which due to the interaction of several new laws and a long administrative process for granting residency were randomly allocated either under an enforced language programme or under a voluntary one. Since the effects of investing in language are fully manifest in the long term, the paper measures outcomes up to nine years after the immigration year (Dustmann & Weiss 2007; Liebig 2007).

The results of this paper are of interest to the more general literature of language effects (Chiswick 1991; Dustman 1994; Carliner 1995; Chiswick & Miller 1992, 1994, 1995; Beenstock 1993; Dustman 1994; Cortes 2004; Dustmann & van Soest 2002; Shields & Price 2002; White & Kaufman 1997). As discussed by Borjas (1994), it is difficult to disentangle language effects from ability, transferability of sending country skills, expected length of residence in the host country (Berman, Lang & Siniver 2003) and other unobservable characteristics.[6] Enforcement effects might be roughly interpreted as the effects of additional language training.

In order to identify the parameter of interest, we compare newcomers who obtained residency at the beginning of 1999, and were subject to enforced language training, with those who were granted residence permit at the end of 1998, and were able to participate in language training without compromising their future residency. Identification is possible due to the fact that the long duration of the residence allowance administrative process (about four-six months long) impedes the individuals who were granted residency immediately after January 1999 to know about the enforcement degree which was revealed on the 28th of December 1998. The practically non-existent out-migration of members of this cohort during the first year in Denmark confirms that family reunited migrants under the introduction programme were not aware of the mandatory element.

The outline of this paper is as follows: Section 2 discusses the selection into enforced language learning. Section 3 describes the dataset. Section 4 proposes a panel regression discontinuity design approach in order to identify and estimate the parameters of interest and in order to deal with selective out-migration. Section 5 presents the empirical results. Section 6

[6] Berman, Lang & Siniver (2003) find that fluency in Hebrew does not affect the wages of Soviet low-skill foreign born residents. Euwals et al. (2007) find that German language proficiency does not contribute to the employment rates of Turkish migrants in Germany. There is evidence that foreign born residents in positions requiring post-secondary education have higher Hebrew ability (Beenstock 1996; Chiswick & Repetto 2001). Basilio, Bauer & Sinning (2009) find for non-western wives who immigrated to Western Germany after their husbands, assimilated both in labour supply and wages without enforced language programme participation.

discusses the robustness of the results and Section 7 concludes. The appendix contains additional figures and a table.

2 Language Training for Family Reunited Migrants

This section describes the selection mechanism into obligatory language training for family reunited migrants. Three laws (the Act on Integration of Aliens, the Act on Danish Language and the Act on Aliens) and a long administrative procedure for granting residence permit determine that close enough to January 1 1999, non-western immigrants are randomly allocated to the new integration policy.

The Integration Act, announced in July 1998, was aimed to increase the economic self-sufficiency of non-western migrants by means of a long introduction programme including language training only. This act supposed a radical change in policy efforts regarding family reunited migrants who were traditionally excluded from the active integration policy. Before January 1999, the counties were responsible for the implementation of Danish language courses. But the Integration Act transferred this competence to the municipalities out of a desire for more coordinated and effective integration efforts.[7] At the same time, the duration of the programme was increased from 1.5 to 3 years.

In the case of family reunited migrants, participation was primarily enforced by linking future permanent residence permits to active participation in the programme. A second enforcement mechanism was the reduction by up to 20% of the welfare benefits in the event of absence from some of the activities of the programme, while refusal to participate results in a complete withdrawal of welfare benefits. The reduction in social assistance only affected a small group of family reunited migrants who lost self-sufficiency.[8]

In the case of family reunited migrants, the municipalities had to draw up an individual plan to choose one among three different levels of language courses. All family reunited migrants who were at least 18 years old when the municipality took over the responsibility for their integration were eligible. Consequently, individuals in the 1999 cohort are subject to introduction measures from the date their individual plans were signed and up to three years after that date. Integration efforts of municipalities are funded by reimbursement and a subsidisation scheme, such that they receive a basic subsidy for each foreign born resident to cover expenses, and an additional subsidy when the three-year programme is completed.

The Act on Integration of Aliens was announced on the 2nd of July 1998 and therefore it is feasible that some immigrants were aware of the potential enforcement nature of the introduction programme.[9] However, the magnitude of enforcement was first made explicit on the 28th of December 1998, such that the foreign born who obtained residence permit during the

---

7 Up to 1999 the municipalities were in charge of activation and housing only.

8 From the 1st of July 2002, all individuals with residence in Denmark were required to have resided in Denmark for a minimum of seven years during the last eight years in this country in order to be eligible for full social assistance; otherwise individuals were only eligible for a lower allowance. This reform affects to the same extent the 1998 and 1999 cohort.

9 Section 52 of the Integration Act states that the Danish Immigration Service and the Refugee Board will collect information on active participation with a view to granting permanent residency. This section is the only reference to a possible enforced language programme.

first 4-6 months in Denmark did not know about the mandatory character of the new introduction programme when they applied for residence permit.

In coordination with the Integration Act, the Act on Danish Language from July 1998[10] aimed to provide foreign born adults with a basic language level in terms of conversation, reading and writing competences. According to this act, from January 1999, municipalities must offer free Danish lessons to *all* adult foreign born residents in the municipality independently on the date of their first residence permit. For those immigrants eligible to the introduction programme, municipalities must offer Danish instruction one month after the municipality has taken the responsibility for their integration. For foreign born not covered by the Integration Act (with residence allowance before the 1st of January 1999), municipalities must offer free Danish lessons within three months after the foreign born resident has submitted an application for lessons.

The duration and conditions for obtaining a participation certificate were contained at an amendment of the Act on Danish as a Second Language for Adult Aliens, announced on the 28th of December 1998. This amendment specified the duration of courses (between 12 and 18 hours a week over a period of three years) and that participation at least at 85% of the lessons was necessary for active participation certificates.[11] Furthermore, the amendment specifies that the language courses must be flexible enough to accommodate those newcomers who found jobs. That is, participation at the introduction programme was compatible with taking up employment.

In contrast to activation measures which were poorly implemented during the first months of 1999, language courses were implemented in a much more satisfactory way. In fact, language teaching took place at 50 schools that have been offering language courses before 1999 under the responsibility of the counties. These schools were spread out among 42 different municipalities.

There is evidence of temporary absenteeism corresponding to about 22% due to maternity leave, sickness or other reasons (Winter 2002), absenteeism of a character that did not compromise the right to permanent residency. It is worth noting that strictly speaking the empirical strategy of this paper identifies the effects of enforcement and not the effects of Danish language lessons, where absenteeism might be seen as part of the effect.

Newcomers excluded from the new Integration Act could decide on the level of participation at free language courses without compromising their future permanent residency. In fact, there are only two differences between the 1998 cohort and the 1999 cohort. First, language training was obligatory for the late cohort and second, the municipality was required to offer language training to the 1998 cohort two months later.

Finally, a third act and a long administrative process determine that newcomers who were granted residence permit just after January 1999 are quite similar to those who were granted residency just before. Concretely, the Act on Aliens establishes that the first resi-

10 Act No. 487 of the 1st of July 1998.

11 The intensity of the instruction was adapted to match the competences of newcomers with a minimum of 12 hours a week for illiterate individuals and 18 hours a week for others.

dence permit is time-limited but issued with a view to permanent residency. Residency grants work permit in the Danish labour market. Entry conditions for family reunited migrant spouses were tightened in July 1998.[12] From summer 1998, resident spouses were required to prove that they could support the newcomer and had to furnish a guarantee for possible future social security payments. In addition, resident spouses were required to have permanent residency in Denmark, this implying that foreign born residing spouses had lived in Denmark for a long time when their partners moved in. In fact, the average number of years after migration for the sample used in our empirical analysis is about 13 years for both the 1998 and the 1999 cohorts. The processing time for a family residence permit is between four and six months, depending on the particularities of each application. In 1999, the Danish Immigration Service granted residence permits for family reunification of spouses in 84% of the cases.

[12] See the amendment Consolidated Act No. 557 of the 30th of July 1998 to the Danish Act on Aliens (announced on 14 August 1998). This act was tightened considerably in 2002 by requiring both resident and non-resident spouses to have a minimum age of 24.

3 Data

This study combines longitudinal data on newcomers and their partners residing in Denmark for the period 1997-2007. Labour participation is measured in terms of a continuous variable which proxies worked hours as a wage earner, the annual contribution to a supplementary pension fund (called ATP Payment). This variable is available for all employees who work at least nine hours a week in Denmark or abroad during a short-term for a Danish employer. Newcomers' annual taxable income is chosen instead of wage earnings in order to take into account self-employed newcomers (about 5%). Longitudinal information on these outcomes is merged characteristics of newcomers upon their arrival in Denmark and to information on their residing spouses covering the years 1997-1998.[13] These covariates are used in the paper to check the validity of the regression discontinuity design approach and some characteristics of the residing partner before migration are used to control for selective out-migration.

We use information on the legal grounds for residency and the date of residence permit supplied by the Immigration Service in order to determine the control and the treated groups and to distinguish family reunited migrants from residing spouses who are not refugees in Denmark – our population of interest. The contents of the introduction programme differ depending on the legal grounds for residency of the newcomer, while the residence permit allowance of the family reunited migrant depends on the legal grounds for the residence of the spouse living in Denmark.

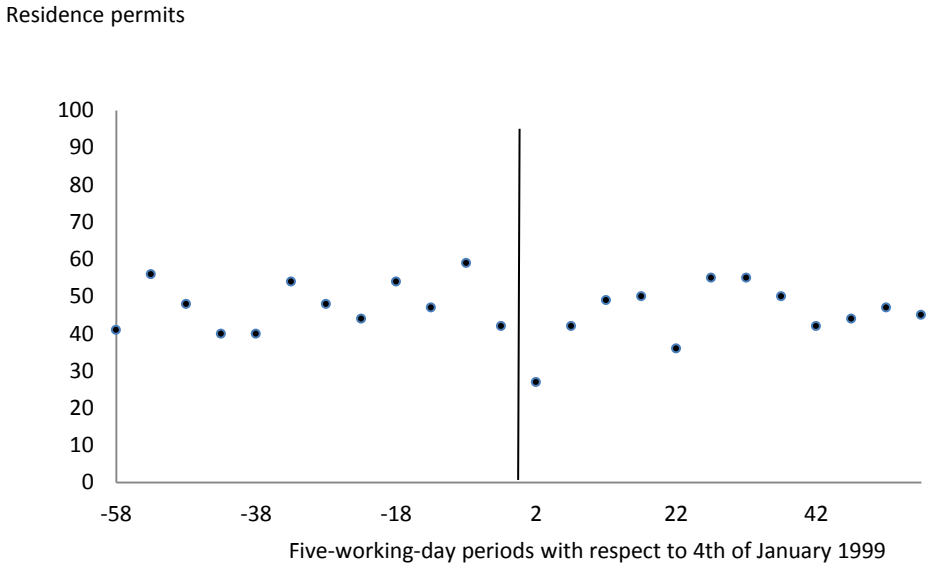
We combine data on the date of arrival and departure in order to restrict newcomers for whom arrival is within at most 60 days of the date of residence permit. We use the date of departure in order to construct out-migration indicators. As is discussed in the empirical section, family reunited migrants are characterised by very low out-migration rates, such that at the end of 2007 about 91% of the 1998 cohort resided in Denmark. However, 1999 newcomers out-migrated to a higher extent than the 1998 cohort particularly in 2000, but also in 2001, suggesting the possibility of some individuals out-migrate as a consequence of the mandatory language course.

The forcing variable in the regression discontinuity design analysis is "Working day with respect to 4th of January 1999". The 4th of January 1999 is the first date where individuals from the 1999 cohort were granted residence permits. Selection is determined by administrative processing time, and it is therefore convenient to measure the forcing variable without weekends or holidays. For example, we do not count 28-31 December 1998 as administrative processing time since no one is granted residence permits between Christmas and New Year's Eve, making the 23rd of December 1998 the last date when individuals from the 1998 cohort were granted residence permits. As seen in figure 3.1, the number of permits issued presents quite a stable pattern when measuring time in terms of working days. This is an important figure for the identification strategy since it shows that the residence permit process is not affected by the new integration act.

[13] See table 3.1 for a complete list of covariates.

As seen in figure 3.1, with the exception of the first five working-day period of 1999, quite a stable number of permits are granted around the policy reform corresponding to about 50 temporary residence permits within each time interval.

Figure 3.1 Residence permits granted to family reunited migrants between 1st of October 1998 and 31st of March 1999



The empirical analysis restricts attention to newcomers between 19 and 57 years of age in 1999. The lower age limit is set in order to avoid including some individuals who at the time the municipality took over the responsibility for integration were not 18 years old yet and therefore not eligible for the obligatory language training. The upper limit is set to 57 years in order to exclude newcomers who are close to the retirement age. In addition, the analysis only considers newcomers whose spouse is at least 19 in 1999, but does not set an upper limit for the spouse's age since eligibility conditions for residence permits and the language programme do not do so.

Table 3.1 reports descriptive summary for the 577 individuals pertaining to the 1998 cohort and the 547 foreign born of the 1999 cohort, who obtained their residence permit within 60 working days from the 4th of January 1999. About 60% of newcomers are females. The average age is 28, reflecting the fact that the inflow is dominated by individuals with short experience at the labour markets of their sending countries. There is a wide dispersion in terms of sending regions, with Turkey being the most common origin with about 28% of newcomers. The second most important sending region is South East Asia (which includes Thailand, Vietnam and the Philippines) with about 18%. It is possible to appreciate minor differences between the control and treated groups in terms of sending regions which reflect the necessary volatility of the inflow from different sending regions within short periods of time. The

1998-1999 cohorts are quite representative of the family reunited migrant spouses who obtained residence permits within the two-year period 1998-1999.[14]

Table 3.1 shows that most of the treated individuals (75%) live in one of the 42 municipalities that host a language centre. Treated and control individuals are quite alike in terms of children in different age groups, and face similar local labour conditions.

It is important to note that regardless of similarities between the first generation immigrant status of the residing spouse, the control and treated newcomers differ in terms of the ethnicity of the residing spouse. Consequently, while 51% of the treated individuals were reunited with an ethnic Danish spouse, only 38% of the control newcomers have an ethnic Danish spouse. This difference is likely to reflect the small sample differences in terms of sending regions. Despite this difference, however, the treated and control individuals are much more similar in terms of labour participation and earnings of the spouse before the arrival of the foreign partner, and as shown in table 3.1, this difference is not detected by the discontinuity test.

Regression discontinuity design identification assumptions stated formally in the next section require that both cohorts are identical in observable and unobservable characteristics for dates of residence allowance very close to the 4th of January 1999. Therefore, it is necessary to check whether there are important discontinuities at the baseline covariates at the time of the policy reform. Column 4 of table 3.1 presents the estimated coefficients and its standard errors of a regression discontinuity design regression where each baseline covariate is used as an outcome variable. As this column illustrates, there are only two variables which seem to present a discontinuity at the time of the policy reform.[15] These are the spouse's age with a 1% significant discontinuity (on average the treated immigrant is 32.96 years old while the control immigrant is on average 32.54 years old) and the spouse's taxable income in 1997 which discontinuity is significant at 10% (the partner of the treated immigrant has on average an income of 155,560 DKK while the partner of the control immigrant had on average an income of 149,629 DKK). Given the absence of discontinuities at the remaining covariates and due to the reduced number of observations and high heterogeneous cohorts, these two disparities do not seem to be enough to invalidate the regression discontinuity design identification approach. The joint test for absence of discontinuities at the entire set of covariates confirms the impression obtained from the individual tests (see the bottom of table 3.1).

---

[14] Concretely, newcomers with residence permits in 1998 or 1999 are on average 28 years old like our narrow defined samples. In terms of sending region, 1998-99 newcomers present also similar distribution in terms of sending region: 0.09 are from East Europe, 0.07 from Ex-Soviet Union countries, 0.23 from Turkey, 0.06 from Latin America, 0.08 from Africa, 0.07 from Maghreb, 0.08 from Pakistan, 0.09 from South West Asia, 0.17 from South East Asia, 0.04 from Central South Asia and 0.04 from East Asia.

[15] See the figures in the Appendix for graphical evidence on regression discontinuity design assumption.

Table 3.1 Baseline covariates for the 1998 and 1999 cohorts in 1999[1]

Covariate	98 cohort	99 cohort	Discontinuity[2]
Female	0.60	0.59	-0.07(0.06)
Age	27.99	27.95	-1.20(0.83)
East European sending country	0.09	0.09	-0.05(0.04)
Ex-Soviet Union sending country	0.07	0.07	-0.01(0.03)
Immigrant from Turkey	0.29	0.27	0.05(0.06)
Latin American sending country	0.04	0.05	-0.01(0.02)
African sending country (excluding Magreb)	0.03	0.09	0.00(0.02)
Maghreb sending country	0.07	0.07	0.02(0.03)
Immigrant from Pakistan	0.09	0.07	0.01(0.03)
South West Asiatic sending country	0.12	0.07	0.02(0.03)
South East Asiatic sending country	0.13	0.18	-0.02(0.05)
Central South Asiatic sending country	0.04	0.03	-0.03(0.02)
East Asiatic sending country	0.03	0.03	0.01(0.02)
Residence close to language centre	0.73	0.75	-0.05(0.05)
Number of children younger than 5 in 1998	0.08	0.07	-0.05(0.04)
Number of children between 5 and 9 in 1998	0.09	0.09	-0.01(0.04)
Number of children between 10 and 16 in 1998	0.11	0.10	-0.02(0.05)
Local unemployment upon arrival	5.73	5.79	-0.21(0.15)
Foreign born partner	0.47	0.40	0.02(0.06)
Ethnic Danish partner	0.38	0.51	0.02(0.06)
Partner's Age	32.54	32.96	-3.65(1.30)***
Partner's years abroad	10.05	8.98	0.14(1.46)
Partner's ATP contribution in 1997	557	553	-87.3(62.2)
Partner's ATP contribution in 1998	638	646	-42.6(62.9)
Partner's ATP cumulated contribution in 1998	4573	5032	-877(644)
Partner's net earnings in 1997	108,939	109,390	-18106(14709)
Partner's net earnings in 1998	129,505	127,450	-17004(14842)
Partner's taxable income in 1997	149,629	155,560	-23274(13311)*
Partner's taxable income in 1998	170,441	172,675	-19736(13224)
Partner is self-employed in 1997	0.06	0.06	0.02(0.03)
Partner is wage earner in 1997	0.56	0.54	-0.06(0.06)
Partner is unemployed at least 6 months in 1997	0.09	0.08	-0.03(0.03)
Partner is student in 1997	0.14	0.12	0.06(0.04)
Partner is retired pensioner in 1997	0.17	0.20	0.01(0.05)
SURE Wald test of covariate discontinuity[3]			35.2 (0.36)
Number of observations	577	547	

1 98 and 99 Cohorts include foreign born who were allowed residence at most 60 workdays away from 4th January, 1999.

2 Discontinuity denotes the t-statistic associated to regression discontinuity design regression of a particular covariate.

3 The SURE Wald test of Covariate Discontinuity is a joint test for absence of discontinuities at the threshold for all covariates (see Lee & Lemieux 2010).

Table 3.2 reports cumulated out-migration rates for both cohorts.[16] As seen in the table the out-migration is low, but it is possible to appreciate a different pattern for both groups from the second year in Denmark. During 1999, there is almost nobody who migrates again. However, the out-migration figures diverge from the second year in Denmark where the

16 No individual from cohort 1998 out-migrates in 1998.

cohort under the introduction programme experiments a relatively remigration rate. Progressively the out-migration rates slightly diverge, such that at the end of the period 9% of cohort 1998 and 13% of cohort 1999 have left Denmark. In spite that these differences are not very large it is important to remind that family reunited migrants are tied migrants, and their out-migration is likely to attract the out-migration of the other partner who had achieved permanent residence and self-sufficiency.

Table 3.2 Permanent out-migration

Out-migration period	98 cohort	99 cohort	Discontinuity
1999	0.002	0.002	0.001(0.002)
1999-2000	0.014	0.040	-0.044(0.023)**
1999-2001	0.037	0.063	-0.064(0.028)**
1999-2002	0.051	0.078	-0.051(0.031)*
1999-2003	0.058	0.089	-0.060(0.033)*
1999-2004	0.073	0.101	-0.058(0.034)*
1999-2005	0.075	0.108	-0.067(0.035)*
1999-2006	0.084	0.123	-0.064(0.037)*
1999-2007	0.094	0.134	-0.053(0.038)
Number of observations	577	547	

In spite that the differences are not very important, it is relevant to check whether the distribution of covariates is still similar for those individuals who remain in Denmark, since otherwise the effects of interest are not identified with standard regression discontinuity design assumptions. Given the low out-migration rates it is unlikely that the remaining immigrants will depart substantially. Tables 3.3 and 3.4 confirm that intuition and show that survivals of both cohorts still are very similar in almost all characteristics, with the exception of partner's age which keeps being slightly higher for control individuals. The only novelty is the discontinuity in terms of number of small children in 1998, which is slightly lower for the treated group.

The reduced size of the samples and the presence of important heterogeneity in certain characteristics imply that discontinuity tests might not be able to detect discontinuities if these are not very important. Table 3.5 reports the means of selected covariates for those individuals in the 1998 and 1999 cohorts who had remained in Denmark from at least 2003 or who had remained in Denmark for all the years covered by the study. As seen in this table, if we compare the characteristics of "survivors" in 2003 and 2007, these present for both the control and the treatment groups fairly similar values in the selected covariates, with the exception of labour-market outcomes before the migration year for the spouses of the treated newcomers. In this case, it is possible to appreciate that ATP payment, cumulated ATP payment and earnings increase slightly for those spouses of treated individuals who remained in the country, while these characteristics remained relatively unchanged for the survivals of cohort 1998. This suggests that enforcement of language training might affect selectively the 1999 cohort, tending to push immigrants from more liquidity constrained families out of Denmark to a higher extent than the 1998 cohort. That is, enforcement tends to enhance out-

migration of those immigrants for whom the mandatory programme might have the biggest impact. Given the low magnitude of out-migration effects, it is not likely that participation results with and without controlling for selective out-migration depart that much.

Table 3.3 Discontinuity test of covariates on the 4th of January 1999 in the years 2000-2003

Covariate	$t = 2000$	$t = 2001$	$t = 2002$	$t = 2003$
Female	-0.05(0.06)	-0.05(0.06)	-0.05(0.06)	-0.05(0.06)
Age	-1.11(0.82)	-1.03(0.83)	-1.11(0.83)	-1.33(0.82)
East European sending country	-0.06(0.03)	-0.04(0.03)	-0.05(0.03)	-0.05(0.03)
Ex-Soviet Union sending country	0.00(0.03)	0.01(0.03)	0.01(0.03)	0.00(0.03)
Immigrant from Turkey	0.03(0.06)	0.02(0.06)	0.02(0.06)	0.03(0.06)
Latin American sending country	-0.02(0.02)	-0.02(0.03)	-0.02(0.03)	-0.02(0.03)
African sending country (excluding Magreb)	0.00(0.02)	0.00(0.03)	0.00(0.03)	0.00(0.03)
Maghreb sending country	0.03(0.03)	0.04(0.03)	0.04(0.03)	0.04(0.03)
Immigrant from Pakistan	0.02(0.03)	0.02(0.03)	0.02(0.03)	0.02(0.03)
South West Asiatic sending country	0.02(0.03)	0.02(0.03)	0.03(0.03)	0.03(0.03)
South East Asiatic sending country	-0.01(0.05)	-0.01(0.05)	-0.01(0.05)	-0.02(0.05)
Central South Asiatic sending country	-0.04(0.02)	-0.04(0.02)	-0.04(0.02)	-0.04(0.02)
East Asiatic sending country	0.01(0.02)	0.01(0.02)	0.01(0.02)	0.01(0.02)
Residence close to language centre	-0.05(0.05)	-0.07(0.06)	-0.07(0.06)	-0.07(0.06)
Number of children younger than 5 in 1998	-0.07(0.04)*	-0.09(0.04)**	-0.08(0.04)	-0.07(0.03)**
Number of children between 5 and 9 in 1998	-0.01(0.04)	-0.01(0.04)	-0.01(0.04)	-0.01(0.04)
Number of children between 10 and 16 in 1998	-0.01(0.05)	0.00(0.05)	-0.01(0.05)	-0.01(0.05)
Local unemployment upon arrival	-0.18(0.15)	-0.21(0.15)	-0.19(0.15)	-0.24(0.16)
Foreign born partner	0.03(0.06)	0.02(0.06)	0.03(0.06)	0.04(0.06)
Ethnic Danish partner	0.02(0.06)	0.01(0.06)**	0.01(0.06)	0.00(0.06)
Partner's age	-3.21(1.28)**	-3.18(1.30)	-3.35(1.30)**	-3.63(1.31)**
Partner's YSM	-1.05(1.14)	-1.66(1.13)	-1.84(1.12)	-1.72(1.13)
Partner's years abroad	-0.09(1.49)	0.13(1.50)	0.41(1.51)	0.39(1.51)
Partner's ATP contribution in 1997	-82.76(61.58)	-86.91(62.27)	-100.79(62.60)	-104.10(62.94)
Partner's ATP contribution in 1998	-32.67(62.29)	-34.72(63.04)	-31.26(63.45)	-38.79(63.86)
Partner's ATP cumulated contribution in 1998	-705(647)	-702(656)	-796(663)	-891(665)
Partner's net earnings in 1997	-15661(14666)	-15534(14903)	-17277(15045)	-18950(15100)
Partner's net earnings in 1998	-14513(14697)	-14535(14917)	-13949(15038)	-16479(15080)
Partner's taxable income in 1997	-18262(13313)	-18942(13526)	-19738(13662)	-21575(13748)
Partner's taxable income in 1998	-16239(13124)	-16562(13324)	-16827(13450)	-19431(13518)
Partner is self-employed in 1997	0.01(0.03)	0.01(0.03)	0.01(0.03)	0.02(0.03)
Partner is wage earner in 1997	-0.07(0.06)	-0.07(0.06)	-0.08(0.06)	-0.07(0.06)
Partner is unemployed at least 6 months in 1997	0.00(0.03)	-0.02(0.03)	-0.02(0.03)	-0.02(0.03)
Partner is student in 1997	0.06(0.04)	0.07(0.04)	0.07(0.04)*	0.08(0.04)*
Partner is retired pensioner in 1997	0.00(0.05)	0.01(0.05)	0.00(0.05)	0.00(0.05)

Table 3.4 Discontinuity test of covariates at the 4th of January 1999 in the years 2004-2007

Covariate	$t = 2004$	$t = 2005$	$t = 2006$	$t = 2007$
Female	-0.05(0.06)	-0.05(0.06)	-0.04(0.06)	-0.04(0.06)
Age	-1.29(0.83)	-1.27(0.83)	-1.20(0.84)	-1.14(0.84)
East European sending country	-0.05(0.03)	-0.05(0.03)	-0.05(0.03)	-0.06(0.03)*
Ex-Soviet Union sending country	-0.01(0.03)	-0.01(0.03)	-0.01(0.03)	0.00(0.03)
Immigrant from Turkey	0.03(0.06)	0.03(0.06)	0.03(0.06)	0.03(0.06)
Latin American sending country	-0.02(0.03)	-0.02(0.03)	-0.02(0.03)	-0.02(0.03)
African sending country (excluding Magreb)	0.00(0.03)	0.00(0.03)	0.01(0.03)	0.01(0.03)
Maghreb sending country	0.05(0.03)	0.05(0.03)	0.04(0.03)	0.04(0.03)
Immigrant from Pakistan	0.02(0.03)	0.02(0.03)	0.02(0.03)	0.02(0.03)
South West Asiatic sending country	0.02(0.03)	0.03(0.03)	0.03(0.03)	0.03(0.03)
South East Asiatic sending country	-0.01(0.05)	-0.01(0.05)	-0.02(0.05)	-0.02(0.05)
Central South Asiatic sending Country	-0.04(0.02)	-0.04(0.02)	-0.04(0.02)	-0.04(0.02)
East Asiatic sending country	0.01(0.02)	0.01(0.02)	0.01(0.02)	0.01(0.02)
Residence close to language centre	-0.08(0.06)	-0.08(0.06)	-0.09(0.06)	-0.09(0.06)
Number of children younger than 5 in 1998	-0.07(0.03)**	-0.07(0.04)*	-0.07(0.04)*	-0.07(0.04)*
Number of children between 5 and 9 in 1998	-0.01(0.04)	-0.01(0.04)	-0.01(0.04)	0.00(0.04)
Number of children between 10 and 16 in 1998	-0.01(0.05)	-0.01(0.05)	-0.01(0.05)	-0.01(0.05)
Local unemployment upon arrival	-0.26(0.16)	-0.26(0.16)	-0.24(0.16)	-0.25(0.16)
Foreign born partner	0.04(0.06)	0.04(0.06)	0.04(0.06)	0.04(0.06)
Ethnic Danish partner	0.00(0.06)	0.00(0.06)	0.01(0.06)	0.01(0.06)
Partner's age	-3.56(1.32)**	-3.51(1.32)**	-3.38(1.33)**	-3.27(1.34)**
Partner's YSM	-1.82(1.12)	-1.70(1.12)	-1.59(1.12)	-1.70(1.12)
Partner's years abroad	0.40(1.51)	0.36(1.51)	0.40(1.52)	0.50(1.52)
Partner's ATP contribution in 1997	-96.54(63.23)	-95.80(63.26)	-99.61(64.01)	-95.46(64.21)
Partner's ATP contribution in 1998	-41.44(64.18)	-41.01(64.25)	-49.57(64.92)	-43.01(65.02)
Partner's ATP cumulated contribution in 1998	-829(669)	-803(670)	-761(677)	-690(678)
Partner's net earnings in 1997	-17799(15073)	-17961(15078)	-19025(15268)	-18715(15331)
Partner's net earnings in 1998	-16826(15025)	-16876(15033)	-18200(15205)	-16959(15281)
Partner's taxable income in 1997	-20822(13719)	-20927(13726)	-21817(13897)	-21591(13976)
Partner's taxable income in 1998	-19551(13459)	-19573(13467)	-19718(13620)	-19229(13709)
Partner is self-employed in 1997	0.02(0.03)	0.01(0.03)	0.01(0.03)	0.01(0.03)
Partner is wage earner in 1997	-0.07(0.06)	-0.07(0.06)	-0.06(0.06)	-0.06(0.06)
Partner is unemployed at least 6 months in 1997	-0.02(0.03)	-0.02(0.03)	-0.03(0.03)	-0.03(0.03)
Partner is student in 1997	0.08(0.04)*	0.07(0.04)*	0.07(0.04)	0.07(0.04)
Partner is retired pensioner in 1997	0.00(0.05)	0.00(0.05)	0.00(0.05)	0.01(0.05)

Table 3.5 Descriptives for newcomers who remained in Denmark in 1999, 2003 or 2007

Covariate	$t = 1999$		$t = 2003$		$t = 2007$	
	98 cohort	99 cohort	98 cohort	99 cohort	98 cohort	99 cohort
Female	0.59	0.59	0.60	0.59	0.60	0.59
Age	27.97	27.98	28.01	28.01	27.91	27.98
East European sending country	0.09	0.08	0.09	0.09	0.10	0.08
Ex-Soviet Union sending country	0.07	0.07	0.07	0.07	0.07	0.07
Immigrant from Turkey	0.29	0.26	0.29	0.25	0.29	0.26
Latin America	0.04	0.05	0.04	0.04	0.04	0.04
African sending country (excluding Magreb)	0.03	0.09	0.03	0.09	0.03	0.09
Maghreb sending country	0.07	0.06	0.07	0.07	0.07	0.07
Immigrant from Pakistan	0.09	0.07	0.09	0.07	0.09	0.07
South West Asiatic sending country	0.12	0.07	0.12	0.06	0.12	0.07
South East Asiatic sending country	0.13	0.18	0.13	0.18	0.13	0.19
Central South Asiatic sending country	0.04	0.03	0.04	0.03	0.04	0.03
East Asiatic sending country	0.03	0.03	0.03	0.04	0.03	0.04
Partner's age	32.45	32.98	32.49	33.00	32.38	32.95
Partner's ATP contribution in 1997	557	556	563	566	565	576
Partner's net earnings in 1997	109,554	110,909	110,579	112,851	110,588	114,506
Partner's ATP cumulated contribution in 1998	4,560	5,104	4,569	5,233	4,546	5,378
Partner is self-employed in 1997	0.06	0.06	0.06	0.06	0.06	0.06
Partner is wage earner in 1997	0.56	0.54	0.56	0.55	0.55	0.56
Partner is unemployed at least 6 months in 1997	0.08	0.07	0.08	0.07	0.08	0.07
Partner is student in 1997	0.14	0.13	0.14	0.13	0.14	0.13
Partner is retired pensioner in 1997	0.17	0.20	0.16	0.19	0.16	0.19
Number of observations	577	547	544	500	523	475

4 Econometric Approach

This section discusses identification and estimation in presence of selective out-migration from the second year in the host country. Intuitively because enforcement increases out-migration of those individuals of the treated group who will benefit more from the introduction programme, estimates obtained with survivors from both cohorts will tend to underestimate the benefits of the introduction programme for all newcomers.

Enforcement effects are given by the discontinuity in potential outcomes at the assignment threshold z_0 (4th of January 1999).[17] The regression discontinuity design framework makes it possible to estimate the average enforcement effects under minimal assumptions. The main drawback of this approach is that effects are identified only for family reunited migrants who obtain residence permits close to the policy reform in January 1999. However, as mentioned in the previous section, the cohort analysed in the paper is quite representative of the family reunited migrants who obtained residence permits between January 1998 and December 1999.

This study is not the first to use the regression discontinuity design approach to estimate the effects of an immigration policy reform. Rosholm & Vejlinz (2010) and Huynh, Schultz-Nielsen & Tranæs (2010) examine the effects of social assistance reduction on short-run employability of humanitarian migrants, while Pons, Husted & Krassel (2011) consider the effect of the same reform on long-run participation and earnings. Sarvimäki & Hämäläinen (2009) evaluate the effects of the 1999 Finnish introduction programme by comparing individuals who entered the population register around May 1997.

4.1 Identification

The parameter of interest is the average effect of enforcement on income and wage employment participation during the first nine years after migration, denoted

$$\tau_t \equiv \lim_{\varepsilon \to 0} E[Y_t(1) - Y_t(0) | E = 1, Z \in \mathcal{N}_\varepsilon]; \qquad t = 1999, \ldots, 2007$$

where $Y_t(1)$ is the potential outcome in case of an obligatory language course (treatment) and $Y_t(0)$ the potential outcome in case of voluntary participation (control), E is a dummy indicator for enforcement, Z is residence allowance with respect to the 4th of January 1999 in terms of administrative time, and $\mathcal{N}_\varepsilon$ denotes the neighbourhood around the threshold date z_0 (4th of January 1999). τ_t captures the difference in potential outcomes attributable to enforced language training, a parameter that subsumes direct effects of language training on those individuals who, in cases of voluntary language training, would not participate with the same intensity.

[17] See section 3.

It is important to note that because 'comparable' natives and local business conditions are similar across the policy reform, the estimated effects τ_t can be interpreted as the contribution of obligatory language training on the assimilation profile of newcomers, i.e. positive effects should be interpreted as the enforced language courses contributing to reduce differences between newcomers and 'comparable' natives.

The parameter of interest is identified for year 1999 under the sharp RD assumption:

SRDD1: $\{Y_{i1999}(1) - Y_{i1999}(0) \perp E_i | Z_i\}$ for Z_i close to z_0
SRDD2: $Y_{i1999}(0)$ is continuous at z_0

However, out-migration in 2000 and 2001 (the second and third year in the host country and last two years of the obligatory language training) is increased by enforcement, such that due to selective outmigration the estimated effects τ_t under assumptions SRDD1-SRDD2 capture the effects of enforcement for those newcomers who remain in Denmark, which differs from the τ_t associated with all newcomers at the beginning of 1999.

Despite the fact that out-migration rates are very low, those who remained in the host country and who were granted residence permits just after the policy reform have spouses with slightly higher participation and earnings in 1997 compared to the spouses of those 98 cohort individuals who remained in the host country, which suggests that enforcement affects the composition of the remaining family reunited migrants.

In order to identify the effects of enforcement from 2000 to 2007, this paper assumes the availability of a set of covariates of newcomers' spouses measured before migration, W_i, such that (see Frölich, 2007):

W-SRDD1: $\{Y_{it}(1) - Y_{it}(0) \perp E_i | Z_i, W_i\}$ for Z_i close to z_0 and $t = 2000, \ldots, 2007$
W-SRDD2: $Y_{it}(0)$ is continuous at z_0 for $t = 2000, \ldots, 2007$

The covariate set can be partitioned into two groups of variables: $W_i = (W_{1i}, W_{2i})$, such that:

$$Y_{it}(0) \perp W_{2i} | E_i, Z_i, W_{1i}$$
$$S_{it} \not\perp W_{2i} | E_i, Z_i, W_{1i},$$

where S_{it} denotes the survival indicator for individual i at year t, that is W_i includes instruments W_{2i} for out-migration, which allows for control for possible correlation of enforcement E_i with unobservable characteristics of those individuals who remained in the host country.

4.2 Estimation

Obviously, a sample of family reunited migrants in a small country like Denmark close enough to the policy reform is a small sample and therefore only very big effects τ_t can be

detected with the standard cross-section regression discontinuity design regression, even after controlling for observables.

In order to gain precision, the paper proposes to take advantage of the fact that in case of significant effects these are likely to change smoothly:

$$\tau_t \equiv \tau_0 + \tau_1 t + \tau_2 t^2,$$

where the parameter τ_0 captures the time invariant effect, the parameter τ_1 allows a time varying effect, and τ_2 allows a time varying effect growth. The typical assimilation pattern in case of earnings or participation is associated to $\tau_0 < 0$, $\tau_1 > 0$ and $\tau_2 < 0$ with $|\tau_2| \ll |\tau_1|$ (see Beenstock, Chiswick & Paltiel 2010). In this case, lock-in effects due to bigger participation of the individuals of the cohort 1999 than the cohort 1998 will be reflected in negative effects during the first three years. Then when language training ceases, fewer earnings are lost, less time is consumed and the effects of language training might be reflected in faster earning growth and participation. Several years after completion of the enforced language programme, the effects are only due to returns from language investment, and therefore the contribution of enforced language training is lower than during the period immediately after programme completion.

The most important advantage of imposing a quadratic functional form is that time effects for each year are estimated with information on the same individual $i = 1, \ldots, N$ from different years $t = 1999, \ldots, 2007$, and this allows estimating τ_t much more precisely by pooling the regression discontinuity design regressions corresponding to all years, rather than using cross-section information only.

In addition the estimation of τ_t by means of τ_0, τ_1 and τ_2 makes it possible to estimate τ_0, τ_1 and τ_2 even in the case that some τ_t are close to zero which might arise around the completion of the introduction programme if $\tau_0 < 0$ and $\tau_1 > 0$, such that initial lock-in effects are compensated for by positive growth effects or in the case that enforcement has lock-in effects only but not long-run effects.

Under sharp RD assumptions, SRDD1 and SRDD2, τ_0, τ_1 and τ_2 can be consistently estimated with a pooled OLS regression (pool W-SRDD in the tables):

$$Y_{it} = \alpha_{0l} + \alpha_{1l} t + \alpha_{2l} t^2 + \tau_0 E_i + \tau_1 E_i t + \tau_2 E_i t^2 + \beta_l \cdot (Z_i - z_0) + \beta_r \cdot E_i (Z_i - z_0) + W_i' \theta + \epsilon_{it},$$

where $z_0 - h \leq Z_i \leq h + z_0$, W_i is a set of baseline covariates measured before the migration year and ϵ_{it} is an error term. This estimator is labelled pool W-SRDD in the tables. Due to the small sample problem, it is not feasible to pick up the sample too close to the threshold 4th of January 1999. In this case, it is therefore possible that for individuals who were granted residence permits far away from z_0, covariates W_i might be correlated with both the enforcement indicator and the forcing variable Z_i. In these circumstances, it is convenient to include baseline covariates in the pooled regression in order to absorb random variation.

However, under SRDD assumption there should not be big differences between estimates which control or do not control for W_i (see Lee & Lemieux 2010).

In the case of selective out-migration due to enforcement, SRDD1 does not hold, and baseline covariates might be correlated with the enforcement indicator even for individuals who obtained residence permits close to the threshold. In order to control for endogenous out-migration, it is necessary to introduce covariates into W_i that affect out-migration but not outcomes, paralleling parametric selection models (see Vella 1998). Under assumptions W-SRDD1 and W-SRDD2, Frölich (2007) proposes a class of estimators which account for differences in W_i between treated and control groups in a fully non-parametric way. In our empirical application, the following version of the Frölich (2007) estimator is used:

$$\hat{\tau}_s = \frac{\sum_{i=1}^{N}\left(\hat{m}_s^+(W_i, z_0) - \hat{m}_s^-(W_i, z_0)\right)\left(0.1 - \frac{3}{16}u_i\right)K_{h_z}^*(u_i)}{\sum_{i=1}^{N}\left(0.1 - \frac{3}{16}u_i\right)K_{h_z}^*(u_i)}; \quad s = 0,1,2$$

where $\hat{m}_s^+(W_i, z_0) = a_s^+$ and $\hat{m}_s^-(W_i, z_0) = a_s^-$ (s=0,1,2) are non-parametric estimators of the conditional mean, trend and quadratic trend of the outcome at period t for cohorts of individuals who obtained residency immediately after and immediately before the beginning of 1999, respectively. $K_{h_z}^*(u_i) = \left(0.1 - \frac{3}{16}u_i\right)K_{h_z}(u_i)$ with $K(u_i)$ the Epanechnikov kernel and $u_j = \frac{Z_j - z_0}{h_z}$. The factor $\left(0.1 - \frac{3}{16}u_i\right)$ is included in order to achieve $N^{-2/5}$ convergence for any dimension of the covariate set W (see Frölich 2007).

The conditional means and deterministic trends are estimated by pooling non-parametric estimators:

$$(a_0^+, a_1^+, a_2^+) = \underset{a_0^+, a_1^+, a_2^+, b, c}{\operatorname{argmin}} \sum_{t=1}^{T_i}\sum_{j=1}^{N} \mathbf{1}_{Z_j \geq z_0}\left(Y_{jt} - a_0 - a_1 t - a_2 t^2 - b\left(Z_j - z_0\right) - c'\left(W_j - w\right)\right)^2 \omega_j^+$$

$$(a_0^-, a_1^-, a_2^-) = \underset{a_0^-, a_1^-, a_2^-, b, c}{\operatorname{argmin}} \sum_{t=1}^{T_i}\sum_{j=1}^{N} \mathbf{1}_{Z_j < z_0}\left(Y_{jt} - a_0 - a_1 t - a_2 t^2 - b\left(Z_j - z_0\right) - c'\left(W_j - w\right)\right)^2 \omega_j^-$$

where $\omega_j^+ = K_{h_z}(u_j)\prod_{l=1}^{L}\bar{K}(v_{lj})$ for j such that $Z_j \geq z_0$, $\omega_j^- = K_{h_z}(u_j)\prod_{l=1}^{L}\bar{K}(v_{lj})$ for j such that $Z_j < z_0$, with, is the Epanechnikov kernel and $\bar{K}(v_{lj})$ is the fourth order kernel $\bar{K}(v_{jl}) = \left(\frac{6}{70} - 0.2v_{jl}^2\right)K_{h_w}(v_{jl})/\left(\frac{6}{70} - 0.2^2\right)$, where $K_{h_w}(v_{jl})$ and $K_{h_z}(u_j)$ are Epanechnikov kernels with $v_{jl}=\frac{W_{jl} - w_l}{h_w}$, and $u_j = \frac{Z_j - z_0}{h_z}$. The fourth order kernel is obtained from the Epanechnikov kernel with a generalised jack-knifing method (see Schucany 1997). This estimator, contrary t0 standard regression discontinuity design estimators, gives data points closer to the cut-off more importance in terms of controlling for W than observations which are more distant to the threshold. Due to the small sample size, the standard error is estimated with bootstrap method.

Due to the sample design, enforcement is uncorrelated with both observables and unobservables of individuals remaining in the host country as long as enforcement itself does not affect out-migration. If enforcement does affect out-migration, we cannot be sure that for the population of family reunited migrant spouses remaining in the host country, enforcement is uncorrelated with observable and unobservable characteristics which might affect labour outcomes in the host country. Table 3.5 suggests that the treated group remaining in the country might include in a major extent than the control group individuals for whom enforcement is not necessary, and therefore pooled W-SRDD will underestimate the intended effects of enforcement.

The previous step to dealing with selective out-migration is obviously to test whether enforcement affects out-migration. A simple way to do so is to estimate the enforcement effects on S_{it}, a dummy indicating whether an individual is observable at year t in the host country ($S_{it} = 1$), or not ($S_{it} = 0$). Variable S_{it}, is observable for all newcomers every year and therefore enforcement effects on S_{it} can be estimated under SRDD assumption with OLS estimation of the following regression:

$$S_{it} = \gamma_{lt} + \psi_t E_i + \upsilon_{lt} \cdot (Z_i - z_0) + \upsilon_{rt} \cdot E_i(Z_i - z_0) + W_i'\varphi_t + \eta_{it},$$

The parameter ψ_t captures the *total* effects of enforcement on out-migration at year t for all participants, which includes the direct effects of enforcement on the out-migration of individuals at year t and the indirect effects of enforcement through its effects on out-migration in previous years $t-1$,...,1999.

Direct effects of enforcement on out-migration can be estimated by two different ways. First, a simple way to do so is to estimate the enforcement effect on out-migration conditional on $S_{it-1} = 1$ with:

$$S_{it} = \delta_{lt} + \pi_t E_i + \rho_l \cdot (Z_i - z_0) + \rho_r \cdot E_i(Z_i - z_0) + W_i'\kappa_t + \eta_{it}, \text{ for } S_{it-1} = 1$$

Due to the presence of selective out-migration from 2000, the estimates π_t from year 2001 can be biased, but given the low out-migration rates we do not expect substantial bias. Alternatively, direct effects from 2001 can be estimated by applying a Frölich estimator t0 the cross-section SRDD (see Frölich 2007) without conditioning on survivors. Since the method controls non-parametrically for newcomers, differences in terms of W_i are allowed to be correlated to E_i, due to selective out-migration.[18]

The number of periods in which direct effects π_t are found significant will determine the minimum number of instruments necessary to control for selective out-migration.

[18] Under the SRDD1 assumption, observable and unobservable characteristics are uncorrelated with enforcement for individuals close to the threshold, such that when estimating the out-migration effects, controlling for W_i in a fully non-parametric way, we allow for any kind of distributional assumption regarding the outcome and selection errors (see Vella 1998).

5 Results

This section presents the estimates of the enforcement effects. The results are obtained for a bandwidth of 60 workdays with respect to the 4th of January 1999. 60 workdays cover approximately three months of calendar time, which is a lower limit for the processing time necessary for granting permits for family reunited migrant spouses.

Tables 5.1 and 5.2 present the total and direct effects of enforcement on out-migration, i.e. the effects on cumulated out-migration and on conditional out-migration. The set of baseline covariates W_i include information on both newcomers and their spouses. Essentially, all observable characteristics described in table 3.1 are included, where *Turkey* is the excluding category for sending region dummies, and *Spouse is Wage Earner* in 1997 is the excluded category in terms of the spouse's socio-economic status in 1997. Table 5.1 reports estimates of total enforcement effects on out-migration rates between 2000 and 2007. The first estimator, SRDD, does not control for observable characteristics, while the W-SRDD controls for all these covariates by including them in the regression. The results suggest that enforcement increases out-migration in 2000 by about 4%. The total effect of enforcement on out-migration in 2001 is about 6%, suggesting that enforcement might push participants to leave Denmark during the second and third years of the integration programme. The total effects of enforcement after 2002 take similar values or are even lower than in 2002, this suggests that enforcement has no effect on out-migration once the participants have completed the language programme or that it may even contribute to retaining participants in the host country.

Table 5.2 reports direct enforcement effects on out-migration obtained with both the Frölich method and standard regression discontinuity design from the conditional out-migration model. The Frölich estimator controls non-parametrically for all continuous covariates, which include newcomer's age, spouse's age, spouse's years outside Denmark (which is obtained combining spouse's age and spouse's years since migration where applicable, and is therefore defined for all newcomers), spouse's ATP payments in 1997 and 1998, spouse's earnings in 1997 and 1998, spouse's taxable income in 1997 and 1998, spouse's cumulated ATP payments in 1998, which approximates the spouse's experience as wage employed in the Danish labour market, and local unemployment rates for the county of residency upon migration.

The discrete covariates include dummies for gender, sending region, ethnic background of the spouse, indicators for residence in a municipality which hosts one of the 50 language centres, dummies for if spouse was self-employed in 1997, if spouse was unemployment for more than 6 months, if the spouse was a student in 1997 and if the spouse was retired from the labour force in 1997, and finally the number of children between 0-4 years in 1998, number of children between 5-9 years in 1998 and number of children between 10-16 years in 1998. These discrete variables are controlled for by residualising the outcome variables and then using the residuals as outcome variables in the Frölich method. The continuous variables are standardised. Frölich (2007) does not provide a clear rule for determining the bandwidth for covariates, and we choose as a starting bandwidth a rather broad bandwidth of 10

for all continuous variables which are standardised. The sensitivity of the results to the different bandwidths is discussed in the next subsection.

Resident foreign born spouses of family reunited spouses have already resided on average 13 years in Denmark, and therefore, it is reasonable to assume the family investment hypothesis away (Baker & Benjamin 1994, 1997). The spouse's age and years outside Denmark are used as instruments, conditionally on the other observable baseline covariates, including spouse's labour-market experience in the host country. In addition to controlling for a number of children aged between 0-4, 5-9 and 10-16 in 1998, we control for the growth rates of the resident spouse's wage employment level, earnings and income, which given the levels of these variables in 1998, are not likely to be correlated with outcomes for spouses later on. We also control for a wide range of confounding characteristics of both spouses measured before the migration year.

These estimates confirm that enforcement directly increases out-migration in 2000 by about 4%. Results for 2001 are borderline insignificant while subsequent effects are insignificant. It is important to note that Frölich estimates depart from the estimates reported in columns 1 and 2, suggesting that controlling for the wide set of spouse's characteristics goes a long way to controlling for selective out-migration.

Table 5.1 The total effects of enforced language training on out-migration

Treatment effect	SRDD	W-SRDD
τ_{2000}	-0.044 (0.023)**	-0.038 (0.023)*
τ_{2001}	-0.064 (0.028)**	-0.063 (0.029)**
τ_{2002}	-0.051 (0.031)*	-0.046 (0.032)
τ_{2003}	-0.060 (0.033)*	-0.057 (0.034)*
τ_{2004}	-0.058 (0.034)*	-0.054 (0.036)
τ_{2005}	-0.067 (0.035)*	-0.063 (0.036)*
τ_{2006}	-0.064 (0.037)*	-0.061 (0.038)
τ_{2007}	-0.053 (0.038)	-0.053 (0.039)
Assumption	$S_{it}(1) - S_{it}(0) \perp E_i \mid Z_i$	

Due to the reduced magnitude of out-migration and the minor differences in terms of the characteristics of those who remain and those who initially arrive in Denmark, we do not expect important differences between estimates after control for selective migration and estimates for individuals who do not migrate. However, the fact that enforcement already affects out-migration in 2000 suggests the necessity of addressing selective migration, since this is likely to introduce attrition bias in our estimates (Wooldridge 2003; Lubotsky 2007; Borjas & Bratsberg 1996).

Table 5.2 The direct effects of enforced language training on out-migration

Treatment effect	Frölich	SRDD	W-SRDD
τ_{2000}	-0.047 (0.025)*	-0.045 (0.022)**	-0.038 (0.023)*
τ_{2001}	-0.029 (0.021)	-0.021 (0.018)	-0.026 (0.019)
τ_{2002}	-0.012 (0.018)	0.013 (0.014)	0.016 (0.016)
τ_{2003}	-0.016 (0.014)	-0.011 (0.013)	-0.014 (0.013)
τ_{2004}	-0.006 (0.014)	0.002 (0.013)	0.001 (0.013)
τ_{2005}	-0.004 (0.006)	-0.010 (0.005)*	-0.009 (0.005)*
τ_{2006}	-0.004 (0.017)	0.002 (0.015)	0.002 (0.015)
τ_{2007}	-0.007 (0.0014)	0.011 (0.011)	0.006 (0.012)
Assumption	$S_{it}(1) - S_{it}(0) \perp E_i \mid Z_i, W_i$	$S_{it}(1) - S_{it}(0) \perp E_i \mid Z_i, S_{it-1} = 1$	

Table 5.3 presents the results for the effects of enforced language training on wage employment participation, which is measured in terms of ATP contributions. ATP payment is a reliable measurement for time employed as wage earner. For full employment for the entire year, the ATP contribution was about DKK 2,683 in 2010. Table 5.3 shows that all parameters determining period effects are highly significant, but with economic insignificant values. As seen in this table, estimates obtained under the SRDD assumption tend to produce slightly higher negative entry effects, but also slightly higher growth effects measured by τ_1. The estimates which correct for selective out-migration in the last column depart from the first two columns but still the value of the estimates is very low to generate economically significative effects on participation. As predicted by the discussion in the previous sections, estimates that do not control for selective out-migration tend to overestimate participation effects in the long-run.

Table 5.3 The effects of enforced language training on ATP contributions (DKK 2010)

	Pooled SRDD	Pooled W-SRDD	Pooled Frölich
Treatment effect	Coef. (SE)[1]	Coef. (SE) [1]	Coef. (SE) [2]
τ_0	-171.98 (35.85)***	-164.85 (33.99)***	-127.42 (55.40)**
τ_1	56.56 (16.01)***	54.91 (15.15)***	46.48 (19.56)**
τ_2	-4.76 (1.65)***	-4.56 (1.56)***	-3.74 (1.75)*

1 Heteroskedasticity-robust standard errors.

2 Bootstrap standard errors. Bandwidth is 60 working days.

Figure 5.1 shows the period effects for both the pooled W-SRDD estimator and the pooled Frölich estimator. Both lines present almost equivalent time evolutions with very moderate growth during the first 4-5 years, and deceleration of this growth afterwards. It is worth noting that despite period effects being insignificant for all periods with the exception of the first year, the underlying parameters τ_0, τ_1 and τ_2 are highly significant.

Table 5.4 presents the results for the effects of enforcement on taxable income. The results show similarly to the participation results that all three underlying parameters are highly significant and with values and signs corresponding to assimilation pattern. However, differently from participation effects, both lock-in effects during the first year and positive effects during the last ones are significant in economic terms. As shown in figure 5.2, the effects for those who stay in Denmark are lower and tend to vanish in the long-run, while lock-in effects are obviously similar, since there are few newcomers that out-migrate during the first years in Denmark. Enforced language training increases newcomer's income from as early as 2002, and the positive effects remain in the long-term. In this case the results obtained when controlling for selective out-migration show that the taxable income of the enforced cohort is about DKK 12,000-19,000 higher than for the cohort that participated voluntarily in the language programme, and that the effect seems to peek during 2006. The estimates that do not control for selective out-migration show that income effects are at the most about 10,000 DKK and start to decline already in 2005. Time effects estimated with Frölich estimator, for all cohorts, and with Pooled W-SRDD for those who do not out-migrate are shown in table 5.5.

The fact that those who stay from cohort 1999 have spouses with higher earnings and experience before migration suggests that out-migrants might be among those newcomers who potentially have benefited more from enforced language training.

Table 5.4 The effects of enforced language training on taxable income (DKK 2010)

	Pooled SRDD	Pooled W-SRDD	Pooled Frölich
Treatment effect	Coef. (SE)[1]	Coef. (SE)[1]	Coef. (SE)[2]
τ_0	-39,603 (7,593)***	-39,095 (7,054)***	-37,301 (10,758) ***
τ_1	15,078 (3,406)***	14,420 (3,580)***	14,706 (3,896) ***
τ_2	-1095 (354)**	-1031 (330)***	-951 (389) ***

1 Heteroskedasticity-robust standard errors.

2 Bootstrap standard errors. Bandwidth is 60 working days.

Table 5.5 Frölich estimation of period enforcement effects

Treatment effect	ATP contribution	Taxable income (DKK 2010)
τ_{1999}	-84.68	-23,546
	(43.71)*	(9,880)**
τ_{2000}	-49.42	-11,693
	(38.39)	(9,748)
τ_{2001}	-21.64	-1,742
	(36.33)	(10,164)
τ_{2002}	-1.34	6,307
	(36.23)	(10,582)
τ_{2003}	11.48	12,454
	(37.10)	(10,797)
τ_{2004}	16.82	16,699
	(38.71)	(10,781)
τ_{2005}	14.68	19,042
	(41.59)	(10,666)*
τ_{2006}	5.06	19,483
	(46.71)	(10,752)*
τ_{2007}	-12.04	18,022
	(55.05)	(11,482)

Note: Bootstrap standard errors. Bandwidth is 60 working days.

Figure 5.1 The effects of enforced language training on annual ATP contribution

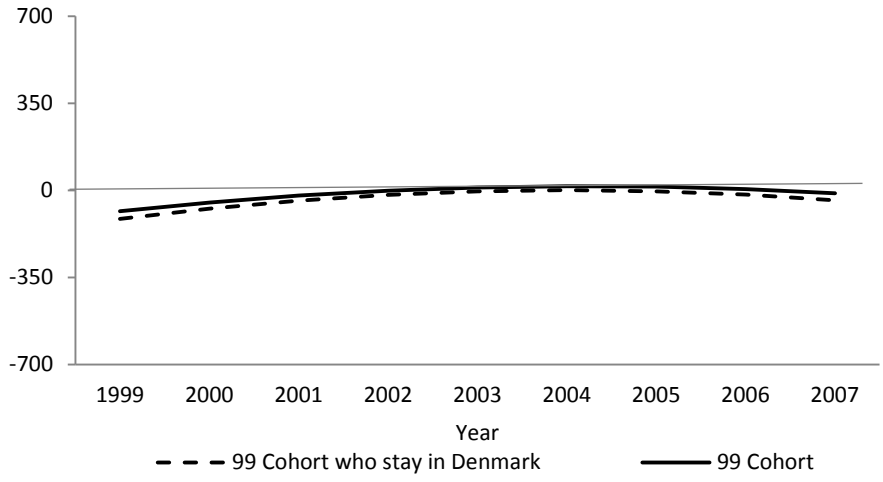
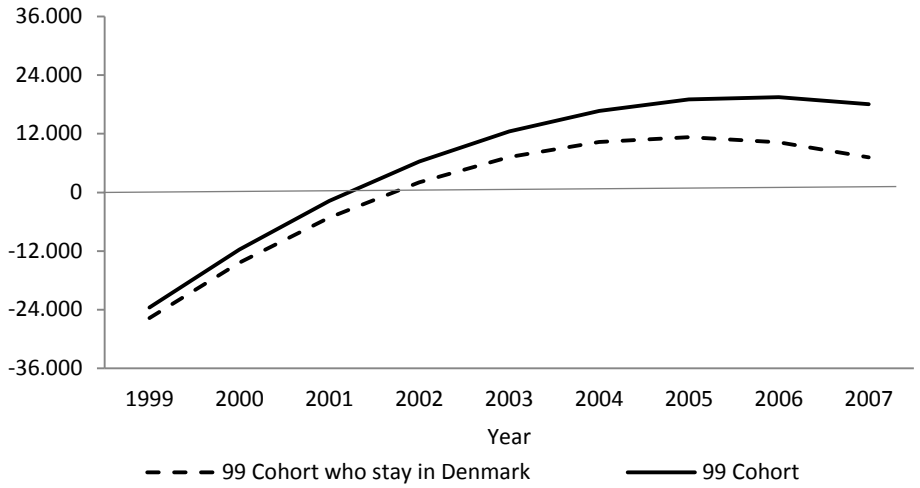


Figure 5.2 The effects of enforced language training on annual taxable income (DKK 2010)



6 Sensitivity Analysis

This section assesses the robustness of the results for smaller bandwidths for the forcing variable, h_z, and for different bandwidths for the baseline covariates used in the Frölich estimator. The results obtained in the previous section for out-migration effects, labour-market participation and income are quite robust to different bandwidths. Table 6.1 presents the out-migration effects for smaller bandwidths of the forcing variable. The results suggest that out-migration effects in the 2000 period are slightly lower than those obtained with longer bandwidths. However, the total out-migration effects in the 2001 period are quite similar to those obtained with h_z=60.

Table 6.2 presents point estimates for the enforcement effects on labour outcomes for different bandwidths h_z. The results suggest that most of the parameters are quite robust to different h_z, with the exception of the lock-in effects regarding ATP payment. In this case, estimated τ_0 for this outcome increases with bandwidth from -139 when $h_z = 40$ to -127 when $h_z = 60$. However, when considering that ATP payment corresponding to annual full employment is DKK 2,683, we do not find this to be a major difference. The results for other parameters and for income effects are much more robust, confirming the conclusions reached in the preceding section.

Table 6.3 considers the sensitivity of the results to different bandwidths for the baseline covariates used by the pooled Frölich estimator. Again estimated τ_0 seems to change with different bandwidths to a major extent compared to the other parameters. However, the differences still cannot be considered big enough to raise any doubt about our identification and estimation strategies.

Table 6.1 Estimates of enforcement effects on out-migration for different bandwidths h_z

	S_{it}								$S_{it}\|S_{it-1}=1$							
h_z	ψ_{2000}	ψ_{2001}	ψ_{2002}	ψ_{2003}	ψ_{2004}	ψ_{2005}	ψ_{2006}	ψ_{2007}	π_{2000}	π_{2001}	π_{2002}	π_{2003}	π_{2004}	π_{2005}	π_{2006}	π_{2007}
40	-0.030	-0.055	-0.038	-0.049	-0.036	-0.034	-0.035	-0.008	-0.030	-0.026	0.017	-0.015	0.014	0.003	-0.001	0.028
	(0.030)	(0.036)	(0.040)	(0.042)	(0.044)	(0.044)	(0.046)	(0.048)	(0.030)	(0.021)	(0.020)	(0.016)	(0.014)	(0.004)	(0.016)	(0.017)
41	-0.027	-0.053	-0.031	-0.043	-0.032	-0.031	-0.035	-0.011	-0.027	-0.028	0.022	-0.016	0.012	0.002	-0.003	0.024
	(0.030)	(0.035)	(0.039)	(0.041)	(0.043)	(0.043)	(0.045)	(0.047)	(0.030)	(0.021)	(0.020)	(0.016)	(0.014)	(0.004)	(0.016)	(0.016)
42	-0.028	-0.060	-0.034	-0.047	-0.037	-0.031	-0.035	-0.014	-0.028	-0.034	0.027	-0.016	0.010	0.007	-0.004	0.021
	(0.029)	(0.035)*	(0.039)	(0.041)	(0.043)	(0.043)	(0.045)	(0.047)	(0.029)	(0.021)	(0.020)	(0.016)	(0.014)	(0.007)	(0.016)	(0.016)
43	-0.030	-0.061	-0.036	-0.048	-0.038	-0.034	-0.038	-0.018	-0.030	-0.034	0.026	-0.016	0.010	0.006	-0.004	0.020
	(0.028)	(0.034)*	(0.038)	(0.041)	(0.042)	(0.042)	(0.044)	(0.046)	(0.028)	(0.021)	(0.020)	(0.016)	(0.014)	(0.007)	(0.016)	(0.015)
44	-0.031	-0.061	-0.037	-0.049	-0.039	-0.036	-0.045	-0.025	-0.031	-0.033	0.025	-0.016	0.010	0.004	-0.010	0.020
	(0.028)	(0.034)*	(0.038)	(0.040)	(0.042)	(0.042)	(0.044)	(0.046)	(0.028)	(0.021)	(0.019)	(0.015)	(0.014)	(0.006)	(0.017)	(0.015)
45	-0.030	-0.060	-0.030	-0.046	-0.039	-0.037	-0.047	-0.029	-0.030	-0.031	0.030	-0.020	0.006	0.003	-0.011	0.018
	(0.027)	(0.033)*	(0.037)	(0.040)	(0.041)	(0.042)	(0.044)	(0.046)	(0.027)	(0.020)	(0.020)	(0.016)	(0.014)	(0.006)	(0.017)	(0.014)
46	-0.032	-0.062	-0.034	-0.054	-0.048	-0.047	-0.056	-0.042	-0.032	-0.032	0.028	-0.025	0.005	0.002	-0.010	0.013
	(0.027)	(0.032)*	(0.037)	(0.039)	(0.041)	(0.041)	(0.043)	(0.045)	(0.027)	(0.020)	(0.019)	(0.016)	(0.014)	(0.006)	(0.017)	(0.014)
47	-0.038	-0.067	-0.040	-0.060	-0.053	-0.052	-0.061	-0.047	-0.033	-0.032	0.027	-0.025	0.006	0.001	-0.010	0.013
	(0.027)	(0.032)**	(0.037)	(0.039)	(0.041)	(0.041)	(0.043)	(0.045)	(0.027)	(0.020)	(0.019)	(0.016)	(0.014)	(0.005)	(0.016)	(0.014)
48	-0.034	-0.061	-0.035	-0.055	-0.049	-0.050	-0.058	-0.046	-0.034	-0.028	0.026	-0.024	0.005	0.000	-0.010	0.011
	(0.027)	(0.033)*	(0.037)	(0.040)	(0.041)	(0.041)	(0.043)	(0.045)	(0.027)	(0.021)	(0.020)	(0.015)	(0.014)	(0.005)	(0.016)	(0.014)
49	-0.033	-0.057	-0.032	-0.050	-0.043	-0.044	-0.053	-0.038	-0.034	-0.025	0.025	-0.022	0.007	0.000	-0.010	0.013
	(0.027)	(0.033)*	(0.037)	(0.039)	(0.040)	(0.041)	(0.043)	(0.044)	(0.026)	(0.020)	(0.019)	(0.015)	(0.014)	(0.005)	(0.016)	(0.014)
50	-0.036	-0.059	-0.035	-0.054	-0.048	-0.050	-0.058	-0.046	-0.036	-0.024	0.023	-0.023	0.005	-0.002	-0.009	0.011
	(0.026)	(0.032)*	(0.036)	(0.038)	(0.039)	(0.040)	(0.042)	(0.043)	(0.026)	(0.020)	(0.018)	(0.015)	(0.014)	(0.005)	(0.016)	(0.013)
51	-0.036	-0.057	-0.035	-0.052	-0.045	-0.048	-0.051	-0.039	-0.036	-0.023	0.023	-0.021	0.006	-0.003	-0.002	0.011
	(0.025)	(0.031)*	(0.035)	(0.037)	(0.039)	(0.039)	(0.041)	(0.043)	(0.025)	(0.020)	(0.018)	(0.014)	(0.014)	(0.004)	(0.017)	(0.013)
52	-0.036	-0.060	-0.038	-0.054	-0.047	-0.050	-0.053	-0.041	-0.036	-0.026	0.022	-0.020	0.006	-0.004	-0.002	0.011
	(0.025)	(0.031)**	(0.034)	(0.037)	(0.038)	(0.038)	(0.041)	(0.042)	(0.025)	(0.020)	(0.017)	(0.014)	(0.013)	(0.004)	(0.016)	(0.013)
53	-0.037	-0.058	-0.037	-0.053	-0.047	-0.051	-0.054	-0.042	-0.037	-0.022	0.021	-0.020	0.005	-0.004	-0.003	0.011

	S_{it}								$S_{it}\|S_{it-1}=1$							
h_z	ψ_{2000}	ψ_{2001}	ψ_{2002}	ψ_{2003}	ψ_{2004}	ψ_{2005}	ψ_{2006}	ψ_{2007}	π_{2000}	π_{2001}	π_{2002}	π_{2003}	π_{2004}	π_{2005}	π_{2006}	π_{2007}
	(0.024)	(0.030)**	(0.034)	(0.036)	(0.038)	(0.038)	(0.040)	(0.042)	(0.024)	(0.020)	(0.017)	(0.014)	(0.013)	(0.004)	(0.016)	(0.013)
54	-0.038	-0.057	-0.037	-0.053	-0.047	-0.051	-0.054	-0.043	-0.038	-0.020	0.020	-0.019	0.005	-0.005	-0.003	0.010
	(0.024)	(0.030)*	(0.034)	(0.036)	(0.037)	(0.038)	(0.040)	(0.041)	(0.024)	(0.020)	(0.017)	(0.014)	(0.013)	(0.004)	(0.016)	(0.013)
55	-0.039	-0.058	-0.039	-0.050	-0.044	-0.049	-0.053	-0.042	-0.039	-0.021	0.019	-0.014	0.005	-0.005	-0.003	0.010
	(0.024)*	(0.030)**	(0.033)	(0.036)	(0.037)	(0.037)	(0.039)	(0.041)	(0.023)*	(0.020)	(0.017)	(0.014)	(0.013)	(0.004)	(0.016)	(0.013)
56	-0.038	-0.057	-0.038	-0.050	-0.047	-0.052	-0.055	-0.045	-0.039	-0.020	0.019	-0.014	0.002	-0.006	-0.003	0.009
	(0.023)*	(0.029)*	(0.033)	(0.035)	(0.037)	(0.037)	(0.039)	(0.040)	(0.023)*	(0.020)	(0.017)	(0.014)	(0.013)	(0.004)	(0.015)	(0.013)
57	-0.039	-0.061	-0.043	-0.054	-0.052	-0.057	-0.061	-0.051	-0.039	-0.024	0.018	-0.015	0.002	-0.006	-0.003	0.009
	(0.023)*	(0.029)**	(0.033)	(0.035)	(0.037)	(0.037)	(0.039)	(0.040)	(0.023)*	(0.020)	(0.016)	(0.014)	(0.013)	(0.005)	(0.015)	(0.012)
58	-0.034	-0.056	-0.039	-0.050	-0.047	-0.055	-0.059	-0.050	-0.034	-0.024	0.018	-0.014	0.002	-0.009	-0.003	0.008
	(0.023)	(0.029)*	(0.033)	(0.035)	(0.036)	(0.037)	(0.039)	(0.040)	(0.023)	(0.019)	(0.016)	(0.014)	(0.014)	(0.005)*	(0.015)	(0.012)
59	-0.034	-0.060	-0.042	-0.053	-0.051	-0.059	-0.059	-0.050	-0.034	-0.027	0.017	-0.014	0.002	-0.009	0.001	0.008
	(0.023)	(0.029)**	(0.032)	(0.034)	(0.036)	(0.036)	(0.039)	(0.040)	(0.023)	(0.019)	(0.016)	(0.014)	(0.013)	(0.005)*	(0.016)	(0.012)
60	-0.038	-0.063	-0.046	-0.057	-0.054	-0.063	-0.061	-0.053	-0.038	-0.026	0.016	-0.014	0.001	-0.009	0.002	0.006
	(0.023)*	(0.029)**	(0.032)	(0.034)*	(0.036)	(0.036)*	(0.038)	(0.039)	(0.023)*	(0.019)	(0.016)	(0.013)	(0.013)	(0.005)*	(0.015)	(0.012)

Table 6.2 **Point estimates of enforcement effects on labour outcomes for different bandwidths h_z**

	ATP contribution			Taxable income (DKK 2010)		
h_z	τ_0	τ_1	τ_2	τ_0	τ_1	τ_2
40	-139.52	46.75	-3.52	-39,689	15,114	-957
41	-139.58	46.03	-3.42	-39,539	14,922	-935
42	-138.32	45.37	-3.35	-38,930	14,720	-915
43	-136.28	44.87	-3.31	-38,234	14,513	-896
44	-135.47	44.45	-3.27	-37,909	14,380	-882
45	-134.89	44.06	-3.24	-37,614	14,253	-871
46	-135.53	44.29	-3.28	-37,546	14,240	-871
47	-136.02	44.49	-3.32	-37,390	14,258	-878
48	-136.13	44.64	-3.36	-37,271	14,274	-883
49	-135.37	44.51	-3.37	-37,185	14,279	-887
50	-132.05	43.89	-3.35	-36,920	14,284	-892
51	-130.91	43.75	-3.38	-36,769	14,284	-900
52	-129.53	43.88	-3.42	-36,656	14,339	-911
53	-129.23	44.04	-3.46	-36,564	14,365	-920
54	-128.19	44.35	-3.51	-36,395	14,432	-931
55	-127.28	44.56	-3.54	-36,275	14,437	-933
56	-126.59	44.86	-3.59	-36,413	14,478	-939
57	-125.96	45.18	-3.62	-36,506	14,509	-940
58	-125.77	45.62	-3.66	-36,641	14,555	-942
59	-126.89	46.05	-3.70	-37,024	14,624	-945
60	-127.62	46.48	-3.74	-37,301	14,706	-951

Table 6.3 **Point estimates of enforcement effects for different bandwidths h_x**

		ATP contribution			Taxable income (DKK 2010)		
h_z	h_x	τ_0	τ_1	τ_2	τ_0	τ_1	τ_2
40	5	-149.98	49.33	-3.83	-43,185	16,682	-1,070
40	6	-151.86	49.86	-3.82	-42,844	16,320	-1,040
40	7	-150.09	49.60	-3.77	-41,855	15,980	-1,014
40	8	-145.05	48.78	-3.69	-40,686	15,648	-991
40	9	-141.84	47.77	-3.60	-40,049	15,363	-972
40	10	-139.52	46.75	-3.52	-39,689	15,114	-957
60	5	-124.98	50.36	-4.27	-37,283	15,631	-1,036
60	6	-128.27	49.86	-4.16	-37,642	15,475	-1,014
60	7	-129.37	49.09	-4.04	-37,795	15,277	-994
60	8	-128.69	48.20	-3.92	-37,608	15,075	-978
60	9	-128.18	47.31	-3.82	-37,442	14,884	-963
60	10	-127.62	46.48	-3.74	-37,301	14,706	-951

7 Conclusion

In 1999 Denmark implemented a very comprehensive introduction programme for all non-western immigrants, where one of the key ingredients was enforced participation in a language programme. Similar integration policies have been adopted in other European countries. Our findings show that the enforcement element of language training has no effect on participation. From an integration policy perspective, this is a rather negative outcome for such an important intervention towards enhancing self-sufficiency of family reunited migrants.

At the same time, enforcement has positive effects on income of family reunited migrants. In this case, 8-9 years after migration the cohort that was forced to learn the Danish language had on average an annual taxable income of about DKK 12,000-19,000 higher than the cohort that voluntarily participated in the same programme. These results suggest that family reunited migrants have incentives to adapt to the Danish labour market in order to find a job, but do not expend enough effort to language training, since enforcement increases their long-run earnings.

The fact that enforcement increases out-migration during the last years of the introduction programme and the lower income effects for those individuals who stay in the country suggests that the enforcement mechanism “scares” those individuals who are more likely to obtain higher returns from language acquisition.

The paper has also proposed a panel regression discontinuity design approach that allows to estimate more precisely the effects of reduced samples and that allows to control for selective out-migration, two important problems that plague empirical migration studies.

References

Baker, M. & D. Benjamin (1994): The Performance of Immigrants in Canadian Labour Market. *Journal of Labor Economics*, 12:369-405.

Baker, M. & D. Benjamin (1997): The Role of the Family in Immigrants' Labor-Market Activity: An Evaluation of Alternative Explanations. *The American Economic Review*, 87(4):705-727.

Basilio, L.; T.K. Bauer & M. Sinning (2009): Analyzing the labor market activity of immigrant families in Germany. *Labour Economics*, 16:510-520.

Bauer, T.; M. Lofstrom & K.F. Zimmermann (2000): Immigration Policy, Assimilation of Immigrants and Natives' Sentiments Towards Immigrants: Evidence from 12 OECD Countries. *Swedish Economic Policy Review*, 7:11-53.

Beenstock, M. (1993): *Learning Hebrew and finding a job: Economic analysis of immigrant absorption in Israel.* Paper presented at the Conference of Immigrant Absorption of Economics of International Migration, Econometric Evidence, Konstanz, Germany.

Beenstock, M. (1996): The acquisition of language skills by immigrants: the case of Hebrew in Israel. *International Migration*, 34:3-30.

Beenstock, M.; B.R. Chiswick & A. Paltiel (2010): Testing the immigrant assimilation hypothesis with longitudinal data. *Review of Economics of the Household*, 8(1):7-27.

Berman, E., K. Lang & E. Siniver (2003): Language-skill complementarity: returns to immigrant language acquisition. *Labour Economics*, 10:265-290.

Borjas, G.J. (1987): Self Selection and Immigrants. *American Economic Review*, 77:531-553.

Borjas, G.J. (1990): *Friends or Strangers: The Impact of Immigrants on the U.S. Economy*. New York: Basic Books.

Borjas, G.J. (1992a): National Origin and the Skills of Immigrants. In G.J. Borjas & R.B. Freeman (Eds.): *Immigration and the Work Force:* 17-47. National Bureau of Economic Research, the University of Chicago Press.

Borjas, G.J. (1992b): Immigration Research in the 1980s: A Turbulent Decade. In Research Frontiers. In D. Lewin; O.S. Mitchell & P.D. Sherer (Eds.): *Industrial Relations and Human Resources:* 417-446. Industrial Relations Research Association, Ithaca, NY.

Borjas, G.J. (1994): The economics of immigration. *Journal of Economic Literature*, 32:1667-1717.

Borjas, G.J. & B. Bratsberg (1996): Who leaves? The Out-migration of the Foreign-Born. *Review of Economics and Statistics,* 78:165-176.

Carliner, G. (1995): *The Language Ability of U.S. Immigrants*. National Bureau of Economic Research Working Paper No 5222.

Carrera, S. (2006): *A Comparison of Integration Programmes in the EU. Trends and Weaknesses*. Challenge Papers 1.

Chiswick, B.R (1978): The Effect of Americanization on the Earnings of Foreign-born Men. *Journal of Political Economy* 85(5):897-921.

Chiswick, B.R. (1991): Speaking, reading, and earnings among low-skilled immigrants. *Journal of Labor Economics*, 9(2):149-170.

Chiswick, B.R. & P.W. Miller (1992): Language in the immigrant labor market. In B.R. Chiswick (Ed.): *Immigration, language, and ethnicity: Canada and the United States:* 229-296.

Chiswick, B.R. & P.W. Miller (1994): Language choice among immigrants in a multi-lingual destination. *Journal of Population Economics*, 7:119-131.

Chiswick, B.R. & P.W. Miller (1995): The endogeneity between language and earnings: International analyses. *Journal of Labor Economics*, 13(2):246-288.

Chiswick, B.R. & G. Repetto (2001): Immigrant adjustment in Israel: literacy and fluency in Hebrew and earnings. In: S. Djajic (Ed.): *International Migration: Trends, Policy and Economic Impact*. New York: Routledge: 204-228.

Cortes, K.E. (2004): *Are Refugees Different from Economic Immigrants? Some Empirical Evidence on the Heterogeneity of Immigrant Groups in the United States*. IZA DP No. 1063.

Duleep, H.O. & M.C. Regets (1997): The Decline in Immigrant Entry Earnings: Less Transferable Skills or Lower Ability. *The Quarterly Review of Economics and Finance,* 37:189-2008, Special Issue.

Duleep, H.O. & Seth Sanders (1993): The Decision to Work by Married Immigrant Women: Evidence from Asian Women. *Industrial & Labor Relations Review*, 46(4):677-690.

Dustmann, C. (1994): Speaking fluency, writing fluency, and earnings of migrants. *Journal of Population Economics*, 7:133-156.

Dustmann, C. & A. van Soest (2002): Language and the Earnings of Immigrants. *Industrial and Labor Relations Review,* 55(3):473-492.

Dustmann, C. & Y. Weiss (2007): Return migration: Theory and Empirical Evidence from the UK. *British Journal of Industrial Relations*, 45:236-256.

Entzinger, H. & R. Biezeveld (2003): *Benchmarking in Immigration Integration*. European Research Centre on Migration and Ethnic Relations. Erasmus University Rotterdam.

Euwals, R.; J. Dagevos, M. Gijsberts & H. Roodenburg (2007): *Immigration, Integration and the Labour Market: Turkish Immigrants in Germany and the Netherlands*. IZA Discussion Paper No. 2677.

Frölich, M. (2007): *Regression Discontinuity Design with Covariates*. University of St. Gallen, Department of Economics, Discussion Paper No. 2007-32; IZA Discussion Paper No. 3024.

Huynh, D.T.; M.L. Schultz-Nielsen & T. Tranæs (2010): The Employment Effects upon Arrival of Reducing Welfare to Refugees. Chapter 1 in "*Essays in Migration and Fertility*", PhD-thesis No. 2010:1 from Aarhus School of Business, Aarhus University.

Jensen, P. & P.J. Pedersen (2007): To Stay or Not to Stay? Out-Migration of Immigrants from Denmark. *International Migration*, 45(5):87-113.

Lee, D. S. & T. Lemieux (2010): Regression Discontinuity Designs in Economics. *Journal of Economic Literature*, 48(2): 281-355.

Liebig, T. (2007): *The Labour Market Integration of Immigrants in Denmark*. OECD Social, Employment and Migration Working Papers, No. 50.

Long, J.E. (1980): The Effect of Americanization on Earnings: Some Evidence for Women. *Journal of Political Economy*, 88:620-629.

Lubotsky, D. (2007): Chutes or Ladders? A Longitudinal Analysis of Immigrant Earnings. *Journal of Political Economy*, 115(5):820-867.

Pons, G.; L. Husted & K.F. Krassel (2011): *Does Social Assistance Reduction Contribute to the Assimilation of Refugees?* Mimeo.

Rosholm, M. & R. Vejlinz (2010): Reducing Income Transfers to Refugee Immigrants; Does Start-Help Help You Start? *Labour Economics*, 17(1): 258-275.

Sarvimäki, M. & K. Hämäläinen (2009): *Moving Immigrants from Welfare to Work. The Impact of an Integration Program*. Mimeo.

Schucany, W.R. (1989): Locally Optimal Window Widths for Kernel Density Estimation With Large Samples. *Statistics and Probability Letters*, 7:401-405.

Shields, M. & S. W. Price (2002): The effect of language proficiency on the wages of immigrant men in England. *Journal of Population Economics*, (15):137-160.

Vella, F. (1998): Estimating Models with Sample Selection Bias: A Survey. *The Journal of Human Resources*, 33(1):127-169.

White, M.J. & G. Kaufman (1997): Language usage, social capital, and school completion among immigrants and native-born ethnic groups. *Social Science Quarterly*, 78:385-98.

Winter, S. (2002): *Kommunernes integrationsindsats efter den ny integrationslov*. AMID Working Paper Series 30/2002. Aalborg: Akademiet for Migrationsstudier i Danmark (AMID): 24.

Wooldridge, J.M. (2003): *Econometric Analysis of Cross Section and Panel Data*. The MIT Press, Cambridge, Massachusetts.

Appendix: Figures and Table

Figure A1 Descriptives for Newcomer by Residence Allowance Date

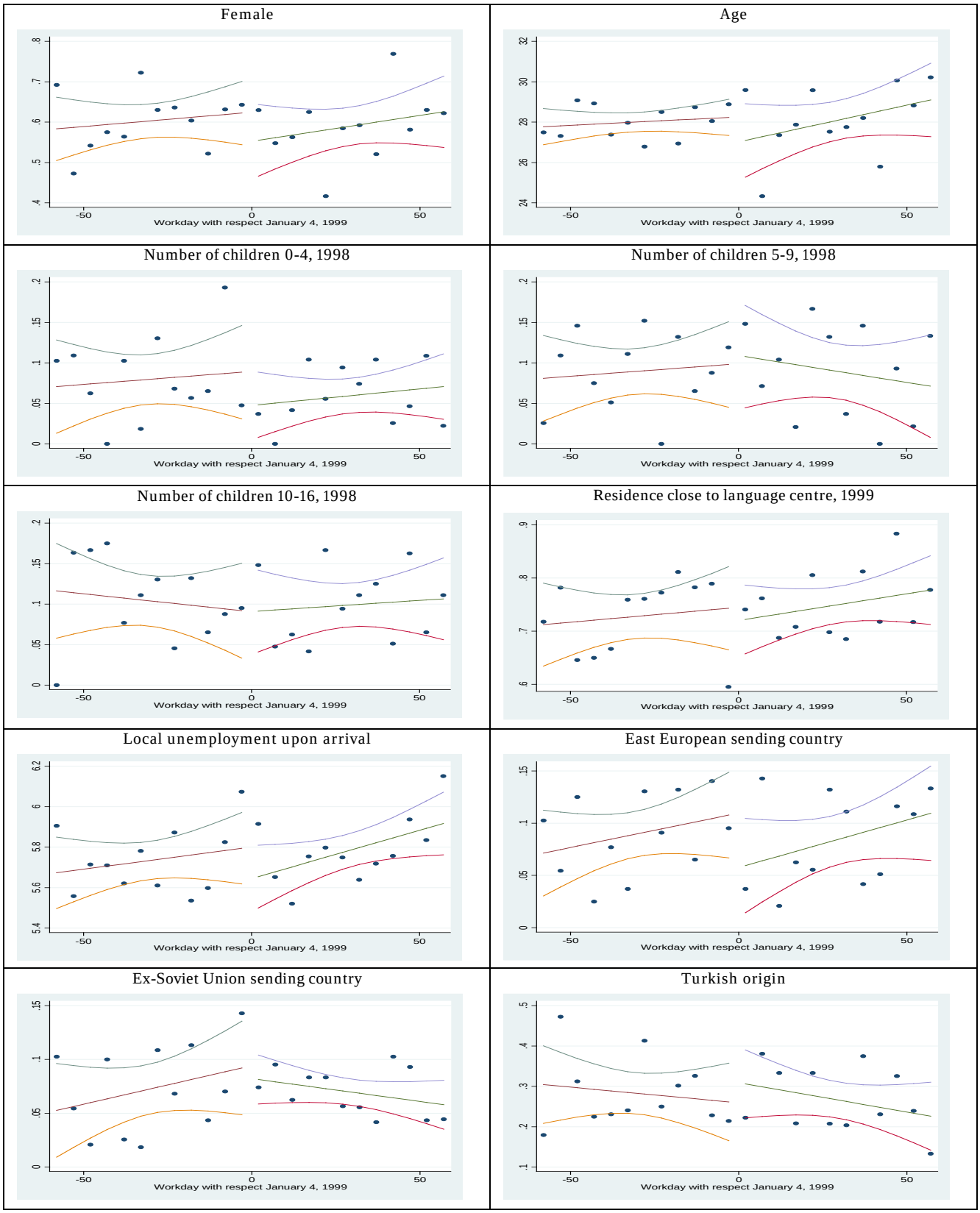


Figure A.1 Descriptives for Newcomer by Residence Allowance Date

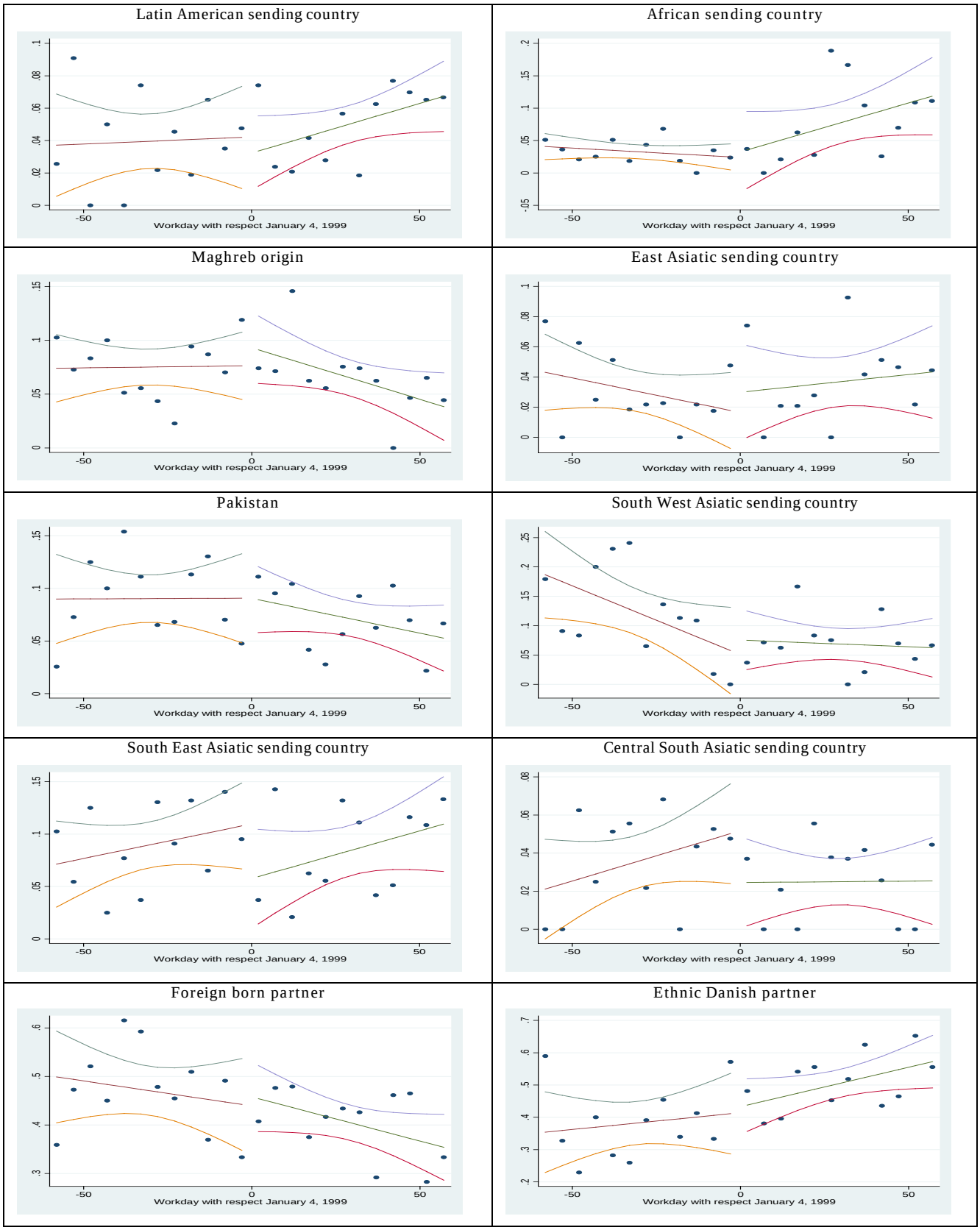


Figure A.2 Descriptives for Newcomer's Partner by Residence Allowance Date

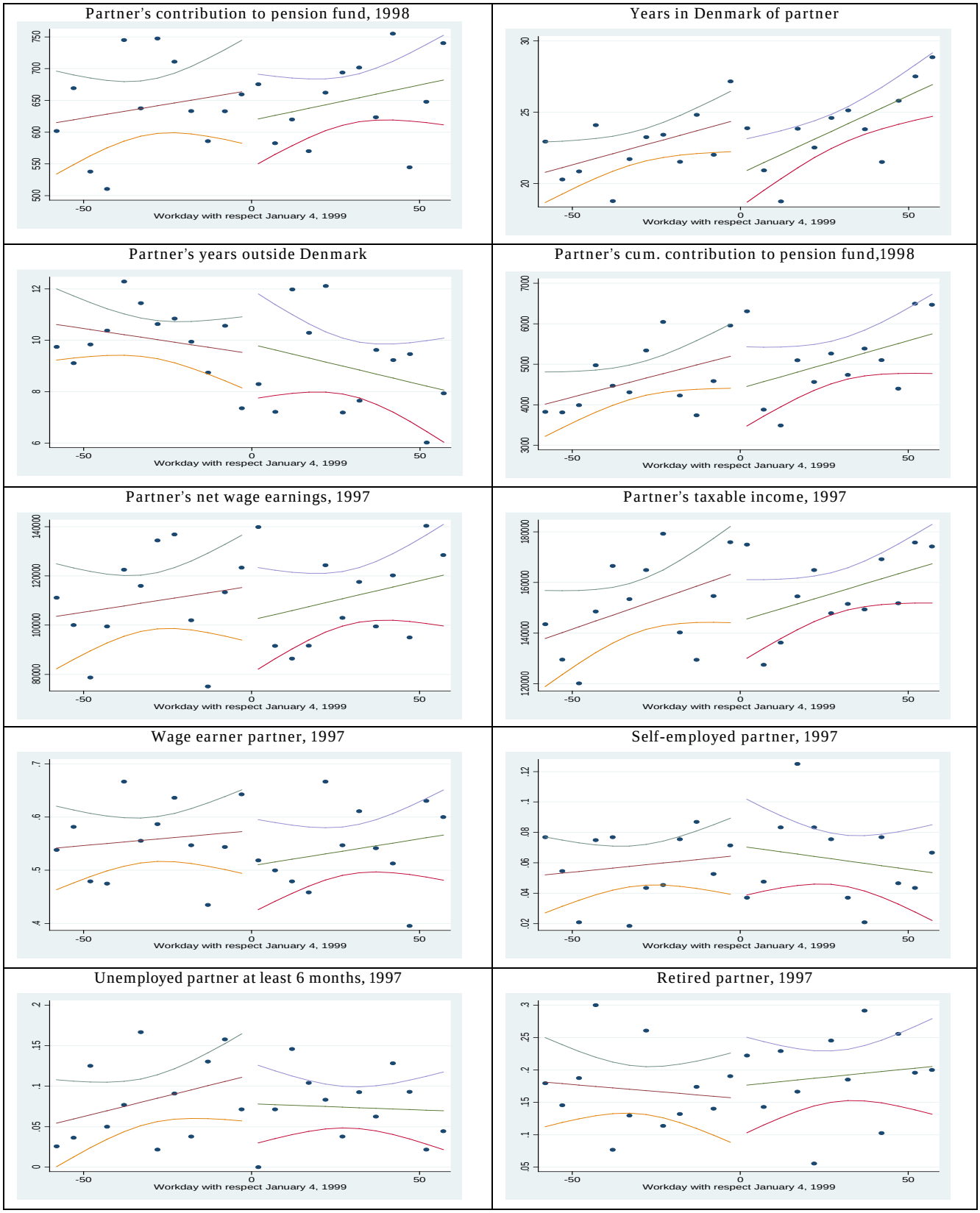


Table A1 Sending Regions

Region of origin	Country of origin
East Europe	Albania, Bulgaria, Yugoslavia, Rumania, Croatia, Bosnia-Herzegovina, Macedonia, Yugoslavia, Ex Rep.
Ex-Soviet Union	Soviet Union, Russia, Ukraine, Belarus, Armenia, Azerbaijan, Moldova, Uzbekistan, Kazakhstan
Turkey	Turkey
Maghreb	Algeria, Libya, Morocco, Tunisia, Egypt
Africa (exclusive of Maghreb)	Angola, Ethiopia, Gambia, Ghana, Guinea, Kenya, Liberia, Mozambique, Mali, Mauritius, Nigeria, Sierra Leone, South Africa, Tanzania, Uganda, Cameroun, Ivory Coast, Senegal, Togo, Burkina Faso, Zimbabwe, Zambia.
South West Asia	Yemen, United Arab Emirates, Afghanistan, Iraq, Iran, Israel, Jordan, Kuwait, Lebanon, Saudi-Arabia, Syria
Pakistan	Pakistan
Central and South Asia	Bangladesh, Sri Lanka, India, Nepal.
South East Asia	Cambodia, Indonesia, Malaysia, Philippines, Singapore, Vietnam, Thailand
East Asia	Taiwan, Japan, Kina, North Korea, South Korea
Latin America	Argentina, Bolivia, Brazil, Guyana, Chile, Colombia, Cuba, Dominican Republic, Ecuador, Guatemala, Honduras, Jamaica, Mexico, Nicaragua, Panama, Paraguay, Peru, El Salvador, Trinidad y Tobago, Venezuela, Belize

Dansk sammenfatning

Gabriel Pons Rotger

Effekten af skærpede krav om danskkundskaber for familiesammenførte indvandreres tilknytning til arbejdsmarkedet

Integrationsloven i 1999 indebærer for første gang et integrationsforløb for familiesammenførte indvandrere, der kommer til Danmark for at etablere sig med deres partner. For selvforsørgede familiesammenførte er det alene kravet om danskuddannelse med henblik på at opnå permanent opholdstilladelse, der er relevant. Der er generelt forskellige formål med danskuddannelse herunder at opnå almene forudsætninger for fortsat uddannelse og almene kundskaber og færdigheder, som er relevante i forhold til arbejdslivet og dermed at få et arbejde. Hvis familiesammenførte med ikke-flygtninge, som denne rapport har fokus på, skal have permanent opholdstilladelse, betyder introduktionsprogrammet i praksis deltagelse i tre års danskuddannelse. Der kræves i den udlændingelov, som trådte i kraft i slutningen af 1998, aktiv deltagelse i danskuddannelse (dvs. 85% af timerne) fra alle indvandrere under introduktionsprogrammet for at kunne opnå tidsubegrænset opholdstilladelse.

Undersøgelsens design sikrer, at indsats- og kontrolgruppen er ens sammensat bl.a. med hensyn til forudgående danskkundskaber.

Analyserne viser:

1. Danskuddannelse har ikke nogen betydning for, hvor meget (målt i arbejdstimer) den familiesammenførte indvandrer deltager på arbejdsmarkedet, efter at introduktionsprogrammet er færdigt og op til ni år efter, indvandreren kom til Danmark.
2. Efter danskuddannelsen er afsluttet, har familiesammenførte indvandrere på arbejdsmarkedet opnået en bruttoindkomst, der er omkring 12.000-19.000 DKK højere pr. år.
3. Danskuddannelse forhøjer genudvandring en lille smule midtvejs i introduktionsprogrammets forløb.

Contribution of Enforced Language Training to the Labour Market Participation of Family Reunited Migrants

The paper analyses whether family reunited migrants have incentives to transfer their home skills to Denmark, and finds that they essentially have since enforced language training does not increase their labour participation and has a moderate effect on earnings of family re-united migrants.

AKF
Anvendt KommunalForskning