

Employment Effects of Reducing Welfare to Refugees¹

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Abstract

In this paper we evaluate the labor supply effect of an integration reform enacted in Denmark in 2002. The employment rate of refugees is very low in Denmark; 16 months after residence being granted only a little more than 9 percent were employed before the reform. The reform increased the financial incentives to find employment significantly by reducing the welfare payment by (up to) 40 %. Using the sharp discontinuity with which the reform was implemented as our instrument, we identify significant and robust positive employment effects of the reduction in welfare benefits. Depending on the analytical specification, the employment rate of refugees increases by 3.4-6.2 percentage points estimated 16 months after residence being granted, which is a relative increase of between 35-70 percent or a labor supply elasticity of between 0.8 and 1.8. This effect has to be weighed against the fact that a large majority of refugees now live on the reduced income, because the employment rate is still very low for refugees after the reform.

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Introduction

Refugees are very poorly integrated into the labor market in Denmark. Even among the male population between 25-54 years of age only 40 percent are employed after five years of residence, and of all males of working age the figure is only 33 percent. With the intention of changing this record the Danish government enacted a new integration policy in 2002 by which the welfare benefits to refugee families were reduced significantly. The reform also introduced a number of new initiatives that restricted migrants' access to Denmark. In this paper we investigate the effect of the stronger financial incentives on the employment rate of refugees 16 months after they obtained asylum.²

In 1999 the government intended to make the labor supply of refugees more compatible with demand by extending the introduction program and by making it more comprehensive. Since then the program has included both language and skill courses as well as a broader introduction to the opportunities and obligations existing in the Danish society. The main element of the 2002 reform was to supplement this effort by strengthening the financial incentives to find employment. The incentives were strengthened both directly and indirectly. Social assistance to refugees who were granted a permit after July 2002 was reduced by up to 40 % compared to refugees granted residence before that date, thus directly providing stronger incentives to work. According to the new rules, lower welfare benefits apply for the first 7 years of residence in Denmark³ and depend on the family situation; the benefit level is lowest for single people without dependent children and highest for single people with dependent children. Moreover, the financial incentives were also changed indirectly. Welfare benefits are means tested, and the income that a married couple can earn before the social assistance (SA) is cut was also reduced due to the reform because this income threshold is linked to the level of SA benefit that the family is eligible for. This rule meant that refugee families where both partners were on welfare got proportionally more or less the same reduction in benefits as did single refugees. Nonetheless, it also had the consequence that a refugee with a working spouse might get nothing in benefit if he or she arrived after the reform. Before the reform, one spouse could get SA even if the other earned an income as long as this income was not too high. Since the reform, the wage income of the breadwinner needs to be very (unrealistically) low for this to be possible. These complications imply that one needs to pay attention to the family status of the refugees when evaluating the effect of the benefit reduction.

²See Tranæs and Zimmermann (2004) for a comprehensive description of the migration to Denmark, the legal situation for immigrants, and the integration of immigrants in Denmark before 2002.

³The name of the low level of social assistance for newly arrived is "Start Aid" and according to the new rules any residents in Denmark need to have been living in Denmark for at least 7 years out of the previous 8 years before being qualified for the ordinary (higher) social assistance. So the reform is aimed at immigrants but it applies to everybody, and many in particular young Danes are also affected for a time, e.g., if they have studied abroad for more than one year and wish to return to Denmark.

The new strict migration rules of the reform package were bound to significantly change the composition of the immigration to Denmark, but not until later, because the new rules only applied to individuals who handed in their applications after July 1 2002. Immigrants who were granted residence in the months thereafter had applied for residence a long time previously. The average time it took to get a permit was more than 200 days in 2002. In principle potential refugees had some time to respond to the reform – to the extent that they had better options – because the reform was suggested more than 5 months in advance, although it was finally approved by parliament only one month in advance. Some might have regretted having applied for residence in Denmark now that the living conditions had changed and therefore migrated further in the period after the announcement of the reform or, more realistically, after having obtained a residence permit in Denmark during the fall of 2002. We have some information on these matters and there is little sign of such a regret phenomenon. Nevertheless, we cannot fully rule out such selection based on unobserved characteristics; but on the other hand, that would also be a reaction to the reform, although the employment effect would not be a labor supply response but a composition effect.

The way we identify the effect of the reform is by using the fact that the benefit reduction was grandfathered in, so that unemployed individuals granted residence before July 2002 are eligible for the high benefits for ever and the individuals granted residence after are eligible only for the reduced benefits the first seven years they reside in the country.

The key data we use for this study are from a yearly register account of the labor market status measured ultimo November for the entire resident population in Denmark including all foreigners. We combine this with information on the date each immigrant got his or her residence permit so that we can distinguish people according to their residence seniority in the country (the time since they were granted residence). This enables us to precisely distinguish the treatment and the control group at any later point in time. By focusing on a specific date, November 2003 (and November 2004), we measure the performance of both groups at the same point in the calendar, which controls for the influence of both the business cycle and seasonality.

Heuristically, we test the effect of the reform by comparing the expected employment rates of the groups of refugees who are governed by the new rules with those who are governed by the old rules, both groups having approximately 16 months of residence in Denmark in November 2003, and 28 months in 2004. To estimate the employment rate after 16 months of residence for both groups we follow a parametric regression discontinuity approach. We estimate the model under many different assumptions, both in terms of how many months before and after the initiation of the reform a refugee could have arrived and still be included in the analysis and in terms of the functional form assumptions used to model the relationship between residence seniority and employment.

Because there is considerable uncertainty regarding the way residence seniority and employment are related we also estimate an extension of the model where we use earlier cohorts of refugees to model this relationship. The interaction going on between residence seniority and employment could reflect institutions and traditions within the country and within the introduction program, the speed at which the language is learned, the timing of skill acquisition, etc. Therefore, in order to model the no-treatment relationship between residence seniority and employment we use as our reference group the refugees who – month for month – arrived one year earlier, and then measure their employment status in November 2002. The refugees of the reference group were subject to exactly the same introduction program as the ones who arrived later. When the difference between the refugees in the control and the treatment groups is tested relative to the reference group, this is done in a difference-in-difference set-up.

Whether we control for the importance of residence seniority by using our reference group or not, the result is a positive employment effect that can be attributed to the reduction in social assistance. In November 2003, after 16 months of residence, we find a positive employment effect of between 3.4 and 6.2 percentage points, depending on the functional form assumptions imposed (the difference-in-difference estimation points to an even bigger effect). Overall the analysis suggests an increase of between 35-70 % on average or an elasticity of between 0.8 and 1.8. The effect varies within the group of refugees. Large effects are found for single males above 30, and insignificant or small effects are found for the young and for single females without children.

In 2004 we also find a significant effect but the effect is a little smaller and the result is less robust than the one for 2003. This might suggest that no long run effect exist. However, the seemingly weaker effect in 2004 could also reflect the fact that in November 2004 both the first treated and the last non-treated refugees were close to having completed their introduction program, and thus there might be a common lock-in effect that makes it more difficult to identify the effect of the benefit reduction. Furthermore, new reforms were enacted on January 2004 that increased the work prospect systematically more for the control group. For that reason we consider the results based on the November 2003 data to be the more reliable ones, and they only measure the effect in the short run.

Finally, there is also an effect of the reform to be detected as early as November 2002, that is, 4 months after the reform. We do not discuss this further because very few of the treated refugees had any significant residence seniority at that time.

Many studies have estimated labor supply elasticities associated with welfare reforms; see for example Meyer (1995) for an early survey and Immervoll, Kleven, Kreiner, and Saez (2007) for a more recent one. Our estimates are somewhat higher than many that can be found in the literature. On the other hand, the employment rates for refugees in Denmark prior to the reform were also much lower than is usually the case, which allows for higher elasticities.

The effect of the Start-Aid reform on the transition from welfare to work for immigrants with refugee status has recently been studied in a duration model by Rosholm and Vejlin (2007). They study only the behavior of refugees themselves, not their spouses, and find a positive effect. This study does not estimate the effect of the benefit reduction on the employment rate.

The reduction in benefits applies in principle only to refugees and their families and not to other types of immigrants, for instance immigrants that are granted residence because of family reunification (for individuals other than refugees). These immigrants are supposed to provide for themselves or to be provided for by their spouses in Denmark. In practise this is not entirely the way things turns out, but it is very difficult to find out who in this group are in fact eligible for social assistance and thus who were affected by the reduction in benefits. Therefore we focus on the refugees and their families, because they are all eligible for social assistance and thus they all experienced the benefit reduction if they got their residence permit after July 1, 2002.

The rest of the paper is organized as follows. First we discuss the reform in greater detail in the next section, and then we take a closer look at the rich data we use. Then in section 3 we discuss methodological issues, and in section 4 we present the results and discusses robustness. Finally, section 5 concludes.

1 Immigration Policy

Like most other North European countries, Denmark banned employment immigration in 1973 following the sharp rise in unemployment in the wake of the oil crises, and until recently the main types of immigrants were refugees and people seeking to be reunified with their families in Denmark. Employment migration was not completely ruled out after 1973; it was possible for citizens from the Nordic Countries and the EU/EAA to go to Denmark to work, but for others there were tight restrictions, and in total employment migration contributed only marginally to the net immigration to Denmark during the following two decades. Recently the restrictions have been relaxed, however.

The current integration policy has in the main existed since 1999, when the government launched a set of new reforms. The intention behind this effort was to facilitate the integration of immigrants into the labor market but also to further the assimilation process more generally.

Under the new integration regime the existing introduction program (IP) was re-invented and extended from $1\frac{1}{2}$ years to three years. The program is offered to all newly arrived refugees and administered by the local municipalities, who also administer the welfare benefits (SA). The IP includes a number of activities that are required of refugees in order to get welfare payments. The mandatory courses include comprehension of the Danish society and language. These courses are normally offered at the beginning of the IP.

During the IP the refugees have to be registered at the Employment Service and have to take up employment if it is offered unless they have problems other than unemployment.⁴ When immigrants are being trained for future employment, the same practise is followed as when unemployed natives, not members of a UI fund, are being trained, and thus the training is in accordance with the Law on Active Social Politics.

Refugees receive an "introduction allowance" - the name given to the social assistance for refugees and other new arrivals – from the moment the municipality council accepts responsibility for them and contingent on participation in the IP. In addition, they are entitled to child allowance, housing benefits and public health care. If the refugees obtain employment the introduction allowance is phased out in proportion to hours worked, and not to money earned, which creates larger incentives to take up part time employment. For refugees who arrived before the reform the introduction allowance is equal to ordinary social assistance benefits (the welfare benefits received by native unemployed persons who are not insured, i.e., who are not members of a UI fund): a refugee with dependent children receives DKK 10,522 per month in introduction allowance (taxable income in 2002), while refugees above the age of 18 without dependent children receive DKK 7,919 per month. After three years in the country the benefits are no longer called "introduction allowance" but social assistance, and the refugees are subject to the same rules as natives.⁵

All types of social assistance (SA) are means tested and thus one cannot get SA if one has other means of support, including a spouse with an income above a certain level. In order to be reunified with a non-refugee spouse in Denmark it is a prerequisite that the spouse living in Denmark can provide for the new family member. Thus family reunified immigrants (to non-refugees) cannot get any kind of SA upon arrival. However, if the providing spouse later loses the ability to provide, then the family reunified immigrant is eligible for SA, and if this happens shortly after arrival the immigrant has to participate in the introduction program as a precondition for receiving SA.

During the period January 1999 to June 2002 the rules were rather stable concerning immigrants' access to Denmark, the introduction program and the welfare benefits.

1.1 The 2002 Reform

In 2002 a major revision was enacted. The reform both restricted immigrants access to Denmark and affected the integration and assimilation of immigrants after their arrival in Denmark. The latter part supplemented the 1999 reform by strengthening the financial incentives to take up employment. Specifically, the reform introduced a new principle for receiving SA. Under the terms of the reform all individuals with residence in Denmark who are eligible for SA but who have not resided in Denmark for at least 7 years during the

⁴For refugees who have not completed level 2 of the Danish language program this is optional.

⁵Initially the benefits to foreigners were lower than the general level of social assistance as a consequence of the 1999 reforms. In February 2000 the level was harmonized again with the general social assistance benefits.

previous 8 years now receive a lower allowance called "Start aid" instead of the ordinary SA.⁶ Thus, this change also affected Danish citizens; if they have been away from the country for more than one year they will only be covered by Start Aid when they return – until they have resided in the country for 7 out of the last 8 years.⁷ The Start-Aid benefit was intended for foreigners but the rule was made general in order not to discriminate.

The division of responsibilities was not changed with the 2002 integration reform and refugees are still offered an introduction program upon arrival and have to be registered at the Employment Office. The introduction program itself continues in almost the same way as before the reform.

The new SA reform was announced in January, but not finally approved by parliament until June, and was effective from July 1, 2002, but only for those who were granted residence after this date, many of whom had applied for asylum many months before. On average it took at least 7 months to get a residence permit as a refugee in 2002. Start Aid is significantly less generous than ordinary SA. Refugees older than 25 years of age with dependent children on Start Aid receive DKK 6,379 per month (in 2002), while refugees with dependent children on SA receive DKK 10,522 per month. For refugees without dependent children the SA and Start-Aid benefits are DKK 7,919 and DKK 5,103 per month respectively. So in general benefits were reduced by around 40 percent.

The integration part of the 2002 reform mainly had the purpose of supplementing the 1999 set-up by increasing the financial incentives to take up employment. The 2002 reform also changed the immigration laws. It was made significantly more difficult to immigrate to Denmark, both through family reunification and as a refugee.

For family reunification to take place in Denmark the following two new conditions were introduced: First, both spouses must be at least 24 years of age, and, second, the aggregate ties of both individuals must be stronger to Denmark than any other country (earlier they could be equally strong). These two conditions were added to the existing list: the individual residing in Denmark must be employed and able to support the spouse; the individual residing in Denmark must prove that he or she was in suitable accommodation facilities during the 3 previous years; the individual residing in Denmark must prove wealth of approximately DKK 57,000; and the individual residing in Denmark must not have received social assistance within the previous 3 years.

These conditions do not apply to people who have at least 28 years of legal residence in Denmark or – enacted 6 months later – can prove at least 28 years of Danish citizenship. The restrictions on refugees' entry to Denmark were also tightened. Asylum applications

⁶The general conditions for receiving the means tested SA are low income (unemployed), no spouse with a sufficiently high income, and not being member of an unemployment insurance fund. The only foreigners that are eligible for SA are refugees and their families, and other immigrants that have lost their means of support after having resided in Denmark for some time. The last group will not be granted residence unless they can provide for themselves upon arrival.

⁷Special rules apply to EU/EAA citizens.

could no longer be made at Danish agencies abroad and the "de facto" refugee status was abolished and replaced by a "protection status".

These rules were implemented on July 1, 2002, but they only covered new applications handed in thereafter. This means that residence applications prior to July 1, 2002 were governed by the old rules and applications handed in after July 1, 2002 were governed by the new rules. Among the refugees granted residence during the second half of 2002 only 0.7 percent received it on the basis of the new immigration laws. During the first six months of 2003 this fraction was 22 percent. As we shall see below, we do not get either qualitatively or quantitatively different results whether we take the treatment group to be the refugees granted residence 6 or 12 months after July 1, 2002.

Later, on January 1, 2004, two more relevant reforms were enacted. One gave the municipalities a considerable economic incentive to get refugees off SA because they would have to carry a bigger share of the expenses in the future. The other one - that became effective from January 1 - put a maximum on the sum of public transfers, SA, housing subsidy and child allowance. People on Start-Aid were practically speaking not affected, the level of Start-Aid is simply too low. Hence, both reforms increased the work prospect systematically more for the SA refugees (the control group) compared to for the Start-Aid refugees (the treatment group).

2 The Data

Our main source of data is a national account of the occupational status during the last week in November of all individuals with residence in Denmark, including all foreigners. This information is collected each year by Statistics Denmark, who also administer a long list of registers with individual information that can be linked with the occupational status in November. From these sources we have constructed a data set for the years 2003 and 2004 with the occupational information for November plus information on age, gender, highest completed education, marital status, and the number of dependent children in the household. This information is collected during the fall and reported at the end of the year in question. We also have information on income (annual), unemployment history and welfare benefit history. Finally, we have added register information on the date at which the immigrants were granted residence, the legal ground upon which residence was granted, and the country of origin.

With the information on the "legal ground for residence", supplied recently by the Immigration Service for the first time, it has been possible to distinguish individuals who are refugees from the family-reunification migrants. This is important, because only the first group are systematically affected by the new rules. The second group of immigrants are only affected to the extent that the spouse already living in the country at some point in time after his or hers arrival becomes unemployed or sick. The legal ground for residence is available for all migrants granted residence after January 1, 1997. Combining

this information with the data from the Database of Historical Migrations, which holds data on the date of arrival and departure, residence spells are constructed.

The focus is on refugees, as they are the target group affected by the reform (i.e., the treatment group). The non-treated group of foreigners, for instance, the non-Western⁸ family-reunification migrants, is not a good control group because, as mentioned before, they might not be affected upon arrival but could well have been affected soon after, depending on how well their spouses were doing.

The identification of the treatment groups is as follows: Initially we select only non-Western foreigners who have not been granted residence based on grounds other than refugee status (of which there are more than one type). To this group we add all family reunified immigrants connected to individuals from this group. These foreigners are referred to as non-Western refugees or simply refugees. Of these, we restrict attention to those between 18 and 59 years of age. This is done in order to remove non-labor market potentials from the analysis. For the purposes of analysis we restrict our attention when analyzing the employment rate in November 2003 to foreigners who at that time had been granted residence between 5 and 28 months previously, that is, they got their residence permit between July 2001 and June 2003. Note that foreigners with 16 months of residence seniority in November 2003 had been granted residence in July 2002 and, hence, were the first to be affected by the reform. That is, we only use information on immigrants granted residence up to 12 months prior to the reform and 12 months after the reform. Similarly, for the analyses in November 2004 we restrict our attention to immigrants granted residence between 17 and 40 months previously at that time. The interval is chosen to satisfy two conditions. First, we wish to minimize possible extrapolation bias by not including immigrants with residence seniority "too far away" from the reform month. Second, we need "enough" observations prior to and after the reform in order to predict the employment rate at the cut-off residence seniority.

The employment rate of refugees is a function of residence seniority. Figure 1 illustrates this. Upon arrival refugees have the "right and responsibility" to participate in an IP unless they are too sick (physically or mentally) to work. During the IP, which has a duration of 36 months, refugees are recorded as being out of the labor force. Job search assistance, active labor market programs, and Danish language courses are offered during the IP. Participants in the IP can search for work while participating in the program.

Panels (a) and (b) of figure 1 show the employment rates of refugees in November 2003 and 2004. Refugees granted residence 16 months previously, or less, in November 2003, are affected by the reform; refugees granted residence more than 16 months previously in November 2003 are not, because they got their permit before July 2002. There seems to

⁸Western foreigners include individuals from the EU (definition prior to January 2006), Iceland, Norway, Switzerland, Andorra, Lichtenstein, USA, Canada, Australia, and New Zealand [Larsen and Matthiessen, 2002]. Non-Western foreigners originate from all other countries.

be an effect on the employment rate when the relationship between residence seniority and employment is taken into account, but even just comparing the employment rates for refugees with a seniority of 11-16 months with those of 17-22 months seems to indicate a positive effect. Panel (c) shows the situation in November 2004.

Table 1 shows descriptive statistics for the refugees. The upper three rows show the employment status, grouped in 12-month intervals in November 2002-2004. Employment clearly depends on the seniority in the country. Furthermore, refugees are most likely to be in their mid 30s; around $\frac{3}{4}$ are married, although, there seems to be a tendency towards a decrease in the share of married persons at the end of the period. Approximately $\frac{2}{3}$ of the refugees have dependent children in the household. Finally, the five largest country groups are Somalia, Afghanistan, Iraq, Iran, and Bosnia-Herzegovina.

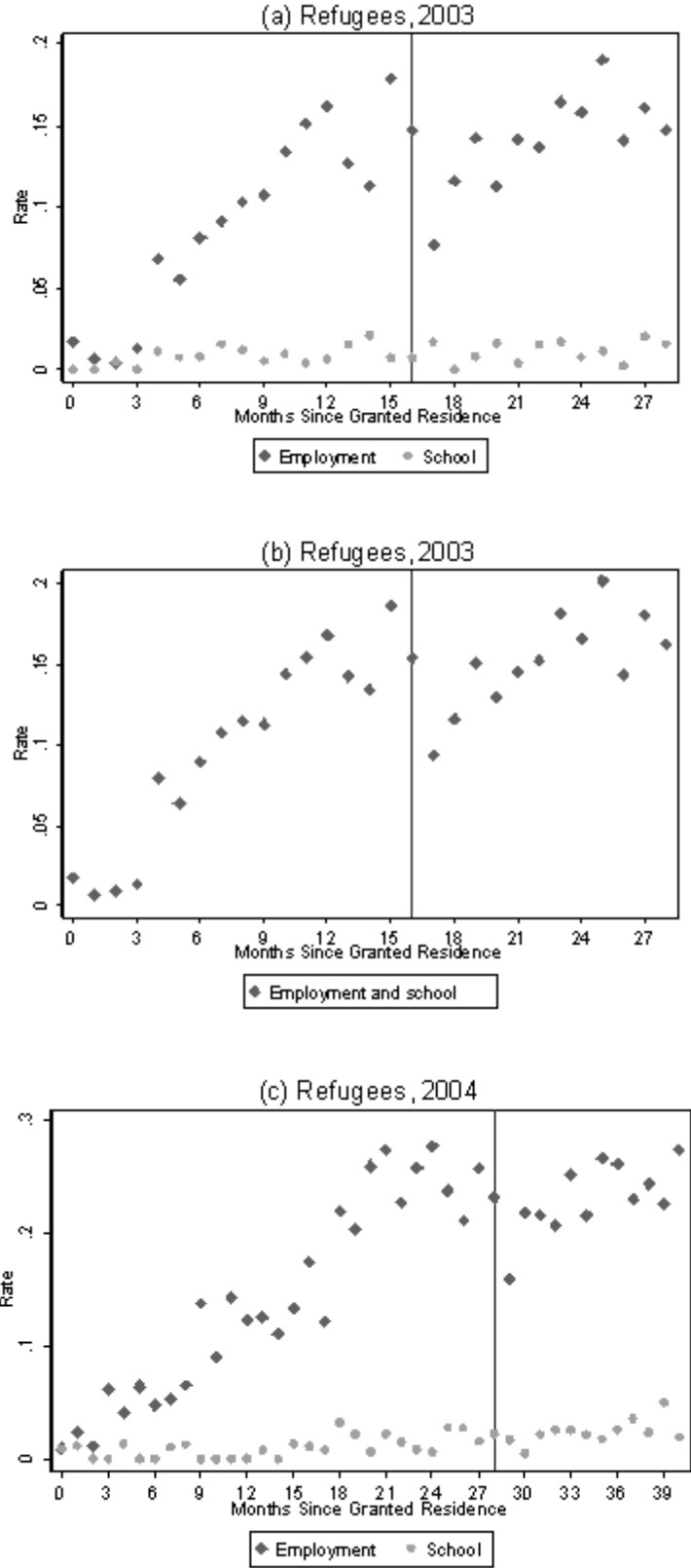
In order to see what impact the reform had on refugees we take a closer look at the amount of SA the refugees received in 2003 and in 2004 as well as the take-up rates. Information on the benefits received is available from the Social Statistics who have collected the data from the local municipalities. In figure 2, panels (a) and (b) show the average monthly SA benefits⁹ refugees and family reunified migrants received in 2003 and 2004 by month of residence seniority. Two features are of great importance. First, residence seniority predicts the level of SA with great accuracy. Refugees who are granted residence in June 2002 (i.e., residence seniority equal to 17 in November 2003) are not affected by the reform. Providing they were getting SA, they received on average approximately DKK 10,600 per month, whereas refugees with seniority of 16 months or less are affected by the reduction, which clearly shows in panels (a). Refugees granted residence in July 2002 received approximately DKK 6,100. A similar picture is revealed for family reunified migrants, although both the take-up rate and the level are lower.

Figure 3 plots the monthly unemployment rate in Denmark between January 2001 and July 2005. Of special interest are the employment rates around July 2002, in November 2003, and in November 2004. The crucial thing to observe is that the treatment group, the one that entered the labor market after July 1, 2002, arrived in a market with about the same unemployment rate as did the refugees of the control group, the one that arrived before July 2002. Notice also that unemployment is high both in November 2003 and in November 2004, which might have contributed to the generally low employment rates among refugees in both years.

⁹The average monthly introduction allowances are obtained as the annual introduction allowance normalized with the rate at which the group of individuals in question have received welfare benefit. That is, let \bar{b} be the average monthly level of introduction allowance, b_i the annual introduction individual i receives, and OLF_i be the fraction of the year for which individual i received introduction allowance. The average monthly introduction allowance is defined as

$$\bar{b} = \frac{1}{n} \sum_{i=1}^n \left(\frac{b_i / OLF_i}{12} \right)$$

Figure 1: Employment and Education Rates of Refugees and their families, November 2003 and November 2004.



Notes: In 2003 and 2004 refugees granted residence 16 months ago or less and 28 months ago or less, respectively, are affected by the reform.

Source: Own calculations based on register data.

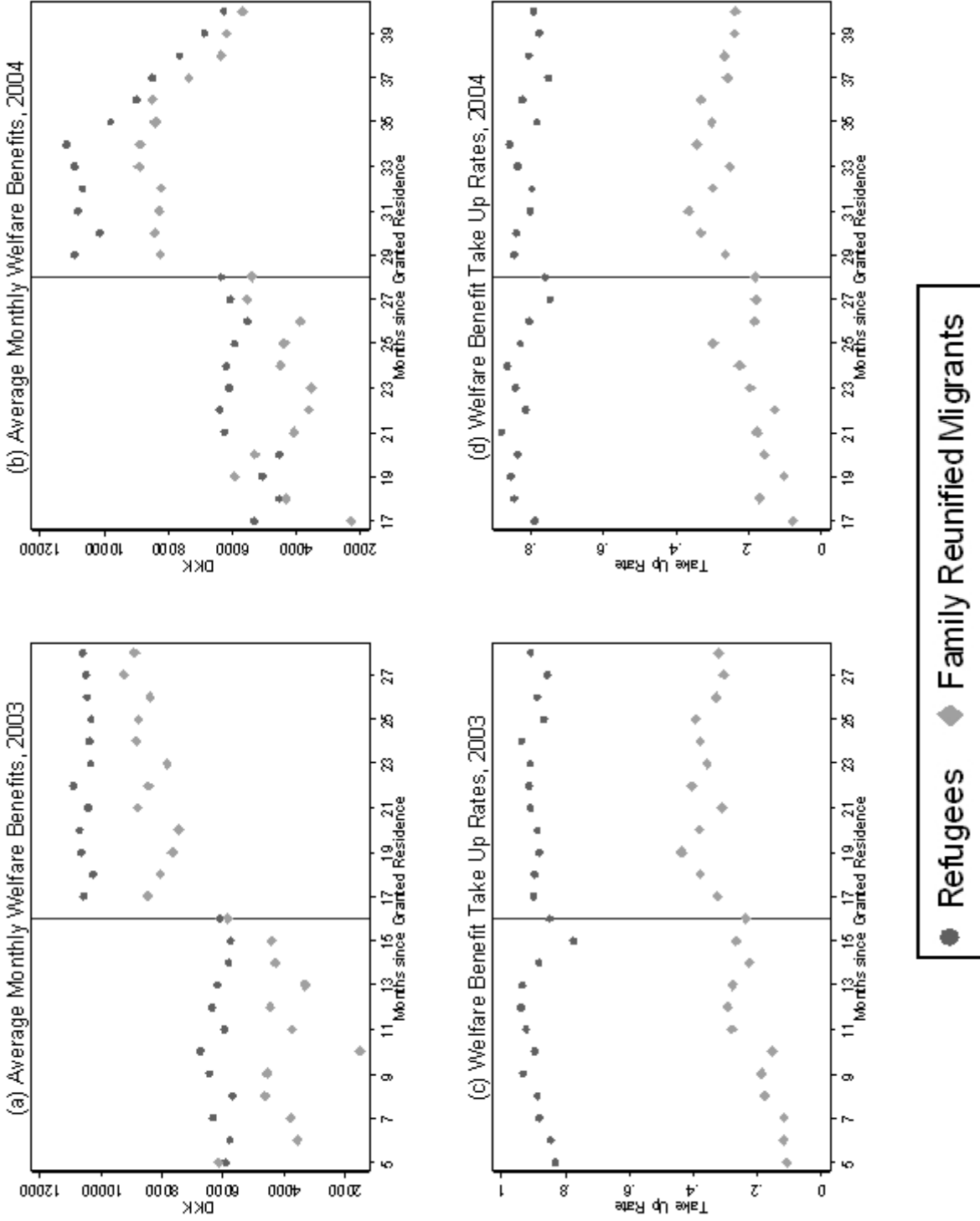
Table 1: Descriptive Statistics of Refugees by Month of Granted Residence, 2003-2004.

S	Year			
	2003		2004	
	11-16	17-22	23-28	29-34
Occupational status				
<i>employed</i>	.146 (.352)	.121 (.326)	.248 (.432)	.211 (.408)
<i>unemployed</i>	.045 (.206)	.061 (.240)	.069 (.253)	.093 (.290)
<i>out of labor force</i>	.799 (.401)	.808 (.394)	.666 (.472)	.677 (.468)
In school	.010 (.101)	.010 (.101)	.017 (.130)	.020 (.139)
Months of residence	13.055 (1.603)	19.447 (1.676)	25.062 (1.598)	31.445 (1.671)
Age	32.610 (8.486)	33.013 (9.002)	33.346 (8.556)	33.511 (9.080)
Female	.553 (.497)	.594 (.491)	.555 (.497)	.600 (.490)
Single	.389 (.488)	.337 (.473)	.340 (.474)	.336 (.473)
Children	.549 (.498)	.613 (.187)	.630 (.483)	.635 (.482)
Less than 9 th grade	.705 (.456)	.690 (.463)	.681 (.466)	.665 (.472)
9 th or 10 th grade	.103 (.304)	.108 (.311)	.125 (.331)	.133 (.340)
High School	.063 (.243)	.070 (.255)	.064 (.244)	.069 (.254)
Vocational Training	.075 (.263)	.067 (.249)	.075 (.264)	.065 (.247)
College	.055 (.228)	.066 (.248)	.056 (.229)	.068 (.251)
Somalia	.154 (.362)	.120 (.325)	.136 (.343)	.113 (.317)
Iraq	.342 (.474)	.306 (.461)	.354 (.478)	.301 (.459)
Iran	.118 (.323)	.033 (.180)	.120 (.325)	.033 (.180)
Afghanistan	.081 (.273)	.223 (.416)	.083 (.276)	.229 (.421)
Bosnia-Herzegovina	.062 (.241)	.075 (.263)	.063 (.243)	.077 (.266)
	1.256	1,351	1,224	1,317

Table 2: Descriptive Statistics of Refugees by Month of Granted Residence, 2003-2004.

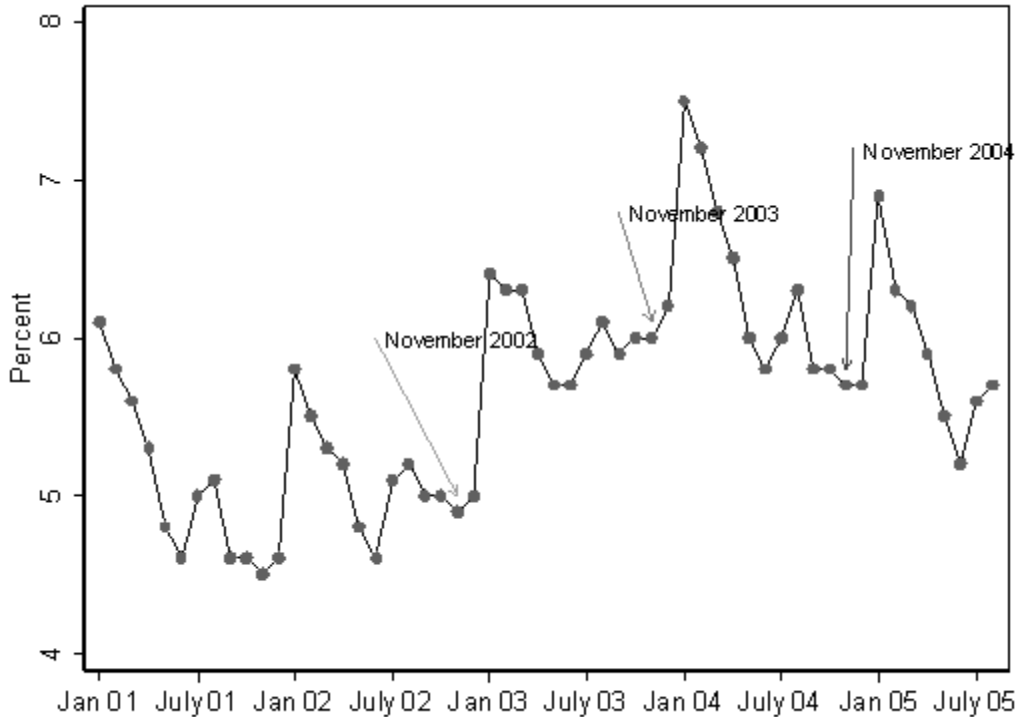
S	Year			
	5-16	2003 17-28	2004 17-28	2004 29-40
Occupational status				
<i>employed</i>	.125 (.331)	.145 (.353)	.237 (.425)	.236 (.425)
<i>unemployed</i>	.034 (.186)	.057 (.232)	.059 (.235)	.118 (.323)
<i>out of labor force</i>	.829 (.377)	.786 (.410)	.687 (.464)	.620 (.485)
In school	.010 (.101)	.012 (.107)	.017 (.131)	.025 (.157)
Months of residence	10.734 (3.087)	23.218 (3.347)	22.727 (3.093)	35.233 (3.344)
Age	33.040 (8.771)	33.635 (9.206)	33.689 (8.805)	34.144 (9.256)
Female	.553 (.497)	.545 (.500)	.553 (.497)	.549 (.498)
Single	.386 (.487)	.342 (.474)	.330 (.470)	.344 (.475)
Children	.545 (.498)	.612 (.487)	.627 (.484)	.623 (.485)
Less than 9 th grade	.829 (.377)	.564 (.496)	.809 (.393)	.544 (.498)
9 th or 10 th grade	.060 (.238)	.159 (.366)	.078 (.268)	.181 (.385)
High School	.037 (.187)	.100 (.300)	.037 (.190)	.101 (.301)
Vocational Training	.043 (.202)	.078 (.267)	.043 (.203)	.077 (.266)
College	.032 (.175)	.099 (.299)	.032 (.177)	.098 (.297)
Somalia	.121 (.326)	.099 (.299)	.106 (.308)	.093 (.291)
Iraq	.345 (.476)	.331 (.471)	.355 (.478)	.330 (.470)
Iran	.092 (.289)	.025 (.157)	.093 (.291)	.025 (.157)
Afghanistan	.116 (.320)	.273 (.445)	.117 (.322)	.279 (.449)
Bosnia-Herzegovina	.079 (.269)	.043 (.204)	.079 (.269)	.044 (.205)
obs	2,242	3,650	2,191	3,580
Treatment status	Treated	Not treated	Treated	Not treated

Figure 2: Average Monthly Welfare Benefits and Welfare Benefit Take Up Rates, 2003 and 2004.



Notes: In 2003 and 2004 refugees granted residence 16 months or less previously and 28 months or less previously were affected by the reform, respectively.

Figure 3: Unemployment Rates.



Source: Statbank Denmark [web]

3 Empirical Strategy and Identification

The estimation approach is to use the cross sectional administrative data that register the employment status of all individuals in November, and then use the fact that in November 2003, 16 months after the benefit reduction for new entrants, the population is divided into two groups according to their residence seniority in the country. Having arrived at November 2003, this division is deterministic; it is not possible for anyone to influence it, other than by leaving the country, something that we shall return to.

Nevertheless, the process by which a particular refugee ended up in the high or the low benefit group was a random process in the main. The argument for this is based on two important facts. The first one is that refugees have to wait quite a number of months from the time they apply for asylum and until they are granted residence and that they have very little control over the duration of the waiting period. The refugees who were granted residence in 2002 waited on average at least 7 months. The second fact is that the new low benefits were grandfathered in, that is, the low Start Aid only applied to new entrants. All refugees who got their residence permit before that date continued and will continue to be entitled to the high SA as long as they stay in the country.

The reform was first suggested $5\frac{1}{2}$ months before being enacted and finally passed through parliament one month before. Admittedly, it is possible that some types of

refugees that would usually have ended up in Denmark would have tried other countries instead, and that some of those granted residence after July 2002 would have migrated further after having tried living off the new low Social Assistance. As we shall see below, there are no signs in the data of any such selection; the number of residence permits did in fact fall in the 3rd quarter of 2002 compared to the first two quarters, but in the 4th quarter the number was back again at the level of the first two quarters. And the rate of re-migration of the newly accepted refugees is very low. Still, we cannot completely rule out the possibility that the composition of the group of refugees in terms of unobservable characteristics did change around July 2002, and that the types that became more rare were also the ones with the lowest chances of employment. This could bias the estimated effect of the benefit reduction upwards. On the other hand, this choice (and thus selection) would still be a reaction to the reform, although not a labor supply response.

Nevertheless, there are good reasons to believe that the randomness by which refugees were ending up receiving a residence permit in Denmark either before or after July 2002 was not influenced by the reform but governed by the forces that create wars and disasters around the world and by the processes that direct the victims of these events to different countries. Still, the composition of the group of refugees – by country, age, gender, and family type – might have been changed significantly around July 2002 simply by these forces. So we have to check whether this did happen.

Thus, the reform has generated some exogenous variation that allows us to compare the labor supply of individuals that have randomly been subjected to very different financial incentives. Having arrived at November 2003 or 2004 there is a sharp and deterministic discontinuity in the residence seniority dimension concerning the level of SA benefits that the refugees are eligible for. Therefore, we will use a regression discounted approach.

Using the familiar notation of Heckman, Lalonde, and Smith (1999), let Y_1 represent the employment status if a refugees is only eligible for Start Aid and Y_0 if the refugee is eligible for the ordinary SA benefits. We want to study the effect of the benefit reduction, $Y_1 - Y_0$, but we do not – of course – observe both Y_0 or Y_1 for any given refugee; either we observe Y_1 or Y_0 . This study is intended to estimate average treatment effects. The average treatment effect, $E[Y_1 - Y_0|D = 1]$, is equal to $E[Y_1|D = 1] - E[Y_0|D = 1]$. Because we can only estimate $E[Y_1|D = 1]$ and $E[Y_0|D = 0]$ and not $E[Y_0|D = 1]$, we need some identifying assumption to get by. We want to justify that $E[Y_0|D = 0]$ is a reasonable proxy for $E[Y_0|D = 1]$, which boils down to saying that the refugees obtaining their residence permits before July 1, 2002 on average have the same characteristics as the ones who got their permits after the reform was enacted. The difference, $E[Y_0|D = 0] - E[Y_0|D = 1]$, is the selection bias.

As already discussed, the self-selection bias is not a big concern – and any bias that might exist is an effect of the reform that has policy relevance. There are remaining selection or composition issues to be considered, and we will do this shortly. But first we

discuss whether to approach the assessment of $E[Y_1|D = 1] - E[Y_0|D = 0]$ parametrically or not. In our data the two groups, $D = 1$ and $D = 0$, are deterministically given, with one group having residence seniority $S \leq 16$ and the other $S > 16$. We are first of all interested in the effect of the reform for $S = 16$, because here we have the tightest control for the treatment. A key question is how far in both directions we should go; that is, what is the interval in terms of number of residence months a refugee can have in November 2003 and still be used to estimate the effect for $S = 16$?

There is a very strong time trend between residence seniority, S , and pre-treatment employment Y_0 , and therefore a non-parametrical approach can only use refugees that arrived shortly before and after the reform. And since the number of refugees arriving each month is relatively low and the variation in their employment status is large, things are pointing in the direction of a parametric approach. The extrapolation bias is less of a concern compared to having very few observations of a very fluctuating random variable. We will, nevertheless, conclude the analysis with some semiparasitic robustness experiments.

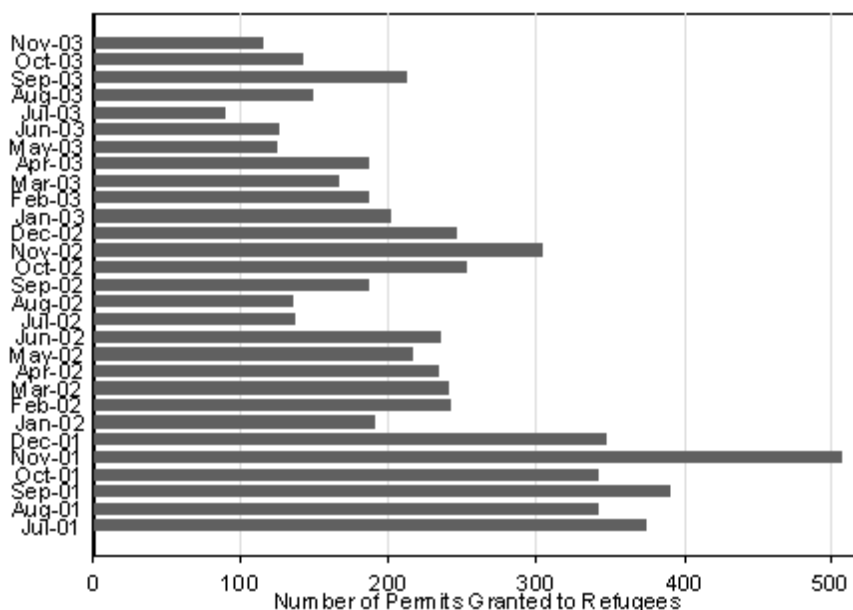
We start out by simply assuming a relationship between seniority and employment, either linear or quadratic, and then subsequently we use earlier cohorts of refugees as a reference group to model the relationship between employment and residence seniority before the reform, $Y_0(S)$. These refugees were subject to the same policy and benefit regime as the ones granted residence before July 2002.

The key identifying assumption that we make in the first part is that the polynomial we use to model the relationship between employment and residence seniority on average fits $Y_0(S)$ well for $S \leq 16$ (in November 2003). In the second approach, we test whether there is a discontinuous change in the difference between observed employment status in November 2003 and the average employment of refugees with the same seniority one year earlier, that is, the difference in the differences. Here the identifying assumption is that the business cycle affects employment chances in the same manner whether one has residence seniority equal to or lower than 16 or higher than 16. In fact, for identification, it is enough that the employment change from 2002 to 2003 depends on seniority in a continuous way: if we detect a discontinuity in the way the employment differences between November 2002 and 2003 related to the seniority at the cut-off point (residence seniority of 16 months), this is then an indication of an effect of the reform.

4 The Analysis

The similarity between the way refugees were allocated to different treatments with a randomized experiment makes a Regression-Discontinuity approach particular attractive for the evaluation of the reform effect. In our case the hypothesis is that the reform has created a discontinuity in the way employment status is related to residence seniority S . And by using an RD design we compare the average employment rate of refugees with

Figure 4: Number of Residence Permits Granted to Refugees.



Source: Own calculations based on register data.

shorter than 16 months and longer than 16 months of residence in November 2003 – in both cases arbitrarily close to 16 months of residence. The effect of reducing the benefits is simply the difference between these two employment rates.

Before we show the results of the estimations we shall briefly discuss the validity of the important assumptions we have to make for identification, namely structural breaks in the observable co-variates and self-selection.

4.1 Self-selection and structural breaks

It can be seen from figure 4 that the number of residence permits granted to refugees did fall in the first three months, July to September, after Start Aid was introduced, but the number was up again during the following three months, October to December, to the same level as during the six months just prior to the reform. Furthermore, in November 2003 the employment rates for the refugees granted asylum in the months July to September are not systematically above the estimated trend, and these three observations do not significantly push up the estimated effect. The refugees with entry data during October to December, the months where many were granted residence, do in fact contribute to a higher estimated reform effect. Finally, in November 2004 the refugees of July to September actually pull the estimated effect down – not up. So there is not much sign of a selection process in terms of the types of refugees that applied for asylum in Denmark that biased the result upwards.

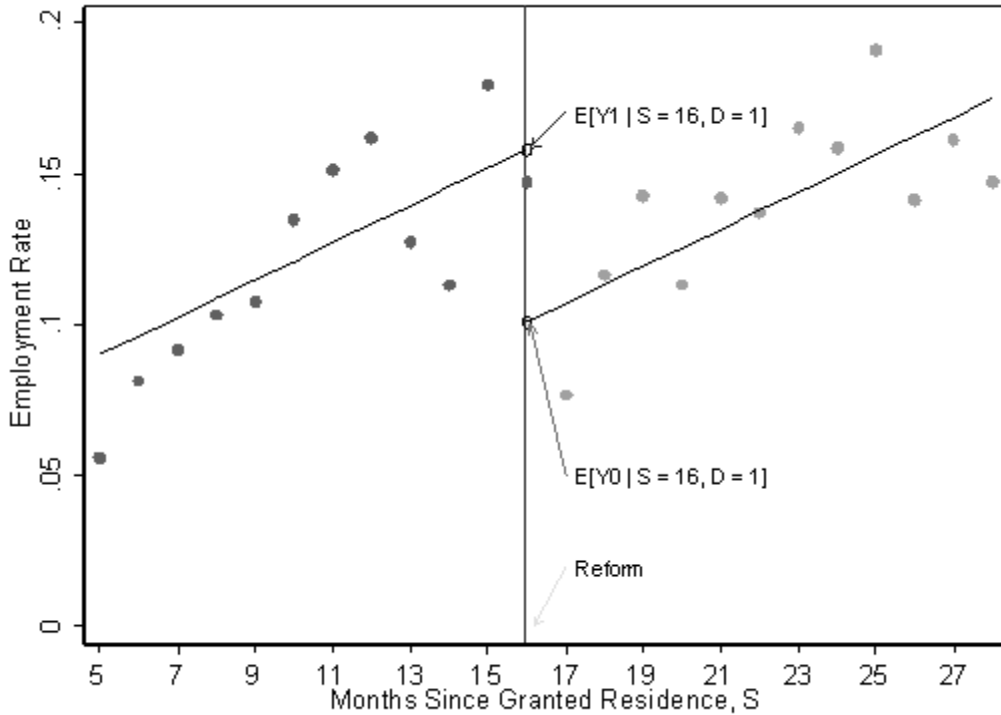
The level of re-migration of refugees is very low but it did increase for the 2002 refugees.

Usually, around 1.5 % of the refugees who arrived in year t re-migrate in year $t+1$. For those arriving in 2002, 3.2 % re-migrated in 2003. This number is far too small to change our result. Even if all those that re-migrated had zero employment chance in Denmark, this could only change our result marginally, because the jobless rate of refugees in 2003 was above 85 percent.

We conclude that the self-selection into the country of more employable refugees and out of the country of the less employable ones does not seem to have taken place in significant numbers. It is likely that the process of getting assylum in a north European country is so difficult and that these countries' welfare systems are so similar, viewed from the typical refugee's home country, that such a response to a benefit reform in a small country like Denmark is simply not an issue. So selection bias on unobservable characteristics is not likely to be a big concern. The consequence of this is that it is preferable to include observable characteristics, X , as controls in the estimations below (Heckman et al. 1999).

Having considered the selection issue we will now continue with another issue. There might be structural changes at the time of the reform in some of the observable characteristics of the group of refugees. Even though we can control for the development of these characteristics, actual discontinuities in these variables might bias the estimate of the benefit effect. Using pseudo experiments we test for structural changes. The specification test we perform is a number of estimations of a logit model where the probability of a given observable characteristic, say whether a refugee originates from Somalia, is detected for structural changes. The dependent variable is z_i ; $z_i = 1$ if country of origin is Somalia and 0 otherwise – or whatever characteristics we test. The specification of the independent variables we use is: $\beta_0 + \beta_1 \Pi_i + f(S_i) + \epsilon_i$, where $\Pi_i = 1$ if month of residence being granted is July 2002 or past this month and 0 otherwise; $f(S_i)$ is some functional form of months since granted residence and ϵ_i is an error term. In the specification tests $f(S_i)$ is either constant or linear. Clear signs of structural changes are consistent with significant estimates of β_1 . Moreover, it is required that pseudo experiments close to the time of implementation show weaker signs of structural changes. In order to test this we conduct pseudo experiments as if the reform took place in June 2002 and August 2002. First we reestimate with $\Pi'_i = 1$ if month of residence being granted is August 2002 or past this month, which produces a corresponding β'_1 . And similarly we obtain a β''_1 by taking the reform month to be June 2002. If the estimate of β_1 is significant and more significant than β'_1 and β''_1 the data support structural changes at the time of implementation and invalidate the RD if no control variables exist for these. In this case the effect is partly due to a compositional effect and partly due to a reform effect.

Figure 5: The Regression-Discontinuity Design.



Note: Observed employment rates of refugees by month since granted residence.

4.2 Results with linear and quadratic employment trends

We first estimate a parametric RD model. Data on employment rates of all refugees are utilized to estimate both $E[Y_0|S = 16, D = 1]$ and $E[Y_1|S = 16, D = 1]$. The treatment effect is measured at the margin for $S = 16$ and equals the difference between these two expectations. Figure 5 illustrates the essence of the RD approach. The figure shows the employment rates by month of seniority in November 2003. Refugees granted residence 16 months or less previously (i.e., to the left of the vertical line) are all governed by the new integration reform and, hence, eligible only for the low Start Aid. Refugees with seniority higher than 16 are not affected by the SA reform. The treatment effect given seniority 16 is the difference between the two predicted outcomes at 16 months since residence being granted, which is marked with circles on the cut-off line.

We start out from a standard logistic model. Let the utility difference between employment and no employment be approximated by the linear index I and assume that the decision to supply labor is subject to error given by ϵ with zero means. Thus refugee i is expected to be employed if and only if

$$I_i - \epsilon_i \geq 0.$$

Let Y_i be the employment status of refugee i ; Y_i equals 1 if i is employed in November 2003. Assume a logistic distribution of the error term. The probability that refugee i is employed is then $P(Y_i = 1) = G(I)$, where G is a cumulative logistic function. Furthermore, the indicator function, I_i , is assumed to take the form $\beta_0 + \beta_1 f(S) + \beta_2 X_i + \epsilon_i$, where $f(S)$ is some functional form that models the relationship between residence seniority and employment, the β 's are coefficients to the co-variates X and $f(S)$, and β_0 is a constant term.

Now the implementation of the RD model is straightforward. Consider the indicator function:

$$I_i = \beta_0 + \beta_1 f(S) + \beta_2 X_i + \alpha D + \epsilon_i \quad (1)$$

where D is a treatment dummy variable with $D = 1$ if the residence seniority $S \leq 16$ and $D = 0$ otherwise. Hence, α measures the treatment effect. Under the assumption that $f(S)$ represents the relationship between employment and residence seniority and a treatment effect that is constant in S , α can be estimated without bias by a logistic estimation of this model. After obtaining the coefficients we can compute the treatment effect after 16 months of residence, that is, at the cut-off point, as the difference between $E[Y_1|S = 16, D = 1]$ and $E[Y_0|S = 16, D = 1]$.

Table 3 below shows the estimation results. The observable characteristics that we control for are gender, age, age squared, marriage, children, country of origin, and education. Model (1) assumes a linear effect of seniority on employment. Model (2) assumes a quadratic trend. Note that for each year only refugees granted residence 6 months prior to the reform and refugees granted residence within 6 months after the reform are included in these estimations. This is done in order to minimize the effect on predicted outcome of observations "too far away" from the discontinuity. The models (3) and (4) repeat the models (1) and (2) but now include refugees granted residence up to 12 months prior to and after the implementation of the reform. All models are logit models estimating the probability of employment.

In November 2003 there seems to be a positive employment effect of the benefit reduction. In November 2004, only model (4) suggests a positive employment effect.

The key identifying assumption here is that there is no selection on unobservables and that $f(S)$ is a good enough model of the influence of seniority. Trend breaks in the observable characteristics at the month of the reform might weaken the RD evaluation because of correlation between the instrument and the variable with the structural break. However, structural changes in observable characteristics are testable. Table 5 shows the t-statistics of the specification tests. Two functional form specifications are applied: a linear and a second order polynomial. There is empirical evidence of structural changes in the probability of originating from Afghanistan or Bosnia-Herzegovina. In both cases

Table 3: RD estimates, All Refugees, November 2003 and November 2004.

	2003				2004			
	(1) Linear Coef.	(2) Quadratic Coef.	(3) Linear Coef.	(4) Quadratic Coef.	(1) Linear Coef.	(2) Quadratic Coef.	(3) Linear Coef.	(4) Quadratic Coef.
Treatment Effect	.794*** (.281)	.792*** (.283)	.688*** (.192)	.819*** (.192)	.544** (.232)	.535** (.235)	.483*** (.156)	.522*** (.157)
S	.084** (.039)	.031 (.210)	.072*** (.014)	.192*** (.046)	.048** (.032)	-.136 (.295)	.035 (.011)	.103* (.055)
S ²		.002 (.006)		-.003*** (.001)		.003 (.005)		-.001 (.001)
Constant	-4.782*** (1.189)	-4.353** (2.045)	-4.564*** (.713)	-5.642*** (.837)	-3.555*** (1.273)	-.943 (4.387)	-3.125*** (.637)	-4.131*** (1.020)
Observable characteristics	YES	YES	YES	YES	YES	YES	YES	YES
R ²	.1779	.1780	.1752	.1768	.1446	.1447	.1567	.1570
sample	±6	±6	±12	±12	±6	±6	±12	±12
No. of observations	2,607	2,607	5,892	5,892	2,541	2,529	5,771	5,771

Notes: *: significant at a 10 percent level; **: significant at a 5 percent level; ***: significant at a 1 percent level. Standard errors in parentheses. Logistic estimation.

Source: Own calculations based on register data.

the largest t-statistic prevails at the time of implementation of the reform. None of the other co-variables show signs of structural changes. In order to check robustness of our results we have performed estimations with refugees originating from Afghanistan and Bosnia-Herzegovina excluded, see table 4.

Table 4 presents the estimation results where refugees originating from Afghanistan and Bosnia-Herzegovina are excluded. Here we show results assuming a quadratic seniority trend, but again, with both 6 and 12 months before and after the discontinuity. Again we test the specification both with and without co-variables. The results in the table suggest that the positive employment effect of the benefit reduction is robust in 2003 – but apparently very sensitive to the specifications when evaluated in 2004. The weaker result that is obtained for 2004 could be due to the expiring of the IP shortly after November 2004. Below we will try to model the relationship between residence seniority and employment using the experience of earlier waves of refugees involved in order to allow for such effects.

The reliability of non-experimental estimators is questioned in Bertrand et al. [2002] and Hoxby [2004]. They show that many pseudo experiments wrongly suggest reform effects in months where there was no reform. The pseudo experiments that we conduct test whether the data also support reforms in other months than the reform month. We conduct experiments as if the reform took place in May 2002, June 2002, August 2002, and September 2002. Table 6 tabulates the t-statistics for the pseudo experiments. In July 2002 significant effects are documented. Assuming a linear seniority trend, no signs of reform prevail in the pseudo experiments. Assuming a second order polynomial trend, we observe that the data suggest a reform in August 2002 even though no reform took place in 2002. As the t-statistic in August is of smaller magnitude than the t-statistic in July 2002 the data favor a reform in July 2002 and not in August 2003. In 2004 there are no signs of an effect of a reform, either at the time of implementation or in any of the other pseudo experiments.

The coefficients in tables 3 and 4 are not the effects measured in percentage points, since the model is non-linear. The only thing we can infer from the coefficients is the direction of the effect. Tables 7 and 8 show the predicted employment rates (necessary to obtain the employment effect) and the reform effect. In most cases positive effects are observed. In 2003 we find a positive employment effect independent of the functional form assumption. Assuming a linear trend implies an effect between 5.5 and 3.8 percent points depending on whether we include 6 or 12 months on either side of the reform seniority month. In the model where a second order polynomial S -trend is imposed the effect is 5.6 and 5.5 respectively. These increases in employment are equivalent to relative changes of 38 - 40 percent and elasticities close to unity, which are large effects. On the other hand there are no robust effects in November 2004, that is 28 months after the reform. The non-existence of a reform effect in 2004 could be due to a retention effect which masks

Table 5: Specification Tests of the RDD, t-statistics.

	(1)			(2)		
	Actual Experiment	Pseudo Experiments		Actual Experiment	Pseudo Experiments	
	July 2002	June 2002	August 2002	July 2002	June 2002	August 2002
Age	-2.45	-3.23	-1.73	0.48	-1.16	1.93
Female	.57	1.79	-.32	-2.47	.10	-4.23
Married	-3.41	-3.17	-4.01	-.53	-.02	-1.79
Adults with Dependent Children	-5.09	-4.73	-5.55	-.93	-.16	-1.93
Somalia	2.64	4.19	.80	3.06	6.39	-.80
Afghanistan	-13.92	-12.94	-12.42	-7.24	-4.46	-4.00
Iraq	1.15	-1.78	1.43	4.69	-1.50	5.17
Iran	10.57	10.93	10.81	4.68	6.29	4.94
Bosnia-Herzegovina	5.60	6.34	5.86	-4.54	-2.67	-3.97

Notes: model (1) assumes a linear residence seniority trend and model (2) assumes a second order residence seniority trend;

*: significant at a 10 percent level; **: significant at a 5 percent level; ***: significant at a 1percent level.

Age is measured in months; Female equals 1 if female and is 0 otherwise; married

Table 6: Pseudo Experiments, RDD, t-statistics.

		(1)			
Actual Experiment		Pseudo Experiments			
July 2002		May 2002	June 2002	August 2002	September 2002
2003	2.05**	-.80	.29	1.52	0.17
2004	.96	-.14	.82	1.45	1.63
		(2)			
2003	2.88**	.01	.53	2.86**	2.35
2004	2.95**	.02	.53	3.04	2.60

Notes: model (1) assumes a linear MGR trend; model (2) imposes a trend described by a second order polynomial; *: significant at a 10 percent level; **: significant at a 5 percent level; *** significant at a 1 percent level.

the reform effect. Another explanation is the reform enacted January 1, 2004, where the municipalities were given considerable economic incentives to get refugees off welfare, because they would have to carry a bigger share of the expenses in the future. Hence, from then on there was a stronger incentive to help refugees on the high SA than those on Start Aid.

In order to shed some light on who contributes to the effect we conduct the estimation on subgroups. Table 9 shows the effects for selected sub-groups. Several features are worth mentioning. First, the quantitative results are not sensitive to the parametric assumptions. For this reason all the following comments are based on the specification where the second order polynomial is assumed, because it is the more flexible specification and it fits the data best. Second, there are positive effects on the employment rates of the 2002 integration reform. The estimated effects in November 2003 vary between insignificant results and highly significant increases of more than 100 percent. For instance, females with dependent children more than doubled their employment rate, which was also very low initially. Males with no dependent children had initially a relatively high employment rate, 21 percent, and increased it by 13 percentage points. The effect for young refugees is hardly significant and for single females the effect is not significant. In November 2004 more or less the same pattern is found. There are differences though, for instance with respect to children – no children. Most effects are large in relative terms, as the labor market performance of refugees, in general, is poor.

4.3 Results using lagged employment trend

The relationship between employment and residence seniority is a very important factor for the identification of the effect. Because there is considerable uncertainty regarding this relationship we go a step further in this section and use earlier cohorts of refugees to identify $Y_0(S)$ as advocated in Cook and Campbell (1979) and Heckman, Lalonde,

Table 7: RD estimations, November 2003 and November 2004.

	2003			
	(1)	(2)	(3)	(4)
Pr(Emp ₂₀₀₃ = 1 D = 1, S = 16)	.150	.148	.135	.153
Pr(Emp ₂₀₀₃ = 1 D = 0, S = 16)	.089	.087	.087	.092
Predicted Effect of Reform at S = 16	.062***	.061***	.047***	.062***
sample	±6	±6	±12	±12
Observable characteristics	YES	YES	YES	YES
Functional form	linear	Quadratic	linear	Quadratic
	2004			
Pr(Emp ₂₀₀₄ =1 D = 1, S = 16)	.238	.232	.241	.2551
Pr(Emp ₂₀₀₄ = 1 D = 0, S = 16)	.180	.175	.184	.188
Predicted Effect of Reform at S = 16	.058**	.056**	.056***	.063***
sample	±6	±6	±12	±12
Observable characteristics	YES	YES	YES	YES
Observable characteristics	linear	Quadratic	linear	Quadratic

Notes: *: significant at a 10 percent level; **: significant at a 5 percent level; *** significant at a 1 percent level.

Table 8: RD estimations, November 2003 and November 2004.

	2003			
	(1)	(2)	(3)	(4)
Pr(Emp ₂₀₀₃ = 1 D = 1, S = 16)	.139	.135	.160	.140
Pr(Emp ₂₀₀₃ = 1 D = 0, S = 16)	.106	.101	.112	.102
Predicted Effect of Reform at S = 16	.033	.038**	.048**	.038***
sample	±6	±6	±12	±12
Observable characteristics	NO	YES	NO	YES
	2004			
Pr(Emp ₂₀₀₄ =1 D = 1, S = 16)	.220	.215	.248	.228
Pr(Emp ₂₀₀₄ = 1 D = 0, S = 16)	.205	.198	.210	.209
Predicted Effect of Reform at S = 16	.015	.017	.038	.020*
sample	±6	±6	±12	±12
Observable characteristics	NO	YES	NO	YES

Notes: *: significant at a 10 percent level; **: significant at a 5 percent level; *** significant at a 1 percent level. Quadratic trend.

Refugees from Afghanistan and Bosnia.Herzegovins excluded.

Table 9: RD estimations by Sub-Groups, Expected employment and treatment effects for $S = 16$, quadratic seniority trend, 2003 and 2004.

	2003			2004		
	Obs	$E[Y_1 S=16]$	$E[Y_0 S=16]$	Obs	$E[Y_1 S=16]$	$E[Y_0 S=16]$
Males	2,661	.296	.192	2,593	.413	.361
Females	3,231	.084	.033	3,178	.178	.089
Single	2,113	.207	.143	1,955	.280	.334
Married	3,779	.138	.065	3,816	.248	.118
Adults with no Dependent Children	2,426	.247	.151	2,166	.352	.354
Adult with Dependent Children	3,457	.100	.054	3,605	.217	.101
Age >30	3,531	.148	.065	3,664	.265	.146
Age ≤ 30	2,361	.164	.130	2,107	.232	.254
Single Males	1,380	.360	.225	1,237	.438	.469
Married Males	1,281	.267	.159	1,356	.416	.260
Single Females	693	.082	.038	718	.207	.155
Married Females	2,498	.086	.031	2,460	.174	.070
Males with no Dependent Children	1,536	.357	.208	1,385	.460	.424
Males with Dependent Children	1,125	.216	.168	1,208	.380	.281
Females with no Dependent Children	899	.132	.055	781	.241	.225
Females with Dependent Children	2,332	.073	.026	2,397	.172	.057

Notes: *: significant at a 10 percent level; **: significant at a 5 percent level; ***: significant at a 1 percent level.
 Controls for gender, age, children, country of origin, and educational attainment are included whenever feasible.

and Smith (1999). The interaction going on between residence seniority and employment could be the influence of traditions within this country as well as the influence of elements of the introduction program such as the timing of the different courses, the speed at which the language is learned, etc. All this is unobserved but likely to be constant for constant observable characteristics and constant macro circumstances. Therefore we wish to model $Y_0(S)$ using earlier cohorts of refugees, but making sure that they have been subject to exactly the same introduction program as the ones who arrived in the month before and in the month after the reform. The treatment and control groups from above arrived ± 12 months from July 2002, that is from July 2001 until June 2003. We continue with the same control and treatment groups, groups C and T respectively. We then supplement the analysis with a reference group R composed of the refugees that month by month arrived exactly one year earlier, that is from July 2000 to June 2002. We measure the employment status of the reference group one year earlier as well, that is, in November 2002. Notice that the refugees in R arriving between July 2000 and June 2001 will have the same residence seniority in November 2002 as the control group will have in November 2003, and similarly, the R -refugees arriving between July 2001 and June 2002 will have the same seniority as the treatment group in November 2003. Everybody will have been subject to the same introduction program, the difference being that the treatment group T , receives the low SA while the other group receives ordinary SA.¹⁰

The idea is to use the reference group to control for $Y_0(S)$ as they represent the relationship between employment and residence seniority before the reform. So by comparing the treatment and the control groups to the reference group we can difference out this relationship. Formally, we assume that the employment status of refugee i is a function of the pre-reform time trend $Y_0(S)$, plus the influence of relevant characteristics like education, gender, age, etc., captured by X_i :

$$Y_i = \gamma_0 + \gamma_1(X_i) + Y_0(S) + \epsilon_i.$$

At any given point in the calendar, let Y_S (X_S) be the average of Y_i (X_i) over all i with seniority S at that point in time. Thus in November 2002 the average employment rate for each S is given by the stochastic equation,

$$Y_S = \delta_0 + \delta_1(X_S) + Y_0(S) + e_S, \tag{2}$$

with error term e_S . Assuming that the Start-Aid reform changes the employment trend to $Y_1(S)$, the employment status of refugees i arriving after the reform is $Y_i = \beta_0 + \beta_1(X_i) +$

¹⁰The reference group is of course also a control group that represents the way employment and residence seniority are related in the absence of a reform. In the traditional DID set-up, the groups T and C would be the treatment groups and group R would be the control group. But since the refugees in the groups T and C are different individuals we prefer the labeling used here.

$Y_1(S) + \varepsilon_i$. The crucial identifying assumption we make is then that $Y_1(S) = Y_0(S) + \alpha$.¹¹ Thus in November 2003, the equation is

$$Y_i = \beta_0 + \beta_1(X_i) + Y_0(S) + \alpha D + \varepsilon_i, \quad (3)$$

where we assume that $\beta_0 = \gamma_0 = \delta_0$ and $\beta_1 = \gamma_1 = \delta_1$. And where $\varepsilon_i = \epsilon_i - e_S$ and

$$D = \begin{cases} 1 & \text{if } S \leq 16 \\ 0 & \text{if } S > 16 \end{cases}.$$

Now by subtracting 3 and 2 recorded in November 2003 and 2002, respectively, we obtain an equation in the differences to the lagged mean (over each S),

$$Y_i - Y_S^{2002} = \beta_0 + \beta_1(X_i - X_S^{2002}) + \alpha D + \varepsilon_i \quad (4)$$

where Y_i is the employment status in November 2003. The general employment shift between 2002 and 2003 affecting all refugees is captured by β_0 ; and ε_i is an error term with mean zero. The central parameter is α , which provides an estimate of the average treatment effect. Under the assumption of a constant treatment effect in S and that $Y_0(S)$ is the same in November 2002 and in November 2003 (i.e., $Y_1(S) = Y_0(S) + \alpha$), α can be estimated without bias by OLS estimation of the difference between the observed employment status of refugee i , Y_i , and the average 2002-employment, Y_S^{2002} , at each value of S as specified in 4.

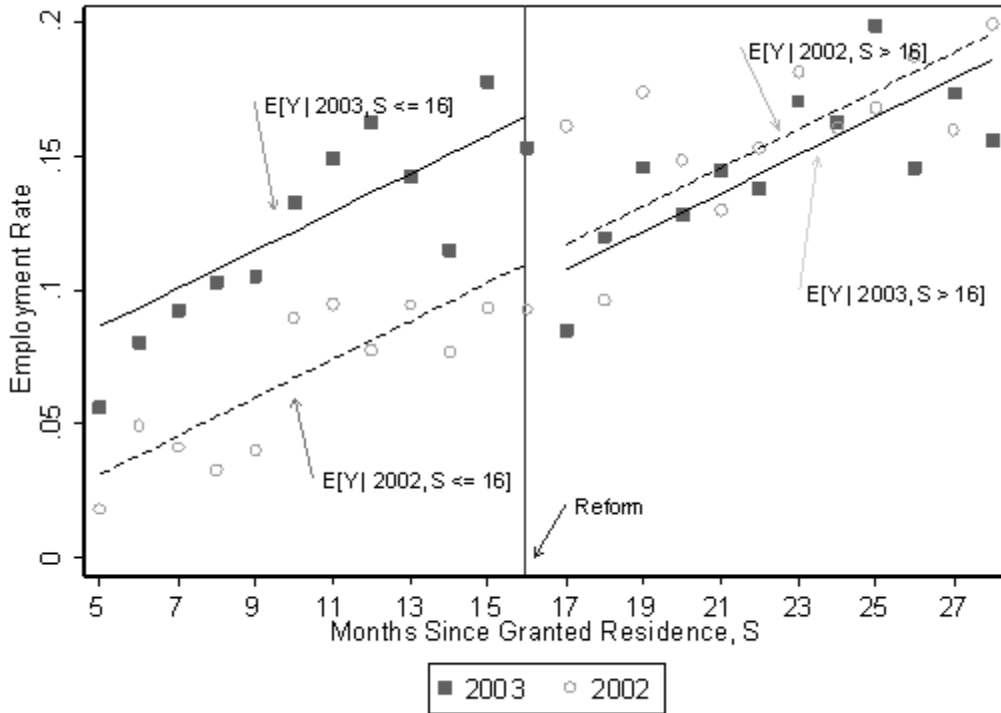
By focusing on a specific date, November 2003 (and 2004), we measure the performance of both groups at the same point in the calendar, which controls for the influence of both the business cycle and seasonality.

Figure 6 illustrates the approach. The figure shows the employment rates for refugees in November 2002 and 2003 as a function of residence seniority (each year). The employment status of refugees in 2003, Y_i , both for the treated and the non-treated, is measured relative to the average employment in 2002 of the reference group, Y_S^{2002} (group of refugees who in November 2002 had the same residence seniority). Thus the discontinuity that would suggest a reform effect should show up in the difference $Y_i - Y_S^{2002}$ between seniority months 16 and 17. We will estimate the magnitude and significance of this directly by estimating the discontinuity in $Y_i - Y_S^{2002}$ between seniority month 16 and 17 in November 2003. This is then a difference-in-difference estimation as formalized above where we compare the difference $Y_i - Y_S^{2002}$ before and after the cut-off point in seniority.

Figure 6 shows $Y_i - Y_S^{2002}$ for the entire group of refugees and for a smaller group where the four big refugee groups have been excluded because the numbers of arrivals of each group changed significantly either around July 2001 or around July 2002. Nevertheless,

¹¹We have also experimented with letting the reform effect α be a function of S (linear and quadratic). This does not change the results below qualitatively.

Figure 6: The Difference-in-Differences.



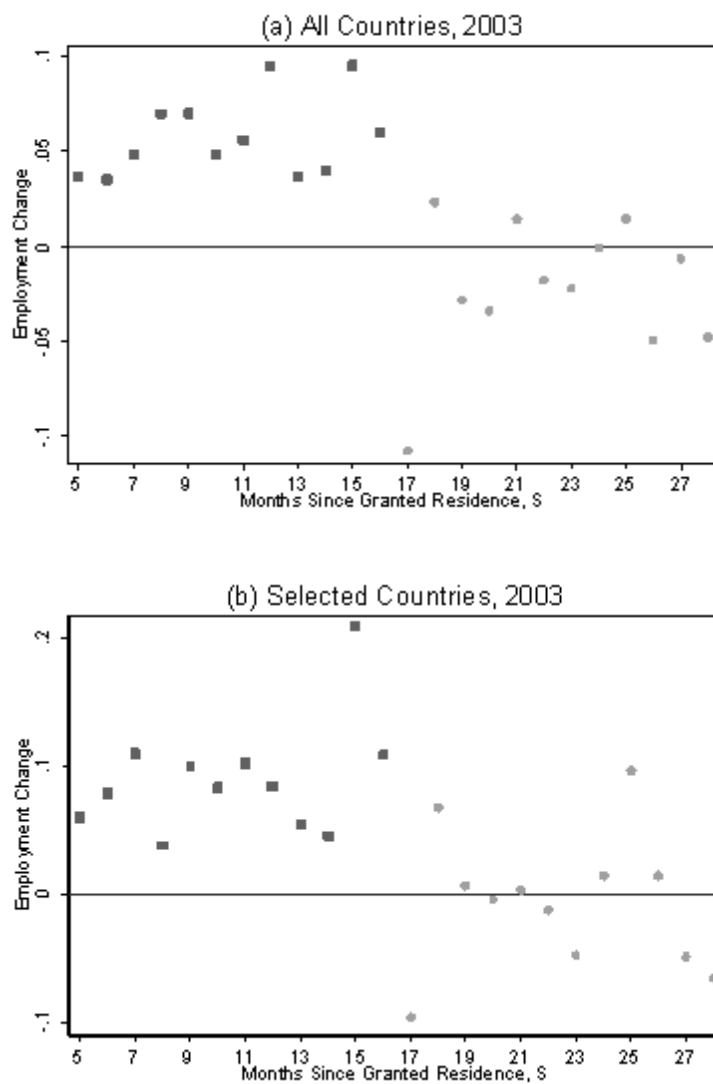
Note: Observed employment rates of refugees

in both cases there is a strong indication of a reform effect.

In order to quantify the effects of the reform on the employment rate, tables 10, 11, and 12 tabulate the effect for all refugees and for selected sub-groups. For November 2003, the results are to a high degree consistent with the findings when assuming a linear or quadratic relationship of $Y_0(S)$ above. The general results point to a somewhat bigger effect after 16 months of residence, namely an effect between 6.9 and 8.9 percentage points. After 28 months of residence, that is in November 2004, we now find positive employment effects of more or less the same magnitude as after 16 months. Focusing on model (3), which we consider to be the best specification, the effect in 2004 is a little smaller than the effect in 2003. Nevertheless, these results should be interpreted with caution, as there is still the concern as to how well we have modeled a potential lock-in effect up to the termination of the IP.

As before, the effect is in particular driven by the married and by refugees with dependent children. But now the largest absolute effect is found for females with dependent children.

Figure 7: DID estimations applying earlier arrived refugees as reference Group, 2003.



Source: Own calculations based on register data.

Table 11: DID, alternative specification, November 2003 and November 2004.

	2003			
	(1)	(2)	(3)	(4)
	Coef.	Std. err.	Coef.	Std. err.
Treatment Effect	.090*** (.013)	.097*** (.014)	.218*** (.014)	.218*** (.066)
Constant	-.026*** (.009)	-.007 (.097)	.488 (.410)	.011* (.006)
AFG + BOS included	YES	NO	YES	NO
Som + iraq included	YES	NO	YES	NO
Observable characteristics	NO	NO	YES	YES
R ²	.0170	.	.1014	.1337
sample	±6	±6	±6	±6
No. of observations	2607	827	2607	827
			2004	
	Coef.	Std. err.	Coef.	Std. err.
Treatment Effect	.071*** (.015)	.115*** (.026)	.054*** (.015)	.072*** (.027)
Constant	-.019** (.010)	-.027* (.018)	-.091 (.107)	-.012 (.212)
AFG + BOS included	YES	NO	YES	NO
Som + iraq included	YES	NO	YES	NO
Observable characteristics	NO	NO	YES	YES
R ²	.0088	.0213	.1414	.1162
sample	±6	±6	±6	±6
No. of observations	2541	814	2541	814

Notes: *: significant at a 10 percent level; **: significant at a 5 percent level; ***: significant at a 1 percent level.

Standard errors in parentheses.

Source: Own calculations based on register data.

Table 12: The DID by Sub-Groups, 2003 and 2004.

SUB-GROUP	2003		2004	
	Obs	Effect	Obs	Effect
Males	2,661	.066***	2,593	.080***
Females	3,231	.077***	3,178	.106***
Single	2,113	.021	1,955	.041*
Married	3,779	.091***	3,816	.117***
Adults with no Dependent Children	2,435	.086***	2,166	.069***
Adults with Dependent Children	3,457	.091***	3,605	.101***
Age >30	3,531	.084***	3,664	.109***
Age ≤ 30	2,361	.024	2,107	.053***
Single Males	1,380	.035	1,237	.040
Married Males	1,281	.100**	1,356	.112***
Single Females	733	.072***	718	.057**
Married Females	2,498	.084***	2,460	.120***
Males with no Dependent Children	1,536	.122***	1,385	.062**
Males with Dependent Children	1,125	.089**	1,208	.098***
Females with no Dependent Children	899	.058**	781	.088***
Females with Dependent Children	2,332	.089***	2,397	.107***

Notes: *: significant at a 10 percent level; **: significant at a 5 percent level; ***: significant at a 1 percent level.

Effect: average treatment effect after 16 months seniority

5 Conclusion

The main results from the analysis of the cross-section data collected in November 2003 show that the employment rate increased on average by 3.4 to 6.2 percentage points depending on the specific assumptions imposed. These are very big effects in any case: relative changes in 2003 in the range 35 to 70 percent, which suggests labor supply elasticities close to or above unity (between 0.8 and 1.8). Our estimates are somewhat higher than many that can be found in the literature. On the other hand, the employment rates for refugees prior to the reform were also much lower than is usually the case, which allows for higher elasticities. This effect has to be weighed against the fact that a large majority of refugees now live on the reduced income, because the employment rate for refugees is still very low after the reform.

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