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Abstract

In 2002 the Danish government reduced the size of cash transfers to new refugees. We exploit the reform to study the effect of lower transfers on educational outomces of refugee children. Surprisingly, the reduction in parental benefits has no negative effect on educational outcomes of the children, such as test scores, probability of completion of the 9th grade or probability of enrollment in upper-secondary education. Likewise, children of parents affected by the reform are not forced to earn more in youth. Refugee parents increase their labour supply and earn more to compensate for the loss in income, but on average the increase in earnings does not compensate for the decline in benefits.

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1 Introduction

More than 3 million individuals have sought asylum in the EU since the beginning of the Syrian Civil War in 2011. 27.4 percent or 829,400 persons among the asylum seekers were children, who either arrived with their families or asked for protection as unaccompanied minors.[1] Sweden, Denmark and Norway alone have granted almost 0.5 million residence permits to refugees and their family members, 169,100 of which were granted to children.

To limit the burden on public finances, it is crucial that the refugees are integrated in the labour market, and that their children get an education which provides them with the skills to find a job when they grow up. Another way of limiting the burden on public finances is to cut down on refugee-related expenditures. In September last year the Danish governement introduced the so-called integration benefit which in effect reduced the cash benefit given to refugees by approximately 40 percent.[2] Reducing benefits may motivate refugees to find a job. On the other hand, it may take refugees several years to find employment initially, if they can find one at all (Bevelander 2016, Bratsberg et al. 2016, Konle-Seidl and Bolits 2016, Aiyar 2016, Schultz-Nielsen 2016). These families will face a lower income which could have adverse effects on integration. For instance, it could force refugees to cut down on educational expenditures or move to an area with schools of lower quality. This will harm the educational outcomes of the refugee children and, consequently, their probability of future employment.

In this paper we investigate the effect of the reduction in cash benefit transfers to refugees on the educational outcomes of their children. We use the introduction of the so-called *Start Help* (in Danish: Starthjælp) benefit to refugees in Denmark on the 1st of July, 2002 which by construction is very similar to the integration benefit introduced in 2015. Our identification relies on the fact that all refugee parents arriving after reform cutoff were entitled to *Start Help* whereas the group arriving just before the reform

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[1] Eurostat\migr_asyappctza.

[2] The exact size of the reduction varies somewhat with the number of children in the family, age of the parents etc. The 40 percent reduction holds for a family consisting of two parents, both older than 30, with children. Source: http://bm.dk/da/Aktuelt/Pressemeddelelser/Arkiv/2015/07/Straksindgreb%20paa%20asylomraadet%20-%20ny%20integrationsydelse%20til%20nytilkomne%20udlaendinge.aspx.

continued to receive the full cash benefit amount (in Danish: kontanthjælp) without any exemptions. The exogenous reform cut-off prevents selection of more skilled or knowledgeable parents, whose children are expected to have better outcomes, into the group receiving higher benefits. We compare the outcomes of children whose parents arrived within a year before the reform to the outcomes of children whose parents arrived within a year after the reform.
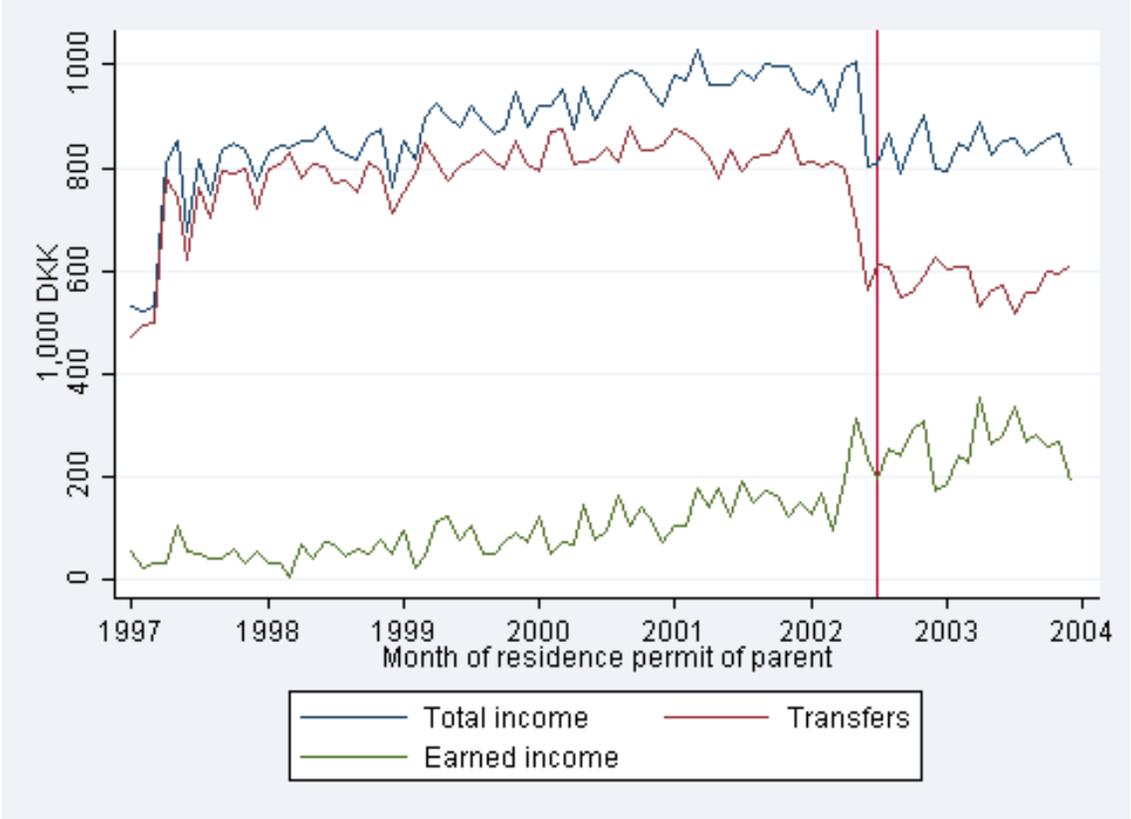

Figure 1: Accumulated Parental Income Over 3 Years since Immigration

According to Figure 1 the reform resulted, as expected, in lower transfer payments. Over the first three years in Denmark refugee parents received around 231,600 Danish

Kroner or 31,130 Euros less following the reform cutoff compared to before.[3,4] However, parents affected by the reform also had a higher earned income, indicating that the reform motivated refugees to find a job. This finding is consistent with Rosholm and Vejlin (2010) and Andersen et al. (2012) who investigate the labour market consequences of the reform. However, the increase in earned income did not fully compensate for the fall in transfers. As Figure 1 shows, the total 3-year income of parents arriving after the reform was about 14% lower than the income of parents arriving just before. This income gap ceased to exist after 7 years of residence in Denmark, when both earned income and transfers received converged for both groups, as shown in Figure 3 in the Appendix. After 7 years of residence in Denmark refugees were again entitled to receive the full cash benefit.

At the same time raw evidence in Figure 2 shows the main result of the paper. It suggests that there is no systematic difference in test scores between children whose parents could receive the full cash benefit and those whose parents were entitled to the lower *Start Help* benefit. The remainder of the paper shows that this result is robust to controlling for various characteristics of the children and their parents. Futhermore, there is no statistitically significant difference in school completion or enrollment in upper-secondary education between the two groups of refugee children. We also investigate earned income in the youth and find no significant difference. Thus, we find no support for the hypothesis that children in families affected by the reform worked more hours in youth to compensate for lower parental income.

We conclude that the reduction of cash transfers improves labour market outcomes of refugee parents in the short run, but has no impact on the education outcomes of their children. Our findings are in line with other findings from Scandinavian research on the impact of parental income on children's outcomes. In particular, Aakvik et al. (2005), Loeken (2010), Humlum (2011), Loeken et al. (2012) find that there is no or

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[3]The average fall in overall transfers does not necessary correspond to the average fall in cash benefit payments, as refugees could substitute across benefits. For example, they could apply for higher housing support benefits due to lower income. The identified fall in transfers and overall income, in combination with increase in employment rates and employment income, is robust to controlling for child's and parent's characteristics as shown in Table 7 in the Appendix.

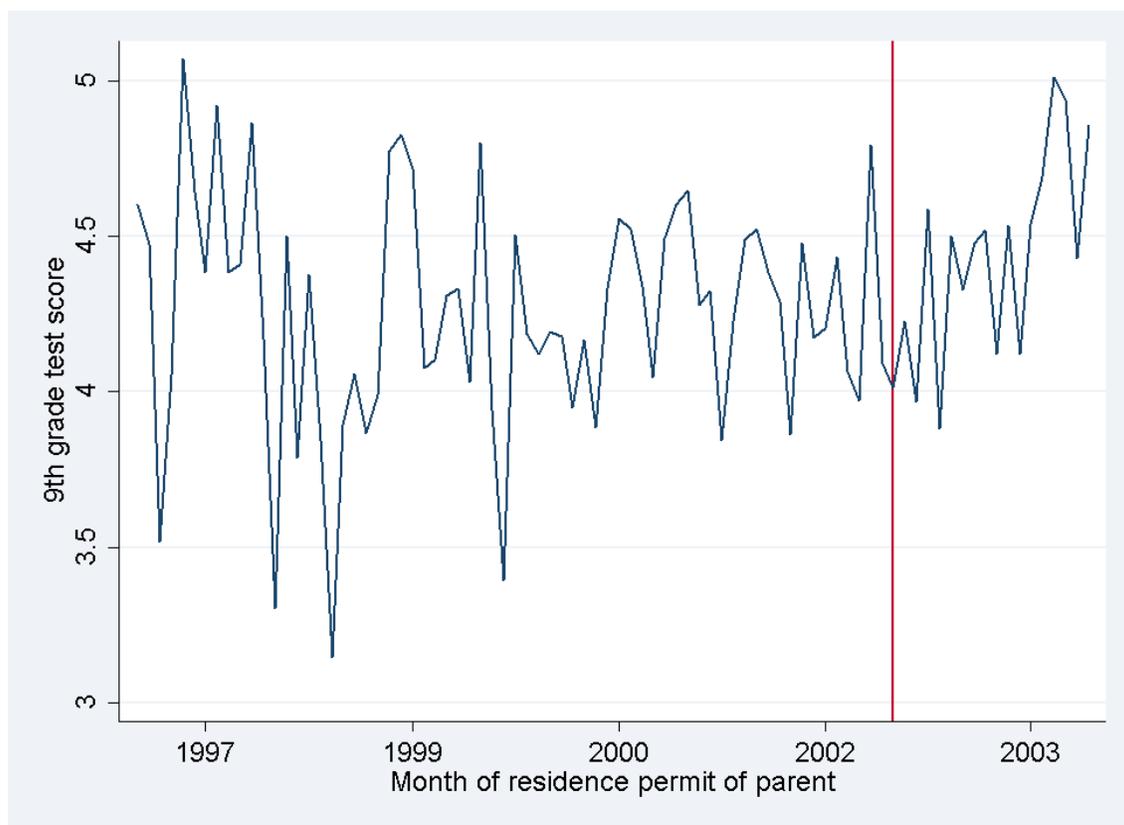[4]At the exchange rate 7.4376 DKK per EUR.

Figure 2: Average Test Score in the 9th Grade Tests.

little impact of parental income on the educational outcomes and adult income of children contrary to the U.S. research by Acemoglu and Pischke (2001), Dahl and Lochner (2012), Oreopoulos et al. (2008) reporting a strong positive impact of parental income. This difference across countries is at least partially explained by the Scandinavian generous welfare state. In Scandinavia children have access to any level of education independently of parental income.[5] Cross-country studies on correlations in fathers'-sons' and brothers' earnings also show that family income is less important in Scandinvian countries. The correlations in family earnings are much weaker in Scandinavia, pointing

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[5]Free education is likely to eliminate the effect of parental income on educational outcomes, but not necessarily the effect of parental education or ability. Such channels explain why there is far from perfect social mobility in the Scandinavian countries.

at parental characteristics having more limited impact on their children's outcomes in Scandinavia than they have in the U.S. and other OECD countries (Björklund et al. 2002, Corak 2006, Schnitzlein 2014, Eurostat 2015).[6]

2 Policy background

The *Start Help* benefit was introduced in 2002. It replaced a more generous cash benefit for the non-EU/EEA immigrants who could not financially support themselves and had lived less than seven out of the past eight years in Denmark. Consequently, the *Start Help* reform primarily affected recently arrived refugees and children and spouses who were reunified with refugees.[7]

Before the reform, unemployed refugees and their reunified family members received a cash benefit paid by the municipality of placement. The municipality began to pay the benefit as soon as it took over the responsibility for a person from the state, namely as soon as the person moved to a municipality from the asylum center. When a refugee was granted a residence permit, that is, when the refugee was given a refugee status, the municipality of placement was allowed two months to find proper housing. Consequently, refugees usually moved from the asylum center within two months after the date of residence permit. The amount of benefit did not depend on the municipality of placement. Newly arrived individuals who received cash benefit had to participate in a special integration program, including language classes and job activation. Furthermore, they had to actively search for a job, unless they had a very poor health or had other considerable impediments to work. The programme lasted three years, during which the refugees had to stay in the municipality of assignment. If they moved, they risked the loss of benefits.[8]

The introduction of the *Start Help* benefit affected the size of the transfers, but it did not alter the procedure of refugee allocation or the rules of the integration program.

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[6]Another OECD (2008) study shows, that probability of son being poor if his father is poor is considerably lower in Scandinavian countries than in the UK or the U.S. However the probability of son being rich if the father is rich is similar across countries.

[7]Danish citizens were also subject to the reform.

[8]https://www.retsinformation.dk/Forms/R0710.aspx?id=9043.

The *Start Help* benefit was 36 to 48% lower than the cash benefit amount refugee families received before the reform, depending on the family type, as shown in Table 1 for 2003. Individuals who were registred in the municipalities before the 1st of July, 2002 continued to receive the entire amount of cash benefit while those arriving after faced a reduction. If the first parent arrived before the reform took place and the other after, the family was entitled to a low amount of transfers equivalent to two *Start Help* amounts plus allowance for children.[9] Refugees affected by the reform received reduced *Start Help* for the first seven years since being placed to municipality. The benefit was first abolished in 2012 implying that some families were affected by the reform over several years.[10]

Table 1: Examples of Yearly Amount in DKK of Start Help Benefit versus Cash Benefit Transfers by Family Type in 2003

Household type	Start Help benefit	Cash benefit
One adult, no children	98,064	63,192
One adult + one child	130,308	78,996
Two adults, no children	196,128	104,784
Two adults + one child	228,372	117,888

Note: Adult above 25. https://www.retsinformation.dk/Forms/R0710.aspx?id=29615

3   Data

The data are from the Danish Administrative Registers owned by Statistics Denmark. We select 4,829 refugee children whose second[11] parent received residence permit in Denmark during the period between the 1st of May, 2001 and the 31st of June, 2003. Children whose second parent arrived the year before the 1st of May, 2002 are unaffected by the reform and are in the control group. Children whose parents arrived the year

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[9]In this case the first parent would continue receiving the full amount of cash benefit, whereas the second parent would be entitled only to difference between the two *Start Help* benefits plus child allowance and the amount the first parent received. http://www.socialjura.dk/content-storage/regler/2004/vejl-9496-af-16-2004/.

[10]Low benefits to immigrants were re-introduced in September 2015 with the so-called integration benefit which closely resembles *Start Help*.

[11]If there is only one parent, that parent's date of being granted residence permit is used.

after the 31st of June, 2002 are affected by the reform and are in the treatment group. We exclude children whose second parent arrived in May 2002 and June 2002 from the analysis. We do this to avoid selection problem. Parents who were granted a residence permit two months before the 1st of July, 2002 could potentially be affected by the reform, since it could take up to two months to be moved from the refugee center to the municipality of placement. Parent who arrived at the municipality of placement later than the 1st of July, 2002 would only be eligible for the low *Start Help* benefit.[12] We use the date of residence of the second parent since this determines whether the family recieves the high or the low amount of transfers.[13] Information about the type and the date of the residence permit to refugees and family reunified is available in the administrative register on the purpose of residence, whereas the children of refugees can be identified using population register. Children are definied as individuals who are less than 18 years old at the date of the second parent's residence permit. We exclude unaccompanied refugee children.

Using a personal identifier we collect information about the educational outcomes and labour market outcomes of the children, such as grade point average in the 9th grade tests, participation in tests in Danish and Mathematics, completion of compulsory school, enrollment in upper-secondary education and employment income. We focus on participation in Danish and Mathematics tests, as all pupils had to be tested in these two subjects in all school completion years we observe.[14] We also collect demographic information about the children which includes age at arrival, gender, origin, whether the children have only one parent in Denmark and whether the children have left the country at some point after the outcome was observed. Likewise, we obtain information on parental education at arrival collected by Statistics Denmark to control for parental human capital in the regressions. The parental education is self-reported and the length of education at various stages can vary across countries. Therefore, instead of computing

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[12]Another possibility is to use data on the date of arrival to the municipality of placement instead of the date of residence permit. However, we choose not do so for two reasons. First, the data on date of arrival at the municipality is more imprecise. Second, the municipality could potentially delay the placement of certain refugees thus selecting who are in the treatment and control groups.

[13]In the remainder of the paper we use wording "the date of the second parent's residence permit" and "the arrival date" interchangeably.

[14]Participation in other subjects was determined randomly.

a continuous measure such as years of schooling, we define a binary variable for whether one or both parents have completed at least an upper-secondary education.[15]

The outcomes of the children are observed in a 10-year window from the date of arrival defined as the date of the residence permit of the second parent. The test scores are observed for up to 11 years since arrival.[16] Ensuring that we observe outcomes for children in the control and treatment groups for an equal number of years is important, since the probability of completing compulsory school or enrolling in an upper-secondary education increases the longer children have lived in the country. Children in the control group have arrived to Denmark earlier, therefore, without this restriction, they would be present in the longitudinal data for more years than those in the treatment group which would bias our results.We restrict the age of the children to be at least 7 years at arrival, so that the child can reach 17 years during the maximum of 10 years in which we observe the child. When evaluating the earned income of the children we restrict the age to be at least 13 years, which is the legal working age in Denmark. The summary statistics for the variables are presented in Table 6 in the Appendix.

4   Methodology and identification

4.1   Identification

We argue that the *Start Help* benefit similarly to other reforms employed in the literature (Acemoglu and Pischke 2001, Dahl and Lochner 2012, Oreopoulos et al. 2006, 2008, Sacerdote 2007)[17] provides us with exogenous cutoff in the size of benefits refugee families received. Use of exogenous cutoff is necessary to disentangle the impact of the reform from the impact of family characteristics, for example to avoid that more skilled parents select in unaffected by the reform group. Refugee parents could not system-

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[15]Statistics Denmark impute missing education data for some refugees. We recode all imputed data to missing.

[16]Test scores are available until 2014 whereas the rest of the register data are available until 2013.

[17]These studies have used tax reforms, firm closures as well as quasi-random allocation of adoptees to families to measure the impact of parental income on the child's outcomes. This approach isolates the impact of parental skills on both - parental income and child's outcomes and allows evaluating only the direct effect of family ressources on how well the child performs at school.

atically select into two groups obtaining the residence before and after the *Start Help* reform based on their unobserved abilities, as they could not predict the introduction of *Start Help*. In particular, the discussion of the reform and it's subsequent implementation lasted a very short period, so it was very difficult to predict if and when it would become effective. The reform discussions were raised by a Danish centre-right government which was formed only in the end of November 2001.[18] The official legislation was signed in June 2002 and became effective shortly after, on the 1st of July, 2002. As a result, most of the individuals in our sample affected by the new law would learn about it in the Danish asylum camps or while waiting for their family reunificaton case to be processed.[19] To the best of our knowledge there was no change in the asylum procedure, so the individual cases would be processed in the order they were received. Moreover, the largest part of refugee flows in 2001-2003 was driven by political disruptions, wars and presecutions in Afghanistan, Iraq, Somalia and former Yugoslavia. Therefore, the flows of refugees from these countries would be mostly driven by the need for safety, rather than economic motives.

Table 2 compares characteristics of the children and their parents in control and treatment groups. Age and gender appear to be well balanced between the two groups. There is also no difference in how likely children are to outmigrate later in life. There is though considerable differences in ethnic composition: Afghans are overrepresentated in the control group, whereas Iraqis are overrepresented in the treatment group. The changes in ethnic composition of refugees were caused by political events, such as Afghan Civil War in 1996-2001 and Iraq War in 2002-2003, and thus not related to the Danish *Start Help* reform. Moreover, refugee arrivals are not clustered around the reform date, as shown in Figure 4 in the Appendix. This suggests that refugee flows were independent of the reform. It is, however, still important to control for the country of origin, since it could be correlated with school performance.[20]

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[18]http://www.stm.dk/_p_5625.html.

[19]Family reunificaton cases in Denmark last several months, depending on how many applications there are submitted at a particular moment in total and how complicated each case is. http://uibm.dk/us/kontakt-udlaendingestyrelsen.

[20]As a robustness check, we exclude individuals from Afghanistan and Somalia. Section 6 shows the results which do not change compared to the baseline estimation.

Parents of children who arrived before the reform were more likely to have at least an upper-secondary education. This unbalance is to a considerable extend driven by difference in country of origin: Fathers coming from Afghanistan were more likely to report that they have at least an upper-secondary education, than fathers from Iraq. It is impossible to distinguish whether fathers from Afghanistan were more likely to report falsely, whether the educational requirements in Afghanistan are lower making it easier to obtain education, or whether they indeed were better educated then Iraqis.[21] Therefore, it is also important to control for parental education in the regressions. If controls for parental education do not fully capture parental human capital, one would expect a negative bias in the estimate of the effect of the reform on educational outcomes of children as parents arriving after the reform had lower education. Finally, we also find that single parent children are overrepresented in the control group. This is though caused by the way the data were constructed as families with the second parent arriving after the cutoff are assigned to the treatment group. However, if one was concerned about children of single parents performing worse inat school[22], our results would again be biased in the direction of finding a larger negative effect of the reform.

4.2   Model

The full specification of the model is as follows:

$$y_i = D_i + X_i + t + orig_i + out_i + \tau_{test} + \varepsilon_i \tag{1}$$

where y_i is the outcome of the child i. The outcome variables are the average score in school completion tests, presence at all tests in Danish and Mathematics, school completion per se, enrollment in an upper-secondary education and youth employment

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[21]48 pct of fathers from Afghanistan reported having at least an upper-secondary education upon arrival, while only 33 pct of Iraqi fathers did. The share of educated mothers is similar across countries - 16 pct for Afganistan and 18 pct for Iraq.

[22]Sociological literature documents that children of single parents are disadvantaged in comparison to children from two-parent households and underperform across various measures, including educational outcomes (see e.g. Mulkey et al. 1992 or Downey 1994).

Table 2: Balancing Tests

	Control	Treatment	Difference	P-Value
Age at arrival	9.042	9.198	-0.156	0.248
Female	0.466	0.469	-0.003	0.856
Only 1 parent	0.043	0.027	0.016	0.006
Migrated out	0.162	0.124	0.038	0.000
Skilled father	0.418	0.212	0.205	0.000
Skilled mother	0.176	0.136	0.040	0.000
From Afghanistan	0.438	0.151	0.286	0.000
From Iraq	0.337	0.529	-0.192	0.000
From Somalia	0.104	0.107	-0.003	0.733
Observations	3195	1634		

Notes: Children whose parents arrived as refugees or family-reunied to refugees. Control group: The second parent arrived in the period from the 1st of May, 2001 to the 30th of April, 2002. Treatment group: The second parent arrived in the period from the 1st of July, 2002 to the 30th of June, 2003.

income.[23]

D_i - a dummy indicating whether the child is in the treatment (after *the Start Help* reform) or the control (before *the Start Help* reform) group.

X_i - a vector containing the charateristics of the children and the parents measured at the date of arrival.

t - the date of arrival.[24]

$orig_i$ - a vector containing dummies for the three largest refugee source countries, Afghanistan, Iraq and Somalia which together account for 90 percent of refugee inflow in 2001-2003.

τ_{test} - fixed effects of test year, 9^{th}grade. The school completion tests are held at the national level, so there is no across school variation in how complicated the tests in a particular year are. However, there can be across year variation in the difficulty of the tests. The test year fixed effects remove such variation.

ε_i - an error term.

5   Results

We begin by evaluating the impact the *Start Help* reform had on the average score in the 9th grade school completion tests. This is the first indicator of how well these children integrate in the host country's society and what are their chances to obtain further education and become employed.

In Table 3 we show that lower parental total income have no overall impact on how

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[23]As explained below, it is not necessary to attend or pass all tests to complete school. In our sample those who do not complete school are either too young, too old, dropouts or grade repeaters who delay school completion to a point in time beyond the window of observation. Since the age distribution is the same for the control and treatment group, differences in the school completion rate will reflect differences in the fraction of dropouts and the fraction of grade repeaters.

[24]We use second parent's residence date instead of de facto child's arrival date, because more skilled parents potentially could move their children to Denmark sooner compared to less skilled parents once they had learned about their own residence permit (and child did not arrive with parents). This can induce bias on the effect of age at arrival variable. Furthermore, if parental skill composition is unequal between the control and treatment groups it would also bias other results as we would observe the outcomes of skilled parents' children at earlier dates.

well the children perform at school completion tests in the 9th grade.[25] The coefficient on the reform dummy is robustly insignificant in all specifications. In five out of eight specifications, the coefficient is positive indicating a positive albeit insignificant correlation between the reform and test scores. Specification (4) contains the lowest estimate of -0.05. This effect is very modest considering that the mean test score in the sample is 4.28 and the standard deviation is 2.25. Overall, the results indicate that the reform did not have detrimental effects on the test scores of refugee children.

The coefficient signs for other characteristics are compatible with earlier findings in the immigration literature: Children who arrive to Denmark at a later age perform worse in school. This finding echoes the results of Böhlmark (2008), Bratsberg et al. (2012), Ohinata and van Ours (2012), Fallesen (2015) who show that the negative correlation between age and educational performance is present for immigrant children in general, especially if they immigrate after the school-start age. Children who arrive later have fewer years to learn the language and culture of the host country and perform worse in school completion tests. Furthermore, girls obtain on average 0.4 points higher score average at school tests compared to boys and in general girls do perform better in school compared to boys.[26,27] Parental human capital and country of origin also affects school performance. Children of parents having at least an upper-secondary education perform better at tests, and mother's education is two to three times more important than father's education (consistent with earlier findings by Oreopoulos et al. 2006, Holmlund et al. 2011, Pronzato 2012), according to specifications (6), (8) and (9) in Table 3.[28]

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[25]In Table 8 in the Appendix we report also that test score results for Danish and Mathematics were not affected by the reform. We chose Danish and Mathematics as the main two main subjects, in which tests have to be taken by all children in all years.

[26]Girls obtain also a higher grade in language tests, but a lower grade in mathematics tests compared to boys. Please see Table 8 in the Appendix.

[27]This observation is also coherent with the general pattern that girls on average perform better in school than boys (Evalueringsinstitut 2006, OECD 2012, 2015, Konle-Seidl and Bolits 2016).

[28]Note, that after controlling for the parental characteristics the sign of the reform dummy changes from negative to positive. As discussed in Identification subsection, parents arriving before the reform were systematically more likely to have an upper-secondary education. This induces bias in the direction of finding the negative impact of the reform on the child's outcomes. The change of sign for the reform dummy after controlling for parental education illustrates the presence of this bias. Our conclusions about the impact of the reform are though unaffected by the bias, as in neither specification

Table 3: Average 9th Grade School Completion Test Score

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	-0.013	-0.009	0.184	-0.049	0.095	0.106	0.287	0.280
	(0.106)	(0.105)	(0.239)	(0.106)	(0.105)	(0.111)	(0.230)	(0.228)
Female		0.430***					0.386***	0.407***
		(0.099)					(0.093)	(0.092)
Date of residence permit			-0.000				-0.000	0.000
			(0.001)				(0.000)	(0.001)
Migrated out				-0.703***			-0.356*	-0.298
				(0.160)			(0.159)	(0.155)
Only 1 parent					-0.133		0.027	0.126
					(0.226)		(0.222)	(0.222)
Skilled father					0.450***		0.379***	0.369***
					(0.107)		(0.103)	(0.103)
Skilled mother					0.983***		1.029***	0.972***
					(0.138)		(0.132)	(0.130)
From Afghanistan						0.239	0.236	0.336*
						(0.156)	(0.152)	(0.149)
From Iraq						-0.325*	-0.408**	-0.305*
						(0.148)	(0.142)	(0.139)
From Somalia						-0.920***	-0.701**	-0.563**
						(0.215)	(0.215)	(0.210)
Age at date of residence permit							-0.199***	-0.391***
							(0.019)	(0.052)
Year of exam FE	No	No	No	No	No	No	No	Yes
Observations	1913	1913	1913	1913	1913	1913	1913	1913
R^2	0.000	0.010	0.000	0.009	0.053	0.024	0.137	0.164

Notes: Children whose parents arrived as refugees or family-reunied to refugees. Control group: The second parent arrived in the period from the 1st of May, 2001 to the 30th of April, 2002. Treatment group: The second parent arrived in the period from the 1st of July, 2002 to the 30th of June, 2003. Only includes children who are 7 years or older at arrival. Outcomes observed in 11 years window since arrival. Heteroscedasticity robust standard errors. *p<0.05, **p<0.01, ***p<0.001.

In Table 4 we focus on further school outcomes. First, we focus on whether children have attended all tests in two main subjects - Danish and Mathematics. In principle all parts of both tests were mandatory, but in practice only 88 per cent chose to attend all parts of the Danish tests and 91 per cent chose to attend all parts of the Mathematics tests. The results in specifications (1)-(4) show that the particpitation rate is lower for children affected by the reform. The effect is only statistically significant for Danish tests and it declines in statististical significance in (2) once the set of control variables is included. Furthermore, the robustness checks in the next section show that the relationship ceases to exist once we limit the sample to children arriving half year around or half year away from the reform cutoff.

In the last two specifications we focus on whether the reduction in parental income impedes school completion for the children. We do not find that this is the case, despite the children affected by the reform were less likely to participate in all Danish tests. An explanation for this finding is that it is not necessary to attend all school tests to receive a school completion certificate.[29]

there is a statistically significant relation between the reform dummy and test scores.

[29]In such a case the child would just have a dash instead of the test score in the school completion certificate.

Table 4: School Outcomes of Refugee Children

	Took all Danish tests		Took all Mathematics tests		Completed 9th grade	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	-0.059***	-0.074*	-0.009	-0.060	0.012	0.060
	(0.017)	(0.036)	(0.014)	(0.031)	(0.017)	(0.038)
Female		0.046**		0.014		0.021
		(0.014)		(0.013)		(0.015)
Age at date of residence permit		-0.038***		-0.021**		-0.035***
		(0.009)		(0.008)		(0.003)
Date of residence permit		0.000		0.000		-0.000
		(0.000)		(0.000)		(0.000)
Migrated out		-0.014		-0.031		-0.268***
		(0.027)		(0.026)		(0.026)
Only 1 parent		-0.014		0.033		0.054
		(0.040)		(0.030)		(0.044)
Skilled father		0.022		0.027		0.007
		(0.015)		(0.014)		(0.017)
Skilled mother		0.033		0.025		0.049*
		(0.018)		(0.017)		(0.020)
From Afghanistan		-0.037		-0.011		-0.053*
		(0.022)		(0.021)		(0.022)
From Iraq		-0.033		-0.012		-0.122***
		(0.022)		(0.020)		(0.022)
From Somalia		-0.109**		-0.086*		-0.157***
		(0.037)		(0.035)		(0.033)
Year of exam FE	No	Yes	No	Yes	No	No
Observations	2015	2015	2015	2015	3016	3016
R^2	0.007	0.124	0.000	0.094	0.000	0.130

Notes: Children whose parents arrived as refugees or family-reunied to refugees. Control group: The second parent arrived in the period from the 1st of May, 2001 to the 30th of April, 2002. Treatment group: The second parent arrived in the period from the 1st of July, 2002 to the 30th of June, 2003. Only includes children who are 7 years or older at arrival. Outcomes observed in 10 years window since arrival. Heteroscedasticity robust standard errors. *p<0.05, **p<0.01, ***p<0.001.

17

We proceed by investigating whether the reform had any impact beyond the compulsory school outcomes. In Table 5, specifications (1) and (2) we find that there is no statistically significant difference in the probability to begin an upper-secondary education between the two groups of children. Consequently, refugee children's educational outcomes were not affected by the reform neither while they had been in the obligatory schooling (up to 9^{th}grade) nor after.

Finally, we also evaluate whether reduction in benefits has increased child labour supply to compensate for the lower parental income. Specifications (3) and (4) in Table 5 focus on the average yearly income the child earns in the first 10 years in the country. We only compute the average over the years where children are older than 13 years, which is the minimum working age in Denmark. We find no relationship between the reform and the income of children in youth which is consistent with our main conclusion that the reform had a limited effect on school outcomes.[30]

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[30]Our results are coherent with the findings in a recent survey study by Benjamisen et al. (2016) about the deprivation of children in poor Danish families. They show that children in poor families are affected only to a limited extend by the lack of resources in the family. The study also finds that parents in poor families compensate for the lack of ressources for children by prioritizing their children's needs over their own needs. On the contrary, survey by Hansen (2013) shows that the lower family income is, the more deprivation are children in those families subject to, being forced to skip visits to dentist or having less opportunities for leisure time activities.

Table 5: Further Outcomes of Refugee Children

	Started in upper-secondary		Child earned income	
	(1)	(2)	(3)	(4)
Treatment	-0.025	-0.027	-2.109	-1.093
	(0.018)	(0.040)	(1.490)	(3.081)
Female		0.009		-13.283***
		(0.016)		(1.204)
Age at date of residence permit		0.015***		5.750***
		(0.003)		(0.263)
Date of residence permit		0.000		-0.001
		(0.000)		(0.006)
Migrated out		-0.288***		-12.751***
		(0.025)		(1.609)
Only 1 parent		0.082		-2.050
		(0.045)		(2.516)
Skilled father		0.077***		1.035
		(0.018)		(1.391)
Skilled mother		0.054**		-2.367
		(0.021)		(1.696)
From Afghanistan		0.003		1.432
		(0.025)		(2.173)
From Iraq		-0.105***		-4.234*
		(0.024)		(2.103)
From Somalia		-0.135***		-8.402**
		(0.036)		(2.619)
Observations	3016	3016	3016	3016
R^2	0.001	0.098	0.001	0.233

Notes: Children whose parents arrived as refugees or family-reunied to refugees. Control group: The second parent arrived in the period from the 1st of May, 2001 to the 30th of April, 2002. Treatment group: The second parent arrived in the period from 1st of July, 2002 to the 30th of June, 2003. (1) and (2) only includes children who are 7 years or older at arrival, (3) and (4) only includes children who are 13 years or older at arrival. Heteroscedasticity robust standard errors. *p<0.05, **p<0.01, ***p<0.001.

6 Robustness checks

To ensure that our conclusions are not driven by a particular sample selection or specification of the model we perform a set of robustness checks in Tables (9) - (16) in the Appendix. Each table is related to a particular outcome (test grade, participation in tests, school completion, enrollment in upper-secondary education and child's employment income) and to save space we present only the results from the full specification.

We employ four different robustness checks for each outcome. In the first robustness check we focus on children who arrived in a half-year interval around the reform cutoff instead of a year (in periods the 1st of November, 2001 to the 30th of April, 2002 and the 1st of July, 2002 to the 31st of December, 2002). By using a shorter interval we ensure that children and parents arrived on the both sides of the reform date are more similar in their observed and unobserved characteristics. This is done at the expense of loosing observations in the sample.

In the second robustness check we instead take children who arrived half a year away from the cuttoff (in periods the 1st of May, 2001 to the 30th of October, 2001 and the 1st of January, 2003 to the 30th of June, 2003). The argument for doing this robustness check is the possibility of selection around the cutoff if refugees could predict the reform.

In the third robustness check we exclude individuals from Afghanistan and Iraq since there is a clear difference in the ethnic composition of the control and treatment groups. Afghans are overrepresented in the sample before the reform and Iraqis are overrepresented in the sample after the reform. This robustness check leads to excluding almost 80 percent of the sample. Therefore, in the main analysis, we keep all individuals in our sample, independently of their origin.

In the fourth robustness check, we include fixed effects for all origin countries dummies. This allows to capture the origin effect for the remaining 10 percent of the sample. The reference category consists of several origin countries with only one or two children from each.

Finally, the reform could have had the negative impact only on the most disadvantaged children. In the last robustness check we focus on the outcomes of the refugee

children in families where at least one parent came as a Convention or a Quota refugee. Rosholm and Vejlin (2010) argue that Convention and Quota refugees are perceived as the weakest among refugees, having had more traumatic experiences in the past. We do not find any negative impact of the reform for this group of children.[31]

Robustness checks confirm that the *Start Help* benefit reform did not influence negatively refugee children's grades or impede children from completing compulsory school and enrolling in an upper-secondary education. Moreover, they also show that the negative impact of the reform on the participation in the Danish tests is not robustly significant across robustness checks, furthermore enforcing conclusion that there is no evidence that the reduction in parental benefits harmed refugee children's education outcomes.

7   Conclusion

By using an exogenous change in the size of public transfers to refugee parents, we show that the reduction in parental transfers does not worsen educational outcomes of refugee children at school. We compare the children whose parents received full cash benefit and those who received a lower *Start Help* benefit. We do not find any statistically significant difference in the 9th grade national tests scores, school completion probability or probability of enrolling in an upper-secondary education between the children in two groups. We find some suggestive evidence that children affected by the reform were less likely to take all parts of Danish language test, but this result is not robust.

We also find no evidence that children of parents entitled to lower benefit were forced to work and earn more to compensate for the decline in family income. On the other hand, parental employment and earned income increased as the result of the reform. However, the increase in earned income did not fully compensate for the fall in

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[31]Rosholm and Vejlin (2010) also find that *Start Help* reform had least impact on the Convention refugees' chance to become employed, as despite economic encentives these refugees were not capable of finding a job. The number of Quota refugees in their sample is too small to draw a conclusion for them separately. Our results suggest though that employment income in families where at least one parent is a Convention or a Quota refugees has on average increased as a result of the reform similarly to the entire sample of refugees, see Figure 5 in the Appendix.

transfers meaning that reform reduced welfare of the refugee families. The results are robust to controlling for the demographic characteristics of the children, such as age at arrival, gender and origin as well as parental education, time of arrival and year when the school completion tests were taken.

Overall, we conclude that - at least in Scandinavian setup, where both compulsory and upper-secondary education is provided for free - lowering cash benefits to refugee parents is not a detriment to their children's educational outcomes. At the same time, parental labour market outcomes improve in the short run and expenses to transfers are reduced.

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24

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8   Appendix (intended for online publication)
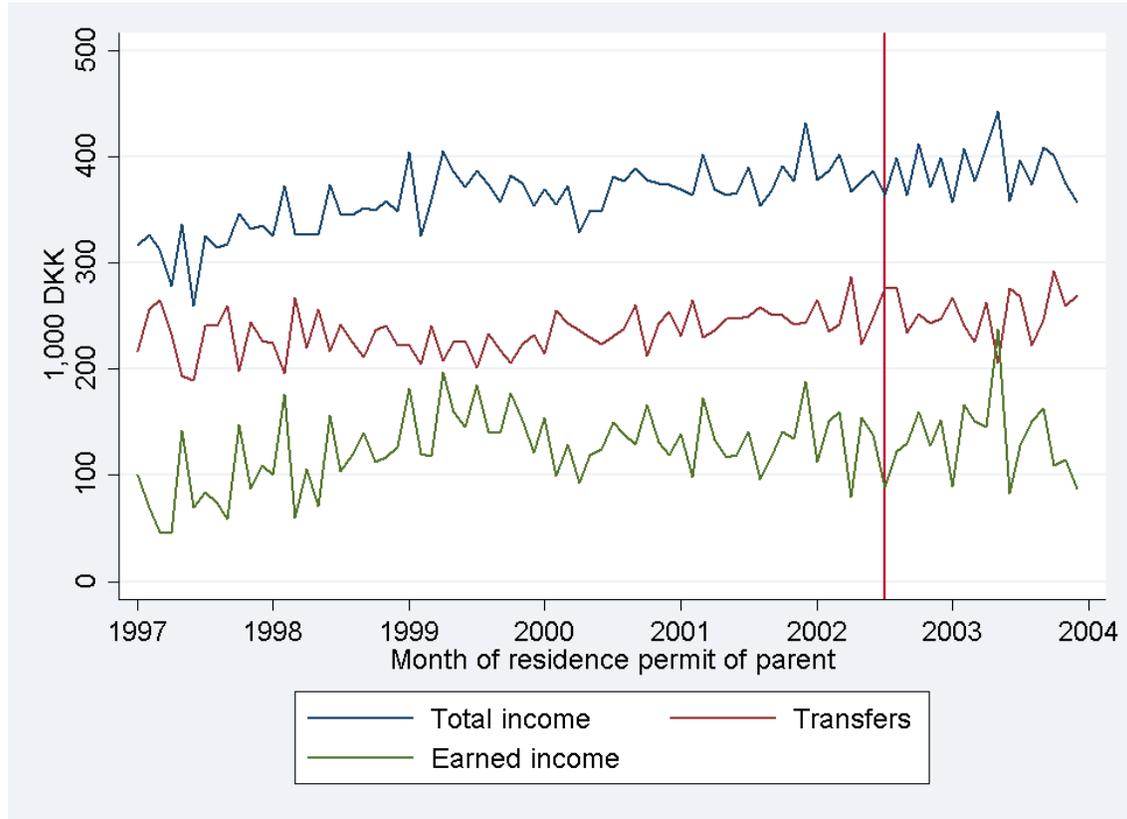
Supplementary Figures

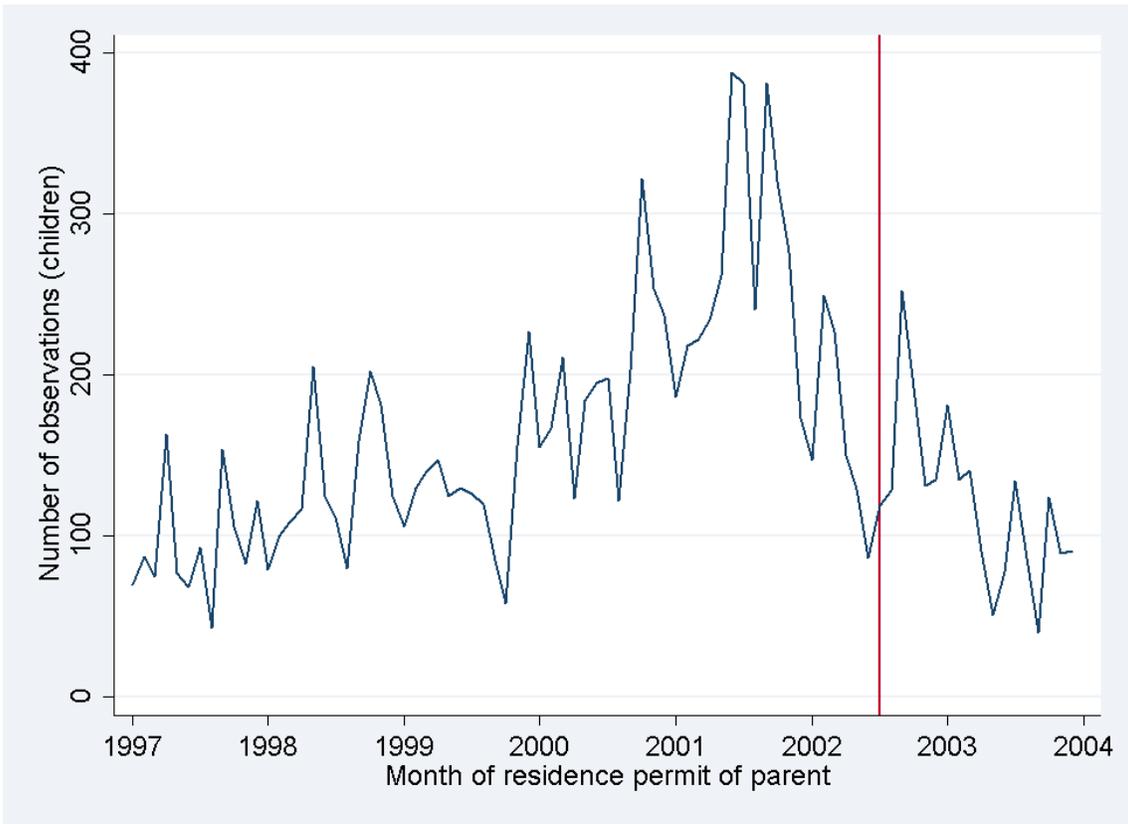Figure 3: Parental Income in the 8th Year since Immigration

Figure 4: Number of Observations

Supplementary Tables

Table 6: Descriptive Statistics

	Mean	Standard deviation	Observations
Average exam grade	4.28	2.25	2310
Took all Danish tests	0.88	0.32	2412
Took all Mathematics tests	0.91	0.28	2412
Finished 9th grade	0.47	0.50	4829
Started education beyond 9th grade	0.44	0.50	4829
(mean) child_income	20.41	34.53	4549
Transfers	738.90	264.40	4829
Earned income	186.43	298.26	4829
Total income	925.33	270.31	4829
Employment rate	0.10	0.12	4181
Average exam grade - Danish	4.04	2.12	2218
Average exam grade - math	4.34	2.89	2235
Age at arrival	9.09	4.43	4828
Female	0.47	0.50	4829
Only 1 parent	0.04	0.19	4829
Migrated out	0.15	0.36	4829
Skilled father	0.35	0.48	4829
Skilled mother	0.16	0.37	4829

Notes: Children whose parents arrived as refugees or family-reunied to refugees in the period from the 1st of May, 2001 to the 30th of April, 2002 or the 1st of July, 2002 to the 30th of June, 2003.
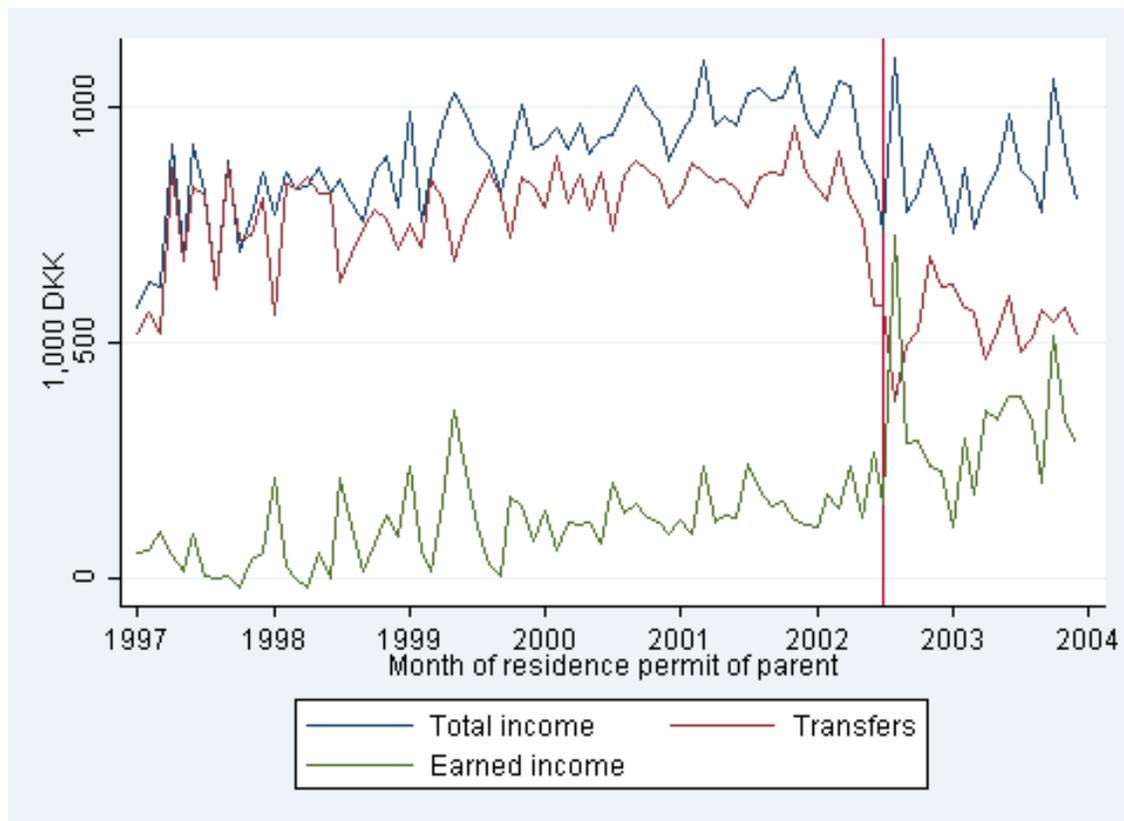
Figure 5: Accumulated Parental Income Over 3 Years since Immigration, Convention and Quota refugees

Table 7: Parental Income and Employment on Both Sides of the Reform

	(1) Transfers	(2) Earned income	(3) Total income	(4) Employment rate
Treatment	-208.680***	86.537	-122.143**	0.048
	(37.613)	(47.917)	(41.420)	(0.025)
Female	6.256	-3.720	2.535	-0.002
	(14.682)	(18.654)	(16.543)	(0.009)
Age at date of residence permit	3.291	-7.463**	-4.172	-0.002
	(2.076)	(2.638)	(2.273)	(0.001)
Date of residence permit	0.048	0.013	0.060	0.000
	(0.082)	(0.099)	(0.088)	(0.000)
Only 1 parent	-16.094	30.457	14.363	0.025
	(35.552)	(42.060)	(42.074)	(0.020)
Migrated out	-112.559***	-55.511**	-168.070***	-0.036**
	(19.811)	(20.523)	(22.315)	(0.012)
Skilled father	49.846**	37.895	87.740***	0.003
	(16.279)	(20.499)	(17.629)	(0.010)
Skilled mother	-36.170*	-21.793	-57.963**	0.002
	(18.287)	(23.007)	(20.478)	(0.012)
From Afghanistan	117.846***	-84.644**	33.202	-0.040**
	(22.765)	(28.427)	(26.109)	(0.015)
From Iraq	96.387***	-99.067***	-2.680	-0.050***
	(20.543)	(28.394)	(24.326)	(0.015)
From Somalia	10.170	-143.694***	-133.525***	-0.044
	(34.786)	(32.043)	(38.254)	(0.028)
Observations	906	906	906	710
R^2	0.253	0.077	0.178	0.089

Notes: Children whose parents arrived as refugees or family-reunied to refugees. Control group: The second parent arrived in the period from the 1st of May, 2001 to the 30th of April, 2002. Treatment group: The second parent arrived in the period from the 1st of July, 2002 to the 30th of June, 2003. Only includes children who are 7 years or older at arrival. *p<0.05, **p<0.01, ***p<0.001.

Table 8: Danish and Mathematics Test Scores

	Danish test score		Mathematics test score	
	(1)	(2)	(3)	(4)
Treatment	0.069	0.304	-0.155	0.305
	(0.100)	(0.211)	(0.140)	(0.317)
Female		0.636***		-0.281*
		(0.086)		(0.127)
Age at date of residence permit		-0.237***		-0.136***
		(0.018)		(0.025)
Date of residence permit		-0.000		-0.000
		(0.000)		(0.001)
Migrated out		-0.478**		-0.061
		(0.149)		(0.222)
Only 1 parent		0.189		-0.008
		(0.213)		(0.299)
Skilled father		0.129		0.768***
		(0.096)		(0.143)
Skilled mother		0.856***		1.076***
		(0.119)		(0.177)
From Afghanistan		0.023		0.556**
		(0.145)		(0.202)
From Iraq		-0.548***		-0.100
		(0.136)		(0.193)
From Somalia		-0.757***		-1.165***
		(0.204)		(0.284)
Observations	1827	1827	1846	1846
R^2	0.000	0.168	0.001	0.094

Notes: Children whose parents arrived as refugees or family-reunied to refugees. Control group: The second parent arrived in the period from the 1st of May 2001 to the 30th of April, 2002. Treatment group: The second parent arrived in the period from the 1st of July, 2002 to the 30th of June, 2003. Only includes children who are 7 years or older at arrival. Outcomes observed in 10 years window since arrival. Heteroscedasticity robust standard errors. *p<0.05, **p<0.01, ***p<0.001.

Robustness Checks Tables

Table 9: Average 9th Grade School Completion Test Score, robustness checks

	(1)	(2)	(3)	(4)
Treatment	-0.066	1.444*	-0.550	0.407
	(0.394)	(0.667)	(0.463)	(0.233)
Female	0.396**	0.267*	0.598**	0.404***
	(0.137)	(0.127)	(0.192)	(0.093)
Age at date of residence permit	-0.447***	-0.411***	-0.486***	-0.362***
	(0.079)	(0.072)	(0.108)	(0.053)
Date of residence permit	0.002	-0.002	0.003***	-0.000
	(0.001)	(0.001)	(0.001)	(0.001)
Migrated out	0.017	-0.402*	-0.069	-0.320*
	(0.256)	(0.199)	(0.305)	(0.153)
Only 1 parent	0.238	-0.017	-0.131	0.136
	(0.302)	(0.310)	(0.431)	(0.226)
Skilled father	0.205	0.499***	0.317	0.346***
	(0.153)	(0.140)	(0.241)	(0.105)
Skilled mother	1.019***	0.981***	1.002***	0.938***
	(0.193)	(0.183)	(0.278)	(0.134)
From Afghanistan	0.211	0.072		
	(0.223)	(0.213)		
From Iraq	-0.427*	-0.448*		
	(0.186)	(0.210)		
From Somalia	-0.883**	-0.628	-0.413	
	(0.292)	(0.325)	(0.221)	
Year of exam FE	Yes	Yes	Yes	Yes
Country of origin FE	No	No	No	Yes
Observations	821	1014	424	1913
R^2	0.169	0.172	0.199	0.199

Notes: Specification (1) - children arrived in a half year around the reform cutoff; specification (2) - children arrived a half year away from the reform cutoff; specification (3) - excluding Afghanis and Iraqis; specification (4) - land dummies. See Robustness Checks section for more details. *p<0.05, **p<0.01, ***p<0.001.

34

Table 10: Presence at All Danish tests, Robustness Checks

	(1)	(2)	(3)	(4)
Treatment	0.009	-0.081	-0.130	-0.077*
	(0.062)	(0.101)	(0.074)	(0.038)
Female	0.019	0.043*	0.042	0.047***
	(0.023)	(0.019)	(0.033)	(0.014)
Age at date of residence permit	-0.044**	-0.043***	-0.020	-0.040***
	(0.016)	(0.013)	(0.020)	(0.010)
Date of residence permit	-0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Migrated out	-0.057	0.020	0.007	-0.016
	(0.052)	(0.031)	(0.052)	(0.027)
Only 1 parent	0.105*	-0.058	-0.002	-0.024
	(0.049)	(0.059)	(0.084)	(0.041)
Skilled father	0.021	0.019	0.064	0.021
	(0.024)	(0.020)	(0.035)	(0.016)
Skilled mother	0.055*	0.011	0.002	0.033
	(0.026)	(0.025)	(0.043)	(0.018)
From Afghanistan	-0.036	-0.049		
	(0.035)	(0.031)		
From Iraq	-0.041	-0.044		
	(0.032)	(0.030)		
From Somalia	-0.135*	-0.103*	-0.120**	
	(0.060)	(0.050)	(0.039)	
Year of exam FE	Yes	Yes	Yes	Yes
Country of origin FE	No	No	No	Yes
Observations	874	1059	449	2015
R^2	0.147	0.143	0.138	0.135

Notes: Specification (1) - children arrived in a half year around the reform cutoff; specification (2) - children arrived a half year away from the reform cutoff; specification (3) - excluding Afghanis and Iraqis; specification (4) - land dummies. See Robustness Checks section for more details. *p<0.05, **p<0.01, ***p<0.001.

Table 11: Presence at All Mathematics Tests, Robustness Checks

	(1)	(2)	(3)	(4)
Treatment	-0.000	-0.018	-0.086	-0.066*
	(0.052)	(0.090)	(0.065)	(0.033)
Female	0.018	-0.005	0.000	0.015
	(0.019)	(0.017)	(0.031)	(0.013)
Age at date of residence permit	-0.026	-0.027*	-0.025	-0.022*
	(0.013)	(0.011)	(0.020)	(0.008)
Date of residence permit	-0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Migrated out	-0.041	-0.014	-0.067	-0.028
	(0.043)	(0.033)	(0.052)	(0.026)
Only 1 parent	0.079*	0.042	0.107**	0.025
	(0.036)	(0.037)	(0.034)	(0.031)
Skilled father	0.046*	0.043*	0.010	0.027
	(0.020)	(0.019)	(0.035)	(0.014)
Skilled mother	0.045*	-0.000	0.018	0.024
	(0.022)	(0.025)	(0.039)	(0.017)
From Afghanistan	-0.019	-0.028		
	(0.028)	(0.032)		
From Iraq	-0.037	-0.022		
	(0.027)	(0.031)		
From Somalia	-0.094	-0.070	-0.078*	
	(0.049)	(0.049)	(0.039)	
Year of exam FE	Yes	Yes	Yes	Yes
Country of origin FE	No	No	No	Yes
Observations	874	1059	449	2015
R^2	0.130	0.102	0.155	0.107

Notes: Specification (1) - children arrived in a half year around the reform cutoff; specification (2) - children arrived a half year away from the reform cutoff; specification (3) - excluding Afghanis and Iraqis; specification (4) - land dummies. See Robustness Checks section for more details. *p<0.05, **p<0.01, ***p<0.001.

Table 12: Completed 9th Grade, Robustness Checks

	(1)	(2)	(3)	(4)
Treatment	0.107	-0.136	-0.014	0.064
	(0.065)	(0.117)	(0.068)	(0.039)
Female	0.015	0.028	0.037	0.021
	(0.023)	(0.021)	(0.030)	(0.015)
Age at date of residence permit	-0.045***	-0.029***	-0.032***	-0.036***
	(0.004)	(0.004)	(0.006)	(0.003)
Date of residence permit	-0.000	0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Migrated out	-0.325***	-0.225***	-0.344***	-0.268***
	(0.038)	(0.037)	(0.042)	(0.026)
Only 1 parent	0.070	-0.001	0.046	0.048
	(0.063)	(0.061)	(0.086)	(0.044)
Skilled father	-0.025	0.024	0.006	0.003
	(0.026)	(0.023)	(0.036)	(0.017)
Skilled mother	0.072*	0.042	0.098**	0.051*
	(0.030)	(0.027)	(0.037)	(0.020)
From Afghanistan	-0.068*	-0.025		
	(0.034)	(0.032)		
From Iraq	-0.121***	-0.108***		
	(0.031)	(0.033)		
From Somalia	-0.139**	-0.194***	-0.131***	
	(0.047)	(0.051)	(0.035)	
Country of origin FE	No	No	No	Yes
Observations	1364	1529	719	3016
R^2	0.175	0.108	0.230	0.142

Notes: Specification (1) - children arrived in a half year around the reform cutoff; specification (2) - children arrived a half year away from the reform cutoff; specification (3) - excluding Afghanis and Iraqis; specification (4) - land dummies. See Robustness Checks section for more details. *p<0.05, **p<0.01, ***p<0.001.

Table 13: Started in upper-secondary, Robustness Checks

	(1)	(2)	(3)	(4)
Treatment	0.048	-0.176	-0.027	-0.024
	(0.069)	(0.121)	(0.075)	(0.041)
Female	-0.014	0.041	0.033	0.009
	(0.024)	(0.022)	(0.034)	(0.016)
Age at date of residence permit	0.011*	0.017***	0.022***	0.015***
	(0.005)	(0.004)	(0.006)	(0.003)
Date of residence permit	-0.000	0.000	-0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Migrated out	-0.334***	-0.248***	-0.262***	-0.286***
	(0.037)	(0.036)	(0.043)	(0.025)
Only 1 parent	0.031	0.064	-0.034	0.083
	(0.061)	(0.062)	(0.093)	(0.046)
Skilled father	0.081**	0.074**	0.054	0.073***
	(0.027)	(0.024)	(0.040)	(0.018)
Skilled mother	0.030	0.077**	0.048	0.047*
	(0.032)	(0.028)	(0.045)	(0.022)
From Afghanistan	0.021	0.053		
	(0.038)	(0.037)		
From Iraq	-0.135***	-0.050		
	(0.035)	(0.037)		
From Somalia	-0.149**	-0.079	-0.151***	
	(0.052)	(0.055)	(0.040)	
Country of origin FE	No	No	No	Yes
Observations	1364	1529	719	3016
R^2	0.128	0.083	0.143	0.113

Notes: Specification (1) - children arrived in a half year around the reform cutoff; specification (2) - children arrived a half year away from the reform cutoff; specification (3) - excluding Afghanis and Iraqis; specification (4) - land dummies. See Robustness Checks section for more details. *p<0.05, **p<0.01, ***p<0.001.

Table 14: Child Earned Income, Robustness Checks

	(1)	(2)	(3)	(4)
Treatment	93.633	-10967.160	-6847.264	239.188
	(5003.389)	(10046.383)	(5890.218)	(3017.050)
Female	-14050.438***	-12008.384***	-7324.327**	-13293.551***
	(1873.377)	(1731.719)	(2555.839)	(1205.831)
Age at date of residence permit	5601.986***	6180.158***	5900.782***	5730.959***
	(405.542)	(392.944)	(560.382)	(264.508)
Date of residence permit	-10.490	16.652	5.417	-3.547
	(19.009)	(18.086)	(12.168)	(6.259)
Migrated out	-11311.557***	-14584.829***	-10678.127***	-12510.688***
	(2646.242)	(2160.699)	(2886.910)	(1628.425)
Only 1 parent	-2117.503	-2475.128	-8180.217	-1170.259
	(4590.359)	(3187.647)	(4381.988)	(2557.035)
Skilled father	-700.141	1960.087	1205.629	1636.997
	(2257.521)	(1986.477)	(3151.246)	(1413.221)
Skilled mother	-258.992	-3177.288	-1661.292	-2927.928
	(2876.854)	(2503.917)	(3723.283)	(1743.099)
From Afghanistan	-4639.463	4275.495		
	(3608.071)	(3066.872)		
From Iraq	-7332.749*	-2749.672		
	(3232.822)	(3096.690)		
From Somalia	-12729.470**	-5706.914	-9255.340***	
	(3961.229)	(3839.761)	(2760.665)	
Country of origin FE	No	No	No	Yes
Observations	1364	1529	719	3016
R^2	0.213	0.245	0.236	0.251

Notes: Specification (1) - children arrived in a half year around the reform cutoff; specification (2) - children arrived a half year away from the reform cutoff; specification (3) - excluding Afghanis and Iraqis; specification (4) - land dummies. See Robustness Checks section for more details. *p<0.05, **p<0.01, ***p<0.001.

Table 15: Performance at Tests, Children of Convention or Quota Refugees

	Average Test Score		Took all Danish tests		Took all Mathematics tests	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	0.023	0.969*	-0.086**	-0.086	0.012	0.004
	(0.183)	(0.403)	(0.029)	(0.063)	(0.022)	(0.052)
Female		0.289*		0.036		0.004
		(0.142)		(0.020)		(0.019)
Age at date of residence permit		-0.416***		-0.022		-0.005
		(0.073)		(0.012)		(0.011)
Date of residence permit		-0.001		-0.000		-0.000
		(0.001)		(0.000)		(0.000)
Migrated out		-0.603**		0.031		-0.012
		(0.225)		(0.033)		(0.035)
Only 1 parent		-0.465		0.055		0.063
		(0.368)		(0.055)		(0.040)
Skilled father		0.405**		0.005		0.020
		(0.152)		(0.023)		(0.021)
Skilled mother		0.937***		0.047		0.043
		(0.199)		(0.027)		(0.025)
From Afghanistan		-0.026		-0.079*		0.003
		(0.232)		(0.032)		(0.035)
From Iraq		-0.821*		-0.060		0.041
		(0.341)		(0.057)		(0.042)
From Somalia		-0.879*		-0.238**		-0.088
		(0.394)		(0.077)		(0.071)
Year of exam FE	No	Yes	No	Yes	No	Yes
Observations	864	864	900	900	900	900
R^2	0.000	0.185	0.013	0.154	0.000	0.084

Notes: Children whose parents arrived as refugees or family-reunied to refugees in the period from the 1st of May, 2001 to the 30th of April, 2002 or the 1st of July, 2002 to the 30th of June, 2003. Only children of Convention or Quota refugees. See Robustness Checks section for more details. *p<0.05, **p<0.01, ***p<0.001.

Table 16: Educational Outcomes and Income, Children of Convention or Quota Refugees

	Finished 9th grade		Started in upper-secondary		Child earned income	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	0.010	0.071	0.006	-0.077	-2905.858	-1172.206
	(0.028)	(0.064)	(0.029)	(0.064)	(2320.742)	(4840.543)
Female		0.038		0.004		-7231.823***
		(0.023)		(0.024)		(1747.118)
Age at date of residence permit		-0.026***		0.025***		6088.712***
		(0.005)		(0.005)		(406.737)
Date of residence permit		-0.000		0.000		-0.101
		(0.000)		(0.000)		(9.349)
Migrated out		-0.305***		-0.234***		-13448.618***
		(0.040)		(0.039)		(2288.448)
Only 1 parent		0.135		0.003		-2530.991
		(0.071)		(0.076)		(4345.095)
Skilled father		-0.029		0.032		1809.636
		(0.025)		(0.025)		(1962.329)
Skilled mother		-0.007		0.071*		-1398.759
		(0.031)		(0.030)		(2615.412)
From Afghanistan		-0.073*		-0.012		1739.514
		(0.034)		(0.037)		(3148.884)
From Iraq		-0.017		0.002		-5619.189
		(0.053)		(0.059)		(4770.237)
From Somalia		-0.178**		-0.165*		-7958.600*
		(0.063)		(0.064)		(3874.082)
Observations	1242	1242	1242	1242	1242	1242
R^2	0.000	0.120	0.000	0.100	0.001	0.261

Notes: Children whose parents arrived as refugees or family-reunied to refugees in the period from the 1st of May, 2001 to the 30th of April, 2002 or the 1st of July, 2002 to the 30th of June, 2003. Only children of Convention or Quota refugees. See Robustness Checks section for more details. *p<0.05, **p<0.01, ***p<0.001.