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Using exogenous tax variation to identify the effect of municipalities' financial condition on the integration outcome of refugees in Norway.

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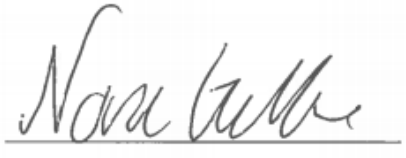

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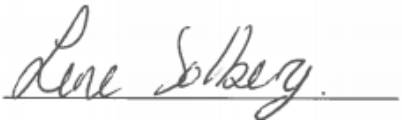

Lene Bonesmo Solberg

Abstract

The inflow of refugees to Europe in the past few years has motivated us to study the outcome of integration of refugees. The Norwegian settlement policy of refugees implies that refugees are assigned to municipalities across the country and that the local governments are responsible for the integration process. In this paper, we rely on an instrumental variable approach where we exploit exogenous variation in tax revenues between municipalities to identify the effect of municipalities' financial condition on the integration outcome of refugees. Exogenous variation, both in terms of the settlement of refugees and municipalities' tax revenues, allows us to estimate a causal effect. The analyses are performed on a sample of 24 wealthy municipalities, where their neighboring municipalities are used as a counterfactual. We measure integration by the variables earnings and social security benefits, by the use of registry data from Statistics Norway. The time period studied is 2005 - 2010, as refugees arriving in this period were exposed to the same treatment in terms of the integration and settlement policies. Little research has been done on municipalities' financial condition and integration outcome of refugees in Norway. This paper therefore aims to make a contribution to the literature. For earnings, we find that the estimated coefficients of unrestricted income are somewhat contradictory and sensitive to various specifications. This suggests that we cannot tell whether an effect is present. For social security benefits, we do not find evidence that refugees settled in wealthier municipalities, receive lower/higher levels of social security benefits. This suggests that more research in this field is necessary to obtain knowledge of how economic resources affect integration outcome, which can have implications for policy design.

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1 Introduction

1.1 Purpose and Motivation

The high inflow of refugees to Europe the past few years has created debate concerning the effectiveness of integration policies in different countries, and has motivated us to study how economic resources affect integration. Yet, there is no extensive literature concerning this issue for the Nordic countries. In relation to settlement policies, Edin et al. (2004) found that immigrants in Sweden experienced substantial long run losses due to the dispersion of refugees. This has motivated us to make a contribution to the literature concerning economical resources, that could potentially be relevant in a debate concerning the Norwegian settlement policy of refugees. This paper studies the integration process in Norway, in light of the integration and settlement policy frameworks that was introduced in 2002 (BLD, 2011). The aim of the policy is to enable refugees to participate in the labour market and the society as quickly as possible. In accordance to the policy, refugees are assigned to municipalities across the country and the municipalities are responsible for the integration process. In 2005, a two-year full-time introduction program was rolled out, with the aim of qualifying refugees for the labour market in order to become financially independent (Brochmann, 2011). However, the completion rate and the quality of the program varies among municipalities. Furthermore, the received governmental grants do not cover the municipal integration expenses, and municipalities therefore have to cover the gap from other revenue sources (Beregningsutvalget, 2016). Thus, differences in municipalities' financial condition may impact the resources devoted to integration of refugees and other local public services. Municipalities' financial condition can be measured in terms of unrestricted income, a source of income that consists of tax revenues and governmental transfers (KMD, 2011). Income levels substantially vary between municipalities due to hydropower production, causing exogenous variation in tax revenues.

1.2 Research Question

We use exogenous tax variation to identify the effects of municipalities' financial condition on the integration of refugees. This could potentially provide information on what type of municipality that more successfully manages to integrate refugees. Our hypothesis is that refugees that are settled in wealthier municipalities, measured by the level of unrestricted income, are more successfully integrated. However, based on the possible economic inefficiency derived from natural resources, the effect can also be the opposite. Thus, our research question is as follows:

"Does the level of municipalities' unrestricted income affect integration outcome of refugees?"

To measure the outcome of refugee integration, we have to ensure that all refugees are given the same treatment. Thus we limit our time period to the years 2005 - 2010. Policies related to settlement of

refugees have remained the same since 2002, but the implementation of the introduction program in 2005 may have created side effects, and 2005 is therefore chosen as base year. Furthermore, we restrict our paper to look at refugees who are eligible for labour market participation, ranging between 20 - 55 years. Retirees, elderly, unaccompanied minors, and children are therefore not included in this paper.

Integration is a broad term and can be measured by different variables post-settlement. Labour market integration and enrollment in education are measures that are in line with the government's objectives of integration (BLD, 2011). In this paper, we measure integration by the outcome variables earnings and social security benefits. Earnings capture both the wage and the employment status of refugees. Social security benefits capture the effect of those who are completely outside the labour force or partly unemployed. Refugees may be strongly influenced psychologically by the situation in their homeland and may experience difficulties adapting to their new host country in terms of cultural and linguistic challenges. This can explain why the unemployment rate amongst refugees is found to be higher than those of other immigrants and individuals (Konle-Seidl and Bolits, 2016). Lack of successful integration may imply large socioeconomic costs to Norwegian society, through the payment of social security benefits. Thus, we find it interesting to use this as an additional measure of integration.

To obtain a causal effect, we depend on exogenous variation, both in terms of the settlement of refugees and the level of municipalities' unrestricted income. Randomness along these dimensions allow us to compare outcome between municipalities, some of which are exposed to and benefit from the revenues generated from hydropower. We select a sample of municipalities with high income levels generated from hydropower. To remove systematic differences, we construct a counterfactual from neighboring municipalities with little or no hydropower revenues. Based on this, we conduct an empirical study where hydropower revenues are used as an instrument for municipalities' unrestricted income. Our empirical analyses for earnings suggest that unrestricted income has a significant effect only on women's earnings, at a 10 % level. However, the significance level is sensitive to changes in the control variables. For the baseline estimate of earnings, unrestricted income is insignificant, and the estimate is sensitive to various specification checks. These results suggest that we cannot say whether unrestricted income has an effect on earnings. For social security benefits, we do not find evidence that unrestricted income has an effect, as estimates for all specifications are insignificant. The baseline estimate seems robust as it is insensitive to modifications in the model.

This paper is structured in eight sections. In Section 2, we present the integration and settlement processes of refugees in Norway. Section 3 presents relevant literature. Section 4 describes the data and Section 5 describes the empirical approach of the paper. Section 6 presents the main results, followed by sensitivity analyses. In Section 7, we provide a discussion of the results and the shortcomings of the data and the empirical approach. Section 8 concludes. A list of acronyms used throughout the paper is provided in Appendix A1.

2 Background

In this section, we present the institutional background of the paper. In order to discuss and interpret the results, knowledge about the process of integration is crucial. The integration policy is described in detail, and the revenue structure of municipalities in relation to revenues from hydropower is elaborated.

2.1 Settlement and Integration of Refugees in Norway

The aim of the Norwegian integration policy is to enable newly arrived refugees to participate in the labour market and society as quickly as possible (Hjelde, 2016). It is in the best interest of both Norwegian society and the newly arrived refugees that they quickly qualify for the labor market and become financially independent (Justis- og beredskapsdepartementet, 2015-2016). To capture the relevant aspects of the institutional background, we find it relevant to elaborate on some definitions related to integration, and describe the groups of refugees that are covered in this paper. A refugee is a person who has fled his or her homeland because of conflict or persecution and shall by international law be protected (UN, 2016). When immigrants are given the status as refugee, they are eligible to apply for asylum. A refugee is given the status as an asylum seeker if asylum is applied for on arrival or shortly after. If the need for protection is genuine, the application will be granted and the refugee is given the right to a residential permit in a municipality (Brochmann, 2011). An exempted group is resettlement refugees, also referred to as quota refugees, who are registered as refugees by the United Nations Refugee Agency (Justis- og beredskapsdepartementet, 2015-2016). The group is by international agreement offered resettlement in a third country since they cannot be offered permanent settlement in the country they are currently in. Resettlement refugees are placed directly in the municipalities and are not part of the reception center system. Asylum applicants and resettlement refugees belong to the group referred to as primary refugees. Family extensions and family reunifications belong to the group called family of primary refugees. Figure 1 illustrates the different groups and their relative size based on data from 2013. We observe that the majority are primary refugees. Family extensions are not considered, as we limit the paper to look at first generation refugees.

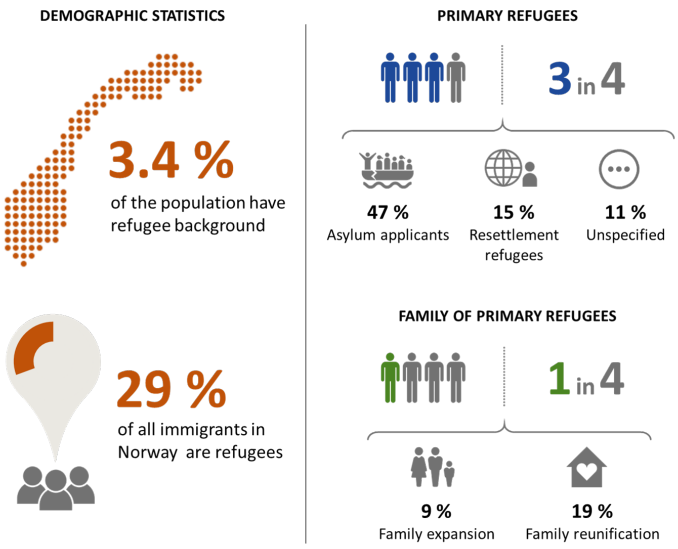


Figure 1 – Refugee statistics 2013

Source: Data from SSB (2016a)

The Settlement Process

The current refugee settlement policy was introduced in 2002 (BLD, 2011). The policy aims to ensure stable and successful settlement that contributes to education, employment, and development of social qualifications. The policy relies on a collaborative model between the municipalities and the Local Government Organization (Kommunesektoren, KS). The war in Bosnia-Hercegovina in the beginning of the 1990s led to an increasing flow of refugees to Norway and put pressure on an effective integration process (Brochmann, 2011). To improve the process, municipalities all over the country had to participate in the settlement of refugees. This has since become an important principle in the settlement policy, in order to avoid enclaves by reducing the concentration of refugees in large cities and reduce the pressure on central municipalities (Hainmueller et al., 2016).

The integration policy requires collaboration between several parties. The Ministry of Children, Equality, and Social Inclusion (Barne- og likestillingsdepartementet, BLD), is the coordinator and has the overall responsibility for the immigration- and integration policy (Brochmann, 2011). The Norwegian Directorate of Immigration (Utlendingsdirektoratet, UDI), is responsible for processing the applications for asylum and residence permits (Justis- og beredskapsdepartementet, 2015-2016). Asylum seekers are offered accommodations in reception centers during the processing time of the application. The aim of the system of the reception centers is to contribute to efficient processing, resettlement, and return. It is important for the government that this period is short, to start the integration process as quickly as possible. However, the processing of the asylum applications can often be very tedious and complex, as individuals who have a real need for protection can be difficult to identify, due to the lack of identification papers. When refugees are granted asylum, the policy aim is that refugees are settled in a municipality

within six months after acceptance (Brochmann, 2011). However, the wait is often longer due to for example the lack of suitable accommodations in the municipalities. The consequence can be delay in the transition to the labor market, and a passive life in the reception center can have demotivating and negative long-term effects on the refugees (BLD, 2011). During the time period of 2005 - 2010, the number of granted applications ranged between 41 - 58 % (UDI, 2016).

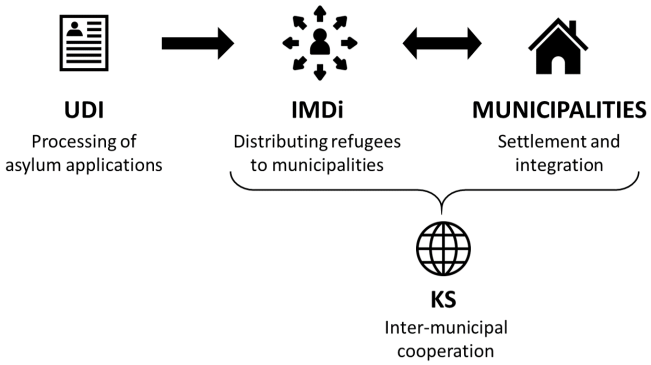


Figure 2 – Stakeholders of the settlement process

Most relevant stakeholders for our research question are the Directorate of Integration and Diversity (Integrerings- og Mangfoldsdirektoratet, IMDi), KS, and the municipalities. Figure 2 illustrates the responsibilities and the relationship between the relevant stakeholders. IMDi is responsible for distributing the refugees who have been granted resident permits in Norway (IMDi, 2016b). First, the total number of refugees with the need for municipal settlement is calculated by a national committee (Justis- og beredskapsdepartementet, 2015-2016). The municipalities are responsible for the actual settlement, while KS is responsible for coordinating the inter-municipal cooperation, to ensure that the municipalities accept enough refugee settlements. IMDi together with KS, send a request for settlement of a certain number of refugees to each municipality. This number is decided based on an assessment of the number of inhabitants, labour market opportunities, suitable permits, and knowledge and experience about refugee integration in the municipality. Based on IMDi's request, the municipalities make the final decision of how many refugees to settle. IMDi assigns individuals to the municipalities and refugees receive an offer for settlement in the assigned municipality. If the offer is rejected and the refugee decides to settle elsewhere, the right to participate in the introduction program and receive introduction benefits lapses. Thus, over 90 % of all refugees accept the offer for settlement by agreement with IMDi and the municipalities.

Prior to 2002, UDI had the main responsibility for the settlement process and KS was not involved to ensure that the collective settlement responsibility was taken care of (BLD, 2011). The municipalities had the opportunity to impose strict conditions on UDI concerning what nationalities and/or groups they were willing to accept. The process was complex and UDI found it challenging to find municipalities willing to settle certain nationalities and/or groups, which led to a difficult process. To achieve a more efficient process, the collaborative model between KS and the municipalities was introduced and IMDi was given the responsibility of finding suitable municipalities.

The Introduction Program

The introduction program is one of two schemes in the Norwegian Introduction Act, which was introduced on September 1st in 2004 (Brochmann, 2011). The costs of integrating refugees are highest the first years after settlement (Justis- og beredskapsdepartementet, 2015-2016). The integration program is therefore aimed at giving refugees qualifications relevant to the labour market and help them to become financially independent. All municipalities that settle refugees are obliged to offer the program in line with the act's intentions and provisions. Municipalities are free to decide how the program is organized and who is given the primary responsibility for implementation (Tronstad, 2015).[1] It is a full-time program that aims to give newly arrived refugees basic qualifications in Norwegian and social studies, and prepare them for participation in the labour market. The program is set to last up to two years, but can be extended up to one additional year if necessary. Refugees and family members between 18 and 55 years have the right and obligation to participate in the program (Hjelde, 2016). According to the act, qualified refugees shall enroll in the program within the three first months after municipal settlement. However, most refugees are found to participate in the program in the second and third year after settlement (Beregningsutvalget, 2016). Participants in the program also have the right to receive introduction benefits.

According to the Introduction Act, all participants have the right to be given an individually adapted plan based on the individual's needs and qualifications (Brochmann, 2011). Since each municipality is responsible for organizing the program, there is significant variation in the quality of the programs offered among municipalities, and the success of adapting the plans based on individual needs (Kavli et al., 2007). Furthermore, the completion rate varies between municipalities. Demographic differences and inequalities in terms of municipalities' revenues are examples of factors that may cause local differences. Despite the differences in terms of organization, quality, and completion rate, the introduction program is considered a success and has resulted in higher labor market participation and fewer applications for social security benefits than prior to 2005. Statistics concerning the outcome of the introduction program show that 61 % of the refugees that completed the introduction program in 2010 were employed or enrolled in education within one year of completion (Enes, 2014). The rate of employment/education enrollment of the participants after completion is found to be higher among men than women. Furthermore, around 10 % were registered as unemployed or receivers of social security benefits. The numbers also vary between the country of origin of the refugees. In addition, structural factors such as local labour market conditions may vary over time, impacting the outcome as refugees are settled in different municipalities across Norway. One example is the Financial Crisis in 2008 which affected the labour market conditions in Norwegian municipalities differently.

[1] In 2010, 44 % of the municipalities organized the program through the Norwegian Labour and Welfare Administration (NAV), 52 % outside NAV, and 4 % through municipal cooperation with another municipality.

Financial Implications

Municipalities receive governmental grants to cover the costs of integration. An annual integration grant is given for every settled refugee, which aims to cover the average costs of integration and settlement the first five years of municipal settlement (Brochmann, 2011). This includes administration costs of the introduction program and other expenses related to health and social security (Brochmann, 2011; Schanche, 2011). The grant was introduced in 1991 and the given amount has remained relatively stable in real terms.[2] Municipalities have the right to retain any surplus of the integration grant, as an incentive to get refugees to participate early in the labour market or educational system [3].

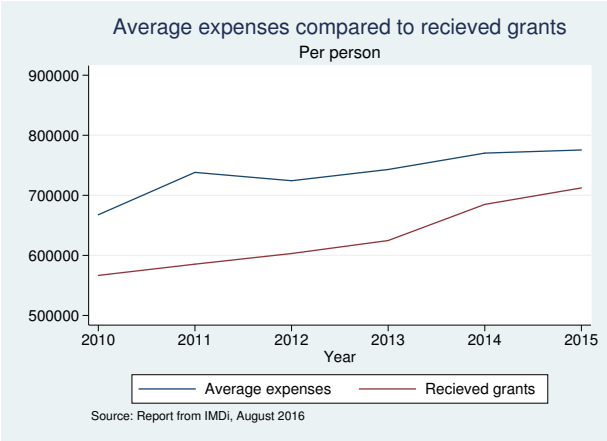


Figure 3 – The ratio between integration grants and expenses

The total expenses related to settlement and integration vary between municipalities (Beregningsutvalget, 2016). Economies of scale, unemployment rates, and municipalities' financial condition are examples of factors that might cause variation in expenses. The actual expenses cannot be obtained for most municipalities, as these are not extracted separately in the municipal accounts (Justis- og beredskapsdepartementet, 2015-2016). A committee consisting of representatives from the government and KS was therefore established in 1990 to calculate the average coverage ratio of the integration grant (Beregningsutvalget, 2016).[4] The committee has found that the average expenses per person exceed the received integration grant in the years 2010 - 2015, with a coverage ratio ranging from 85 % in 2010 to 92 % in 2015. Figure 3 displays the gap between received grants and the actual expenses per refugee for the sample municipalities. As the refugee integration policy has remained the same since 2002 and the fact that there have only been small adjustments to the received grants in earlier years, we find it reasonable to assume that this gap was present during the time period of 2005 - 2010. This finding implies that municipalities on average must cover the gap from other revenue sources. Municipalities do not necessarily want expenses to exceed

[2] In 2010, the grant was 551 500 NOK per refugee (Schanche, 2011).

[3] Grants for education in Norwegian and social studies are also given, but will not be discussed further in this paper (Justis- og beredskapsdepartementet, 2015-2016).

[4] The analysis is performed annually on a sample of around 20 representative municipalities, selected based on geographical distribution, municipality size, and share of refugees.

the received grant. On the other hand, the gap can also result from the fact that municipalities in better financial situations choose to devote more resources on the integration and settlement process (Justis- og beredskapsdepartementet, 2015-2016). Settlement of refugees may contribute to increased expenses on local public services such as labour schemes, social services, health services, and cultural activities (IMDi, 2016a). Appendix A1 displays the per capita expenses on these services for the sample of municipalities in our study.[5] The expenses are higher in the wealthier municipalities for all services displayed.

2.2 Municipal Revenues

Since the coverage ratio of the received integration grant is only around 90 % on average, we will elaborate on the financial situation of municipalities. Municipal revenues mainly come from three sources; governmental transfers, taxes, and fees (KMD, 2011). Figure 4 illustrates the relative distribution of revenues by source. The first source is through a fixed framework of transfers from the government to the municipalities, also referred to as general grants. These transfers are given on a per capita basis and aim to account for inequalities in terms of different geographic, demographic, and social structure between municipalities. The grants are to ensure a minimum level of services offered by the municipality and therefore works as a redistribution mechanism. The second source is tax revenues, which consist of income and property tax (KMD, 2011). The tax revenues and the general grants, together account for what is referred to as unrestricted income, and represent around $\frac{2}{3}$ of the municipal sector's total income. Municipalities are free to use this income with no restriction other than applicable laws and regulations. Unrestricted income can therefore serve as a measure of the municipalities economic flexibility and financial condition. Since the expenses related to integration are not displayed in the municipal accounts, unrestricted income can serve as a measure of municipalities' available resources to cover these expenses. The third source of revenues is through fees and other earmarked governmental grants. These revenues are restricted to the fulfillment of minimum standards and statutory responsibilities and do not affect unrestricted income (KMD, 2016a).

[5]Integration expenses cannot be obtained. Thus, the figure displays per capita expenses on certain large municipal service areas. The figure shows data for the sample municipalities which will be elaborated in Section 4.

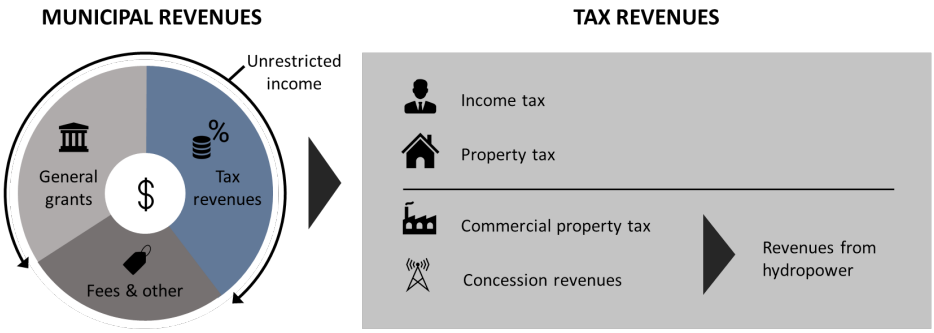


Figure 4 – Municipal revenues

Source: Information by KMD and LVK (2016)

Revenues from Hydropower

Revenues substantially vary between municipalities due to the existence waterfalls in the municipalities, a natural resource which is used to generate hydropower (THEMA Consulting Group, 2011). Norway is very abundant on the resource and 99 % of all power generation in Norway comes from hydropower (Hydro, 2016). Municipalities can be eligible for three main types of revenues generated from hydropower (LVK, 2016).[6] These are property taxes from hydropower plants, concession power and fees, and natural resource tax. The natural resource tax does not affect unrestricted income, as it is comprised of the government's revenue transfer system and will therefore not be considered in this paper (THEMA Consulting Group, 2011). This study concentrates on tax revenues from hydropower plants and concession revenues. Tax revenues represent a large share of the property tax base in many municipalities (Haegeland et al., 2012). Municipalities can choose to exempt residential housing from property tax and only impose the tax on commercial buildings, which include hydropower plants. A large share of the municipalities with hydropower plants has applied the maximum tax rate. The property tax amounts to a maximum of 7 ‰ of the property value (LVK, 2016). Revenues from concessions consist of concession fees and revenues from concession power. These revenues are received as a compensation for the interventions in the municipalities' environment, which might cause environmental damages. Revenues from concession fees are explicitly earmarked for business development funds (Olje- og energidepartementet, 1917). Concession power revenues vary with the production level and the market price of electricity. The market price of electricity is affected by supply-side factors, such as wind and rain, and demand-side factors, such as temperature. The volume of received concession power is given by a share of the hydropower plants' annual average production, capped at 10 % (THEMA Consulting Group, 2011). As illustrated in Figure 4, the sources of revenues discussed above are relevant for this paper as they affect the municipalities' unrestricted income. They are not comprised by the government's revenue transfer system, and can thus

[6]Revenues can also occur through ownership in power stations in other municipalities. However, municipal ownership has been reduced during the last two decades as many of the power companies have been sold, due to deregulation of the electricity market (Borge and Torvik, 2015). It does not affect unrestricted income and will therefore not be considered.

be used freely by the local government (THEMA Consulting Group, 2011).

Production of hydropower provides a tax base for a group of municipalities. In 2013, 922 developed power plants were located in 300 municipalities according to data from the Norwegian Water Resources and Energy Directorate (Norges vassdrags- og energidirektorat, NVE). To illustrate the uneven distribution of hydropower revenue between municipalities, per capita revenues from hydropower in the year 2005 - 2010 are displayed in Figure 5. An interesting aspect is to look at the revenues from hydropower in relation to municipalities' unrestricted income to see the impact of the presence of hydropower. This relationship is displayed in Appendix A4 for the sample municipalities in this study. To sum up, revenues from hydropower constitute a large share of municipalities' finances for a group of municipalities. This is the basis for our identification strategy, described in the Empirical Approach in Section 5.

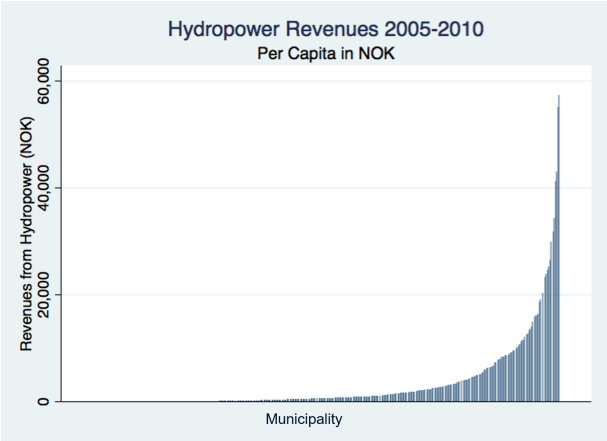


Figure 5 – Distribution of hydropower revenues

Source: Data from TBU (2008)

3 Literature Review

There exists a vast amount of literature on immigration, but literature that study the effect of municipalities' financial condition on refugees' earnings and social security benefits by developing a similar instrument as ours, does not exist to our knowledge. Thus, we will first present literature with relevant aspects concerning labour market outcomes of refugees and empirical application of settlement policies. Second, studies using geographical variation as an instrument in other applications will be presented.

3.1 Literature on Refugees

Edin et al. (2003) study the economic success of immigrants using data from an immigrant policy initiative in Sweden when government authorities distributed refugee immigrants randomly, conditional on observed characteristics. The paper argues that the settlement policy provided a unique natural experiment, which allowed for estimation of a causal effect on labour market outcome of living in enclaves. The researchers base the documentation of the practical implementation of the settlement policy on interviews with placement officers. We follow the same approach, as little documentation on the practice of the settlement policy is available. Edin et al. (2003) study the time period 1987 - 1989, as this was the strictest application of the settlement policy. We select our time period based on the same criteria, to ensure that all refugees are given the same treatment. Another study concerning labour marked performance is a study by Sarvimäki and Hämäläinen (2016). In contrast to Edin et al. (2003), this study uses earnings in levels as the outcome variable. The motivation for using earnings in levels is that an important part of assimilation to the Finnish labour market consists of moving from zero to positive earnings. As our outcome variables contain observations ranging from zero to five years post-settlement, we find the approach appropriate for our purpose.

A study by Åslund et al. (2011) examine to what extent school performance of immigrants is affected by the characteristics of the neighborhoods that the immigrants grow up in. The issue is addressed by exploiting exogenous variation in the initial location of residence in Sweden. More specifically, they assume that the resident assignment is random with respect to unobserved characteristics. The refugees can indeed express a preference on location. However, based on the institutional setup, scarce housing, the short time frame between permit and placement, and no direct interaction between local placement officers and individual refugees, Åslund et al. (2011) argue that any selection must have been on observed characteristics. As the settlement process is quite similar, we base some of our assumptions concerning the Norwegian settlement policy on this paper.

The paper by Edin et al. (2004) measures immigrant outcome eight years after immigration, as they are interested in long-run impacts from the settlement policy in Sweden. Specifically, they use the Swedish settlement policy as a natural experiment to investigate the economic success of immigrants. The main

finding is that immigrants receive long-run losses due to the settlement policy, caused by a shift in policy focus from labour marked assimilation to income support. An implication of their findings is that the losses could to some extent be avoided by a more careful choice of settlement municipalities, and a more labour market-oriented policy. As the Nordic countries are similar along several dimensions, the implication of this study is the main motivation for this paper. Thus, we want to investigate whether certain types of municipalities perform better in terms of integration. This could potentially be relevant in a debate concerning the Norwegian settlement policy.

3.2 Geographical Variation as an Instrument

The study by Borge and Torvik (2015) is directly relevant to our paper as is constructs an instrument based on hydropower. The paper investigates whether higher public revenues harm efficiency in the production of local public goods. The study takes advantage of the variation in tax revenues generated from hydropower production, as hydropower revenues generate a tax base for only a group of municipalities. This exogenous variation can address the typical problems of endogeneity arising from revenues collected from natural resources. They obtain an exogenous measure of local revenues by instrumenting the variation in hydropower revenues by topology, average precipitation, and river length in steep terrain to predict hydropower revenue. This differs slightly from our approach as we do not base our data on predicted revenues based on locational variables. The paper provides a list of the eight municipalities with highest per capita revenues in 2007. These municipalities are in line with the hydropower municipalities in our sample, selected based on data assembled on hydropower tax revenues by TBU (2008).

Another relevant study by Haegeland et al. (2012) uses exogenous tax variation to identify effects of resources on pupil achievement. The empirical strategy is a standard two stage least squares model, where hydropower tax revenues are used as an instrument for school expenditures. Their identification strategy rests on the idea that the wealthier municipalities spend more resources on schooling. This is in line with our approach, as our strategy builds on the fact that the integration expenses on average exceed received grants. One difference is that we have access to municipal data on tax revenues generated by hydropower plants from 2007, and such data were not readily available when the paper by Haegeland et al. (2012) was written, as the accounting practice did not split property tax by source.

4 Data Description

This section describes the data used in the analyses and sample selection process. First, the data on refugees are described, followed by the data on municipalities. Lastly, descriptive statistics of the data are presented.

4.1 Refugee Data

The empirical analysis is partly based on cross-sectional data on refugees from the resident registry of Statistics Norway (SSB). All individual variables in our analyses are based on registry information. The original data set contains approximately 250 000 observations and 65 000 individual refugees from the time period 2001 - 2010. This means that we on average have 3.85 observations per refugee. The data set is unbalanced as we have a various number of observations on each refugee (Wooldridge, 2009). The data is divided in skill-cells according to year of birth, country of origin, year of settlement, resident municipality, education level, gender, children, and marital status. Thus, each refugee belongs to one skill-cell group. For refugees in the same skill-cell, mean values of annual earnings and social security benefits are displayed. These contain a time dimension and can be considered outcome variables, as they serve as potential measures of integration post settlement.

Identification of Refugees

The data set originally consists of all immigrants arriving in Norway from 2001 - 2010. Thus, immigrants that come to Norway for the purposes of family reunification, labour, flight, or education are included. Immigrant status is not specified, and hence we cannot identify immigrants with refugee status directly from the data. This is a common issue in the literature and we use a standard procedure to overcome this problem. To extract primary refugees from the data, we use country of origin as identification, since typical refugee-countries can be identified based on historical events (Edin et al., 2004; Foged and Peri, 2016). This approach enables us to restrict the data set to include immigrants from 22 countries. The choice of countries is based on a report from SSB containing causes of immigration (Dzamarija, 2013). The report includes a list of immigrants by country of origin who have been granted residency in Norway, where flight from their home country is given as the cause. In addition, some countries are selected based on historical events, such as crisis and wars which have generated large waves of refugees in the years of interest. As a general rule, only countries outside Western Europe are included (Edin et al., 2003). Figure 6 provides an overview of the selected countries of origin, where the size of the circle represents the number of refugees settled in Norwegian municipalities in the time period 2001 - 2010. We observe that the majority of the refugees come from Russia, Somalia, Iraq, China, and Afghanistan. Appendix A2 displays the selected countries in an ascending order by the total number of refugees settled.

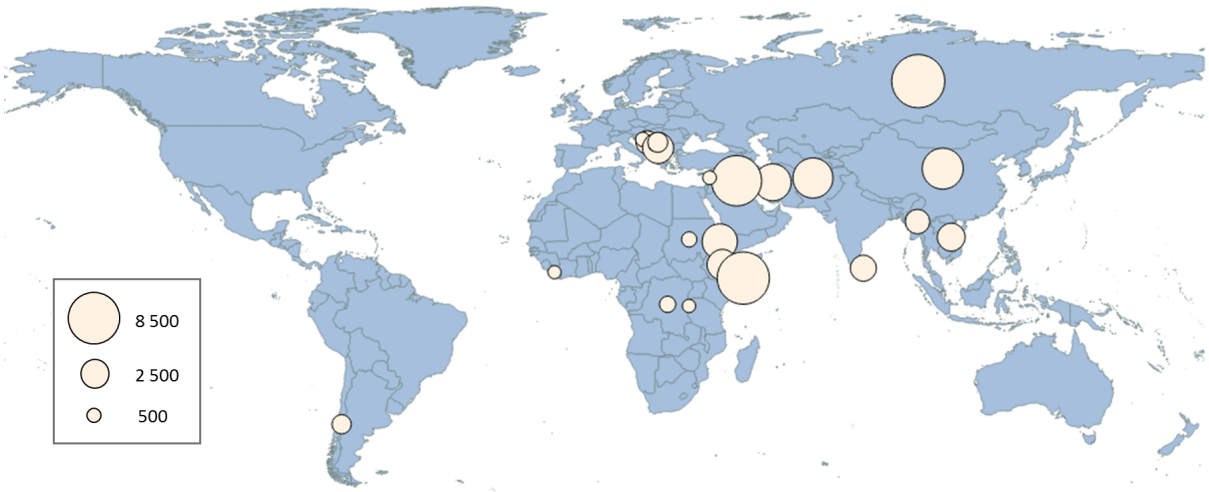


Figure 6 – Refugee settlements by country of origin

Source: Registry data, SSB

The approach of selecting certain countries based on SSB's report and historical events relies on the assumption that all individuals from the selected countries are refugees. According to SSB, there were refugees from 169 different countries living in Norway in 2015 (Østby, 2016). SSB have a longer list as they are can determine the cause of immigration. Thus, they can include countries in which the immigrants mainly have other reasons for immigration than flight. For example, labor immigration to Norway is common from many emerging European countries. Yet, immigration as a result of flight also occurs from these countries, a situation our approach cannot account for. Including these countries in our data set would lead to biased estimates, as their background in terms of education, earnings, and life situation is often very different from the background of refugees. Thus, a precautionary approach is followed when identifying refugees.

Another cause of immigration is family reunification. In these cases, the settlement policy cannot be considered random as the refugees reside in the same municipalities as their family (Edin et al., 2003). Family reunification is not displayed in the data, but as of 2013, these cases consisted of around 20 % of all refugees in Norway (SSB, 2016a). This makes the initial settlement of refugees somewhat less random for the group of women and very young men, as they typically belong to the group of family reunification. A discussion concerning the random element in the settlement of refugees will follow in Section 5.2 in the Empirical Approach.

Selection of Refugees Eligible for the Labour Market

As this paper aims to analyze integration outcome of refugees, measured in terms of earnings and social security benefits, we select a sample of refugees that are eligible for the labour market [7] We base the analysis on refugees aged 20 - 55 years. By imposing a lower age limit, we exclude children and unaccompanied minors. Furthermore, the lower age limit to be eligible for participation in the introduction program is 18 years. As we look at the effect on labour market outcomes, we assume that refugees are unlikely to enter the labour market before the age of 20. The upper age limit is set based on the argument that refugees over the age of 55 at the time of settlement are unlikely to enter the labour force, nor are they eligible for the introduction program (Edin et al., 2003).

Selection of Time Period

In order to measure integration outcome of refugees, we have to ensure that all refugees are given the same treatment. We therefore limit our time period to the years 2005 - 2010. The upper restriction is based on the fact that we do not have observations beyond 2010. An exemption is the analyses of social security benefits where we use the time period 2005 - 2008 as we do not have observations on this variable beyond 2008.

The choice of 2005 is based on the following argumentation. During the time period of which we have data, there have been changes in the political reforms of integration. The collaborative model between KS, IMDi and the municipalities was introduced in 2002. This caused the settlement policy to become more random since municipalities could no longer request certain nationalities and/or groups (BLD, 2011). To avoid selection bias, we choose the period in which the municipalities could not be as selective in terms of the settlement of refugees. Furthermore, the introduction program was implemented in 2005 to ensure that all refugees are equally integrated and given the same opportunities. Appendix A3 displays box plots with average values of earnings and social security benefits for the period 2002 - 2008, adjusted for inflation.[8] We observe a slight increase in earnings and a slight decrease in the receipt of social security benefits during the time period. As this paper does not evaluate the

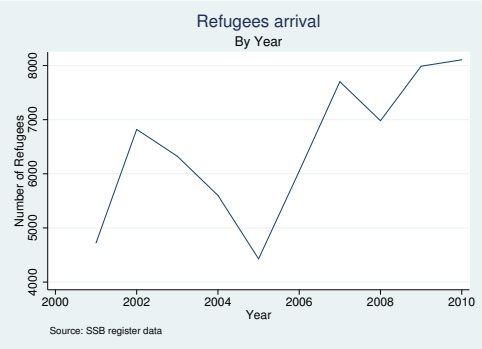


Figure 7 – Annual settlement of refugees, 2005 - 2010

Source: Registry data, SSB

[7] The data contains information on the year in which the refugee is settled in a municipality and the year of birth. Based on these two variables, we can calculate the refugee's age at settlement.

[8] Only the years 2002 - 2008 are displayed, as there are no observations for social security benefits in 2009 and 2010.

effects of the introduction program, we choose to be precautionary about the time period. As all refugees that were settled after the implementation are eligible for the same program, and in theory receive the same treatment, we base the empirical analysis on refugees' post to the introduction program. Based on the new reform in 2002, the implementation of the introduction program in 2005, and the fact that number of settlements is higher in the second half of the time series as displayed in Figure 7, we find it appropriate to use data from the years 2005 - 2010 as a basis for our analysis. In addition, selecting a shorter time period is beneficial as we expect the characteristics of refugees to be more similar.

4.2 Municipal Data

Municipal Demographics

For the analyses, we use demographic data at municipality level. Data from 2005 - 2010 is assembled from SSB on the number of inhabitants, average earnings adjusted for inflation, and the unemployment rate. The number of inhabitants is used as selection criteria for the comparable municipalities. Average earnings and unemployment rate are used as control variables in the empirical analysis. Descriptive statistics of the variables are provided in Table 1 on page 21.

Unrestricted Income

Based on the gap between the received grants and the costs of integration and the differences in terms of organization, quality, and completion rate of the introduction program, municipalities' financial condition may affect the integration process of refugees and thus the labour market outcome. Unrestricted income can be used as a measure of municipalities' financial condition and is therefore relevant for our empirical analysis. We have collected data on municipalities' unrestricted income from SSB. The data contains information of unrestricted income per capita for each municipality in the years 2005 - 2010. Unrestricted income per capita provides a relative measure which is suitable for comparison purposes. The measure also takes population growth into account. From the available data, we observe a systematic increase in the level of unrestricted income over the time period. The increase is partly because the data is unadjusted for inflation and due to changes in the municipalities' operational responsibilities (KMD, 2016b).[9] We cannot map responsibility changes and assume that all municipalities are equally affected by such changes. However, we adjust the time series for inflation to provide a comparable measure.

[9] An example of a change in responsibility, is when tasks are transferred from the government to the municipalities.

Hydropower Revenues

Revenues from hydropower contribute to variation in unrestricted income between municipalities. For this purpose we have collected data from reports by the Ministry of Local Government and Modernisation (KMD), written by the Statistical Reports Committee [10]. The data contains annual observations on property tax revenues and concession revenues per capita on municipality level, hereby denoted revenues from hydropower. Only reports from 2007 and onwards contain detailed information on revenues from property tax divided by source of revenue. We aggregate the revenues generated from property tax on hydropower plants and concessions for the observable period. We use data on revenues from hydropower from the years 2007 - 2010 to predict the corresponding revenues for 2005 - 2006, by using the average revenues in the observable time period. To illustrate the distribution of hydropower revenues between municipalities, we divide the municipalities with positive hydropower revenues per capita by percentiles, displayed in Figure 8. The 10th percentile displays that the top 10 % of the municipalities with highest revenues from hydropower account for over 50 % of the total revenues. Percentile number 9 and 10 together account for 75 % of the total revenues.

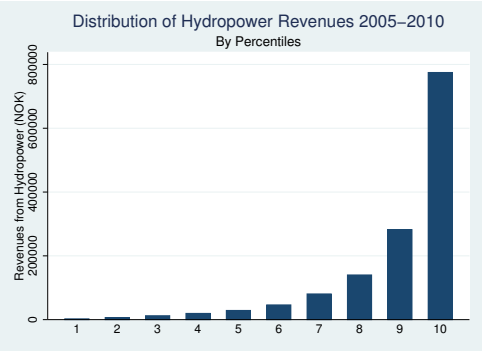


Figure 8 – Percentile distribution of hydropower revenues

Source: TBU (2008)

The prediction of revenue generated from hydropower is only a valid estimate if there have been few changes in the time period 2005 - 2010. To get an overview of developed hydropower plants, we have obtained data from the Norwegian Water Resources and Energy Directorate (NVE).[11] We use the data to track changes in operating hydropower plants in the two periods for the municipalities included in the samples (described in section 4.3). As operating plants indicate activity level, they could be an indicator of revenues generated from hydropower. In the time period 2005 - 2010 there are no new developments of hydropower plants with production capacity 10 - 100 MW and above 100 MW. There are 14 new developed plants in seven of the municipalities in the time period 2005 - 2006, with production capacity between 1 - 10 MW. The corresponding number for 2007 - 2010 is 24 plants in 14 municipalities. However, these plants can be considered low production capacity plants. The new developments are also spread between the municipalities. Based on this, we believe that the predicted revenues from hydropower are valid.

[10] Data is assembled from the following reports: TBU (2008, 2009, 2010, 2011)

[11] The data contains observations on the operating year, municipality, maximum megawatt and stipulated annual production.

4.3 Sample Selection

In the following section, we describe how two samples are selected for the analysis. These are referred to as the "restricted" and "extended" samples. Each sample consists of a selection of municipalities wealthy from hydropower, which is necessary since the paper relies on an instrumental variable approach. Furthermore, the samples consist of a set of comparable municipalities selected by a matching approach, where a counterfactual without hydropower revenues from neighboring municipalities is constructed (Haegeland et al., 2012). These are hereby denoted as comparable municipalities. Lastly, descriptive statistics are presented in order to check the balance of the samples.

Hydropower Municipalities

Based on data on hydropower revenues, we generate two subgroups of municipalities that have the highest per capita revenues in the time period. For the restricted sample, the 20 municipalities with the highest per capita revenues from hydropower are selected. Around half of the municipalities are excluded as they receive zero or few refugees in the period of interest, and we are left with a sample of nine municipalities.[12] They are all in the 10th percentile in terms of revenues from hydropower, with minimum and maximum per capita values of respectively 20 328 and 57 564 NOK. Our sample is in line with Borge and Torvik (2015), that provides a list of the eight municipalities with the highest per capita revenues in 2007. Four out of the eight municipalities included are present in the list above. The differences are due to the exclusion of municipalities that receive too few refugees, and the fact that our selected sample is larger to obtain more observations on refugees.[13] We find that the sample has few inhabitants, ranging from 906 to 4 217. As the restricted sample receives relatively few refugees, we also consider an extended sample where a cutoff at 10 000 NOK hydropower revenues per capita is imposed. We are left with a sample of 24 municipalities, where the number of inhabitants ranges between 906 and 9 595. The municipalities can still be considered wealthy as they are all in the 9th and 10th percentile in terms of hydropower revenues, with $\frac{2}{3}$ of the municipalities belonging to the 10th percentile. Appendix A4 displays that a large share of the municipalities' unrestricted income is derived from hydropower revenues.

Comparable Municipalities

Norwegian municipalities are different along several dimensions. In order to evaluate the effect of unrestricted income, we construct a counterfactual by using a matching approach. For the analysis, a set of non-hydropower municipalities is selected for the restricted and the extended sample. Neighboring municipalities can be considered suitable candidates as they are very similar in terms of economic structure,

[12] Appendix A5 and A6 display settled refugees in the restricted sample.

[13] Aurland, Eidfjord, Modalen, and Tydal are excluded.

labour market opportunities, and other demographic measures. Thus, they form a natural basis for comparison. The idea behind the set of comparable municipalities is to remove systematic differences. If all municipalities are selected, there may be systematic differences between characteristics of the treatment and control groups which will cause misleading results (Angrist and Pischke, 2008; Haegeland et al., 2012). First, all neighboring municipalities of the hydropower municipalities are selected. Second, we check that the demographic structure in terms of the number of inhabitants is similar to ensure comparability by imposing a cutoff at approximately 11 000. This makes sure that we do not compare the small hydropower municipalities with for instance municipalities with large urban areas, where the economic activity and labour market conditions differ greatly. Third, a cutoff for the hydropower revenues is imposed at respectively around 1 600 and 2 600 NOK per capita for the restricted and extended sample. We expect this to have little impact on the municipalities' revenues when taking the number of inhabitants into consideration. Lastly, the number of refugee settlements is considered in order to achieve a sufficient number of observations.

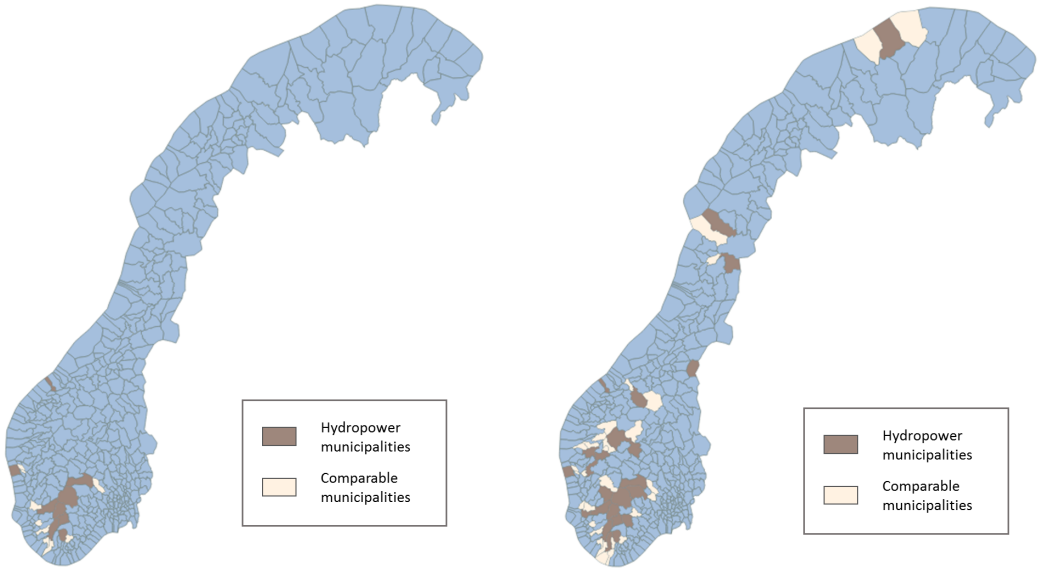
Of all neighboring municipalities of the hydropower municipalities in the restricted sample, we are left with a sample of nine comparable municipalities after imposing restrictions discussed above. The number of inhabitants ranges between 926 and 11 012, and hydropower revenues range from zero to 1 619 NOK per capita. For the extended sample, we are left with 30 comparable municipalities after considering the neighboring municipalities. The number of inhabitants ranges between 647 and 11 012 and there are 2 151 refugee settlements. Hydropower revenues range from zero to 2 579 NOK per capita, which can still be considered low as the revenues in the hydropower municipalities is around four times as high per capita.

4.4 Descriptive Statistics

Figure 9 provides a geographical illustration of the two samples. The restricted sample contains 18 municipalities, nine of which are considered very wealthy from hydropower and nine comparable municipalities. The sample contains 419 refugees from all 22 selected countries of origin and represents seven counties. The sample of hydropower municipalities is in line with the distribution of hydropower revenues displayed in Figure 8, where we observe that a small number of municipalities account for the bulk of the revenues. Conversations with employees from the Association of Hydropower Municipalities (Landssamanslutninga av Vasskraftkommunar, LVK), confirm this finding. The extended sample contains 54 municipalities, 24 of which are considered wealthy from hydropower and 30 comparable municipalities. This is constructed to achieve more observations on refugees that strengthen the statistical power, as the restricted sample only contains 419 refugees. The extended sample contains 2 151 refugees and represents 13 counties, without compromising the revenues from hydropower. In general, the samples mostly consist of small inland municipalities in terms of number of inhabitants. The samples do not include large cities, as the number of inhabitants is too low. The labour market in the sample municipalities is expected to be smaller

compared to the larger cities, and therefore we do not expect there to be many labour immigrants in our samples. Due to the exclusion of large cities we expect the economic structure within the municipalities to be quite similar and avoid the existence of several sub-economies, which may cause the undesirable effect of diverse labour market conditions within a municipality.

Figure 9 – Map of municipalities included in the samples



A) Restricted sample

B) Extended sample

Checking for Balance

To ensure that the subgroups of the samples can be considered comparable, refugee- and municipality characteristics are displayed in Table 1. We use weighted averages to account for the skill-cell structure. The outcome variables used in the analyses are also displayed in the tables. The rightmost column in the table display statistics for all municipalities in the data set. As large cities are included in this category, we expect the characteristics to differ somewhat from our samples.

Table 1 – Descriptive statistics (mean 2005 - 2010)

	Restricted sample		Extended sample		
	Hydropower	Comparable	Hydropower	Comaparable	All
Individual characteristics					
Age at settlement	34.65	32.15	32.46	32.60	31.41
Male (=1)	0.39	0.46	0.46	0.46	0.47
Married (=1)	0.89	0.77	0.81	0.79	0.75
Children (=1)	0.41	0.46	0.36	0.39	0.39
Education level distribution:					
0 years	0.54	0.54	0.57	0.57	0.54
5 - 8 years	0.02	0.03	0.02	0.03	0.02
9 - 11 years	0.07	0.06	0.05	0.05	0.06
12 - 14 years	0.03	0.05	0.05	0.05	0.05
$\geq$ 15 years	0.34	0.31	0.31	0.30	0.33
Observations	208	757	1 925	2 295	97 693
Municipality characteristics					
Inhabitants	2 480	6 539	4 191	4 679	11 018
Unemployment rate	0.015	0.018	0.019	0.020	0.024
Average earnings (NOK)	219 987	236 282	216 179	216 392	212 778
Unrestricted income (NOK)	44 799	32 972	40 559	37 319	37 863
Hydropower revenues (NOK)	32 564	537	19 847	854	3 296
Hydro share of unrestricted inc.	0.73	0.02	0.49	0.02	0.09
Observations	9	9	24	30	423
Outcome variables					
Earnings (NOK)	138 387	172 271	166 769	152 083	177 076
Social security benefits (NOK)	76 093	48 511	37 504	39 994	42 608
Earnings (=1)	0.58	0.57	0.63	0.55	0.61

Note: Standard errors are displayed in parenthesis. Outcome variables and individual characteristics are weighted to account for the skill-cell structure. The variable "Age" is the the average age at the time when the individual is settled in the municipality. The descriptive statistics for social security benefits is based on the time period 2005 - 2008, since we do not have observations post 2008. The municipality characteristics are displayed as average values of the time period 2005 - 2010. We did not include the number years post settlement, as this variable increases over time and thus will have a downward bias.

Restricted sample

As of the refugee demographics, the baseline characteristics can be considered similar in the two subgroups. The average age of settlement is slightly over two years higher than in the comparable municipalities. There is a smaller majority of men, greater share of married refugees and less who have children in the hydropower municipalities. When comparing labour market outcome, educational level is of great importance as research shows that this variable affects earnings (Angrist and Kruger, 1991). The majority have zero education. Very few have between five and eight years of education, approximately equivalent to primary school in Norway. 6 % have between nine and 11 years, equivalent to Norwegian secondary school and one year of high school. Those between 12 and 14 years of education, approximately equal to a Norwegian high school, differ slightly in the subgroups. Approximately $\frac{1}{3}$ of the refugees in the subgroups have 15 years of education or more. We do not know whether these possess a formal degree of higher education, only that they have taken education equivalent to what is beyond Norwegian high school level (13 years). Overall, we consider the educational level similar in the two subgroups. However, looking at the educational distribution for all municipalities, this group is an equally good counterfactual. In terms of the municipal characteristics, we observe that the number of inhabitants differs slightly as the comparable municipalities on average have over twice as many inhabitants. However, all 18 municipalities are in the 1st quartile in terms of the number of inhabitants, and compared to all other municipalities, the difference is not significant. Average earnings differ with just above 15 000 NOK and the unemployment rate is quite similar. As the number of inhabitants and unemployment rate are variables that might reflect the labour market, similarities between municipality characteristics are of great importance. Thus, the subgroup of comparable municipalities is considered a good counterfactual. As expected, hydropower revenues and the share of unrestricted income are very different given our selection criteria. We also find it interesting to look at the descriptive statistics of the outcome variables used in the analyses. We observe that earnings are lower and social security benefits higher in the hydropower municipalities than the comparable.

Extended sample

The individual characteristics can be considered very similar along all dimensions, which suggests that they are comparable and that refugees are randomly assigned. The municipality characteristics are also very similar. The hydropower revenues and the share of unrestricted income are slightly lower than the hydropower municipalities in the restricted sample, but can still be considered significant as they are all in the 9th and 10th percentile. A larger sample containing more observations might be a reason why the individual and municipality characteristics are more similar than in the restricted sample. Based on educational level, and the number of inhabitants, we consider the two subgroups similar, and find the comparable sample suitable as a counterfactual. As for the outcome variables, we observe that average earnings are higher in the hydropower municipalities than in the comparable sample.

5 Empirical Approach

To estimate the effect of municipalities' unrestricted income on integration outcome of refugees, an instrumental variable approach is used as empirical strategy. Our identification strategy rests on the idea that municipalities with higher unrestricted income devote more resources to integration. As the expenses related to integration on average exceed the received grants, extra resources from the municipalities' unrestricted income must be used to cover expenses that exceed the received grants. This means we have evidence that supports our identification strategy (Beregningsutvalget, 2016). The main model that we aim to estimate, given no endogeneity problems, is displayed in the following equation:

$$(1) \qquad Y_{im} = \alpha + \beta_1 UnrestrictedIncome_{mt} + \beta_2 IndCharcs_{im} + \beta_3 MunCtrl_{mt} + v_{imt}$$

Since unrestricted income is not randomly distributed, differences between municipalities with high and low levels of unrestricted income can potentially cause endogeneity problems in terms of selection bias. For instance, there may be reversed causality in the sense that labour market outcomes affect unrestricted income. If the the labour force participation rate is particularly high, this might result in higher level of unrestricted income, i.e., the causality can go both ways. Comparing the level of municipalities' unrestricted income by performing an ordinary least square model (OLS) is therefore likely to be misleading (Angrist and Pischke, 2014). Selection bias is a result of unobserved differences, because of the non-random nature of unrestricted income across municipalities. Thus, we cannot interpret a causal relationship from an OLS regression.

5.1 Instrumental Variable

To overcome the problem of selection bias, a valid instrument, Z, can be used (Angrist and Pischke, 2014). In our instrumental variable approach, we exploit the location of hydropower plants and use hydropower revenues as an instrument as it constitutes a large share of municipalities unrestricted income. The instrument needs to meet two requirements for the coefficient to equal the causal effect (Angrist and Pischke, 2008). These will be elaborated below, under the section concerning requirements. The IV equation takes the form of the OLS equation as represented in equation (1) above, but the variable of interest is replaced by predicted unrestricted income:

$$(2) \qquad Y_{im} = \alpha + \beta_4 \widehat{UnrestrictedIncome}_{mt} + \beta_5 IndCharcs_{im} + \beta_6 MunCtrl_{mt} + v_{imt}$$

Where $\widehat{UnrestrictedIncome}_{mt}$ is generated by the first stage regression in the IV framework:

$$(3) \qquad UnrestrictedIncome_{mt} = \alpha + \beta_1 HPRevenues_{mt} + \beta_2 MunCtrl_{mt} + \beta_3 IndCharcs_{im} + u_{imt}.$$

The refugee data contains both cross-sectional observations in terms of individual refugee characteristics,

i, and a time dimension, t. Since refugees are free to move out of the assigned municipality, the data set in unbalanced. Furthermore, refugees do not have any unique identification, which implies that we are not able to follow the unique individuals over time. Thus, we only have a panel structure on data on municipality-level. We control for an extensive set of individual characteristics, denoted $IndCharcs_i$, and municipal characteristics, denoted $MunCtrl_{mt}$, to isolate the causal effect of unrestricted income (Wooldridge, 2009). The individual characteristics are age at settlement, number of years post-settlement, gender, marital status, children, country of origin, and years of education. The municipal control variables are of panel data structure on a municipality level, m, with a time dimension, t, and consist of average earnings and unemployment rate. We expect the controls to reduce the error variance and lead to more precise estimates of the effect of unrestricted income (Angrist and Pischke, 2008). The definitions and explanations for the control variables are presented in Appendix A3.

The key coefficient of interest in equation (2) is β_4. Y_{im} represents the outcome variable of integration, where the outcome measures are earnings and social security benefits. Earnings are displayed as annual earnings in NOK, adjusted for inflation. We include zero observations throughout the analysis and thus, both the variation in wage and employment are captured. Furthermore, we cannot distinguish between part-time and full-time work. We use earnings in levels, which is in line with the paper by Sarvimäki and Hämäläinen (2016) based on the motivation of capturing the refugees that move from zero to positive earnings after finishing the introduction program. As the time period is short, it is not economically intuitive that earnings reach very high levels. Social security benefits consist of disability and sickness benefits paid by NAV, and social security benefits paid by the municipalities. The variable is displayed as average annual social security benefits receipts, adjusted for inflation. We use the variable in levels based on the same economic arguments as the functional form of earnings. The number of observations per refugee ranges from one to six for earnings and one to four for social security benefits.

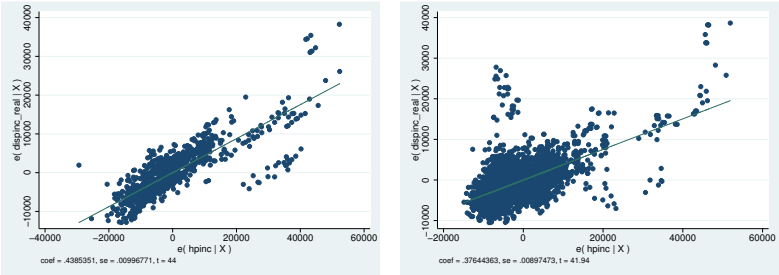
Requirements

For the instrument to be valid, it needs to be correlated with unrestricted income, but not with the error term (Angrist and Pischke, 2008). Thus, the first requirement implies that the instrument must have a clear effect on the treatment variable. To satisfy the first stage, unrestricted income must be explained by revenues generated from hydropower. This can be mathematically tested and is defined as:

$$Cov(UnrestrictedIncome_{mt}, HPRevenues_{mt}) \neq 0$$

Figure 10 displays the first-stages, namely the relationship between unrestricted income and revenues generated from hydropower, when controlling for covariates. From the figure, we clearly observe a positive relationship between unrestricted income and hydropower revenues for both samples, and the requirement can be considered fulfilled.

Figure 10 – First-stages: Unrestricted income by hydropower revenues



A) Restricted sample B) Extended sample

The second requirement is referred to as the exclusion restriction (Angrist and Pischke, 2008). This requirement consists of two parts, whereas the first part is that the instrument is randomly assigned. To satisfy this, we need an exogenous component of municipal revenues. As nature and available technology determine the location of hydropower plants, they constitute a source of exogenous variation (Haegeland et al., 2012). Thus, nature decides who has access to the immobile tax source. Hydropower technology was introduced about 100 years ago, and many of the power plants were established around that time. This timing of events therefore avoids the potential connection between the location of hydropower plants and characteristics improving labour market outcomes. Thus, the location of hydropower plants can be considered a natural experiment.

The second part of the exclusion restriction states that conditional on the controls in the model, the instrument has no effect on outcomes other than through the first stage channel (Angrist and Pischke, 2008). The treatment variable of interest must be the only channel through which the instrument affects the outcome. In this case, it implies that revenues generated from hydropower only affect earnings and social security benefits through unrestricted income. This is mathematically defined as:

$$Cov(HPRevenues_{mt}, v_{imt}) = 0$$

The requirement cannot be tested and a theoretical argument is needed for the requirement to be considered fulfilled. The exclusion restriction fails if earnings or received social security benefits of refugees in some way are affected by hydropower other than through the level of unrestricted income. The main argument is that the instrument captures geographic factors, and therefore it seems unlikely that the same geographical factors affect labour market outcomes. However, the presence of hydropower plants in a municipality can create additional jobs, which can affect the labour market conditions of the inhabitants in the municipality. Better labour market opportunities may cause more refugees to find a job and reduce the need for social security benefits. As there have been few new developments of hydropower plants in the studied time period, only the operation of the plant can affect the outcome variables. Hydropower is considered a capital-intensive industry and the typical professions of the employees are mostly engineers or persons with qualifications within machinery and/or electronics. Today's operation is largely automated and remote controlled, hence the number of jobs generated from hydropower plants is limited. In 2013,

the total hydropower industry in Norway had only 7 018 full-time equivalents (Multiconsult, 2015). Furthermore, we do not expect many refugees to have engineering background as the educational level of refugees is often lower. From Table 1 in the Data Description (Section 4), we observe that the majority have zero education. Furthermore, as hydropower is a very country specific resource, we do not expect engineers from the refugee countries to possess the specific skills relevant to work in the hydropower industry.

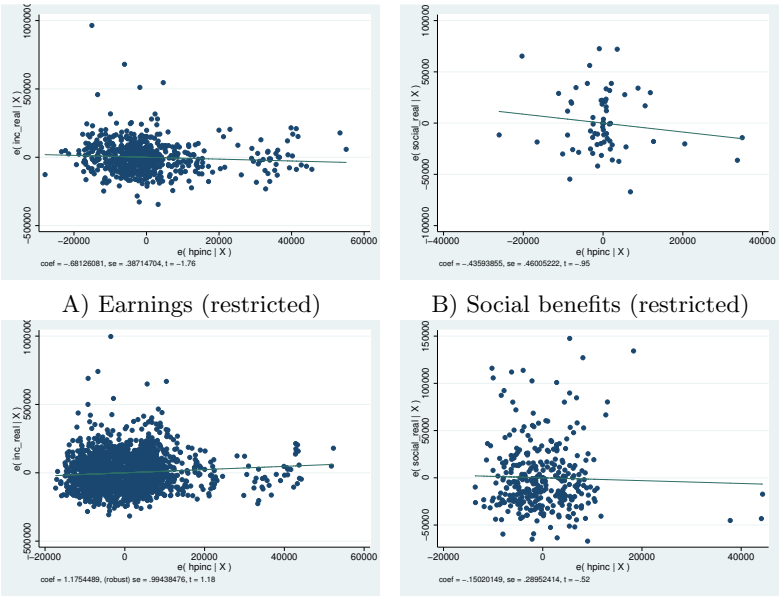
A concern in our case is that the sample consists of small municipalities in terms of the number of inhabitants. The hydropower industry is therefore likely to constitute a large share of the economy in some municipalities. As the average wage of engineers is above the average wage level in Norway, higher purchasing power of engineers may lead to increased spending in the municipality, given that the employees live or spend money in the same municipality as they work. The employment of engineers may therefore have second-round effects in terms of higher spending that can create additional demand for labour in other industries that are more likely to employ low-skilled workers. However, due to high capital-labour ratio in the hydropower industry, we expect this effect to be small and disregard this concern. In addition, Lillegård and Seierstad (2013) find that little of the variation between municipalities in Norway can be explained by local labour market conditions concerning the time from settlement to encountering the labour market. Based on the discussion above, we consider the exclusion restriction fulfilled.

The concept of the IV approach is apparent in the reduced-form specification:

$$(4) \qquad Y_{im} = \alpha + \beta_{10} HPRevenues_{mt} + \beta_{11} IndCharcs_{im} + \beta_{12} MunCtrl_{mt} + \varepsilon_{imt}$$

The reduced form illustrates the relationship between the instrument and the dependent variables, i.e, it is a regression of the earnings and social security benefits on the covariates and the instrument, displayed in Figure 11 (Angrist and Pischke, 2008). We observe a decreasing relationship between hydropower revenues and social security benefits, and a slightly decreasing relationship between hydropower revenues and earnings. For the extended sample, we observe an increase in earnings, and no relationship between hydropower revenues and social security benefits. Because revenues generated from hydropower is probably unrelated to earnings and social security benefits for refugees, it seems credible that this downward pattern in social security benefits, and the upward pattern in earnings, results from unrestricted income.

Figure 11 – Reduced forms for earnings and social security benefits



A) Earnings (restricted) B) Social benefits (restricted)

C) Earnings (extended) D) Social benefits (extended)

To measure a Local Average Treatment Effect (LATE) of β_4, the assumption of monotonicity must hold. The assumption implies that while the instrument may have no effect on some municipalities, all of those who are affected, are affected in the same way (Angrist and Pischke, 2014). In this case, it means that municipalities with high levels of hydropower revenues spend more on integration, as a result of the high levels of hydropower revenues. We do not have data on the direct expenses related to the integration process. However, since spending is restricted by income, it is reasonable to assume that municipalities with a more flexible budget spend more. Since the total integration expenses are found to exceed the received grants, we assume that the monotonicity assumption holds. As the first stage, the exclusion restriction, and the monotonicity assumptions are satisfied, IV estimates Local Average Treatment Effect (LATE) (Angrist and Pischke, 2008). LATE is the average effect of unrestricted income on earnings and social security benefits for those who live in municipalities with high levels of hydropower revenues.

5.2 Specifications

The structure of the data set can potentially pose issues that need to be addressed. First, we explain how we address the skill-cell structure by weighting the observations. Second, we describe how we use clustering to solve problems concerning intra-group correlation and serial correlation. Lastly, we provide a discussion concerning the random assignment of refugees to municipalities in order to avoid selection bias.

Weighting

The data on refugees is grouped in skill-cells. We know n_x, where $\frac{n_x}{N}$ is the relative frequency of x in the underlying sample. Thus, by weighting the regressions by n_x, we get the same result as if we duplicate each observation as many times as the number in each skill-cell, and then run the same regression without weighting by skill-cell size (Angrist and Pischke, 2008).

Clustering

Since each refugee is settled in a municipality, the standard errors of the regression coefficients may not satisfy the assumption that the errors are uncorrelated. The most important form of dependence arises in data with a group structure, such as in our case where the dependent variable of an individual, i, is observed within a municipality, m, which indexes the group (Angrist and Pischke, 2008). If we consider the simple model below, the problem that arises in the error term can be displayed:

$$Y_{im} = \alpha + \beta_1 x_m + \beta_2 z_{im} + \varepsilon_{im}$$
$$\varepsilon_{im} = u_m + v_{im}$$
$$i = 1, ..., I_m$$
$$m = 1, ..., M$$

The regressor of interest, x_m, varies only at the municipality level. u_m is the unobserved group effect which can impact the statistical inference and result in biased standard errors (Wooldridge, 2003). Another issue that arises is due to the time dimension of the municipality regressors. We might expect serial correlation in our data, meaning that the error term, ε_{imt}, might reflect idiosyncratic variation in outcomes that varies across time. For example, we expect the unemployment rate in Bykle in 2010 to be correlated with the unemployment rate in 2009 due to the economy being in a business cycle. Both intra-group correlation and serial correlation can be accounted for by clustering the standard errors. However, too few clusters might lead to underestimation of intra-group- or serial correlation. Angrist and Pischke (2008) suggest a minimum of 42 clusters for reliable inference. Thus, the restricted sample do not contain enough groups to apply the method and we only use clustering on the extended sample for the outcome variable earnings, that consists of 54 municipalities.

Random Settlement of Refugees

To measure the effect of municipalities' financial condition on the integration outcome of refugees, we need to ensure that refugees are randomly distributed across municipalities conditional on observable characteristics, to avoid selection bias. We use the information provided in the Background in Section 2 as a basis for discussion. In addition, part of the information is based on interviews with placement officers from IMDi, since there is very little documentation about the practical implementation of the settlement policy. The first issue posing a threat to the random distribution refers to refugees often being

assigned to a municipality close to the reception center where they are enrolled. However, the decision concerning placement to a reception center is considered random and often depends on available capacity (Edin et al., 2003). Moreover, reception centers are located all over Norway and the port of entry is not correlated with the location of the center. Given these aspects, we do not consider this a threat to the desired randomness. A second issue that may cause selection bias refers to settlement requests. Refugees cannot choose their first place of residence and there is no interaction between municipal officers and the refugees prior to settlement. However, when the application for asylum is approved, refugees are given an interview to identify their background, current situation, and requests for settlement, if any. Examples of settlement requests are the need for special arrangements, such as hospital treatment or family reunification. Since children and youths under 20 years are excluded from the data set, the specific settlement of refugees due to family reunification is to some extent limited and women are mostly our concern. Furthermore, the municipal assignment by placement officers is mostly based on characteristics such as country of origin, family size, and education. These are observable characteristics in which we can control for in our model. However, special arrangements can be hard to control for, a point we will address in Section 7, concerning the limitations of the data. Based on this discussion, we assume that the municipal assignment is exogenous with respect to the random components of the outcome variables, conditional on observed characteristics, in line with the paper by Åslund et al. (2011). This provides the basis for a natural experiment where the municipal settlement is considered exogenous.

6 Results

In the following section, we present the main findings, followed by a sensitivity analysis. In the main analysis, we divide the samples by gender and educational level in order to check if there are demographic differences between the subgroups, as research has shown that educational level may have an impact on earnings (Angrist and Kruger, 1991). Refugees with education ranging from zero to eight years are grouped in the low education category. Thus, we assume that the group possesses low to no education qualifications, whereas those with the highest number of educational years undertaken within the group is equivalent to one year above completion of Norwegian primary school. As displayed in the descriptive statistics in Table 1, the majority within this group have no education and might have more difficulties transitioning to the labour market than those with several years of basic education. The high education group is equivalent to Norwegian secondary school or higher, and might more easily transit to the labour market. Some might possess a formal degree beyond high school, but this information is not provided in the data.

6.1 Main Results

First, we present estimates of earnings for both samples. Estimates of social security benefits are not displayed for the restricted sample, due to lack of observations. The outcome variables are assumed to depend on observed individual characteristics.[14] The municipality control variables, average earnings, and unemployment rate, are also included in the model as we assume that municipal conditions can affect earnings and/or employment status, and thereby the need for social security benefits.

Earnings

Table 2 displays the regressions results for the OLS, first stage, reduced form, and IV for the restricted sample, which are all significant. The significant first stage implies that hydropower revenues have a clear effect on unrestricted income. The first stage displays that an increase in hydropower revenues by 1 000 NOK per capita, is estimated to increase unrestricted income by 440 NOK per capita. According to the IV estimate in column (4) in the first part of the table, the coefficient is negative and suggests that an additional 1 000 NOK per capita of unrestricted income decreases earnings by 1 530 NOK. Dividing the sample according to education reveals that unrestricted income has a negative effect on earnings for refugees with high education, significant at a 5 % level. By means of gender, unrestricted income has a negative effect on earnings for males, significant at a 1 % level. The results suggest that the level of unrestricted income seems to affect refugees' earnings negatively. We note that the standard errors are

[14] The individual control variables are gender, children, marital status, country of origin, age at settlement, number of years in Norway post-settlement, and educational level.

quite high, thus the precision of the estimates can be questioned. Furthermore, the number of refugees is quite small, as only 419 refugees were settled during the time period of 2005 - 2010. Thus, caution is required in the interpretation of the variables.

Table 2 – Dependent variable: Earnings (restricted sample)

	(1) OLS	(2) First Stage	(3) Reduced Form	(4) IV
Unrestricted income	-1.98***			-1.53*
	(0.72)			(0.90)
Hydropower revenues		0.44***	-0.68*	
		(0.01)	(0.39)	
Individual control variables	Yes	Yes	Yes	Yes
Municipality control variables	Yes	Yes	Yes	Yes
Observations	615	979	615	615
R^2	0.37	0.78	0.37	0.37
	Sample divided by educational level and gender			
	(1) High Educ.	(2) Low Educ.	(3) Female	(4) Male
Unrestricted income	-4.19**	-0.42	-0.03	-3.05***
	(1.85)	(0.86)	(1.04)	(1.18)
Individual control variables	Yes	Yes	Yes	Yes
Municipality control variables	Yes	Yes	Yes	Yes
Observations	254	361	321	294
R^2	0.43	0.37	0.31	0.48

Standard errors are shown in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Note: As there are few municipalities, the standard errors are not clustered. We could potentially have applied a block bootstrap. However, bootstrap is not meant to be used with weighted calculations (Corporation, 2001). The individual control variables are gender, children, martial status, country of origin, age at settlement, number of years in Norway post settlement, and educational level. The municipality control variables are unemployment rate and average earnings. An explanation of the control variables is provided in Appendix A3.

Table 3 displays the regression results for the extended sample. The first stage is statistically significant at a 1 % level. Increasing hydropower revenues by 1 000 NOK per capita results in an additional 350 NOK per capita of unrestricted income. The marginal effect of revenues from hydropower on unrestricted income only drops from 0.44 to 0.35 when we extend the sample. This implies that our instrument is still powerful and that the effect not only applies to the municipalities that are extremely wealthy from hydropower. According to the main IV estimate, hereby denoted baseline estimate, the effect of unrestricted income on earnings is positive. However, the effect is insignificant, meaning that higher levels of unrestricted income do not seem to have an effect on refugees' earnings.

Table 3 – Dependent variable: Earnings

	(1) OLS	(2) First Stage	(3) Reduced Form	(4) IV
Unrestricted income	-0.20			3.39
	(0.89)			(2.98)
Hydropower revenues		0.35***	1.18	
		(0.06)	(0.99)	
Individual control variables	Yes	Yes	Yes	Yes
Municipality control variables	Yes	Yes	Yes	Yes
Observations	2533	4327	2533	2533
Number of Clusters	53	54	53	53
R^2	0.25	0.40	0.26	0.23

Standard errors are clustered on municipality level, and are shown in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Note: The individual control variables are gender, children, martial status, country of origin, age at settlement, number of years in Norway post settlement, and educational level. The municipality control variables are unemployment rate and average earnings. An explanation of the control variables is provided in Appendix A3.

Table 4 presents the IV regression results divided by education and gender. In terms of educational level, unrestricted income is positive, but statistically insignificant for both groups. In terms of gender, only the estimate for women is significant at a 10 % level, suggesting that an increase in unrestricted income of 1 000 NOK, results in additional earnings of 4 190 NOK. We will conduct a variety of robustness tests to check the sensitivity of these results in Section 6.2. Compared to the OLS estimates in Table 3, all the coefficients for IV estimates are substantially higher. This suggests that the OLS estimate has a downward bias. In terms of the control variables, the controls for gender, children and marital status are all significant.[15] The large magnitude of the coefficients for the control variable males is rather surprising. We observe that any number of years post-settlement are estimated to result in higher earnings than settlement lasting for less than a year.[16] This seems reasonable as refugees are enrolled in the introduction program the first years of settlement, aimed at providing the refugees with basic qualifications and host country specific skills (Justis- og beredskapsdepartementet, 2015-2016). Furthermore, all but one educational level affect earnings negatively for males, which is counter-intuitive based on research on the effect of education (Angrist and Kruger, 1991).

[15] An exception is refugees with low education where marital status is insignificant.

[16] An exception is refugees with low education where five years of settlement is insignificant.

Table 4 – Dependent variable: Earnings, divided by educational level and gender

	(1) High Education	(2) Low Education	(3) Female	(4) Male
Unrestricted income	5.69	2.36	4.19*	2.82
	(6.24)	(1.68)	(2.47)	(3.72)
Male	71027.59***	63674.51***		
	(15463.62)	(9478.21)		
Children	21175.14**	27162.93***	28610.07***	33885.57***
	(9123.70)	(3558.36)	(5003.16)	(4625.63)
Married	-41021.63***	5584.97	10650.04*	-31743.30***
	(12884.10)	(5028.66)	(6205.27)	(6956.84)
Age at settlement	-260.48	1386.63***	1345.74***	-125.98
	(896.70)	(469.61)	(485.95)	(872.14)
Years post settlement:				
1 year	30516.00**	55042.67***	56159.95***	43017.83***
	(12651.90)	(6373.32)	(5847.95)	(8068.80)
2 years	82078.41***	64776.66***	74833.05***	67195.04***
	(12515.46)	(8084.13)	(6365.31)	(9433.53)
3 years	96725.43***	82136.09***	84456.88***	94569.35***
	(14054.11)	(15028.49)	(13308.54)	(15854.21)
4 years	97419.01***	83923.50***	91579.97***	99773.52***
	(22591.50)	(18771.77)	(15057.75)	(19136.43)
5 years	116703.36***	-1830.32	113484.35***	108680.49**
	(27328.93)	(33870.19)	(21988.09)	(44454.83)
Education:				
5 - 8 years			-20388.50	-50087.04***
			(23641.81)	(17374.96)
9 - 11 years			21757.06*	-28517.64*
			(12253.66)	(15599.24)
12 - 14 years			32018.22	-36094.41
			(29655.60)	(30061.22)
$\geq$ 15 years			-26239.09	-57531.03***
			(19440.52)	(13533.70)
Country of origin dummies	Yes	Yes	Yes	Yes
Municipality control variables	Yes	Yes	Yes	Yes
Observations	990	1543	1339	1194
Number of Clusters	53	53	52	50
R^2	0.24	0.25	0.19	0.26

Standard errors are clustered on municipality level, and are shown in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Note: All individual control variables are included. The base group for number of years post settlement is zero years, and the base group for education is zero years. The municipality control variables are unemployment rate and average earnings.

Social Security Benefits

Table 5 displays that the OLS is negative and significant at a 5 % level. The first stage shows that the instrument is significant, indicating that revenues from hydropower have a strong positive effect on unrestricted income.[17] The IV baseline estimate is negative, but insignificant. According to the estimates in Table 6, unrestricted income does not seem to have an effect on social security benefits, regardless of educational level or gender. The signs of the estimated coefficients are negative for all specifications, except for males. The standard errors for all specifications constitute a large share of the estimates which may affect the precision of the estimates. The magnitude of the estimated coefficient for high education is substantial compared to the baseline estimate. However, there are only 56 observations in this subgroup. In terms of the the control variables, only age at settlement is significant for all subgroups.

Table 5 – Dependent variable: Social security benefits

	(1) OLS	(2) First Stage	(3) Reduced Form	(4) IV
Unrestricted income	-0.84**			-0.27
	(0.41)			(0.50)
Hydropower revenues		0.38***	-0.15	
		(0.01)	(0.29)	
Individual control variables	Yes	Yes	Yes	Yes
Municipality control variables	Yes	Yes	Yes	Yes
Observations	337	3146	337	337
R^2	0.19	0.48	0.18	0.19

Standard errors in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Note: As there are few municipalities, the standard errors are not clustered. We could potentially have applied a block bootstrap. However, bootstrap is not meant to be used with weighted calculations (Corporation, 2001). The individual control variables are gender, children, martial status, country of origin, age at settlement, number of years in Norway post settlement, and educational level. The municipality control variables are unemployment rate and average earnings. An explanation of the control variables is provided in Appendix A3.

[17] The first stage estimates for the analyses of earnings and social security benefits are slightly different, as observations for social security benefits are not displayed for the years of 2009 and 2010.

Table 6 – Dependent variable: Social security benefits, divided by educational level and gender

	(1) High Education	(2) Low Education	(3) Female	(4) Male
Unrestricted income	-8.38	-0.05	-0.61	1.94
	(5.43)	(0.49)	(0.56)	(1.60)
Male	-39449.74**	5105.34	0.00	0.00
	(15446.79)	(4627.26)		
Children	32334.91*	4020.79	408.80	6602.89
	(18060.63)	(4715.21)	(6456.60)	(6694.69)
Married	-19224.86	3796.25	5320.77	12843.07
	(18808.05)	(5580.38)	(6845.81)	(9323.60)
Age at settlement	2035.73**	1028.89***	1011.79**	1169.82***
	(988.10)	(306.39)	(466.31)	(399.00)
Years post settlement:				
1 year	15849.76	3542.32	4947.99	-1462.07
	(20081.88)	(5229.98)	(7212.82)	(9291.03)
2 years	-28725.67	-4415.60	-3051.34	-23948.48*
	(18726.20)	(7627.51)	(8770.98)	(12814.73)
3 years			12822.04	1854.28
			(12236.70)	(21294.72)
4 years				
5 years				
Education:				
5 - 8 years			-23199.29	8064.12
			(25234.42)	(17328.94)
9 - 11 years			22260.14**	-9286.02
			(9473.08)	(12174.78)
12 - 14 years			23830.88	-140.06
			(17762.86)	(19395.31)
$\geq$ 15 years			-17783.93	-46126.89***
			(14274.96)	(15741.88)
Country of origin dummies	Yes	Yes	Yes	Yes
Municipality control variables	Yes	Yes	Yes	Yes
Observations	56	281	174	163
R^2	0.29	0.17	0.27	0.19

Standard errors in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Note: As there are few municipalities, the standard errors are not clustered. We could potentially have applied a block bootstrap. However, bootstrap is not meant to be used with weighted calculations (Corporation, 2001). All individual control variables are included. The base group for number of years post settlement is zero years, and the base group for education is zero years. The municipality control variables are unemployment rate and average earnings.

6.2 Sensitivity Analyses

In this section, we perform various specification checks to test the sensitivity of our estimates. First, we test the sensitivity with respect to changes in the control variables, as this could expose potential weaknesses. Second, we test whether our results are robust to the exclusion of extreme outliers. Third, we perform the analyses for earnings and social security benefits in logs to see if the estimates vary with the functional form. Finally, we create a binary variable for earnings to test the extensive margin. All sensitivity analyses are performed with the extended sample only.[18]

Changing Control Variables

We aim to test how sensitive our estimates are to modifications of the regression specifications by including/excluding controls. In each regression specification, a few individual core controls are included.[19] The analysis for earnings in Appendix A4 displays that all estimates are insignificant, in line with the baseline estimate for earnings. However, the coefficient changes slightly when changing the control variables. This indicates that there is some heterogeneity in the model, which we control for in the baseline estimate. The baseline estimate changes from 3.39 to 4.12 when the municipality controls are excluded. Appendix A5 displays a sensitivity analysis for women's earnings. When municipal controls are excluded, the estimated coefficient becomes insignificant. Based on the change in significance level when excluding municipal controls, we are somewhat critical to the estimated effect on earnings for women. The analysis for social security benefits in Appendix A6, displays that all estimates are insignificant, in line with the baseline estimate. When municipal controls are excluded, the coefficient only changes with 0.002. This indicates that the result is robust, as the estimate is insensitive to changes in control variables when core control variables are included (Angrist and Pischke, 2014).

Excluding Extreme Observations

With few observations, regression estimates can be sensitive to the inclusion of one or several extreme observations (Wooldridge, 2009). Since the data mostly consist of binary and categorical variables of limited range, this foremost concerns the continuous variables in our analyses. We observe a few worrisome high values for earnings, which could possibly be explained by labour immigration, since immigrant status is not displayed in the data.[20] We must be careful not to exclude too many observations, so that the precision of our estimates are not compromised. Appendix A7 examines the robustness by subsequently excluding extreme values. Observations which are approximately three to four times the mean average

[18] The restricted sample lacks precision due to few observations.

[19] All regressions include the core control variables; country of origin, years post-settlement, gender, educational level, and age at settlement.

[20] Extreme values are only excluded in the analysis for earnings, as this issue do not apply for social security benefits.

earnings in the municipalities are excluded.[21] We exclude extreme observations for both the baseline estimate and the estimate for women. When extreme observations are excluded, the coefficients of unrestricted income are slightly greater than the baseline estimates, but not significant nor distinguishable. All estimates are within the confidence interval of both the baseline estimate and the estimate for women. The fact that our estimates do not change much, indicate that the baseline regression estimations are not driven by these observations. Overall, this analysis indicates that the baseline estimate, and the estimate for women, are robust to the exclusion of the extreme observations.

Different Functional Form

In the main analyses, we rely on a linear relationship between the dependent and independent variables. To test the sensitivity of our estimates, we perform the analyses with the dependent variables in logs.[22] This approach puts more weight on lower values, which can prove relevant for this study, as a goal of the integration policy is to achieve employment.[23] A problem that arises when taking logs of the dependent variables is the zero observations, since the natural logarithm of a product is only defined for values above zero (Wooldridge, 2009). This occurs for a number of observations in our data. We account for the problem in two different ways in order not to lose too many observations and check whether the results are independent of which method we use. First, we include zero values by transforming them into defined values of the natural logarithm (0.0001). Second, we exclude all zeros and therefor only the wage aspect of earnings and the value of the social security benefits are captured.

From Appendix A8, we observe that the estimates for earnings are significant at a 10 and 5 % level, respectively for inclusion and exclusion of zero values. The significance level is contrary to our baseline estimate. The estimate when zeros are included suggests that an increase of 1 000 NOK in unrestricted income, leads to a 0.013 % increase in earnings. For a refugee with an income equal to the low income limit of 113 000 NOK, this will increase earnings by 14 690 NOK.[24] The size of the coefficient is around half when zeros are excluded, which is natural in our case as the range of earnings becomes more limited.[25] The finding that unrestricted income does not seem to affect social security benefits still holds, regardless of method of analysis. As all our IV estimates for social security benefits are insignificant, the variable seems to be unaffected by unrestricted income.

[21] In total, 5 observations are excluded.

[22] We apply a different functional form for the baseline estimates only.

[23] Part-time jobs that generate low income levels are therefore for instance preferred over unemployment.

[24] See explanation of the calculation of the low income limit under the section of Extensive Margin.

[25] 93 zero values are excluded.

Extensive Margin

As part of our sensitivity analyses, we perform the analysis for the dependent binary variable earnings.[26] The method of applying a dependent binary variable in multiple regression is called a linear probability model (LPM), where the variable equals one if refugees are financially independent (Wooldridge, 2009). The coefficient of unrestricted income measures the predicted change in the probability of success, $Y_i = 1$, when the variable increases by one unit (1 NOK).[27] Since the model assumes linearity, it can predict probabilities outside the interval [0,1], which is a drawback of the model (Wooldridge, 2009). The motivation for testing earnings as a binary variable is that the extensive margin could capture the effect of unrestricted income on financial independence. We define a person as financially independent if the income is above the low income limit.[28] The results are displayed in Appendix A9. We find that the estimated coefficient for women diverges from the baseline estimate for the intensive margin, as the coefficient for the extensive margin is negative and insignificant. In contrast to the intensive margin estimate for males, the estimate of the extensive margin is significant.[29]

6.3 Summary of Results

Since the restricted sample contains very few observations compared to the extended sample, we find the IV estimates for the extended sample more reliable. The main finding for earnings is that the effect of unrestricted income is insignificant for all specifications, except women. The baseline estimate of earnings seems to be insensitive to the exclusion of extreme observations. However, the estimate seems to be slightly sensitive to changes in the control variables and the chosen functional form. For women's earnings, the effect of unrestricted income is positive and significant at a 10 % level. However, the sensitivity analyses reveal that the estimate is sensitive to changes in the control variables. The analyses of social security benefits indicate that an increase in the level of unrestricted income has no significant effect, regardless of gender and educational level. This finding seems robust to changes in the control variables and the functional form.

[26] If we define a dummy variable for social security benefits, in which the received value could be a substitute for income making refugees financially independent, we get few observations for $social(dummy) = 1$. Thus, we do not test the extensive variable for social security benefits.

[27] This is mathematically displayed as: $P(Y_i = 1|X_i) = \beta_i \Delta \widehat{Unrestricted Income}_{mt}$.

[28] The low income limit is often referred to as the subsistence level. A method commonly used is to define all persons that have net income per consumption unit less than 50 or 60 % of the median income, as belonging to a low income group (SSB, 2016b). The median income in Norway after tax, for individuals between 30 - 44 years, was 226 000 in 2005. This gives a lower income limit at 113 000 NOK given 50 % of the median income. As our data on refugees are adjusted for inflation with 2005 as base year, we use the median income from 2005.

[29] The probability of being financially independent is positively affected for males, at a 10 % level. The coefficient is marginal, as it only changes the probability of employment by 0.0000105.

7 Discussion

In this section, we provide a discussion of the main results from Section 6. Furthermore, implications of this study and proposals for further research are presented. Finally, we address some of the limitations in regards to the data and empirical approach.

7.1 Discussion of the Results

There are several aspects concerning the restricted sample that could potentially explain the contradictory estimates between the restricted and the extended sample. One reason might be the different size of the two samples, where the extended sample contains approximately four times the observations of the restricted sample. Therefore, the extended sample has higher statistical precision. Furthermore, since the restricted sample contains too few municipalities to impose clustered standard errors, the standard errors might display a downward bias. Another reason for the contradictory estimates might be the type of municipalities that are included in the two samples. Municipalities are very different along several dimensions, such as different economic, social, and geographic characteristics. Thus, there might be other inequalities beyond our selection criteria and included control variables. Examples are political party, distance to closest city, and business structure, which we do not control for given the scope of this study. For further research, these aspects could be interesting to include.

Another important aspect to address is the size of the estimated coefficients of unrestricted income. This can be discussed from an economic viewpoint by estimating the effect from an increase in unrestricted income that may seem economically intuitive. The annual growth in municipalities' unrestricted income in the studied time period is approximately 4 %.[30] Given the estimate for women's earnings, the additional unrestricted income of 1 500 NOK is estimated to increase earnings by 6 285 NOK.[31] For a woman with an income equal to the low income limit of 113 000 NOK, this is equal to a 5.6 % increase. For a woman with an income of equal to the median income of 226 000, the increase is 2.8 %. Thus, the magnitude of the coefficient may seem reasonable for higher income levels, but for income levels close to zero the real income effect may not seem economically intuitive. However, as the coefficient is sensitive to modifications, we are somewhat critical to the estimate.

The baseline estimate for earnings in the log-level model is positive, which is in line with the baseline estimate in levels for the extended sample.[32] In contrast to the model in levels, the coefficient is significant, indicating that an increase in unrestricted income of 1 NOK per capita, leads to 0.013 % increase in

[30] Calculated from the data obtained from SSB, adjusted for inflation.

[31] Given that the average level of unrestricted income per capita for all municipalities in the time period is 37 863 NOK (see descriptive statistics in Table 1). This translates into an annual increase of approximately 1 500 NOK per capita.

[32] In the discussion we prefer the log-level model where zeros are transformed to 0.0001, as it is important to include refugees with zero earnings.

earnings. This implies that an increase in unrestricted income of 1 500 NOK leads to an increase in earnings of 22 035 NOK, given the low income limit, and 44 070 NOK, given the median income. For very low income levels, the log-level model seems more economically intuitive, but for higher income levels, the effect does not seem economically plausible. The significance level and the functional form for the estimates for the full sample diverge depending on the functional form. Based on the ambiguous results, we believe that we cannot say whether unrestricted income has a true effect on earnings.

In terms of social security benefits, the sample consists of few observations which may affect the precision of the estimates. Both the signs of the OLS and the IV are negative, but only the OLS is statistically significant. The significance level indicates that the choice between an OLS and IV is decisive for the estimated results. Few of the control variables included are significant and the model could potentially be improved by including controls for health indicators, such as disabilities, mental health, depression, and other illnesses. The IV estimate indicates that an increase in the level of unrestricted income has no significant effect on social security benefits, regardless of gender and educational level. The effect on social security benefits is insensitive to changes in controls, and the significance level does not depend on the functional form. These results indicate that the IV estimate is robust, which support our baseline estimate.

7.2 Implications

Literature on the abundance of natural resources argue that high public revenues derived from natural resources have negative effects for economic efficiency (Borge and Torvik, 2015).[33] For the analysis of earnings using the restricted sample, the negative coefficient of interest is in line with this research. However, based on the discussion above, there are several critical aspects concerning the restricted sample which indicate that caution is required in the interpretation. Johansson (2002) shows that labor market programs have relatively large and positive effects on labor force participation in Sweden.[34] Interestingly, Appendix A1 indicates that hydropower municipalities on average spend more on labour schemes and social services. The expenses for labour schemes and social services are respectively three times and twice as high in the hydropower municipalities than the comparable. In accordance to Johansson (2002), we would therefore expect unrestricted income to have implications on earnings and social security benefits. However, as our main results concerning the effect on earnings are somewhat contradictory and sensitive to various specification checks, it leaves us uncertain about the true effect, if any. In terms of social security benefits, we do not find evidence that refugees settled in municipalities with a better financial condition, receive lower/higher levels of social security benefits. This suggests that more research in this field is necessary to obtain knowledge of how economic resources affect integration outcome, which can

[33] The paradox is often referred to as the resource curse.

[34] This paper does not concern refugees, but we assume that the results are to some extent transferable, as municipalities have economic incentives to engage refugees in the labour market.

have implications for policy design.

7.3 Further Research

Lack of successful integration may imply large socioeconomic costs to the Norwegian society. Thus, a proposal for further research is to measure the effect of the introduction program by applying a fuzzy regression discontinuity (RD) design. An RD could potentially address whether individuals settled in municipalities after the implementation of the introduction program, significantly differ in terms of integration outcome from those who arrived prior. The running variable would be time, and the year of implementation would be the cutoff. This research design could obtain the LATE of the introduction program. Quantifying a possible effect would be helpful for assessment and development of integration and settlement policies. Furthermore, research concerning the introduction programs' effect on children could provide useful information. Differences in birth weight and/or test scores between children with parents with different enrollment status could potentially show the effect of the program on children.

7.4 Limitations of the Data

The first issue concerns that different groups of refugees can not be identified in the data, as highlighted in Section 4.1 in the Data Description. This has two main implications for our analyses. First, refugees that immigrate due to family reunification purposes or need special arrangements, are placed in specific municipalities, making the distribution of refugees between municipalities less random. Second, from Section 2.1, we know that 90 % of refugees in Norway rely on governmental settlement, which ensures that the refugees are given the same treatment in terms of enrollment in the introduction program (Justis- og beredskapsdepartementet, 2015-2016). On average, we therefore expect 10 % of the individuals displayed in the data not to be given this treatment as self-settlement is chosen. This may impact our analyses, as some refugees deliberately choose to settle in certain municipalities. However, this is not assumed to have major implications for our analyses as our samples mainly consists of small inland municipalities, and self-settlement mostly occurs to the big cities.

A second limitation of the data is that it only displays the year of settlement in the initial municipality and not the year of arrival in Norway. The year of arrival may differ from the year of settlement as the time from the processing of the asylum application to actual settlement can take from six months to two years. Research shows that the length of the asylum process affects the success of integration in terms of decreasing unemployment (Hainmueller et al., 2016). As the data lack information concerning this issue, we can not control for such differences, which can be considered a weakness.

A third limitation is that we are not able to track refugees over time, due to the lack of complete panel

data structure concerning refugees. Given this structure, it is not possible to control for unobserved, but fixed omitted variables. As we do not have a complete time dimension on the data on refugees, unobserved abilities could potentially lead to biased estimates. We do have a time dimension in the data at municipality level. However, introducing a fixed effects component, such as efficiency, institutional setup etc., by using a fixed effects estimator would eliminate our identification strategy.

A fourth limitation concerns missing observations in the refugee data set, causing less precision. Whether this poses a threat to internal validity depends on the reason why the data is missing (Stock, 2011). When the reason for missing observations is completely random, the result is reduced sample size, which reduces the statistical power. This mostly concerns the outcome variable social security benefits, where 89 % of the observations are missing. However, missing observations do not necessarily produce biased estimates as long as the missing data is not a function of the outcome variable (Stock, 2011). This can be problematic if there is a systematic tendency that certain municipalities for some reason do not report data. However, 52 of the 54 municipalities in the extended sample are still represented despite the lack of observations for this variable.[35]

The last limitation concerns the data for the instrument. Hydropower revenues include revenues from concession power, which depends on both the supply and demand side of the electricity market (LVK, 2016). The value of the concession power comes from the two sources electricity price and volume. The profit the municipalities receive per kWh for the concession power depends mostly on the market price of electricity, which is affected by supply side factors such as wind and rain, and demand side factors such as temperature. As the price is affected by geographical factors, it tends to fluctuate. The volume of received concession power is given by a share of the hydropower plants' annual average production, which is up to 10 % (THEMA Consulting Group, 2011). The level of production depends on the supply side factors mentioned above. Wind and rain may create local and seasonal differences between municipalities that we do not control for. The model could therefore be improved by including such control variables. However, since our data consists of annual observations, seasonal variation can be difficult to capture.

7.5 Limitations of the Empirical Approach

A first limitation concerns the potential problem of treatment migration, i.e. when individuals in the treatment or control group (hydropower municipalities and non-hydropower municipalities) change their status (Angrist and Pischke, 2014). This could be the case if refugees move from a non-hydropower municipality to a hydropower municipality, or the opposite. These scenarios are potential threats to the validity of our empirical approach. However, we can argue that this is not of great concern. The two samples consist of small inland municipalities that are considered similar along several dimensions. Thus, we find it more likely that the refugees that move, is more likely to move to a different type of

[35]STATA automatically applies a list-wise deletion, i.e., only observations with data on each variable is included.

municipality, for instance to larger cities. Thus, we do not expect treatment migration to be a major concern.

A second limitation concerns the validity of our estimates. There is a distinction between internal validity and external validity (Angrist and Pischke, 2008). By definition, a good instrument captures an internally valid causal effect. This is the causal effect on the group subject to experimental manipulation, i.e., those living in hydropower municipalities. External validity is the predictive value of internally valid causal estimates in context beyond those generating the estimate. In this case, we look at municipalities with distinct features. It would be hard to argue that these municipalities are comparable with larger municipalities and cities. Thus, our results are not predictive for other types of municipalities, which is a weakness with our approach. Even though our results are not predictive for different types of municipalities, we find this research question highly relevant as Norway consists of many small municipalities, and the fact that the current settlement policy of refugees relies on the principle that all municipalities participate in the settlement policy in order to avoid enclaves and reduce the pressure on central municipalities (Hainmueller et al., 2016).

The last limitation concerns the location of refugees, i.e., that there is only the initial location that is randomly assigned, given observable characteristics. As refugees are free to move, the initial location might not be consistent with the actual location. This can introduce a measurement error in the actual settlement as our main source of identification comes from individuals who remain in the assigned municipality. Edin et al. (2003) found that the majority stayed on in the assigned municipality. Thus, the instrument (initial location) had predictive power in the first-stage regressions (Edin et al., 2003). Furthermore, few refugees are found to move from the initial municipality in Norway (Lillegård and Seierstad, 2013). Of approximately 4 700 refugees that were settled in 2005, 73 % still lived in the initial municipality five years after settlement (Høydahl, 2011). Based on the low degree of secondary migration from the initial municipality and empirical research, we find it reasonable to assume that the initial location represents the present location.

8 Conclusion

The main purpose of this paper has been to empirically address the question whether municipalities' financial condition affects the integration outcome of refugees. Differences in municipalities' financial condition may impact the resources devoted to integration, as received grants on average do not cover the total integration expenses. We measure integration by the outcome variables earnings and social security benefits. The time period studied is 2005 - 2010 as refugees arriving in this period were exposed to the same treatment concerning the settlement policy and enrollment in the two-year introduction program. By relying on an instrumental variable approach, we exploit exogenous variation in tax revenues between municipalities to identify the effects of the level of unrestricted income on integration outcome of refugees. Hydropower revenues are used as an instrument, as nature and available technology determines the location of hydropower plants. Based on hydropower revenues, we select municipalities with high income levels generated from hydropower. To remove systematic differences, we construct a counterfactual from neighboring municipalities with little or no hydropower revenues. By controlling for basic observable characteristics, municipal assignment is exogenous with respect to the random components of the outcome variables, conditional on observed characteristics. In principle, we compare refugees in comparable municipalities that differ only in terms of hydropower revenues. This allows us to estimate how different levels of unrestricted income affects earnings and social security benefits.

Our empirical analyses in regards to earnings suggest that unrestricted income has a significant effect only on women's earnings, at a 10 % level. However, the significance level is sensitive to changes in the control variables. For the baseline estimate of earnings, unrestricted income is insignificant, and the estimate is sensitive to various specification checks. These results suggest that we cannot say whether unrestricted income has an effect on earnings. For social security benefits, we do not find evidence that unrestricted income has an effect, as estimates for all specifications are insignificant. The baseline estimate seems robust, as it is insensitive to modifications in the model. The findings of this paper suggest that more research in this field is necessary to obtain knowledge of how economic resources affect integration outcome, which can have implications for policy design.

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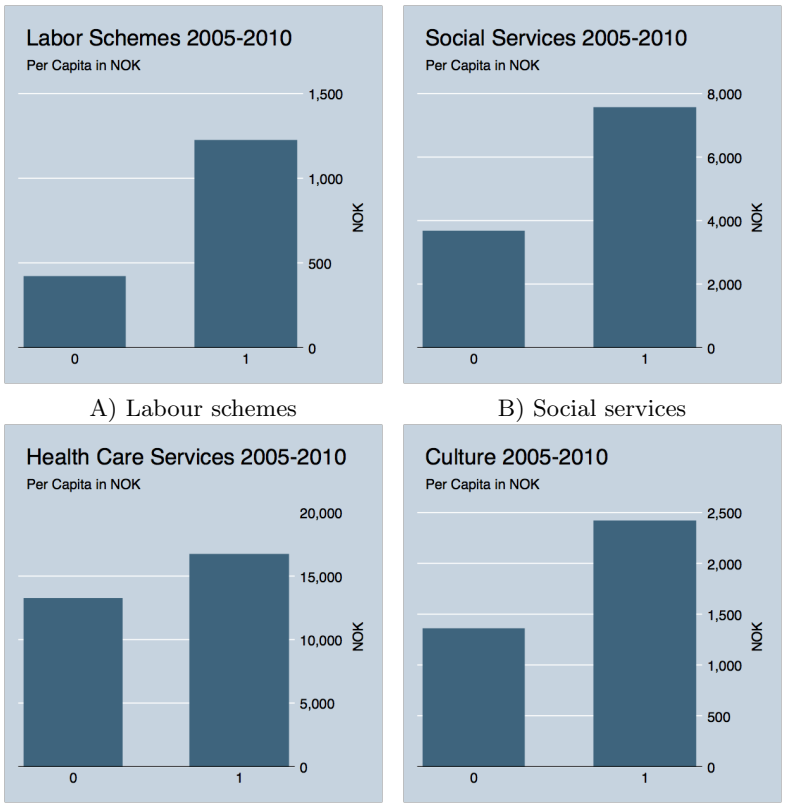
10 Appendix

Table A1 – List of acronyms

Acronyms	
BLD	Ministry of Children, Equality and Social Inclusion
FN	United Nations
IMDi	Directorate of Integration and Diversity
IV	Instrumental Variable
JD	The Ministry of Justice and Public Security
KMD	Ministry of Local Government and Modernisation
KS	Local Government Organisation
LATE	Local Average Treatment Effect
LPM	Linear Probability Model
LVK	Association of Hydropower Municipalities
NAV	Norwegian Labour and Welfare Administration
NVE	Norwegian Water Resources and Energy Directorate
OED	Ministry of Petroleum and Energy
OLS	Ordinary Least Squares
RD	Regression Discontinuity
SSB	Statistics Norway
TBU	Technical Reporting Committee
UDI	Norwegian Directorate of Immigration

Figure A1 – Local public services expenses (extended sample)

Source: KOSTRA database, SSB



A) Labour schemes B) Social services

C) Health services D) Cultural activities

Note: The expenses are displayed as average net expenses per capita for the time period of 2005 - 2010, to match the time period we study. The numbers are adjusted for inflation with 2005 as a base year. There is a systematic tendency that the expenses in the hydropower municipalities exceed expenses in the comparable municipalities each year, and thus the figure displays the average. The expenses for labour schemes are three times as high and twice as high for social services. Hydropower municipalities spend 20 % more on health services and 40 % more on culture.

Table A2 – List of selected countries

Refugee countries of origin	
Afghanistan	Iraq
Bosnia-Herzegovina	Kosovo
Burnudi	Liberia
Chile	Myanmar
China	Russia
Congo (earlier Zarie)	Serbia - Montenegro
Eritrea	Somalia
Ethiopia	Sri Lanka
Iran	Sudan

Figure A2 – Number of refugee settlements by country of origin

Source: Registry data, SSB

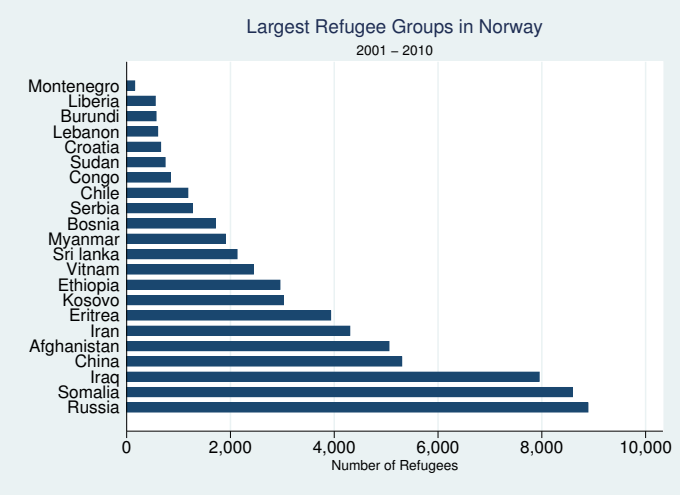
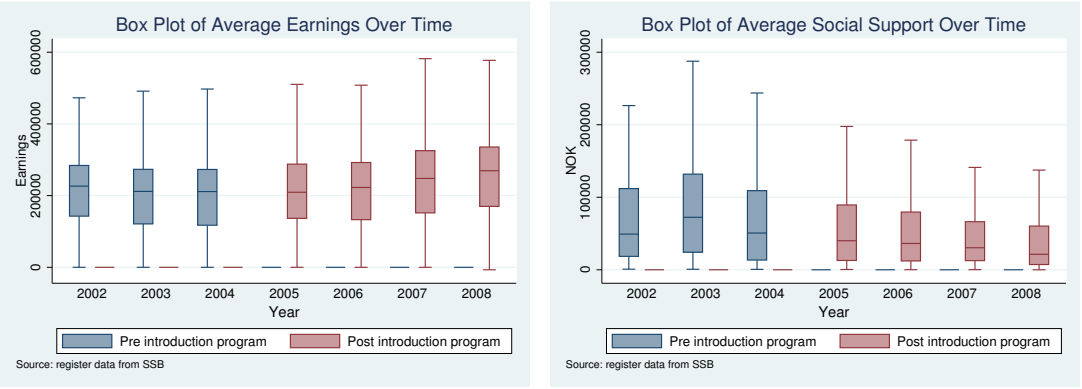
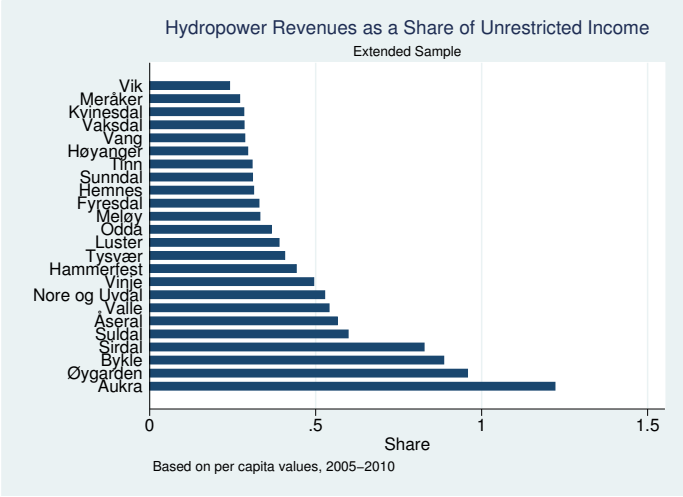


Figure A3 – Box plots for the outcome variables



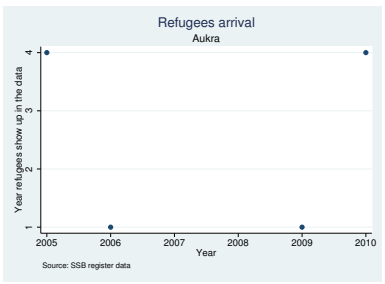
A) Earnings B) Social security benefits

Figure A4 – Hydropower revenues as a share of unrestricted income

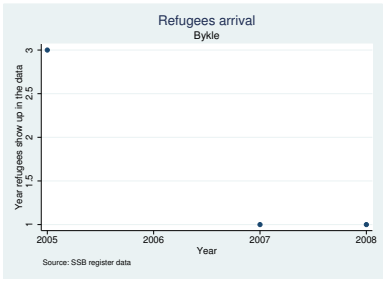


Source: TBU (2008) and SSB

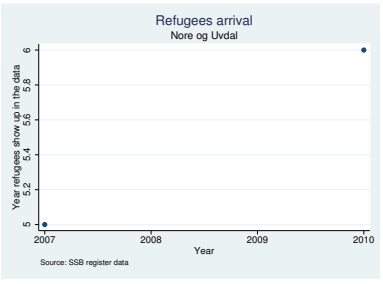
Figure A5 – Number of refugee settlements in hydropower municipalities (restricted sample)



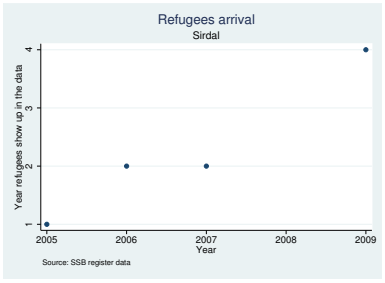
Aukra



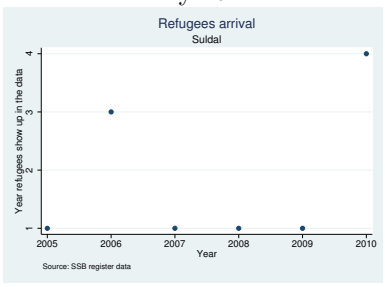
Bykle



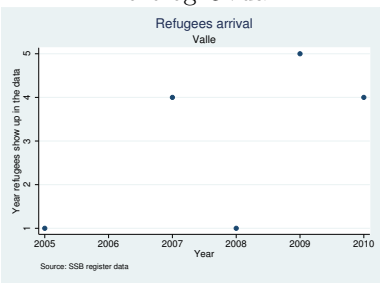
Nore og Uvdal



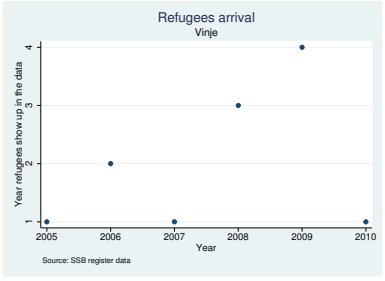
Sirdal



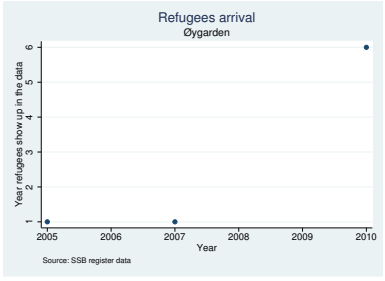
Suldal



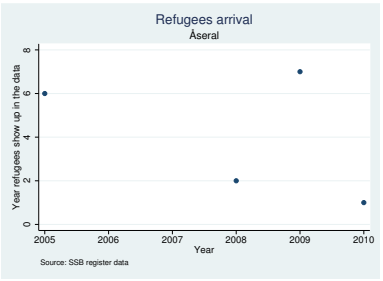
Valle



Vinje

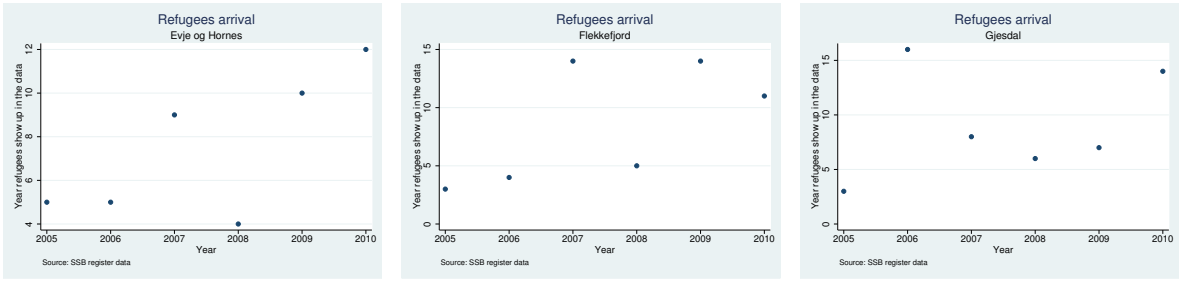


Øygarden



Åseral

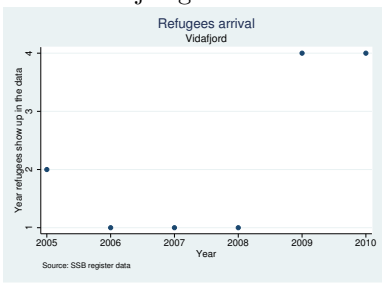
Figure A6 – Number of refugee settlements in comparable municipalities (restricted sample)



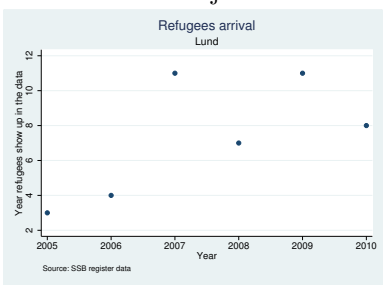
Evje og Hornes

Flekkefjord

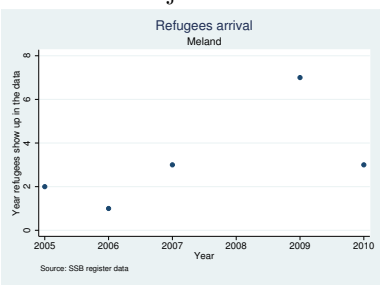
Gjesdal



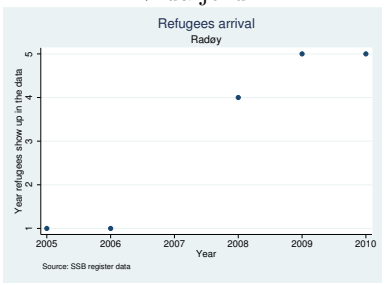
Vidafjord



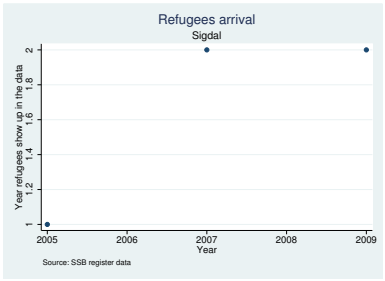
Lund



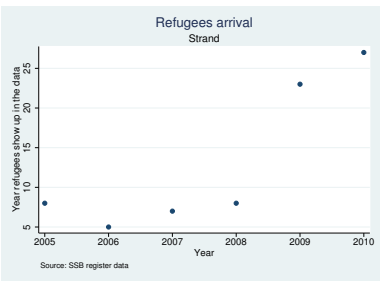
Meland



Radøy



Sigdal



Strand

Table A3 – List of control variables

	Description of control variables
	Individual characteristics
Age at settlement	Displays the age of the individual at the time of settlement in Norway. The variable is calculated by subtracting the year of birth from year of settlement. Since the exact birth date is not displayed, all individuals within the same year of birth and year of settlement are given the same age
Years post settlement	Displays the number of years post settlement. It is constructed by subtracting the year of settlement from the year of the observation. As we use the time time period 2005 - 2010, the values ranges from zero to five. We expect earnings to increase and/or greater likelihood of becoming employed over time.
Gender, martial status, and children	The variables are measured by means of dummies respectively reflecting the gender of the refugee displayed by male equal to one, whether an individual is married, or have young children in the household.
Education level	The level of education is classified into categories ranging from 1 to 5. Category 1 is equal to zero education, category 2 represents education between five and eight years, category 3 between nine and 11 years, and category 4 between 12 and 14 years. Category 5 represents education of 15 years or more and is equal to higher education.
Country of origin dummies	In order to control for systematic differences between individuals from the different countries, dummies for each country are created.
	Municipal characteristics
Average earnings	The variable displays annual average earnings in real terms as we adjust it according to CPI suing 2005 as a base year. Thus, it is a measure of the inhabitant's purchasing power. Labour market opportunities may vary between municipalities, creating variation in earnings. Earnings affect the tax base of the municipalities and thus unrestricted income.
Unemployment rate	The variable is represented by yearly data on municipal level from the Labour Force survey by SSB. As we use data from the time period prior and during the financial crisis, we expect variations in the unemployment rate both over time and across municipalities. Furthermore, we expect a higher transition to the labour market for refugees that are settled in municipalities with low unemployment rate.

Table A4 – Regression sensitivity analysis: Earnings

	(1) Core controls	(2) Individual controls	(3) Core controls Municipal controls	(4) IV Main result
Unrestricted Income	3.64	4.12	3.05	3.39
	(4.08)	(4.13)	(3.04)	(2.98)
Observations	2533	2533	2533	2533
Number of Clusters	53	53	53	53
R^2	0.20	0.21	0.23	0.23

Standard errors are clustered on municipality level, and are shown in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Note: All regressions include the core control variables: Country of origin, years post settlement, gender, educational level and age. Model (1) contains the core variable only. Model (2) contains children and marital status in addition to core controls. Model (3) include the municipal controls (unemployment and average earnings) in addition to the core variables. Model (4) is the baseline IV estimate, where core controls, age and marital status as well as municipality controls are included.

Table A5 – Regression sensitivity analysis: Earnings (females)

	(1) Core controls	(2) Individual controls	(3) Core controls Municipal controls	(4) IV Main result
Unrestricted Income	4.51	4.49	4.26*	4.19*
	(3.14)	(3.17)	(2.46)	(2.47)
Observations	1339	1339	1339	1339
Number of Clusters	52	52	52	52
R^2	0.18	0.19	0.19	0.19

Standard errors are clustered on municipality level, and are shown in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

All regressions include the core control variables: Country of origin, years post settlement, gender, educational level and age. Model (1) contains the core variable only. Model (2) contains children and marital status in addition to core controls. Model (3) include the municipal controls (unemployment and average earnings) in addition to the core variables. Model (4) is the baseline IV estimate, where core controls, age and marital status as well as municipality controls are included.

Table A6 – Regression sensitivity analysis: Social security benefits

	(1) Core Controls	(2) Individual controls	(3) Core controls Municipal controls	(4) IV Main result
Unrestricted Income	-0.25	-0.29	-0.24	-0.27
	(0.53)	(0.53)	(0.51)	(0.50)
Observations	337	337	337	337
R^2	0.17	0.17	0.18	0.19

Standard errors in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Note: All regressions include the core control variables: Country of origin, years post settlement, gender, educational level and age. Model (1) contains the core variable only. Model (2) contains children and marital status in addition to core controls. Model (3) include the municipal controls (unemployment and average earnings) in addition to the core variables. Model (4) is the baseline IV estimate, where core controls, age and marital status as well as municipality controls are included. As there are few municipalities, the standard errors are not clustered. We could potentially have applied a block bootstrap. However, bootstrap is not meant to be used with weighted calculations (Corporation, 2001).

Table A7 – Dependent variable: Earnings, outliers subsequently excluded

	Whole sample			Female		
	(1)	(2)	(3)	(4)	(5)	(6)
Unrestricted income	3.39	3.53	3.46	4.19*	4.30*	4.30*
	(2.98)	(2.96)	(2.86)	(2.47)	(2.44)	(2.44)
Individual control variables	Yes	Yes	Yes	Yes	Yes	Yes
Municipality control variables	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2533	2531	2528	1339	1338	1338
Number of Clusters	53	53	53	52	52	52
R^2	0.23	0.24	0.24	0.19	0.20	0.20

Standard errors are clustered on municipality level, and are shown in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Note: The individual control variables are gender, children, martial status, country of origin, age at settlement, number of years in Norway post settlement, and educational level. The municipality control variables are unemployment rate and average earnings. Model (1) and (5) contain the baseline estimates for the IV for the full sample and females. Model (2) and (6) exclude observations for earnings above 1 000 000 NOK, model (3) and (7) above 800 000 NOK.

Table A8 – Dependent variables: Earnings and social security benefits (logs)

	Earnings		Social security benefits	
	(1)	(2)	(3)	(4)
Zero values	Included	Excluded	Included	Excluded
Unrestricted income	0.00013*	0.00006**	0.00000	-0.00001
	(0.00)	(0.00)	(0.00)	(0.00)
Individual control variables	Yes	Yes	Yes	Yes
Municipality control variables	Yes	Yes	Yes	Yes
Observations	2533	2440	337	336
Number of Clusters	antall	antall		
R^2	0.08	0.18	0.11	0.16

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Note: Standard errors are only clustered for earnings. The individual control variables are gender, children, martial status, country of origin, age at settlement, number of years in Norway post settlement, and educational level. The municipality control variables are unemployment rate and average earnings. An explanation of the control variables is provided in Appendix A3.

Table A9 – Dependent variable: Earnings (extensive margin)

	(1)	(2)	(3)	(4)	(5)
	IV	High Educ.	Low Educ.	Female	Male
Unrestricted income	0.00	0.00	0.00	-0.00	0.00*
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Individual control variables	Yes	Yes	Yes	Yes	Yes
Municipality control variables	Yes	Yes	Yes	Yes	Yes
Observations	4327	1764	2563	2335	1992
Number of Clusters	54	54	53	54	53
R^2	0.05	0.03	0.11	0.07	0.05

Standard errors are clustered on municipality level, and are shown in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Note: The individual control variables are gender, children, martial status, country of origin, age at settlement, number of years in Norway post settlement, and educational level. The municipality control variables are unemployment rate and average earnings. An explanation of the control variables is provided in Appendix A3.