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Limited time or secure residence? A study on the short-term effects of temporary and permanent residence permits on labour market participation


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Limited time or secure residence?
A study on the short-term effects of temporary and permanent residence permits on labour market participation*

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December 19, 2018

Abstract

In this study we exploit a sudden policy change implemented in Sweden in order to evaluate the effects of permanent residency on labour market participation. In short, the policy change implied that Syrians were granted permanent instead of temporary residency as before the new regulations. Using detailed Swedish registry data, we examine the effect of the introduction of permanent residency on three measures of labour market inclusion in the short-term. We analyze the data through a simple difference-in-means as well as through comparison to groups unaffected by the policy in a difference-in-differences design and a synthetic control group approach. Our conclusions are twofold. On the one hand, we conclude that temporary residents that are subject to a relatively less-inclusive situation earn more and are unemployed less. However, at the same time, they are less likely to spend time in education than are those with permanent residency.

Keywords: Labor Market Inclusion, Asylum Policy, Sweden, Residence Permits
JEL Classifications: J15; J24; O15

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1 Introduction

Humanitarian crises throughout the Middle East, South Asia, and Africa have greatly increased immigrant inflows to Europe in recent years which has subsequently impacted upon the politics of European recipient nations. Anti-immigration parties have grown – largely on the basis of ethnocentric and nationalistic rhetoric – and a large debate has arisen as how best to integrate those individuals that are eligible for asylum in host countries into the labour market and wider society.

In recent decades, there has been a shift in migration politics across European countries. This shift has frequently been labeled as a convergence towards increasingly assimilatory politics (Joppke, 2004, 2017) and a retrenchment of multiculturalism (Vertovec et al., 2010). From a theoretical standpoint it has been noted that the shift has decreased emphasis on individual rights for a greater insistence on individual responsibilities (Borevi, 2010). In short, responsibilities-based models argue that individuals need to make an effort to belong, whereas rights-based models perceive individual rights as the driver of inclusion. Of particular focus has been residency status. Responsibilities-based models argue that migrants should be given short-term permits subject to re-evaluation for which only those that succeed in integrating into the labour market should be given right to stay. Rights-based models on the other hand argue that migrants should be given permanent residency, and that this right to stay should not be conditional on labour market inclusion. This inclusion is in turn intended to improve migrants’ possibilities to integrate into the labour market and wider society. However, the implications of residency status on labour market inclusion are largely unknown. The research question this paper addresses is thus: *What is the effect of permanent residency status on labour market inclusion?*

Looking at the current state of literature, there are a number of works that study *mandatory integration* which base residency or citizenship on language skills, norm adherence, and cultural-historical knowledge shown by the migrant (Strik et al., 2010; Goodman and Wright, 2015). Other lines of research focus on the impact of residency status on non-labour outcomes such as health or psychological well-being (Ryan et al., 2008; Bogic et al., 2015; Bakker et al., 2014), or have compared labour market outcomes across different nation-states (Koopmans, 2010; Kogan, 2007, 2006; Mansouri et al., 2010; Kesler, 2006; Bevelander and Pendakur, 2014). Although providing valuable insights, comparisons between nation-states are problematic as the included cases may differ along other dimensions, for instance, internal migrant populations, institutional setups, pre-existing

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1Mandatory integration policies demand acquisition of citizen-like skills, such as host country language, norms, culture, or history, in order to become a full member. See Goodman and Wright (2015) for a detailed description.
immigrant networks, or reception policies, all which may obstruct causal inference. Two recent contributions study the effect of residency permit explicitly, but have led to mixed conclusions (Larsen et al., 2018; Blomqvist et al., 2018). Furthermore, these studies include groups that largely consist of refugee-status migrants which limits the scope of the treatment – residency permit – as the factors that define refugee-status are largely time-invariant.\footnote{These papers likely suffer from difficulties in estimating a treatment effect because the incentive mechanism does not manifest. If protection status is based upon one of the largely non time-variant criteria that define refugees according to the Geneva convention, for example persecution due to gender, sexual orientation, or ethnicity among others, the granting of temporary residency should not generate the same incentives to integrate into the labour market; upon re-evaluation of one’s case for renewed residency, the factors that led to refugee status are still present. Our sample on the other hand consist almost entirely of individuals granted subsidiary protection status due to the Syrian conflict itself, not the above mentioned criteria (see Table 1).}

With this in mind, this paper takes a somewhat different approach by exploiting a swift change of policy implemented in Sweden concerning asylum seekers from Syria. Before implementation of the policy change, most Syrian asylum seekers that came to Sweden were given temporary residency. In 2013 the Swedish Migration Agency (SMA henceforth) abruptly re-assessed their evaluation of the Syrian conflict which had as a consequence that all asylum seekers from Syria were to be given permanent rather than temporary residency.\footnote{Note that the recognition rate for Syrian asylum seekers was 100 % before and after the policy change.} As the policy change was implemented without prior warning, it provides a threshold between Syrian refugees that applied for residency under two different regulations. We exploit this threshold as a quasi-experiment to study the effect of permanent residency on labour market inclusion in the short term.

This study brings a few additions to the existing literature. First and foremost, it explicitly focuses on the effects of residency status on labour market inclusion. Until this point, there is little knowledge of the specific impact of temporary and permanent settlement on these outcomes in existing studies. This is of interest because labour market outcomes are the most commonly debated in policy and theory in which both models, the rights-based and responsibilities-based, are claimed to be superior. Second, we make use of detailed individual data containing the full population under scrutiny. The dataset allows us to sort between different categories of migrants and identify refugees, as opposed to labour migrants, family migrants and students. Opposite to comparative approaches, this study investigates the effect of temporary and permanent residence permits within one nation thus holding confounding variables such as institutional, cultural, and historical variation constant. Lastly, while this study only focuses on the short-term inclusion into the labour market, we do so with estimates of outcomes after two different lengths of time in Sweden. The importance of early settlement have been emphasized in previous
studies and this approach allows us to identify at which point observed outcomes as the result of residency permit manifest (OECD, 2016a,b).

The conclusions of this study are two-fold. We find that temporary residents have higher incomes and are unemployed less. However, at the same time, they are less likely to spend time in education than are those with permanent residency. Given these findings it is clear that less secure residence status is beneficial to labour market inclusion in the short term. However, the greater focus on education among those with permanent residency raises the possibility that long-term inclusion may not follow the same patterns. Our results therefore suggest that both approaches to migration policy can be supported empirically, albeit with different metrics for success. In our view then, the issue should be viewed as largely normative. Rather than debate whether one approach will lead to greater inclusion than the other, focus should be shifted to discussing the type of inclusion that the different approaches are likely to provide.

2 A Theoretical Framework on Security of Residence

We propose a theoretical model, inspired by Borevi (2002) and Koopmans (2005), in which residency status is defined as a uni-dimensional scale ranging between different levels of security. The position at the left-side of the continuum, insecure residency, refers to a position in which individuals are largely excluded from full membership and residency status relies exclusively on attachment to a specific job or studies. If the required attachment is lost, then the basis for residency is immediately withdrawn. Hence, this position refers to an unpredictable and insecure status in which the individual has to perform in a pre-stipulated manner in order to maintain residency. At the right end of the continuum, secure residency refers to a position in which full membership is granted into a community of citizens. In this position, residency cannot be withdrawn and hence refers to an increasingly predictable and secure type of residency.

<table>
<thead>
<tr>
<th>Residency status</th>
<th>Guest workers</th>
<th>Temporary residents</th>
<th>Permanent residents</th>
<th>Citizens</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Insecure residence</td>
<td>Secure residence</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Figure 1: Theoretical model on security of residence

Notes: Figure displays the continuum in residency status ranging from insecure residency to secure residency.

In our theoretical model, we argue that temporary and permanent residency are found in between the above extremes. This is visualized in Figure 1. Temporary residency is
found closer to the left, insecure, end of the scale because an individual’s status will be re-assessed after a pre-determined period of time. In the event that the grounds for residency change, and one lacks attachment to the host society such as through employment, residence status is not typically renewed upon re-assessment. However, temporary residents do not rely on a specific attachment during their limited time of residence. On the other hand, permanent residence is found on the right-side of the scale. Individuals granted permanent residency are not yet fully included in the membership of citizens but they benefit from secure residency that is not subject to re-assessment after a given time period, and thereby is not conditional on employment or study.

3 Residency Status and the Labor Market

Human capital theory stipulates that an individual’s earnings are determined as a function of work experience and skills such as those developed in education (Mincer, 1974; Chiswick, 1978; Dahlstedt and Bevelander, 2010). If individuals can bear the cost of education in the short-term, as well as the indirect costs of loss of income and less experience, long term benefits will lead one to prefer education. This model has been highly influential in understanding why individuals with higher education have higher earnings on average. The human capital framework can be applied to explain migrant earnings as well with respect to education in a recipient country. Migrants that study in their new country, whether it be language training (Chiswick, 1991; Lemaître and Liebig, 2007; Ferrer et al., 2006; Delander et al., 2005), higher education (Nekby et al., 2002; Hammarstedt, 2003; Aldén and Hammarstedt, 2014), or a specific labour market certificate (Dahlstedt and Bevelander, 2010) are consistently found to have higher incomes in the long term than those migrants that do not study upon arrival to their new country as they develop skills adapted to the local labor market.

For those granted permanent residency renewal of residency status is guaranteed and the short-term long-term trade-off as outlined in the human capital function remains unchanged. However, an insecure residency status has the possibility to influence this model by altering the short-term long-term trade-off. The literature on civic integration suggests that residence status should depend on performance such as labour market attachment. Migrants that fail to accommodate the desired requirements are refused renewal of residency (Bee and Pachi, 2014; Borevi, 2010; Koopmans, 2010). This incentive, or restriction, means that individuals that aim to remain in their recipient country must discount long-term earnings that may come from education in favour of short-term labour market attachment that will ensure residency renewal. In line with this argumentation, we propose the following hypotheses:
• H1: Permanent residency should lead individuals to work less than temporary residents in the short term.

• H2: Permanent residency should lead individuals to study more than temporary residents in the short term.

4 The Institutional Setting

After the outbreak of the conflict in Syria the Swedish Migration Agency (SMA) crafted a number of internal documents containing guidelines and descriptions of the development in the conflict. These documents, referred to as RCI (Instructions from the General Counsel), served to guide case workers in the assessment of the mounting number of applications from Syria. Without going into the specifics of these documents, the general guideline prior to the 2013 change which we exploit was that Syrian asylum seekers were to be granted temporary residence in Sweden, allowing three years of settlement (RCI 14/2012, 2012). However, permanent residency could be granted if individuals were considered to be convention refugees. In 2012 the share of temporary residence permits was 61% of all granted Syrian applications. This figure had risen somewhat in 2013 in which the share of temporary residence permits was roughly 73% of all granted Syrian applications.

On September 3, 2013 the SMA made a new evaluation of the conflict in Syria (RCI 14/2013, 2013) which resulted in the policy change of focus in this study. The General Counsel stated that the conflict was in a dead lock position, in which both sides believed close victory was possible. The SMA also noted that the number of actors participating in the conflict had increased. As a consequence of the increased complexity of the conflict, the General Counsel made the judgment that the unrest in Syria would go on for an extensive period of time and stated that all Syrian asylum seekers should be granted permanent residence. Hence, after the policy change, 100% of applications were granted permanent residence permits even if not considered convention refugees.

There are a few important details about the policy change that have significant implications for the choice and implementation of our research design. First, the change was implemented immediately, providing us with a cut off between those awarded temporary and permanent residence permits. As shown in Figure 2, there is a clear jump at the

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4“Rättschefens instruktioner” in Swedish.
5For a more detailed description of the the guidelines provided by the SMA, see Andersson and Jutvik (2018).
6In accordance with the Geneva Convention, a person is a refugee if he or she has a well-founded fear of persecution due to race, nationality, religious or political beliefs, gender, sexual orientation or affiliation to a particular social group.
implementation of the new directives, where the share of permanent residence permits sharply rises from about 35% to 100%. Second, the change of directives were implemented by the SMA without prior announcement. Hence, it was not a political decision or the result of a long parliamentary debate – no awareness was made of an impending change in the period prior to the reform. This setup made it impossible to react to the change before the actual implementation. The sudden implementation, in combination with detailed data from the SMA, allows us to identity a treatment group unaffected by any potential sorting – those individuals that applied before, but were granted residency after, the change in policy. Lastly, the policy change implied that all individuals that had been granted temporary residence permits prior the change could apply for a re-evaluation of their permits in order to make them permanent. The majority of Syrians with temporary permits also applied for the re-evaluation immediately. These applications for extensions were largely processed by the SMA before the end of the year.\footnote{Data from the SMA indicate that 99\% of all applications for re-evaluation were handed in to the SMA before the end of 2013. The SMA had processed 77\% of these applications in the end of 2013 and 91\% in the end of January 2014.}

4.1 The Introduction Program

The introduction program is available to \textit{all} newly arrived migrants during their first two years of residence in Sweden. The program contains language training (SFI) and civic orientation, but also the development of an individual plan aiming to fasten labour
market introduction (Larsson, 2015). Although the program is not mandatory, those who choose not to participate do not receive any attached economic benefits. Hence, there is a strong incentive to participate. The economic support provided in the program is slightly higher than the general social assistance available to all Swedes and is not affected by the income of other household members. Participants are also allowed to work during the program (OECD, 2016a).

Given the structure of the program, in which benefits are dependent on participation, we assume that the individuals under scrutiny here were part of the program during the time of investigation. The program gives several alternatives for participants in terms of language training, preparation for work, and educational activities in combination with activity on the labour market (OECD, 2016a). In that manner, the program, in combination with the cut off of the policy change, allows us to track individual behavior during the extension of the program as well as attachment to the labor market in the treatment and control group.

5 Methodological Setup, Data and Samples

This study relies on a detailed database of individual-level Swedish register data. The database, GEOSWEDEN, contains anonymous information on all residents with a registered address in Sweden between 1990 and 2014. Assessing this data-set we obtain information on, among many other things, individual demographics, labor market status, education, country of birth, month of granted residence permit, and reason for approved application (grund för bosättning).

In our methodological setup, we make use of the sharp introduction of SMA’s policy change which provides a cut-off point determining residency status. Because the decision from the SMA was so sudden and without prior indications of an impending change, those that were approved for asylum after, but who applied before, September 3, 2013 were entirely unaware that they would be guaranteed to receive permanent residency upon approval. As such, their applications would have been made with the knowledge that the majority of successful asylum applicants received temporary status. This group of individuals, those that applied before but for whom a decision was taken after the change in policy, make up our treatment group.

We do not observe the date of application and date of decision at the individual-level in GEOSWEDEN. Rather, our data allows us to see only the month a residency decision was granted. This creates two potential problems in defining the start and end of our treatment group, however, we circumvent this issue in two ways. First, in defining the start of our treatment group we rely on the fact that the change was made at the
beginning of the month, on September 3, 2013. That means that effectively all individuals
for whom a decision was made in the month of September were subject to the new rules.\footnote{September 3 was a Tuesday which means that only individuals whose decisions were made on Monday, September 2 would have been subject to the old rule, all others who were decided in September were subject to the new rule. There were 927 decisions made in the entire month of September which equates to 44 per working day on average. However, the length of time for a decision decreased drastically after the rule change (there were on average 37 decisions per day in July 2013 and 33 per day in August 2013) which would indicate that more decisions were made per day after the rule change than before, and that the number of decisions made in September 2013 but before the reform is likely lower than this daily average.} Inferring application outcomes based on monthly-level data is therefore not problematic
and allows us to overcome the issue of treatment “start” in the absence of daily data.

The second issue is that we cannot define the “end” of our treatment period at the
individual-level. That is, we know that all individuals that were awarded residency after
September 3, 2013 were given permanent residence, but if we include individuals into
our treatment group that applied for residency after September 3 our sample will have
self-selected into treatment. In order to solve this problem, we make use of data from
the SMA containing anonymous individual-level application and decision dates (though
we are unable to connect this to individuals in the GEOSWEDEN database through lack
of identifying information). The data from the SMA shows us that 96.38% of all Syrian
asylum decisions made in September 2013 were based on applications that had been
submitted prior to the September 3 threshold. This proportion decreases quite rapidly in
the following months; only 57.6% of applications granted in October were based on pre-
reform submissions, and this drops further to 23.13% in November. As such, we can with
confidence define our treatment window as all of those individuals for whom an asylum
decision was granted in September 2013. After removing those below the age of 18 and
over 65 from our sample, we obtain a treatment group comprised of 629 individuals.

We define our control group as all individuals from Syria that were granted residency in
September of 2012, exactly one year prior to our treatment group. Such a definition allows
us to hold constant the amount of time individuals had been in Sweden across treatment
and control groups when we measure our dependent variables. Further, migration is
highly seasonal so the selection of migrants based on granted asylum month should further
ensure comparability. The most natural control group would have been to select those
individuals that were granted residency in the period immediately prior to the change in
policy directive, for example those that were granted residency in August 2013. However,
the provision which allowed all of those individuals who had been previously been granted
temporary residence to apply for a re-evaluation, prevents the use of such a strategy.

Comparing labour market indicators over different years can naturally lead to prob-
lems if economic conditions differ year-to-year. However, for our two years of interest we
see very little change in economic indicators. GDP growth was 1.2% in 2013 and 2.6% (World Bank) in 2014 and foreign-born unemployment was exactly equal at 16.4% in both years (OECD). In general, the labour market conditions for our two groups were largely equal, if not slightly beneficial to the 2013 cohort of permanent residents. However, it is nevertheless possible that the labour market for new arrivals specifically differs year-to-year. We therefore leverage the labour-market outcomes of all other non-Syrian asylum seekers as a comparison group. These approaches will be further described below.

<table>
<thead>
<tr>
<th>Table 1: Individual characteristic-differences between asylum seekers arriving 2012 and 2013</th>
</tr>
</thead>
<tbody>
<tr>
<td>Year</td>
</tr>
<tr>
<td>Population (September):</td>
</tr>
<tr>
<td>Men</td>
</tr>
<tr>
<td>Age</td>
</tr>
<tr>
<td>Young</td>
</tr>
<tr>
<td>Middle age</td>
</tr>
<tr>
<td>Married</td>
</tr>
<tr>
<td>With children</td>
</tr>
<tr>
<td>With university education</td>
</tr>
<tr>
<td>Protection status (%)</td>
</tr>
<tr>
<td>Subsidiary protection</td>
</tr>
<tr>
<td>Convention refugees</td>
</tr>
<tr>
<td>Place of residence (%)</td>
</tr>
<tr>
<td>Metropolitan cities</td>
</tr>
<tr>
<td>Stockholm</td>
</tr>
<tr>
<td>Outcome variables:</td>
</tr>
<tr>
<td>Unemployment days</td>
</tr>
<tr>
<td>Declared income</td>
</tr>
<tr>
<td>Study grants</td>
</tr>
</tbody>
</table>

*Notes:* Table display differences in characteristics among those arriving in September 2012 compared to those arriving in September 2013. The variables are measured in the end of the year that the residence permit was granted.  

5.1 Data and Sample

The data for our primary analyses are taken from all asylum seekers in the years 2012-2014 which is the height of Syrian migration to Sweden. In Table 1 we present some descriptive statistics of Syrian asylum seekers for whom residency was granted in September 2012 and September 2013. As seen in the table, gender and the age structure is relatively stable. However, moving on to the share of married individuals and individuals with children reveals an increase by 7 and 13 percentage points respectively. In addition

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92014 is the latest wave of data available to us.
to that, there is also a substantial increase in individuals with university education.\textsuperscript{10}

The focus of this paper is labour market inclusion which motivates the selection of our dependent variables. We use three measures, number of days registered as unemployed, amount of study support received (in 100s of Swedish Kronor), and declared income (in 100s of Swedish Kronor). These are all measured as totals for the calendar year.

A primary assumption that our research design relies upon is the comparability of our control and treatment cohorts. As can be seen in the Table 1, 2012 arrivals are largely similar to the 2013 arrivals in terms of gender, age and the proportion that are considered convention refugees. As mentioned, there are some differences. If those that make up the 2012 cohort have baseline characteristics that make them more likely to succeed on the labour market than the 2013 cohort, our results could be biased. Level of education is precisely such a potential factor. However, we find here that the proportion of university educated is higher among our 2013 cohort than the 2012; in other words, our treatment group is more highly educated than the control group. As such, based on the descriptive differences we observe between cohorts, this sample provides a hard test of our hypotheses that permanent residents should work less and study more in the short term than temporary residents.

6 Results

6.1 Baseline results - did the change in residency status have any effect on inclusion?

In Table 2 we present the estimated effects of residency status in a direct comparison of permanent and temporary residents that arrived in September 2013 versus September 2012 respectively. All outcomes are measured in December of the year after arrival. For each of the three dependent variables we estimate the effect of residency status with and without demographic controls. \textit{Perm Res} is the coefficient of interest which represents the estimated difference between those with permanent (coded as 1) and temporary residency (coded as 0). Because this variable is dichotomous, we can interpret it as a difference in means of the dependent variable for the two residency status groups (conditional on the demographic covariates in the models for which they are included).

Models 1 and 2 show the estimated effect of residency status on total number of registered unemployment days in an individual’s first 16 months in Sweden. Based on the Model 1 estimate, those with permanent residence were registered as unemployed for

\textsuperscript{10}The data regarding education is somewhat problematic since validation of foreign education might differ over cases. We control for these differences in our estimations but acknowledge that the differences between the groups might be smaller.
Table 2: Baseline results: Effects of residence status

<table>
<thead>
<tr>
<th></th>
<th>Unemployed Days</th>
<th>Study Grants</th>
<th>Declared Income</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
</tr>
<tr>
<td>Perm Res</td>
<td>Basic W. controls</td>
<td>Basic W. controls</td>
<td>Basic W. controls</td>
</tr>
<tr>
<td></td>
<td>23.52***</td>
<td>17.94**</td>
<td>6.447**</td>
</tr>
<tr>
<td></td>
<td>(8.449)</td>
<td>(8.439)</td>
<td>(2.896)</td>
</tr>
<tr>
<td>N</td>
<td>903</td>
<td>903</td>
<td>903</td>
</tr>
</tbody>
</table>

Notes: Table displays the estimated effects of residency status. Perm Res shows the comparison between the 2012 and 2013 cohort after 16 months with a residence permit in Sweden. Controls include age, marital and parental status, education, and gender. Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

Source: GEOSWEDEN (2018)

24 more days on average than those that arrived in September 2012. When we include demographic controls the coefficient changes only marginally indicating that those with permanent residence permits registered 18 more unemployment days.

The variable Study Grant is measured in 100s of SEK (roughly equivalent to $12 USD) and as such the coefficient on Perm Res shows that permanent residents took out on average roughly 650 SEK more in their first year of residency. This form of financial assistance is only available to those that are enrolled in a study program so the fact that temporary residents received less study grants than permanent residents indicates that they generally spent less time in education. The 2012 September cohort of Syrians received on average 644 SEK in study grant compared to 1288 among the 2013 September cohort yielding a difference of 644 SEK, equivalent to the -6.44 point estimate in the basic model. Such a figure is low in absolute terms, but represents a doubling of expected study grants among the permanent resident group. When controls are included the magnitude of the coefficient increases to 8.4 (840 SEK).

Lastly, the estimated effect of residency status on income is such that individuals with temporary residency, our control group, declared on average 19,400 SEK more after their first year than did those with permanent residency. The inclusion of demographic controls reduces this estimate somewhat, but even so the estimate is an increase in declared income of 15,900 SEK among temporary residents.

In order to further test the robustness of our baseline results, we extend the time period of comparison, increasingly adding one month at the time, until December. Although this test gives larger samples, it also brings problems with sorting as a larger share of individuals applied after the policy change was instigated. However, the estimates are rather stable as the time period is extended. We also perform the same analysis but instead of comparing Syrians, we compare all other newly arrived migrants granted residence in the same time period. Estimates with this placebo group yield largely insignificant estimates. More specifically, the estimates regarding unemployment days as well as declared income

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are insignificant. There is, however, an effect on the usage of study grants in the placebo group although the estimates negative, opposite of our main test. Lastly, we conduct estimates with a matched sample derived from propensity score matching. These tests gives further support to our analysis regarding unemployment days and declared income but yields insignificant, but positive, estimates considering the usage of study grants.\textsuperscript{11}

### 6.2 Difference-in-differences

While the above comparison is intuitive, it is potentially invalidated by year-on-year trends in the outcome variables. In order to wash such potential trends we utilize a difference-in-differences approach. Effectively, the method sets out to compare the trends in labour market inclusion observed cohort-to-cohort among Syrian asylum seekers with that of an assumed comparable control group. The difference in post-policy outcomes of these two groups is compared to the difference in pre-policy outcomes thereby providing an estimated effect of treatment that is not confounded by time.

In our first specification we make use of non-Syrian asylum seekers that arrived in the same period as our Syrian sample.\textsuperscript{12} That is, we compare the 2012 Syrian cohort to the 2012 cohort of all other asylum seekers to Sweden, and the 2013 Syrian cohort to the 2013 cohort of asylum seekers. We estimate the year-on-year trend as identified by the Year term in Table 3 which takes on the values of 1 for the 2013 cohort (outcomes measured in 2014) and 0 for the 2012 cohort (outcomes measured in 2013). Further, we include a dummy variable which indicates if the observation belongs to the Syrian sample (1) or if they belong to the group of other countries (0). Lastly, we interact these two terms to obtain our difference-in-differences estimate. If the cohort trend in the outcome variable differs for the Syrian sample in comparison to the group of other arrivals, we assume that this difference is due to the change in SMA directives, and thus, is the result of permanent residency. We also include the same pre-treatment controls as above. As can be seen in Table 3, the estimates produced from the difference-in-differences specification are substantially larger in magnitude compared to the experimental setup for for all included measures. Having said that, they mirror the base-line results presented above. Further, statistical significance at the 95% confidence interval is achieved in all 6 models.

There is a clear increase in the usage of unemployment days in the Syrian cohort after the introduction of permanent residency. The amount of declared income, on the other hand, decreases after the introduction of permanent residency. Lastly, the usage of study

\textsuperscript{11}For robustness regarding extended time periods and propensity score matching, see Appendix.

\textsuperscript{12}We have restricted our sample of other newly arrived migrants (non-Syrian) to exclude those migrating from “uncommon” sending-countries with less than 35 individuals over the time period. This is done to avoid smaller groups of migrants to get significant weights in the synthetic control group approach. The sample consist of 19 countries/geographical areas.
<table>
<thead>
<tr>
<th></th>
<th>Unemployed Days (1) Basic</th>
<th>(2) W. controls</th>
<th>Study Grants (3) Basic</th>
<th>(4) W. controls</th>
<th>Declared Income (5) Basic</th>
<th>(6) W. controls</th>
</tr>
</thead>
<tbody>
<tr>
<td>Year</td>
<td>-12.45*</td>
<td>-14.17**</td>
<td>-5.347</td>
<td>-4.949</td>
<td>36.28</td>
<td>24.39</td>
</tr>
<tr>
<td></td>
<td>(7.12)</td>
<td>(6.94)</td>
<td>(3.45)</td>
<td>(3.19)</td>
<td>(31.03)</td>
<td>(30.63)</td>
</tr>
<tr>
<td></td>
<td>(8.89)</td>
<td>(8.72)</td>
<td>(4.31)</td>
<td>(4.01)</td>
<td>(38.73)</td>
<td>(38.50)</td>
</tr>
<tr>
<td>Year x Syrian</td>
<td>35.97***</td>
<td>32.91***</td>
<td>11.79**</td>
<td>10.93**</td>
<td>-230.37***</td>
<td>-204.27***</td>
</tr>
<tr>
<td></td>
<td>(11.17)</td>
<td>(10.93)</td>
<td>(5.42)</td>
<td>(5.03)</td>
<td>(48.67)</td>
<td>(48.25)</td>
</tr>
<tr>
<td>Constant</td>
<td>138.37***</td>
<td>81.55***</td>
<td>24.39***</td>
<td>71.64***</td>
<td>131.55***</td>
<td>59.55</td>
</tr>
<tr>
<td></td>
<td>(5.24)</td>
<td>(9.80)</td>
<td>(2.54)</td>
<td>(4.51)</td>
<td>(22.83)</td>
<td>(43.25)</td>
</tr>
<tr>
<td>Observations</td>
<td>2,025</td>
<td>2,025</td>
<td>2,025</td>
<td>2,025</td>
<td>2,025</td>
<td>2,025</td>
</tr>
<tr>
<td>R²</td>
<td>0.01</td>
<td>0.05</td>
<td>0.01</td>
<td>0.16</td>
<td>0.02</td>
<td>0.06</td>
</tr>
</tbody>
</table>

Notes: Table displays the estimated effects of residency status. The coefficient of interest is the interaction term (Year x Syrian). Controls include age, marital and parental status, education, and gender. Robust standard errors in parentheses. 

*** p<0.01, ** p<0.05, * p<0.1

Source: GEOSWEDEN (2018)

Table 3: Difference-in-differences specification - Other new arrivals

6.3 Synthetic control group

A difference-in-differences model provides unbiased causal estimates under the assumption that the treated group would have followed the same trend as the comparison group in the absence of treatment (Angrist and Pischke, 2008). However, in the event that this assumption does not hold, resulting estimates are potentially invalid. As such, we further implement a synthetic control group approach which allows one to empirically define the comparison group in a generalized difference-in-differences approach (Abadie et al., 2010).

This approach has two distinct advantages which contribute to the overall aim of the paper. First, it provides an empirically chosen comparison group that perfectly matches the trend in labour market outcomes for Syrian asylum seekers in the pre-treatment period. As such, we have a stronger claim that the common trend assumption is valid. Second, because we must include a second pre-treatment cohort, we must adjust the measurement period for our dependent variables such that outcomes are measured in the same year as the migrant arrived in Sweden rather than one year later. This provides the opportunity to identify not only if there is a shift in expected outcomes as the result of the change in SMA directive, but also to determine if such differences manifest at the
earliest stages of a migrants time in Sweden, or if they are only visible later.

We use three cohorts as the basis for our synthetic method estimates, those granted residency in 2012, 2013, and 2014. However, we restrict our sample to only those that arrived in August or prior in each given year because the policy directive changed in September 2013. This ensures that both the 2012 and 2013 cohorts can be viewed as fully untreated, and the 2014 cohort as fully treated. A second potential limitation to this estimation strategy is related to data availability. 2014 is the last year of data available which means that we must measure the outcomes in the same year in which one was granted asylum. As such, results should be viewed as the estimated effect of residency status in the very short term.

The synthetic control method is appropriate when treatment is assigned at the aggregate level (Abadie et al., 2010, 2015; Fowler, 2013). In our case, the treatment assignment is the assignment of permanent residency which is applied to the group of Syrian asylum seekers from September 2013 and onwards. Since the change in process affected Syrian migrants only, the pool of migrants from other countries can be seen as potential control units which can act as counterfactual cases. Given a set of pre-intervention outcomes, in our case the average unemployment measured in 2012 and 2013, the synthetic control method seeks to find a set of weights, w, that can be applied to the donor pool of potential control cases such that the outcome measures in the pre-treatment period between the synthetic control and observed treatment group are equal. In other words, the method finds an weighted combination of control units such that the common trend assumption is satisfied in the pre-treatment period (we still must make the assumption that it is satisfied in the post-treatment period, which is inherently untestable). In effect, the method finds a control group such that:

$$\tilde{Y}_t^k = \sum_{c=1}^{j} w_c \cdot \bar{Y}_c^k$$  \hspace{1cm} (1)

where Y is the variable of interest, subscripts t and c denotes the treatment and control cases, j is the number of control units, and superscript k indexes the pre-treatment measurements of Y.

The donor pool of potential control cases consists of 19 non-EU units (SMA designated geographical regions – primarily countries but in some instances groups of smaller countries from a distinct geographic area) – from which at a minimum 30 individuals were awarded residence permits on humanitarian grounds.

13 These are the same 19 geographical regions that make up the comparison group in the standard difference-in-differences estimates discussed.
days), every possible donor unit is given positive weight. However, in all three cases the empirical control is largely dominated by two units, with the remaining units receiving marginal weights. See the Appendix for the actual weightings.

Figure 3: Trends in unemployment days (a), declared income (b), and study grants (c) for Syrians and the synthetic control unit

Notes: The figures presents the level of unemployment days (a), declared income (b), and study grants (c) among Syrians and the synthetic control unit across three points in time. The synthetic control unit consist of a weighted average of all other individuals granted residence permits on humanitarian grounds in Sweden.


Given that the algorithm was successful in finding a pre-treatment match for Syrian migrants, we can examine the difference in the trends post-treatment in order to draw inference of the effect of the reform. Figure 3 (a) shows that the level of unemployment days is lower in the synthetic control unit after the introduction of permanent residency. Effectively, Syrian migrants in 2014 on average were registered as unemployed for 16 more days in 2014 than the weighted average of those individuals that make up the control unit. The results for declared income are similarly perfectly consistent with our main estimates; the synthetic control group is expected to have declared 7100 SEK more in 2014 than did Syrian migrants. The short time frame of these estimates must be kept in mind. These estimates are measured at the end of 2014 for individuals that were granted
residency in 2014. Given such a short time period, and given that the average income for the 2014 cohort of Syrian migrants in 2014 was approximately 54,100 SEK, 7,100 SEK is a non-trivial estimate. The final outcome variable in our study is the use of study grants, defined as the amount of study grants in SEK that migrants received in a given year. Here we find that Syrian migrants received on average more study grants than did their counterparts in the synthetic control group. On average, a Syrian is expected to have secured 200 SEK more at the end of their first year in Sweden than was observed in the synthetic control group.

Taken together, the estimates above provide a rather coherent picture, and one which is highly consistent with our main investigation. In addition, we perform randomization inference on the other 19 countries in the data-set. For each dependent variable we perform the same analysis, while excluding Syrians. Regarding unemployment days, only three other groups provide a larger increase than the Syrian sample; in the randomization inference regarding declared income four other groups provides larger negative estimates; lastly, regarding study grants, six other populations show similar increases. Therefore, although connected with some uncertainties, the results consistency of the synthetic control tests with our other estimation techniques increases our overall confidence in the effect of permanent residency. See the Appendix for the randomization inference results.

7 Discussion and Concluding Remarks

A number of scholars have pointed to a general trend of convergence towards increasingly restrictive migration policies and a multicultural backlash across European nation-states (Joppke, 2007; Vertovec et al., 2010). Similarly, political parties and scholars have emphasized that individuals that need to make an effort to remain in the country of destination in order to have a better chance to integrate into the main society. A few studies have evaluated the effect of civic integration measures. However, few of these studies explicitly focus on residency status and its effect on labour market inclusion.

In this study, we have attempted to fill this knowledge gap by exploiting a policy change concerning the residency status of Syrian asylum seekers in Sweden. We examine the policy change with respect to labour market inclusion defined as unemployment days, study grant receipt, and declared income. A few problematic aspects of the policy change in combination with the difficulty of estimating labor market outcomes led us to perform several analyses; a difference-in-means approach as well as a difference-in-differences design and a synthetic control group approach. Our estimation techniques produce very similar patterns which give greater confidence in our overall results than can be derived from a single method. Further, the methods provide us with estimates at different time
points - outcomes in the cohort study are measure 16 months after residency and outcomes in the synthetic control models are measured 4 to 8 months after granted residency. This helps us to understand not only if, but when differences become apparent in the short term.

We argue that the shift in directives by the SMA represented a fundamental change in the security of residence for newly arrived Syrians. We argue that a shift to more secure residency represents a shift towards a more rights-based approach to migration, which is in itself derived from the normative standpoint of multicultural migration policy. Consequently, this shift that the SMA implemented represents movement away from a responsibilities-based approach to migration which is typically grounded in theories of civic integration. Migration policy in its entirety is defined by many more features than this single dimension, and we are careful to avoid any claim to the study of different migration policy regimes. Rather than a weakness, this continuity in migration policy is a strength of this study as it allows us to identify an individual dimension such that all other relevant institutional and cultural factors of the recipient country, Sweden, remain constant. Such an opportunity allows us to estimate the effect of differing levels of security of residency within a single institutional context, rather than deriving inference from the comparison of many contexts, in which all factors that define migration policy are bundled.

In general we find that temporary residents perform significantly better than permanent residents with respect to unemployment and declared income. Across both our cohort studies and the synthetic control we find that Syrian asylum seekers that were granted temporary residence registered for fewer unemployment days, and that they have higher declared incomes. With respect to study grants received, our operationalization of time spent in education, we find rather that permanent residents outperformed temporary residents.

The differences we observe between permanent and temporary residents are not large in absolute terms. However, it must be kept in mind that these differences are observed only in the short term. It is nevertheless telling that differences are observable after such a short period of time. Having that said, it must be stressed that there is a not insignificant element of uncertainty to these results. A series of placebo tests for both our synthetic control and cohort studies have given mixed results. Specifically, with regards to our synthetic control study, randomization inference indicates that several other nationalities had expected differences from a synthetic control group equal or larger than that observed for Syrians in absolute terms. In several of these cases the nationality of interest constitutes a very small number of asylum seekers per year which should lead to greater year-on-year variance observed. With access to samples of equal size to our Syrian
cohorts it is very likely that some of these extreme estimates would be reduced. Placebo
tests for the cohort study were much more conclusive with respect to unemployment days
and study grants, finding convincing null effects among non-Syrian asylum seekers from
the four largest sending countries. We do find an increase in placebo incomes among this
group, but one that is substantially smaller than the observed difference among our true
treatment group. In summary, we find quite robust evidence to suggest that permanent
residency led to a larger number of registered unemployment days, and to decreased
declared income. The robustness of the observed difference in study grants is however
less certain as placebo tests from the cohort study were supportive of the overall findings,
but the cohort matching design and randomization inference from the synthetic control
method resulted in statistically insignificant estimates. Nevertheless, in all cases were the
point estimates consistent with the main results.

Advocates of responsibilities-based migration policy frequently claim that such ap-
proaches provide an incentive, or “push”, for individuals to integrate into society and the
labour market. From this perspective our results can be seen as supportive of this pos-
tulation, at least in the short-term - temporary residents that are subject to a relatively
less-inclusive situation earn more and are unemployed less. However, at the same time,
they are less likely to spend time in education than are those with permanent residency.
Given that those that study in their new country after they receive residency have higher
incomes and fewer unemployment days in the long-term, this is a potential worry for the
success of temporary relative to permanent residents in the long-term.

With a discrepancy in the observed differences due to residency status in unemploy-
ment, income, and study grants, such that we find temporary residency beneficial for
employment, and permanent residency beneficial for education, our results cannot be
said to offer a clear adjudication of which approach is empirically advantageous. Rather,
it would appear that both approaches have their own distinct benefits. While the political
debate about the issue is often framed in such empirical terms - that one approach will
lead to greater or worse outcomes for the target group according to a common outcome
- our results suggest that both approaches can be supported empirically, albeit with dif-
ferent metrics for “success”. In our view then, the issue is should be viewed as largely
normative. Rather than debate whether one approach will lead to greater inclusion than
the other, focus should be shifted to discussing the type of inclusion that the differ-
ect approaches are likely to provide - short-term labour-market benefits versus potential
long-term benefits.

Our findings advocate for further research into how other outcomes are affected by
the shift from temporary to permanent residency - or similar shifts in the rights - and
responsibilities-based migration framework. While we study labour market inclusion in
the short-term, migration policy is wide-ranging and its potential outcomes innumerable. So while our conclusions are drawn in relation to labour market inclusion, it is entirely plausible, and indeed likely, that other outcomes such as inclusion along social, political, or migrant well-being dimensions could be more clearly differentiated along empirical lines.
References


Appendix

A  Difference-in-means approach

Figure 4 below visualizes the control and treatment group of the study. The figure further display the time of measurement, which occurs 16 months after a residency permit is granted in the cohort study.

![Figure 4: Control and treatment group in our main analysis](image)

Figure 5 below visualizes the estimate size in the sample used in the original article and with additional months. The point to the left in the graphs compares Syrians arriving in September 2012 with those arriving in September 2013, 16 months after receiving a residence permit (as in the original article). Each step to the right adds one additional month. The point at the right-side of the graphs compares Syrians granted residence permits in September-December 2012 with September-December 2013. Although this gives a larger sample (6500 observations in the comparison to the right), it also brings problems with sorting as a larger share of individuals knew about the policy change before applying for asylum.

As suggested by the figure, adding additional months does only marginally affect the significance and the size of the estimates. In general, the results remains quite stable. In the lower set of graphs, the Syrian cohorts have been replaced by a placebo group, constituting all other newly arrived asylum seekers except for Syrians. As can be seen in the figures, there is no effect regarding unemployment days or declared income. However, the estimates for study grants are negative and significant, but only if more months than September is included in the sample. Thus, the general trend regarding study grants for other newly arrived migrants is slightly negative comparing the cohorts 2012 and 2013.
Figure 5: Estimate size in treatment (Syria) and placebo group (other newly arrived migrants)

Figure displays the estimate size and confidence intervals in the Syrian sample and placebo group regarding unemployment benefits (top-left), study grants (top-right), and declared income (bottom-left) comparing those arriving in 2012 with those arriving in 2013, adding one month to the sample in incremental step to the right.

Moving on to Figure 6, graphs below display the distribution in the dependent variables used in the study. As can be seen from the figures, the distribution is heavily skewed in which the majority have a low or, zero value on the dependent variable. This mirrored in the distribution among other newly arrived migrants.

Table 4 displays the outcome using three matching techniques relying on propensity scores. From the left, the table shows the number of observations in the treatment and control group, the size of the estimate for each variable and the T-score. Two conclusions can be drawn. Firstly, the estimate size of each variable is nearly identical to our main specification. Secondly, the tests regarding unemployment days and declared income are significant while the tests regarding study grants yield insignificant results.

Given the highly non-normal distributions in our dependent variables and that our models are based on only 903 observations we calculate bootstrapped confidence intervals (95% confidence interval) for the bivariate models in our main analysis. These are reported in Table 5.
Figure 6: Histograms of the outcome variables among Syrians (blue) and other newly arrived migrants (red)

Figure displays the distribution in the three dependent variables included in the study. The blue line represent the distribution in among Syrians and the red line represent the distribution among other new arrived migrants.


| Table 4: Matching with propensity scores |

<table>
<thead>
<tr>
<th>Variable</th>
<th>Treated (N)</th>
<th>Control (N)</th>
<th>Estimate</th>
<th>T-score</th>
</tr>
</thead>
<tbody>
<tr>
<td>(1) Unemployment days</td>
<td>601</td>
<td>231</td>
<td>25.57</td>
<td>2.157</td>
</tr>
<tr>
<td>Declared income</td>
<td>601</td>
<td>231</td>
<td>-145.9</td>
<td>-2.792</td>
</tr>
<tr>
<td>Study grants</td>
<td>601</td>
<td>231</td>
<td>2.11</td>
<td>0.81</td>
</tr>
</tbody>
</table>

| (2) Unemployment days | 601         | 262         | 22.11    | 2.468   |
| Declared income  | 601         | 262         | -155.24  | -3.326  |
| Study grants     | 601         | 262         | 2.58     | 1.002   |

| (3) Unemployment days | 601         | 262         | 18.96    | 2.053   |
| Declared income  | 601         | 262         | -118.42  | -2.781  |
| Study grants     | 601         | 262         | 2.67     | 1.106   |

Notes: Table display estimates using (1) ATT estimation with nearest Neighbor Mathing method, (2) Kernel matching method, and (3) ATT Estimation with Stratification method.
Table 5: Bootstrap intervals

<table>
<thead>
<tr>
<th></th>
<th>Bootstrap Lower</th>
<th>Bootstrap Upper</th>
<th>Parametric Lower</th>
<th>Parametric Upper</th>
</tr>
</thead>
<tbody>
<tr>
<td>Study Grant</td>
<td>-11.94</td>
<td>-1.04</td>
<td>-12.74</td>
<td>-0.2</td>
</tr>
<tr>
<td>Income</td>
<td>108.87</td>
<td>289.88</td>
<td>119.48</td>
<td>278.37</td>
</tr>
</tbody>
</table>

B Synthetic control group approach

The following tables presents: (1) the weights in the synthetic control group (largest weights in bold), (2) the predictor balance between the treated and the synthetic control unit, and (3) the placebo tests.

Table 6 display the composition of the synthetic control groups in each analysis. As can be seen in table, the two biggest weights in the synthetic control group regarding unemployment benefits constitute individuals from Morocco and Egypt. Regarding declared income, the two biggest groups in the control unit consist of individuals from Morocco and China. Lastly in the control unit for study grants, the two largest weights are received by individuals from Bangladesh and a sample of countries in Central America.

Table 6: Weights in synthetic control units

<table>
<thead>
<tr>
<th>Country</th>
<th>Unemployment benefits Unit Weight</th>
<th>Declared income Unit Weight</th>
<th>Study grants Unit Weight</th>
</tr>
</thead>
<tbody>
<tr>
<td>Russia</td>
<td>0.004</td>
<td>0.053</td>
<td>0.018</td>
</tr>
<tr>
<td>Ethiopia</td>
<td>0.008</td>
<td>0.038</td>
<td>0.011</td>
</tr>
<tr>
<td>Somalia</td>
<td>0.004</td>
<td>0.056</td>
<td>0.019</td>
</tr>
<tr>
<td>Morocco</td>
<td>0.336</td>
<td>0.133</td>
<td>0.008</td>
</tr>
<tr>
<td>Uganda</td>
<td>0.02</td>
<td>0.049</td>
<td>0.012</td>
</tr>
<tr>
<td>Egypt</td>
<td>0.485</td>
<td>0.054</td>
<td>0.012</td>
</tr>
<tr>
<td>Eritrea</td>
<td>0.008</td>
<td>0.056</td>
<td>0.017</td>
</tr>
<tr>
<td>Rest of Africa</td>
<td>0.007</td>
<td>0.053</td>
<td>0.02</td>
</tr>
<tr>
<td>Lebanon</td>
<td>0.018</td>
<td>0.038</td>
<td>0.006</td>
</tr>
<tr>
<td>Turkey</td>
<td>0.053</td>
<td>0.045</td>
<td>0.022</td>
</tr>
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<td>Iraq</td>
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<td>0.046</td>
<td>0.012</td>
</tr>
<tr>
<td>Iran</td>
<td>0.003</td>
<td>0.043</td>
<td>0.008</td>
</tr>
<tr>
<td>Rest of west Asia</td>
<td>0.004</td>
<td>0.044</td>
<td>0.009</td>
</tr>
<tr>
<td>China</td>
<td>0</td>
<td><strong>0.094</strong></td>
<td>0.008</td>
</tr>
<tr>
<td>Afghanistan</td>
<td>0.008</td>
<td>0.052</td>
<td>0.005</td>
</tr>
<tr>
<td>Bangladesh</td>
<td>0.014</td>
<td>0.031</td>
<td><strong>0.489</strong></td>
</tr>
<tr>
<td>Pakistan</td>
<td>0.008</td>
<td>0.041</td>
<td>0.009</td>
</tr>
<tr>
<td>Rest of Asia</td>
<td>0.008</td>
<td>0.048</td>
<td>0.009</td>
</tr>
<tr>
<td>Central America</td>
<td>0.007</td>
<td>0.025</td>
<td><strong>0.307</strong></td>
</tr>
</tbody>
</table>

Notes: Table displays the weights in the synthetic control units for unemployment benefits, declared income, and study grants.
Source: GEOSWEDEN (2018)

Table 7 below displays the balance between the treated sample (Syrians) and the
synthetic control unit. As can be seen in the table, there is generally a strong balance between the Syrian sample and the control unit in the dependent variables. That is, the difference and the trend in the dependent variables between the Syrian sample and the synthetic control unit are similar in the pre-intervention period (2012 and 2013). However, it is also important to note that there are some discrepancies in the control variables included. In sum, the age and the share of married individuals are generally higher in the Syrian sample. Likewise, the share of men and individuals with children is also higher in the Syrian sample.

Table 7: Predictor balance between treated and the synthetic control unit

<table>
<thead>
<tr>
<th>VARIABLES</th>
<th>Treated</th>
<th>Synthetic</th>
</tr>
</thead>
<tbody>
<tr>
<td>Difference in unemployment days</td>
<td>35.96</td>
<td>35.94</td>
</tr>
<tr>
<td>Unemployment days</td>
<td>86.94</td>
<td>86.85</td>
</tr>
<tr>
<td>Age</td>
<td>35.46</td>
<td>32.56</td>
</tr>
<tr>
<td>Married</td>
<td>0.57</td>
<td>0.49</td>
</tr>
<tr>
<td>Sex</td>
<td>0.62</td>
<td>0.54</td>
</tr>
<tr>
<td>Children</td>
<td>0.43</td>
<td>0.39</td>
</tr>
<tr>
<td>Difference in declared income</td>
<td>-97.97</td>
<td>-97.82</td>
</tr>
<tr>
<td>Declared income</td>
<td>130.64</td>
<td>130.48</td>
</tr>
<tr>
<td>Age</td>
<td>35.46</td>
<td>32.36</td>
</tr>
<tr>
<td>Married</td>
<td>0.57</td>
<td>0.46</td>
</tr>
<tr>
<td>Sex</td>
<td>0.62</td>
<td>0.53</td>
</tr>
<tr>
<td>Children</td>
<td>0.43</td>
<td>0.33</td>
</tr>
<tr>
<td>Difference in study grants</td>
<td>0.84</td>
<td>0.83</td>
</tr>
<tr>
<td>Study grants</td>
<td>2.19</td>
<td>2.20</td>
</tr>
<tr>
<td>Age</td>
<td>35.46</td>
<td>33.39</td>
</tr>
<tr>
<td>Married</td>
<td>0.57</td>
<td>0.37</td>
</tr>
<tr>
<td>Sex</td>
<td>0.62</td>
<td>0.67</td>
</tr>
<tr>
<td>Children</td>
<td>0.43</td>
<td>0.26</td>
</tr>
</tbody>
</table>

Notes: Table displays the predictor balance between the treated sample and the synthetic control units for unemployment benefits, declared income, and study grants.

Source: GEOSWEDEN (2018)

Lastly, Table 8 displays the placebo tests in the synthetic control group approach. Here we excluded the Syrian sample, and performed the same analysis as in the original article. As can be seen in the table, three other groups of newly arrived migrants (from Lebanon, Turkey, and Bangladesh) display a higher increase in the usage of unemployment days. Regarding declared income four other groups (from Morocco, Lebanon, Asia and a sample from Central America) have larger decreases compared to Syrians. Lastly, regarding study grants six other groups (Somalia, Egypt, Asia, China Afghanistan, and Pakistan) have larger increases compared to Syrians in regards of study grants.
Table 8: Synthetic control group: Placebo tests

<table>
<thead>
<tr>
<th>Country</th>
<th>Unemployment benefits</th>
<th>Declared income</th>
<th>Study grants</th>
</tr>
</thead>
<tbody>
<tr>
<td>Syria</td>
<td>16.89</td>
<td>-71.57</td>
<td>2.08</td>
</tr>
<tr>
<td>Russia</td>
<td>3.35</td>
<td>114.90</td>
<td>0.58</td>
</tr>
<tr>
<td>Etiophia</td>
<td>-4.84</td>
<td>-51.57</td>
<td>-2.72</td>
</tr>
<tr>
<td>Somalia</td>
<td>-6.23</td>
<td>-24.23</td>
<td>7.71</td>
</tr>
<tr>
<td>Morocco</td>
<td>-11.86</td>
<td>-128.49</td>
<td>-40.20</td>
</tr>
<tr>
<td>Uganda</td>
<td>3.77</td>
<td>-5.05</td>
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<td>Egypt</td>
<td>-1.41</td>
<td>-13.46</td>
<td>1.18</td>
</tr>
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<td>Eritrea</td>
<td>12.94</td>
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<td>2.96</td>
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<td>-16.71</td>
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<td>-1.30</td>
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Notes: Table displays the estimates in the placebo test for unemployment benefits, declared income, and study grants.

Source: GEOSWEDEN (2018)