Contacts with Benefits: How Social Networks Affect Benefit Receipt Dynamics in the Netherlands

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Contacts with Benefits

How Social Networks Affect Benefit Receipt Dynamics in the Netherlands

Contacten met steun

Hoe sociale netwerken uitkeringsdynamieken in Nederland beïnvloeden

(*met een samenvatting in het Nederlands*)

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Contents

Chapter 1

Synthesis[1]

[1]This chapter has benefited greatly from discussions with Ineke Maas and Cok Vrooman.

1.1 Introduction

The welfare state and the social security it provides are key features of modern societies. In a narrow sense, it consists of various social insurance and tax-financed benefit schemes that aim to provide citizens with a minimum level of income and offer income security against risks such as job loss, illness, disability, and old-age. A broader perspective on the welfare state extends beyond mere income security, to also include prevention, reintegration, and social participation (see Vrooman 2009, 111–26). The broader perspective includes policy efforts that seek to prevent benefit dependency, and promote public health and participation in the labor market and society. From a public finance point of view, ensuring a high labor market participation and minimizing (long-term) reliance on social benefits is also important, because it increases tax revenues and minimizes public spending. In this dissertation, we focus on the receipt of benefits among the working-age population in the Netherlands, which includes social assistance, unemployment, and disability and sickness benefits. We also study exits from social assistance and the subsequent income development. In the Netherlands, about 2 percent received unemployment benefits, 4 percent received social assistance benefits, and 6 percent received disability benefits among 15 to 65 year-olds in 2018 (Statistics Netherlands 2020).

It is natural that people for shorter and sometimes longer periods of time rely on social benefits, given that these aim to provide citizens with income security. This is, for instance, the case when a person experiences a job loss and cannot directly find new employment. The person would then need an alternative source of income – e.g. unemployment benefits – to maintain his or her standard of living or to get by while searching for a new job. Still, there is also quite some research indicating that benefit receipt is a recurrent phenomenon over the life course for some people. In the Netherlands, for instance, official statistics show that about 28 percent of people starting to receive social assistance in the first quarter of 2015 had also relied on this type of benefit in the previous year (Statistics Netherlands 2019c). There are also some studies indicating state or duration dependence in benefit receipt, meaning a higher chance to continue depending on benefits the longer a person has received benefits (Arranz and García-Serrano 2014; Mood 2013). Further, benefit receipt is to some degree transferable between parents and children (see Boschman et al. 2019). Additionally, research indicates that long-term benefit receipt is associated with worse health and mortality (Naper 2009), and there is an established association between low income in general and poor health (e.g. Lindahl 2005). All of these findings imply that benefit receipt can be problematic and have detrimental consequences for some individuals. It is therefore

important to gain knowledge on the determinants of benefit receipt, including how to facilitate transitions from benefit receipt to the labor market.

One issue that is often linked to contemporary discussions of the welfare state is rising (non-Western) immigration. In particular, immigrants' lacking labor market integration in the form of lower rates of labor market participation and higher benefit dependency rates is seen as a key policy challenge. Across EU countries, particularly immigrants with a non-Western, or non-EU or -OECD background have a poorer labor market attachment than the native population (Eurostat 2020), although the differences vary across countries and between different national-origin groups. The same is true for benefit receipt (Barrett and Maître 2013). In the Netherlands, people with a non-Western immigrant background more often receive a benefit than the native Dutch. To illustrate, take for example the receipt of social assistance benefits: almost 14 percent of 15 to 65 year-olds with a non-Western background received such benefits in 2015, whereas about 2 percent did so among the native Dutch (Statistics Netherlands 2016, 61). Within the broader view on the role of the welfare state, a key policy issue should therefore be how to reduce these differences for all residents thereby preventing benefit receipt and ensuring participation in work and employment.

In this dissertation, we focus on the role social networks plays in benefit receipt, and in exits from social assistance and the subsequent income development. Benefit receipt is understood as deriving one's major source of income from benefits. By a social network, we mean the totality of individuals' personal relationships, including the attributes of the people who make up this network. There are several reasons why this focus is interesting. First, social networks are frequently mentioned as one explanation of benefit receipt, and persistent poverty and low income in particular, pointing to 'deficiencies' in or characteristics of people's personal networks that lead to (long-term) benefit receipt. Wilson (2012) posited that lacking social contact with employed individuals explained persistent poverty among inner-city Blacks in the US. Along these lines, some argue that the concentration of benefit recipients in certain areas spurs higher dependence on benefit receipt and makes for 'welfare cultures' that are difficult for individuals to break out of – see Pinkster (2009) for a similar argument regarding low-income networks. Individuals are surrounded by other benefit recipients that in turn contribute to a larger distance to the labor market and more benefit receipt.

A second reason relate to that having people in the network who are familiar with the welfare system may not necessarily be undesirable. People do not always claim benefits they are entitled to. This phenomenon is labeled non-take-up and several studies find evidence of it for various benefit programs (Bargain, Immervoll, and Viitamäki 2012; Hernanz, Malherbet, and Pellizzari 2004; Kayser and Frick 2000; Matsaganis, Levy, and

Flevotomou 2010; Mood 2006; van Oorschot 1994). Lack of information and social stigma surrounding benefit receipt can partially explain this phenomenon, implying a potentially important role of social networks to remedy non-take-up. As such, individuals' social environments may contribute to the optimal usage of welfare rights.

Last, social networks can improve individuals' labor market outcomes (e.g. Flap and Völker 2013; Lin 1999). Individuals frequently turn to people in their surroundings for job leads, how to apply for jobs, and advice on dealing with issues in the workplace, such as difficulties with their managers or co-workers. Access to such social resources may therefore lower benefit receipt and increase labor market participation. This is especially relevant for (non-Western) immigrants who frequently originate from societies and labor markets vastly different from their new 'host' country, and could therefore benefit from information on the functioning of the host-country labor market. Immigrants' social integration – in particular through their contact with members of the native or ethnic-majority population – is viewed as vital for their labor market integration. Contact with members of the native population can offer crucial information on the workings of the host-country labor market (see, e.g., Lancee 2010) as well as an opportunity to practice the host-country majority language. This is frequently coupled with worries over residential ethnic segregation (Gijsberts and Dagevos 2005; van Kempen et al. 2000), which leads to separation from the 'mainstream' society and lower incentives for host-country language acquisition (Lazear 1999).

Overall, this dissertation deals with the role that characteristics of individuals' social networks play in benefit receipt. We do so by, first, studying this role in the general population. Second, we zoom in on differences between natives and immigrants in benefit receipt and the impact of social networks therein. Last, we focus on a specific group of immigrants, namely refugees, and investigate their transitions from social assistance to the labor market and their subsequent income development. The focus on exits from social assistance and subsequent income development offers important insights into the aftermath of benefit receipt and speaks to the broader role of the welfare state. As such, this dissertation expands the body of knowledge on the role of networks in the occurrence of benefit receipt and in exiting the benefit system. This is achieved by using existing survey and administrative data from the Netherlands. In the remainder of this chapter, we detail the overarching goals of this dissertation and their theoretical background as addressed in the empirical chapters 2 through 5; how we

have investigated these empirically and the results of these investigations; and, finally, offer overall conclusions and implications emanating from these results.

1.2 Background and Research Questions

1.2.1 Previous research

Existing scientific research on the influence of social network characteristics on benefit receipt and labor market outcomes is largely disconnected – mainly in terms of topics, but also along disciplinary lines. In this section, we provide an overview of these streams of research and argue that combining the various insights derived from them will extend our knowledge of how networks affect benefit receipt.

Social influence

One stream of research mainly rooted within economics focuses specifically on social networks and benefit receipt (e.g. Åslund and Fredriksson 2009; Bertrand, Luttmer, and Mullainathan 2000; Markussen and Røed 2015; Mood 2010b). The aim has been to answer the broader question of to what extent the (average) behavior in someone's social network causally affects the individual propensity to behave in a similar fashion. For this body of research, this means: To what extent does a higher rate of benefit recipients among network members lead to a higher individual likelihood of benefit receipt?

Theoretically, applying for and receiving a benefit is assumed to be costly. Specifically, there are two 'costs' that social networks are thought to modify. First, applying for and receiving benefits requires information about the process and is thus associated with *informational costs*. Benefit-receiving network members convey such information about the welfare system (Bertrand, Luttmer, and Mullainathan 2000). The information may include such aspects as what programs exists and their rules of entitlement and eligibility, how to fill in applications and what documents to show case workers, and how to comply with obligations and behave toward case workers. The more network members receive benefits, the more readily available the access to such information will be. Subsequently, this reduces the informational costs associated with applying for and retaining benefits, thereby increasing the likelihood of receiving a benefit. Second, benefit receipt may be associated with *social costs*. These stem from perceived stigma and/or actual stigmatization by others, that are linked to benefit receipt (e.g. Lindbeck, Nyberg, and Weibull 1999; Mood 2004). Receiving benefits breaks with the norm of self-reliance and fending for yourself. By observing more benefit recipients in the network, individuals could perceive depending on benefits as more normal, which

reduces the perceived stigma for individuals. More benefit recipients in the network may also make it less likely to experience stigmatization from these benefit-receiving network members. This reduces the social costs involved with benefit receipt and increases the likelihood of receiving one in the future. Both the informational and stigma mechanisms lead to the hypothesis that the more benefit recipients there are in individuals' network, the higher the likelihood of receiving a benefit. This can be referred to as a *social influence* effect.

In testing this hypothesis, researchers in the 'social influence' literature typically seek to disentangle the causal social influence of benefit-receiving network members from selection, 'indirect' and/or contextual effects, which may bias or confound the estimation of the social influence effect (see Manski 1993)[2]. This means that the central aim of the empirical strategies is to ascertain to what extent individuals' likelihood of benefit receipt is affected by the prevalence of benefit receipt in individuals' social networks. Other sources of influence – including other network characteristics such as whether network members are employed or their level of education – are not considered, although these may also be causal (see Mood 2010b). Common to all empirical strategies is the use of administrative data. Nearly all studies also rely on the neighborhood and the prevalence of benefit receipt in the neighborhood to indicate the number of benefit recipients in an individual's social network (see Markussen and Røed 2015 for an exception). These empirical approaches all hinge on the ability to identify e.g. the neighborhood members that belong to an individual's social network. One such strategy involves making use of language spoken at home and national origin to zoom in on the people in the neighborhood that are presumably part of individuals' networks (e.g. Bertrand, Luttmer, and Mullainathan 2000; Furtado and Theodoropoulos 2013; 2016). Other approaches, for instance, draw on instrumental variables (Rege, Telle, and Votruba 2012) or natural experiments (Åslund and Fredriksson 2009). Irrespective of the strategy employed, the empirical findings all seem to point in the same direction: A higher prevalence of benefit recipients in the network – chiefly neighborhood – makes benefit receipt more likely. However, it is less clear how social network characteristics other than the prevalence of benefit recipients affect individuals' benefit receipt both

02 In Manski's terminology, the causal social influence effect is referred to as an 'endogenous effect', whereas contextual effects are labeled 'exogenous effects', and selection or indirect effects are called 'correlated effects'. We do not follow this terminology here, but it is common in the economic studies in this line of research.

theoretically and empirically. Additionally, the focus on neighborhoods effectively ignores the broader network and personal relationships that are not linked to the neighborhood.

Social resources

The second stream of research we draw on is mainly sociological, and focuses on the importance of social relations for labor market outcomes. It has mainly centered around the concepts individual-level social capital or social resources[3] (Flap and Völker 2013; Lin 1999; McDonald et al. 2013). The core idea in this 'social resources' literature is that individuals can access and/or mobilize social resources that are embedded in their social relations. These social resources can in turn be employed to achieve better labor market outcomes such as employment, higher wages, or a higher occupational status. Whereas the social influence literature stresses the welfare-related information and normative influences flowing through networks, the social resources literature emphasizes, among other things, the labor-market information network members may provide (Chen and Volker 2016; Granovetter 1973; Lin 2001). Such information may include actual job leads, how to write a resume, how to behave in a job interview, and knowledge about the norms and expectations in the workplace. Better access to these social resources, or being able to mobilize these, can help individuals in both finding a job and obtaining a better one. The central hypothesis is that the more social resources available to individuals, the better their labor market outcomes will be.

Empirically, this line of research has applied various methods to capture the resources embedded in people's networks (see van der Gaag (2005) and Marsden (2005) for overviews). One common approach is to delineate a certain part of the network – for instance people with whom the person has discussed important matters – and subsequently record the characteristics of the people in this network[4]. The characteristics recorded include aspects such as whether network members are employed, their level of education, or type of occupation. There have been raised valid concerns about causality issues in (some of) the empirical work (Mouw 2003; 2006). Nevertheless, most findings – including recent ones – support the notion that more social resources make for better labor market outcomes (see, e.g., Chen and Volker 2016; Flap and Völker

03 In this chapter, we prefer the term social resources over social capital, mainly because the latter often takes on a broader meaning that encompasses so-called community or macro-level social capital (Portes 1998). When we do use the term social capital, e.g. in chapter 3, we refer to individual-level social capital.

04 Additionally, some scholars have focused on the properties of a relationship or tie such as its strength (Gee et al. 2017; Gee, Jones, and Burke 2017; Granovetter 1973), the structural position of a person within the larger network (e.g. Burt 2001), and the extent to which individuals have people in their networks that can help them with specific tasks (van der Gaag and Snijders 2005).

2013), although not every measure of social resources predicts all labor market outcomes equally well (Marsden and Hurlbert 1988). The topics studied range from chances of employment and (ending) unemployment spells (Bonoli and Turtschi 2015; Cappellari and Tatsiramos 2015; Hällsten, Edling, and Rydgren 2017; Gee, Jones, and Burke 2017; Korpi 2001) to occupational status and wages (e.g. Cappellari and Tatsiramos 2015; Chen and Volker 2016; Lin, Ensel, and Vaughn 1981; Lin, Vaughn, and Ensel 1981; de Graaf and Flap 1988). To our knowledge, this line of research has not yet investigated the impact of social resources on benefit receipt specifically.

Networks and immigrants

A third research streams that draws on the social resources literature, has investigated the importance of social relations for immigrants' labor market integration (e.g. Auer, Bonoli, and Fossati 2017; Heath and Cheung 2007; Lancee 2012; Lancee and Hartung 2012; Kanas et al. 2012; Kanas, van Tubergen, and van der Lippe 2011). The starting point is the finding from several studies across Europe that native-immigrant differences in labor market outcomes still exist after taking into account the 'usual suspects' – i.e. demographic and human capital indicators such as age and level of education (Heath, Rothon, and Kilpi 2008). This unexplained difference is sometimes referred to as the 'ethnic penalty' (Heath and Cheung 2007). The core premise in this line of research is that immigrants and their descendants have worse labor market outcomes compared to natives because, among other things, they lack the necessary social capital (Koopmans 2016; Lin 2000). This may include both a deficit in access to social resources, and differences in the mobilization of social resources – see for instance Smith (2000; 2005; 2008).

Theoretically, the core interest has been the importance of social integration for immigrants' labor market integration. Social integration is generally conceptualized in terms of bridging and bonding social capital (Putnam 2000); two concepts that emphasize the potential usefulness of social ties that span or do not span group divisions, such as ethnic boundaries. More unique information is expected to follow from social ties that span ethnic divisions – i.e. bridging social capital, for example between a Dutch native and an immigrant. Ties that do not span – i.e. bonding social capital – are argued to be a better source of solidarity and social support (Coleman 1988), but do not necessarily provide unique information. Empirical research supports the notion that inter-ethnic ties in the form of contact with natives or ethnic majority members improve immigrants' labor market outcomes such as avoiding poverty, employment probabilities, occupational status, and income (e.g. Heizmann and Böhnke 2016; Kanas et al. 2012; Lancee 2010; 2012). Little evidence has been found that supports the no-

tion that intra-ethnic ties further immigrants' labor market outcomes (e.g. Kanas et al. 2012; Lancee 2010; 2016). However, the studies in this stream of research have so far not investigated benefit receipt specifically. This also holds true for the few studies that try to explain native-immigrant differences in labor market outcomes using social resources, which typically have focused on unemployment periods without specifically addressing whether people receive benefits (Auer, Bonoli, and Fossati 2017; Lancee and Hartung 2012).

Other scholars hailing from a slightly different but related line of research, have argued that intra-ethnic ties or so-called migrant networks may in certain circumstances be beneficial for (recently-arrived) immigrants (Kalter and Kogan 2014). One example of this is the ethnic enclave argument (Portes and Stepick 1985; Portes and Jensen 1989; Portes and Shafer 2007; Wilson and Portes 1980). At its core, this argument supposes that immigrants draw advantages from living in an ethnic enclave – understood as a spatial concentration of an ethnic group – because they have more opportunities to establish their own businesses, and because they can obtain jobs through co-ethnics with a smaller (or no) penalty to their country-of-origin human capital than in the main 'native-dominated' labor market. However, this presumed mechanism has also been criticized: Ethnic enclaves may not be that efficient in furthering career development and earnings growth, especially for employees, because the enclaves mainly offer 'dead-end', lower paying jobs with few possibilities for career advancement (Sanders and Nee 1987; Xie and Gough 2011). It is important to note that the ethnic enclave argument stem from the US where ethnic enclaves are more pronounced than in many European countries. Nevertheless, the argument emphasizes the potentially positive impact of the co-ethnic community and the characteristics thereof, on immigrants' labor market integration.

A recent number of empirical studies from the US and Europe has tested these theoretical notions using natural experiments for recently arrived refugees that are exogenously spread out across the country. In general, they find little support for the positive impact of ethnic enclaves – defined as the population share of co-ethnics or co-nationals – on refugees' subsequent labor market integration (e.g. Beaman 2012; Damm 2009; 2014; Edin, Fredriksson, and Åslund 2003; Martén, Hainmueller, and Hangartner 2019). Rather, it seems that the socio-economic characteristics of the co-ethnics – e.g. their employment rate – matter more. As such, these studies point to the importance of looking not only at the ethnic background of network members,

but also at their socio-economic characteristics – e.g. being employed – in explaining immigrants' labor market outcomes.

1.2.2 Research questions

As is clear from the literature review in the previous section, we may speak of largely disparate streams of research: One chiefly economical focusing on the 'social influence' of networks on benefit receipt, and two sociological in which one focuses on the impact of social resources or social capital on labor market outcomes generally, and one on social resources and social integration specifically for immigrants.

In the first part of this dissertation, we combine the insights from these disparate yet interrelated streams of research to improve our knowledge of how social networks affect benefit receipt. This is the chief contribution of chapters 2 and 3 where we directly juxtapose, on the one hand, the influence of benefit recipients in one's network on benefit receipt, with on the other hand, the influence of social resources on benefit receipt. In relation to the economic 'social influence' literature, this means that we advance a broader theoretical model: Individuals' benefit receipt is not only determined by benefit-receiving network members, but also by their access to labor-market enhancing social resources. This means that we investigate the relative importance of two interrelated, yet different network effects: One related to welfare-related information and social stigma, and the other to labor-market enhancing resources and information. Our argument is that these network effects are complementary, and together will increase our understanding of how individuals' social networks affect benefit receipt.

In relation to the sociological 'social resource' literature, we investigate benefit receipt explicitly as an outcome – to our knowledge, hitherto absent in this body of research. While unemployment has been studied as an outcome in this tradition, this is arguably different from benefit receipt. Benefit receipt is subject to an application process and is determined by eligibility. Eligibility is often dependent on (1) previous labor market attachment; (2) meeting obligations and requirements during receipt; and, in some instances, (3) the earnings and wealth of other household members. Whereas unemployment is the 'opposite' of having a job[5], benefit receipt highlights one

05 In a narrow sense, unemployment is defined as people without a job but actively looking for one at least according to most official statistics. More broadly, 'unemployment' may include people who for various reasons decide to (temporarily) not look for a job but for instance take care of the household and/or children. This broader definition encompasses people who depend on others to get by.

of the strategies individuals can use to get by and achieve some income security when they do not have sufficient earnings. The first general research question is therefore:

To what extent do characteristics of social networks increase and/or decrease individuals' likelihood of benefit receipt?

A second focus of this dissertation concerns immigrants' likelihood of benefit receipt and subsequent integration into the labor market. Specifically, the empirical chapters go from general to specific in terms of the study population: We start by examining the relationship in the general population (chapter 2), move on to differences between natives and the four major migrant groups in the Netherlands (chapter 3), before zooming in on the refugee population (chapter 4 and 5). This means that we can investigate the role played by characteristics of social networks in benefit receipt generally, and more specifically in immigrants and refugees' use of and exits from social security schemes, which is indicative of their labor market integration. In relation to previous studies into immigrants' limited labor market integration and the role of social networks, we are, to our knowledge, the first to investigate benefit receipt specifically as an outcome.

Further, there are several reasons why refugees are particularly interesting. First, refugees arrive in their new country with few existing social ties (see Andersson, Musterd, and Galster 2019), and have very different reasons for migrating compared to other types of migrants such as family or labor migrants. This is evident when we compare refugees to the four immigrant groups studied in chapter 3 – i.e. Turkish, Moroccan, Antillean, and Surinamese – that originally, mostly arrived as guest workers or inhabitants of (former) Dutch colonies. Second, most refugees in the Netherlands start out receiving social assistance benefits (Statistics Netherlands 2017), mainly because very few are able to find work straightaway. Their labor market integration improves over time, but it is still poor compared to natives (Bakker, Dagevos, and Engbersen 2017; Engbersen et al. 2015). From a societal point of view, it is therefore relevant to study refugees' exits from social assistance (chapter 4) as well as their initial steps on the labor market (chapter 5). Third, we zoom in on a period in which the Dutch settlement policy placed refugees exogenously in their first municipality. As we explain in more detail in section 1.4.3, this natural or quasi-experimental setting means that we are able to reduce bias stemming from self-selection into contexts. Hence, we can obtain stronger causal evidence on the importance of characteristics of the neighborhood context than in the empirical approaches in chapter 2 and 3. Moreover, our research expands on a growing number of studies stemming chiefly from Scandinavian coun-

tries (Åslund and Fredriksson 2009; Beaman 2012; Damm 2009; 2014; Markussen and Røed 2015; Rege, Telle, and Votruba 2012), by adding evidence from the Netherlands.

In sum, the second objective of this dissertation is to study the role of social networks in immigrants' benefit receipt, and refugees' exits from benefits and subsequent income development. Hence, our second general research question reads:

How do characteristics of social networks affect immigrants' benefit receipt and income development after benefit receipt?

1.3 Benefit Receipt and the Dutch Welfare State

In the Netherlands, various social benefit programs exist that either supplement individuals' income in specific instances – such as housing or health insurance benefits – or are meant to offer income security. In this dissertation, we focus on the latter programs in as far as they relate to the working-age population. Specifically, this boils down to three major benefit programs: Unemployment insurance ('*werkloosheidsuitkering*'), long-term disability insurance ('*arbeidsongeschiktheidsuitkering*'), and social assistance benefits ('*bijstandsuitkering*').Within the welfare state typology, the Dutch welfare state is often designated a 'hybrid' (Ferragina and Seeleib-Kaiser 2011; Vrooman 2012) – with elements of corporatist, liberal, and social-democratic regimes. The programs we focus on to some extent embody these three elements.

Unemployment- and disability-insurance benefits are organized as contribution-based social insurance schemes, typical of corporatist welfare states. All employees are required to participate. The benefits are administered by the central government through the Employee Insurance Agency ('*Uitvoeringsinstituut Werknemersverzekeringen*' or UWV). Eligibility is contingent on previous contributions, in addition to job loss or poor health. For unemployment benefits the duration is determined by the number of months employed (and contributed) in the past. The amount of benefits received is determined by previous earnings, as is also the case with disability benefits. Both schemes can be received partially. For disability, this involves that people can be declared partially disabled, meaning they have to work or find other means of supplementing their partial disability benefits. In the case of unemployment, this means that a person who for instance is forced to work less hours could be entitled to a partial unemployment benefit.

Social assistance is organized as a universal, means-tested household-level benefit, which exemplifies both the social-democratic (universal) and liberal (means-testing) influences on the Dutch welfare state. The scheme is administered by the municipal

authorities, and, in contrast to unemployment and disability benefits, applies to all who fall below a certain statutory minimum level of income. The means-testing also considers wealth and possessions, in addition to (lack of) income. The statutory minimum depends on the household composition, and the amount of benefits received is, in the absence of any other incomes, commensurate to this minimum level. It is important to note, however, that social assistance can also be received partially when the household has a level of income below the statutory minimum; for example to supplement a low-paying job or other social benefits.

In the empirical chapters, we study exits from social assistance for refugees in chapter 4, and refugees' subsequent income development in chapter 5. In chapters 2 and 3, however, we analyze the separate unemployment, disability, and social assistance schemes as a single outcome. There are some reasons why this makes sense in the Dutch case. As indicated above, different benefits may be combined. This could occur over time; for instance when a person loses his or her rights to unemployment benefits after a certain amount of time, but still has not found a new job and applies for social assistance. Alternatively, the separate schemes may be combined at a single point in time, as mentioned above. This blurs the lines between the different benefit schemes. Additionally, one might argue that the reasons for entering are different: Job loss in the case of unemployment benefits, health deficiencies in disability schemes, and indigence in social assistance. Empirical research, however, has shown that disability benefits include some 'hidden unemployment'. That is, job loss or unemployment may be a reason for entering disability receipt as a result of difficulties in assessing work capacity and degree of disability correctly (IBO 2017; Koning and van Vuuren 2007). This is not unique to the Netherlands (see Bratsberg, Fevang, and Røed 2013; Rege, Telle, and Votruba 2009). In a similar vein, starting to receive unemployment and social assistance benefits is often associated with mental and physical health problems (Muilwijk-Vriend et al. 2019), and social assistance recipients frequently report such impairments (Divosa 2011). Some people with serious health problems may also never qualify for disability benefits if they have not previously participated on the labor market, owing to the contribution-based nature of disability benefits in the Netherlands. As such, the reasons for entering the separate schemes are in practice not as distinct as they formally may seem.

1.4 Research Design and Data

In this section, we provide an overview of the empirical and analytical approach we employ. The chief novelty of our approach is that we combine individual-level survey and administrative data in chapters 2 and 3. As will be clear, this combination offers some clear advantages. Additionally, we exploit a natural experimental setting involving the exogenous placement of refugees in chapters 4 and 5. Table 1.1 provides an overview of the data sources and other key characteristics of the empirical and analytical approach used in each chapter. We start out with an overview of the different data sources used in this dissertation, before discussing how we operationalize networks in the empirical chapters. We refer to the specific empirical chapters for more details about the data and analytical samples.

1.4.1 Data sources

The empirical chapters are mainly based on three data sources. Common to all chapters is that they use individual-level longitudinal administrative data in measuring the response variables. The administrative data stem from multiple linked registers known as the Social Statistical Database ('SSD')[6] that enables linkage across registers based on unique individual identifiers. The data contain information on several socioeconomic characteristics and basic demographics, including among other things individuals' major sources of income, yearly income, place of residence, and household situation. To measure the response variables, we draw on information on people's major source of income (chapters 2-4) and information on yearly tax returns (chapter 5). The information on people's major source of income means that we can identify the sources of individuals' income such as specific benefit schemes, employment, self-employment, or no income. In chapter 2 and 3, benefit receipt is defined as when people derive their major source of income from unemployment, disability, and social assistance benefits. In chapter 4, we investigate refugees exits from social assistance and into the labor market – i.e. when their major source of income changes. In the final chapter, we make use of information on people's yearly tax return to measure refugees' income from the labor market. We also combine that information with information on people's monthly source of income to reconstruct their monthly labor market income per months active

06 Under certain conditions, these microdata are accessible for scientific research. For further information: microdata@cbs.nl.

on the labor market per year. In the following, we describe more specifically how we combined and used the data sources in each of the empirical chapters.

In chapters 4 and 5, we employ only the administrative data and focus on refugees who settled and received their first regular housing during 1999-2009. To identify this group of refugees, we rely on (1) administrative individual-level data on the timing and main reason for migrating including asylum, together with (2) information from the Central Agency for the Reception of Asylum Seekers (COA) on the exact location of asylum-seeker centers and their opening and closing dates[7]. This enables the identification of people who are registered as asylum migrants, whether they have undergone a regular asylum application process, and the exact timing of leaving asylum-seeker centers. In chapter 4, we analyze data on all these refugees and investigate the timing of their transition into the labor market. In chapter 5, we focus on the income development for those refugees who were able to become active on the labor market.

Table 1.1. Overview of the data, and methods employed in each chapter.

Chapter	Data sources	Data structure	Sample selection	Method	Response variable
2	Longitudinal Internet Studies for the Social sciences (LISS) enriched with administrative data	Longitudinal – explanatory variables measured at *t-1* predict benefit receipt at *t*	Nationally representative sample, 18-65 year-olds	Pooled logistic regression	Benefit receipt = major source of income from social benefits
3	Survey Integration of Minorities (SIM) enriched with administrative data	Semi cross-sectional – explanatory variables measured at *t* predict benefit receipt at *t+1*	Nationally representative sample, of native Dutch, Turks, Moroccans, Antilleans and Surinamese;18-65 year-olds	Pooled OLS linear probability model	Benefit receipt = at least one month of benefit receipt in the two-year period after survey year
4	Administrative data	Longitudinal	Refugees; 25-55 year-olds	Multilevel discrete time event-history, linear probability model	Transition from social assistance to labor market
5	Administrative data	Longitudinal	Refugees; 25-55 year-olds	Multilevel linear growth-curve model	Yearly income divided by number of months active on labor market

07 We are indebted to COA and Mark Kattenberg (CPB) for obtaining these data.

In chapter 2, we make use of the LISS (Longitudinal Internet Survey for the Social sciences) panel administered by CentERdata (Tilburg University, the Netherlands) which covers a broad range of topics relevant for the social sciences. It is a longitudinal panel that started out with a national probability sample of 4,500 Dutch households containing 7,000 individuals in 2007 (Scherpenzeel and Das 2010)[8]. We use data covering the period 2008-2014 and information concerning respondents' health; social contacts and networks; and work and schooling. The survey data were linked to the individual-level longitudinal administrative data. Because some respondents did not allow their survey responses to be linked, the linkage was not possible for the entire sample[9]. By having longitudinal administrative information, we are able to separate the measurement of the explanatory and response variables. Specifically, the yearly and monthly explanatory variables are measured at *t-1* and predict the response variable – benefit receipt – at *t*.

In chapter 3, we make use of two waves of a cross-sectional survey – the Survey Integration of Minorities ('*Survey Integratie Minderheden*' or SIM) from 2006 and 2011 (Statistics Netherlands 2006; 2011)[10]. These data comprise stratified probability samples of Dutch natives and the four major ethnic minority groups in the Netherlands – Turkish, Moroccan, Antillean, and Surinamese – aged 15 or older. Ethnic minority is defined as first- or second-generation immigrants in accordance with the definition of Statistics Netherlands. This means either being born abroad with at least one foreign-born parent (first-generation), or born in the Netherlands with at least one foreign-born parent (second-generation). The data comprise 5,250 people in 2006, and 4,125 people in 2011. The data have also been enriched with longitudinal administrative data. This means that we, similarly to the enriched dataset employed in chapter 2, are also able to separately measure the explanatory variables (*t*) and the response variable (*t+1*), implying we predict future benefit receipt in the two years following the survey year. The combination of survey and administrative data in chapter 2 and 3 offer some unique advantages. By measuring benefit receipt using objective administrative information, we eliminate bias stemming from misreporting of benefit receipt in surveys (Bruckmeier, Müller, and Riphahn 2015; 2014; Meyer and Mittag 2015). As such, it represents a clear

08 More information about the LISS-panel can be found at: http://www.lissdata.nl.

09 Research from Germany on possible bias resulting from consenting to linkage of survey records, indicates that this produces little bias for responses related to benefit receipt and employment – in particular compared to other sources of bias (Sakshaug and Kreuter 2012).

10 We use the version of these data that are linked to individual-level administrative data. See the references and https://easy.dans.knaw.nl/ui/datasets/id/easy-dataset:40304 and https://easy.dans.knaw.nl/ui/datasets/id/easy-dataset:67677 for information on how to obtain the publicly available versions.

advantage compared to empirical approaches that only rely on survey self-reports of social benefits (e.g. Renema and Lubbers 2019).

Additionally, survey data are in general 'richer' than administrative data – meaning they contain more detailed information on a broader range of topics. As the name suggests, administrative registers are maintained for administrative purposes and therefore do not necessarily contain 'tailor-made' information in the same way a survey often does. For our purposes, this is most clear when it comes to measurements of social networks: The administrative data do not record actual ties between individuals, which surveys tend to do. We will elaborate on this point in detail in the next section.

Further, enriching survey and administrative data allows for the possibility to include 'time-lagged' measurements. By time-lagged, we mean that explanatory and response variables can be measured at different time points – an analytical strategy we employ in both chapters 2 and 3. Obviously, this design is only possible when at least one of the data sources is longitudinal. Separating the measurement of explanatory and response variables in time is advantageous: It allows for a stronger causal interpretation as cause precedes outcome, and reduces potential bias stemming from reversed causality. For instance, suppose that a researcher relies on (aggregate) neighborhood measures, e.g. the share of benefit recipients, in order to capture a network effect on the individual likelihood of benefit receipt. It may be, however, that certain individuals, in anticipation of benefit receipt, decide to move to a different, 'poorer' neighborhood that has a higher share of benefit recipients. When these variables are measured at the same time, the bias stemming from this process is likely to be higher than if they are measured at different time points. As such, the lagged approach partially reduce bias from self-selection.

1.4.2 Operationalizing networks

How to best measure social networks or, more precisely, empirically capture the theoretical mechanisms argued to emanate from personal relationships is a complicated issue. The measures we employ in this dissertation can be placed in two broad categories: A 'direct' and an 'indirect' one. These are closely intertwined with the data sources we use. In this section, the objective is to discuss overarching advantages and disadvantages associated with the different types of measures.

Direct measures capture who people have social contact or a personal relationship with. We employ two such measures: The core discussion network and social contact, both stemming from survey data. The core discussion network (Burt 1984; Marsden 1987) draws on a survey item prompting respondents to name up to five people with whom they have discussed important matters in the past six months. Respondents are then asked a series of questions – so-called name interpreter questions – about the

attributes of these people. The characteristics of these individuals, such as the share of employed contacts in the core network, are then used to indicate e.g. respondents' access to labor market information. The core discussion network and other name generators are common in the social network and social resources literature (see Flap and Völker 2013), and have been used in recent empirical research in the Netherlands (van Tubergen 2014; 2015). Yet, the measure has also received some criticism, particularly in the US (e.g. Bearman and Parigi 2004; Brashears 2011; Small 2013; Small, Deeds Pamphile, and McMahan 2015). One critique concerns the types of ties the measure captures. Some researchers have interpreted the ties as 'strong' or important ties, while it has been argued that the measure does not capture strong ties exclusively (Small 2013). People tend to also discuss important matters with others whom they are not emotionally close with, such as professionals, in turn casting some doubt on the presumed social significance of these discussion partners. However, we argue that, theoretically, it is debatable to presume that the strength of ties is that important – as advocated by Granovetter (1973) and subsequent researchers – relative to, for instance, contacts' socioeconomic characteristics. For example, Gee and colleagues have found that the hypothesized 'strength of weak ties' stems from the higher number of weak ties in people's network (Gee et al. 2017; Gee, Jones, and Burke 2017), running counter to the notion that one weak tie necessarily is better than one strong tie. Therefore, when we use this measure in this dissertation, we are less focused on the types of ties and more focused on the characteristics of people in the core discussion network.

Social contact measures are, in our case, used to indicate the frequency of social contact with people with a different or similar ethnic background. Respondents are asked to report how often – e.g. daily, weekly, monthly, or less often – they have contact with, for instance, native or co-ethnic friends and acquaintances. While they do not record actual social ties or the number of social ties as such[11], they nevertheless imply that the respondent maintains social ties with people with a certain (non-)immigrant background. As such, it can be considered a 'direct' measure. These types of measures have been used in studies that examine the role of social integration – understood as inter- and intra-ethnic social contact – on immigrants' labor market outcomes (e.g. Lancee 2010; 2012). Theoretically, these measures are linked to the concepts of bridging and bonding capital (Putnam 2000).

11 By a social tie, we here mean a specific relationship or a specific person as opposed to contact with others.

Indirect measures do not record the actual ties or social contact between people. Rather, they capture whether (a group of) individuals belong to a certain context, for instance neighborhood or voluntary associational membership, and social relations are assumed. The 'social influence' literature has by and large employed these kinds of measures for the neighborhood. To make it more plausible that the individual has some level of social contact with his or her fellow neighbors, one approach has been to draw on neighbors with the same national origin or who speak the same non-native language at home (e.g. Bertrand, Luttmer, and Mullainathan 2000). Another approach has been to also study other contexts. Markussen and Røed (2015), for instance, focus on former schoolmates who attended the same grade among which social contact is presumably more plausible. The advantage of using such indirect measures is the ability to draw on large population-level data. This is exemplified by researchers' ability to zoom in on groups that are relatively small and can be difficult to reach in survey research, such as refugee groups. Nevertheless, social ties among neighbors – or in the other contexts studied – are not directly observed, raising doubt on to what extent such measures can be interpreted as effects of social networks. Counter to this, there is some empirical evidence indicating that (1) neighborhoods are social contexts in which individuals get to know one another and sometimes represents communities (e.g. Völker, Flap, and Lindenberg 2007; Wellman and Wortley 1990); (2) attributes of neighborhoods – such as their ethnic composition – affect the composition of individuals' networks (e.g. van Tubergen and Volker 2015; Tulin, Volker, and Lancee 2019; Vervoort, Flap, and Dagevos 2011), and (3) neighbors do affect individuals' labor market participation (e.g. Pinkster 2009). Therefore, neighborhoods will to some extent reflect one part of individuals' social networks, and the neighborhood indicators employed in the empirical chapters are interpreted along these lines. We return to this issue in section 1.4.3.

We also use measures of participation in voluntary associations and their reported ethnic composition to indicate individuals' social networks and/or their access to social resources. The assumption is that participation in voluntary associations is associated with a larger and more resourceful network. There are some empirical studies that show the role of voluntary associations in enhancing individuals' social resources (Benton 2016; Wollebaek and Selle 2002). Moreover, empirical findings also suggest that voluntary associations play a role in job searching (Beggs and Hurlbert 1997), and can make for better labor market outcomes (Ruiter and de Graaf 2009). Still, participation in voluntary associations may also indicate human capital skills, or to some extent reflect personality traits (Tulin, Lancee, and Volker 2018).

1.4.3 Methods and causality

The previous two sections have provided an overview of the data and measurements of networks we use in the empirical chapters. It is also important to discuss how we use these in more detail, including issues of causality.

One central theme of this dissertation is how characteristics of social networks affect individuals' benefit receipt. In using the neighborhood as an indicator of networks, there are two interrelated problems worth discussing (see also Galster 2008) – setting aside the question of the extent to which neighborhoods indicate properties of individuals' networks[12]. First, we may either under- and/or overestimate the 'true' network effect of neighbors. Underestimation may arise because neighborhood measures are likely to capture neighbors that are a part of individuals' networks, as well as neighbors who are not. People who do not know one another would presumably not affect each other, at least not through social influence or social resources mechanisms[13]. This means that both people who are thought to affect the individual and people who are not are included in the measure. As such, the effect is averaged out over network members and non-network members, in which the latter have no effect but the former do – causing the estimated effect to be biased downward. Overestimation stems from a failure to account for confounding variables that affect or are correlated with both the neighborhood indicator – i.e. the concentration of benefit recipients – and the individual-level outcome – i.e. individuals' likelihood of benefit receipt. Examples of such confounders may be the general socioeconomic standing of the neighborhood or fluctuations in the overall economy. The estimated effect may therefore be an overestimate of the true network effect, because it is inflated by unobserved explanatory (neighborhood-level) variables.

Second, neighborhood effects are often marred by issues of self-selection. For our purposes, it means that individuals with a higher likelihood of benefit receipt – owing to unobserved individual characteristics – tend to end-up living or choose to live in neighborhoods that also have higher rates of benefit recipients.

In chapters 2 and 3, we employ control variables to try to tackle the issue of self-selection, which would to some extent also take into account possible confounding vari-

12 Social or network-related mechanisms are but one way through which neighborhoods can affect individual-level outcomes (see Galster 2012 for a theoretical overview).

13 Research focusing on how structural features of complete networks would, however, suggest that people whom an individual does not know – i.e. are not part of the individual's immediate social circle – can affect individuals' outcomes. One example is Burt's (2001) research into the importance of occupying brokering positions and structural holes, which are determined by a person's position in the whole network and thus people whom the person does not directly know, for reaping rewards on the labor market.

ables. Specifically, we use the value of residential housing, which has also been applied in previous research (van der Klaauw and van Ours 2003). Additionally, we measure the neighborhood variables and response variable at separate points in time thus making use of the longitudinal administrative data to complement survey measures. This partially reduces bias stemming from simultaneous selection into neighborhoods and an increased likelihood of benefit receipt.

Another solution to the selection issue clouding research into neighborhood effects is the use of natural experiments. This is precisely what we do in chapters 4 and 5 where we draw on a natural experiment of refugees that involved the exogenous placement of refugees in municipalities in the Netherlands. The policy applied to refugees – asylum migrants who were granted asylum – during 1999-2009, and involved placing refugees in their first regular housing. The exogenous placement meant that refugees had no choice in where they ended up, with a few notable exceptions that we can control for (see section 4.2.3 for more details). Municipalities are required to settle a number of refugees, which is determined by the size of its population. As such, initial placement in a given year should be uncorrelated with individual-level (unobserved) characteristics, and the characteristics of the municipality do not affect the location of refugees' first housing. This brings clear advantages when it comes to estimating the effect of the initial neighborhood on later labor market outcomes, but it does not actually capture the underlying network mechanism: We still rely on indirect measures from administrative data.

Benefit receipt is often defined and operationalized as a binary or dichotomous outcome – either the individual receives benefits, or the individual does not. How to treat these types of outcomes statistically deserves some more attention.

Commonly, sociologists tend to prefer non-linear regression models such as logistic regression[14]. Recently, logistic regression has come under scrutiny and in particular the use of log-odds and odds ratios in interpreting the estimated effects based on it (Hellevik 2009; Mood 2010a; Norton 2012; Karaca-Mandic, Norton, and Dowd 2012). One problem relates to omitted variable bias and unobserved heterogeneity (Mood 2010a). This problem affects the estimation of odds ratios, making it difficult to, for instance, compare changes in coefficients across different models estimated on the same sample, or compare groups within the same sample. A second problem in logistic regression is that interaction effects are difficult to interpret (Ai and Norton 2003). One aspect of this is that even the main effect of one explanatory variable depends on the values of

14 Economists have typically favored probit regression over logistic regression, but have also routinely employed linear probability models as well.

other explanatory variables, which is averaged out in odds ratios. Another aspect relates to the shape of the effect: A main effect follows the s-shape of the logistic distribution and thus changes at different points in this distribution, meaning an interaction effect specifies a change in this s-shape.

To circumvent these problems, we draw on average marginal effects as estimated from logistic regressions in chapter 2, and linear probability models in chapters 3 and 4. These are acceptable to solutions to the problems described above (e.g. Hellevik 2009; Mood 2010b), and estimate the average effect of an explanatory variable on the probability of scoring 1 on the outcome variable. Both approaches yield estimates that are easy to interpret, and in particular linear probability models have a straightforward interpretation of interaction effects. However, there are two common criticism raised against linear probability models: (1) They may yield unrealistic predictions outside of the 0-1 range; and (2) heteroscedasticity and incorrect calculation of standard errors. We solved these by inspecting the predicted values, which by and large were inside the 0-1 range; and estimated the models with robust standard errors.

1.5 Overview of Empirical Chapters

1.5.1 From relationships to receipt: Social networks, precariousness and benefit receipt (Chapter 2)

In chapter 2, we pose two research questions related to social networks and benefit receipt. First, we investigate (1) the role of benefit-receiving network members that are argued to *increase* individuals' likelihood of benefit receipt, while simultaneously investigating (2) the role of social resources that are argued to *decrease* individuals' chances of benefit receipt. As outlined in section 1.2, these aspects capture two different effects that can flow through individuals' networks. Second, we ask to what extent these network effects on benefit receipt are affected by whether individuals hold a precarious labor market position.

Analyses of longitudinal survey data (LISS) that are enriched with administrative data show that a higher concentration of benefit recipients in the neighborhood increase individuals' chances of benefit receipt. At the same time, better access to social resources – e.g. in the form of more employed people in the core discussion network – decreases individuals' likelihood of benefit receipt. It seems that the relative impact of these two aspects are similar. This is in line with the notion that in understanding

the influence of social networks on benefit receipt, it is important to consider a broader range of network influences as these are at work simultaneously.

Additionally, we find no evidence to support the idea that precariousness or a vulnerable labor market position makes people rely on their social networks more, thus amplifying the effects of social networks on benefit receipt. In fact, social networks are found to exert the same influence irrespective of whether the person has either a low income or a non-standard work arrangement, or whether the person has a poor health.

Core findings:

- Net of each other, the share of benefit recipients in the neighborhood increases while access to social resources decreases individuals' probability of benefit receipt.
- The influence of social networks on benefit receipt is not affected by whether individuals are in a precarious labor market position.

1.5.2 Social benefits among immigrants and natives in the Netherlands: The role of social capital (Chapter 3)

In chapter 3, we examine the impact of characteristics of social networks on benefit receipt for natives vis-à-vis first- and second-generation immigrants. Specifically, we investigate how (1) inter- and intra-ethnic social contact, and (2) benefit recipients among the same ethnic in-group in the neighborhood affect the chances of benefit receipt differently for natives, and first- and second-generation immigrants. Additionally, we study to what extent these indicators of social networks can explain the native-immigrant differential in benefit receipt.

The SIM data contain information on Dutch natives, and the four major immigrant groups in the Netherlands – people with a Turkish, Moroccan, Antillean, or Surinamese origin. The analyses reveal that contact with native Dutch friends or acquaintances is associated with lower chances of future benefit receipt. In other words, immigrants' inter-ethnic contact and natives' intra-ethnic contact make for less benefit receipt. Interestingly, this only pertains to general contact with friends and acquaintances rather than contact through voluntary associations, which do not affect natives nor immigrants' likelihood of benefit receipt. The findings support the notion that contact with natives improves social resources.

Furthermore, we find that the share of benefit recipients among the ethnic in-group in the neighborhood is associated with a higher likelihood of benefit receipt. Overall, this effect does not differ between natives and immigrants. As such, our findings do

not support the idea that the normative and informational mechanisms – thought to drive the effect of benefit recipients in the network on individuals' likelihood of benefit receipt – work differently for natives and immigrants.

Finally, we find that it is mainly the share of benefit recipients among the ethnic in-group in the neighborhood that explains the native-immigrant differential in overall benefit receipt. Although social contact affects benefit receipt, it does not explain away the differential in benefit receipt between natives, and first- and second-generation immigrants. After accounting for several explanatory factors, the native-immigrant differential in benefit receipt is the largest for Turkish and Moroccan first- and second-generation immigrants, and for first-generation Antilleans.

Core findings:

- Contact with natives is associated with a *lower* likelihood of benefit receipt, while the share of benefit recipients among co-ethnics in the neighborhood is associated with a *higher* likelihood of benefit receipt.
- The share of benefit recipients among the ethnic in-group partially explains away differences in benefit receipt between natives and immigrants, while social contact with natives does not.

1.5.3 Refugees and the transition from welfare to work: A quasi-experimental approach of the impact of the neighborhood context (Chapter 4)

Chapter 4 and 5 focus on refugees and rely on the exogenous placement of refugees as a natural experiment to obtain better causal evidence on the impact of neighborhoods than chapters 2 and 3 (see section 1.4.3 and chapters 4 and 5 for more details). Chapter 4 deals with the impact of characteristics of the initial neighborhood on refugees' transitions from social assistance to work in the Netherlands. In particular, the chapter has two main objectives. First, we investigate the influence of characteristics – employment rate and median level of income – pertaining to co-ethnics and natives in the initial neighborhood refugees are placed in on their chances of entering the labor market. Second, we investigate to what extent the influence of these characteristics among natives depends on the share of co-ethnics in the area.

Employing individual-level administrative data, we find that the employment rate among co-ethnics as well as among natives make for a higher likelihood of transitioning into the labor market. However, the median income among co-ethnics and natives

does not make refugees more likely to enter the labor market. In fact, a higher level of income among natives makes refugees less likely to exit social assistance and enter the labor market. Overall, our results supports the idea that refugees' chances of labor market entry are affected by the environment they first settle in, possibly because they are able to draw on the social resources of their employed neighbors.

Further, we do *not* find that the importance of the characteristics among natives differ depending on the share of co-ethnics in the area. Hence, the results do not lend support to the idea that refugees are more likely to turn to natives as sources of social resources when there are less contact opportunities with co-ethnics.

Core findings:

- A higher employment rate among co-ethnics and natives makes refugees more likely to make a transition from social assistance into the labor market
- The median income among co-ethnics has no effect and the median income among natives has a negative effect on the transition probability.
- The influences of the employment rate and the level of income among natives in the neighborhood are not more (nor less) important depending on the size of the co-ethnic community.

1.5.4 The income development of working refugees in the Netherlands: Does initial context matter? (Chapter 5)

In the final chapter, we expand upon the previous analyses on refugees in two ways. First, we look at the influence of characteristics of the initial neighborhood for refugees' income development following labor market entry. Specifically, we investigate the employment rate and level of median income among co-ethnics and natives in refugees' first neighborhood for their initial and subsequent labor market income. Investigating the development of labor market income reveals important insights into the permanence of social assistance exits for refugees. Second, we also explicitly take into account residential mobility, with the caveat that while initial placement is exogenous in terms of refugees' unmeasured characteristics, moving to a different neighborhood or municipality may be the result of (un)observed characteristics.

The analyses reveal that refugees on average experience an income growth after entering the labor market, although there is quite some variation in their initial earnings and subsequent growth. Further, characteristics of the initial neighborhood, overall, do not affect initial income levels nor subsequent income growth. This means that the effects found in chapter 4 regarding e.g. the effect of the employment rate among

co-ethnics, do not hold for the later income or career development. Rather, these effects are confined to the initial steps into the labor market. These findings thus lend support to the idea that there are limits to the advantages of ethnic enclaves: Co-ethnics may help in overcoming the initial hurdle into the labor market but do not matter for further career development once the initial step has been taken.

It seems that, overall, length of stay in the initial municipality does not impact the effect of the characteristics of the initial placement on income development. This speaks against the notion that the longer refugees stay in their initial neighborhood, the stronger their social integration which could in turn strengthen the impact of the characteristics of the initial neighborhood on income development. The results also do not imply a general advantage of moving elsewhere, as we found no direct effect of length of stay on income development.

Core findings:

- Refugees experience on average an increase in their monthly income after first entering the labor market.
- Once active on the labor market, the characteristics of the first area refugees were placed in do not affect initial income nor income growth.
- Overall, length of stay in the initial municipality does not impact the influence of characteristics of the neighborhood on income development.

1.6 Conclusions, Limitations and Implications

1.6.1 Discussion and conclusions

This dissertation investigated to what extent characteristics of social networks increase and decrease individuals' benefit receipt. Additionally, we studied the role of social networks in explaining native-immigrant differences in benefit receipt and refugees' exits from benefits and subsequent income development.

The first overall conclusion to emerge from this dissertation is that individuals make use of two aspects of their social networks that both are important in determining their benefit receipt. One aspect concerns welfare-related information or norms, while the other concerns labor-market-related information. The former – e.g. the presence of benefit recipients in the neighborhood – increase, whereas the latter – e.g. social resources in individuals' core discussion networks – decrease individuals' chances of benefit receipt. We present evidence that both aspects affect benefit receipt when the impact of the other is taken into account. Theoretically, it thus seems that individuals

make use of the social resources available to them through the network and are simultaneously subject to social influences from network members in decisions related to benefit receipt. These decisions may involve weighing the various options available to an individual in securing a livelihood, in which (temporarily) relying on benefits and chances of attaining a new job quickly are examples of such options. Deliberating about these issues could arise in the face of an anticipated or a sudden job loss, or worsening health. Alternatively, pursuing and ultimately securing a permanent high-paying job would dramatically decrease the future need for income security in the form of social benefits. In these decisions, it may be that individuals draw on the welfare-related information and/or the labor-market-related information available to them. It could also be that norms and perceived social costs play a part: Either in the form of personal stigma or perceived stigmatization from others, or in the form of a stronger work ethos – for instance as a result of a higher number of employed individuals in the core network (see chapter 2, table 2.2).

Our findings do not offer clues as to which of these underlying mechanism – information or norms – are more important. This is also not clear from previous research. For instance, Aizer and Currie (2004) find evidence less consistent with information sharing in the utilization of government-financed prenatal care among mothers in California, drawing on administrative data and using neighborhood measures of networks. It is not clear, however, how applicable these findings are to major cash benefits, which we study here. Using self-reported data, Baumberg (2016) finds that stigmatization of benefit use is *more* prevalent in areas with a higher concentration of benefit receipt, which runs counter to the notion of normalization among benefit recipients. As such, there are inconsistent findings on the relative importance of the informational versus normative mechanisms, and more research is needed to assess their magnitude. Our dissertation adds to this the need to investigate different albeit interrelated types of information and norms related to (1) the welfare state and benefit use, and (2) the labor market and work in understanding the influence of social network characteristics on benefit receipt.

The second overall conclusion concerns the importance of social contact and the possible social resources derived from such contact. We find that social contact with natives is associated with less benefit receipt for both natives and first- and second-generation immigrants. Social contact with co-ethnics does not seem to matter for benefit receipt (chapter 3), nor do contact with co-ethnics in the form of a higher concentration of co-ethnics seem to matter for refugees' labor market entry (chapter 4) and refugees' income development (chapter 5). Moreover, it does not seem that social contact explains the native-immigrant difference in benefit receipt. Theoretically,

these findings suggest that social resources in the form of host-country knowledge and labor market information obtained from natives are important in determining natives' and immigrants' benefit receipt. Social contact with co-ethnics, however, does not offer such advantages. Still, the importance of social resources derived from social contact with natives is qualified by the finding that it cannot explain the relative higher chance of benefit receipt among first- and second-generation immigrants vis-à-vis natives. As such, it does not seem to tap into the social resources that could contribute to the differences between natives and immigrants in benefit receipt.

The third main conclusion we draw is that the socioeconomic characteristics of network members are important. They affect immigrants' benefit receipt and, in contrast with social contact, can partially explain the native-immigrant differential in benefit receipt. This is reinforced by the more causal evidence provided for refugees, in which the employment rate among co-ethnics and natives affects their labor market entry – assuming this evidence is generalizable to the whole immigrant population. Taken together with the second conclusion, these findings could signify the relative unimportance of bridging and bonding social capital as envisioned by Putnam (2000) and employed in studies into labor market outcomes in general (e.g. Heizmann and Böhnke 2016; Lancee 2010). Social integration in the form of social contact with natives may of course be important for practicing host-country language, but, overall, cannot really explain native-immigrant differences in benefit receipt. Therefore, it seems more important to consider the actual socioeconomic behavior of network members, such as whether they are employed, rather than their ethnic background to be able to capture immigrants' access to social resources.

The fourth conclusion concerns the role of characteristics of initial placement for refugees' labor market integration. We find that these characteristics affect the transition from social assistance into the labor market, but not subsequent income development. In particular, placement in areas in which both natives and co-ethnics are more employed seems crucial in facilitating refugees' labor market integration. As such, our results imply that social resources from co-ethnics and natives are helpful for labor market entry and exits from social assistance, but not for subsequent career development. In other words, they are important for gaining a foothold on the labor market and getting off social assistance, but unimportant for long-term success on the labor market. Theoretically, these results could be driven by better access to social resources in these neighborhoods – in the sense of more contact with employed co-ethnics and natives that offer valuable labor-market-related information. In terms of the role of characteristics of the co-ethnics, there are arguments suggesting both a positive and a limited or negative role (see Kalter and Kogan 2014). The 'positive' view

is exemplified by arguments stemming from the ethnic enclave literature (e.g. Portes and Shafer 2007; Wilson and Portes 1980) that, among other things, emphasize the role of co-ethnics and opportunities for starting a business and employment in co-ethnic owned companies as a way to cushion penalties in the main, native-dominated labor market. Yet, co-ethnics and intra-ethnic ties may represent a trap for immigrants, as they only provide access to low-paying jobs (e.g. Sanders and Nee 1987), or little new information and ideas flow through these ties compared to inter-ethnic or native ties as suggested by the distinction between bridging and bonding social capital (Putnam 2000). In this regard, the finding that subsequent income development is unaffected is interesting: It implies a limited role of co-ethnics and natives in facilitating further income security for this group. The ability of refugees to obtain a higher labor market income following social assistance receipt is linked to their likelihood of relying on benefits in the future.

As our evidence on refugees is based on a natural experiment, it offers stronger causal evidence with respect to the neighborhood indicators, which we employ throughout this dissertation. In particular, bias from self-selection into certain neighborhoods is minimized. Still, to what extent the neighborhood effects are (only) driven by networks, or whether they are indicative of the broader (socio-)economic or labor market context these refugees encounter is uncertain. As pointed out in chapter 5, we may argue that particularly the employment rate or median income among natives in the initial neighborhood is more indicative of other explanatory factors as well, including the economic circumstances refugees are placed in. Hence, when we take the characteristics of natives into account, it is more plausible that the characteristics of co-ethnics are more indicative of a network effect. This is backed-up by empirical research showing a large share of co-ethnics in refugees' network (see van Doorn 2011 for Dutch evidence), for which presumably co-ethnics in the immediate vicinity of refugees' first regular housing in particular constitute a significant proportion. Nonetheless, we do not observe the social ties empirically, implying (1) we only capture one part – the neighborhood – of refugees' social environment that in turn could affect their chances of relying on benefits; and (2) we underestimate the true impact of the employed social contacts in this area because the observed impact is averaged over people with whom the refugee has no social contact. These caveats are not limited to the results for refugees, but also applies to the other results that rely on neighborhoods as an indicator of networks. Overall,

we conclude that initial placement matters for refugees' entry into the labor market, and this impact is likely in part driven by social-network mechanisms.

1.6.2 Limitations and future research

This dissertation is not without limitations. First, benefit receipt is often conceptualized as a complex process involving the decision to apply and eventually being granted a benefit (van Oorschot 1994) that includes two interrelated processes at the individual level: Becoming eligible, and deciding to apply. In these processes, the role of policy design and the administration of benefits (see van Mechelen and Janssens 2017), in addition to characteristics of the individuals, are considered important. With the data at our disposal, we have not been able to delve into these processes in more detail empirically. We partially capture this complexity in chapter 2, in which we focus on precariousness and vulnerability on the labor market as one way of capturing the likelihood of being eligible in the (near) future. The theoretical argument is that this would make individuals more likely to utilize the resources available to them and more subject to their networks' social influence, compared to people in a secure labor market position. We find *no* support for this argument. However, future research could extend our knowledge of how social networks affect benefit receipt by taking into account the role of both welfare-related information and stigma, and labor-market-enhancing social resources on certain steps in the benefit receipt process. This exercise may include separating between becoming eligible on the one hand and applying for benefit on the other. Yet, it is important to consider these as not necessarily distinct processes in which one follows from the other over time. Rather, we posit that these are linked and may be occurring simultaneously: Individuals may routinely judge their options in this process and probe their network for advice. Future research could study this process by collecting and analyzing more detailed data. Such data could include direct measures of the proposed theoretical mechanisms, including informational and normative aspects related to both welfare and the labor market (see Lin and Ao 2008).

Second, we have not considered the impact of benefit receipt on social networks. It is likely that benefit receipt – either because it is stigmatizing or because it is associated with a lower income – makes it difficult to participate in certain social activities. Some clues as to the effects of low incomes on networks are provided by Böhnke and Link (2017), who study the impact of poverty on social networks using longitudinal data from Germany. They find that the core elements of networks – i.e. family and close friends – remain unchanged, whereas a decline in overall network size, resource availability, and frequency of contact is observed in the face of poverty (see Mood and Jonsson 2016 for Swedish evidence). Although related to poverty, benefit receipt should not

be equated with it[15]. Moreover, it is sometimes argued that benefit-receiving network members increase the individuals' likelihood of benefit receipt because the perceived value of leisure time increases (see Rege, Telle, and Votruba 2012 in terms of disability benefits). Given the increase in leisure time associated with benefit receipt, having more people to spend that extra time with would make benefit receipt more attractive. According to this argument, benefit receipt should make for (1) an increase in the number of benefit recipients in the network; and/or (2) a higher contact frequency with these network members. Future research could investigate to what extent this is in fact the case. A possible confounder would be whether individuals are required to attend reemployment programs that (unintentionally) bring benefit recipients together and could serve as a focus (Feld 1981).

Third, future research in welfare states that differ from the Dutch one is needed. Social benefit schemes differ greatly across countries (Ferragina and Seeleib-Kaiser 2011; Vrooman 2012), for instance in terms of who are covered, schemes' generosity and application procedure, and who – employers, local government, or national government – are in charge of administering the benefits. Inherent in typologies of the welfare state is a division of responsibility in terms of individuals, local communities, markets, families, and the state. This division may in turn have consequences for the overall role of social networks as well as specific parts of the network. For example, family relationships are probably more important in providing social security and support in countries that ascribe a small role to the state in providing welfare for their citizens. In welfare states ascribing a larger role to the state, the overall importance of individuals' networks may be smaller, although friends and acquaintances may be an important source of information and advice on how to deal with the authorities.

1.6.3 Policy implications

Our results underline the importance of placing refugees in areas where the employment rate is high. While such settlement policies are likely to be effective in promoting refugees' entry to the labor market, the policies probably will not further refugees' income development and career success. The latter seem to require a different set of policy interventions. Additionally, ethnic residential segregation has often been put forward as detrimental to immigrants' labor market integration. In the case of refu-

15 One reason is that some benefit schemes ensures that recipients receive a certain part of their former incomes, which may or may not involve incomes below a given poverty line.

gees, these worries may be overstated, as refugees placed in areas with relatively more co-nationals are neither worse nor better off.

We show that some elements of the network increase while others decrease individuals' likelihood of benefit receipt, and this 'duality' arguably has implications for future policy. In studying the effects of governments' social investment policies (Hemerijck 2018), such as labor market activation measures that increase employment or reeducation programs that enhances people's human capital, scholars have suggested that their estimated consequences are difficult to pinpoint because they frequently do not capture 'social multiplier' effects (Rege, Telle, and Votruba 2012). These effects arise from additional, 'indirect' effects running through individuals' networks – i.e. when policy helps one person get off welfare, it simultaneously increases the chance that his or her friend also will get off in the future. Our results suggest that these social multiplier effects could be larger than previously thought, because they are driven by both welfare-related and labor-market-related network influences.

It is also important to consider the overall role of characteristics of social networks for the phenomena and groups we have studied here. We have seen that they are important for the benefit receipt in the general population; affect immigrants' benefit receipt and can partially explain their relatively higher chances of benefit receipt vis-à-vis the native population; and can play a part in explaining refugees' entry to the labor market but not subsequent success. Overall, however, the magnitude and substantive impact of network characteristics are at par or lower than that of other explanatory factors, such as education, age and household characteristics. Despite their limited impact, individuals' networks may still be important, because (1) networks can give raise to the social multiplier effects discussed earlier; and (2) networks may shape other elements of people's lives and psychology.

This second point may include people's sense of belonging and exclusion, whether they are able to lead the lives they want, and how they think about the solidarity and protection offered by the welfare state and society at large. These elements may not directly affect whether people depend on benefits but can be decisive for the quality of life experienced by benefit recipients.

Chapter 2.

From Relationships to Receipt: Social Networks, Precariousness and Benefit Receipt[16]

16 A slightly different version of this chapter has been submitted to an international scientific journal. This chapter was co-authored by I. Maas and J. C. Vrooman. Kristiansen wrote the main part of the manuscript and conducted the analyses. Maas and Vrooman contributed substantially to the manuscript. The authors jointly developed the idea and research design, in the context of the project From network to work? at Utrecht University. An earlier version of this chapter was presented at the RC28 summer meeting in Bern, 2016, and at the Migration and Social Stratification seminar at Utrecht University.

Abstract

This chapter investigates the impact of social networks on the probability of receiving social benefits in the Netherlands. We add to previous research by simultaneously considering the concentration of benefit recipients in one's social environment and investigating access to social resources. Furthermore, we hypothesize that whether individuals are in a precarious labor market situation has implications for the way they make use of their social networks, which in turn affects their chance to receive a benefit. We employ a unique combination of longitudinal administrative and survey data and analyze these using pooled logistic regressions. The results indicate that access to social resources decrease the chance of benefit receipt, while benefit recipients in the social environment increase the chance of benefit receipt. There is, however, no evidence that precariousness affects the influence of social networks on benefit receipt.

2.1 Introduction

There is a longstanding sociological research interest in how an individual's social capital and access to social resources shape inequality on the labor market (Bonoli and Turtschi 2015; Chen and Volker 2016; Lin 1999; 2000; Lin and Erickson 2008; McDonald et al. 2013). The main focus in this line of research is to what extent and how the access to and/or the mobilization of social resources affects earnings, occupational status, and employment (see, e.g., Lin 1999; McDonald et al. 2013). The core argument states that social resources enhance labor market outcomes, because individuals gain, among other things, access to valuable labor-market-related information. Although there has been some controversy surrounding the causality underlying this relationship (Mouw 2003), empirical results generally suggest a positive relationship between individuals' social resources and their labor market outcomes (Chen and Volker 2016).

In this study, we aim to extend this line of research by focusing explicitly on the receipt of unemployment, social assistance, and disability benefits. So far, most sociological studies in this line of research have not examined benefit receipt, but rather the related outcome of unemployment. As an outcome, however, being on social benefits differs from unemployment in two ways. First, unemployment is but one of the reasons for receiving a benefit. Second, research has indicated that the decision to claim social benefits is not determined by economic incentives alone, and that a substantial part of the eligible population does not apply for the benefits they are entitled to (Bargain, Immervoll, and Viitamäki 2012; Matsaganis, Levy, and Flevotomou 2010; van Oorschot 1994). This implies that benefit receipt is not a straightforward corollary of unemployment.

Another stream of studies – mainly originating in economics – has investigated the influence of individuals' social networks on benefit receipt (Åslund and Fredriksson 2009; Bertrand, Luttmer, and Mullainathan 2000; Furtado and Theodoropoulos 2016; Mood 2004; 2010b; Rege, Telle, and Votruba 2012). The key argument suggests that social networks can facilitate benefit dependence, because they may contain people who are knowledgeable about how to apply for a benefit. Mainly operationalizing social networks as neighborhoods (but see Markussen and Røed 2015), these studies find a positive relationship between the concentration of benefit recipients in the network and individual benefit receipt. These chiefly economic studies pay little attention to the possibility that other characteristics of the social network, i.e. social capital as studied by sociologists, may actually *lower* an individual's likelihood to receive a social benefit.

We contribute to earlier research by explicitly theorizing on characteristics of the social network that may (1) increase, and (2) decrease the likelihood of benefit receipt.

These characteristics are negatively correlated: Networks with many social resources that enhance labor market success, usually contain few benefit recipients. As we argue below, however, they work through two distinct mechanisms. Studies that either focus on network characteristics that increase labor market success or on network characteristics facilitating benefit receipt, may mistakenly find support for one mechanism because they did not include the other. We will test both theoretical mechanisms by drawing on measures of neighborhood composition, the core discussion network – people with whom one discusses important matters (Burt 1984) – and involvement in voluntary associations.

As a second contribution, we argue that the influence of social networks on benefit receipt is stronger for individuals with a precarious labor market position. Recent studies on the link between an individual's social networks and the likelihood of benefit receipt typically take people's pre-benefit economic situation or labor market position into account as control variables (Åslund and Fredriksson 2009; Bertrand, Luttmer, and Mullainathan 2000; Furtado and Theodoropoulos 2016; Markussen and Røed 2015; Mood 2004; 2010b; Rege, Telle, and Votruba 2012). This implies that the impact of social networks is equal regardless of individuals' (previous) labor market position and level of income (see Mood 2010b). However, there are reasons to assume that this influence may be stronger for individuals in a precarious labor market situation. Precariousness may indicate to what extent a person will be eligible for a benefit; and for the ineligible, the social network theoretically cannot affect benefit receipt. Additionally, people who find themselves in a precarious situation are more likely to mobilize their social network in order to compensate for a lack of resources (Bonoli and Turtschi 2015; Lin 2000).

Summarizing, this study aims to answer two research questions: (1) To what extent do characteristics of social networks increase and/or decrease individuals' likelihood of benefit receipt? Next, we ask: To what extent is the influence of social network characteristics on benefit receipt affected by whether individuals are in a precarious labor market position? To investigate these questions empirically, we employ a unique dataset that combines administrative and survey data from the Netherlands. The administrative data provide reliable information on individual benefit receipt. The survey data are part of the Longitudinal Internet Studies for the Social Sciences (LISS), which contain a range of network-related variables from a representative sample of the Dutch population that is followed over time. The chapter proceeds by elaborating on the theoretical relationship between social networks or social resources, and benefit receipt, and how these may be moderated by precariousness. We do not consider how social networks affect the extent to which an individual is in a precarious labor market

situation, as this has been studied extensively elsewhere (e.g. Chen and Volker 2016; Lin 1999; Lin and Erickson 2008).

2.2 Theory and Background

2.2.1 How social networks influence benefit receipt

Benefit recipients in people's network

Two main arguments are typically made in positing a positive impact of characteristics of an individual's social network on the likelihood to receive a benefit (Bertrand, Luttmer, and Mullainathan 2000; Markussen and Røed 2015; Mood 2010b). More benefit recipients in an individual's network will (1) increase the individual's knowledge about the welfare system and how to best apply for a benefit, and (2) decrease the individual's perceived stigma concerning benefit receipt. Both factors make it more likely that he or she will receive a benefit. Knowledge about the welfare system may include information about available benefit programs, rules of eligibility, how to apply for benefit, what documents to bring to caseworker interviews, and how to behave toward the benefit agency. This will lower the (informational) costs associated with benefit receipt. The extent of perceived social stigma concerning the receipt of benefits is associated with the 'social costs' of benefit receipt. Observing more benefit recipients in one's personal network contributes towards normalizing benefit receipt and thus lowering these social costs. In sum, the presence of benefit recipients in an individual's social network reduces the informational and social costs associated with benefit receipt, thus increasing the likelihood of receiving a benefit.

Employing various research strategies, previous studies support the notion that a higher concentration of benefit recipients in an individual's social network makes for a higher likelihood of benefit receipt (Åslund and Fredriksson 2009; Bertrand, Luttmer, and Mullainathan 2000; Furtado and Theodoropoulos 2016; Markussen and Røed 2015; Mood 2004; 2010b; Rege, Telle, and Votruba 2012). However, these studies have so far not tested the influence of benefit recipients – argued to make for *more* benefit receipt – vis-á-vis the influence of access to social resources that we will argue makes

for *less* benefit receipt. Assuming that the influence of benefit recipients holds once the influence of access to social resources has been taken into account, we hypothesize that:

H1: More benefit recipients in a person's personal network makes for a higher likelihood of receiving a benefit in the future.

Access to social resources

Whereas the above arguments focus on the social influence of benefit recipients that contribute towards a higher likelihood of benefit receipt, we will in the following argue that access to social resources embedded in individuals' personal networks, contributes towards a *lower* likelihood of benefit receipt.

Individuals' access to social resources is argued to improve their labor market position (Lin 1999; Lin and Ao 2008; McDonald et al. 2013; Mouw 2003). This can be accomplished in three ways. First, their contacts can provide them with job relevant information: The availability of specific job openings, how to write a good résumé, how to behave during a job interview, and what is expected from an employee (Lin and Ao 2008; Wilson 2012). Second, contacts may be able to exert influence on the hiring decision, for instance by putting in a word in favor of their 'protégé'. Finally, an individual's contacts may serve as social credentials, with potential employers perceiving these as added value to the individual's resources. In the context of benefit receipt, we argue that individuals with better access to social resources are better able to secure a stable job or, if facing a job loss, better able to find a new job.

For example, research equating access to social resources with the labor market characteristics of social contacts, suggests that having more employed friends is associated with a higher likelihood of transitioning into employment (Cappellari and Tatsiramos 2015). Employed social contacts have also been found to be better able to aid job seekers in their job search (O'Connor 2013). Similarly, Sprengers, Tazelaar and Flap (1988) find that the higher the occupational status of kin and friends – another indicator of access to social resources, the higher the likelihood of reemployment for the long-term unemployed. Relatedly, Chen and Volker (2016) show that social contacts' resources are associated with better job outcomes, such as higher wages and higher occupational status – demonstrating the role of social resources in attaining better and presumably securer jobs. By obtaining these advantages, an individual with better access to social resources should be less likely to receive or depend on social benefits. Based on these arguments, we formulate the following hypothesis:

H2: Better access to social resources makes for a lower likelihood of benefit receipt in the future.

It is important to point out that although hypothesis 1 and 2, and their underlying arguments, may seem like two sides of the same coin, we envision these to be interrelated yet separate mechanisms affecting individuals' benefit receipt. The two network characteristics are likely to be negatively related in the same personal networks – in the sense that as the availability of benefit-related information increases, the access to social resources may decrease, and vice versa[17]. Yet, they represent two distinct pathways: Access to social resources, on the one hand, refers to mechanisms running through the labor market opportunities that affect an individual's likelihood of benefit receipt. Such resources may either prevent benefit receipt – because employees can use them to obtain a better and more secure job – or stimulate the labor market (re-) entry of benefit recipients – who can employ them in finding a job. The concentration of benefit recipients, on the other hand, affects the informational and social costs associated with benefit receipt. This makes it more likely that people depend on benefits, e.g. by making it easier to apply for benefits, and remain so for example by increasing knowledge of how to comply with obligations and manage case workers.

2.2.2 Networks, precariousness and benefit receipt

The importance of social networks and social contacts may depend on individuals' labor market situation. In the following, we focus on precariousness as a specific situation on the labor market that arguably affects the influence of benefit recipients in the network and access to social resources on benefit receipt.

Within sociology and related disciplines, the term 'precariousness' typically relates to temporary employment, part-time and contract work, and low-paid jobs (Campbell and Price 2016; Kalleberg 2000). In this study, we follow this conceptualization and define precariousness in terms of the circumstances that make an individual more likely to apply for a benefit in the near future. One dimension of precariousness is related to an individual's economic situation: A low income and/or non-standard work arrangements lead to lower job security. The other dimension is related to an individual's health: A poor health can make it difficult to achieve a stable and full-time labor market attachment (e.g. van de Mheen, Stronks, and Mackenbach 1998). Both dimensions indicate precariousness in the sense that individuals have a higher risk of losing their job or

17 This would, in our case, imply a negative correlation between the share of benefit recipients in the neighborhood and the indicators of social resources. Empirically, this seems to be the case as shown in the correlation matrix in table A2.1 in the appendix, section 2.6.

experience difficulties in finding a suitable job (e.g. OECD 2015; Western et al. 2012). This makes it likely that they will apply for a benefit in the short or long run. Because of the individuals' economic- or health-related circumstances, they are also more likely to be eligible for a benefit. Consequently, being in a precarious labor market situation leads to a higher likelihood of benefit receipt.

It should be mentioned that a precarious labor market situation does not imply that an individual inevitably will end up receiving a benefit in the (near) future. People in precarious circumstances can remain employed and thus not qualify for a benefit. It is also possible that individuals do not experience a precarious labor market situation prior to receiving a benefit: An unexpected job loss, divorce or a sudden illness can evoke an immediate need for collective financial support. Precariousness is thus related to benefit receipt in a probabilistic rather than deterministic way.

Whether or not an individual is in a precarious situation affects the likelihood of benefit receipt; and this has implications for the hypothesized relationships between individuals' social networks and their chance of benefit receipt. We offer three (inter-) related arguments for why this is the case. First, we noted above that precariousness is related to eligibility and the likelihood of applying for and receiving a benefit. A non-precarious individual is less likely to consider applying for a benefit; and in that case it is not self-evident to assume that, for example, being surrounded by benefit recipients in the neighborhood will affect that individual's likelihood of receiving a benefit. If a non-precarious individual nevertheless decides to apply for a benefit, it is probable that he or she will not be able to meet the eligibility conditions. A higher proportion of benefit recipients in the neighborhood, therefore, would not lead to a higher likelihood of benefit receipt, as stated in hypothesis 1, if an individual is not in a precarious situation. The same should also be true for the other social-network effects hypothesized here. If an individual is not in a precarious situation, access to social resources (H2) is likely to have a lower effect on the likelihood of benefit receipt.

Second, scholars of social capital have suggested that social resources may play a compensating role (Bonoli and Turtschi 2015; Lin 2000). The 'compensation argument' stresses the distinction between access to and the use or mobilization of social resources. It suggests that even though some advantaged social groups have better access to social resources, they may not actively employ these to find a job or prevent benefit receipt. On the other hand, being in a precarious labor market situation, arguably a disadvantaged position, can trigger an active mobilization of social resources to prevent benefit receipt. The same may also be true for precarious individuals surrounded

by many benefit recipients: These people are more likely to turn to their contacts for advice, consequently increasing their chance of benefit receipt.

Third, we consider the process of actualization (Mood 2010b). According to this process, being in a precarious labor market position can make the decision of whether or not to apply for a benefit more prominent in an individual's mind. Consequently, an individual may be more active in probing his or her personal network for relevant information regarding this choice. It is therefore likely that being in a precarious situation induces a more active use of any kind of resource at the individual's disposal. If the individual mobilizes contacts who receive benefits, this will increase the likelihood of benefit receipt; but if he or she mobilizes job-relevant social resources, a lower likelihood of benefit receipt will ensue.

The above arguments all suggest that being in a precarious situation reinforces the effects of social networks on individuals' likelihood of benefit receipt. We therefore pose the following hypotheses:

H3: Being in a precarious labor market situation strengthens the positive effect of benefit recipients in the personal network on the likelihood that an individual will receive a benefit in the future.

H4: Being in a precarious labor market situation strengthens the negative effect of access to social resources on the likelihood that an individual will receive a benefit in the future.

2.3 Data and Methods

2.3.1 Data

This study draws on panel data from the Longitudinal Internet Studies for the Social Sciences (LISS) that have been enriched with administrative data. The LISS (Scherpenzeel and Das 2010)[18] is an ongoing survey that started in October 2007. The panel features a random sample of Dutch households, in which household members who agreed to participate are interviewed monthly. The original sample included 4,500 households and 7,000 individuals. Van der Laan (2009) showed that single households, households with a higher average age, and households with first-generation immigrants were underrepresented. Three refreshment samples were drawn in 2009, 2011 and 2013 to account for this underrepresentation and panel attrition. The survey consists of monthly web-questionnaires, including eight core questionnaires, which are

18 See http://www.lissdata.nl for further information.

repeated annually. In this study, we apply three of these core modules, pertaining to the respondents' social contacts and networks; work and schooling; and health. These questionnaires are complete for the years 2008 to 2014. We complement these with data from the so-called 'background variables'-dataset. This dataset contains information about the household situation as well as income. The response rate for each module varies between 58 and 79 percent over the years.

We enrich the panel data with administrative data from Statistics Netherlands[19], stemming from the Netherlands System of Social Statistical Datasets (SSD), a collection of anonymized longitudinal registrations obtained from tax and social security authorities and municipalities that in principle covers the entire Dutch population. We select monthly data about people's major sources of income, and individual and household demographic information. Because research has indicated that individuals tend to underreport periods of benefit receipt in surveys (Bruckmeier, Müller, and Riphahn 2014), using administrative data should yield more accurate information. Respondents of the LISS-panel have been asked to consent to their survey responses being linked with register data. Although this introduces some non-consent bias in our sample, research from Germany suggests that non-consent bias (1) is rather small relative to other sources of bias, and (2) affects demographics such as age, but not 'substantive variables' such as employment, income, and benefit receipt (Sakshaug and Kreuter 2012).

The analytical sample is obtained as follows. First, we select the LISS respondents who participate at least once in either of the relevant modules. These are 12,614 cases[20] over the whole duration of the LISS-panel. Second, these respondents are linked to the administrative data by Statistics Netherlands. Only 8,190 respondents remain because of a combination of two factors: (1) About 10% of all LISS-respondents do not consent to letting their survey-responses be linked with register-data (Das and Couper 2014); and (2) some individuals cannot be reliably identified in the register-data, implying that for them a link cannot be established between the two data sources. Third, we exclude individuals who do not participate in all relevant survey-modules for at least one year (5,580 individuals remain)[21]. In the fourth step, we exclude individuals younger than 18, and 65 or older, because they are not eligible for any of the benefits investigated in this

19 Under certain conditions, these microdata are accessible for scientific research. For further information: microdata@cbs.nl.

20 This number is necessarily higher than the 7,000 respondents that participated in the LISS-panel originally, because of the refreshment samples.

21 Although this may introduce some sample selectivity, it is important to keep in mind that even if a respondent only participates fully in one year and subsequently drops out, this respondent will be included in our analytical sample given the person allowed administrative linkage of his/her survey responses.

study. For the same reason, students and people receiving early retirement benefits are excluded. This selection results in 4,532 individuals. Last, individuals who have missing values on any variable are excluded. This leaves us with an analytical sample of 3,970 individuals[22]. The resulting dataset is an unbalanced panel with monthly (and yearly) information nested within individuals. The information pertaining to the explanatory variables is time-lagged and linked to information on whether an individual receives a benefit in a given month. For the yearly information, this means that data from the period 2008-2013 is used to predict benefit receipt for the period 2009-2014.

2.3.2 Operationalizations

Response variable

Benefit Receipt is defined as receiving social assistance, unemployment, or disability benefits[23] in a given month. Respondents are coded as benefit recipients if social benefits constitute their major source of income, as indicated by the administrative records. The administrative data also contain information on 'other' benefits. This category includes several smaller benefit schemes and benefits of unknown type. Because the status of this category is ambiguous, we do not include it in our definition of benefit receipt. Among the total observed instances of benefit receipt at person-month level, 43.1 percent pertains to unemployment benefits, 14.8 percent pertains to social assistance, and 42.1 percent pertains to disability benefits.

There are a few reasons for treating these separate benefit programs as one. First, the three benefit programs form an integral welfare system that protects against market risks and medical incapacity. Some people may not qualify for the social insurance programs and immediately start receiving social assistance, while others end up in social assistance after the maximum duration of unemployment or disability benefits has been reached. The different benefits may also be combined at the same time. One example of this is when the amount received from social insurance is below the statutory minimum income (e.g. because the right to receive unemployment benefits was derived from part-time employment), people would be entitled to additional social assistance. Second, although the reasons for receiving these benefits may appear to fall into distinct categories – job loss and medical incapacity – there is evidence that

22 We have decided against using multiple imputation to impute missing data because this is complicated with our unbalanced longitudinal multilevel data and research suggests that only modest improvements in estimates and standard errors are to be expected (Young and Johnson 2015).

23 Social assistance is a household-level, means-tested universal benefit, whereas unemployment and disability are individual insurance-based benefits.

recipients of disability benefits also include 'hidden unemployment' (Koning and van Vuuren 2007), whereas medical issues also affect those on unemployment benefits and social assistance. According to the Dutch municipalities, in 2011 30 percent of their social assistance clients had physical handicaps and 26 percent suffered from mental problems (Divosa 2011, 61).

Social network indicators

Benefit recipients in the network. To measure the presence of benefit recipients in a person's network, we follow previous literature (e.g. Bertrand, Luttmer, and Mullainathan 2000; Markussen and Røed 2015; Rege, Telle, and Votruba 2012) and operationalize this as the proportion of benefit recipients in the neighborhood. We use the same definition of benefit recipients as described in the preceding paragraph. The proportion is calculated by dividing the total number of benefit recipients by the total number of residents aged 18 to 65 in an individual's neighborhood, excluding the respondent. By excluding the respondent his- or herself, the measure captures the proportion of benefit recipients among the other residents in the neighborhood, implying it is not necessarily the same for all respondents living in the same area. In the administrative data, the neighborhood is measured at the level of four-digit postcode areas. The values are obtained for January 1 for all the years between 2009 and 2014. This design ensures that the proportion at the beginning of the year affects the likelihood of benefit receipt in the remainder of the year.

Access to social resources. We employ four measures to operationalize a person's access to social resources. Two of these are derived from questions about the respondent's core discussion network (Burt 1984). In the LISS-data, respondents are asked annually to name up to five contacts based on the following question: "If you look back on the last six months, with whom did you discuss important things?" Subsequently, respondents are asked so-called name interpreter questions concerning the characteristics of the named contacts. First, we consider the *number of employed contacts*, counting the total number of contacts working either full-time or part-time a person mentions. Second, respondents are asked to mention the *level of education of the contact(s)* in their network. We sum the answers to this question[24], and we exclude contacts for which the

24 The level of education and values attached to a specific level, is defined as follows: (1) 'not (yet) finished any education'; (2) finished primary education; (3) 'lower secondary vocational education'; (4) 'higher general secondary education'; (5) 'higher secondary vocational education'; (6) 'higher professional education'; (7) 'university'. We take the sum because having a larger network with more higher educated persons increases the likelihood of receiving help or information. We performed additional analyses with mean education in the network to test this assumption. In line with our expectation, these analyses show smaller and non-significant effects.

respondent does not know the level of education. Whether a person's social contacts work and/or their level of education are frequently evoked measures of the social resources embedded in a person's network (e.g. Hällsten, Edling, and Rydgren 2017; Lin 1999). One potential issue with these measures is that they partially reflect the size of the core discussion network. To ensure that this does not affect the estimates noteworthy, we also include a variable for the number of contacts a respondent mentions in the LISS-panel. Additionally, individuals with no core discussion ties are coded as zero on all of the measures concerning an individual's core discussion network. We enter a separate dummy variable for these individuals in our models, to check whether this is a select group.

Third, we consider the person's *voluntary association involvement*. In the LISS-panel, respondents are annually asked about their relation to a set of voluntary associations[25]. The respondents are requested to check what currently applies to them, or has applied to them over the past 12 months: (1) 'no connection', (2) 'donated money', (3) 'participated in an activity', (4) 'member', or (5) 'performed voluntary work'. We define involvement in an association as the individual's number of memberships. Voluntary association involvement is argued to enlarge a person's social circle, and has been found to relate to other measures of access to social resources (Benton 2016; van Tubergen and Volker 2015) as well as to better labor market outcomes such as earnings and the likelihood of starting a new job (Ruiter and de Graaf 2009). Based on this, we consider it an indicator of a person's access to social resources.

The last indicator of an individual's access to social resources, captures the perceived *proportion of higher and intermediary educated in the neighborhood*. We construct the measure by using existing aggregated survey data (Knol 2012), in which respondents are asked about their perceptions of the socioeconomic standing of residents in their neighborhood. In the survey, one respondent is randomly chosen from each 6-digit zip code area that has at least one hundred residents. Responses are aggregated to the 4-digit zip code area, which consists of on average 106 6-digit zip code areas. The dataset contains several measures, including the perceived proportion of lower educated in their neighborhood[26]. For the analyses, we have reversed this variable so that it

25 The list includes the following types of voluntary associations: A sports club or club for outdoor activities; a cultural association or hobby club; a trade union; a business, professional, or agrarian organization; a consumers' organization or automobile club; an organization for humanitarian aid, human rights, minorities or migrants; an organization for environmental protection, peace organization or animal rights organization; a religious or church organization; a political party; a science, education, teachers' or parents' association; a social society, an association for youth, pensioners/senior citizens, women, or friends' club; and other organizations that you can freely join.

26 Respondents are also asked about the income and unemployment status of their co-residents.

reflects the proportion with a secondary or higher education. There is in general a high correlation between subjective and related objective measures (Knol 2012; Tesser et al. 1995; Zwiers, Kleinhans, and Van Ham 2015). This variable is available for the years 2006, 2010, and 2014. We used the information from 2006 to predict benefit receipt in 2009 and 2010, and the information from 2010 to predict benefit receipt from 2011 to 2014. Observe that this measure also partially takes into account other neighborhood characteristics that may be correlated with the proportion of benefit recipients in the neighborhood. A higher value on our four measures of access to social resources is assumed to indicate a better access to social resources.

Precarious labor market situation

We operationalize a 'precarious labor market situation' by using two indicators based on information from the LISS-data. First, we construct a measure which takes into account whether an individual has a low income, works under 'non-standard work arrangements', or both. The information on income is based on the respondent's monthly net income in euros. Individuals with a low net-income (below the third quartile[27] of the analytical sample) are coded as one. To construct this measure, we use information from the individual's pre-benefit income. This means that we code individuals who received a benefit during the entire period as missing. The arrangements at work are coded based on the type of contract and employment relation mentioned by the respondent each year. We consider a full- or part-time employee with a regular permanent contract as the 'standard' work arrangement, and include chief executives and majority shareholders in this category. Non-standard employment refers to people on a temporary contract and to self-employed persons[28], independent professionals, on-call workers, and those working as a temporary worker for an employment agency. Individuals are coded as being in a *vulnerable work or income situation* if they have a low-income and/or non-standard work[29]. Only individuals that have a missing value on both the income and work-type variable are coded as missing.

Second, we consider whether an individual has *poor health*. We use the following question: "How would you describe your health, generally speaking?" The respondents

27 Which corresponds to 1,000 euros per month.

28 The number of self-employed has grown rapidly in the Netherlands over the past decade, especially sole traders, who have been identified as the most important 'high risk group' experiencing poverty among the employed. They are followed by self-employed workers with staff, on-call workers, and temporary or permanent employees with small part-time jobs (Vrooman et al. 2018).

29 We also tried running the analyses with this variable coded as both low income and non-standard work, instead of at least low income or non-standard work. Operationalizing the variable this way [results not shown], did not substantially change the results presented below.

can rate their health from (1) poor to (5) excellent. We recode this information into a binary variable, in which individuals are coded 1 if they report their health to be either 'poor' (1) or 'moderate' (2), and 0 if they report their health to be 'good' (3), 'very good' (4) or 'excellent' (5). Note that although poor health has been shown to predict future benefit receipt (Henderson, Stansfeld, and Hotopf 2013; Vaalavuo 2016), it is also possible that benefit receipt affects health[30]. As described below, we employ a lagged research design to reduce this type of bias.

Control variables

We add several control variables to account for their possible influence on both the social network indicators and the propensity to receive a benefit. The respondent's *age* is included as a time-varying variable. We also include a dummy for whether a respondent has a *non-Dutch origin* (coded 1). A non-Dutch origin is defined by having a parent who was born outside the Netherlands, or if the respondent was born outside the Netherlands. We add dummies for *observation year* to the models, using 2009 as the reference category. In doing so, we can (partially) account for different labor market conditions and changes in the rules and regulations that would affect overall benefit receipt over time. A set of dummy variables for the respondent's *level of education* as measured through LISS is also included. We distinguish between (1) no education or only primary education; (2) lower or intermediate secondary education; (3) higher or preuniversity secondary education; (4) intermediate professional degree; (5) higher professional degree; and (6) university degree. Those who have a university degree constitute the reference category. We also include a variable for *gender*, with men coded one.

Furthermore, we include three variables on household status, all based on monthly administrative data. The *partner* variable is coded one if the respondent has a spouse, and zero otherwise. Whether the *partner is working* – coded one if working and zero otherwise – is relevant as this increases total household earnings and because social assistance benefits are means-tested, thus affecting both the individual's need and eligibility. Third, we enter the *number of children* as a continuous variable.

Finally, we include a control variable at the level of neighborhoods to account for selection of benefit recipients into 'bad' neighborhoods: The *average value of residential housing* in the neighborhood (see van der Klaauw and van Ours 2003 for a similar

30 In the literature on socio-economic inequalities in health, recent evidence suggests that the causal direction is likely to vary substantially with type of health, type of socio-economic status or outcome studied, and over the life course (Hoffmann, Kröger, and Pakpahan 2018). Empirically, we may nevertheless run the risk of overestimating the effects of other variables associated with health (for example low income), if we do not include health – aside from our theoretical interest in the concept.

approach). This information is taken from publicly available data from Statistics Netherlands (Statistics Netherlands 2019a), and is matched onto the data based on the respondent's place of residence as of January 1 for the different years. The variable reflects the average value of all residential properties in the neighborhood and is measured in 1,000 euros. Because the variable is skewed – with some extremely high values – we take the natural logarithm and center the variable at the person-month mean of these values.

2.3.3 Analytical strategy

We run three pooled logistic regression models containing all available information about individuals over time; meaning monthly observations that are nested within individuals. The response variable is whether an individual receives a benefit in a given month. All explanatory variables are entered time-lagged, implying that benefit receipt in a given month is predicted based upon the explanatory variables' value in the previous month or year. This reduces the possible bias stemming from reversed causality. All non-dichotomous variables are centered at their means to ease interpretation. In order to account for multiple observations per individual, we estimate individual-level robust standard errors. To check the robustness of our analyses, we perform two tests. First, because of the relatively low number of benefit recipients in the sample, roughly 8 percent, we run the same models using the complementary log-log link function, which is better able to deal with many failures (zeros), compared to the standard logistic link function. Second, we run the models with household-level robust standard errors. Neither of these checks leads to substantially different conclusions.

We run three different models to test our hypotheses. In the first model, the individual's likelihood of benefit receipt is predicted based on all of the control variables and the proportion of benefit recipients in the neighborhood. Subsequently, we add the main effects of all other network indicators. By comparing model 1 and 2, we can assess to what extent our measures of social resources add to the model. The second model allows us to test hypotheses 1 and 2. In the third model, we include interaction terms between the social network indicators and the two precariousness indicators. To interpret the results, we present the average marginal effect (AME) estimates of models 1, 2 and 3, in addition to the predicted log-odds estimates. The average marginal effects indicate the average effect of an explanatory variable over the different values of that variable on the probability of benefit receipt (Mood 2010a, 75). AMEs are preferred over e.g. odds ratios, because AMEs can be interpreted in a straightforward manner and are comparable across models (Mood 2010a; Norton 2012). To make the average marginal effects of the continuous variables more comparable, we also report

the predicted effects at two times the standard deviation. We estimate all models in Stata SE 14.2. The descriptive statistics for these analyses are presented in table 2.1.

2.4 Results

2.4.1 Benefit recipients and access to social resources

Table 2.2 shows the estimated average marginal effects from the logistic regression models (the corresponding log-odds are depicted in table A2.2 in the appendix, section 2.6). In model 1, we see that a higher proportion of benefit recipients in the neighborhood is significantly associated with a higher likelihood of benefit receipt. When evaluated at two times the standard deviation, the estimated change in the probability of benefit receipt in a given month is 1.7 percent. Given that the likelihood of benefit receipt in general is 7.9 percent, this is a substantial effect.

Model 2 shows that the inclusion of the social network indicators improves the model fit. Both the AIC and the BIC decrease substantially relative to model 1. If we compare the chi-square of the two models, this also suggests a significantly improved fit (2=90.32 (6), p<.001). Substantively, this shows that the indicators of access to social resources add to the explanation of the likelihood of benefit receipt. Important to note is that the effect of the proportion of benefit recipients in the neighborhood does not decrease in model 2. Including the additional social network indicators adds to rather than explains away the neighborhood association.

Table 2.1. Descriptive statistics at the level of person-months including means, standard deviations, and 1st and 99th percentile.

Variable	Mean	SD	P1	P99
Benefit receipt (ref. no benefit receipt)	.079			
Benefit recipients in the network				
Prop. of benefit recipients in neighb.	.101	.043	.038	.233
Access to social resources				
Prop. of high and interm. educated in neighb.	.683	.141	.295	.988
Nr. of memberships in voluntary org.	1.536	1.497	0	6
Sum of contacts' level of education	14.912	9.115	0	33
Nr. of employed contacts	2.479	1.550	0.	5
Precariousness				
Low subjective health	.122			
Vulnerable work or income situation	.321			
Control variables				
Core-network size	3.296	1.709	0	5
No core-network contacts	.099			
Number of children in the household	.928	1.080	0	4
Has partner (ref. has no partner)	.761			
Partner works (ref. no partner and partner does not work)	.610			
Level of education (ref. university)	.116			
Up to primary education	.017			
Lower/intermediate secondary edu.	.220			
Higher/preuniversity secondary edu.	.097			
Intermediate professional	.256			
Higher professional	.294			
Year (ref. 2009)	.176			
2010	.154			
2011	.176			
2012	.156			
2013	.175			
2014	.162			
Age	44.940	11.079	21	63
Male (ref. female)	.480			
Non-Dutch origin (ref. native Dutch)	.110			
Average value residential housing (*1000 Euro)	237.066	81.819	109	509

LN(average value residential housing)	5.416	.320	4.691	6.232
Number of person-months	153,512			
Number of individuals	3,970			

Notes: For categorical or binary variables, the mean reflects the proportion. Standard errors and percentiles only displayed for continuous variables. Due to rounding, some categorical variables may not add up to exactly 1 (or 100%).

Regarding the indicators of access to social resources, we first see that a higher involvement in voluntary associations is significantly associated with a lower likelihood of benefit receipt. An increase of two times the standard deviation is associated with a 2.7 percent decrease in the probability of benefit receipt. The size of this effect is similar to that of the proportion of benefit recipients in the neighborhood. Second, the level of education in the core network does not affect the likelihood of benefit receipt, considering the insignificant coefficient. Third, the more employed contacts in the core network an individual has, the lower the likelihood of benefit receipt is. The effect is significant and amounts to a predicted decrease in the probability of benefit receipt of 3.4 percent, evaluated at two times the standard deviation of the variable[31]. This is higher than the effect of the proportion of benefit recipients in the neighborhood. Fourth, the proportion of higher and intermediary educated in the neighborhood has no significant effect, which runs counter to hypothesis 2. Considering that two of our four indicators are significant and in the expected direction, model 2 provides some support for hypothesis 2 and, taken together, suggests that better access to social resources makes a person less likely to receive benefits in the future.

Model 1 and 2 in table 2.2 also include estimates of the effects of precariousness. In line with our definition of this concept, being in a vulnerable position is associated with a higher likelihood of benefit receipt. Specifically, having poor subjective health, and being in a vulnerable work and income situation are both significantly associated with a higher probability of benefit receipt. In model 1, having poor subjective health is associated with an 11.6 percent increase in the likelihood of benefit receipt. Being in a vulnerable economic situation is associated with a 4.0 percent increase in this likelihood. These effects are slightly lower in model 2.

Regarding the control variables, we see that a higher number of contacts in the core network is associated with a *higher* likelihood of benefit receipt. Relative to the other

31 The effect of the number of employed contacts could be positively biased, because people may mention colleagues in their core discussion network (81.7 % of the person-months report no colleagues). As a check, we reran the analyses excluding observations in which colleagues are mentioned. This yielded comparable results regarding the effect of number of employed contacts [results not shown].

social network effects, the core-network size has the largest impact on the likelihood of benefit receipt (5.8 percent) in model 2, table 2.2. This effect has to be interpreted as net of the sum of core-network contacts' level of education and the number of employed contacts in the core network. This could imply that the unique variance captured by the variable pertains to the number of individuals in the core network who are not employed[32] and/or have a relatively low level of education. Additionally, we note that there is no significant effect of reporting no core discussion ties on the likelihood of benefit receipt. Turning to the household characteristics, the number of children in the household does not significantly affect the likelihood of benefit receipt. Having a partner is associated with a 3.2 percent lower likelihood of benefit receipt (model 1). If the partner works there is an additional 2.0 percent decrease according to model 1, but this effect disappears in model 2. Relative to having a university degree, only those with at most primary education are significantly more likely to receive a benefit. This effect is quite substantial, amounting to an 11.3 percent increase in the probability of benefit receipt in model 1. In model 2, however, the effect is 2.8 percent lower, suggesting that the social network variables partially account for this association.

Furthermore, there seems to be a trend over the studied period. Relative to 2009, the likelihood of benefit receipt is lower in the following years. The trend is not linear over time, since the average marginal effects are stronger in 2010 and 2011, 3.0 and 2.6 percent, than in the later years. This is at first sight somewhat surprising because the total number of benefit recipients increased in the Netherlands from 2009 to 2014, but note that we control for precariousness which is an important mediator between economic crisis and benefit receipt. Age has a large effect on the probability of benefit receipt. Evaluated at two times the standard deviation, there is a predicted 6.7 percent increase in the probability of benefit receipt. This is a larger effect than any of the social network indicators. An individual's gender does not affect the likelihood of benefit receipt. Having a non-Dutch origin, is associated with a 3.1 percent higher likelihood of benefit receipt relative to native Dutch in model 1. This effect decreases slightly in model 2, which implies that some of this effect can be attributed to differences in social networks between native Dutch and individuals with an immigrant origin. Last, we see that the average value of residential housing does not significantly affect the likelihood

32 Note that 'not employed' does not necessarily mean that these contacts receive benefits nor are actively searching for work. In the survey, there is no distinction between a narrow and wide conception of unemployment, the later including for example retirees, students, and stay-at-home partner.

of benefit receipt. This could indicate a low degree of self-selection of benefit recipients in poor neighborhoods, assuming this variable captures this tendency of self-selection.

Table 2.2. Average marginal effects estimates from pooled logistic regression models, predicting the likelihood of receiving a benefit in a given month.

	Model 1		Model 2		Model 3[c]	
	AME	Δ2SD[b]	AME	Δ2SD[b]	AME	Δ2SD[b]
Intercept	-.169***		-.163***		-.164***	
	(.014)		(.015)		(.015)	
Benefit recipients in the network						
Prop. of benefit recipients in neighb.[a]	.192*	.017	.247*	.021	.256*	.022
	(.096)		(.102)		(.102)	
Access to social resources						
Prop. of high and interm. educated in neighb.[a]			.047	.013	.049	.014
			(.026)		(.026)	
Nr. of memberships in voluntary org.[a]			-.009***	-.027	-.009***	-.027
			(.002)		(.002)	
Sum of contacts' level of education[a]			-.001	-.018	-.001*	-.018
			(.001)		(.001)	
Nr. of employed contacts[a]			-.011***	-.034	-.011***	-.034
			(.003)		(.003)	
Precariousness						
Low subjective health	.116***		.112***		.111***	
	(.012)		(.012)		(.012)	
Vulnerable work or income situation	.040***		.037***		.038***	
	(.008)		(.008)		(.008)	
Control variables						
Core-network size[a]			.017***	.058	.017***	.058
			(.004)		(.004)	
No core-network contacts			.002		.002	
			(.010)		(.010)	
Number of children in the household[a]	-.005	-.011	-.006	-.013	-.005	-.011
	(.004)		(.004)		(.004)	
Has partner (ref. has no partner)	-.032**		-.034**		-.034**	
	(.011)		(.011)		(.011)	
Partner works (ref. no partner and partner does not work)	-.020*		-.014		-.014	
	(.009)		(.009)		(.009)	

Table 2.2 continued

Level of education (ref. university)						
Up to primary education	.113**		.085**		.085**	
	(.035)		(.032)		(.032)	
Lower/intermediate secondary edu.	.021		.004		.004	
	(.013)		(.014)		(.014)	
Higher/preuniversity secondary edu.	.007		-.004		-.003	
	(.016)		(.016)		(.016)	
Intermediate professional	.004		-.007		-.006	
	(.013)		(.014)		(.014)	
Higher professional	-.006		-.011		-.011	
	(.012)		(.013)		(.013)	
Year (ref. 2009)						
2010	-.030***		-.031***		-.031***	
	(.006)		(.006)		(.006)	
2011	-.026***		-.025***		-.024***	
	(.006)		(.007)		(.007)	
2012	-.019**		-.018**		-.018**	
	(.007)		(.007)		(.007)	
2013	-.019**		-.018*		-.018*	
	(.007)		(.007)		(.007)	
2014	-.016*		-.017*		-.016*	
	(.007)		(.007)		(.007)	
Age[a]	.003***	.067	.003***	.067	.003***	.067
	(.000)		(.000)		(.000)	
Male (ref. female)	.009		.013		.014	
	(.007)		(.007)		(.007)	
Non-Dutch origin (ref. native Dutch)	.031**		.027*		.027*	
	(.012)		(.011)		(.011)	
Average residential housing (*1000 euro)[d]	-.015	-.010	-.013	-.008	-.013	-.008
	(.013)		(.014)		(.013)	
Model degrees of freedom	21		27		37	
Pseudo Log-likelihood	-36,810.36		-36,361.14		-36,290.74	
AIC	73,663.72		72,776.29		72,635.47	
BIC	73,871.49		73,044.71		72,903.89	

Model Chi-square	2,434.10	2,524.42	2,557.21
N (person-period)	153,512		
N (individuals)	3,970		

Notes: [a]= Variables are mean-centered; [b]= the predicted change in probability of benefit receipt at two times the SD of the variable (only reported for continuous explanatory variables); [c]= the interaction effects are averaged out in model 3, but are displayed in table 2.3; [d]= variable reflects the mean-centered natural-logarithmic values. *= p<.05; **= p<.01; ***= p<.001 (two-sided). AMEs are obtained from logit models (table A2.2) and standard errors between parentheses are clustered at the individual level. The AMEs for the categorical and dummy variables reflect the discrete change from the reference category.

2.4.2 Social networks and precariousness

Next, we turn to the interaction effects in model 3 in table 2.2. In this model, all of the proposed interaction effects are tested. Because the table displays the average marginal effects, which estimate the average change in probability attributable to a specific variable 'absorbing' any interaction terms included in the model, separate interaction effects cannot be shown in this table. The log-odds estimates displayed in table A2.2 in the appendix (section 2.6), however, do include the magnitude and significance of interaction effects.

The changes in model fit indicate whether it is worthwhile to include the 12 interaction terms. There is a decrease in both the AIC and BIC in model 3 compared to model 2, which implies that the final model fits the data best. Comparing the chi-square of model 3 and model 2 also reveals a significant improvement of the model fit (2=32.97 (10), p<.001). Thus, all metrics suggest that including the interaction effects significantly improves the model fit.

Next, we examine the significance of the log-odds estimates in model 3 in table A2.2 to decide on which interactions to interpret. These suggest that only two interaction effects are significant: The interaction between the number of employed core-network contacts and being in a vulnerable work and/or income situation, and the interaction between sum of contacts' level of education and vulnerable work and/or income situation[33]. We will proceed by interpreting only these interactions using average marginal effects, as suggested by Karaca-Mandic, Norton, and Dowd (2012).

33 As a robustness check, we included all interactions with vulnerable work or income situation, and all interactions with health in two separate models – i.e. testing only five interactions at a time [results not shown]. This procedure yielded the same findings: Only the two interactions between the number of employed core-network contacts and vulnerable situation, and sum of contacts' level of education and vulnerable situation are significant.

We estimated the average marginal effect of the two social network indicators for when an individual is, and when an individual is not in a vulnerable situation. By taking the difference between these two effects, we get the estimated interaction effect (Karaca-Mandic, Norton, and Dowd 2012). The results of this procedure are provided in table 2.3.

First, we consider the interaction between the number of employed contacts in the core network and being in a vulnerable situation. The negative effect of employed contacts increases by 0.9 percent when an individual is in a vulnerable situation compared to when he or she is not. This means that the number of employed contacts has a smaller effect on the likelihood of benefit receipt when an individual is in a vulnerable situation. In fact, the latter effect amounts to 1.6 percent decrease in the probability if evaluated at two times the standard deviation. In contrast, when an individual is not in a vulnerable situation, the probability of benefit receipt decreases by 4.3 percent. Thus, especially individuals who are *not* in a vulnerable situation seem to make use of the social resources provided by employed contacts to lower their chances of benefit receipt. This runs counter to hypothesis 4.

Second, the estimated interaction term between sum of contacts' level of education and vulnerable situation is significant and negative, although the main effect is not. This suggests that there is only a negative effect of contacts' level of education in the core network when an individual is in a vulnerable situation. Table 2.3 shows that when an individual is *not* in a vulnerable work or income situation, there is an estimated 1.8 percent decrease in the probability of benefit receipt (evaluated at two times the standard deviation); but when an individual is in a vulnerable work or income situation the same figure is 5.5 percent. This is in line with hypothesis 4, as the interaction implies that the effect of the contacts' level of education on the probability of benefit receipt is stronger – or rather is only present – when an individual is in a vulnerable work or income situation.

In sum, although including the interaction terms seemed to improve the overall model fit, only two out of ten interaction effects are significant according to the log-odds results (table A2.2) and only one of these is in the direction we expected. Taken together, this suggests that being in a precarious labor market situation, in general, does not strengthen the main effect of the social network indicators on the probability of benefit receipt, counter to hypotheses 3 and 4.

Table 2.3. Estimated average marginal effects for the interaction effects between the number of employed core- network contacts and being in a vulnerable work or income situation, and between sum of contacts' level of education and being in a vulnerable work or income situation evaluated at different values of vulnerable work or income situation.

	Average Marginal Effect	**Δ2SD[a]**
Nr. of employed contacts		
If Vulnerable situation=0	-.014	-.043
If Vulnerable situation=1	-.005	-.016
Sum of contacts' level of education		
If Vulnerable situation=0	-.001	-.018
If Vulnerable situation=1	-.003	-.055

Notes: [a]= Estimated at two times the standard deviation of the core-network variables 'nr. of employed contacts' and 'sum of contacts' level of education'. The average marginal effects show the discrete change in the probability of receiving a benefit in the next month for two values of vulnerable work or income situation for the two variables. The estimates are based on model 3 in table 2.2 (and table A2.2). N(person-period)= 153,512, and N(individuals)= 3,970.

2.5 Discussion and Conclusions

There has been a longstanding sociological interest in understanding how social networks and the resources embedded in them affect an individual's labor market outcomes. However, this interest has so far not been directed at investigating benefit receipt as an outcome. The aim of this chapter was to expand upon previous research by (1) simultaneously studying the social network influences that increase and decrease an individual's probability of benefit receipt; and (2) investigating to what extent the impact of social networks depends on an individual's labor market situation, here understood in terms of precariousness. A unique combination of administrative and survey data allows us to examine these research questions for the Netherlands. Importantly, the

data allow testing arguments from hitherto separate strands of literature. We employed a pooled logistic regression, which makes use of the longitudinal nature of our data.

We hypothesized that the proportion of benefit recipients in the neighborhood, as well as the social resources individuals can derive from their networks affect individuals' receipt of social benefits. In line with previous research, we find that individuals who live in a neighborhood with a high proportion of benefit recipients have a higher likelihood of benefit receipt (H1). This holds true even when we include additional indicators of an individual's access to social resources in the analyses. The results further suggest that access to social resources affects the chance of benefit receipt (H2) and adds to the explanation of benefit receipt beyond the presence of benefit recipients in the neighborhood. Specifically, we find that people who are more involved in voluntary associations have a lower chance of benefit receipt and that this effect is of similar size as that of the proportion of benefit recipients in the neighborhood. Concerning social resources in the core network, only the number of employed contacts is associated with a lower chance of receiving benefits. The impact of the number of employed contacts is relatively high compared to the influence of voluntary association involvement. There is, however, no evidence indicating that the level of education in the neighborhood affects the chance of benefit receipt, nor that the level of education among core-network contacts, on average, affects benefit receipt. Finally, our results as a whole do not support the notion that social networks have a stronger effect for individuals in precarious labor market circumstances, although this seems to be case for one indicator – the level of education in the core network – and only when a person is in vulnerable work or income situation. Thus, the findings do not corroborate hypotheses 3 and 4.

These results suggest that different social network influences may both increase and decrease the probability of benefit receipt, even when we account for both influences. The magnitude of these juxtaposed mechanisms – in terms of their absolute change in the likelihood of benefit receipt – is rather similar. With regard to the economic literature that has focused on the social influence of the neighborhood (e.g. Åslund and Fredriksson 2009; Bertrand, Luttmer, and Mullainathan 2000), our results indicate that non-neighborhood related social network influences also matter. From a theoretical point of view this makes sense: In their search for information about the welfare system and a job, individuals are not necessarily confined to the contacts that are available in their neighborhood; and their norms and values may be affected by relationships with people living elsewhere, too. Individuals may draw on a range of social resources in deciding whether to claim a benefit, and they may use these resources to pursue alternatives to claiming a benefit. If the individual's personal network makes alternatives, such as finding a job, more attractive or feasible, it is less likely that he or she will claim

a benefit. As such, we show the utility of drawing on arguments from the 'social capital' literature (Lin 1999; McDonald et al. 2013) in explaining benefit receipt, which has so far focused on the related outcome of unemployment.

Counter to our expectations, there is in general little evidence for labor market precariousness playing a reinforcing role on the influence of personal networks on the receipt of benefits. This suggests that individuals who are not in a precarious labor market situation mobilize their social network similarly to individuals who are. Consequently, our arguments related to eligibility, the compensating or reinforcing role of social resources, and the actualization process do not hold. One explanation for these unexpected results could be that we overestimated the role of the social network for individuals in a precarious situation. Maybe they – like individuals who are not precarious – are not concerned so much with a possible job loss and thus do not activate their network for help. Alternatively, we may have underestimated the importance of the social network for individuals who are not in a precarious situation. They may be much more inclined to activate their network at the smallest indication of a possible job loss, because such a loss has large consequences for their economic situation.

This study also comes with some limitations. Our results may be biased because of selection effects and unobserved heterogeneity that may impact the causal interpretation of our results. We cannot rule out the possibility that individuals' personal network characteristics are the result of unmeasured characteristics, which also affect the likelihood of benefit receipt. Additionally, present or past benefit receipt may affect the current characteristics of individuals' personal networks. For example, individuals' social contacts may change while receiving benefits. Similarly, subjective health is likely both a predictor and an outcome of benefit receipt. Although we partially accounted for these sources of bias by using time-lagged predictors, we cannot entirely disregard their possible impact on our results. Finally, while our measure of benefit recipients in the neighborhood follows previous research, it is likely to result in an overestimation of the social-network effect for two reasons: (1) A person may only know some of his or her (benefit-receiving) neighbors, and (2) the indicator may capture aspects of the broader socioeconomic context not controlled for by the variables in our current model. Accounting for the average residential value in the neighborhood in our empirical analyses should reduce the second problem, while the first problem is more difficult to remedy with our data.

Future research should ideally model simultaneously the effects of the social network on benefit receipt and of benefit receipt on social networks. In addition, it should investigate in greater detail the role 'good' and 'bad' social influences and resources play in social stratification. In this chapter, we have made a first attempt at doing so by considering not only social processes that increase the likelihood of benefit receipt,

but also those that decrease its likelihood. The present study indicates that such an approach is worthwhile, because empirically both social processes seem to affect benefit receipt; and the influence of social networks is not confined to the relatively small group of people in precarious circumstances, but is probably relevant among the entire labor force.

2.6 Appendix

Table A2.1. Correlation matrix showing correlation coefficients between indicators of social network characteristics.

	Proportion benefit recipients in neighborhood	Proportion high and intermediate educated in neighborhood	Nr of memberships in voluntary organizations	Sum of contacts' level of education	Nr. of employed contacts
Proportion benefit recipients in neighborhood	1.000				
Proportion high and intermediate educated in neighborhood	-.492	1.000			
Nr of memberships in voluntary organizations	-.070	.018	1.000		
Sum of contacts' level of education	-.049	.096	.179	1.000	
Nr. of employed contacts	-.060	.074	.105	.792	1.000

Notes: Correlation coefficients at level of person-months. All variables are mean-centered and coefficients are significant at p<.001 (two-sided).

Table A2.2. Log-odds estimates from pooled logistic regression models, predicting the likelihood of receiving a benefit in a given month.

	Model 1	Model 2	Model 3
Intercept	-2.577***	-2.522***	-2.534***
	(.221)	(.227)	(.229)
Benefit recipients in the network			
Prop. of benefit recipients in neighb.[a]	2.936*	3.819*	2.477
	(1.459)	(1.569)	(2.015)
Access to social resources			
Prop. of high and interm. educated in neighb.[a]		.726	.431
		(.400)	(.541)
Nr. of memberships in voluntary org.[a]		-.144***	-.173***
		(.035)	(.049)
Sum of contacts' level of education[a]		-.021	-.006
		(.011)	(.014)
Nr. of employed contacts[a]		-.171***	-.242***
		(.047)	(.065)
Precariousness			
Low subjective health	1.276***	1.255***	1.250***
	(.103)	(.104)	(.108)
Vulnerable work or income situation	.566***	.540***	.540***
	(.109).	(.109)	(.115)
Interaction effects			
*Prop. of benefit rec. in neigh.*Vulnerable situation*			2.900
			(2.430)
*Prop. of benefit rec. in neigh*Low subj. health*			1.343
			(2.385)
*Prop. of high and interm. educated in neigh.*Vulnerable situation*			.266
			(.746)
*Prop. of high and interm. educated in neigh.*Low subj. health*			.739
			(.759)
*Nr. of voluntary org. memb.*Vulnerable situation*			.037
			(.068)

*Nr. of voluntary org. memb.*Low subj. health*			.054
			(.069)
*Sum of contacts' edu.*Vulnerable situation*			-.030*
			(.014)
*Sum of contacts' edu.*Low subj. health*			-.009
			(.015)
*Nr. of employed contacts*Vulnerable situation*			.177*
			(.081)
*Nr. of employed contacts*Low subj. health*			.002
			(.086)
Control variables			
Core-network size[a]		.259***	.259***
		(.054)	(.054)
No core-network contacts		.035	.032
		(.152)	(.153)
Number of children in the household[a]	-.082	-.087	-.084
	(.057)	(.058)	(.058)
Has partner (ref. has no partner)	-.457**	-.481***	-.490***
	(.143)	(.144)	(.143)
Partner works (ref. no partner and partner does not work)	-.310*	-.221	-.215
	(.129)	(.132)	(.132)
Level of education (ref. university)			
Up to primary education	1.217***	.931**	.934**
	(.308)	(.307)	(.307)
Lower/intermediate secondary edu.	.309	.056	.061
	(.207)	(.215)	(.216)
Higher/preuniversity secondary edu.	.117	-.066	-.052
	(.249)	(.252)	(.254)
Intermediate professional	.067	-.102	-.089
	(.210)	(.214)	(.216)
Higher professional	-.104	-.181	-.176
	(.205)	(.205)	(.207)
Year (ref. 2009)			

Table A2.2 continued

2010	-.448***	-.470***	-.469***
	(.089)	(.090)	(.090)
2011	-.373***	-.361***	-.347***
	(.093)	(.096)	(.096)
2012	-.260**	-.259*	-.254*
	(.100)	(.103)	(.103)
2013	-.289**	-.276**	-.275**
	(.102)	(.105)	(.105)
2014	-.228*	-.231*	-.228*
	(.102)	(.104)	(.105)
Age[a]	.044***	.047***	.048***
	(.005)	(.005)	(.005)
Male (ref. female)	.145	.203	.209
	(.111)	(.113)	(.114)
Non-Dutch origin (ref. native Dutch)	.423**	.383**	.379**
	(.142)	(.140)	(.140)
Average residential value (*1000 euro)[b]	-.232	-.204	-.196
	(.207)	(.210)	(.209)
Model degrees of freedom	21	27	37
Pseudo Log-likelihood	-36,810.36	-36,361.14	-36,290.74
AIC	73,663.72	72,776.29	72,635.47
BIC	73,871.49	73,044.71	72,903.89
Model Chi-square	2,434.10	2,524.42	2,557.21
N(person-period)	153,512		
N(individuals)	3,970		

Notes: [a]= Variables are mean-centered; [b]= variable reflects the mean-centered natural-logarithmic values. *= p<.05; **= p<.01; ***= p<.001 (two-sided). Estimated with cluster robust standard errors at the individual level (displayed between parentheses). See tables 2.2 and 2.3 for the AME estimates.

Chapter 3.

Social Benefits among Ethnic Majority and Minority Groups in the Netherlands: The Role of Social Capital[34]

34 A slightly different version of this chapter has been submitted to an international scientific journal. The chapter was co-authored by I. Maas and J. C. Vrooman. Kristiansen wrote the main part of the manuscript and conducted the analyses during a research stay at SOFI, Stockholm University. Maas and Vrooman contributed substantially to the manuscript. The authors jointly developed the idea and research design, in the context of the project From network to work? at Utrecht University. Earlier versions of this chapter have been presented at the Nordic Work-Life Conference in Oslo, 2018; at the ISA World Congress of Sociology in Toronto, 2018; and at the Migration and Social Stratification seminar at Utrecht University.

Abstract

The low labor market integration of first- and second-generation immigrants became a key scholarly interest over the past decades. Few studies examined the receipt of social assistance, unemployment or disability benefits as an outcome, and little is known about the role of social capital in this regard. This chapter investigates to what extent individual social capital explains the native-immigrant differential in benefit receipt by combining survey and register data. Specifically, we examine the influence of inter- and intra-ethnic contact, and of the presence of people on benefits in the social environment on benefit receipt for natives and the four major immigrant groups in the Netherlands: People of Turkish, Moroccan, Surinamese and Antillean origin. Employing linear probability modeling, we find while high inter-ethnic contact lowers the likelihood of benefit receipt among people with an immigrant background, but high intra-ethnic contact does not. Benefit receipt among native Dutch people is only affected by intra-ethnic relations. In explaining the differences between natives and immigrants in benefit receipt, however, only the concentration of benefit recipients in the social environment matters.

3.1 Introduction

Foreign-born migrants and their children tend to fare worse on European labor markets (Heath and Cheung 2007) and more often receive benefits than native people (Barrett and Maître 2013; Zorlu 2013). The difference in social assistance, disability, or unemployment benefit receipt is typically attributed to diverging human capital and background characteristics (Boeri 2010), although in some countries a significant part is unexplained (Zorlu 2013). Koopmans (2016) suggests that socio-cultural factors, such as inter-ethnic social ties, could explain the native-immigrant differential. While Renema and Lubbers (2019) point out the role of social capital in predicting benefit receipt – especially social assistance – among immigrants, few studies have investigated its relevance for explaining the native-immigrant gap.

Two aspects of social capital emerge from the literature as potentially relevant in this respect. The first line of research stresses that inter- and intra-ethnic social contact may provide valuable social resources, such as knowledge about the host country and psychological support (Auer, Bonoli, and Fossati 2017; Heizmann and Böhnke 2016; Kanas, van Tubergen, and van der Lippe 2011; Kanas et al. 2012; Lancee 2010; 2012; Lancee and Hartung 2012). This approach typically emphasizes the positive impact of social contacts on employment, earnings and occupational status, but tends to neglect their effects on benefit receipt.

A second strand of the literature points at the relevance of benefit receipt among one's social contacts. Various studies suggest that being surrounded by benefit recipients increases the likelihood that people themselves depend on social insurance or assistance (Åslund and Fredriksson 2009; Bertrand, Luttmer, and Mullainathan 2000; Furtado and Theodoropoulos 2013; 2016; Markussen and Røed 2015; Mood 2004; 2010b; Rege, Telle, and Votruba 2012). Theoretically, this is because other benefit recipients may provide information about the welfare system or reduce the stigma associated with benefit dependency. However, to our knowledge this social capital approach has not been considered in relation to native-immigrant differences in benefit receipt.

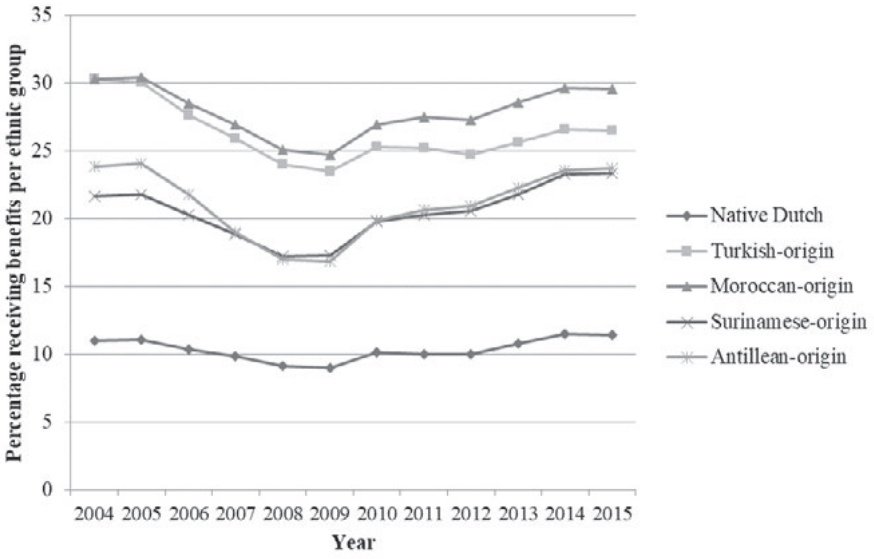
This chapter investigates whether social capital, understood as (1) inter- and intra-ethnic social contact and (2) acquaintances receiving benefits explains differences in benefit receipt between natives and first- and second-generation immigrants. We contribute to existing research by simultaneously analyzing the impact of the two types of social capital on the native-immigrant benefit gap. Specifically, we ask the following research questions: (1) *How do social capital affect natives and first- and second-generation*

immigrants' benefit receipt? (2) To what extent can social capital explain native-immigrant differences in benefit receipt?

We investigate these questions by drawing on a unique Dutch dataset that combines survey responses with information derived from administrative sources. We use two cross-sectional surveys stemming from 2006 and 2011, consisting of nationally representative samples of the Dutch ethnic majority population and the four largest immigrant groups in the Netherlands – people of Turkish, Moroccan, Surinamese and Antillean origin. Turks and Moroccans migrants started to arrive as guest workers in the 1960s and 1970s, migration from Surinam peaked between 1975 and 1980 (related to the independence of this former Dutch colony), and Antilleans increasingly moved to the European part of the Kingdom of the Netherlands when the economic circumstances on the islands started to deteriorate in the 1980s (Tesser and Dronkers 2007). The register data include longitudinal measures of the major source of income and other socioeconomic characteristics of all Dutch inhabitants and were linked to the surveys. This ensures a reliable measure of benefit receipt, which is defined as having unemployment insurance, disability, or social assistance as the major source of income. We employ a modelling strategy in which explanatory factors at *t* predict benefit receipt at *t+1*, thus improving on cross-sectional designs.

Figure 3.1 provides an overview of the overall differences in benefit receipt by ethnic background. It shows that benefit receipt was consistently lowest among the Dutch native population between 2004 and 2015. First- and second-generation immigrants of Surinamese- and Antillean-origin attain considerably higher levels of benefit receipt; until 2007 benefit dependency is slightly higher among Antillean immigrants, but in subsequent years benefit rates are comparable. Benefit receipt is highest among Turkish and Moroccan immigrants, and over the years there is a widening gap between them, with people of Moroccan-origin clearly reaching the highest benefit rates in 2015 (while they were on par with Turkish immigrants in 2004). In all ethnic groups, benefit rates follow the economic cycle, with a decreasing trend in 2005-2008 and an increase after 2009. These trends emerge more clearly in the four immigrant groups than among the native Dutch.

Figure 3.1. Percentage receiving social benefits as major source of income for 18-65-year-olds within each ethnic group for the period 2004-2015.



Notes: Ethnic background is based on national origin and includes both first- and second-generation immigrants. *Source:* Authors' own calculation based on non-public individual-level administrative data from Statistics Netherlands.

3.2 Theory and Background

3.2.1 Inter- and intra-ethnic social contact

Inter- and intra-ethnic social contact is often considered an important part of immigrants' social integration and social capital. These concepts emphasize the ethnic background of immigrants' social ties and are theoretically related to the concepts of bridging and bonding social capital (Putnam 2000).

Bridging social capital – social ties that span different ethnic backgrounds – is thought to be particularly beneficial for immigrants because it provides access to host-country knowledge, ranging from specific information about job openings and how to best apply for a job (Kanas and van Tubergen 2009) to more general 'cultural' information (Koopmans 2016). The latter may include better mastery of the host country language, as well as information about general social norms. More host-country knowledge arguably makes it easier to succeed on the labor market. Empirical research supports this argumentation, finding that inter-ethnic contact, in particular contact with the majority

population, makes for a lower risk of poverty (Heizmann and Böhnke 2016), a higher likelihood of employment (de Vroome and van Tubergen 2010; Lancee 2010; 2012; Kanas, van Tubergen, and van der Lippe 2011), and a higher occupational status and income (Kanas et al. 2012; Lancee 2010; 2012; de Vroome and van Tubergen 2010). None of these empirical studies, however, has examined benefit receipt as an outcome. We presume that inter-ethnic contact will lower immigrants' chances of benefit receipt. Alternatively, Renema and Lubbers (2019) suggest that more host-country knowledge – as obtained through inter-ethnic contact – makes for a better understanding of the benefit programs' administrative procedures. Conditional on being unemployed, this should result in a higher reliance on benefits; but the empirical results do not support this (Renema and Lubbers 2019).

Bonding social capital – intra-ethnic contact among people with the same ethnic background – is also argued to enhance immigrants' labor market outcomes. It can provide access to psychological and financial or material support (Heizmann and Böhnke 2016), which may help people obtain a job, for instance by facilitating one's presentation at job interviews or dealing with emotional problems. This type of support is less likely to cross ethnic lines, and more likely to stem from kin and other close relations (Coleman 1988). A slightly different argument may be derived from ethnic enclave theory. This argument stipulates that social contacts from the same ethnic group can help the immigrant obtain a job in the 'ethnic' economy (e.g. Damm 2009; Wilson and Portes 1980; Zhou 2004), which is characterized by immigrant-owned firms and a co-ethnic employment network that immigrants may draw on. Empirical evidence, however, generally has not found that intra-ethnic contact makes for better labor market outcomes for ethnic minorities (Lancee 2010; Kanas et al. 2012); but it is not clear whether this conclusion also holds for benefit receipt.

Based on the literature, we argue that intra-ethnic contact can reduce benefit receipt for both natives and immigrants (see table 3.1 for an overview of the hypotheses). We expect inter-ethnic contact to reduce benefit receipt among immigrants, but we do not expect an influence of inter-ethnic contact on benefit receipt among native Dutch people. In their case, this would typically imply associating with ethnic minority groups who have a weaker labor market attachment and little social capital that is relevant for the ethnic majority (Lancee and Hartung 2012). Assuming social capital – in the form of inter- and intra-ethnic contact – is more concentrated among natives, we expect that accounting for inter- and intra-ethnic contacts partially reduces the observed immigrant-native differential in benefit receipt.

3.2.2 The social influence of other benefit recipients

Contact with other benefit recipients theoretically increases a person's likelihood to receive benefits in two ways (Bertrand, Luttmer, and Mullainathan 2000; Mood 2010b). First, benefit-receiving contacts may convey information about the welfare system. This includes information about the existence and requirements of social benefit programs, how to act towards caseworkers, and how to fill in applications for benefits. Access to such information reduces the perceived costs associated with claiming social benefits. Second, benefit-receiving contacts can affect the norms and values an individual holds. They may 'normalize' benefit receipt and reduce the stigma associated with it. In addition, having contacts with benefit recipients may weaken the work ethic and the norm that people should provide for themselves (Lindbeck, Nyberg, and Weibull 1999). These normative influences increase the likelihood of benefit receipt, and their effects will be stronger the more benefit recipients people have in their social environment (Mood 2010b).

Previous empirical work in this tradition – 'social influence' literature – has mainly used the neighborhood as a proxy for the number of benefit recipients in one's social network, and generally corroborate the theoretical expectations (Åslund and Fredriksson 2009; Bertrand, Luttmer, and Mullainathan 2000; Furtado and Theodoropoulos 2013; 2016; Markussen and Røed 2015; Mood 2004; 2010b; Rege, Telle, and Votruba 2012). Some of these studies focused specifically on benefit receipt and the ethnic background of other benefit recipients in one's neighborhood. Markussen and Røed (2015) found a positive relation between benefit receipt among immigrants from low-income countries and benefit receipt among people in their neighborhood originating from the same country. Similar findings are reported by Åslund and Fredriksson (2009). Interestingly, there is some evidence to suggest that benefit receipt in the neighborhood among other migrants or among natives has no impact on immigrants benefit receipt vis-à-vis benefit receipt among immigrants originating from same country (Markussen and Røed 2015). Therefore, we assume that degree of benefit receipt among people of the *same* ethnic group in the neighborhood affects a person's benefit receipt.

The 'social influence' literature did not investigate how this influence can account for differences in benefit receipt between various migrant groups and the native population. There is reason to expect that the impact of contacts with other benefit recipients differs for members of various ethnic groups. First, it may be more pivotal for members of immigrant groups to have people in their social environment that can inform them about the welfare system (Renema and Lubbers 2019). Because of institutional differences between their country of origin and the host country, ethnic minority groups may be less informed about what they are entitled too. Further, a lower language

proficiency may result in difficulties navigating the application process and obtaining information from other sources (e.g. governmental websites). In our case, this would suggest a distinction between first-generation Turkish and Moroccan immigrants on the one hand – who tend to be lower educated and had little previous knowledge about the Dutch language and cultural practices – and Surinamese and Antillean immigrant on the other, who are better acquainted with the history and language and generally are better integrated in Dutch society (Kanas and van Tubergen 2009).

In addition, normative influences may be stronger among immigrant groups that are numerically smaller, and tend to be concentrated in certain neighborhoods (Hartog and Zorlu 2009). Members of such immigrant groups are likely to be better informed about each other's views and behavior, implying that they are more likely to obtain normative information. The smaller group size coupled with neighborhood concentration, also implies that if the concentration of benefit recipients is relatively high, a person is more likely to know other benefit recipients and therefore be subject to normative influence from a benefit-receiving peer. For the native population the neighborhood concentration of benefit recipients is less of a constraint, as they have more contact opportunities elsewhere.

These arguments suggest an interaction effect between the proportion of benefit recipients among the ethnic in-group in the neighborhood and a person's ethnic background. We expect that this works more strongly for Turkish and Moroccan immigrants vis-à-vis Antillean and Surinamese immigrants. We also expect that accounting for these differential influences will partially explain the observed immigrant-native differential in benefit receipt, assuming that overall the proportion of benefit recipients among the in-group is higher for the immigrant groups. Table 3.1 summarizes all of our hypotheses.

Table 3.1. Overview of the hypotheses

Nr	Hypothesis
H1a	Higher inter-ethnic contact is associated with lower benefit receipt for members of immigrant groups.
H1b	Higher intra-ethnic contact is associated with lower benefit receipt for members of immigrant groups.
H2a	Higher inter-ethnic contact does not affect benefit receipt for members of the native population.
H2b	Higher intra-ethnic contact is associated with lower benefit receipt for members of the native population.

H3	Accounting for the effects in H1a, H1b, H2a, and H2b, will partially explain the differences in benefit receipt between natives and immigrant groups.
H4	The proportion of benefit recipients with the same ethnic background in the neighborhood is associated with more benefit receipt.
H5a	The proportion of benefit recipients with the same ethnic background in the neighborhood has a stronger positive association with benefit receipt for immigrants compared to natives.
H5b	The relationship in H5a is stronger for Turks and Moroccans relative to Antilleans and Surinamese.
H6	Accounting for the effects in H4, H5a and H5b will partially explain immigrant-native differences in benefit receipt.

3.3 Data and Methods

3.3.1 Data

We employ two waves of a national survey, enriched with individual-level administrative data covering the entire Dutch population. The 2006 and 2011 Surveys Integration of Minorities (‘*Survey Integratie Minderheden*’ or SIM) consist of first- and second-generation Turks, Moroccans, Antilleans, and Surinamese and a reference sample of native Dutch (Statistics Netherlands 2006; 2011)[35]. The two surveys are consistent in terms of their sampling strategy, interviewing, and questionnaire items, which facilitates joint analyses. SIM 2006 includes 5,250 respondents, with approximately 1,050 respondents per ethnic group. SIM 2011 comprises a slightly lower number of respondents (N=4,125), with between 800-900 respondents per ethnic group[36]. The response rate was on average around 50% in both surveys, which is common for surveys in the Netherlands. While the data are generally representative relative to national averages, people younger than 35 are slightly underrepresented in both surveys, and men and second-generation migrants are somewhat underrepresented in the 2006 wave.

The survey data were linked to the Social Statistical Database (SSD), which contains anonymized longitudinal information on income and basic demographic data

35 Observe that we use the non-public versions of these data that are linked to administrative data from Statistics Netherlands.

36 This wave also included a mode experiment in which one set of respondents was interviewed using face-to-face computer-assisted interviewing (as in SIM 2006), and another through a sequential mixed-mode design. To avoid potential mode bias, we only include the respondents that were interviewed face-to-face in SIM 2011 to ensure comparability across the two cross-sectional waves.

relating to all Dutch inhabitants[37]. It was compiled by Statistics Netherlands from the population registry and the tax and social security administrations. We use the data on individuals' major source of income to determine benefit receipt. Administrative data has been shown to be superior to self-reported survey measures in assessing benefit receipt (Bruckmeier, Müller, and Riphahn 2015; Meyer, Mok, and Sullivan 2015).

We restrict our analyses to respondents in the 18-62-year age bracket. People below age 18 and above age 64 do not qualify for the major cash benefits we study here. By setting an upper limit at 62 years we minimize the risk of biased outcomes ensuing from the fact that virtually all people start receiving the Dutch statutory old age pension at age 65. We also exclude respondents who were full-time students or in receipt of early retirement benefits in the survey year. Through these selections, our sample size reduces from 9,375 to 6,767 respondents.

3.3.2 Operationalizations

Response variable

The response variable in our analyses, benefit receipt, is defined as deriving one's major source of income from the Dutch income replacement schemes, excluding old age pensions. These schemes consist of unemployment and disability employee insurance and social assistance[38]. Unemployment and disability insurance benefits typically have limited duration; eligibility mostly depends on past labor experience, and a loss of working hours or a deterioration in health, respectively. Social assistance is available to all households with an income below the statutory social minimum who meet certain conditions (including a means test on wealth and earnings of spouses and co-dwellers).

37 Under certain conditions, these microdata are accessible for scientific research. For further information: microdata@cbs.nl.

38 Specifically, the following schemes are included: Algemene Bijstandswet, Wet Arbeidsongeschiktheid, Ziektewet, Werkloosheidswet, and a 'rest' category (Sociale Voorziening Overig) which includes disability schemes for self-employed and artists, and Wet Arbeidsongeschiktheidsvoorzieningen Jonggehandicapten – see https://www.cbs.nl/nl-nl/onze-diensten/maatwerk-en-microdata/microdata-zelf-onderzoek-doen/microdatabestanden/secmbus-personen-sociaaleconomische-categorie for more information.

We measure benefit receipt as having income from either scheme in at least one month in the two year-period after the survey. This implies that for SIM 2006 we assess whether respondents received any benefit in 2007 and 2008, and for SIM 2011 in 2012 and 2013. By measuring future benefit receipt, we reduce the potential bias stemming from simultaneous measurement of the response and explanatory variables. In our response variable receipt of the three types of benefits is combined, because in the Dutch case these form an integrated welfare system shielding against market risks and incapacity. People may receive a combination of these benefits simultaneously, for example if they are declared partially disabled but do not have a job to supplement their income and would therefore qualify for social assistance. Furthermore, while the schemes theoretically cover distinct risks, in practice there is a considerable overlap in the problems experienced by their recipients. For instance, disability receipt tends to include 'hidden unemployment', and a substantial share of social assistance recipients are in bad physical and mental health (see Boschman et al. 2019; Markussen and Røed 2015 for similar approaches in the Dutch and Norwegian contexts).

Explanatory variables

Ethnic background. We include a categorical variable distinguishing between natives (reference category) and first- and second-generation Turks, Moroccans, Surinamese, and Antilleans. Following Statistics Netherlands' official definition, a person is considered native Dutch if both parents were born in the Netherlands. Non-natives consist of people with at least one parent born outside of the Netherlands. Those who were born abroad themselves belong to the first generation, those born in the Netherlands to the second generation. This information stems from SSD.

Proportion of benefit recipients with the same ethnic background in the neighborhood. For the survey years, we first calculate the share of benefit recipients of working age (18-65) within specific ethnic groups in the neighborhood, excluding the respondent. The neighborhood is defined at the level of four-digit zip code areas (containing 6,000 inhabitants on average). Subsequently, we match these shares to the respondent's own ethnic background. The variable is constructed using register data.

Intra-ethnic contact. We measure intra-ethnic contact through the following survey item: "How often do you have contact with friends or acquaintances from the same ethnic- or national-origin group?" Response categories are (1) "never/less than 1 time

a year"; (2) "a couple of times a year"; (3) "every month"; (4) "every week"; and (5) "every day"[39].

Inter-ethnic contact. Inter-ethnic contact is derived from three survey items: (1) "How often do you have contact with friends or acquaintances from the Dutch native/ethnic minority groups?"; (2) "Do you often, sometimes, or never get a visit from friends or neighbors with a native Dutch/ethnic minority background?"; and (3) "Do you often, sometimes, or never have contact with native Dutch/ethnic minority people in your spare time?" The variable is constructed by recategorizing the three variables (0 = is no or less than annual contact; 1 = more frequent contact)[40] and subsequently taking the sum of these. A zero score therefore signifies no or hardly any inter-ethnic contact, while a score of 3 implies frequent inter-ethnic contact on all of the aspects measured in the survey.

Ethnic contact in voluntary associations. Ethnic contact in voluntary associations[41] is often regarded as an important source of social capital (Renema and Lubbers 2019). The measures differ somewhat in the two survey waves. In SIM 2006, respondents were asked about whether they are members or donate to any organization or association, and subsequently about the ethnic/migration background of the people they mainly socialize with in this organization (or the one they spend the most time in). In SIM 2011 respondents were asked whether they sometimes participate in meetings or activities organized by one or several associations, and subsequently the ethnic or migration background of the people they mainly socialize with. We assessed whether respondents in voluntary associations mainly had contact with (1) people of the same ethnic background; or (2) people of an outgroup or diverse ethnic backgrounds. We entered this classification as two dummy variables in the analyses, with 'no participation or membership in voluntary associations' as the reference category.

39 Respondents were explicitly asked not to consider contacts living in the same household or living outside of the Netherlands in their responses.

40 Response categories differ between the three items: Item (1) has five response categories ranging from (1) "never/less than yearly" to (5) "every day"; whereas items (2) and (3) have three response categories, namely (1) "no, never", (2) "yes, sometimes", and (3) "yes, often".

41 Respondents are asked about participation or membership in (1) sports association; (2) social association such as hobby club or musical association; (3) neighborhood association; (4) labor union or professional association; (5) organization by or for foreigners/people with a migration background; (6) political party or other political organization; (7) religious organization; (8) environmental, nature, or international solidarity association; or (9) other type of organization(s). In the SIM 2006, 'library' is mentioned as a separate category.

Control variables

Level of education is a continuous variable capturing the number of years associated with the highest level of education followed[42]. We allow the effect of education to differ depending on whether the educational program was followed abroad or in the Netherlands. This information is taken from the survey.

The respondent's *age* is included in years and as a quadratic term. We control for the *presence of a partner* and the *number of children below age 18* in the household. These two variables are included as interactions with *gender*, because the effects of these two variables are likely to be different for females (the reference category) and *males*. We also control for individuals self-rated, general *subjective health*, which is measured on a 5-point scale ranging from (1) poor to (5) excellent. *Survey year* is included as a binary variable, with 2006 as the reference category. This should account for variation between the two time points, especially in the overall risk of benefit receipt.

Finally, we include contextual variables that indicate the overall socioeconomic status of the respondent's neighborhood. The *average housing value* in the area is taken from public aggregate-level administrative data for the years 2006 and 2011 (CBS, 2018). This variable has been measured (in thousands of euros) considering all residential housing; as it is skewed, with a few extreme values, we include the natural logarithm in the analyses. Finally, we include a variable for *proportion of benefit recipients among ethnic out-group in the neighborhood*, which was constructed the same way as the ethnic in-group benefit recipiency in the local area.

3.3.3 Analytical strategy

To test our hypotheses, we use linear probability models that estimate the likelihood of at least one month of benefit receipt in the two years following the survey. Linear probability models are a good alternative to binary logistic regression, especially because they facilitate a straightforward interpretation of interaction effects (Mood 2010a). All models have been estimated using robust standard errors to minimize problems stemming from heteroscedacity. Continuous variables without a meaningful zero value have been mean-centered to facilitate the interpretation of the interaction effects. We also checked whether multilevel modeling with individuals nested within neighborhoods was necessary. Because the intraclass-correlation was low (.035), we

42 Up to primary = 6 years; lower secondary vocational training ('LBO') = 9 years; lower secondary general ('MAVO') = 10 years; intermediate secondary vocational ('MBO') = 11 years; preuniversity or general higher secondary ('HAVO, gymnasium, VWO') = 11.5 years; higher professional tertiary degree ('HBO') = 15 years; university degree ('WO') = 16 years.

decided against such an approach[43]. To account for missing values[44], we use multiple imputation (Nimputed datasets= 50) based on chained equations in Stata 14[45].

In our models, we, first, include the main explanatory variables related to social capital and social contact in separate steps, and subsequently introduce the covariates. In doing so, we are able to investigate the impact of our social capital variables and to what extent these persist when introducing other covariates. We also introduce the variables in a different order to test their additional explanatory power.

We reduce potential bias stemming from the simultaneous selection into high benefit-receiving neighborhoods and the propensity to receive benefits in two ways. First, we do not measure the response variable at the same time as the proportion of benefit recipients; our explanatory variables predict future benefit receipt. Second, we include two control variables that are indicative of the general socioeconomic status of the neighborhood: The average housing value, and the proportion of benefit recipients among ethnic out-group members. The concentration of benefit recipients in low-status neighborhoods is presumably driven by these characteristics; and controlling for these variables allows us to better distinguish the effect of a high proportion of benefit recipients among the ethnic in-group in the neighborhood from the association due to selection of benefit recipients in 'bad' neighborhoods.

3.4 Results

3.4.1 Effects of social contact and benefit recipients in neighborhood

Table 3.2 shows the percentage receiving a benefit per ethnic group, as observed in our analytical sample. It is worth noting that the percentage of respondents receiving benefits is somewhat higher than for the overall working-age population (see figure 3.1). This can be attributed to the fact that we observe benefit receipt over a two-year period. In addition, our analytical sample excludes students and early retirees that are

43 We ran the same models using cluster robust standard errors at the neighborhood level as an additional check. These models lead to the same conclusions [results not shown].

44 87 percent of cases has no missing values.

45 We have also checked for possible differences between the 2006 and 2011 SIM-surveys by running the models for each survey separately. The only major difference that appears is that the estimated ethnic majority-minority differences are smaller in SIM 2011, which is likely due to a higher occurrence of benefit receipt among the ethnic majority sample. The substantive conclusions regarding the other main explanatory variables remain the same.

by definition ineligible for social benefits. Descriptive statistics for all other variables in the analyses are listed in table A3.1 in the appendix (section 3.6).

Table 3.2. Percentage receiving benefits at least 1 month in the two years after the survey year per ethnic group/immigrant background.

Immigrant background	Percent benefit receipt	N(individuals)
Dutch native	15.08	1167
Turkish 1st generation	41.92	1200
Turkish 2nd generation	28.90	263
Moroccan 1st generation	42.40	1151
Moroccan 2nd generation	37.07	232
Surinamese 1st generation	28.30	1000
Surinamese 2nd generation	23.08	364
Antillean 1st generation	38.86	1073
Antillean 2nd generation	15.86	290
Total	32.03	6740

Notes: Based on analytical sample.

Table 3.3 shows the results of the linear probability models estimating the likelihood of at least one month of benefit receipt in the two years after the survey. Model 1 reproduces the 'unadjusted' differences in benefit receipt between Dutch natives and the immigrant groups, as depicted in table 3.2.

We find that people with an immigrant background profit from inter-ethnic contact: An increase of one standard deviation on the inter-ethnic contact scale is on average associated with a 4.5 percent point lower probability of benefit receipt[46], providing support for hypothesis 1a. As more variables are added to the models (3-6) the effect

46 This is obtained by taking the difference between the main effect of inter-ethnic contact and the interaction effect for immigrant background (in model 2, equal to .006-.046 = -.040), and multiplying by the standard deviation of inter-ethnic contact (-.040*1.134=-.045).

becomes smaller but persists. Net of other factors, an increase of one point on the inter-ethnic contact scale is associated with a 1.5 percent point decrease in the probability of benefit receipt (model 6).

Additionally, intra-ethnic contact makes for a slightly *higher* likelihood of benefit receipt for immigrants relative to natives, but on average the total effect is small: A one standard deviation increase in intra-ethnic contact is associated with a 0.9 percent point higher probability of benefit receipt (model 2). The estimated effect becomes negligible in the other models (3-6). Overall, the results offer no support for our expectation that more intra-ethnic contact lowers the likelihood of benefit receipt among members of ethnic minorities (H1b): Contact with co-ethnics does not affect immigrants' probability of benefit receipt.

The non-significant main effect of inter-ethnic contact across all models suggests that natives' likelihood of benefit receipt is not affected by the extent of their contacts with non-natives, which is in line with our theoretical expectation (H2a). Initially, we also find no effect of more frequent intra-ethnic contact for members of the ethnic majority group (model 2). However, once individual- and neighborhood-level control variables are included (models 5 and 6), the impact of this variable turns significant. According to the final model, an increase of one standard deviation in intra-ethnic contact results in a 2.9 percent point decrease in the probability of benefit receipt. For natives more contact with co-natives therefore is associated with lower benefit receipt, lending support to hypothesis 2b.

Table 3.3. Results for linear probability models predicting the likelihood of receiving benefits at least 1 month in the 2 years after the survey – reduced table.

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Intercept	.151***	.190***	.257***	.345***	.348***	.332***
	(.010)	(.023)	(.025)	(.042)	(.042)	(.042)
Ethnic group (ref. Dutch native)						
Turkish 1st generation	.269***	.319***	.213***	.132**	.093**	.101*
	(.018)	(.030)	(.033)	(.047)	(.045)	(.045)
Turkish 2nd generation	.138***	.220***	.116**	.034	.091	.101*
	(.030	(.040)	(.042)	(.053)	(.051)	(.051)
Moroccan 1st generation	.273***	.336***	.218***	.139**	.137**	.148***
	(.018)	(.031)	(.034)	(.047)	(.045)	(.045)
Moroccan 2nd generation	.221***	.311***	.190***	.112*	.157**	.169**
	(.033)	(.043)	(.046)	(.056)	(.054)	(.054)

Surinamese 1st generation	.132***	.223***	.152***	.065	.022	.029
	(.018)	(.032)	(.033)	(.047)	(.044)	(.044)
Surinamese 2nd generation	.080**	.186***	.113**	.026	.057	.067
	(.024)	(.037)	(.038)	(.051)	(.048)	(.048)
Antillean 1st generation	.238***	.328***	.245***	.157***	.104*	.112*
	(.018)	(.032)	(.033)	(.047)	(.044)	(.044)
Antillean 2nd generation	.007	.130***	.067	-.020	.013	.025
	(.024)	(.037)	(.038)	(.051)	(.048)	(.048)
Inter-ethnic contact		.006	.005	.004	.008	.008
		(.009)	(.009)	(.009)	(.009)	(.009)
*Inter-ethnic contact*Immigrant backg.*		-.046***	-.041***	-.040***	-.024*	-.023**
		(.011)	(.011)	(.011)	(.011)	(.011)
Intra-ethnic contact[a]		-.024	-.023	-.022	-.026*	-.025*
		(.013)	(.013)	(.013)	(.012)	(.012)
*Intra-ethnic contact*Immigrant backg.*		.032*	.028*	.027	.025	.024
		(.014)	(.014)	(.014)	(.013)	(.013)
Voluntary association (ref. no participation/membership)						
Mainly intra-ethnic contact		-.065**	-.056*	-.044	.006	.005
		(.024)	(.024)	(.024)	(.023)	(.023)
**Immigrant background*		.039	.035	.024	-.014	-.014
		(.034)	(.034)	(.034)	(.032)	(.032)
Mixed or mainly inter-ethnic contact		-.045	-.043	-.040	.019	.018
		(.035)	(.035)	(.034)	(.033)	(.033)
**Immigrant background*		-.028	-.022	-.026	-.035	-.033
		(.038)	(.037)	(.037)	(.036)	(.036)
Prop. BR in ethnic in-group in neighb[a]			.609***	1.399***	.776**	.661**
			(.073)	(.277)	(.249)	(.260)
*Prop. BR in-group*Turk/Moroccan*				-.920**	-.411	-.287
				(.303)	(.274)	(.280)
Prop. BR *in-group*Surinamese/* Antillean				-.773**	-.376	-.272
				(.293)	(.264)	(.266)
Individual-level covariates	NO	NO	NO	NO	YES	YES
Neighborhood-level covariates	NO	NO	NO	NO	NO	YES

Notes: [a]= Variables are mean-centered; [b]= variable is ln-transformed and then mean-centered, in 1000 euros; BR= benefit receipt. *= p<.05; **= p<.01; ***= p<.001 (two-sided). Estimated on multiply imputed data. Robust standard errors in parentheses. See table A3.2 in the appendix (section 3.6) for coefficients of covariates. N(individuals)= 6,767.

Intra- or inter-ethnic contacts through voluntary associations do not affect people's chance of benefit receipt. For native people, model 2 initially suggests a beneficial effect of intra-ethnic contact through this channel, but this becomes insignificant in model 4. The same holds for immigrants: The interaction effect is not significant in all models. Regarding mixed or mainly inter-ethnic contacts through voluntary associations, all coefficients are insignificant in model 2, and this does not change when we add individual and neighborhood level covariates. Overall, these results regarding contact through voluntary associations offer little support of our hypotheses (H1b and H2b).

Persons living in neighborhoods with a higher proportion of benefit recipients among one's ethnic in-group have a higher probability of receiving benefits themselves, which is in line with hypothesis 4. Model 3 shows that this effect is rather substantial: If the neighborhood share of co-ethnics on benefits rises by one standard deviation, the probability of benefit receipt increases by 6.1 percent point. In model 4, we include interaction effects between the proportion of benefit recipients among co-ethnics in the neighborhood and the ethnic background. The positive main effect and negative interaction effects in model 4 initially suggest that the impact runs in the opposite direction of our expectations the proportion of benefit recipients in the neighborhood has a stronger effect among native Dutch than among immigrants. However, the interaction effects become insignificant when introducing control variables in models 5 and 6. This implies that we do not find support for hypothesis 5a, nor for our expectation that the impact of the proportion of benefit recipients among the ethnic in-group on individual benefit receipt is stronger for people of Turkish or Moroccan origin than for Surinamese and Antilleans immigrants (H5b). Because the main effect of proportion of benefit recipients among the ethnic in-group in the neighborhood remains significant across the models, the relationship holds irrespective of ethnic background. This general effect is also substantial: According to model 6, a standard deviation increase in the proportion of benefit recipients among the ethnic in-group is associated with a 6.6 percent rise in the probability of benefit receipt. It is important to note that this holds even when the impact of the proportion of benefit recipients among ethnic outgroup members is taken into account (model 6).

3.4.2 Explaining native-immigrant differences

Table 3.4 shows the estimated average native-immigrant difference in likelihood of benefit receipt, and the impact of the control variables and the social network indicators in reducing these differences (H3 and H6). First, we see that the native-immigrant differential is reduced once taking into account individual and neighborhood covariates. Overall, the reduction in the differential is most pronounced for first-generation immigrants. For Turkish and Moroccan immigrants, the relative difference in benefit

receipt becomes comparable for first- and second-generation immigrants, suggesting the composition of socioeconomic characteristics are uneven across the groups. Further, adding the individual and neighborhood covariates can for a larger part explain away the differences between Surinamese and Antillean immigrants, and natives. This is evidenced by the overall lower differential for these groups compared to Turkish and Moroccan immigrants, except for Antillean first-generation immigrants whose differential of 32.8 roughly equals that of Turkish first- and second-generation immigrants. For Antillean second-generation immigrants, the differential is nearly non-existent with only a 1.7 percent higher likelihood of benefit receipt compared to natives.

Adding inter- and intra-ethnic contact to the model does not alter the native-immigrant differential substantially. For Turkish and Moroccan first- and second-generation migrants, there is no change in their relatively higher chance of benefit receipt compared to natives. For Surinamese and Antillean first- and second-generation migrants, we even see a slight increase in the differential. On account, this does not suggest that immigrants' relatively higher benefit receipt can be explained by our indicators of inter- and intra-ethnic social contact, lending no support to hypothesis 3.

Including the effects of the proportion of benefit recipients among the ethnic in-group in the neighborhood makes for a drop in the relatively higher chance of immigrants to receive benefits, in line with hypothesis 6. Among Moroccan first- and second-generation migrants, the native-immigrant differentials decrease from 50.0 and 57.2 to 29.3 and 35.4 percent, respectively. Turkish first- and second-generation, and Antillean first-generation migrants experience a similar decrease in the native-immigrant differential from around 34 to around 16 or 19 percent. For Surinamese second-generation immigrants, the differential drops to 6 percent and it is negative at -6 and -5 for Antillean second-generation and Surinamese first-generation immigrants, respectively, implying a lower chance of benefit receipt compared to natives. In sum, table 3.4 makes clear that the native-immigrant differential can be partially explained by the individual and neighborhood control variables, and by the presence of benefit recipients among one's own ethnic group in the neighborhood. Inter- and intra-ethnic contact do not explain the native-immigrant differential, although it does to some extent affect individuals' probability of benefit receipt (table 3.3).

Table 3.4. Predicted probabilities of benefit receipt by ethnic background and relative difference in probability of benefit receipt between natives and immigrant groups.

	Estimated P(Y=1) and relative native-immigrant differential			
	(1) Only ethnic background[a]	(2) + All covariates[b]	(3) + Inter- and intra-ethnic contact[c]	(4) + Prop. benefit recipients among ethnic in-group[d]
Dutch natives	.151	.296	.304	.348
Turkish 1st gen	.420	.398	.408	.403
Relative difference	178.15	34.46	34.21	15.81
Turkish 2nd gen	.289	.394	.407	.403
Relative difference	91.39	33.12	33.88	15.81
Moroccan 1st gen	.424	.445	.456	.450
Relative difference	180.80	50.34	50.00	29.31
Moroccan 2nd gen	.372	.463	.478	.471
Relative difference	146.36	56.42	57.24	35.35
Surinamese 1st gen	.283	.314	.330	.331
Relative difference	87.42	6.08	8.55	-4.86
Surinamese 2nd gen	.231	.352	.370	.369
Relative difference	52.98	18.92	21.71	6.03
Antillean 1st gen	.389	.393	.409	.414
Relative difference	157.62	32.77	34.54	18.97
Antillean 2nd gen	.158	.301	.320	.327
Relative difference	4.64	1.69	5.26	-6.03

Notes: [a]= Dummy variables for having first- or second-generation Turkish, Moroccan, Surinamese, or Antillean immigrant background with Dutch native as reference category included (the same model as model 1 in table 3.3); [b]= level of education (in years), only education abroad and its interaction with education, self-rated health, male, whether living with partner and its interaction with male, nr. of children younger than 18 in household and its interaction with male, age (at time of survey) and age-squared, survey year (ref. 2006), proportion of benefit recipients among ethnic out-group in neighborhood, and ln-transformed average value of residential property are added to the model; [c]= inter-ethnic contact, intra-ethnic contact, type of ethnic contact in voluntary associations, and their interactions with

ethnic group are added to the model; [d]= proportion of benefit recipients among ethnic in-group, and its interaction with Turkish or Moroccan, and Surinamese or Antillean ethnic background are added to the model (the same model as model 6 in table 3.3). Predicted probabilities based on linear probability models and estimated at variables zero or mean-centered zero, except for inter-ethnic contact that is evaluated at its sample mean. Based on same multiply data as in table 3.3. See table A3.3 for results of models *not* shown in table 3.3, i.e. (2) and (3). N(individuals)= 6,767.

3.5 Discussion and Conclusions

The chief aim of this chapter was to investigate how social contacts and social networks affect benefit receipt among natives and immigrants, and how this may explain the observed native-immigrant differential in benefit receipt. Drawing on two largely disconnected strands of literature, we investigated the role of (1) inter- and intra-ethnic contact, generally expected to enhance ethnic minorities' labor market outcomes; and (2) the concentration of benefit recipients in an individual's social environment, generally thought to lead to poorer labor market outcomes. Examining these two factors simultaneously and to what extent they account for differences in benefit receipt between natives and immigrant groups have so far not been done. We investigated this using Dutch cross-sectional survey data enriched with individual-level longitudinal administrative data, containing a nationally representative sample of Dutch natives and first- and second-generation immigrants from the four largest ethnic minority groups (Turkish, Moroccan, Surinamese, and Antillean) in the Netherlands. Based on the results, we draw three main conclusions.

First, we find that more social contact with native Dutch friends or acquaintances decreases the chance of benefit receipt for both migrants and natives. For migrants such contacts are inter-ethnic, for natives intra-ethnic. Having more friends or acquaintances with an immigrant background does not matter for the chance of benefit receipt. It seems that it is 'general' social contact with friends and acquaintances rather than social contacts acquired through voluntary associations that matter, as the type of contact in voluntary associations does not affect benefit receipt. On the whole, these findings are largely in line with our theoretical expectations, and corroborate findings from previous literature (Heizmann and Böhnke 2016; Kanas, van Tubergen, and van der Lippe 2011; Kanas et al. 2012; Lancee 2010; 2012; de Vroome and van Tubergen 2010). In particular, the results suggest that general contact with natives make for a lower chance of benefit receipt possibly because they serve as a vital source of labor market information for both natives and immigrant groups. For immigrants, such contact may also include vital host-country knowledge, including practicing of host-country language. This may explain why we find that intra-ethnic contact does not affect immigrants' likelihood of benefit receipt.

Second, the results suggest that both natives and first- and second-generation immigrants are affected equally by the concentration of benefit recipients among their own ethnic group in the neighborhood. Specifically, the higher the share of benefit recipients among their in-group, the higher the likelihood of benefit receipt. Counter to our theoretical expectations, we do not find that this relationship is stronger for people with an immigrant background compared to natives, nor that the relationship is stronger for Turkish and Moroccan immigrants compared to Antillean and Surinamese immigrants. Importantly, this holds even when the impact of benefit recipients among the ethnic out-group is considered, which on its own does not affect individuals' chances of benefit receipt. One explanation for this unexpected finding might be that although the immigrant groups studied here are relatively small, they are numerically large enough to counter increased normative pressures: There were between about 340,000 to 380,000 people with a Turkish, Moroccan and Surinamese background, and roughly 130,000 people with an Antillean background in the Netherlands in 2009.

Third, we find that the concentration of benefit recipients among the ethnic in-group explains the native-immigrant differential in benefit receipt. Intra- and inter-ethnic contact do not seem to explain the differences to a large extent, although it does affect the likelihood of benefit receipt. This runs counter to our theoretical expectation. The explanatory value of the concentration of benefit recipients in the social environment may indicate that studying not only the ethnic background of people in individuals' social environment, but rather the overlap of ethnic background and behavior is pivotal in understanding social contact's influence on benefit receipt. That would entail moving away from concepts of bridging and bonding social capital (Putnam 2000) along ethnic lines, toward a broader conceptualization of social capital or social resources that stems from the socioeconomic resources of people's social contacts (Lin 1999). In line with this, findings from a Swiss study suggest that the self-reported number of employed people in the network is associated with a higher likelihood of exiting unemployment for both Swiss natives and immigrant groups (Auer, Bonoli, and Fossati 2017); yet this study did not explicitly test whether this effect differs among natives and immigrants. To investigate this further, future research should collect systematic information on the network of individuals, including on their (former) employer and the employment situation of their family members and friends.

We could not, however, completely explain away the increased likelihood of benefit receipt for first- and second-generation Turks and Moroccans, and first-generation Antilleans. This may suggest that other explanatory factors are at stake, such as norms and values (see Koopmans 2016), which we have not explicitly accounted for in our empirical models. Additionally, we did not include any measures of language proficiency, which is known to affect immigrants' labor market integration (Chiswick and Miller 2002). The unexplained variance could partially reflect this, although some of

the influence of language abilities for the immigrant groups may have been captured by the inter-ethnic contact measure – in so far as host-country language acquisition is a prerequisite for inter-ethnic friendships for immigrants (Martinovic, van Tubergen, and Maas 2011). We encourage future research to pay explicit attention to the interdependence between language abilities and (inter-ethnic) social contact in explaining the ethnic majority-minority differential in benefit dependence.

Because we tested interaction effects separately for natives and immigrant groups for the relatively rare event of benefit receipt, our models may suffer from a lack of statistical power. This can particularly be the case for the Dutch native respondents, in which only 176 respondents received a benefit. Consequently, our conclusions here are conservative in the sense that small effects will go undetected. Overall, while social network effects tend to be small, their cumulative impact may nevertheless be substantial (see Rege, Telle, and Votruba 2012).

Another limitation pertains to selection and reversed causality. Individuals may move to neighborhoods with low rents and high rates of benefit receipt, because they receive benefits themselves or are very likely to do so in the near future. Furthermore, patterns of inter- and intra-ethnic contact may be shaped by one's employment status (Martinović 2013; Martinovic, van Tubergen, and Maas 2011). Especially immigrants may have more contact with Dutch natives because they meet them through work. We have tried to reduce the bias stemming from these processes by (1) including relevant control variables in the analyses, and (2) measuring the response variable (benefit receipt) after the measurement of the explanatory variables. Nevertheless, we cannot rule out that these processes have biased our results. Experimental and longitudinal data are necessary to avoid the bias stemming from particularly selection into neighborhoods.

This chapter examined how social networks may explain differences between natives and first- and second-generation immigrants in benefit receipt, which has so far received relatively little scholarly attention. Future research could investigate this relationship in other countries that differ in terms of their social security design (e.g. by comparing network effects among ethnic groups in Anglo-Saxon, Continental and Nordic welfare states), their immigration history (e.g. colonial legacies, refugee flows), and their labor market and integration policies towards migrants (e.g. requirements for legal status, social inclusion attempts, involvement of local policy actors and the civil society). This chapter has shown that in explaining native-immigrant differences in benefit receipt, it is particularly important to consider the concentration of benefit receipt in people's (ethnic in-group) social environment and to not focus solely on the degree of inter- and intra-ethnic contact.

3.6 Appendix

Table A3.1. Descriptive statistics for the analytical sample including means, standard deviations, 1st and 99th percentile, and N non-missing observations. N(individuals)= 6,767.

Variable	Mean	SD	P1	P99	N
Benefit receipt future 2 yrs*	.320		0	1	6740
Inter-ethnic contact	2.069	1.134	0	3	6765
Intra-ethnic contact	3.641	1.172	1	5	6430
Contact in associations (ref. no participation/membership)	.526		0	1	6767
Mainly intra-ethnic contact	.165		0	1	6767
Mixed/mainly inter-ethnic contact	.309		0	1	6767
Prop. BR in ethnic in-group in neighb	.223	.100	.000	.450	6706
Level of education (in years)	11.069	2.746	6	16	6332
Followed education abroad only (ref. followed education in NL)	.304		0	1	6767
Self-rated general health	2.871	.845	1	4	6765
Living with partner* (ref. not living with partner)	.646		0	1	6767
Nr. of children <18	1.013	1.164	0	4	6756
Male* (ref. female)	.478		0	1	6767
Age (at time of survey)	39.802	11.104	19	62	6767
Survey 2011* (ref. 2006)	.427		0	1	6767
Prop. BR in ethnic out-group in neighb	.073	.067	.001	.274	6706
Average value of residential property**	180.288	66.633	89	395	6723
Ln(av. value of residential property)	5.138	.328	4.489	5.979	6723
Ethnic group* (ref. Dutch native)	.173		0	1	6767
Turkish 1st generation	.178		0	1	6767
Turkish 2nd generation	.039		0	1	6767
Moroccan 1st generation	.171		0	1	6767

Moroccan 2nd generation	.034	0	1	6767
Surinamese 1st generation	.148	0	1	6767
Surinamese 2nd generation	.054	0	1	6767
Antillean 1st generation	.160	0	1	6767
Antillean 2nd generation	.043	0	1	6767

Notes: *= Binary/categorical variables for which standard deviations are not shown; **= ln 1000 Euro; BR= benefit receipt.

Table A3.2. Results for linear probability models predicting the likelihood of receiving benefits at least 1 month in the 2 years after the survey – full table.

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Intercept	.151***	.190***	.257***	.345***	.348***	.332***
	(.010)	(.023)	(.025)	(.042)	(.042)	(.042)
Ethnic group (ref. Dutch native)						
Turkish 1st generation	.269***	.319***	.213***	.132**	.093**	.101*
	(.018)	(.030)	(.033)	(.047)	(.045)	(.045)
Turkish 2nd generation	.138***	.220***	.116**	.034	.091	.101*
	(.030)	(.040)	(.042)	(.053)	(.051)	(.051)
Moroccan 1st generation	.273***	.336***	.218***	.139**	.137**	.148***
	(.018)	(.031)	(.034)	(.047)	(.045)	(.045)
Moroccan 2nd generation	.221***	.311***	.190***	.112*	.157**	.169**
	(.033)	(.043)	(.046)	(.056)	(.054)	(.054)
Surinamese 1st generation	.132***	.223***	.152***	.065	.022	.029
	(.018)	(.032)	(.033)	(.047)	(.044)	(.044)
Surinamese 2nd generation	.080**	.186***	.113**	.026	.057	.067
	(.024)	(.037)	(.038)	(.051)	(.048)	(.048)
Antillean 1st generation	.238***	.328***	.245***	.157***	.104*	.112*
	(.018)	(.032)	(.033)	(.047)	(.044)	(.044)
Antillean 2nd generation	.007	.130***	.067	-.020	.013	.025
	(.024)	(.037)	(.038)	(.051)	(.048)	(.048)
Inter-ethnic contact		.006	.005	.004	.008	.008
		(.009)	(.009)	(.009)	(.009)	(.009)
*Inter-ethnic contact*Immigrant backg.*		-.046***	-.041***	-.040***	-.024*	-.023**
		(.011)	(.011)	(.011)	(.011)	(.011)
Intra-ethnic contacta		-.024	-.023	-.022	-.026*	-.025*
		(.013)	(.013)	(.013)	(.012)	(.012)
*Intra-ethnic contact*Immigrant backg.*		.032*	.028*	.027	.025	.024
		(.014)	(.014)	(.014)	(.013)	(.013)
Voluntary association (ref. no participation/membership)						
Mainly intra-ethnic contact		-.065**	-.056*	-.044	.006	.005
		(.024)	(.024)	(.024)	(.023)	(.023)

**Immigrant background*	.039	.035	.024	-.014	-.014
	(.034)	(.034)	(.034)	(.032)	(.032)
Mixed or mainly inter-ethnic	-.045	-.043	-.040	.019	.018
	(.035)	(.035)	(.034)	(.033)	(.033)
**Immigrant background*	-.028	-.022	-.026	-.035	-.033
	(.038)	(.037)	(.037)	(.036)	(.036)
Prop. BR in ethnic in-group in neighb.[a]		.609***	1.399***	.776**	.661**
		(.073)	(.277)	(.249)	(.260)
*Prop. BR in-group*Turk/ Moroccan*			-.920**	-.411	-.287
			(.303)	(.274)	(.280)
*Prop. BR in-group*Surinamese/ Antillean*			-.773**	-.376	-.272
			(.293)	(.264)	(.266)
Level of education (in years[a]				-.016***	-.016***
				(.003)	(.003)
Education abroad only (ref. edu. NL)				.035*	.034*
				(.016)	(.016)
*Level of education*edu. abroad*				.002	.002
				(.005)	(.005)
Self-rated general healtha				-.131***	-.131***
				(.007)	(.007)
Living with partner (ref. not living with partner)				-.168***	-.166***
				(.017)	(.017)
*Living with partner*Male*				.049*	.049*
				(.024)	(.024)
Nr. of children <18				.014	.014
				(.009)	(.009)
*Nr. of children*Male*				-.024*	-.024*
				(.011)	(.011)
Male (ref. female)				-.017	-.016
				(.020)	(.020)
Age (at time of survey)[a]				.003***	.003***
				(.001)	(.001)
Age2 (at time of survey)[a]				-.000	-.000
				(.000)	(.000)
Survey 2011 (ref. 2006)				.077***	.082***
				(.011)	(.011)

Table A3.2 continued

Prop. BR in ethnic out-group in neighb.[a]	-.081
	(.135)
Average value of residential property[b]	-.032
	(.021)

Notes: [a]= Variables are mean-centered; [b]= variable is ln-transformed and then mean-centered, in 1000 euros. *= p<.05; **= p<.01; ***= p<.001 (two-sided). Estimated on multiply imputed data. Robust standard errors in parentheses. N(individuals)= 6,767.

Table A3.3. Results for linear probability models predicting the likelihood of receiving benefits at least 1 month in the 2 years after the survey – additional models underlying table 3.4.

	Model 1	**Model 2**
Intercept	.296***	.288***
	(.021)	(.028)
Ethnic group (ref. Dutch native)		
Turkish 1st generation	.102***	.154***
	(.022)	(.033)
Turkish 2nd generation	.098**	.153***
	(.032)	(.041)
Moroccan 1st generation	.149***	.202***
	(.022)	(.034)
Moroccan 2nd generation	.167***	.224***
	(.037)	(.045)
Surinamese 1st generation	.018	.076*
	(.018)	(.031)
Surinamese 2nd generation	.056*	.116**
	(.025)	(.037)
Antillean 1st generation	.097***	.155***
	(.019)	(.032)
Antillean 2nd generation	.005	.066
	(.025)	(.037)
Inter-ethnic contact		.008
		(.009)
*Inter-eth contact*Immigrant backg.*		-.025*
		(.011)
Intra-ethnic contacta		-.024*
		(.012)
*Intra-eth contact*Immigrant backg.*		.024
		(.013)
Voluntary association (ref. no participation/membership)		
Mainly intra-ethnic contact		.001
		(.023)
**Immigrant background*		.017
		(.033)
Mixed or mainly inter-ethnic		-.009
		(.032)
**Immigrant background*		-.033
		(.036)

Table A3.3 continued

Prop. BR in ethnic in-group in neighb.[a]		
*Prop. BR in-group*Turk/Moroccan*		
*Prop. BR in-group*Surinamese/Antillean*		
Level of education (in years)[a]	-.017***	-.016***
	(.003)	(.003)
Education abroad only (ref. edu. NL)	.042**	.034*
	(.015)	(.016)
*Level of education*edu. abroad*	.003	.002
	(.005)	(.005)
Self-rated general health[a]	-.133***	-.131***
	(.007)	(.007)
Living with partner (ref. not living with partner)	-.169***	-.168***
	(.017)	(.017)
*Living with partner*Male*	.050*	.049*
	(.024)	(.024)
Nr. of children <18	.016	.015
	(.009)	(.009)
*Nr. of children*Male*	-.026*	-.025*
	(.011)	(.011)
Male (ref. female)	.003***	.003***
	(.001)	(.001)
Age (at time of survey)[a]	-.000	-.000
	(.000)	(.000)
Age2 (at time of survey)[a]	-.017	-.016
	(.020)	(.020)
Survey 2011 (ref. 2006)	.089***	.089***
	(.011)	(.011)
Prop. BR in ethnic out-group in neighb.[a]	.250*	.259**
	(.097)	(.098)
Average value of residential property [b]	-.088***	-.082***
	(.017)	(.017)

Notes: [a]= Variables are mean-centered; [b]= variable is ln-transformed and then mean-centered, in 1000 euros. *= p<.05; **= p<.01;***= p<.001 (two-sided). Estimated on multiply imputed data. Robust standard errors in parentheses. Models 1 and 2 underpin models (2) and (3), respectively, in table 3.4. N(individuals)= 6,767.

Chapter 4.

Refugees and the Transition from Welfare to Work: A Quasi-Experimental Approach of the Impact of the Neighborhood Context[47]

47 A slightly different version of this chapter is under review at an international scientific journal. The chapter is co-authored by I. Maas, S. Boschman, and J. C. Vrooman. Kristiansen wrote the main part of the manuscript and conducted the analyses. Maas, Boschman, and Vrooman contributed substantially to the manuscript. The authors jointly developed the idea and research design, in the context of the project From network to work? at Utrecht University. Earlier versions of this chapter have been presented at the IAB Workshop 'Welfare and Unemployment Dynamics' in Nuremberg, 2018, and at the 'Vinterseminar' of the Norwegian Sociology Association in Geilo, 2019. A special thanks to the Dutch Central Agency for the Reception of Asylum Seekers (COA) and to Mark Kattenberg at the Netherlands Bureau for Economic Policy Analysis (CPB) for lending help with the data.

Abstract

This chapter analyzes the impact of the neighborhood context on the likelihood that refugees move from social assistance to paid employment. It makes use of a Dutch policy measure that resulted in an exogenous placement of refugees with a residence permit in their first regular housing. This natural quasi-experiment allows us to estimate intent-to-treat effects of initial neighborhood characteristics on the likelihood of transitioning from welfare to work. We consider the impact of the employment share and the median level of income among natives and co-ethnics, using monthly Dutch longitudinal administrative data and discrete time event-history modeling. Our findings indicate that refugees are more likely to enter the labor market when the neighborhood's employment share among co-ethnics and natives is higher. However, there is no evidence that the placement of refugees in an area with a higher median income among co-ethnics or natives facilitates the transition from welfare to work.

4.1 Introduction

Over the past decades, immigration and asylum seeker flows have become a major concern in the public debate of many European countries. These discussions do not only revolve around the possibility and desirability of regulating entry at national borders, but also around the weak labor market integration of newcomers and processes of residential segregation by ethnic origin. Refugees tend to end up in a particularly vulnerable state, compared to both the native population and labor migrants (see Bakker, Dagevos, and Engbersen 2017; Engbersen et al. 2015 for Dutch evidence). While the labor market position attained by refugees improves over time (Bakker, Dagevos, and Engbersen 2017), they usually do not catch up completely (Wooden 1991; Zwysen 2019). For instance, Engbersen et al. (2015) find that among the refugees arriving in the Netherlands between 1995-1999, about 50 percent had a job after 10 years of residency, up from about 22 percent after 2 years of residency. From a policy perspective, this makes it pertinent to understand the factors affecting refugees' labor market outcomes.

The social context refugees encounter upon arrival in their host country is arguably particularly important in shaping their future labor market outcomes. Refugees initially do not have an extensive personal network, and are more likely to form new social ties compared to other types of immigrants (see Andersson, Musterd, and Galster 2019). Under the implicit assumption that refugees mainly form social relations with people from the same ethnic background, the characteristics of co-nationals in the receiving neighborhood or local area – sometimes termed the 'ethnic enclave' – have been argued to either promote or inhibit refugees' labor market outcomes. Refugees are supposed to fare better if they become embedded in an ethnic enclave dominated by employment, as these co-nationals will be able to provide information about job leads. Similarly, larger ethnic enclaves may offer employment opportunities within an 'ethnic' or niche economy (e.g. Wilson and Portes 1980). However, a large ethnic enclave may also serve as a disincentive for acquiring relevant host-country skills (especially language) because it lowers contact with natives (Lazear 1999). This can in turn decrease the labor market chances of newly arrived refugees.

Clear scientific evidence on the relative strength of these arguments, however, is difficult to obtain. It is hard to separate the influence of (1) sorting or self-selection into certain areas, from (2) the actual – or causal – influence of the composition of the neighborhood on an individual. Previous empirical analyses provide conflicting evidence on the influence of living in an ethnic enclave on immigrants' labor market outcomes. Some studies show a negative relationship between co-ethnic clustering and immigrants' labor market outcomes (e.g. Borjas 2000; Clark and Drinkwater 2002; Gal-

ster, Metzger, and Waite 1999; Logan, Zhang, and Alba 2002), while others have found more mixed or nuanced evidence (e.g. Andersson, Musterd, and Galster 2014; Sanders and Nee 1987; Zhou and Logan 1989; Xie and Gough 2011). One strategy to deal with selection bias is to identify natural quasi-experiments, in which sorting is independent of unobserved individual characteristics. Along this line, a more recent stream of research has drawn on refugees' initial place of residence that – due to the settlement policy in some countries – is independent of the refugees' unobserved characteristics (Åslund and Fredriksson 2009; Beaman 2012; Damm 2009; 2014; Edin, Fredriksson, and Åslund 2003). The outcomes of these studies are, however, not clear-cut either.

In this chapter, we draw on an exogenous placement procedure of Dutch refugees to deal with potential sorting bias. We add to earlier related studies from Denmark, Sweden, and the US by, first, offering evidence from the Netherlands, which is often regarded as a unique 'hybrid' welfare state (Ferragina and Seeleib-Kaiser 2011). Second, we model the transition from welfare to work as a dynamic process. Whereas previous research has mainly looked at refugees' labor market outcomes 5 or 10 years after settlement, we make use of monthly, longitudinal data and employ event-history models to analyze these. Third, we also consider the characteristics of the native population in the area. Drawing on the ethnic enclave literature, past studies have mainly focused on the characteristics of co-nationals. We presume that the native population can play an important role in shaping the labor market outcomes of refugees, especially if these end up in an area with relatively few co-nationals.

We employ administrative data from the Netherlands to investigate the following research question: To what extent do the concentration and characteristics of co-nationals and of the native population in the neighborhood affect refugees' likelihood of transitioning from social assistance to work? The data enable us to identify refugees – an asylum migrant with a granted residence status – over time, their initial place of residence, and their subsequent labor market activity. We focus on refugees placed in regular housing between 1999 to 2009 and analyze to what extent they enter the labor market during the ten years following their initial placement in regular housing.

4.2 Theory and Background

4.2.1 The influence of co-ethnics

Migration scholars have argued that living in ethnic enclaves – or ethnic concentration more generally – can either improve or hamper immigrants' labor market outcomes. Ethnic concentration may provide them with access to valuable 'ethnic' social capital.

For example, Portes and colleagues (Portes and Shafer 2007; Wilson and Portes 1980) have argued that ethnic enclaves can foster ethnic-niche economies that offer immigrants ample opportunity to find jobs. In such a niche economy, co-ethnics – people with the same national-origin background – provide job leads and access to jobs, and thereby a higher likelihood of employment. It is debated to what extent immigrants' earnings are also positively affected (e.g. Sanders and Nee 1987; Xie and Gough 2011). A more general version of this argument states that co-ethnics may offer recently arrived immigrants with a broad range of information, such as direct job leads, how to apply for jobs, or the best way to navigate the host country's labor market – and this is not necessarily confined to jobs within an ethnic niche economy.

Scholars have also pointed to the potentially detrimental influences of ethnic concentration. By lowering contact with natives, immigrants are discouraged to acquire human capital that is specific to the host country (Lazear 1999), especially the host-country language. This makes it in turn more difficult for immigrants to succeed on the host-country labor market (e.g. Chiswick and Miller 2001; de Vroome and van Tubergen 2010; Kanas and van Tubergen 2009). This mechanism is argued to be stronger if the relative size of the immigrant group is larger (Lazear 1999).

A slightly different argument posits that it is not the ethnic concentration and number of co-ethnics that matters the most for immigrants' labor market outcomes. Rather, it is crucial to consider the 'quality' (Damm 2014) or socioeconomic characteristics of co-ethnics. This fits within a social capital framework (e.g. Lin 1999; McDonald et al. 2013), in which social resources among co-ethnics determine to what extent co-ethnics can help immigrants integrate in the labor market. These resources include the extent of employment among co-ethnics and the type of jobs they have. For example, a higher employment rate among co-ethnics will increase the flow of important host-country labor market information, which can facilitate a swift labor market integration for recently arrived immigrants. In the economic literature, this notion is in line with the 'ethnic capital' or the human externality argument (Borjas 1995).

The empirical results from the literature so far do not offer conclusive evidence in favor of either arguments. Some studies suggest a negative relationship, others a mixed or no relationship between the share of co-ethnics in the neighborhood and immigrants' labor market outcomes (see Andersson, Musterd, and Galster 2019). One reason for these diverging results is arguably the difficulty in dealing with selection bias: How to separate the effect of the neighborhood from the tendency of people with specific (unobservable) characteristics to cluster in a specific neighborhood? Here, we briefly review research that is closest to our approach: Studies that draw on the exogenous placement of refugees. Methodologically, the initial exogenous placement of refugees

upon granted asylum has mainly been used as an instrument to study the influence of the (current) neighborhood on labor market outcomes after settlement (Åslund and Fredriksson 2009; Damm 2009; 2014; Edin, Fredriksson, and Åslund 2003). Regarding the concentration of co-ethnics, the main result is that there is no effect on immigrants' welfare receipt (Åslund and Fredriksson 2009) and employment (Damm 2009; 2014). For earnings, the empirical results are more mixed. Swedish evidence suggests that a higher number of co-ethnics makes for higher earnings, but only for the lower educated (Edin, Fredriksson, and Åslund 2003). In Denmark, Damm (2009) finds that a higher number of co-ethnics positively affects earnings, but Damm (2014), controlling for the quality of the neighborhood[48], finds no impact of the number of co-ethnics in the neighborhood. Separating the effects of yearly inflows of exogenously placed refugees in the US, Beaman (2012) finds that a recent higher inflow of co-ethnics makes for lower employment probabilities, and lower earnings, whereas a higher inflow three years prior to settlement, in contrast, makes for higher employment probabilities, and higher earnings. This suggests that the influence of number of co-ethnics may depend on when the other co-ethnics arrived.

There is more clear-cut evidence that the socioeconomic characteristics of the co-ethnics affect refugees' labor market outcomes. Damm (2009; 2014) finds that both the level of education and the level of income among co-ethnics positively affect earnings and employment. Similarly, Åslund and Fredrikson (2009) conclude that a higher share of welfare recipients among co-ethnics increases the likelihood of welfare receipt.

The results emanating from previous studies thus are somewhat mixed. Taken together, however, they point to a relationship between better labor market outcomes among co-ethnics and better individual labor market outcomes among refugees. As for the share of co-ethnics in the population, there does not seem to be any relationship

48 The quality means in this case level of income among the co-ethnics in the neighborhood, and percentage employed among the co-ethnics.

with the refugee's labor market outcomes. Combining theory with these results, we formulate the following hypotheses:

H1a: A higher employment rate among co-ethnics makes refugees more likely to transition from social assistance to work.

H1b: A higher level of income among co-ethnics makes refugees more likely to transition from social assistance to work.

4.2.2 The influence of natives

The studies concerned with the impact of ethnic enclaves and ethnic concentration have mainly investigated the role of co-ethnics. Little attention has been paid to the potential role of natives for immigrants' labor market achievement. Implicitly, it is assumed that immigrants tend to form social ties with co-ethnics – i.e. a (strong) preference for ethnic homophily (McPherson, Smith-Lovin, and Cook 2001) – and that these are the most consequential for their labor market outcomes. In general it is true that immigrants – and people in general – tend to form social ties with people with a similar ethnic background (e.g. DiPrete et al. 2011; McPherson, Smith-Lovin, and Cook 2001; Schaeffer 2013; van Tubergen 2015). However, other studies show that ethnic minorities also maintain social contacts with natives and other ethnic minority groups (e.g. Martinović 2013; Vervoort, Flap, and Dagevos 2011).

Moreover, social ties with natives and other ethnic groups may be relatively more important for refugees' labor market outcomes. As argued by Putnam (2000) it is particularly bridging social capital, which spans socioeconomic or ethnic boundaries, that offers high access to unique information. Along this line, a qualitative study among Romanians in London points to the important role social ties with natives can play in the acquisition of cultural resources needed to navigate the formal labor market in the host country (Moroşanu 2016). Similarly, Gericke et al. (2018) offer qualitative evidence on Syrian refugees in Germany indicating that particularly social contacts with a different ethnic background were helpful in gaining access to both low- and high-skilled jobs. Social contacts with co-nationals tended to only yield access to low-skilled jobs. This is partially mirrored in quantitative research, where some studies suggest that social contact with natives is associated with better labor market outcomes, while the evidence regarding contact with co-ethnics is more mixed (e.g. Heizmann and Böhnke 2016; Kanas, van Tubergen, and van der Lippe 2011; Lancee 2016).

Especially for relatively small and/or recent refugee groups, social contact with natives may represent a valuable source of information about the host country. This latter argument also follows from Lazear (1999) who posits that relatively small ethnic

minorities have a stronger incentive to form social ties with the ethnic majority population and thus are better integrated in the host society. Here, we argue that refugees residing in areas with fewer co-ethnics will be influenced more strongly by natives in the area due to limited contact opportunities with co-ethnics. To our knowledge, studies that have investigated this empirically for refugees are few. Markussen and Røed (2015) juxtapose the influence of co-ethnics in the neighborhood with the influence of other (non-Western) immigrants and natives, and find that it is particularly the co-ethnics in the neighborhood that affect individual social insurance receipt. Their study, however, does not focus solely on refugee groups but also includes first-generation non-Western immigrants who migrated for other reasons. Damm (2014) investigates refugees exclusively, and suggests that a high employment rate among non-Western immigrants from a different country-of-origin in the area improves immigrants' employment probability but does not affect earnings, although co-nationals seem to be more influential. Yet it remains unclear how the natives in the neighborhood affects refugees' labor market outcomes. Based on the theoretical considerations and the outcomes of previous research discussed above, we presume that:

H2a: A higher employment rate among natives makes refugees more likely to transition from social assistance to work.

H2b: A higher level of income among natives makes refugees more likely to transition from social assistance to work.

H3a: The influence of natives' employment rate on a refugee's likelihood of transitioning from social assistance to work weakens as the share of co-ethnics in the area increases.

H3b: The influence of natives' level of income on a refugee's likelihood of transitioning from social assistance to work weakens as the share of co-ethnics in the area increases.

4.2.3 The Dutch asylum procedure

This section provides a brief overview of the Dutch settlement policy as well as the application procedure preceding refugees' settlement, focusing on the policies effectuated between 1999-2009. During this period, the policies remained fairly unchanged.

Upon entry to the Netherlands, a refugee[49] undergoes a stepwise procedure from the moment a claim is submitted until the final decision is reached. An asylum request is first processed at an application center ('*aanmeldcentrum*') for about one week. The aim is to quickly reject invalid claims, in particular regarding refugees from countries that are considered safe. If the claim passes this initial check, refugees are moved to an asylum seeker reception center[50] ('AZC') to await further screening. These are housing units administered by the Central Agency for the Reception of Asylum Seekers ('COA'), a national governmental agency in charge of housing asylum seekers. COA assigns asylum seekers to the reception centers without considering their own preferences (Arnoldus, Dukes, and Muster 2003). The processing time in an AZC might take six months or longer, depending on the amount of asylum claims under review by the Dutch immigration and naturalization authorities. Especially between 1995-2001, this resulted in long waiting periods. Once a claim is approved, the refugee is granted a temporary or permanent residence permit[51].

A refugee may under specific conditions work while awaiting a final decision on his or her asylum claim. The most important condition is that the asylum processing time has exceeded six months. Additionally, the work has to meet the standard or normal working conditions, including wage, for that type of work. Formally, the employer must apply for and obtain a special permit ('*tewerkstellingsvergunning*') prior to hiring. If these conditions are met, the refugee may work up to 14 weeks – in 2008 extended to 24 weeks – during a 52-week period. Because the refugee has additional income while residing in an AZC, a certain amount of the income is subtracted and given to COA to cover e.g. housing costs. In 2008, the refugee could keep 25 percent of the income up to

49 Note that the person is technically an asylum migrant or seeker until the asylum claim has been approved. Because we focus on refugees who have 'successfully' undergone the asylum application procedure, we use the term refugee also in describing the asylum application process.

50 Periodically, the AZCs have been supplemented due to housing shortages by the so-called supplemental reception centers 'AVO'. We will in the following refer to AZC, but this also includes these equivalent housing arrangements.

51 A residence permit is typically always granted on a temporary basis at first. After up to five years, the refugee may apply for a permanent residence permit.

a maximum of 185 euros per month and the rest was paid to COA. After being granted a residence permit, however, these legal restrictions no longer apply[52].

Upon receiving a residence permit, the refugee has a legal obligation to integrate into Dutch society and must leave the asylum seeker center. Regular accommodation is with few exceptions assigned to the refugee, mainly in the form of rented social housing. All municipalities are required to provide accommodation for a certain number of refugees, depending on their number of inhabitants. COA is responsible for matching refugees to houses, and generally does not take into account their preferences. The agency may apply some 'objective' criteria, however, such as (1) whether the refugee has a job, (2) if family members (most notably partner/parent, children or siblings) reside in the Netherlands, or (3) whether the refugee requires (or undergoes) medical treatment only offered at a specific hospital. In these instances, housing is sought in proximity to the workplace, family members, or hospital. It is also known that refugees occasionally reject to move to small municipalities, although they are formally not allowed to do so (Dagevos 2007).

To ensure that our sample of refugees has been placed in regular accommodation exogenously – that is independent of unobserved characteristics – we add some selection criteria to our analytical sample. First, we exclude individuals who have worked while residing in an asylum-seeker center. This ensures that current or past employment will not affect the initial placement. Second, we exclude asylum seekers who have stayed in a housing arrangement that is specifically designed for asylum seekers with close relatives in the Netherlands. Such housing arrangements are also under the authority of COA, which implies we can detect these cases in our data. In doing so, we minimize the risk that family already present in the Netherlands affects the initial placement in regular housing. We cannot identify people who reject a housing offer. However, this is likely linked to the refugees' household situation; for instance, single refugees might be more inclined to reject offers from small municipalities. This will be accounted for by including detailed variables for the household composition in the initial place of residence in our statistical models. It should be noted that refugees' knowledge of the Netherlands is typically limited. We therefore assume that they will not reject housing offers based on their perception of local employment opportunities, and that rejections will not systematically bias our results.

52 If a refugee – after being granted a residence permit – has found work and still resides at an AZC, the refugee will receive reduced or no benefits while living at an AZC

4.3 Data and Methods

4.3.1 Data

We employ Dutch administrative data that contain longitudinal information on individuals' residence history, their household, migration history, major source of income, and several socioeconomic and demographic characteristics from 1999 to 2017[53]. These data cover the entire population and allow for the identification of refugees. To zoom in on our group of interest, we apply two main criteria. (1) We initially select those people who are registered with asylum as their main motivation for migrating to the Netherlands. This information is taken from an administrative dataset on immigrants with a non-Dutch nationality and their reason for immigrating, which is collected by the Dutch Immigration and Naturalization Service and was linked to the administrative data. (2) Subsequently we select the refugees that we can observe in an AZC. This is made possible by data on the exact address and opening (and closing) year and month of all COA-administered housing[54]. There are two reasons for this selection: First, by observing a person in an AZC reception center we ensure that the person has undergone the regular asylum procedure, and we are able to exclude refugees whose first regular housing is influenced by having close relatives in the Netherlands, as these people typically are placed in other types of reception centers. Second, in this way we can with certainty observe the exact timing of people's exit from COA-administered housing and thus pinpoint the first address after having left the reception center.

In addition to these two inclusion criteria, we impose some additional sample restrictions. We focus on refugees who are placed in regular accommodation during the period 1999-2009. The upper bound was chosen due to major changes in the asylum application procedure from 2010 and onwards, which affected the placement of refugees. In addition, we confine the analysis to people originating from the twelve largest refugee-sending countries during this period, to ensure sufficient numbers of co-ethnics[55]. These were in descending order: Afghanistan; Iraq; the former Soviet-Union; former Yugoslavia; Somalia; Angola; Iran; Sierra Leone; Sudan; China; Syria; and Turkey. Finally, we focus on people of prime working age, who were aged 25-55 when

53 All results are based on calculations by authors using non-public microdata from Statistics Netherlands. Under certain conditions, these microdata are accessible for statistical and scientific research. For further information: microdata@cbs.nl.

54 We have received this information from Mark Kattenberg (CPB) and COA, and subsequently linked it to the administrative data.

55 To determine the largest refugee-sending countries, we used publicly available data from Statistics Netherlands on the national origin of non-Dutch immigrants with asylum as main motive (Statistics Netherlands 2018).

they moved out of the AZC. This ensures that we can follow the oldest refugees for up to 10 years before they reach retirement age.

We arrived at our analytical sample in the following way. In our data, 174,194 people had asylum as main reason for immigration from the 12 countries of origin. Of these, 81,964 people immigrated in the period 1996-2009. After accounting for having ever stayed on an address administered by COA – i.e. is a refugee – and deselecting refugees in a housing arrangement for asylum seekers with close relatives in the Netherlands, 61,592 people remain. When we disregard people that receive a resident permit and leave a COA-address prior to 1999 – implying we cannot track their labor market status – 51,830 are left. After selecting for working age when first moving to regular housing, 24,166 refugees remain. Of these, 15,811 are observed in an AZC reception center – meaning they follow a regular application procedure involving exogenous placement in regular housing. 11,936 are left once removing those who move to regular accommodation after 2009. Finally, we exclude refugees who worked prior to moving to regular housing, who at the time of moving to regular housing were ineligible for social assistance (explained below), or who did not receive social assistance within the first 6 months. This resulted in an analytical sample consisting of 5,483 refugees. Obviously, this is only a small portion of the total population of refugees arriving to the Netherlands within this period; but we need to apply these selection criteria to ensure that the refugees in question have been exogenously placed.

To assess to what extent the placement is exogenous in terms of observed characteristics, we ran several regression analyses. Specifically, we modeled whether there are any systematic differences in the neighborhood or municipality characteristics by household position, ethnic-origin groups and age, respectively, across the years of placement. The results reinforce the assumption of exogenous placement[56], as we find nearly no systematic relation of observed personal characteristics with the neighborhood and municipalities characteristics we are interested in here. The differences we do find are mainly attributable to a very low number of refugees for certain subgroups. Note, however, that it is impossible to properly test whether refugees have been exogenously placed in regular housing as we assume that the placement is exogenous to *un*observed characteristics – unobservable in the data.

56 Unfortunately, we cannot show these results because certain subgroups have a too low number of refugees, which violates the CBS microdata privacy rules regarding identifiability and degrees of freedom in statistical analyses.

4.3.2 Operationalizations

Response variable

We rely on monthly information about individuals' major source of income to identify the transition from social assistance receipt to employment. Specifically, we define the transition as the moment that the individual's major source of income shifts from social assistance to labor. Because we are interested in the transition from welfare to work, we censor individuals who stop receiving social assistance for other reasons. In practice, this involves censoring individuals who (1) live in a household in which another household member starts working, or (2) who start receiving retirement benefits. Both (1) and (2) imply that the household is no longer eligible for social assistance. Social assistance is a means-tested benefit provided to households whose income and assets fall below the Dutch statutory social minimum. As a rule, refugees are automatically enrolled in the social assistance scheme upon being placed in regular housing. Among refugees with granted residence in 2014, 90 percent received social assistance after one year (Statistics Netherlands 2017). Exceptions to this are refugees who move in with and/or form a household with relatives or other individuals who receive an income above the social minimum. We include only individuals who receive social assistance benefits in the first month they are at risk. Additionally and to allow for administrative delays, we include individuals who are initially registered as 'others no income', if they (1) start receiving social assistance benefits within the first 6 months since moving to regular housing, and (2) there are no employed household members during this period.

Explanatory variables

The main explanatory variables are the characteristics of co-ethnics and natives in the first neighborhood in which refugees live after leaving the AZC, as well as the share of co-ethnics in the neighborhood. The 'neighborhood' is measured at two different levels: The 4-digit zip code for the characteristics for natives, and the municipality level for those of co-ethnics. We base this choice on the assumption that refugees have social contacts with native Dutch people in their immediate surrounding (neighborhood as defined by 4-digit zip code), whereas they are more willing to travel throughout the municipality to meet co-ethnics. Additionally, we need to ensure an adequate number of co-ethnics; an initial check showed this is very low if measured at the level of 4-digit zip codes.

We focus on two variables, namely (1) the proportion of employed people defined as receiving their major source of income from the labor market, and (2) the level of income. For both characteristics, we consider the people in the neighborhood or

municipality aged between 18-65, excluding any early retirees. People's ethnic origin is based on the country of origin as defined by Statistics Netherlands. We calculate the measures for each year on January 1 for the period 1999-2009, and match these with the refugees' first address in regular housing. If a refugee leaves a COA-administered address and moves to regular housing in e.g. June 1999, the neighborhood or municipality characteristics as of January 1, 1999 are used. Refugees without any co-ethnics in the municipality are coded at the mean of the employment share in the municipality and included in a separate dummy variable. Thus, we avoid having to drop these cases from the fairly small analytical sample. The share of co-ethnics is similarly constructed at the municipality level and measures the proportion of co-ethnics among the 18-65-year-old population as of January 1 of the year in question.

Level of income is measured as the median gross yearly income in Euros. Specifically, we draw on individuals' primary income that indicates earnings from waged employment and self-employment, thus excluding taxes, social security contributions, social transfers and other income sources. The variable is aggregated to the neighborhood or municipality level from individuals' income, excluding individuals who have no income according to this measure. Substantively, this implies that our measure captures the median yearly income for people active on the labor market. Additionally, some median income levels turn out negative for the co-ethnics, which is due to the combination of (1) very few co-ethnics who (2) report net losses. These were coded as 0.1. For some refugees, it was not possible to calculate a median neighborhood income, although some co-ethnics were employed; in this case they were coded 0.01.[57] Finally, refugees who do not have any co-ethnics living in the municipality are given the average score on this variable and identified by a separate binary variable in the analyses. We include the logged values of the income variables in our analyses. Note that because data on individual income is only available from 2003 and onwards, this variable is not available for the whole sample of refugees from 1999 to 2009. We return to this point in the analytical strategy.

Control variables

Several time-constant covariates are included. We account for the individual's position in and the configuration of the *household*. This variable is time-invariant, reflecting

57 The cases are set at a low but non-zero value to ensure taking the log of income in the neighborhood is possible. In a robustness analysis, we ran the analyses without the cases set at 0.01. This involved removing 244 and 150 individuals from the main and men only income analyses, respectively, and the substantive findings remain unchanged in this robustness analysis [results not shown].

the household type during initial housing[58]. We capture the following configurations: (1) Child in a household; (2) single (reference category); (3) living with partner without children; (4) living with partner and youngest child below 4 years of age; (5) living with partner and youngest child 4-12 years; (6) living with partner and youngest child older than 12 years; (7) single parent with youngest child below 4 years of age; (8) single parent with youngest child 4-12 years; (9) single parent with youngest child older than 12 years; (10) other household. Note that for categories 3 through 8, there may be multiple dependent children in the household. In addition, we account for the individual's *age*. Age is fixed when the person first moves to regular accommodation and measured in number of years. Age has been specified as a time-invariant characteristic to avoid conflating it with time at risk (see analytical strategy below). Finally, a set of dummy variables for the year the refugee was placed in regular housing measures the *immigration year* – using 1999 as the reference category – which takes into account differences between these years, such as divergent unemployment rates.

4.3.3 Analytical strategy

We employ multilevel linear probability discrete time event-history modelling with robust standard errors to analyze the data. Linear probability models are to be preferred over logistic and similar non-linear models when testing interaction effects, and the coefficients can readily be compared across models (Mood 2010a). Specifically, we model the conditional likelihood of transitioning from social assistance to employment. Refugees are considered 'at risk' from the month they first move to regular housing. As we do not explicitly consider subsequent moving behavior, the estimated influence of the neighborhood context should be interpreted as intent-to-treat estimates.

To test our hypotheses, we draw on two analytical samples; a *main sample* of refugees placed in regular housing from 1999 to 2009, and an *income sample* of refugees placed in regular housing from 2003 to 2009, when information on income is available in our data. We take account of the fact that some refugees are placed in the same neighborhood (4-digit postcode) – for which the employment share among natives is the same – by using multilevel modelling for each unique neighborhood and year combination. This results in 3,811 groups in the main analytical sample, with an average of 104.3 observation-months in each group. For the income sample, we observe on average 103.6

58 We make it time-invariant because later changes in the household situation may be endogenous to and contingent on changes in the income situation and thus whether the individual exits social assistance.

person-months across 2,534 groups[59]. Time is measured as months since moving to regular accommodation divided by 12 – meaning an increase of 1 equals one year at risk – and is modelled using splines. In the main sample, the cut-off points are set at 2, 4, and 7 years, meaning we allow the conditional effect of time to change three times. For the income sample, we also employ splines but use one cut-off point at 4 years[60].

As explained above, individuals are censored if they become ineligible for social assistance benefits. Additionally, we censor refugees who have not found work within 10 years after becoming at risk. We do this to ensure that we compare refugees for a relatively equal amount of time – although we can only observe the most recently settled refugees for 8 and 9 years at most, because our data run until 2017. Note that the substantive conclusions regarding the hypotheses do not change if we follow refugees for as long as the data permit. We check whether our results differ when we only include male refugees[61], which they do not (see appendix section 4.6). To ease the interpretation of the results, we mean-center all the continuous variables at their respective sample mean. All models are estimated using Stata 15.

59 As argued and shown by Allison (2014), it is not necessary to account for the fact that each individual contributes multiple observations – months in our case – as long as each individual only contributes one event (or no event at all) and the event in question is not repeatable.

60 These cut-off points roughly correspond to the 25th, 50th, and 75th percentile of the person-month distribution of the observed cases. In the income sample, the 4 year cut-off point roughly corresponds to the 55th percentile of the observed person-months in the income sample.

61 Ideally, we would also run separate models for women. However, because the number of observed transitions for women is very low, especially for the income sample (180 observed events), this turned out to be not statistically feasible. Male refugees, in contrast, are more likely to enter the labor market – see table 4.1.

Table 4.1. Descriptive statistics of time at risk and hazard rate for the main and income sample.

	Main sample	Income sample	Main sample (men only)	Income sample (men only)
Nr events	1,930	1,190	1,611	1,010
% of individuals	35.20	33.03	49.72	46.31
Mean time at risk (in months)	72.52	72.83	63.91	64.23
Hazard rate (mean)	.0049	.0045	.0078	.0072
N(person-months)	397,614	262,410	207,080	139,759
N(individuals)	5,483	3,603	3,240	2,176

Notes: Mean time at risk reflects the average across individuals in which first month at risk is coded 1, whereas time at risk in table 4.2 and in the rest of the analyses is at the level of person-months and first month at risk is coded 0.

The descriptive statistics for the main and income samples are shown in table 4.1 and 4.2. Table 4.1 display some general information about the samples, including how many experiences the event or transition from social assistance to work and the mean time in months at risk for each individual. The main and income samples are quite similar in terms of percentage of individuals transitioning (35.2 vs 33.0 percent), mean months at risk (72.5 vs 72.8), and the overall hazard rate (.0049 vs .0045). Table 4.2 shows the overall descriptive statistics for the variables included in the analyses at the level of person-months. In general, the income sample is very similar to the main sample, except with respect to ethnic origin, which reflects historical changes in refugee migration flows.

Table 4.2. Descriptive statistics for main and income sample.

Variable	Main sample		Income sample	
	Mean/Prop. (SD)	[P5, P95]a	Mean/Prop. (SD)	[P5, P95]a
Enters labor market	.005		.005	
Months at risk/12	3.881	[.25, 8.833]	3.879	[.25, 8.750]
	(2.712)		(2.720)	
Prop employed among co-ethnics (municipality)	.283	[0, .536]	.294	[.095, .538]
	(.158)		(.152)	
Prop employed among natives (neighborhood)	.714	[.597, .797]	.722	[.597, .802]
	(.064)		(.064)	[.01, 33,866]
Median yearly income among co-ethnics (municipality)			22,357.49	
			(10,524.56)	[28,820, 42,193]
Median yearly income among natives (neighborhood)			35,194.76	
			(4,159.23)	
No co-ethnics in municipality (ref. >0 co-ethnics)	.017		.018	
Share of co-ethnics (municipality)	.003	[.000, .007]	.003	[.000, .007]
	(.003)		(.003)	
Female (ref. male)	.479		.467	
Age/10	3.746	[2.6, 5.2]	3.737	[2.5, 5.2]
	(.828)		(.847)	
Household-position (categorical)				
Child in household	.011		.010	
Single (ref.)	.331		.401	
Partner without children	.078		.081	
Partner with youngest child <4	.155		.118	
Partner with youngest child 4-12	.199		.152	
Partner with youngest child 12<	.077		.079	
Single parent with youngest child <4	.039		.042	

Single parent with youngest child 4-12	.067	.069
Single parent with youngest child 12<	.034	.035
Other	.009	.012
Ethnic origin (categorical)		
Iraq (ref.)	.308	.384
Afghanistan	.230	.099
Former Yugoslavia	.073	.047
Former Soviet-Union	.067	.078
Somalia	.206	.286
Angola	.009	.008
Iran	.039	.041
Sierra Leone	.019	.015
(former) Sudan	.026	.016
China	.003	.004
Syria	.020	.020
Turkey	.002	.002
Year of placement (categorical)		
1999 (ref. in main sample)	.057	
2000	.054	
2001	.106	
2002	.120	
2003 (ref. in income sample)	.083	.125
2004	.048	.072
2005	.067	.102
2006	.059	.089
2007	.095	.144
2008	.136	.205
2009	.175	.262
N(person-months)	397,614	262,410
N(individuals)	5,483	3,603
N(neighborhood*year)	3,811	2,534

Notes: [a]= For privacy reasons, Statistics Netherlands does not allow the release of minimum and maximum values, particularly for income; instead, we show the 5th (P5) and 95th (P95) percentiles for the continuous variables. Descriptive statistics calculated at the level of person-months. Because the income data are only available from 2003 and onwards, the income sample covers the period 2003-2009 only. Proportions are depicted for categorical variables and may due to rounding not add up to exactly 1.

4.4 Results

4.4.1 Findings main analyses

We report the results of the analyses for the main sample in table 4.3 and the income sample in table 4.4 (see appendix section 4.6 for full tables). In both tables, the coefficients represent the estimated effect on the probability of transitioning from social benefit receipt to employment in a given month, conditional upon not (yet) having made this transition, for a one-unit change in the variable. Model 1 (Table 4.3) shows the likelihood to find work as a function of time. The likelihood increases in the two first years at risk, remains unchanged between 2-4 years, and then decreases when not (yet) having found work after 4 years at risk. Without controls (model 2), there is a positive and significant effect of the proportion of employed co-ethnics in the municipality (b= .0017; p<.05, two-sided), in line with hypothesis 1a. This effect remains significant and increases somewhat when we take into account the year of moving into regular housing, household position, ethnic background, gender, and age in model 3 (b= .0021; p<.05, two-sided). Specifically, this effect suggests that a refugee placed in a municipality in which the share of employed co-ethnics is high (95th percentile) compared to a municipality that scores low (5th percentile), has a .0011 higher conditional likelihood of transitioning to employment in a given month, keeping all other variables constant. This amounts to a relative increase in the likelihood of 12.5 percent between the high and low municipality[62]. In sum, the results support hypothesis 1a and suggest that refugees placed in an area with a higher share of employed co-ethnics have a higher likelihood of transitioning into employment.

Model 2 shows a negative relationship between the share of employed natives and the conditional likelihood of transitioning to employment (b= -.0055; p<.05, two-sided). However, when control variables are added (model 3), the direction turns, now indicating that the higher the share of employed natives the higher the conditional likelihood of

62 These predictions are derived by first calculating the ŷ95th = intercept+b*(95th percentile-mean); second calculating the ŷ5th = intercept+b*(5th percentile-mean); third, taking ŷ95th- ŷ5th to obtain the absolute difference and (ŷ95th/ŷ5th)-1*100 to obtain the relative difference in percent. See table 4.2 for descriptive statistics.

transitioning into employment (b= .0066; p<.005, two-sided)[63]. The estimate suggests that a refugee placed in a neighborhood where the proportion of employed natives is high (95th percentile) relative to a neighborhood where it is low (5th percentile), has a .0013 higher conditional likelihood of transitioning into employment, which amounts to a relative increase of roughly 15 percent. This is in line with hypothesis 2a.

Table 4.3. Results from analyses on main sample. Coefficients represent variables' effect on the conditional likelihood of transitioning from social assistance to work estimated using multilevel linear-probability, discrete-time event-history modelling with robust standard errors.

	Model 1	Model 2	Model 3	Model 4
Intercept	.0030	.0030	.0096	.0097
	(9.38)**	(9.34)**	(11.40)**	(11.41)**
Months at risk/12	.0019	.0019	.0021	.0021
(<2 years)	(7.28)**	(7.32)**	(8.15)**	(8.15)**
Months at risk/12	.0003	.0003	.0006	.0006
(2-4 years)	(1.30)	(1.33)	(2.36)*	(2.36)*
Months at risk/12	-.0008	-.0008	-.0006	-.0006
(4-7 years)	(-5.07)**	(-5.06)**	(-3.59)**	(-3.59)**
Months at risk/12	-.0005	-.0005	-.0005	-.0005
(≥7 years)	(-2.40)*	(-2.44)*	(-2.31)*	(-2.31)*
Main explanatory variables				
Prop employed among co-ethnics (munic.)[a]		.0017	.0021	.0021
		(1.97)*	(2.17)*	(2.17)*
Prop employed among natives (neighb.)[a]		-.0055	.0066	.0071
		(-2.50)*	(1.97)**	(3.09)**

63 To ascertain why the estimated effect shifts from negative to positive, we tried adding the control variables to the model in various ways: (1) One by one; (2) removing only one; and (3) including all different combinations [not shown]. It turns out that the negative relationship in part is due to variation over time, and in part from the combination of year and ethnic-origin groups or household-position. This pattern would be consistent with historical migration flows that overlap with fluctuations in the general economy.

Table 4.3 continued

No co-ethnics in munic (ref. >0 co-ethnics)		.0006	-.0005	-.0005
		(0.63)	(-.51)	(-.50)
Share of co-ethnics (municipality)[a]				-.0050
				(-.10)
*Prop employed among natives[a]*Share of co-ethnics[a]*				-.9969
				(-1.43)
Control variables				
Female (ref. male)			-.0063	-.0063
			(-24.64)**	(-24.69)**
Age/10[a]			-.0034	-.0034
			(-19.09)**	(-19.15)**
Household-position dummies	NO	NO	YES	YES
Ethnic-origin dummies	NO	NO	YES	YES
Year dummies	NO	NO	YES	YES
Var(group-level)	.0000	.0000	.0000	.0000
Var(residual)	.0048	.0048	.0048	.0048
Log pseudolikelihood	496,109.19	469,114.33	496,853.06	496,854.13
BIC	-992,218	-992,100	-993,165	-993,141
N(person-months)	397,614			
N(neighborhood*year)	3,811			

Notes: [a]= Mean-centered at mean of main sample. *= p<.05; **= p<.005 (two-sided). Z-statistic between parentheses. N(individuals)= 5,483. See appendix section 4.6 table A4.1 for the effects of all categorical variables.

We expected that the effect of the share of employed natives would depend on the share of co-ethnics in the municipality. Model 4 indicates that this is not the case (b= -.9969; p>.05, two-sided). The main effect of the share of co-ethnics in the municipality

is not significant either. An additional analysis revealed that this also applies when the interaction term is not included in the model (see model 5 in table A4.1). Moreover, the indicator for no co-ethnics in the municipality is insignificant in all models, further evidence of the negligible effect of ethnic composition on transitioning to work. Hence, the results provide no support for hypothesis 3a. The effect of the share of employed natives does not seem to depend on the share of co-ethnics in the population.

4.4.2 Additional analyses on the income sample

The outcomes of the analyses on the income sample are shown in table 4.4. We estimate similar models to those for the main sample. It is worth noting that we include the employment share variables together with the income variables, to assess whether the above conclusions hold if we also include the median income among the same groups.

First, we expected that a higher level of income among co-ethnics would make refugees more likely to transition from social assistance to work. Model 2 in table 4.4 suggests that this is not the case: The median income among co-ethnics does not affect the conditional likelihood of transitioning from social assistance to work (b= -.0000; p>.05, two-sided). This is also true when control variables are added in model 3 (b= -.0000; p>.05, two-sided), and implies that refugees do not fare worse nor better because of the median level of income among co-ethnics in the municipality they are placed in. Thus, the results are not in line with our theoretical expectation (H1b).

Second, there is no significant effect of the median income level among natives on a refugee's conditional likelihood of transitioning into work in model 2 in table 4.4 (b= -.0030; p>.05, two-sided). When we also take into account possible cofounding variables in model 3, the effect increases somewhat and is now significant (b= -.0041, p<.05, two-sided). The effect in model 3 suggests that a refugee placed in a neighborhood where the median income among natives is high (95th percentile), has, on average, a .0016 lower conditional likelihood of transitioning into work per month, compared to a refugee placed in a neighborhood where the median income among natives is low (5th percentile). This amounts to a relative decrease in the conditional likelihood of 18.2 percent. Counter to our hypothesis (H2b), the results suggest that refugees placed in a neighborhood in which natives have a higher median income, have a *lower* conditional likelihood of transitioning into work.

Next, we test whether the effect of median income among natives (model 4), and of the proportion employed among natives (model 5) depend on the share of co-ethnics in the municipality. Regarding the influence of median income among natives, model 4 in table 4.4 shows that this effect does not vary by the share of co-ethnics in the municipality (b= -.1649; p>.05, two-sided). The results in model 5 corroborate the

findings from the main sample (model 4, table 4.3) – the positive effect of the share of employed among natives on the conditional probability of transitioning into work does not depend on the share of co-ethnics in the municipality (b= -1.6480; p>.05, two-sided). In fact, there does not seem to be any influence of the share of co-ethnics on refugees' likelihood of transitioning out of social assistance, as indicated by the insignificant main effects in model 4 (b= .0298; p>.05, two-sided), and in model 5 (b= .0093; p>.05, two-sided). This is also true when we include the share of co-ethnics as a main effect without any interactions (see model 6 in table A4.2). In other words, the median income among natives has an equal impact on a refugee's likelihood of transitioning into employment, irrespective of the share of co-ethnics in his/her environment. This runs counter to hypothesis 3b.

Table 4.4. Results from analyses on the income sample. Coefficients represent variables' effect on the conditional likelihood of transitioning from social assistance to work estimated using multilevel linear-probability, discrete-time event-history modelling with robust standard errors.

	Model 1	Model 2	Model 3	Model 4	Model 5
Intercept	.0040	.0040	.0078	.0078	.0078
	(13.35)**	(13.14)*	(12.12)**	(12.07)**	(11.97)**
Months at risk/12 (<4 year)	.0008	.0008	.0011	.0011	.0011
	(6.90)**	(6.92)**	(8.86)**	(8.85)**	(8.88)**
Months at risk/12 (≥4 years)	-.0009	-.0009	-.0008	-.0008	-.0008
	(-9.85)**	(-9.88)**	(-8.39)**	(-8.40)**	(-8.39)**
Main explanatory variables					
Prop employed among co-ethnics (municipality)[a]		.0009	.0015	.0015	.0013
		(.65)	(.95)	(.96)	(.85)
Prop employed among natives (neighborhood)[a]		.0011	.0101	.0104	.0109
		(.37)	(3.25)**	(3.34)**	(3.46)**
Median income among co-ethnics (municipality)[b]		-.0000	-.0000	-.0000	-.0000
		(-.05)	(-.06)	(-.12)	(.04)
Median income among natives (neighborhood)[b]		-.0030	-.0041	-.0040	-.0037
		(-1.77)	(-2.10)*	(-2.06)*	(-1.92)
No co-ethnics in munic. (ref. >0 co-ethnics)		.0012	.0000	.0000	.0000
		(1.03)	(.00)	(.04)	(.03)
Share of co-ethnics (municipality)[a]				.0298	.0093
				(.56)	(.16)
Median income among natives (neighb.)[b]*Share of co-ethnics[a]				-.1649	
				(-.54)	
*Prop employed among natives[a]*Share of co-ethnics[a]*					-1.6480
					(-1.81)
Control variables					
Female (ref. male)			-.0059	-.0059	-.0059
			(-18.63)**	(-18.67)**	(-18.70)**

Table 4.4 continued

Age/10[a]			-.0032 (-14.84)**	-.0032 (-14.87)**	-.0032 (-14.91)**
Household-position dummies	NO	NO	YES	YES	YES
Ethnic-origin dummies	NO	NO	YES	YES	YES
Year dummies	NO	NO	YES	YES	YES
Var(group-level)	.0000	.0000	.0000	.0000	.0000
Var(residual)	.0045	.0045	.0045	.0045	.0045
Log pseudolikelihood	336,629.77	336,299.37	336,719.62	336,719.84	336,721.65
BIC	-672,531	-672,474	-672,965	-672,941	-672,944
N(person-months)	262,410				
N(neighborhood*year)	2,534				

Notes: [a]= Mean-centered at mean of income sample; [b]= natural logarithm of variable included, and median income pertains to median yearly income from (self-) employment. *= p<.05; **= p<.005 (two-sided). Z-statistic between parentheses. N(individuals)= 3,603. See appendix section 4.6 table A4.2 for the effects of all categorical variables.

Finally, we briefly comment on the effects of employment share among co-ethnics and among natives, which are net of the level of income in the area in table 4.4. In general, the estimated effects of employment share among co-ethnics in the municipality decrease slightly compared to the estimates in table 4.3 and are now insignificant. Given the lower N – fewer people who enter the labor market in absolute terms – this could be related to the lower statistical power in the income analyses. This explanation is backed up by the fact that the effect estimates are comparable across the two samples, and across model 2 through 5 in table 4.4. Regarding the employment share among natives in the neighborhood, the estimated effects in table 4.3 are somewhat larger compared to the estimates in table 4.4. Considering the negative effect of median income among natives, this may suggest that the effect found on the main sample in table 4.3 is suppressed – in the sense that the employment share among natives may have been biased downward because it partially captures the negative effect that median income among natives has on the conditional transition probabilities. In sum, we observe that the point estimates for the employment share among co-ethnics and among natives are in

the same direction as in table 4.3, and thus in line with their hypothesized effect (H1a and H2b). However, results in table 4.4 indicate that the employment share among co-ethnics is slightly less relevant once taking into account the median income among co-ethnics and among natives, although this could also be an issue of statistical power.

4.5 Discussion and Conclusions

In many European countries, the labor market integration of refugees is a key element of the policy debate. This chapter aimed to shed light on the importance of the neighborhood context in which refugees are placed for their future employment opportunities. Specifically, we have drawn on a natural quasi-experiment in the Dutch housing policy for refugees – asylum migrants who were granted asylum – regarding their first regular accommodation. This allowed us to obtain intent-to-treat estimates of the contextual effects that are not biased by self-selection. We focused on the employment share and median income in the neighborhood among co-ethnics and natives, respectively, and expected that in areas where these are high, refugees would have a higher likelihood of transitioning from welfare to work. Additionally, we presumed that the influence of the employment share and median income among natives would depend on the concentration of co-ethnics in the area. These arguments were tested using Dutch longitudinal administrative data and multilevel linear probability, discrete time event-history modeling.

First, the findings suggest that refugees placed in areas in which their co-ethnics or natives are employed more often transition into work. This is in line with our expectations (H1a and H2a) and corroborates findings from previous research, which indicates that a higher employment share among neighbors leads to more favorable labor market outcomes (e.g. Damm 2014). This points to the role of employed others as an important social resource for facilitating refugees' first step on the labor market. Contrary to our expectations, however, we do not find any evidence that the importance of the employment share among natives changes depending on the share of co-ethnics in the area (H3a). One possible explanation could be that refugees seek out co-ethnics outside the municipality and are willing to travel farther to do so. A further explanation could question whether these effects should be interpreted as social resources – in the sense that refugees can obtain help or assistance from people in these contexts. That is, the effects we find may also partially depend on the situation in the local or

regional labor market in which the refugees are placed. More detailed data are needed to examine these mechanisms more precisely.

Second, there is no evidence that refugees placed in an area with a higher level of income among co-ethnics or natives are better able to enter the labor market, which runs counter to our initial hypotheses (H1b and H2b). In fact, there is some indication that refugees are on average *less* likely to enter the labor market when placed in an area where the level of income among natives is higher. This is rather puzzling, as we would expect higher income levels to indicate better social resources available to refugees. Moreover, it runs counter to the outcomes of previous studies that generally find that higher income levels in the area of residence tends to advance refugees' labor market outcomes (Damm 2009; 2014; Edin, Fredriksson, and Åslund 2003). One possible explanation for this unexpected finding is that higher-income contacts may not be the most relevant ones for refugees. Labor market disadvantages related to language proficiency and recognition of foreign educational credentials (e.g. de Vroome and van Tubergen 2010), especially, impede refugees' access to the higher end of the labor market, and higher-income contacts are not useful in mitigating those (Lin 1999). However, most refugees initially have to turn to lower-status jobs, and for those type of jobs, high-income neighbors may not be much of an asset. In other words, in higher-income neighborhoods there could be a mismatch between the information that is accessible to refugees, and its usefulness for their labor market entry. Assuming that the neighborhood's level of income indicates the match of refugees with the regional labor market, it may also be that fewer relevant jobs are available as the income level rises.

A possible limitation of the present study is its focus on refugees of prime-working age (25 to 55 years old). Future research could expand on this, by investigating the role of the social context for younger refugees (although in the Netherlands this group tends to be subject to different policies regarding placement in regular housing). One might expect the initial social context to be more important for younger refugees, due to their limited previous work experience.

Furthermore, we have investigated refugees who in principle have no say in the location of their first regular housing. We argue that unobserved heterogeneity stemming from self-selection into these initial contexts cannot bias our estimated contextual effects. The estimates of the neighborhood characteristics can therefore be interpreted as the average influence of being exposed to these social contexts, irrespective of subsequent residential mobility. Because it takes time to establish social contacts, it is reasonable to assume that refugees who remain in (the vicinity of) their first regular housing will be more affected by this context. However, a potential problem related to assessing the strength of the area characteristics by time of exposure – i.e. length of

stay in initial housing - to the context, is that length of stay and subsequent residential moving behavior may be subject to self-selection: Refugees' perceived chances on the labor market could affect their choice of moving. Hence, if one were to include length of stay in the analyses, one would also introduce selection bias. Therefore, the effects we find of the initial, exogenous placement should be interpreted as 'intent-to-treat' estimates as actual 'treatment' would depend on the length of residence. Our approach is nevertheless warranted because initial placement is malleable by policy whereas later residential behavior is not.

Our results point to a somewhat mixed picture. The employment share among natives, and among co-ethnics is important in facilitating refugees' transition from welfare to work, whereas the median level of income in the area is less important and runs counter to our initial hypotheses. However, the findings of this chapter align with recent changes in the policies concerning the initial placement of refugees to the Netherlands (Gerritsen, Kattenberg, and Vermeulen 2018). These aim to match refugees to a specific region or municipality based on their predicted compatibility and opportunities on the labor market.

4.6 Appendix

Table A4.1. Results from analyses on main sample. Coefficients represent variables' effect on the conditional likelihood of transitioning from social assistance to work estimated using multilevel linear probability, discrete time event-history modelling with robust standard errors.

	Model 1	Model 2	Model 3	Model 4	Model 5
Intercept	.0030	.0030	.0096	.0097	.0097
	(9.38)**	(9.34)**	(11.40)**	(11.41)**	(11.39)**
Months at risk/12 (<2 years)	.0019	.0019	.0021	.0021	.0021
	(7.28)**	(7.32)**	(8.15)**	(8.15)**	(8.15)**
Months at risk/12 (2-4 years)	.0003	.0003	.0006	.0006	.0006
	(1.30)	(1.33)	(2.36)*	(2.36)*	(2.36)*
Months at risk/12 (4-7 years)	-.0008	-.0008	-.0006	-.0006	-.0006
	(-5.07)**	(-5.06)**	(-3.59)**	(-3.59)**	(-3.59)**
Months at risk/12 (≥7 years)	-.0005	-.0005	-.0005	-.0005	-.0005
	(-2.40)*	(-2.44)*	(-2.31)*	(-2.31)*	(-2.31)*
Main explanatory variables					
Prop employed among co-ethnics (munic.)[a]		.0017	.0021	.0021	.0021
		(1.97)*	(2.17)*	(2.17)*	(2.17)*
Prop employed among natives (neighb.)[a]		-.0055	.0066	.0071	.0067
		(-2.50)*	(2.97)**	(3.09)**	(2.96)**
No co-ethnics in munic (ref. >0 co-ethnics)		.0006	-.0005	-.0005	-.0005
		(.63)	(-.51)	(-.50)	(-.50)
Share of co-ethnics (municipality)[a]				-.0050	.0055
				(-.10)	(.12)
*Prop employed among natives[a]*Share of co-ethnics[a]*				-.9969	
				(-1.43)	
Control variables					
Female (ref. male)			-.0063	-.0063	-.0063
			(-24.64)**	(-24.69)**	(-24.65)**
Age/10[a]			-.0034	-.0034	-.0034
			(-19.09)**	(-19.15)**	(-19.10)**
Household-position (ref. Single)					
Child in household			-.0004	-.0004	-.0004
			(-.27)	(-.31)	(-.28)

Partner without children	-.0005	-.0005	-.0005
	(-.90)	(-.94)	(-.90)
Partner with youngest child <4	-.0025	-.0025	-.0025
	(-5.70)**	(-5.71)**	(-5.69)**
Partner with youngest child 4-12	-.0015	-.0015	-.0015
	(-3.71)**	(-3.69)**	(-3.70)**
Partner with youngest child 12<	-.0015	-.0015	-.0015
	(-3.14)**	(-3.16)**	(-3.13)**
Single parent with youngest child <4	-.0029	-.0029	-.0029
	(-5.41)**	(-5.43)**	(-5.41)**
Single parent with youngest child 4-12	-.0016	-.0016	-.0016
	(-3.61)**	(-3.61)**	(-3.60)**
Single parent with youngest child 12<	-.0002	-.0002	-.0002
	(-.37)	(-.30)	(-.37)
Other	.0013	.0013	.0013
	(.82)	(.83)	(.82)
Ethnic-origin groups (ref. Iraq)			
Afghanistan	.0007	.0007	.0007
	(1.20)	(1.25)	(1.20)
Former Yugoslavia	-.0021	-.0020	-.0020
	(-5.20)**	(-5.02)**	(-5.03)**
Former Soviet-Union	-.0008	-.0008	-.0008
	(-2.07)*	(-1.91)	(-2.03)*
Somalia	.0007	.0006	.0007
	(1.15)	(1.05)	(1.12)
Angola	.0013	.0014	.0013
	(1.61)	(1.64)	(1.61)
Iran	.0018	.0018	.0018
	(1.75)	(1.78)	(1.75)
Sierra Leone	.0061	.0062	.0061
	(3.97)**	(4.01)**	(3.96)**
(former) Sudan	.0052	.0053	.0052
	(3.09)**	(3.11)**	(3.09)**
China	.0015	.0014	.0015
	(1.50)	(1.48)	(1.51)

Table A4.1 continued

Syria			.0039	.0038	.0039
			(1.22)	(1.19)	(1.22)
Turkey			-.0014	-.0008	-.0015
			(-.68)	(-.36)	(-.70)
Year of placement (ref. 1999)					
2000			-.0023	-.0023	-.0023
			(-2.77)*	(-2.80)*	(-2.77)*
2001			-.0023	-.0024	-.0023
			(-3.02)**	(-3.07)**	(-3.02)**
2002			-.0024	-.0024	-.0024
			(-3.09)**	(-3.16)**	(-3.09)**
2003			-.0026	-.0026	-.0026
			(-3.20)**	(-3.23)**	(-3.19)**
2004			-.0019	-.0020	-.0019
			(-2.10)*	(-2.16)*	(-2.10)*
2005			-.0009	-.0010	-.0010
			(-1.05)	(-1.09)	(-1.06)
2006			-.0028	-.0028	-.0028
			(-3.00)**	(-3.03)**	(-3.00)**
2007			-.0037	-.0037	-.0037
			(-4.39)**	(-4.43)**	(-4.38)**
2008			-.0044	-.0044	-.0044
			(-5.47)**	(-5.51)**	(-5.45)**
2009			-.0055	-.0056	-.0056
			(-6.95)**	(-6.96)**	(-6.91)**
Var(group-level)	.0000	.0000	.0000	.0000	.0000
Var(residual)	.0048	.0048	.0048	.0048	.0048
Log pseudolikelihood	496,109.19	469,114.33	496,853.06	496,854.13	469,853.07
BIC	-992,218	-992,100	-993,165	-993,141	-993,152
N(person-months)	397,614				
N(neighborhood*year)	3,811				

Notes: [a]= Mean-centered at mean of main sample.*= p<.05; **= p<.005, (two-sided). Z-statistic between parentheses. N(individuals)= 5,483. Shows the effects of all categorical variables for models in table 4.3.

Table A4.2. Results from analyses on income sample. Coefficients represent variables' effect on the conditional likelihood of transitioning from social assistance to work estimated using multilevel linear probability, discrete time event-history modelling with robust standard errors.

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Intercept	.0040	.0040	.0078	.0078	.0078	.0078
	(13.35)**	(13.14)**	(12.12)**	(12.07)**	(11.97)**	(12.08)**
Months at risk/12 (<4 year)	.0008	.0008	.0011	.0011	.0011	.0011
	(6.90)**	(6.92)**	(8.86)**	(8.85)**	(8.88)**	(8.86)**
Months at risk/12 (≥4 years)	-.0009	-.0009	-.0008	-.0008	-.0008	-.0008
	(-9.85)**	(-9.88)**	(-8.39)**	(-8.40)**	(-8.39)**	(-8.40)**
Main explanatory variables						
Prop employed among co-ethnics (municipality)[a]		.0009	.0015	.0015	.0013	.0015
		(.65)	(.95)	(.96)	(.85)	(.97)
Prop employed among natives (neighborhood)[a]		.0011	.0101	.0104	.0109	.0103
		(.37)	(3.25)**	(3.34)**	(3.46)**	(3.32)**
Median income among co-ethnics (municipality)[b]		-.0000	-.0000	-.0000	.0000	-.0000
		(-.05)	(-.06)	(-.12)	(.04)	(-.11)
Median income among natives (neighborhood)[b]		-.0030	-.0041	-.0040	-.0037	-.0041
		(-1.77)	(-2.10)*	(-2.06)*	(-1.92)	(-2.12)*
No co-ethnics in munic (ref. >0 co-ethnics)		.0012	.0000	.0000	.0000	.0000
		(1.03)	(.00)	(.04)	(.03)	(.04)
Share of co-ethnics (municipality)[a]				.0298	.0093	.0303
				(.56)	(.16)	(.58)
*Prop employed among natives[a]*Share of co-ethnics[a]*				-.1649		
				(-.54)		
*Median income among natives (neighb.)[b]*Share of co-ethnics*					-1.6480	
					(-1.81)	
Control variables						
Female (ref. male)			-.0059	-.0059	-.0059	-.0059
			(-18.63)**	(-18.67)**	(-18.70)**	(-18.67)**
Age/10[a]			-.0032	-.0032	-.0032	-.0032
			(-14.84)**	(-14.87)**	(-14.91)**	(-14.86)**

Table A4.2 continued

Household-position (ref. Single)				
Child in household	.0004	.0004	.0004	.0004
	(.26)	(.23)	(.21)	(.24)
Partner without children	-.0005	-.0005	-.0006	-.0005
	(-.82)	(-.83)	(-.85)	(-.82)
Partner with youngest child <4	-.0021	-.0021	-.0021	-.0021
	(-3.80)**	(-3.78)**	(-3.76)**	(-3.78)**
Partner with youngest child 4-12	-.0008	-.0008	-.0007	-.0008
	(-1.41)	(-1.41)	(-1.38)	(-1.41)
Partner with youngest child 12<	-.0014	-.0014	-.0014	-.0014
	(-2.45)*	(-2.45)*	(-2.45)*	(-2.44)*
Single parent with youngest child <4	-.0030	-.0030	-.0030	-.0030
	(-5.26)**	(-5.24)**	(-5.28)**	(-5.24)**
Single parent with youngest child 4-12	-.0018	-.0018	-.0018	-.0018
	(-3.52)**	(-3.50)**	(-3.53)**	(-3.51)**
Single parent with youngest child 12<	-.0003	-.0003	-.0003	-.0003
	(-.55)	(-.52)	(-.42)	(-.54)
Other	.0010	.0010	.0010	.0009
	(.55)	(.55)	(.58)	(.54)
Ethnic-origin groups (ref. Iraq)				
Afghanistan	.0005	.0005	.0006	.0005
	(.80)	(.83)	(.90)	(.82)
Former Yugoslavia	-.0020	-.0020	-.0019	-.0020
	(-4.61)**	(-4.33)**	(-4.27)**	(-4.31)**
Former Soviet-Union	.0002	.0002	.0003	.0002
	(.28)	(.34)	(.45)	(.32)
Somalia	-.0001	-.0002	-.0002	-.0001
	(-.10)	(-.20)	(-.19)	(-.17)
Angola	.0003	.0003	.0003	.0003
	(.27)	(.33)	(.36)	(.31)
Iran	.0021	.0022	.0022	.0022
	(1.53)	(1.58)	(1.62)	(1.57)
Sierra Leone	.0036	.0037	.0038	.0037
	(1.80)	(1.84)	(1.91)	(1.84)

(former) Sudan			.0042	.0043	.0044	.0043
			(2.14)*	(2.18)*	(2.23)*	(2.17)*
China			.0010	.0011	.0010	.0011
			(.82)	(.86)	(.81)	(.86)
Syria			.0043	.0042	.0041	.0042
			(1.31)	(1.30)	(1.25)	(1.29)
Turkey			-.0033	-.0038	-.0019	-.0039
			(-1.25)	(-1.47)	(-.61)	(-1.54)
Year of placement (ref. 2003)						
2004			.0009	.0009	.0008	.0009
			(1.13)	(1.14)	(1.07)	(1.14)
2005			.0018	.0018	.0018	.0018
			(2.49)*	(2.51)*	(2.49)*	(2.51)*
2006			.0002	.0002	.0002	.0002
			(.29)	(.32)	(.31)	(.31)
2007			-.0004	-.0004	-.0005	-.0004
			(-.68)	(-.68)	(-.73)	(-.67)
2008			-.0012	-.0012	-.0013	-.0012
			(-1.95)	(-1.96)*	(-2.03)*	(-1.95)
2009			-.0023	-.0023	-.0024	-.0023
			(-3.76)**	(-3.77)**	(-3.86)**	(-3.77)**
Var(group-level)	.0000	.0000	.0000	.0000	.0000	.0000
Var(residual)	.0045	.0045	.0045	.0045	.0045	.0045
Log pseudolikelihood	336,629.77	336,299.37	336,719.62	336,719.84	336,721.65	336,719.74
BIC	-672,531	-672,474	-672,965	-672,941	-672,944	-672,953
N(person-months)	262,410					
N(neighborhood*year)	2,534					

Notes: [a]= Mean-centered at mean of income sample; [b]= mean-centered at logged values of variable. *= p<.05; **= p<.005, two-sided. Z-statistic between parentheses. N(individuals)= 3,603. Shows the effects of all categorical variables for models in table 4.4.

Table A4.3. Results from additional analyses on main sample, including men only. Coefficients represent variables' effect on the conditional likelihood of transitioning from social assistance to work estimated using multilevel linear probability, discrete time event-history modelling with robust standard errors.

	Model 1	Model 2	Model 3	Model 4
Intercept	.0045	.0045	.0100	.0100
	(8.70)**	(8.67)**	(7.61)**	(7.58)**
Months at risk/12	.0032	.0032	.0034	.0034
(<2 years)	(7.48)**	(7.50)**	(7.92)**	(7.92)**
Months at risk/12	.0005	.0005	.0006	.0006
(2-4 years)	(1.12)	(1.14)	(1.56)	(1.56)
Months at risk/12	-.0009	-.0009	-.0007	-.0007
(4-7 years)	(-3.05)**	(-3.05)**	(-2.28)*	(-2.27)*
Months at risk/12	-.0010	-.0010	-.0011	-.0011
(≥7 years)	(-2.62)*	(-2.62)*	(-2.81)**	(-2.81)**
Main explanatory variables				
Prop employed among co-ethnics (munic.)[a]		.0019	.0038	.0039
		(1.18)	(2.19)*	(2.21)*
Prop employed among natives (neighb.)[a]		-.0003	.0149	.0150
		(-.09)	(4.10)**	(3.94)**
No co-ethnics in munic (ref. >0 co-ethnics)		.0001	-.0002	-.0004
		(.04)	(-.16)	(-.24)
Share of co-ethnics (municipality)[a]				-.1041
				(-1.34)
*Prop employed among natives[a] *Share of co-ethnics[a]*				-1.3770
				(-1.34)
Control variables				
Age/10[a]			-.0054	-.0054
			(-18.10)**	(-18.11)**
Household-position dummies	NO	NO	YES	YES

Ethnic-origin dummies	NO	NO	YES	YES
Year dummies	NO	NO	YES	YES
Var(group-level)	.0000	.0000	.0000	.0000
Var(residual)	.0077	.0077	.0077	.0077
Log pseudolikelihood	209,861.15	209,861.92	210,149.04	210,150.43
BIC	-419,637	-419,601	-419,796	-419,775
N(person-months)	207,080			
N(neighborhood*year)	2,678			

Notes: [a]= Mean-centered at mean of men-only sample. *= p<.05; **= p<.005, two-sided. Z-statistic between parentheses. N(individuals)= 3,240.

Table A4.4. Results from additional analyses on income sample, including men only. Coefficients represent variables' effect on the conditional likelihood of transitioning from social assistance to work estimated using multilevel linear probability, discrete time event-history modelling with robust standard errors.

	Model 1	Model 2	Model 3	Model 4	Model 5
Intercept	.0059	.0059	.0066	.0067	.0066
	(12.01)**	(11.82)**	(6.16)**	(6.16)**	(6.11)**
Months at risk/12	.0016	.0016	.0017	.0017	.0017
(<4 year)	(7.48)**	(7.46)**	(8.25)**	(8.28)**	(8.27)**
Months at risk/12	-.0013	-.0013	-.0012	-.0012	-.0012
(≥4 years)	(-7.74)**	(-7.80)**	(-7.17)**	(-7.16)**	(-7.17)**
Main explanatory variables					
Prop employed among co-ethnics (municipality)[a]		-.0004	.0046	.0045	.0044
		(-.15)	(1.67)	(1.63)	(1.59)
Prop employed among natives (neighborhood)[a]		.0148	.0220	.0218	.0223
		(3.05)**	(4.57)**	(4.46)**	(4.50)**
Median income among co-ethnics (municipality)[b]		.0001	-.0000	-.0000	-.0000
		(1.06)	(-.19)	(-.13)	(-.05)
Median income among natives (neighborhood)[b]		-.0097	-.0084	-.0083	-.0082
		(-3.57)**	(-2.85)**	(-2.75)*	(-2.75)*
No co-ethnics in munic (ref. >0 co-ethnics)		.0006	-.0006	-.0007	-.0007
		(.33)	(-.36)	(-.42)	(-.40)
Share of co-ethnics (municipality)[a]				-.0715	-.0718
				(-.88)	(-.83)
Median income among natives (neighb.)[b]*Share of co-ethnicsa				-.3903	
				(-.68)	
*Prop employed among nativesa*Share of co-ethnicsa*					-1.5289
					(-1.22)

Control variables					
Age/10a			-.0050 (-13.74)**	-.0050 (-13.73)**	-.0050 (-13.73)**
Household-position dummies	NO	NO	YES	YES	YES
Ethnic-origin dummies	NO	NO	YES	YES	YES
Year dummies	NO	NO	YES	YES	YES
Var(group-level)	.0000	.0000	.0000	.0000	.0000
Var(residual)	.0071	.0071	.0071	.0071	.0071
Log pseudolikelihood	146,749.40	146,757.02	146,908.75	146,909.10	146,909.55
BIC	-293,440	-293,396	-293,379	-293,356	-293,357
N(person-months)	139,759				
N(neighborhood*year)	1,777				

Notes: [a]= Mean-centered at mean of men-only income sample; [b]= mean-centered at logged values of variable. *= p<.05; **= p<.005 (two-sided). Z-statistic between parentheses. N(individuals)= 2,176.

Chapter 5.

The Income Development of Working Refugees in the Netherlands: Does Initial Context Matter?[64]

64 A slightly different version of this chapter has been submitted to an international scientific journal. The chapter was co-authored by I. Maas, and J. C. Vrooman. Kristiansen wrote the main part of the manuscript and conducted the analyses. Maas and Vrooman contributed substantially to the manuscript. The authors jointly developed the idea and research design, in the context of the project From network to work? at Utrecht University. A special thanks to the Dutch Central Agency for the Reception of Asylum Seekers (COA) and to Mark Kattenberg at the Netherlands Bureau for Economic Policy Analysis (CPB) for lending help with the data.

Abstract

This chapter utilizes a Dutch settlement policy, which exogenously placed refugees in their first municipality, to investigate the income development of working refugees. We hypothesize that being placed in an initial context with a higher percentage of employed people among co-ethnics or among natives, and a larger share of co-ethnics in the population, induces higher initial earnings and subsequently a stronger income growth. We further argue that the impact of the initial context on income growth is stronger when refugees reside longer in their first municipality. Employing multilevel growth models on individual-level administrative data, we find that the characteristics of the initial context do not affect refugees' initial income, nor their income growth at a later stage. The impact of the initial context on income growth does not depend on length of stay in the initial municipality. While the characteristics of the initial neighborhood may matter for entering the labor market, our analyses suggest that the first neighborhood has no impact on refugees' subsequent labor incomes.

5.1 Introduction

How to integrate refugees in the labor market poses a key policy issue for their destination countries, especially in Europe. Research suggests that for this group it is particularly difficult to find a job that makes them economically independent (e.g. Bakker, Dagevos, and Engbersen 2017). In the Netherlands, for instance, about 55 percent of refugees worked for at least 8 hours per week, and only 33 percent were employed for 30 hours or more after 10 years of stay (Engbersen et al. 2015). While poverty is high among people with a non-western background, it is even more prevalent if they originate from countries many people leave due to war or other humanitarian crises (Hoff et al. 2019). In this chapter, we will focus on refugees' income development to gauge to what extent their labor market position improves over time. This sheds light on whether and when refugees obtain a sufficient labor market income, thereby avoiding that they have to rely on social assistance or other governmental benefits.

Several previous studies have investigated the importance of the first area or neighborhood in which immigrants reside to explain their later labor market integration (Andersson, Musterd, and Galster 2019; Beaman 2012; Damm 2009; 2014; Edin, Fredriksson, and Åslund 2003; Martén, Hainmueller, and Hangartner 2019; Åslund and Fredriksson 2009). Theoretically, the neighborhood can indicate the social context and access to social capital in the form of job-referral networks (Damm 2009; Martén, Hainmueller, and Hangartner 2019) and/or indicate ethnic enclaves (Wilson and Portes 1980). Empirically, the interest in refugees is driven by the settlement policies in several countries that effectively lead to an exogenous placement of refugees into neighborhoods. This makes for a natural experimental setting that removes bias associated with self-selection into neighborhoods based on unobserved characteristics, which poses a major problem in studies on neighborhood effects (see van Ham, Boschman, and Vogel 2018). To our knowledge, only three studies have so far used such a natural experiment to examine refugees' income development (Åslund and Rooth 2007; Damm 2014; Godøy 2017).

This chapter investigates the importance of the initial neighborhood context for refugees' income development drawing on a natural experimental setting. Specifically, we analyze refugees settled in the period 1999-2009, when the placement of refugees was exogenous in the Netherlands, and ask: *To what extent does refugees' placement in certain neighborhoods affect their initial income and their long-term income development?* We focus on the influence of the social context by studying the concentration of co-ethnics and the percentage employed among co-ethnics and native Dutch.

This chapter contributes to previous research, first, by offering a dynamic approach to income. Previous research has mainly looked at earnings at *t* years after settlement

in the host country (Damm 2009; Edin, Fredriksson, and Åslund 2003; Åslund and Fredriksson 2009; Beaman 2012). Only a few studies have adopted a longitudinal approach. Damm (2014) uses panel methods and follows refugees up to six years after settlement, and Åslund and Rooth (2007) estimate separate models for each year since immigration up to 10 years. Godøy (2017) employs a more dynamic modelling approach similar to ours. However, she investigates the importance of the aggregated immigrant population such as non-OECD countries or specific geographical regions, rather than co-ethnics with the *same* national origin as in our study. We will analyze refugees' income development using growth curve modelling, which enables us to gauge their income development and its determinants in an integrated way up to 10 years after their first labor market entry. Second, we explicitly consider how long the refugees live in their first neighborhood. Refugees do not always remain in the vicinity of their first housing (Gerritsen, Kattenberg, and Vermeulen 2018), which may impact the extent to which this context has a lasting effect on their subsequent income trajectory. In our analysis, we clarify the role of moving behavior on later income development, and how it may affect the impact of characteristics of the initial neighborhood. Third, we also consider the level of income among co-ethnics and natives in the area refugees are placed in, whereas previous research has focused on the employment rate, the population share of co-ethnics and/or immigrants, and sometimes the level of education (e.g. Damm 2009; 2014; Godøy 2017). Level of income may reflect the degree of success on the labor market more specifically, as opposed to the employment rate which refers to labor market attachment more generally. By investigating the level of income among co-ethnics, we can therefore investigate how the labor market success of co-ethnics affect the success of recently arrived refugees.

To investigate our research question, we make use of longitudinal administrative data from Statistics Netherlands. The data allow us to identify refugees and their first regular housing after being granted asylum, and to follow their income from 2003 until 2017. We focus on labor market income, including both wages and income from business(es), and employ multilevel linear-growth modelling to analyze initial and subsequent income growth.

5.2 Theory and Background

5.2.1 Income development and initial placement

The vast literature on neighborhood effects has a longstanding interest in how local areas affect individual outcomes (van Ham et al. 2012; Sampson, Morenoff, and Gan-

non-Rowley 2002) such as income. There are several theoretical mechanisms thought to underlie these effects, including social mechanisms such as social contagion and social network dynamics (see Galster 2012). In recent research into the role of neighborhoods for *immigrants'* labor market outcomes, mainly two related arguments are put forth. First, ethnic enclaves – defined as spatial concentrations of immigrant owned businesses – are argued to offer immigrants jobs and an opportunity to set-up their own businesses (Wilson and Portes 1980; Portes and Jensen 1989; Bailey and Waldinger 1991; Portes and Shafer 2007; Logan, Zhang, and Alba 2002). In the recent literature, ethnic enclaves have been equated with the number or relative share of co-ethnics in the *residential* neighborhood (e.g. Damm 2009; Martén, Hainmueller, and Hangartner 2019), which is sometimes referred to as 'immigrant enclaves' (Logan, Zhang, and Alba 2002), and we follow this convention here. At its core, the ethnic enclave argument suggests that a higher co-ethnic concentration is beneficial to immigrants as a larger ethnic community means more opportunities. These include a market for 'ethnic' goods that facilitates setting up new businesses, as well as job offers in existing immigrant-run businesses. Presumably, educational credentials and human capital from the country of origin is more valued within the ethnic community than in the general labor market. The ethnic enclave literature originated in the United States where the immigrant communities are larger than in the Netherlands and Europe. The argument nevertheless stresses the importance of co-ethnics in facilitating a swift entry to the labor market for recently arrived immigrants. Therefore, we would expect refugees to have an easier time integrating on the labor market when there are more co-ethnics, which should materialize in higher initial incomes at labor market entry. It is less clear whether this effect persists over time (Sanders and Nee 1987; Xie and Gough 2011) and thus how subsequent income growth is affected. Therefore, it is sometimes argued that jobs in the ethnic enclave often do not offer possibilities for growth but are confined to the lower segment of the labor market.

The second theoretical mechanism relates to the potential social capital co-ethnics represent. Specifically, co-ethnics may convey job leads or other types of information that immigrants can use to improve their labor market position (Munshi 2003; Bayer, Ross, and Topa 2008). Empirically, this has been linked to both the quantity and the quality of the co-ethnics (e.g. Damm 2009). Quantity refers to the number or relative share of co-ethnics, whereas quality pertains to their socioeconomic characteristics such as employment rate or level of income. Commonly, the focus is on the co-ethnic community – implicitly assuming that social capital is chiefly obtained from fellow co-ethnics. We argue, however, that co-residents that do not share the refugees' ethnic background may also provide valuable social capital. This is captured by the notion of

bridging social capital (Putnam 2000), which suggests the most valuable social capital is obtained by crossing boundaries, for instance between ethnic groups. Based on the social capital argument, we would expect refugees to generally perform better on the labor market if placed in a neighborhood where the employment rate among co-ethnics or natives is higher. This should make for both better initial income upon labor market entry and a relatively higher income growth.

Refugees do not only encounter a social environment upon settlement, but also a set of economic structures. The (local) labor market could be favorable or unfavorable for the refugee. For instance, if there is high demand for labor and presumably more suitable jobs available, refugees are more likely to swiftly integrate in the labor market and probably better able to obtain higher incomes. This would allow refugees to accumulate labor market experience and country-specific human capital faster (Godøy 2017), which in turn furthers their career development. Empirically, it is difficult to disentangle the economic structures facing refugees from the social resources argument discussed above when employing measures of the aggregate employment rate or income level in a given area. This issue is arguably especially problematic for the employment rate among natives, and less so for that among co-ethnics, in so far as natives' employment share is more indicative of the overall labor market conditions. Therefore, it is important to note that especially for the employment rate among natives, we presuppose that both social and labor market mechanisms are at play.

There is a recent stream of empirical research that, similar to our study, draws on the exogenous placement of refugees to investigate the role of the neighborhood on refugees' labor market outcomes. The studies that have examined the number or relative share of co-ethnics on refugees' labor market outcomes find no or a positive effect. Studies from Denmark and Sweden suggest a positive effect on earnings but no effect on employment seven or eight years after settlement (Damm 2009; Edin, Fredriksson, and Åslund 2003), whereas Marten et al. (2019) find a positive effect on refugees' employment chances over five years after settlement in Switzerland. As for the quality of the co-ethnics, the evidence generally points to a positive effect. For example, more employed co-ethnics in the area improves refugees' annual earnings seven years after settlement (Damm 2009). Further, Damm (2014) studies income development among Danish refugees from two to six years after settlement. Using panel methods and separate regression models for each year, the study finds that the employment rate among co-ethnics in the neighborhood increases real annual earnings. Yet, the study finds that

refugees' employment probabilities over a 2- to 6-year period after settlement, are not affected by the employment rate among co-ethnics.

Other studies have investigated the impact of general labor market conditions on refugees' income development. Åslund and Rooth (2007) find that for Swedish refugees, earnings development – modelled as separate regressions for up to eleven years after settlement – crucially depend on favorable regional labor market circumstances upon settlement. Labor market circumstances are here defined as the employment and unemployment rate in a local labor market region. Godøy (2017) extends these analyses using Norwegian data, and finds that the effect of the regional labor market on later earnings is mainly driven by refugees' lingering in adverse regional labor markets. None of these studies have to our knowledge investigated the influence of the level of income among co-ethnics (or natives) for refugees' income development. Based on the theoretical mechanisms and previous empirical evidence, we formulate the following hypothesis:

H1: Refugees placed in a neighborhood with a higher share of co-ethnics, or a higher employment rate or level of income among co-ethnics, or a higher employment rate or level of income among the native Dutch, will have (a) a relatively higher income upon labor market entry, and (b) experience a higher income growth during their career.

5.2.2 Length of stay in the first municipality

In addition to the role of the characteristics of the initial placement, we also consider how long refugees remain in their initial neighborhood, which is linked to selective moving behavior. For example, refugees may move away from their initial neighborhood because there are no suitable jobs available in the area or because they get a job offer elsewhere – or decide to stay because they found a job straightaway. Alternatively, refugees may decide to move for reasons that have less to do with their perceived labor market opportunities, such as moving in with a partner. Empirical studies have shown that a considerable share of refugees move away from their initial neighborhood (Damm 2009; Edin, Fredriksson, and Åslund 2003; Gerritsen, Kattenberg, and Vermeulen 2018). For the Swedish case, Edin et al. (2003) found that 46 percent of refugees no longer resided in their initial municipality 8 years after settling there, and the ones who moved tended to opt for more heavily populated municipalities with higher rates of immigrants. Dutch evidence also suggests that after 10 years, about 50 percent of refugees had left the region they were initially placed in (Gerritsen, Kattenberg, and Vermeulen 2018).

For the refugees who stay, we expect a stronger social integration into the initial area. Recently arrived refugees often have no (extensive) personal network in their country of destination (see Andersson, Musterd, and Galster 2019), compared to other types of immigrants (e.g. those who migrate for family reunification). As such, their initial placement may greatly shape their subsequent social integration. Yet, it is likely that their length of stay in this initial location affects to what extent refugees are 'shaped' by their initial neighborhood – what Galster (2012, 28) refers to as 'dosage duration'. Theoretically, one would expect that the longer people reside in a neighborhood, the more socially embedded they become, implying stronger effects of initial placement among refugees the longer they stay. Empirical research has shown that length of residence is positively associated with local friendships and community attachment (Sampson 1988). There is also evidence to suggest that past neighborhood characteristics still affect individuals' current income while also accounting for current neighborhood characteristics (Miltenburg and van der Meer 2018), implying a lasting long-term impact of neighborhoods.

In sum, there are reasons to expect a stronger impact on income growth for those who remain – mainly because they are more exposed to their initial context. This 'effect' is likely partially the result of self-selection. In terms of refugees' initial income upon labor market entry, it is less clear what to expect. Moves may be contingent on a lack of labor market opportunities and the expectation of a better match elsewhere. Alternatively, the refugees who quickly find a job in the proximity of their initial neighborhood are likely to remain the longest, especially if this job pays relatively well. Likewise, those who

spend more time following language courses or other educational programs – likely to be close to their initial residence – may spend more time in their initial neighborhood and attain better paying jobs upon labor market entry. We therefore do not formulate an expectation on the association of length of stay with the neighborhood's influence on initial income. Based on this discussion, we formulate a second hypothesis:

H2: The positive effect of the initial neighborhood on income growth is stronger the longer a refugee resides there.

5.2.3 The settlement of Dutch refugees 1999-2009[65]

In this section, we provide an overview of the asylum procedure, including the settlement policy during 1999-2009. When entering the Netherlands, an asylum migrant undergoes several steps between submitting an asylum request and the final decision. For about a week the request is initially processed at an application center (*'aanmeldcentrum'*). This is a screening phase that tries to quickly reject invalid claims, especially concerning asylum migrants from countries that are deemed safe. If the claim passes this initial process, asylum migrants move to an asylum seeker reception center[66] ('AZC') for screening. These consist of housing units administered by the Central Agency for the Reception of Asylum Seekers ('COA'), the national governmental agency responsible for housing asylum seekers. COA assigns asylum migrants to the reception centers without considering their own preferences (Arnoldus, Dukes, and Muster 2003). Processing in an AZC might take six months or longer, depending on the influx of asylum migrants at the national level. Once a claim is approved, the asylum migrant is granted a temporary or permanent residence permit[67]. We refer to people with an approved asylum claim and a residence permit as 'refugees'.

Upon receiving a residence permit, the refugee must leave the asylum seeker center and has a legal obligation to integrate into Dutch society. Refugees are with few exceptions assigned to regular housing – typically subsidized rental accommodation – in a municipality. Municipalities are required to reserve accommodation for a specific number of refugees in proportion to their number of inhabitants. A refugee is matched to housing by COA, which generally does not consider refugees' housing or location

65 See also section 4.2.3 for a more detailed description, particularly regarding the possibilities for asylum migrants to work while awaiting a final decision on their asylum claim.

66 Periodically, the AZCs have been supplemented due to housing shortages by the so-called supplemental reception centers 'AVO'. When referring to AZC, we mean AZC and equivalent housing arrangements.

67 A residence permit is in most instances granted on a temporary basis initially. After up to five years, the refugee may apply for a permanent residence permit, which is typically approved.

preferences. COA may, however, consider some 'objective' criteria, which include (1) whether the refugee has a job, (2) if close family members already live in the Netherlands, or (3) whether the refugee will be in need of or undergoes medical treatment offered at specific locations only. If any of these criteria apply, housing is sought in proximity to the workplace, family members, or hospital. Although not being formally allowed, in practice refugees sometimes decline housing offers in small municipalities (Dagevos 2007). We describe below how we deal with these exceptions to the otherwise exogenous placement of refugees.

5.3 Data and Methods

5.3.1 Data

We make use of individual-level administrative data from Statistics Netherlands[68]. These cover the entire Dutch population and contain linked longitudinal information on key socioeconomic and demographic characteristics, including personal income, residential history, national origin, and immigration motives. Most data are available for the whole period from 1999 to2017, but unfortunately the income data are only available from 2003 onwards. We also obtained contextual data on all forms of housing administered by the COA – including exact addresses, the type of reception center, and the center's opening and closing dates[69].

The data do not include a straightforward identification of refugees, but we are able to identify refugees based on (1) ever having resided in an asylum seeker reception center, and (2) having asylum as the main migration motive. This strategy also enables us to pinpoint the first regular housing after having left an AZC. Furthermore, we focus on the 12 largest countries-of-origin for refugees[70] as well as refugees in their prime working-age, i.e. 25 to 55 years old at time of first settlement.

68 All results are based on calculations by authors using non-public microdata from Statistics Netherlands. Under certain conditions, these microdata are accessible for statistical and scientific research. For further information: microdata@cbs.nl.

69 These data were linked with the administrative data. We are grateful that Mark Kattenberg (CPB) and COA made these contextual data available to us.

70 These are Afghanistan; Iraq; the former Soviet-Union; former Yugoslavia; Somalia; Angola; Iran; Sierra Leone; Sudan; China; Syria; and Turkey.

To ensure that our selection of refugees has been placed in regular housing exogenously – that is, independent of unobserved characteristics – we introduce some selection criteria to our analytical sample. First, we exclude individuals who have worked while residing in an asylum-seeker center. In this way we minimize the risk that the location of a (prior) job affects the housing offer.[71] Second, we exclude refugees who have stayed in housing arrangements for people with close relatives already living in the Netherlands. These housing arrangements are also administered by COA and thus included in our data. By excluding these refugees, we minimize the risk that family already residing in the Netherlands affects the initial placement in regular housing. It also means that we only include the first refugee of a family that settles, rather than any family members that arrive and apply for asylum at a later stage. We cannot identify people who reject a housing offer. However, this is likely linked to the refugees' household situation; for instance, single refugees might be more inclined to reject offers from small municipalities. This will be accounted for by including detailed variables for the household composition in the initial place of residence in our statistical models. Additionally, refugees' knowledge of the Netherlands is typically limited. We therefore assume that they will not reject housing offers based on their perception of local employment opportunities, and that rejections will not systematically bias our results. In total, 5,483 refugees meet the criteria discussed above and were exogenously placed in their initial municipality. As we are studying income development, we focus specifically on those refugees who ever became active on the labor market, which means that we are left with 2,846 refugees.

5.3.2 Analytical strategy and operationalizations

The resulting dataset is an unbalanced person-year dataset with in total 2,846 refugees observed for 16,723 person-years. It is unbalanced in the sense that refugees are observed for an uneven number of years. This stems from mainly two aspects: (1) That we include only years in which refugees are active on the labor market – meaning refugees can reappear if they become active again after some years of non-employment; and (2) that we include all refugees who were active on the labor market during the period 2003-2017 – implying that refugees who enter in 2016, for instance, can only be observed for two years at the most. Refugees are followed for a maximum of 10 years after their initial entry year – i.e. 11 years in total.

71 Asylum seekers may work – for a limited number of months per year – if the application procedure takes more than six months. The employer and asylum seeker have to apply for a work permit in these instances.

To test our theoretical expectations and investigate refugees' income development, we employ multilevel linear growth-curve modeling (Rabe-Hesketh and Skrondal 2008; Singer and Willett 2003) of the general form:

$$y_{ij} = {}_1 + {}_2 t_{ij} + {}_3 t^2_{ij} + {}'_4 X_i + {}'_5 (X_i \times t_{ij}) + {}'_6 Z_{ij} + {}'_7 (Z_{ij} \times t_{ij}) + {}_{1i} + {}_{2i} t_{ij} + {}_{3i} t^2_{ij} + {}_{ij} \quad (1)$$

in which years *j* are nested in refugees *i* and their income y is predicted. Time effects are captured by t and t2, Xi is a set of neighborhood characteristics, Zij is a set of time-constant and time-varying control variables, and 1i, 2itij, and 3it2ij allow for random effects of the intercept, linear time, and quadratic time, respectively. This method is attractive because it allows us to disentangle (1) initial income differences between refugees upon labor market entry from (2) subsequent changes in income. It also allows testing the effect of a variable on change in income by including interactions with time ('5and '7)[72]. Time is defined as years active on the labor market counting only years that the refugee was working, with the first year active on the labor market coded zero. Refugees who are first active on the labor market prior to 2003 are included, although their income development can only be observed from 2003 onwards. The intercept can be interpreted as refugees' average first or initial income.

Response variable

The response variable is refugees' *personal gross income* from the labor market – including wages and income from business(es)– in euros. We opt for the gross labor-market income because this measure excludes social transfers and is therefore a better indicator for refugees' labor market success, compared to total income for instance. Because we compare refugees in different years, we adjust income using the harmonized consumer price index keeping year 2003 as the reference point[73]. Yearly negative or zero income are set at 1. We here consider absolute income rather than the log income, as we are interested in refugees' income mobility rather than their relative income change or reduction of income risk, which are better studied with changes in log income. Additionally, the income distribution among the refugees analyzed here was not very skewed, with few high incomes, which typically pose a larger statistical problem when studying the income distribution of the population at large. A robustness

72 We also test for quadratic changes over time. We explain this further in the results section.

73 We checked whether adjusting incomes using a different index that captures yearly increases in salaries for employees covered by collective labor agreements ('CAOs') would change the results. These agreements cover about 8 in 10 employees (Statistics Netherlands 2019b) and indicate general developments in the salaries. This adjustment does not alter the conclusions [results not shown].

check revealed that running the analyses with log income as the response variable did not alter the substantive conclusions [results not shown].

To avoid problems related to shorter employment spells within a year – for instance starting to work in December or only work a few months a year – which would inflate the estimated growth per year, we divide the yearly incomes by the number of months active on the labor market. Whereas the income data stem from yearly tax reports, the labor market activity data are based on slightly different register information on monthly labor market activity, such as whether the person holds a job or is self-employed. This leads to a few minor inconsistencies because some people are registered as receiving labor market income according to the tax records yet are *not* registered as active on the labor market. This was the case for 283 person-month observations which we left out of the analyses. Additionally, self-employment is only registered on a yearly basis, and is therefore by default divided by 12.

Main explanatory variables

Further, we include characteristics of the initial neighborhood in which refugees were first settled. These are constructed by aggregating the individual-level administrative data and focus on the population aged 18-65 in the area. First, we measure the *percentage of co-ethnics in the municipality*. This variable captures the percentage of residents in the municipality with the same national-origin background as the refugee. We also include a binary variable that indicates the special cases that have no co-ethnics in the municipality they are first settled in. Second, we include a measure of the *percentage employed among the co-ethnics in the municipality*, in which employment is defined as deriving one's major source of income from the labor market. All indicators are measured on January 1 in the year of settlement. Third, we include a measure of the *percentage employed among the natives* in the neighborhood, measured at 4-digit zip code level. Whereas we assume that refugees' contact with other co-ethnics is better approximated at the municipality level, a more local indicator, e.g. at the neighborhood level, may be more suitable for refugees' contact with natives – keeping in mind that this measure probably also captures overall (local) labor market conditions. Fourth, we also include measures of the *median annual income among co-ethnics* in the municipality, and the *median annual income among natives* in the neighborhood. As the income-data are only available from 2003 onwards,

we could only construct these measures for refugees who settled from 2003 and onwards[74]. Therefore, we present these as a separate set of analyses. Because we are comparing refugees who arrive in a neighborhood in different years – and the distribution of refugees over years is not random, we center the neighborhood measures at the mean per year of settlement. In doing so, the estimates reflect the relative deviations from the yearly means, ensuring that annual fluctuations in overall employment rates do not affect our results.

We also consider whether the impact of these initial neighborhood characteristics varies by *how long the refugee resides in the initial municipality*. To test this, we include the time-varying total number of months living in the municipality. The variable captures the number of months resided in the initial municipality and stops counting when the refugee moves elsewhere. By including the time-varying total number of months, we presume that social integration increases the longer the refugee resides in this municipality. We opt for municipality rather than a more localized measure as moves within a municipality are less likely to alter a person's social life than moves across municipalities. The measure is divided by twelve and grand-mean centered to ease interpretation.

Finally, we include some other covariates that are described in table A5.1 in the appendix. The descriptive statistics for all variables are displayed in table 5.1.

74 For the explanatory variables pertaining to the level of income, we cannot include refugees settled prior to 2003 because we have no information on the level of income in the neighborhood or municipality they settled in prior to 2003. For the response variable, we do include refugees who settled prior to 2003 because we can still observe the employment share and other neighborhood characteristics for these refugees.

Table 5.1. Descriptive statistics for all variables.

Variable	Mean	SD	5th percentile	95th percentile
Monthly income (€)	1486.175	1533.694	92.899	3490.082
Time (Years active LM)	3.669	2.902	0	9
% employed co-ethnics	29.087	16.616	0	55.587
% employed natives	70.951	6.296	59.868	79.119
% co-ethnics	.242	.289	.021	.652
No co-ethnics (ref. any)	.018			
Nr. months initial municipality/12	6.180	3.801	.833	13.167
Years outside LM	.416	1.086	0	3
Whether self-employed	.134			
After 2010	.534			
Female	.243			
Age 1st entered LM[c]	36.867	6.983	27	49
National unemployment rate (%)	5.475	1.133	3.700	7.400
Household-position (categorical)				
Child in household	.014			
Single (ref.)	.386			
Partner in couple wo/children	.077			
Partner in couple w/child(ren)	.460			
Single parent	.050			
Other	.013			
National origin (categorical)				
Iraq (ref.)	.293			
Afghanistan	.246			
Former Yugoslavia	.105			
Former Soviet-Union	.073			
Somalia	.094			
Angola	.016			
Iran	.056			
Sierra Leone	.044			
(former) Sudan	.038			
China	.006			
Syria	.026			

Table 5.1 continues

Turkey	.003
N(person-years)	16,723
N(individuals)	2,845

Notes: Descriptive statistics at person-year level. SD (standard deviation) is only displayed for continuous variables. Mean represents proportion for categorical or binary variables.

5.4 Results

Table 5.2 shows the results of the growth-curve regression models for the main explanatory variables; table A5.2 contains all estimates. Following the suggestions of Singer and Willet (2003), we first estimate a series of 'empty' models with only the effect of time. These serve as a reference point for the more extensive models that includes explanatory variables. Model 1 is an unconditional means model, with only an intercept and a random-intercept. This model suggests that refugees on average earn 1349 euros per month across all years, and that there is considerable variation around this mean – with a standard deviation of 874 euros. Model 2 additionally includes a linear effect of time and its random effect, which is sometimes referred to as an unconditional growth model. The reduction in the deviance statistic[75] between the two models suggest that a model that allows for linear changes in monthly income is better (2(3)=2353.1, p<.001). This model shows that refugees earn on average 1043 euros per month in their first active year on the labor market, and that there is quite some variation in the initial labor market income (SDintercept=1017). Refugees' labor market income increases by about 104 euros per year active on the labor market (p<.005), but again with considerable individual variation in the rate of change (SDtime=184.6, p<.005). It is worth noting that the correlation between the random-intercept and random-effect of time is significant and negative (-.511, p<.005), implying that the higher refugees' initial income, the slower their linear increase over time.

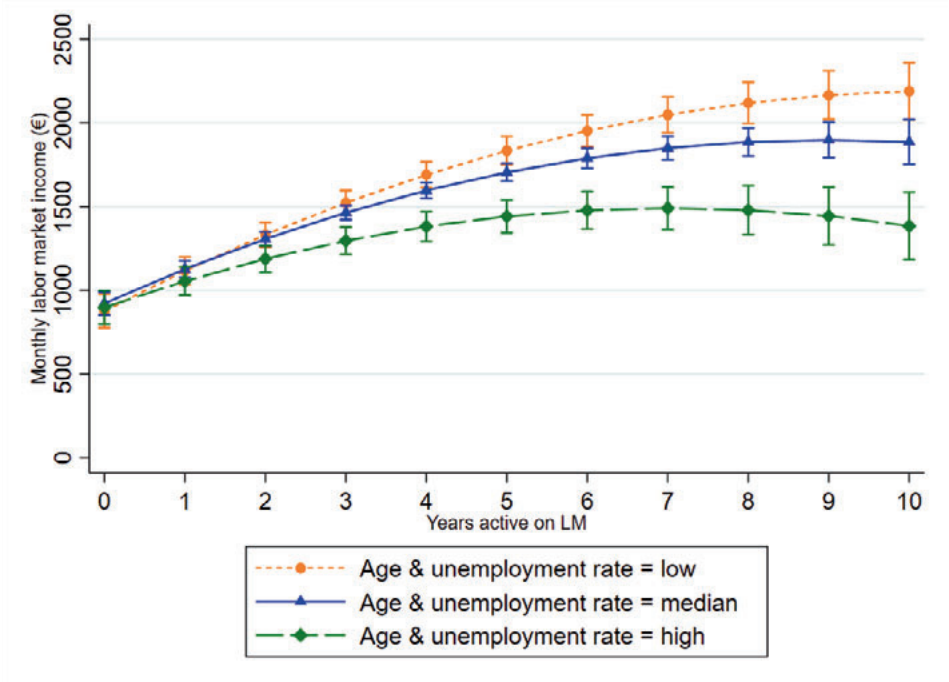
Model 3 adds to model 2 a quadratic effect of time and its random-effect. As reduction in the deviance statistic is significant, these additions improve model fit (2(4)=1695.7, p<.001). The model shows that refugees' monthly labor market income increases at a decelerating rate by the years they were active on the labor market. Initially, the increase is quite high: After 1 year of work the growth rate is 168. However, after 10 years of labor market activity, the growth is negative, and estimated at -31. We also tried out

75 Defined as -2ln(likelihood).

some alternative time specifications, including splines, but a quadratic time function proved a reasonable compromise over more complexity.

In the next models, we add various groups of explanatory variables step-by-step. Model 4 includes all covariates except the main explanatory variables of interests. In a separate step [not shown], we tested for interactions with linear and quadratic time for all covariates – except for household situation at settlement and national-origin group. In order to preserve model parsimony, we included only those that were significant. Unsurprisingly, the deviance statistic shows a significant improvement in model fit (2(23)=258.6, p<.005) compared to model 3 without any covariates. To illustrate refugees' income trajectories and the influence of some of the control variables, figure 5.1 shows three predicted income trajectories based on model 4. It depicts the impact of varying age at labor market entry and the annual national unemployment rate, while keeping the other (categorical) variables constant. The graph makes clear that refugees' labor market income is about the same at labor market entry, but over time refugees experience a better income development when they enter the labor market at relatively young age and while the national unemployment rate is relatively low – i.e. the 10th percentile and/or the median. The income development is worse, however, when refugees enter the labor market when they are older and while the national unemployment rate is relatively high – i.e. at the 90th percentile.

Figure 5.1. Estimated average income trajectories when varying age entered labor market and national unemployment rate, with 95% confidence intervals.



Notes: Based on model 4 in table 5.2. Low and high mean 10th and 90th percentile, respectively. All other variables kept constant.

Table 5.2. Results from multilevel growth models regressing income on characteristics of the initial neighborhood (reduced table).

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7
Fixed-part							
Intercept	1349.487	1043.348	953.376	920.439	930.106	931.877	927.791
	(68.23)**	(43.83)**	(30.40)**	(17.73)**	(17.72)**	(17.75)**	(17.17)**
Time		104.059	190.496	256.837	256.681	255.395	259.030
		(18.79)**	(13.26)**	(13.45)**	(13.43)**	(13.36)**	(12.97)**
$Time^2$			-11.057	-17.696	-17.687	-17.536	-17.577
			(-8.00)**	(-7.38)**	(-7.38)**	(-7.30)**	(-7.13)**
% working among co-ethnics munic.[a]					-.807	-2.458	-2.270
					(-.57)	(-1.20)	(-1.11)
*% working among co-ethnics munic.[a]*Time*						1.104	.808
						(1.24)	(.91)
% working among co-ethnics munic.[a]$Time^2$*						-.115	-.098
						(-1.35)	(-1.15)
% working among native neighb.[a]					6.119	8.358	7.794
					(1.78)	(1.54)	(1.44)
*% working among native neighb.[a] *Time*						-2.032	-1.924
						(-.83)	(-.79)
*% working among native neighb.[a] *$Time^2$*						.295	.252
						(1.26)	(1.08)

Table 5.2 continued

% co-ethnics in munic.[a]					-46.370	12.149	5.135
					(-.57)	(.11)	(.04)
% co-ethnics in munic.[a]**Time*						-48.911	-45.089
						(-.96)	(-.89)
% co-ethnics in munic.[a]**Time*[2]						6.395	5.790
						(1.23)	(1.11)
Whether any co-ethnics munic.[a]					.080	75.518	66.111
					(.00)	(.35)	(.30)
Whether any co-ethnics munic.[a]**Time*						-10.389	-10.391
						(-.10)	(-.10)
Whether any co- ethnics munic.[a]**Time*[2]						-6.100	-6.086
						(-.54)	(-.54)
Covariates[b]	NO	NO	NO	YES	YES	YES	YES
Mediating covariates[c]	NO	NO	NO	NO	NO	NO	YES
Random-part							
$SD_{residual}$	1212.954 (1203.59)**	1054.138 (1102.38)**	927.378 (1017.83)**	922.860 (1017.32)**	922.846 (1017.32)**	922.852 (1017.31)**	920.675 (1017.27)**
$SD_{intercept}$	874.152 (384.39)**	1016.865 (352.93)**	1437.712 (417.13)**	1425.254 (414.84)**	1424.156 (414.57)**	1423.510 (414.51)**	1423.139 (414.76)**
SD_{time}		184.588 (208.74)**	557.097 (306.06)**	553.426 (306.11)**	553.644 (306.15)**	553.029 (305.82)**	553.854 (306.65)**

SD_{time2}			45.302	45.245	45.271	45.166	45.136
			(149.83)**	(150.75)**	(150.81)**	(150.36)**	(150.74)**
Cor(time, intercept)		-.511	-.796	-.804	-.804	-.804	-.809
		(-19.63)**	(-38.55)**	(-38.86)**	(-38.85)**	(-38.83)**	(-39.19)**
Cor(time, time^2)			-.962	-.963	-.963	-.963	-.965
			(-43.17)**	(-43.42)**	(-43.45)**	(-43.31)**	(-43.29)**
Cor(intercept, time^2)			.768	.777	.776	.776	.781
			(27.30)**	(27.46)**	(27.45)**	(27.42)**	(27.57)**
Deviance	288,612.7	286,259.6	284,563.9	284,305.0	284,300.8	284,294.0	284,164.9
Parameters	3	6	10	33	37	45	50
AIC	288,618.7	286,271.6	284,583.9	284,371.0	284,374.8	284,384.0	284,264.9
BIC	288,641.8	286,318.0	284,661.1	284,625.9	284,660.6	284,731.6	284,651.1
$R^2_{intercept}$				.017	.019	.020	.020
R^2_{time}				.013	.012	.015	.012
R^2_{time2}				.003	.001	.006	.007
$R^2_{residual}$		.245	.415	.421	.421	.421	.424
N(person-years)	16,723						
N(individuals)	2,845						

Notes: [a]= Centered at mean of year of settlement; [b]= age 1st entered LM, female, after 2010, national unemployment rate, national origin (ref. Iraq), household-position (ref. single); [c]= years outside LM, self-employment. *= p<.05, **= p<.005 (two-sided). Z-statistic between parentheses for fixed-part, standard error between parentheses for random-part. See table A5.2 in appendix for all estimates.

In models 5 and 6, we include the indicators for the initial neighborhood characteristics in two steps. Model 5 adds the main effects of the percentage employed among co-ethnics and among natives, and that of the percentage of co-ethnics – that is, their effects on refugees' monthly income at labor market entry. We see that neither effects are significant, nor does the model fit improve significantly compared to model 4 ($\chi^2(4)=3.8$, $p>.05$). The same holds true when we test whether the neighborhood characteristics affect the rate of change in refugees' income in model 6: The characteristics of the initial neighborhood do not affect the development of refugees' income over time. As a check, we included the neighborhood effects and their interactions with time one by one – including linear and quadratic time, which did not yield different results. These results run counter to what we hypothesized (H1) and suggest that once active on the labor market, refugees' income development is unaffected by the first living environment they encountered.

In model 7, we add two covariates – whether the refugee receives some income from self-employment and the number of years outside of the labor market after they first started working (all coefficients are displayed in table A5.2 in appendix section 5.6). Given the role attested to self-employment in the ethnic-enclave literature and that placement in a poor neighborhood could involve more or longer unemployment spells, we suspected that these variables could underlie the relationship between the initial neighborhood characteristics and income development. However, after including self-employment and the number of years spent outside of the labor market, we still do not find an effect of initial neighborhood characteristics. Judged by the significant reduction in the deviance statistic compared to model 6 ($\chi^2(5)=129.1$, $p<.001$), these variables by themselves do add to the explanation of refugees' income development. The results suggest that refugees' who enter self-employment have lower initial incomes but subsequently experience a faster linear growth in income. Years spent outside the labor market after refugees first started working is associated with a slower growth.

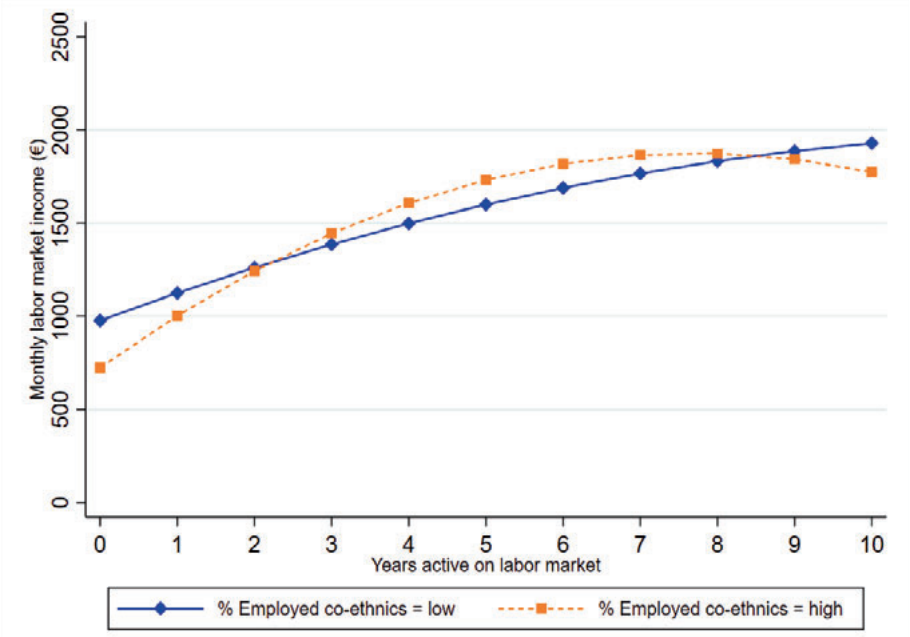
Next, we tested whether the neighborhood effects on income development differ by how long a refugee stayed in their first municipality. Although model 5 and 6 suggest that, on average, refugees' income trajectories are not affected by their initial area, refugees who remain and who move to a different municipality may by impacted differently by their initial surroundings. Because this involves testing a three-way interaction between time, number of months in the initial municipality, and the neighborhood characteristics, we do this in four steps as shown in table 5.3 (see table A5.3 for all estimates). Model 8 includes the main effect of time spent in the initial municipality and its effect

on income development. It indicates that length of stay, on average, does not affect refugees' initial level of income nor their income development.

In models 9a-9c, we include the three-way interaction effects between initial neighborhood characteristics, length of stay in the initial municipality, and years active on the labor market. Two findings emerge from these models. Model 9a indicates that there is a significant three-way interaction for the employment rate among co-ethnics, length of stay, and time. To better interpret this interaction effect, figures 5.2a and 5.2b depict it as income trajectories. The graphs show the income trajectories by the number of years active on the labor market, when the percentage of employed co-ethnics is either high or low, and how these trajectories differ when the length of stay is short (figure 5.2a) or long (figure 5.2b)[76]. We hypothesized that the positive effect of the initial neighborhood characteristics on *income growth* would be stronger the longer the refugee resided in their initial municipality (H2). This would mean that, for instance, the growth for refugees placed in a municipality where the employment rate was high (the dashed lines in figures 5.2a and 5.2b) would be lower when time spent in the initial municipality is short (figure 5.2a) than when time spent in the initial municipality is long (figure 5.2b). It turns out that this is not the case: Refugees' income growth seems higher when placed in an area where the employment rate among co-ethnics is low and the refugee remains in that area longer (compare the shape of the solid line in figure 5.2a versus the one in figure 5.2b), implying the opposite. Overall, however, we see that despite some differences in the shape of the income trajectories, the differences are relatively small: After 10 years of labor market activity, refugees' predicted incomes are about the same, between 1750 and 2000 euros per month.

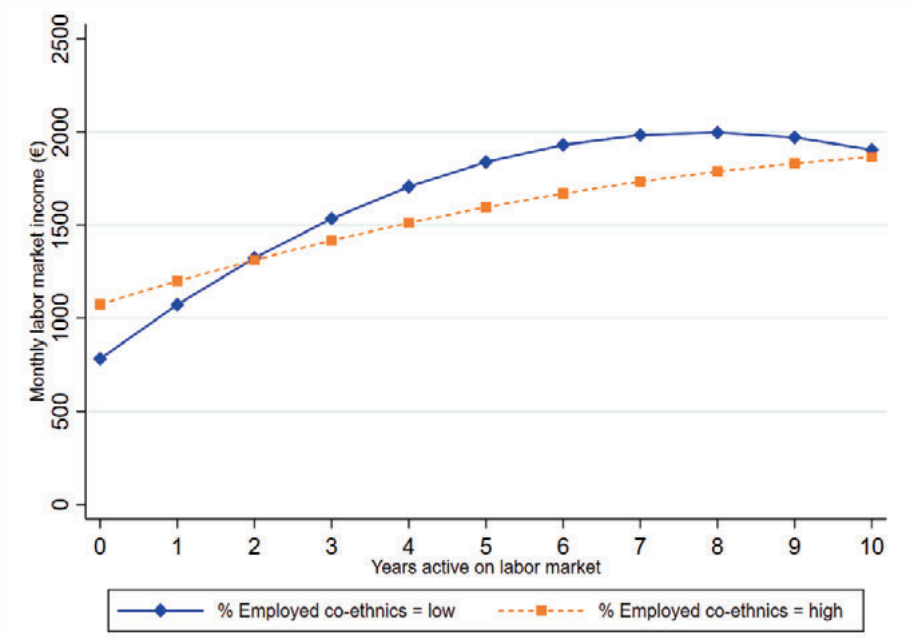
76 The figure shows what the trajectories would look like if the employment rate among co-ethnics and time spent in first or initial municipality were fixed over refugees' career. Because time spent in initial municipality is a time-varying variable, however, refugees may gradually 'move' towards the long length of stay trajectories during their career.

Figure 5.2a. Estimated income trajectories by employment rate among co-ethnics in initial municipality and years active on the labor market when length of stay in first municipality is short.



Notes: Based on model 9a in table 5.3. Low/short and high/long mean 10th and 90th percentile, respectively.

Figure 5.2b. Estimated income trajectories by employment rate among co-ethnics in initial municipality and years active on the labor market when length of stay in first municipality is long.



Notes: Based on model 9a in table 5.3. Low/short and high/long mean 10th and 90th percentile, respectively.

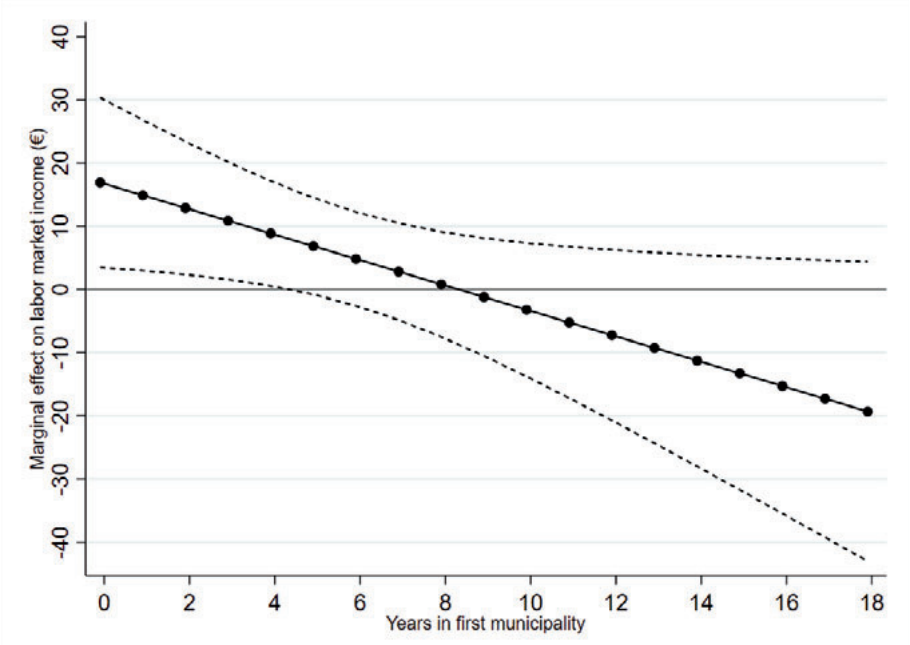
It is worth noting that the trajectories also reveal some initial income differences on which we did not formulate any hypotheses. We find that being placed in a municipality where relatively many co-ethnics work is associated with *lower* initial incomes when length of stay is *short*, compared to when length of stay is long. However, when refugees are placed in a municipality where the employment rate among co-ethnics is relatively low, *shorter* stays in the initial municipality is associated with *higher* initial income. These initial differences even out the more years refugees are active on the labor market and become negligible. In sum, we find that it is not the case that the influence of the employment rate among co-ethnics in the initial municipality on income growth is higher the longer a refugee resides in the initial municipality – counter to what we hypothesized (H2). This finding is corroborated by figure A5.1 in the appendix (section 5.6), which depicts the average marginal effect of the employment rate among

co-ethnics by years active on the labor market when the time refugees reside in their first municipality is short and long.

The second finding to emerge from models 9a-9c is that there is a significant, negative interaction effect on the level of income in model 9b between the number of months refugees stay in their initial municipality and the percentage of employed Dutch natives in the initial neighborhood, which we did not formulate a theoretical expectation about. Figure 5.3 displays how the average marginal effect of percentage working among natives in the initial neighborhood on income varies by length of stay in initial municipality, or, in other words, how the relative shift in the level of income varies by the employment rate among natives and years in the initial municipality at a given point during the refugees' career. The figure makes clear that while shorter periods of stay in the initial municipality are associated with a higher and positive effect of percentage working among natives, the positive effect of the employment rate among natives decreases as length of stay increases and becomes insignificant after about 4 years in the initial municipality. For instance, we see that the average marginal effect of percentage employed natives is about 15.8 when length of stay is short (5th percentile). This means that a one percent increase in the employment rate among natives is on average associated with a 15.8 euro increase in refugees' income, which amounts to an almost 99.5 euro increase in refugees' income with a one standard deviation increase in the employment rate among natives[77].

77 We used the standard deviation of the employment rate among natives as shown in table 5.1, which is 6.296. The standard deviation is similar although slightly lower for the group mean-centered variable included in the analyses, specifically 5.957.

Figure 5.3. Average marginal effect of employment rate among natives in initial municipality on refugees' level of income by time spent in initial municipality.



Notes: Based on model 9b in table 5.3. Dotted lines above and below solid line represent 95% confidence interval.

Finally, we ran analyses with the median income among co-ethnics in the municipality, and among natives in the neighborhood – instead of the employment rate among these groups. Because we had to restrict the selection of refugees to those settled from 2003 and onwards, the number of refugees included is smaller (N=1,730). Consequently, the models would not converge with a random effect for quadratic time, which we therefore left out. Otherwise, the models are fitted similarly to the models presented in tables 5.2 and 5.3. The results are presented in the appendix (section 5.6) and are overall in line with the results presented thus far. Still, the analyses reveal two findings worth paying some attention to. Model 6 in table A5.4 indicates that there is a significant effect of median income among co-ethnics on income growth, which disappears once mediating covariates – self-employment and years outside the labor market – are added in model 7. Further investigations revealed that the effect in model 6 is overall small and does not make for drastically different income trajectories. This is depicted in figures A5.2 and

A5.3 in the appendix. Second, model 9a in table A5.5 shows that there is a significant 3-way interaction between median level of income among co-ethnics, time spent in initial municipality, and years active on the labor market, which has the same shape as its 'equivalent' in model 9a in table 5.3 for the employment rate among co-ethnics. Again, the effect is negligible and does not affect refugees' income development substantively. This is clearly visible in figures A5.4a, A5.4b, and A5.5 in the appendix.

To further examine the robustness of these findings, we ran analyses in which we included the logged median income among co-ethnics and natives in refugees' initial neighborhood. These analyses do not find a significant effect of median income [results not shown], further reinforcing the interpretation that these results are of little substantive importance. Summarizing, the results from the analyses reveal the following: Refugees' initial level of income and subsequent income development are overall not affected by the level of income among co-ethnics or among Dutch natives, again providing no support to hypothesis 1. The impact of the level of income in the initial neighborhood on refugees' income growth does not depend on their length of stay in the initial municipality, again lending no support hypothesis 2.

Table 5.3. Results from multilevel growth curve models regressing income on characteristics of initial neighborhood and length of stay in initial municipality (reduced table).

	Model 8	Model 9a	Model 9b	Model 9c
Fixed-part				
Intercept	952.142 (14.85)**	952.559 (14.84)**	950.618 (14.84)**	952.949 (14.86)**
Time	253.369 (10.68)**	252.888 (10.65)**	254.264 (10.71)**	253.140 (10.66)**
$Time^2$	-17.368 (-6.24)**	-17.260 (-6.20)**	-17.452 (-6.27)**	-17.357 (-6.23)**
% working among co-ethnics munic.[a]	-2.331 (-1.14)	.054 (.02)	-2.330 (-1.14)	-2.340 (-1.14)
*% working among co-ethnics munic.[a]*Time*	.781 (.88)	-.188 (-.18)	.794 (.89)	.784 (.88)
*% working among co-ethnics munic.[a]*Time2*	-.096 (-1.12)	-.006 (-.06)	-.097 (-1.14)	-.097 (-1.13)
% working among native neighb.[a]	7.913 (1.46)	7.836 (1.44)	-1.663 (-.25)	7.759 (1.43)
*% working among native neighb.[a] *Time*	-1.895 (-.77)	-1.804 (-.74)	1.469 (.51)	-1.806 (-.74)
*% working among native neighb.[a] *Time²*	.251 (1.07)	.243 (1.04)	.042 (.15)	.245 (1.05)

Table 5.3 continued

% co-ethnics in munic.[a]	.219	-8.965	-.965	28.575
	(.00)	(-.08)	(-.01)	(.23)
*% co-ethnics in munic.[a]*Time*	-45.263	-38.686	-46.979	-51.757
	(-.89)	(-.76)	(-.92)	(-.88)
*% co-ethnics in munic.[a]*Time²*	5.770	5.283	5.943	5.066
	(1.11)	(1.01)	(1.14)	(.79)
Whether any co-ethnics munic.[a]	65.713	64.346	72.953	103.507
	(.30)	(.30)	(.33)	(.39)
*Whether any co-ethnics munic.[a]*Time*	-8.914	-8.904	-9.963	-32.416
	(-.08)	(-.08)	(-.09)	(-.27)
*Whether any co- ethnics munic.[a]*Time²*	-6.159	-6.188	-6.153	-3.671
	(-.54)	(-.55)	(-.54)	(-.29)
Nr. months initial munic./12[b]	6.923	6.622	6.464	7.073
	(.61)	(.58)	(.57)	(.62)
*Nr. months initial munic./12[b]*Time*	-.095	.020	.114	-.201
	(-.02)	(.00)	(.03)	(-.05)
*Nr. months initial munic./12[b]*Time²*	-.027	-.038	-.047	-.009
	(-.07)	(-.10)	(-.13)	(-.02)
Nr. months initial munic./12[b]% working among co-ethnics munic.[a]*		1.310		
		(2.13)*		

Nr. months initial munic./12[b]% working among co-ethnics munic.[a]*-Time*	-.781 (-3.40)**		
Nr. months initial munic./12[b]% working among co-ethnics munic.[a]*-Time²*	.068 (3.33)**		
Nr. months initial munic./12[b]% working among native neighb.[a]*		-4.344 (-2.68)*	
Nr. months initial munic./12[b]% working among native neighb.[a]*-Time*		1.164 (1.96)	
Nr. months initial munic./12[b]% working among native neighb.a*-Time²*		-.089 (-1.70)	
Nr. months initial munic./12[b]% co-ethnics in munic.[a]*			24.293 (.72)
Nr. months initial munic./12[b]% co-ethnics in munic.[a]*Time*			-8.992 (-.68)
Nr. months initial munic./12[b]% co-ethnics in munic.[a]*Time²*			.966 (.76)
*Nr. months initial munic./12[b]*Whether any co-ethnics munic.[a]*			25.964 (.36)
*Nr. months initial munic./12[b]*Whether any co-ethnics munic.[a]*Time*			-25.119 (-.87)

Table 5.3 continued

Nr. months initial munic./12[b]**Whether any co-ethnics munic.*[a]**Time*[2]				2.785 (1.01)
Covariates[b]	YES	YES	YES	YES
Mediating covariates[c]	YES	YES	YES	YES
Random-part				
SDresidual	45.130 (150.69)**	45.209 (150.75)**	45.132 (150.65)**	45.119 (150.65)**
SDintercept	553.842 (306.61)**	554.570 (306.67)**	553.814 (306.46)**	553.876 (306.56)**
SDtime	1423.116 (414.73)**	1423.653 (414.81)**	1421.282 (414.32)**	1422.999 (414.60)**
SDtime^2	-.809 (-39.19)**	-.809 (-39.21)**	-.809 (-39.16)**	-.809 (-39.18)**
Cor(time, intercept)	-.965 (-43.23)**	-.965 (-43.31)**	-.965 (-43.23)**	-.965 (-43.17)**
Cor(time, time^2)	.781 (27.56)**	.781 (27.59)**	.781 (27.55)**	.782 (27.54)**
Cor(intercept, time^2)	920.668 (1017.21)**	920.050 (1016.92)**	920.612 (1017.18)**	920.708 (1017.13)**
Deviance	284,164.0	284,152.0	284,156.4	284,162.0

Nr parameters	53	56	56	59
AIC	284,270.0	284,264.0	284,268.4	284,280.0
BIC	284,679.4	284,696.6	284,700.9	284,735.8
R2intercept	.020	.019	.023	.020
R2time	.012	.009	.012	.012
R2time^2	.008	.004	.008	.008
R2residual	.424	.425	.424	.424
N(person-years)	16,723			
N(individuals)	2,845			

Notes: [a]= Centered at mean of year of settlement; [b]= age 1st entered LM, female, after 2010, national unemployment rate, national origin (ref. Iraq), household-position (ref. single); [c]= years outside LM, self-employment. *= p<.05, **= p<.005 (two-sided). Z-statistic between parentheses for fixed-part, standard error between parentheses for random-part. See table A5.3 in appendix for all estimates.

5.5 Discussion and Conclusions

A key policy challenge in many countries over the past few decades was how to ensure refugees integrate (swiftly) into the labor market. This chapter investigates refugees' income development after they have entered the labor market. Specifically, it examines whether the initial context in which refugees are placed affects their initial income and/or later income development. We make use of a Dutch settlement policy that was in effect between 1999-2009 and placed refugees exogenously in their first regular housing. We use individual-level administrative data to capture the refugees placed exogenously and focus specifically on those who ever become active on the labor market. Employing multilevel growth-curve modeling, we examine the impact of initial area characteristics among co-ethnics and natives on refugees' initial labor market income, and its subsequent growth.

First, we find that once active on the labor market, refugees' incomes are unaffected by the initial area they were placed in after being granted asylum. Neither their initial nor their subsequent growth in labor market income depend on the share of co-ethnics, the employment rate among co-ethnics or among natives, or the level of income in the initial area refugees are placed. As such, the results lend some support to Sanders and Nee's (1987) conclusion that there are limits to the gains that may be reaped from the co-ethnic community or the ethnic enclave – although our outcomes do not suggest the contrary: there is no *lower* income growth when refugees are placed in an area with relatively many co-ethnics. Furthermore, our results partially contradict findings from previous studies. Damm (2014) concluded that the employment rate among co-ethnics positively affects refugees' earnings over time, and Godøy (2017) found that a higher employment rate among other non-OECD immigrants positively affects earnings especially during the early career. In contrast, our analyses indicate that for the Dutch case, initial co-ethnic employment rates affect neither initial income nor income growth. To some extent, these diverging results may be the result of how refugees without any incomes have been treated. In our analyses we focus specifically on refugees who are active on the labor market, whereas the previous studies also included refugees who are not active and have no labor market income (Damm 2014; Godøy 2017). This suggests that the initial neighborhood characteristics may be more important for finding a job rather than for the further career development of refugees. Additionally, we find that neither the employment rate nor the level of income among natives in the initial neighborhood affect refugees' income development. Contrary to our theoretical arguments, this finding implies that the characteristics of natives in the initial neighborhood do *not* represent valuable social capital for the income position

of working refugees. Similarly, the labor market circumstances the characteristics of natives may also represent, do not affect refugees' income development.

Second, we do *not* find that length of stay in the initial municipality strengthens the impact of the initial context on income growth, counter to our theoretical reasoning. Although the results suggest that there are some differences pointing in the opposite direction, substantively, however, this impact is quite small and we therefore conclude that there is little evidence that the impact of initial placement on income development is affected by length of stay, or in other words residential mobility. There is some evidence, however, that when refugees have stayed relatively short in their initial municipality, the employment rate among natives has a higher positive effect on refugees' *level* of income – but not on their income growth. This suggests that those placed in an area with a higher employment rate among natives obtain higher paying jobs, but do not subsequently attain a better income growth. As length of stay increases, however, the impact of the employment rate among natives in the initial neighborhood wanes off and becomes negligible. In other words, refugees' level of income is less impacted by their initial environment the longer they remain in this environment. Moving behavior may play some part in explaining this: Placement in a 'better' area could be beneficial because refugees obtain better jobs elsewhere. Similarly, moving behavior may also mean that the refugees who are more mobile also have better capacity to increase their incomes because of other unmeasured characteristics. This could mean that refugees who linger in their initial municipality become more homogenous along unmeasured individual characteristics – leading to a smaller difference as length of stay increases. Future research may investigate this further.

This chapter used the exogenous placement of Dutch refugees to their first regular housing, to study their income development once they become active on the labor market. Because of the placement procedure, the measures associated with their initial placement are not confounded with other individual characteristics – including educational attainment. However, as we find that characteristics of the initial area overall do not affect refugees' income, it is likely that these other characteristics are particularly important for their career and income development. With the register data at our disposal, we are unfortunately not able to gauge the impact of for instance educational level or language proficiency (see, e.g., de Vroome and van Tubergen 2010).

In sum, this chapter finds that refugees on average experience a growth in their monthly income. This could result from securing better-paid work, an increase in the number of hours worked, or a combination of the two. Overall, it seems that their initial level of income and income growth are not affected by the initial context they are placed in – in terms of its co-ethnic composition and the socioeconomic characteristics of the

co-ethnics or native Dutch in the initial context. It is necessary to stress that while the initial area refugees are placed in does not seem to matter for income once active on the labor market, we know from other studies that the first residence does matter for labor market entry and subsequent employment probabilities (Damm 2009). From a policy perspective, our results point to the importance of factors other than initial context (e.g. individuals' skills and education), to ensure the long-term labor market integration and a favorable income development of refugees.

5.6 Appendix

5.6.1 Additional tables

Table A5.1. List of covariates not described in the text.

Variable	Definition
Years outside of labor market	Time-varying number of years outside of the labor market after initially having entered the labor market.
Income from self-employment	Binary time-varying variable coded 1 if some income was received from self-employment. We opt for the broad definition 'income from self-employment' rather than 'being self-employed' because the data do not allow distinguishing on when people enter self-employment and people may combine income from self-employment with e.g. employee salaries.
After 2010	Binary variable coded 1 if income observed after 2010 to capture possible differences caused by changes in the registration of incomes.
Household-position	6 categories of person's position in the household measured at year of settlement: (1) Child in household; (2) single-person household (ref.); (3) partner in couple without children; (4) partner in couple with children; (5) single parent; (6) other. Entered as a set of dummy variables.
Age	Age at labor market entry in years.
Female	Binary variable coded 1 if female.
National origin	12 national-origin groups: (1) Iraq (ref.) ; (2) (former) Soviet Union; (3) Somalia; (4) Afghanistan; (5) (former) Yugoslavia; (6) Iran; (7) (former) Sudan; (8) Sierra Leone; (9) Angola; (10) Syria; (11) China; (12) Turkey. Note that the Soviet-Union and Yugoslavia also include their present day equivalents. Entered as a set of dummy variables.
National unemployment rate	Yearly national unemployment rate in percentages, time-varying.

Notes: All information stems from individual-level administrative register data, except yearly national unemployment rates which stem from official statistics.

Table A5.2. Results of multilevel growth models regressing income on characteristics of the initial neighborhood with all estimates (table 5.2 in full).

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7
Fixed-part							
Intercept	1349.487	1043.348	953.376	920.439	930.106	931.877	927.791
	(68.23)**	(43.83)**	(30.40)**	(17.73)**	(17.72)**	(17.75)**	(17.17)**
Time		104.059	190.496	256.837	256.681	255.395	259.030
		(18.79)**	(13.26)**	(13.45)**	(13.43)**	(13.36)**	(12.97)**
$Time^2$			-11.057	-17.696	-17.687	-17.536	-17.577
			(-8.00)**	(-7.38)**	(-7.38)**	(-7.30)**	(-7.13)**
% working among co-ethnics munic.a					-.807	-2.458	-2.270
					(-.57)	(-1.20)	(-1.11)
*% working among co-ethnics munic.a*Time*						1.104	.808
						(1.24)	(.91)
*% working among co-ethnics munic.a*Time2*						-.115	-.098
						(-1.35)	(-1.15)
% working among native neighb.[a]					6.119	8.358	7.794
					(1.78)	(1.54)	(1.44)
*% working among native neighb.a *Time*						-2.032	-1.924
						(-.83)	(-.79)
*% working among native neighb.a *Time2*						.295	.252
						(1.26)	(1.08)
% co-ethnics in munic.[a]					-46.370	12.149	5.135
					(-.57)	(.11)	(.04)

*% co-ethnics in munic.a*Time*			-48.911	-45.089
			(-.96)	(-.89)
*% co-ethnics in munic.a*Time2*			6.395	5.790
			(1.23)	(1.11)
Whether any co-ethnics munic.[a]		.080	75.518	66.111
		(.00)	(.35)	(.30)
Whether any co-ethnics munic.a Time*			-10.389	-10.391
			(-.10)	(-.10)
*Whether any co- ethnics munic.a*Time2*			-6.100	-6.086
			(-.54)	(-.54)
Covariates				
Years outside LMb				-1.393
				(-.05)
*Years outside LMb*Time*				-32.872
				(-2.30)*
*Years outside LMb*Time2*				1.425
				(1.02)
Self-employment				-423.572
				(-6.53)**
*Self-employment*Time*				38.354
				(3.07)**
Age 1st entered LMc	12.781	12.761	12.904	12.662
	(3.56)**	(3.56)**	(3.59)**	(3.54)**
*Age 1st entered LMc*Time*	-4.351	-4.366	-4.416	-4.700
	(-5.43)**	(-5.45)**	(-5.48)**	(-5.89)**

Table A5.2 continued

Female	-271.706	-270.263	-269.852	-270.979
	(-5.29)**	(-5.26)**	(-5.25)**	(-5.32)**
After 2010	-25.863	-26.329	-28.968	4.335
	(-.49)	(-.49)	(-.54)	(.08)
*After 2010*Time*	-87.421	-87.127	-85.332	-77.134
	(-3.65)**	(-3.64)**	(-3.56)**	(-3.14)**
*After 2010*Time2*	10.595	10.578	10.364	9.351
	(3.91)**	(3.90)**	(3.82)**	(3.38)**
National unemployment rateb	-71.521	-71.585	-71.909	-71.383
	(-7.11)**	(-7.11)**	(-7.15)**	(-7.11)**
National origin (ref. Iraq)				
Afghanistan	58.004	49.471	49.288	78.143
	(1.03)	(.87)	(.86)	(1.38)
Former Yugoslavia	167.199	199.265	196.437	173.542
	(2.19)*	(2.36)*	(2.33)*	(2.08)*
Former Soviet Union	320.364	329.314	328.851	331.465
	(3.76)**	(3.79)**	(3.79)**	(3.86)**
Somalia	60.483	43.152	42.052	2.144
	(.89)	(.61)	(.60)	(.03)
Angola	28.765	29.301	28.779	-.617
	(.17)	(.18)	(.17)	(-.00)
Iran	201.868	209.176	208.602	230.281
	(2.18)*	(2.23)*	(2.22)*	(2.48)*

Sierra Leone				205.434 (1.97)*	206.989 (1.92)	205.778 (1.91)	187.457 (1.76)
(former) Sudan				250.889 (2.36)*	231.455 (2.13)*	231.287 (2.13)*	217.158 (2.02)*
China				-457.301 (-1.75)	-414.592 (-1.57)	-411.355 (-1.56)	-406.822 (-1.56)
Syria				198.326 (1.52)	191.358 (1.45)	189.313 (1.43)	203.277 (1.55)
Turkey				134.699 (.39)	240.817 (.63)	231.381 (.60)	228.084 (.60)
Household-position (ref. Single)							
Child in household				143.065 (.79)	131.868 (.73)	131.485 (.73)	149.073 (.84)
Partner in couple wo/kid(s)				-22.081 (-.29)	-31.424 (-.41)	-31.445 (-.41)	-8.759 (-.12)
Partner in couple w/kid(s)				-64.138 (-1.24)	-80.871 (-1.55)	-80.878 (-1.55)	-47.413 (-.91)
Single parent				-26.793 (-.26)	-45.187 (-.44)	-46.593 (-.46)	-25.858 (-.26)
Other				289.516 (1.59)	287.837 (1.58)	288.269 (1.58)	293.168 (1.63)
Random-part							
$SD_{residual}$	1212.954 (1203.59)**	1054.138 (1102.38)**	927.378 (1017.83)**	922.860 (1017.32)**	922.846 (1017.32)**	922.852 (1017.31)**	920.675 (1017.27)**
$SD_{intercept}$	874.152 (384.39)**	1016.865 (352.93)**	1437.712 (417.13)**	1425.254 (414.84)**	1424.156 (414.57)**	1423.510 (414.51)**	1423.139 (414.76)**

Table A5.2 continued

SD_{time}		184.588 (208.74)**	557.097 (306.06)**	553.426 (306.11)**	553.644 (306.15)**	553.029 (305.82)**	553.854 (306.65)**
SD_{time2}			45.302 (149.83)**	45.245 (150.75)**	45.271 (150.81)**	45.166 (150.36)**	45.136 (150.74)**
Cor(time, intercept)		-.511 (-19.63)**	-.796 (-38.55)**	-.804 (-38.86)**	-.804 (-38.85)**	-.804 (-38.83)**	-.809 (-39.19)**
Cor(time, $time^2$)			-.962 (-43.17)**	-.963 (-43.42)**	-.963 (-43.45)**	-.963 (-43.31)**	-.965 (-43.29)**
Cor(intercept, $time^2$)			.768 (27.30)**	.777 (27.46)**	.776 (27.45)**	.776 (27.42)**	.781 (27.57)**
Deviance	288,612.7	286,259.6	284,563.9	284,305.0	284,300.8	284,294.0	284,164.9
Parameters	3	6	10	33	37	45	50
AIC	288,618.7	286,271.6	284,583.9	284,371.0	284,374.8	284,384.0	284,264.9
BIC	288,641.8	286,318.0	284,661.1	284,625.9	284,660.6	284,731.6	284,651.1
$R^2_{intercept}$				.017	.019	.020	.020
R^2_{time}				.013	.012	.015	.012
R^2_{time2}				.003	.001	.006	.007
$R^2_{residual}$		.245	.415	.421	.421	.421	.424
N(person-years)	16,723						
N(individuals)	2,845						

Notes: [a]= Centered at mean of year of settlement; [b]= centered at grand-mean; [c]= centered at individual-level mean. *= p<0.05, **= p<0.005 (two-sided). Z-statistic between parentheses for fixed-part, standard error between parentheses for random-part.

Table A5.3. Results from multilevel growth curve models regressing income on characteristics of initial neighborhood and length of stay in initial municipality (table 5.3 in full).

	Model 8	Model 9a	Model 9b	Model 9c
Fixed-part				
Intercept	952.142	952.559	950.618	952.949
	(14.85)**	(14.84)**	(14.84)**	(14.86)**
Time	253.369	252.888	254.264	253.140
	(10.68)**	(10.65)**	(10.71)**	(10.66)**
$Time^2$	-17.368	-17.260	-17.452	-17.357
	(-6.24)**	(-6.20)**	(-6.27)**	(-6.23)**
% working among co-ethnics munic.[a]	-2.331	.054	-2.330	-2.340
	(-1.14)	(.02)	(-1.14)	(-1.14)
*% working among co-ethnics munic.[a]*Time*	.781	-.188	.794	.784
	(.88)	(-.18)	(.89)	(.88)
% working among co-ethnics munic.[a]$Time^2$*	-.096	-.006	-.097	-.097
	(-1.12)	(-.06)	(-1.14)	(-1.13)
% working among native neighb.[a]	7.913	7.836	-1.663	7.759
	(1.46)	(1.44)	(-.25)	(1.43)
*% working among native neighb.[a] *Time*	-1.895	-1.804	1.469	-1.806
	(-.77)	(-.74)	(.51)	(-.74)
*% working among native neighb.[a] *$Time^2$*	.251	.243	.042	.245
	(1.07)	(1.04)	(.15)	(1.05)
% co-ethnics in munic.[a]	.219	-8.965	-.965	28.575
	(.00)	(-.08)	(-.01)	(.23)
*% co-ethnics in munic.[a]*Time*	-45.263	-38.686	-46.979	-51.757
	(-.89)	(-.76)	(-.92)	(-.88)

Table A5.3 continued

*% co-ethnics in munic.[a]*Time²*	5.770	5.283	5.943	5.066
	(1.11)	(1.01)	(1.14)	(.79)
Whether any co-ethnics munic.[a]	65.713	64.346	72.953	103.507
	(.30)	(.30)	(.33)	(.39)
*Whether any co-ethnics munic.[a]*Time*	-8.914	-8.904	-9.963	-32.416
	(-.08)	(-.08)	(-.09)	(-.27)
*Whether any co- ethnics munic.[a]*Time²*	-6.159	-6.188	-6.153	-3.671
	(-.54)	(-.55)	(-.54)	(-.29)
Nr. months initial munic./12[b]	6.923	6.622	6.464	7.073
	(.61)	(.58)	(.57)	(.62)
*Nr. months initial munic./12[b]*Time*	-.095	.020	.114	-.201
	(-.02)	(.00)	(.03)	(-.05)
*Nr. months initial munic./12[b]*Time²*	-.027	-.038	-.047	-.009
	(-.07)	(-.10)	(-.13)	(-.02)
Nr. months initial munic./12[b]% working among co-ethnics munic.[a]*		1.310		
		(2.13)*		
Nr. months initial munic./12[b]% working among co-ethnics munic.[a]*Time*		-.781		
		(-3.40)**		
Nr. months initial munic./12[b]% working among co-ethnics munic.[a]*Time²*		.068		
		(3.33)**		
Nr. months initial munic./12[b]% working among native neighb.[a]*			-4.344	
			(-2.68)*	
Nr. months initial munic./12[b]% working among native neighb.[a]*Time*			1.164	
			(1.96)	

Nr. months initial munic./12[b]% working among native neighb.[a]*Time²*			-.089 (-1.70)	
Nr. months initial munic./12[b]% co-ethnics in munic.[a]*				24.293 (.72)
Nr. months initial munic./12[b]% co-ethnics in munic.[a]*Time*				-8.992 (-.68)
Nr. months initial munic./12[b]% co-ethnics in munic.[a]*Time²*				.966 (.76)
*Nr. months initial munic./12[b]*Whether any co-ethnics munic.[a]*				25.964 (.36)
*Nr. months initial munic./12[b]*Whether any co-ethnics munic.[a]*Time*				-25.119 (-.87)
*Nr. months initial munic./12[b]*Whether any co-ethnics munic[a]*Time²*				2.785 (1.01)
Covariates				
Years outside LM[b]	-4.564 (-.15)	-1.562 (-.05)	-7.007 (-.22)	-3.826 (-.12)
*Years outside LM[b]*Time*	-32.734 (-2.24)*	-34.021 (-2.33)*	-31.729 (-2.17)*	-33.287 (-2.28)*
*Years outside LM[b]*Time²*	1.427 (1.01)	1.555 (1.10)	1.318 (.93)	1.489 (1.05)
Self-employment	-425.357 (-6.55)**	-427.420 (-6.58)**	-423.606 (-6.52)**	-424.549 (-6.54)**
*Self-employment*Time*	38.721 (3.09)**	39.104 (3.12)**	38.488 (3.08)**	38.600 (3.09)**

Table A5.3 continued

Age 1st entered LM[c]	11.941	11.969	11.844	11.851
	(3.22)**	(3.23)**	(3.20)**	(3.20)**
*Age 1st entered LMc*Time*	-4.668	-4.666	-4.661	-4.656
	(-5.71)**	(-5.71)**	(-5.70)**	(-5.69)**
Female	-277.231	-274.860	-274.968	-275.819
	(-5.40)**	(-5.35)**	(-5.36)**	(-5.37)**
After 2010	-3.362	-11.841	-2.766	-4.008
	(-.06)	(-.20)	(-.05)	(-.07)
After 2010*Time	-75.790	-71.707	-76.163	-75.536
	(-2.75)*	(-2.60)*	(-2.76)*	(-2.74)*
*After 2010*Time2*	9.311	8.917	9.346	9.287
	(3.03)**	(2.90)**	(3.04)**	(3.02)**
National unemployment rate[b]	-71.424	-71.110	-71.235	-71.494
	(-7.11)**	(-7.08)**	(-7.09)**	(-7.12)**
National origin (ref. Iraq)				
Afghanistan	68.991	67.686	68.004	68.547
	(1.20)	(1.18)	(1.19)	(1.20)
Former Yugoslavia	332.395	333.563	327.218	332.128
	(3.87)**	(3.88)**	(3.81)**	(3.87)**

Former Soviet Union	176.444	174.898	171.949	175.961
	(2.11)*	(2.09)*	(2.06)*	(2.10)*
Somalia	-.936	1.641	-.559	-4.035
	(-.01)	(.02)	(-.01)	(-.06)
Angola	-.676	2.056	-1.288	-.749
	(-.00)	(.01)	(-.01)	(-.00)
Iran	230.813	231.997	226.925	229.973
	(2.49)*	(2.50)*	(2.45)*	(2.48)*
Sierra Leone	183.003	184.098	183.915	178.947
	(1.72)	(1.73)	(1.73)	(1.68)
(former) Sudan	211.026	212.616	211.852	207.245
	(1.96)	(1.97)*	(1.97)*	(1.92)
China	-400.782	-399.217	-407.830	-392.824
	(-1.54)	(-1.53)	(-1.56)	(-1.50)
Syria	204.342	202.712	203.634	202.766
	(1.56)	(1.55)	(1.56)	(1.55)
Turkey	241.293	226.204	254.003	216.450
	(.63)	(.59)	(.67)	(.56)
Household-position (ref. single)				
Child in household	144.954	140.097	141.148	142.236
	(.81)	(.78)	(.79)	(.80)
Partner in couple wo/kid(s)	-9.070	-8.780	-13.607	-10.203
	(-.12)	(-.12)	(-.18)	(-.13)

Table A5.3 continued

Partner in couple w/kid(s)	-50.792	-48.811	-50.809	-51.074
	(-.98)	(-.94)	(-.98)	(-.98)
Single parent	-28.171	-27.596	-29.265	-28.091
	(-.28)	(-.27)	(-.29)	(-.28)
Other	300.571	299.852	303.167	299.185
	(1.67)	(1.66)	(1.68)	(1.66)
Random-part				
$SD_{residual}$	45.130	45.209	45.132	45.119
	(150.69)**	(150.75)**	(150.65)**	(150.65)**
$SD_{intercept}$	553.842	554.570	553.814	553.876
	(306.61)**	(306.67)**	(306.46)**	(306.56)**
SD_{time}	1423.116	1423.653	1421.282	1422.999
	(414.73)**	(414.81)**	(414.32)**	(414.60)**
SD_{time2}	-.809	-.809	-.809	-.809
	(-39.19)**	(-39.21)**	(-39.16)**	(-39.18)**
Cor(time, intercept)	-.965	-.965	-.965	-.965
	(-43.23)**	(-43.31)**	(-43.23)**	(-43.17)**
Cor(time, $time^2$)	.781	.781	.781	.782
	(27.56)**	(27.59)**	(27.55)**	(27.54)**
Cor(intercept, $time^2$)	920.668	920.050	920.612	920.708
	(1017.21)**	(1016.92)**	(1017.18)**	(1017.13)**

Deviance	284,164.0	284,152.0	284,156.4	284,162.0
Nr parameters	53	56	56	59
AIC	284,270.0	284,264.0	284,268.4	284,280.0
BIC	284,679.4	284,696.6	284,700.9	284,735.8
$R^2_{intercept}$	.020	.019	.023	.020
R^2_{time}	.012	.009	.012	.012
R^2_{time2}	.008	.004	.008	.008
$R^2_{residual}$	.424	.425	.424	.424
N(person-years)	16,723			
N(individuals)	2,845			

Notes: [a]= Centered at mean of year of settlement; [b]= centered at grand-mean; [c]= centered at individual-level mean. *= p<.05, **= p<.005 (two-sided). Z-statistic between parentheses for fixed-part, standard error between parentheses for random-part.

Table A5.4. Main results of multilevel growth models regressing income on characteristics of the initial neighborhood (income sample).

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7
Fixed-part							
Intercept	*1246.974* (*49.95*)**	*985.072* (*28.46*)**	*956.766* (*25.78*)**	*920.610* (*13.93*)**	*927.766* (*13.87*)**	*926.192* (*13.84*)**	*926.808* (*13.31*)**
Time		*105.182* (*12.68*)**	*137.053* (*7.91*)**	*233.656* (*5.21*)**	*233.336* (*5.20*)**	*234.435* (*5.23*)**	*232.486* (*5.05*)**
Time^2			*-4.419* (*-2.10*)*	*-17.571* (*-1.79*)	*-17.565* (*-1.79*)	*-17.900* (*-1.82*)	*-17.668* (*-1.78*)
Income among co-ethnics munic.[a]					*-.002* (*-.74*)	*-.006* (*-1.58*)	*-.006* (*-1.67*)
*Income among co-ethnics munic.[a]*Time*						*.003* (*2.04*)*	*.003* (*1.92*)
*Income among co-ethnics munic.[a]*Time^2*						*-.000* (*-2.04*)*	*-.000* (*-1.92*)
Income among natives neigh.[a]					*.004* (*.55*)	*.008* (*.82*)	*.009* (*.89*)
Income among natives neigh.[a]*Time						*-.007* (*-1.43*)	*-.007* (*-1.51*)
*Income among natives neigh.[a]*Time^2*						*.001* (*1.89*)	*.001* (*1.91*)
% co-ethnics in munic.[a]					*-93.840* (*-.84*)	*-56.798* (*-.42*)	*-46.589* (*-.34*)

*% co-ethnics in munic.[a]*Time*			*-23.757* *(-.43)*	*-26.764* *(-.48)*
*% co-ethnics in munic.[a]*Time²*			*1.983* *(.28)*	*2.365* *(.34)*
Whether any co-ethnics munic.[a]		*-38.983* *(-.20)*	*-12.838* *(-.05)*	*-24.887* *(-.10)*
*Whether any co-ethnics munic.[a]*Time*			*34.830* *(.23)*	*40.554* *(.27)*
*Whether any co-ethnics munic.[a]*Time²*			*-13.327* *(-.55)*	*-11.432* *(-.47)*
Covariates				
Years outside LM[b]				*11.018* *(.18)*
*Years outside LM[b]*Time*				*-42.020* *(-1.37)*
*Years outside LMt[b]*Time²*				*.907* *(.26)*
Self-employment				*-385.596* *(-3.67)***
*Self-employment*Time*				*52.197* *(2.39)**
Age 1st entered LM[c]	*17.623* *(3.58)***	*17.791* *(3.61)***	*17.783* *(3.60)***	*17.998* *(3.65)***

Table A5.4 continued

*Age 1st entered LM[c]*Time*	*-4.775* *(-4.18)***	*-4.780* *(-4.19)***	*-4.801* *(-4.18)***	*-5.019* *(-4.41)***
Female	*-220.148* *(-2.98)***	*-217.991* *(-2.95)***	*-220.691* *(-2.98)***	*-221.826* *(-3.02)***
After 2010	*-82.709* *(-1.16)*	*-81.601* *(-1.15)*	*-81.831* *(-1.15)*	*-51.295* *(-.71)*
*After 2010*Time*	*-69.801* *(-1.39)*	*-69.714* *(-1.39)*	*-69.340* *(-1.38)*	*-54.464* *(-1.07)*
*After 2010*Time²*	*11.780* *(1.18)*	*11.770* *(1.18)*	*11.760* *(1.18)*	*9.984* *(.99)*
National unemployment rate[b]	*-66.053* *(-3.91)***	*-65.987* *(-3.91)***	*-67.025* *(-3.97)***	*-69.615* *(-4.12)***
National origin (ref. Iraq)				
Afghanistan	*182.503* *(1.92)*	*167.190* *(1.74)*	*166.728* *(1.74)*	*173.416* *(1.82)*
Former Yugoslavia	*147.143* *(1.17)*	*178.653* *(1.39)*	*185.647* *(1.44)*	*167.834* *(1.31)*
Former Soviet Union	*377.174* *(3.65)***	*380.177* *(3.67)***	*382.340* *(3.69)***	*388.804* *(3.79)***
Somalia	*117.430* *(1.58)*	*96.212* *(1.23)*	*97.314* *(1.24)*	*74.320* *(.95)*

Angola	*4.689* *(.02)*	*-16.728* *(-.08)*	*-19.983* *(-.09)*	*-44.470* *(-.20)*
Iran	*221.886* *(1.71)*	*218.490* *(1.67)*	*223.041* *(1.70)*	*242.053* *(1.87)*
Sierra Leone	*46.980* *(.26)*	*17.358* *(.09)*	*20.330* *(.11)*	*31.012* *(.17)*
(former) Sudan	*386.269* *(2.26)**	*352.646* *(2.02)**	*349.975* *(2.00)**	*376.935* *(2.17)**
China	*-442.999* *(-1.69)*	*-423.630* *(-1.61)*	*-422.909* *(-1.60)*	*-441.709* *(-1.69)*
Syria	*198.234* *(1.15)*	*168.127* *(.96)*	*164.049* *(.94)*	*174.485* *(1.01)*
Turkey	*-40.893* *(-.07)*	*371.715* *(.49)*	*369.089* *(.49)*	*287.525* *(.38)*
Household-position (ref. single)				
Child in household	*347.723* *(1.32)*	*371.496* *(1.41)*	*369.190* *(1.40)*	*412.851* *(1.57)*
Partner in couple wo/kid(s)	*-42.673* *(-.41)*	*-45.323* *(-.43)*	*-45.769* *(-.44)*	*-17.410* *(-.17)*
Partner in couple w/kid(s)	*-125.930* *(-1.82)*	*-130.633* *(-1.88)*	*-129.747* *(-1.86)*	*-112.864* *(-1.63)*
Single parent	*76.592* *(.55)*	*68.211* *(.49)*	*70.309* *(.50)*	*94.092* *(.67)*
Other	*257.071* *(1.16)*	*259.153* *(1.17)*	*262.642* *(1.19)*	*250.274* *(1.14)*

Table A5.4 continued

Random-part							
$SD_{residual}$	*1428.942 (897.12)***	*1263.187 (819.12)***	*1262.990 (819.07)***	*1260.501 (819.54)***	*1260.519 (819.54)***	*1259.149 (818.96)***	*1256.929 (818.77)***
$SD_{intercept}$	*753.224 (242.04)***	*1120.958 (269.92)***	*1117.599 (268.89)***	*1095.308 (264.00)***	*1095.260 (263.93)***	*1097.532 (264.42)***	*1096.866 (264.43)***
SD_{time}		*188.898 (147.36)***	*188.589 (146.94)***	*183.307 (143.84)***	*183.304 (143.91)***	*184.431 (144.33)***	*182.612 (144.05)***
Cor(time, intercept)		*-.666 (-18.83)***	*-.662 (-18.61)***	*-.665 (-18.22)***	*-.666 (-18.23)***	*-.667 (-18.34)***	*-.680 (-18.59)***
Deviance	*155,163.7*	*154,255.2*	*154,250.8*	*154,148.6*	*154,147.0*	*154,138.7*	*154,084.3*
Parameters	*3*	*6*	*7*	*30*	*34*	*42*	*47*
AIC	*155,169.7*	*154,267.2*	*154,264.8*	*154,208.6*	*154,215.0*	*154,222.7*	*154,178.3*
BIC	*155,191.0*	*154,309.7*	*154,314.4*	*154,421.2*	*154,456.0*	*154,520.5*	*154,511.5*
$R^2_{intercept}$			*.006*	*.045*	*.045*	*.041*	*.043*
R^2_{time}			*.003*	*.058*	*.058*	*.047*	*.065*
$R^2_{residual}$		*.219*	*.219*	*.222*	*.222*	*.224*	*.226*
N(person-years)	*8,853*						
N(individuals)	*1,730*						

Notes: [a]= Centered at mean of year of settlement; [b]= centered at grand-mean; [c]= centered at individual-level mean. *= p<.05, **= p<.005 (two-sided). Z-statistic between parentheses for fixed-part, standard error between parentheses for random-part.

Table A5.5. Results from multilevel growth curve models regressing income on characteristics of initial neighborhood and length of stay in initial municipality (income sample).

Fixed-part	Model 8	Model 9a	Model 9b	Model 9c	Model 9d
Intercept	951.743 (11.33)**	951.453 (11.32)**	952.090 (11.34)**	957.276 (11.39)**	956.606 (11.38)**
Time	226.402 (4.39)**	226.129 (4.38)**	225.809 (4.38)**	224.780 (4.36)**	225.437 (4.37)**
Time^2	-17.577 (-1.69)	-17.629 (-1.70)	-17.497 (-1.69)	-17.514 (-1.69)	-17.631 (-1.70)
Income among co-ethnics munic.[a]	-.006 (-1.68)	-.002 (-.58)	-.006 (-1.72)	-.006 (-1.63)	-.006 (-1.62)
*Income among co-ethnics munic.[a]*Time*	.003 (1.88)	.002 (.83)	.003 (1.90)	.003 (1.88)	.003 (1.88)
*Income among co-ethnics munic.[a]*Time^2*	-.000 (-1.89)	-.000 (-.80)	-.000 (-1.90)	-.000 (-1.89)	-.000 (-1.90)
Income among natives neigh.[a]	.008 (.85)	.010 (1.02)	-.002 (-.14)	.008 (.85)	.008 (.83)
*Income among natives neigh.[a]*Time*	-.007 (-1.51)	-.008 (-1.65)	-.004 (-.59)	-.007 (-1.50)	-.007 (-1.50)
*Income among natives neigh.[a]*Time^2*	.001 (1.92)	.001 (1.99)*	.001 (1.27)	.001 (1.92)	.001 (1.92)
% co-ethnics in munic.[a]	-52.115 (-.38)	-41.911 (-.31)	-59.810 (-.44)	21.518 (.15)	26.131 (.18)

Table A5.5 continued

*% co-ethnics in munic.[a]*Time*	-27.884 (-.50)	-32.479 (-.59)	-27.634 (-.50)	-71.004 (-.92)	-74.664 (-.97)
*% co-ethnics in munic.[a]*Time²*	2.475 (.36)	2.788 (.40)	2.412 (.35)	3.056 (.26)	3.504 (.30)
Whether any co-ethnics munic.[a]	-23.816 (-.09)	-9.890 (-.04)	-22.090 (-.09)	-34.552 (-.14)	75.974 (.23)
*Whether any co-ethnics munic.[a]*Time*	44.242 (.29)	36.551 (.24)	46.040 (.30)	48.794 (.32)	43.862 (.18)
*Whether any co-ethnics munic.[a]*Time²*	-12.011 (-.50)	-11.413 (-.47)	-12.265 (-.51)	-13.271 (-.54)	-19.646 (-.47)
Nr. months initial munic./12[b]	9.346 (.55)	10.058 (.59)	10.211 (.60)	10.463 (.61)	10.482 (.61)
*Nr. months initial munic./12[b]*Time*	-.005 (-.00)	.089 (.01)	-.442 (-.07)	-.055 (-.01)	.192 (.03)
*Nr. months initial munic./12[b]*Time²*	-.074 (-.12)	-.115 (-.18)	-.032 (-.05)	-.069 (-.11)	-.127 (-.19)
*Nr. months initial munic./12[b]*Income among co-ethnics munic.[a]*		.003 (2.23)*			
*Nr. months initial munic./12[b]*Income among co-ethnics munic.[a]*Time*		-.001 (-2.61)*			
*Nr. months initial munic./12[b]*Income among co-ethnics munic.[a]*Time²*		.000 (2.03)*			
*Nr. months initial munic./12[b]*Income among natives neigh.[a]*			-.005 (-1.34)		

*Nr. months initial munic./12[b]*Income among natives neigh.[a]*Time*			.001 (.95)		
*Nr. months initial munic./12[b]*Income among natives neigh.[a]*Time²*			-.000 (-.88)		
Nr. months initial munic./12[b]% co-ethnics in munic.[a]*				64.194 (1.45)	68.171 (1.53)
Nr. months initial munic./12[b]% co-ethnics in munic.[a]*Time*				-7.847 (-.44)	-8.507 (-.47)
Nr. months initial munic./12[b]% co-ethnics in munic.[a]*-Time²*				*.631* *(.31)*	*.646* *(.32)*
*Nr. months initial munic./12[b]*Whether any co-ethnics munic.[a]*					*63.033* *(.59)*
*Nr. months initial munic./12[b]*Whether any co-ethnics munic.[a]*Time*					*-.759* *(-.01)*
*Nr. months initial munic./12[b]*Whether any co-ethnics munic.[a]*Time²*					*-2.164* *(-.22)*
Covariates					
Years outside LM[b]	*-4.394* *(-.11)*	*-5.552* *(-.14)*	*-7.660* *(-.19)*	*-5.053* *(-.12)*	*-5.183* *(-.13)*
*Years outside LMb*Time*	*-34.362* *(-3.39)***	*-33.882* *(-3.34)***	*-33.956* *(-3.34)***	*-34.109* *(-3.36)***	*-34.001* *(-3.35)***
*Years outside LMb*Time2*	*-387.518* *(-3.68)***	*-383.281* *(-3.64)***	*-383.505* *(-3.64)***	*-388.541* *(-3.69)***	*-388.988* *(-3.69)***
Self-employment	*52.597* *(2.40)**	*51.573* *(2.36)**	*52.170* *(2.38)**	*52.881* *(2.41)**	*52.999* *(2.42)**

Table A5.5 continued

*Self-employment*Time*	*17.116* *(3.33)***	*17.124* *(3.34)***	*16.864* *(3.28)***	*16.655* *(3.24)***	*16.586* *(3.23)***
Age 1st entered LM[c]	*-4.974* *(-4.20)***	*-5.010* *(-4.24)***	*-4.946* *(-4.18)***	*-4.940* *(-4.17)***	*-4.930* *(-4.16)***
*Age 1st entered LM[c]*Time*	*-229.550* *(-3.10)***	*-229.199* *(-3.10)***	*-228.851* *(-3.09)***	*-227.815* *(-3.08)***	*-226.674* *(-3.07)***
After 2010	*-62.080* *(-.78)*	*-71.218* *(-.89)*	*-59.766* *(-.75)*	*-65.559* *(-.82)*	*-65.247* *(-.81)*
*After 2010*Time*	*-53.680* *(-.95)*	*-50.067* *(-.89)*	*-54.059* *(-.96)*	*-52.217* *(-.93)*	*-51.738* *(-.92)*
*After 2010*Time²*	*10.167* *(.96)*	*9.971* *(.94)*	*10.180* *(.96)*	*9.985* *(.94)*	*9.841* *(.93)*

Table A5.5 continued

National unemployment rate[b]	*-70.149* *(-4.15)***	*-69.657* *(-4.11)***	*-70.236* *(-4.15)***	*-70.299* *(-4.15)***	*-70.338* *(-4.15)***
National origin (ref. Iraq)					
Afghanistan	*167.961* *(1.76)*	*170.074* *(1.78)*	*166.701* *(1.75)*	*165.084* *(1.73)*	*166.170* *(1.74)*
Former Yugoslavia	*169.929* *(1.33)*	*169.020* *(1.32)*	*171.197* *(1.34)*	*165.198* *(1.29)*	*164.989* *(1.29)*
Former Soviet Union	*388.113* *(3.78)***	*388.784* *(3.79)***	*383.986* *(3.74)***	*387.803* *(3.78)***	*388.279* *(3.79)***

Somalia	*70.185* *(.90)*	*68.560* *(.87)*	*69.318* *(.89)*	*61.770* *(.79)*	*62.712* *(.80)*
Angola	*-48.300* *(-.22)*	*-42.125* *(-.19)*	*-55.817* *(-.25)*	*-61.524* *(-.28)*	*-60.753* *(-.28)*
Iran	*240.698* *(1.86)*	*243.545* *(1.88)*	*240.425* *(1.85)*	*238.053* *(1.84)*	*237.462* *(1.83)*
Sierra Leone	*23.845* *(.13)*	*28.762* *(.16)*	*17.756* *(.10)*	*13.007* *(.07)*	*16.523* *(.09)*
(former) Sudan	*371.999* *(2.14)**	*367.047* *(2.12)**	*370.752* *(2.14)**	*360.927* *(2.08)**	*361.992* *(2.09)**
China	*-438.053* *(-1.68)*	*-434.863* *(-1.66)*	*-437.930* *(-1.68)*	*-416.016* *(-1.59)*	*-414.552* *(-1.59)*
Syria	*172.770* *(1.00)*	*184.290* *(1.06)*	*168.792* *(.97)*	*164.920* *(.95)*	*156.511* *(.90)*
Turkey	*300.301* *(.40)*	*281.529* *(.38)*	*349.181* *(.47)*	*-94.940* *(-.12)*	*-110.802* *(-.14)*
Household-position (ref. single)					
Child in household	*405.408* *(1.55)*	*397.559* *(1.52)*	*410.617* *(1.57)*	*405.670* *(1.55)*	*404.957* *(1.55)*
Partner in couple wo/kid(s)	*-18.325* *(-.18)*	*-17.899* *(-.17)*	*-17.411* *(-.17)*	*-22.767* *(-.22)*	*-23.232* *(-.22)*
Partner in couple w/kid(s)	*-117.000* *(-1.69)*	*-114.339* *(-1.65)*	*-115.232* *(-1.66)*	*-118.336* *(-1.71)*	*-118.869* *(-1.71)*
Single parent	*93.197* *(.67)*	*92.400* *(.66)*	*100.820* *(.72)*	*94.625* *(.68)*	*94.895* *(.68)*

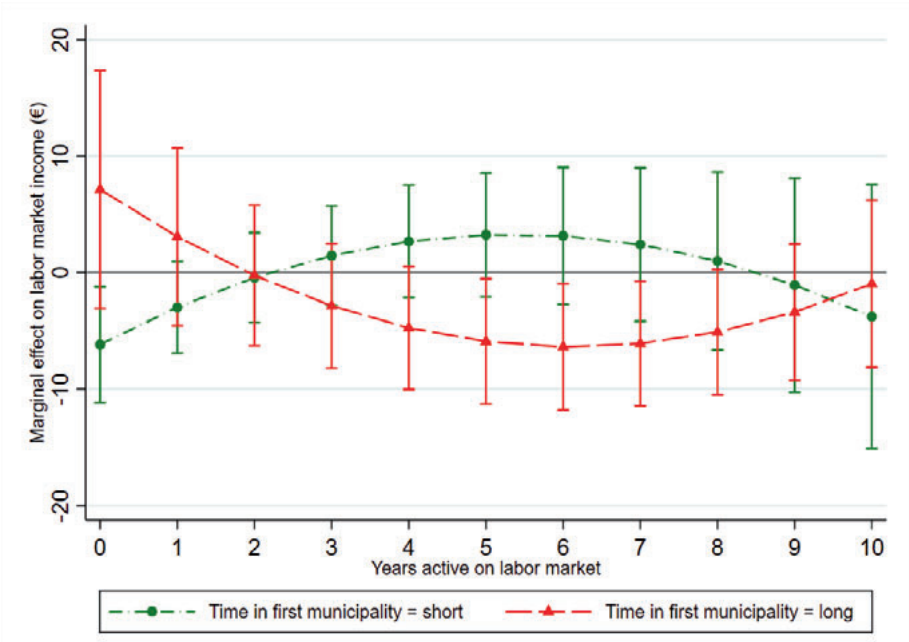
Table A5.5 continued

Other	*257.840* *(1.17)*	*255.780* *(1.16)*	*265.400* *(1.21)*	*257.353* *(1.17)*	*261.671* *(1.19)*
Random-part					
$SD_{residual}$	*1096.466* *(264.35)***	*1095.934* *(264.21)***	*1095.600* *(264.17)***	*1095.375* *(264.09)***	*1095.416* *(264.01)***
$SD_{intercept}$	*182.595* *(143.87)***	*182.032* *(143.50)***	*182.357* *(143.62)***	*182.426* *(143.76)***	*182.324* *(143.57)***
SD_{time}	*-.680* *(-18.60)***	*-.679* *(-18.53)***	*-.680* *(-18.58)***	*-.681* *(-18.59)***	*-.681* *(-18.59)***
Cor(time, intercept)	*1256.964* *(818.57)***	*1256.526* *(818.59)***	*1256.987* *(818.56)***	*1257.065* *(818.52)***	*1257.050* *(818.44)***
Deviance	*154,083.7*	*154,076.5*	*154,081.4*	*154,080.7*	*154,080.1*
Nr parameters	*49*	*52*	*52*	*52*	*55*
AIC	*154,181.7*	*154,180.5*	*154,185.4*	*154,184.7*	*154,190.1*
BIC	*154,529.0*	*154,549.1*	*154,554.0*	*154,553.3*	*154,579.9*
$R^2_{intercept}$	*.043*	*.044*	*.045*	*.045*	*.045*
R^2_{time}	*.066*	*.071*	*.068*	*.067*	*.068*
$R^2_{residual}$	*.226*	*.227*	*.226*	*.226*	*.226*
N(person-years)	*8,853*				
N(individuals)	*1,730*				

Notes: [a]= Centered at mean of year of settlement; [b]= centered at grand-mean; [c]= centered at individual-level mean. *= p<.05, **= p<.005 (two-sided). Z-statistic between parentheses for fixed-part, standard error between parentheses for random-part.

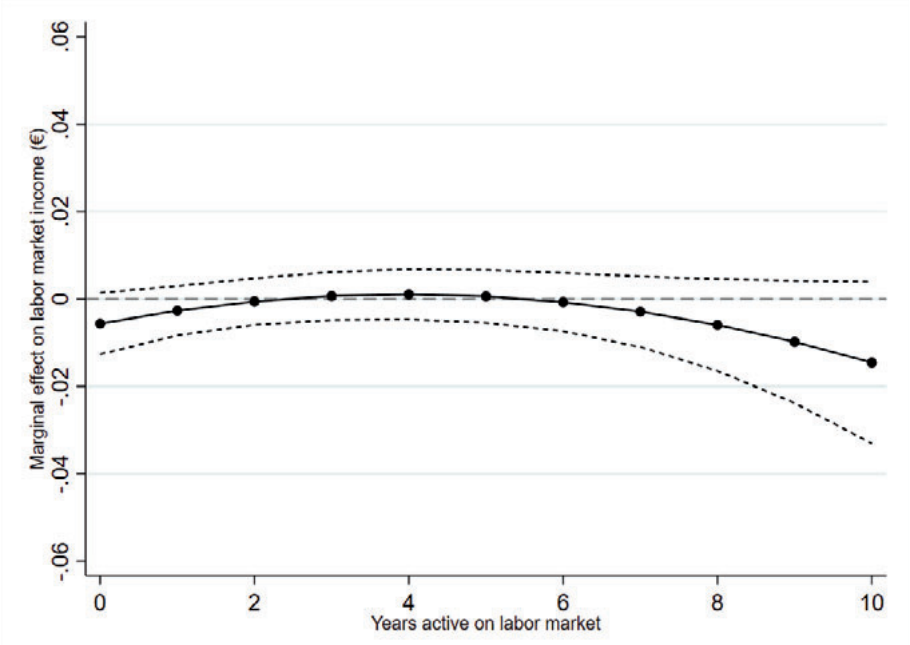
5.6.2 Additional figures

Figure A5.1. Average marginal effect of employment rate among co-ethnics in initial municipality on refugees' income, by time in first municipality and years active on the labor market.



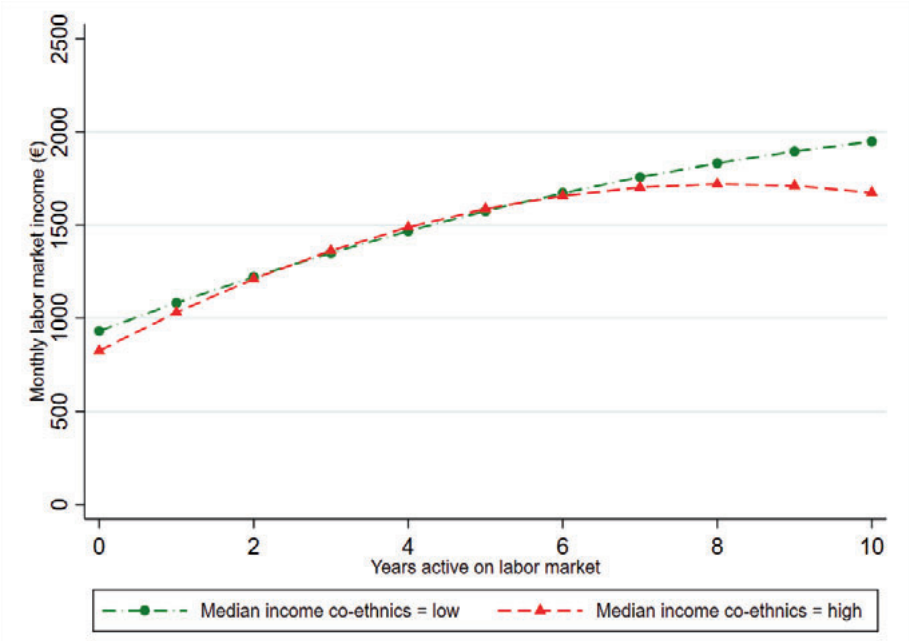
Notes: Time in first municipality is set at short (10th percentile) and long (90th percentile). Based on model 9a in table 5.3.

Figure A5.2. Average marginal effect of median income among co-ethnics in initial municipality on refugees' income, by years active on the labor market.



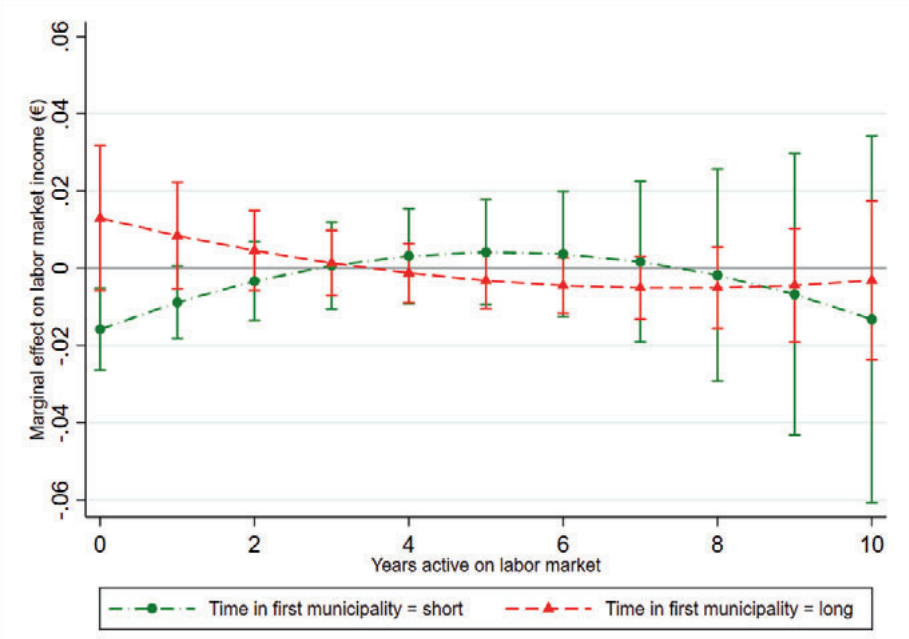
Notes: Based on model 6 in table A5.2. See figure A5.3 for predicted income trajectories at different values of median income among co-ethnics.

Figure A5.3. Estimated income trajectories by median income among co-ethnics in initial municipality and years active on the labor market.



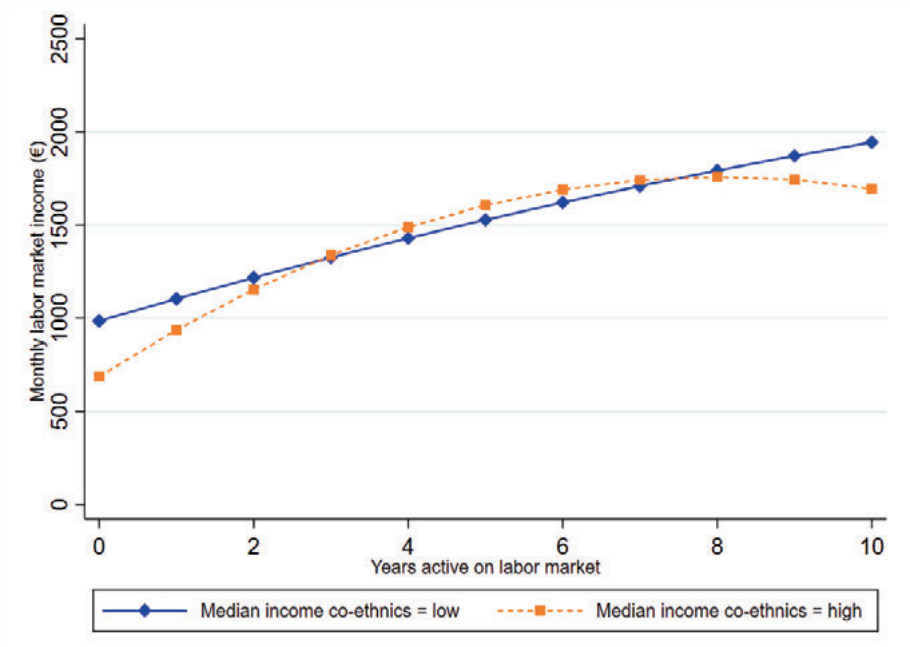
Notes: Based on model 6 in table A5.2. Low means 10th percentile and high means 90th percentile. See figure A5.2 for changes in the average marginal effect of median income among co-ethnics.

Figure A5.4. Average marginal effect of median income among co-ethnics in initial municipality on refugees' income, by time spent in first municipality and years active on the labor market.



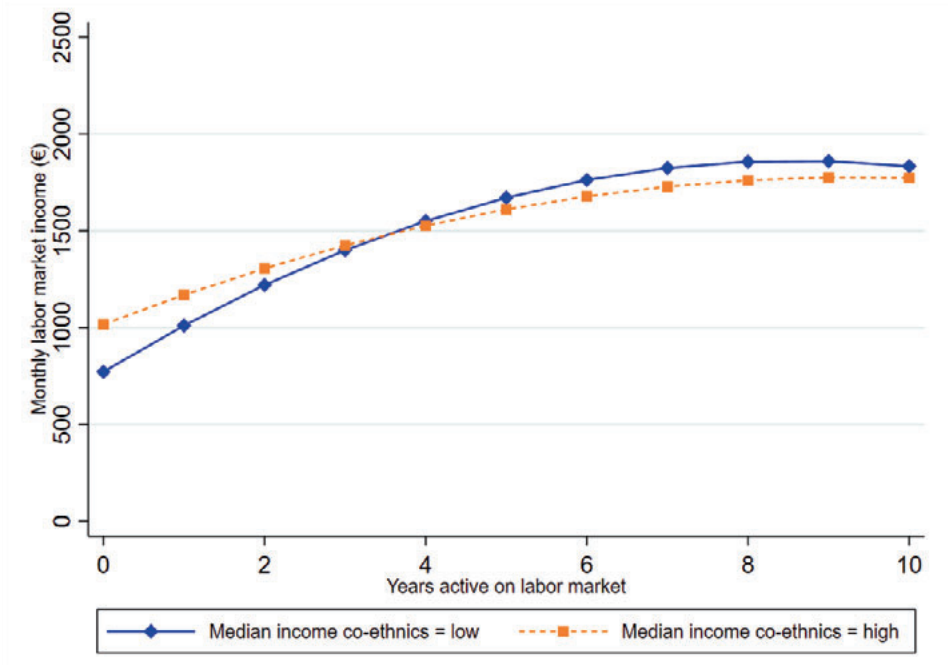
Notes: Based on model 9a in table A5.3. Short and long refer to 10th and 90th percentile, respectively. See figure A5.5a and A5.5b for differences in income trajectories.

Figure A5.5a. Estimated income trajectories by median income among co-ethnics and years active on the labor market when time in initial municipality is short.



Notes: Based on model 9a in table A5.3. Low/short and high/long mean 10th and 90th percentile, respectively.

Figure A5.5b. Estimated income trajectories by median income among co-ethnics and years active on the labor market when time in initial municipality is long.



Notes: Based on model 9a in table A5.3. Low/short and high/long mean 10th and 90th percentile, respectively.

Nederlandse Samenvatting

Inleiding

In deze dissertatie staat de rol van de kenmerken van iemands sociale netwerk bij uitkeringsafhankelijkheid en (duurzame) uitstroom uit een uitkering centraal. Met uitkeringsafhankelijkheid bedoelen we dat iemands belangrijkste inkomensbron een werkloosheids-, bijstands- of arbeidsongeschiktheidsuitkering is. Kenmerken van sociale netwerken refereren naar de persoonlijke relaties die iemand heeft en de eigenschappen van de mensen waarmee iemand verbonden is. De focus van deze dissertatie ligt op twee sociale maatschappelijke ontwikkelingen: de verzorgingsstaat en de instroom van migranten naar onder andere Nederland en andere Europese landen.

Vanuit een smal perspectief bezien biedt de verzorgingsstaat zijn inwoners een vorm van sociale zekerheid door het geven van uitkeringen. Uitkeringen zijn bedoeld om financiële steun te bieden aan mensen die bijvoorbeeld vanwege werkloosheid, een slechte gezondheid of ouderdom niet in staat zijn om voor zichzelf te zorgen. Een breder perspectief op de verzorgingsstaat neemt niet alleen mee of beleid inkomenszekerheid biedt, maar ook of beleid ingezet wordt om onder andere uitkeringsafhankelijkheid tegen te gaan, de volksgezondheid te verbeteren of deelname op de arbeidsmarkt of in de samenleving te bevorderen. Zo kan breed beleid beogen om te voorkomen dat mensen gedwongen worden om een uitkering aan te vragen en mensen hulp te bieden om terug te keren op de arbeidsmarkt. Wetenschappelijk onderzoek onderstreept het belang van dit soort beleid door aan te tonen hoe nadelig de effecten zijn van uitkeringsafhankelijkheid voor bijvoorbeeld de menselijke gezondheid.

In de afgelopen jaren is volop aandacht geweest voor de instroom van (niet-westerse) migranten en asielzoekers. De lage arbeidsdeelname en hoge uitkeringsafhankelijkheid onder deze groepen worden vaak gezien als belangrijke beleidsuitdagingen. In Nederland ontving ongeveer veertien procent van de mensen met een niet-westerse migratieachtergrond tussen de 15 en 65 jaar een bijstandsuitkering in 2015; voor mensen zonder een migratieachtergrond was dit twee procent. Vanuit een breder perspectief op de verzorgingsstaat is het van belang beleid te ontwerpen dat zulke verschillen weet te verkleinen.

Kortom, in deze dissertatie richten we ons op de rol van sociale netwerken op uitkeringsafhankelijkheid, uitstroom uit de bijstand en inkomensgroei na het verlaten van de bijstand. Er zijn meerdere redenen waarom het interessant is om op de rol van sociale netwerken in te zoomen. Ten eerste worden sociale netwerken vaak genoemd als een verklaring van uitkeringsafhankelijkheid en langdurige armoede. Bepaalde kenmerken van iemands sociale netwerk kunnen zorgen voor (langdurige) uitkeringsafhankelijkheid, bijvoorbeeld omdat contact met mensen met een baan ontbreekt. Daarnaast

Figure A5.5b. Estimated income trajectories by median income among co-ethnics and years active on the labor market when time in initial municipality is long.

wordt regelmatig gesuggereerd dat als er (veel) mensen met een uitkering in iemands netwerk zitten, iemand zelf ook een grotere kans heeft om afhankelijk te worden van een uitkering. Dit is een belangrijke assumptie in het zogeheten *'welfare cultures'*-argument.

Een tweede reden waarom het interessant is om verder in te zoomen op de rol van sociale netwerken is dat het niet per se onwenselijk is dat iemand mensen kent die een uitkering ontvangen. Een gebrek aan informatie is bijvoorbeeld een belangrijke reden waarom veel mensen die recht hebben op een uitkering er toch geen aanvragen. In de wetenschappelijke literatuur wordt dit *'non-take-up'* of niet-gebruik van inkomensvoorzieningen genoemd. Het gevolg hiervan is dat mensen geen gebruik maken van de regelingen waarop zij wel recht hebben. De sociale zekerheidsvoorzieningen van de verzorgingsstaat worden op deze manier dus niet optimaal benut.

Een derde reden is dat iemands sociale netwerk gezien kan worden als een belangrijke hulpbron op de arbeidsmarkt. Sociale netwerken helpen bij het vinden en behouden van een baan en voorkomen daarmee dat iemand genoodzaakt wordt om een uitkering aan te vragen. Dit wordt ook wel sociaal kapitaal genoemd. Vaak wordt gedacht dat sociale hulpbronnen vooral van belang zijn voor mensen met een migratieachtergrond. Mensen met een migratieachtergrond moeten zich verhouden tot een arbeidsmarkt of samenleving die vaak in verschillende opzichten verschilt met die van hun (voor)ouders. Het hebben van sociale contacten met mensen zonder migratieachtergrond zou daarom kunnen helpen bij, onder andere, het opdoen van kennis over de arbeidsmarkt en biedt mensen met een migratieachtergrond meer mogelijkheden om hun taalvaardigheden te verbeteren.

Wetenschappelijke bijdragen

In deze dissertatie staan twee onderzoeksvragen centraal: (1) In hoeverre hebben kenmerken van iemands sociale netwerk invloed op uitkeringsafhankelijkheid? En (2) hoe beïnvloeden kenmerken van sociale netwerken uitkeringsafhankelijkheid en inkomensgroei na uitkeringsontvangst onder mensen met een migratieachtergrond?

In de wetenschappelijke literatuur naar de invloed van sociale netwerken op uitkeringsafhankelijkheid bestaat tot op heden veel belangstelling voor de vraag hoe sociale netwerken bijdragen aan uitkeringsafhankelijkheid. In deze dissertatie dragen we bij aan de bestaande literatuur door niet alleen te onderzoeken hoe sociale netwerken de kans op uitkeringsafhankelijkheid vergroten maar (tegelijkertijd) mogelijk ook verkleinen.

Er wordt daarbij gekeken naar verschillende onderzoekspopulaties. Ten eerste richten wij ons op de invloed van sociale netwerken op uitkeringsafhankelijkheid in de algemene populatie (hoofdstuk 2). Vervolgens wordt ingegaan op mogelijke verschillen

in uitkeringsafhankelijkheid tussen mensen zonder en met een migratieachtergrond (hoofdstuk 3). Ten slotte richten we ons op statushouders (hoofdstuk 4 en 5). Op deze manier verkrijgen wij meer inzicht in de rol van kenmerken van sociale netwerken op uitkeringsafhankelijkheid onder de algemene bevolking maar ook onder mensen met diverse migratieachtergronden – eerste en tweede generatie immigranten en statushouders. In recente studies is onderzocht waarom mensen met een (niet-westerse) migratieachtergrond in het algemeen een grotere afstand tot de arbeidsmarkt hebben dan mensen zonder migratieachtergrond, en welke rol sociale netwerken daarbij spelen. Maar tot dusverre is er nog geen onderzoek gedaan waarin expliciet uitkeringsafhankelijkheid wordt bestudeerd. In deze dissertatie wordt getracht dit kennisgat op te vullen.

Ter beantwoording van de centrale onderzoeksvragen in deze dissertatie vormen statushouders om meerdere redenen een interessante onderzoeksgroep. Ten eerste komen statushouders aan in een nieuw land waarin zij minder sociale betrekkingen hebben dan bijvoorbeeld familie- of arbeidsmigranten. Ten tweede is het interessant om te focussen op statushouders omdat het overgrote deel van deze groep vrijwel direct instroomt in de bijstand nadat zij in Nederland gehuisvest zijn, met name omdat maar een enkeling direct een baan weet te vinden. Hun arbeidsmarktdeelname verbetert weliswaar naarmate zij langer in Nederland verblijven maar loopt sterk achter op die van mensen zonder een migratieachtergrond. Ten derde zoomen wij in op statushouders die zich vestigden in een periode waarin zij op willekeurige wijze werden gehuisvest in Nederland. Door deze willekeurige plaatsing van statushouders is sprake van een quasi-experimentele onderzoeksopzet waarin (on-)gemeten kenmerken van statushouders niet gecorreleerd zijn met kenmerken van hun eerste huisvesting. Een belangrijk voordeel hiervan is dat met deze opzet nauwkeuriger onderzoek gedaan kan worden naar het causale effect van buurtkenmerken dan met de gegevens en/of methoden die we in hoofdstuk twee en drie gebruiken. In het buitenland, voornamelijk in Scandinavische

landen, is dit onderzoeksdesign al enkele keren toegepast. Een belangrijke bijdrage van deze dissertatie is om dit design nu ook in de Nederlandse context toe te passen.

Samenvatting per hoofdstuk

Deze dissertatie bestaat uit vier empirische hoofdstukken waarmee we de centrale onderzoeksvragen op verschillende manieren trachten te beantwoorden. Per hoofdstuk worden de specifieke onderzoeksaanpak en de belangrijkste bevindingen samengevat.

Hoofdstuk 2

In hoofdstuk twee staan twee vragen centraal. Allereerst wordt er gekeken naar de vraag welke kenmerken van sociale netwerken de kans op uitkeringsafhankelijkheid kunnen *vergroten* en welke kenmerken die kans *verlagen*. Om dit te onderzoeken bestuderen we, enerzijds, of het aandeel mensen met een uitkering in het netwerk samenhangt met een verhoogde kans op uitkeringsafhankelijkheid en, anderzijds, onderzoeken we of de hoeveelheid sociale hulpbronnen in het kernnetwerk – zoals het aantal personen met een baan in het kernnetwerk – samenhangt met een verlaagde kans op uitkeringsafhankelijkheid. Ten tweede bestuderen we of de effecten van netwerkkenmerken op uitkeringsafhankelijkheid afhangen van de mate van kwetsbaarheid op de arbeidsmarkt. De veronderstelling is dat mensen kwetsbaarder zijn op de arbeidsmarkt naarmate zij minder gezond zijn of een lager inkomen hebben en/of niet-standaard werk uitvoeren (bijvoorbeeld tijdelijk werk).

In dit hoofdstuk maken we gebruik van longitudinale enquêtegegevens uit het LISS-panel (*'Longitudinal Internet Survey for the Social Sciences'*) die verrijkt zijn met administratieve gegevens van het CBS. Het LISS-panel bevat tijdreeksdata van een representatieve steekproef van de Nederlandse bevolking. Deze data worden met een logistische regressiemodel geanalyseerd. De resultaten laten zien dat de concentratie uitkeringsontvangers in de buurt de kans op uitkeringsafhankelijkheid vergroot. Tegelijkertijd blijkt dat een betere toegang tot sociale hulpbronnen – met name het hebben van meer werkende personen in het kernnetwerk – de kans op uitkeringsafhankelijkheid verlaagt. Dit duidt erop dat de invloed van iemands sociale netwerk op uitkeringsafhankelijkheid zowel negatief als positief kan zijn en dat beide effecten tegelijkertijd plaats kunnen vinden. Om het totale effect van sociale netwerken op uitkeringsafhankelijkheid goed te begrijpen moet dus rekening gehouden worden met deze beide effecten.

Verder blijkt uit de analyse dat netwerkeffecten niet afhankelijk zijn van de mate waarin iemand kwetsbaar is op de arbeidsmarkt. We vinden daarmee geen empirische

ondersteuning voor onze verwachting dat met name mensen met een grotere kwetsbaarheid op de arbeidsmarkt actiever hun sociale netwerk raadplegen voor advies en informatie. Dit houdt concreet in dat het netwerkeffect voor mensen met een kwetsbare arbeidsmarktpositie dan sterker zou zijn dan voor mensen met een niet-kwetsbare positie. Netwerkeffecten blijken juist even sterk te zijn, onafhankelijk of iemand minder gezond is of een laag inkomen heeft en/of niet/standaardwerk uitoefent (bijv. tijdelijk werk).

Hoofdstuk 3

In hoofdstuk drie zoomen we in op de vraag of de effecten van sociale netwerkkenmerken op de kans op uitkeringsafhankelijkheid verschillen tussen mensen zónder en mét een migratieachtergrond. Specifiek richten we ons op twee netwerkkenmerken: (1) inter- en intraetnisch sociaal contact en (2) het aandeel mensen met een uitkering van dezelfde etnische herkomst in de buurt. Daarnaast bekijken we of en in welke mate deze netwerkkenmerken de etnische verschillen in uitkeringsafhankelijkheid tussen mensen zonder en met een migratieachtergrond kunnen verklaren.

Om deze vragen te bestuderen maken we gebruik van de *Survey Integratie Minderheden* (SIM) uit 2006 en 2011. Deze enquêtegegevens bevatten een steekproef die representatief is voor mensen zonder en met een Turkse, Marokkaanse, Antilliaanse en Surinaamse migratieachtergrond in Nederland. We hebben deze enquêtegegevens verrijkt met longitudinale administratieve gegevens van het CBS. Dit betekent dat we de cross-sectionele surveygegevens van SIM hebben gekoppeld aan longitudinale administratieve gegevens van het CBS. Daardoor kunnen we de surveygegevens gebruiken om toekomstige uitkeringsafhankelijkheid – gemeten met de administratieve registers – te verklaren. Dit zorgt voor een sterker onderzoeksdesign dan een design waarin wij of alleen de SIM-data of alleen de administratieve gegevens analyseren. De data worden geanalyseerd met een 'linear probability model' (lineaire kansmodel).

Uit de analyses blijkt dat sociaal contact met mensen met een Nederlandse achtergrond samenhangt met een kleinere kans op uitkeringsafhankelijkheid. Dit geldt voor zowel individuen zonder als individuen met een migratieachtergrond. Dit betekent dat interetnisch contact voor mensen met een migratieachtergrond en intraetnisch contact voor mensen zonder een migratieachtergrond samenhangt met een kleinere kans op uitkeringsafhankelijkheid. Deze bevindingen ondersteunen het idee dat sociaal contact met mensen zonder migratieachtergrond een belangrijke sociale hulpbron kan zijn voor mensen ongeacht hun migratieachtergrond.

Verder laten de analyses zien dat het aandeel mensen van dezelfde etnische herkomst met een uitkering in de buurt samenhangt met een hogere kans op uitkeringsafhankelijkheid. Het blijkt dat dit effect even groot is voor mensen met en zonder een

migratieachtergrond. Deze bevinding is daarmee niet in lijn met het idee dat netwerkmechanismes een belangrijkere rol spelen voor mensen met een migratieachtergrond dan voor mensen zonder een migratieachtergrond.

Ten slotte is onderzocht of verschillen in uitkeringsafhankelijkheid tussen mensen met en zonder migratieachtergrond door sociale netwerken verklaard kunnen worden. Uit de resultaten komt naar voren dat etnische verschillen in uitkeringsafhankelijkheid voornamelijk verklaard worden door het aandeel mensen van dezelfde etnische herkomst met een uitkering in de buurt. Ondanks dat sociaal contact een effect heeft op uitkeringsafhankelijkheid, verklaart het niet het etnische verschil in de kans op uitkeringsafhankelijkheid. De bevindingen suggereren verder dat sociaaleconomische kenmerken – zoals opleiding, leeftijd en huishoudenskenmerken – een groot deel van de resterende etnische verschillen kan verklaren.

Hoofdstuk 4

In hoofdstuk vier verleggen wij de aandacht naar statushouders en richten ons op het belang van de eerste buurt waarin zij geplaatst worden na aankomst in Nederland. Specifiek maken we in dit hoofdstuk en in hoofdstuk vijf gebruik van de willekeurige plaatsing van statushouders binnen Nederlandse gemeentes tussen 1999-2009. Door gebruik te maken van een quasi-experimenteel design is het in deze hoofstukken beter mogelijk om causaliteit aannemelijk te maken dan met de data en aanpak in hoofdstuk twee en drie. In dit hoofdstuk komen twee vragen aan bod. Allereerst bestuderen wij of de kans voor statushouders om de transitie van de bijstand naar werk te maken afhangt van de volgende buurtkenmerken[78] : het aandeel werkenden en het mediaaninkomen in de buurt van eerste huisvestiging. We onderzoeken zowel de effecten van het aandeel werkenden en het mediaaninkomen van mensen met dezelfde etnische herkomst als de statushouder in de buurt als de effecten van het aandeel werkenden en het mediane inkomen van mensen zonder migratieachtergrond in de buurt. Ten tweede bestuderen we of de effecten van het aandeel werkenden en het mediane inkomen van mensen zonder migratieachtergrond in de buurt interacteren met het aandeel mensen in de buurt met dezelfde etnische afkomst als de statushouder. Het idee is dat statushouders meer beïnvloed worden door mensen zonder migratieachtergrond in hun directe leefomgeving wanneer het aandeel mensen met dezelfde etnische herkomst lager is.

Om deze vragen te onderzoeken maken we gebruik van longitudinale registerdata van het CBS. Deze gegevens stellen ons in staat om statushouders over tijd te volgen

78 De kenmerken van mensen met dezelfde herkomst als de statushouder worden op het gemeenteniveau gemeten. Dit is ook het geval voor hoofdstuk 5. In deze samenvatting gebruiken wij de term 'buurt' ook om de kenmerken gemeten op gemeenteniveau te omschrijven.

om te zien of ze de transitie van de bijstand naar de arbeidsmarkt maken. We passen een '*linear probability, descrete time model*' toe om de gegevens te analyseren. Uit de analyses blijkt dat de kans om van de bijstand naar werk te gaan groter is als het aandeel werkenden onder mensen met dezelfde etnische herkomst als de statushouder in de buurt groter is. Een vergelijkbaar effect vinden we voor het aandeel werkenden onder mensen zonder een migratieachtergrond in de buurt: des te hoger het aandeel werkenden zonder migratieachtergrond in de buurt is, des te groter de kans is om vanuit de bijstand werk te vinden. Hetzelfde geldt echter niet voor het mediane inkomen van mensen met dezelfde etnische herkomst als de statushouder in de buurt: voor statushouders neemt de kans op het vinden van werk niet toe wanneer het mediane inkomen onder mensen met dezelfde etnische herkomst in de buurt hoger ligt. Als gekeken wordt naar het effect van het mediane inkomen van mensen zonder een migratieachtergrond in de buurt vinden wij dat een hoger mediaan inkomen samenhangt met een kleinere kans voor statushouders om uit de bijstand te stromen. Kortom, de resultaten duiden erop dat statushouders beïnvloed worden door de omgeving waarin zij bij aankomst in Nederland geplaatst worden. Dit heeft dus mogelijk te maken met de beschikbare sociale hulpbronnen in deze omgeving.

Verder vinden we geen bewijs dat de effecten van het mediane inkomen of het aandeel werkenden onder mensen zonder een migratieachtergrond in de buurt afhangen van het aandeel mensen met dezelfde herkomst als de statushouders in de buurt. Wij vinden daarmee geen ondersteuning voor het idee dat statushouders de aanwezige mensen zonder migratieachtergrond in de buurt meer als een sociale hulpbron gaan zien wanneer het aandeel mensen met dezelfde etnische herkomst in de buurt lager is.

Hoofdstuk 5

In hoofdstuk vijf bouwen wij op twee manieren voort op het vorige hoofdstuk. Ten eerste richten wij ons op de invloed van buurtkenmerken van de plaats waarin statushouders na aankomst in Nederland eerst gehuisvest worden op hun latere inkomensontwikkeling. Specifiek kijken we naar de invloed van buurtkenmerken op hun eerste inkomen na intrede op de Nederlandse arbeidsmarkt én hun latere inkomensgroei. Dit hoofdstuk werpt een nieuw licht op de arbeidsmarktintegratie van statushouders door ook te kijken naar de langere termijnontwikkelingen. Op deze manier is het mogelijk om ook iets te zeggen of de uitstroom uit de bijstand duurzaam is. Ten tweede houden we rekening met de invloed van de verhuismobiliteit van statushouders en – meer specifiek - of statushouders na plaatsing in hun eerste woonlocatie in Nederland verhuizen naar een andere Nederlandse buurt. Statushouders hebben geen invloed op de plaats waar zij in Nederland voor het eerst gehuisvest worden maar mogen nadien wel ver-

huizen. Methodisch betekent dit dat (on)gemeten kenmerken van statushouders hun verhuisgedrag kunnen beïnvloeden waardoor het mogelijk lastiger wordt om causale effecten van buurtkenmerken vast te stellen.

Net als in het vorige hoofdstuk maken we hier gebruik van longitudinale administratieve gegevens van het CBS. Deze individuele data worden geanalyseerd met een *multilevel growth model* (multilevel groeimodellen). Uit het regressiemodel blijkt dat statushouders een inkomensgroei meemaken na hun intrede op de arbeidsmarkt. Echter, de resultaten laten zien dat deze groei niet beïnvloed wordt door de kenmerken van de buurt waarin statushouders voor het eerst gehuisvest werden. Deze kenmerken blijken daarnaast ook geen effect te hebben op het inkomensniveau van statushouders op het moment van arbeidsmarktintrede. Dit betekent dat de effecten die we in hoofdstuk vier vinden – zoals het effect van het aandeel werkenden onder mensen met dezelfde etnische herkomst in de buurt – niet gevonden worden in de latere fases van de loopbaan. Deze bevindingen zijn in lijn met het idee dat er grenzen zijn aan de voordelen van '*etnische enclaves*': mensen met dezelfde etnische herkomst bieden vooral hulp bij intrede op de arbeidsmarkt maar níet bij de verdere loopbaanontwikkeling.

De hoeveelheid tijd die statushouders doorbrengen in de eerste gemeente waarin ze geplaatst werden blijkt over het algemeen de grootte van de effecten van de buurtkenmerken van de eerste huisvesting niet te beïnvloeden. Dit spreekt het idee tegen dat een langere verblijfsduur samenhangt met een sterkere sociale integratie en, als gevolg daarvan, een grotere invloed van de kenmerken van de eerste gemeente waarin statushouders gehuisvest werden. Tegelijkertijd laten de resultaten zien dat statushouders die snel uit deze eerste gemeente in Nederland verhuizen het niet beter (of slechter) doen qua inkomensontwikkeling dan statushouders die daar langer verblijven.

Algemene conclusies

Op basis van de resultaten kunnen vier algemene conclusies getrokken worden. Ten eerste concluderen we dat sociale netwerken via twee onderliggende mechanismes invloed hebben op uitkeringsafhankelijkheid. Eén mechanisme is gerelateerd aan informatie of normen met betrekking tot het gebruik van uitkeringen. Dit mechanisme – gemeten met de hoeveelheid uitkeringsontvangers in de buurt– vergroot de kans op uitkeringsafhankelijkheid. Het andere mechanisme is gerelateerd aan informatie over (mogelijke kansen op) de arbeidsmarkt. Dit mechanisme – gemeten met de sociale hulpbronnen in het kernnetwerk – verkleint de kans op uitkeringsafhankelijkheid. In deze dissertatie leveren we bewijs dat beide mechanismes de kans op uitkeringsafhan-

kelijkheid beïnvloeden, ook wanneer beide mechanismes tegelijkertijd mee worden genomen in de analyse. Individuen lijken dus zowel gebruik te maken van de sociale hulpbronnen die aanwezig zijn in hun persoonlijke netwerken alsook beïnvloed te worden door de uitkeringsgerelateerde keuzes die anderen in hun netwerken maken. Op basis van onze bevindingen kunnen we niet vaststellen welk mechanisme – informatie of sociale normen gerelateerd aan uitkeringen – de belangrijkste rol speelt. Ook eerder onderzoek kon hier geen uitsluitsel over geven. Toekomstig onderzoek is dus nodig om hier meer inzicht in te krijgen. De resultaten van deze dissertatie laten dus zien dat studies naar uitkeringsafhankelijkheid oog moeten hebben voor zowel de beschikbare informatie alsook de heersende sociale normen binnen sociale netwerken. Het gaat daarbij om (1) informatie en sociale normen ten aanzien van het uitkeringsstelsel en het ontvangen van een uitkering; en (2) informatie en sociale normen gerelateerd aan werk en de arbeidsmarkt.

De tweede conclusie gaat over het belang van sociaal contact voor het verkrijgen van hulpbronnen. Voor zowel mensen zonder als met een migratieachtergrond blijkt dat sociaal contact met mensen zonder een migratieachtergrond samenhangt met een lagere kans op uitkeringsafhankelijkheid. Vergelijkbare effecten vinden we niet voor sociaal contact met mensen met een migratieachtergrond of, specifiek voor statushouders, voor contact met mensen met dezelfde etnische herkomst. Verder blijkt dat sociaal contact het verschil in uitkeringsafhankelijkheid tussen mensen zonder en met een migratieachtergrond *niet* kan verklaren. Dit zou kunnen betekenen dat met name sociale hulpbronnen die mensen zonder een migratieachtergrond kunnen bieden – zoals kennis over de samenleving en arbeidsmarktgerelateerde informatie – belangrijk zijn om uitkeringsafhankelijkheid te verklaren. Sociaal contact met mensen met dezelfde etnische herkomst heeft weinig invloed op uitkeringsafhankelijkheid. Opvallend genoeg vinden we echter ook niet dat sociaal contact met mensen zonder een migratieachtergrond de 'juiste' sociale hulpbronnen bieden om het verschil in uitkeringsafhankelijkheid tussen mensen zonder en met een migratieachtergrond te kunnen verklaren.

Ten derde laten we zien dat de sociaaleconomische kenmerken van mensen in het netwerk van belang zijn om uitkeringsafhankelijkheid te verklaren. In het bijzonder vinden wij dat de sociaaleconomische compositie van sociale netwerken invloed heeft op de kans op uitkeringsafhankelijkheid onder mensen met een migratieachtergrond, verschillen in uitkeringsafhankelijkheid tussen mensen zonder en met een migratieachtergrond gedeeltelijk kan verklaren en invloed heeft op de kans dat statushouders de transitie van de bijstand naar werk maken. Samen met de tweede conclusie wijzen deze resultaten uit dat theoretische concepten zoals *bridging* en *bonding* sociaal kapitaal – ofwel inter- en intraetnisch sociaal contact – die vaak worden aangehaald in

empirisch onderzoek, minder belangrijk zijn dan gedacht bij het verklaren van de kans op uitkeringsafhankelijkheid. Met andere woorden, om inzicht te krijgen in de sociale hulpbronnen van mensen met een migratieachtergrond is het mogelijk belangrijker om meer aandacht te besteden aan de sociaaleconomische kenmerken van mensen in het netwerk dan aan hun etnische- of migratieachtergrond. Dit wil daarmee niet zeggen dat sociale integratie in de vorm van sociaal contact met mensen zonder een migratieachtergrond in zijn geheel onbelangrijk is – bijvoorbeeld bij het leren van een nieuwe taal – maar dat dit soort contact niet het verschil in uitkeringsafhankelijkheid tussen mensen zonder en met een migratieachtergrond kan verklaren.

De vierde conclusie gaat over de invloed van de kenmerken van de buurt waarin statushouders geplaatst worden na hun aankomst in Nederland op hun integratie op de arbeidsmarkt. Uit de resultaten blijkt onder andere dat een hogere arbeidsdeelname onder buren met dezelfde etnische herkomst als de statushouder, en een hogere arbeidsdeelname onder buren zonder een migratieachtergrond de kans vergroot om de transitie van de bijstand naar werk te maken. Onze bevindingen impliceren dat zowel de sociale hulpbronnen van mensen met dezelfde herkomst als die van de statushouder alsook de sociale hulpbronnen van mensen zonder migratieachtergrond statushouders in de bijstand kunnen helpen bij het vinden van hun eerste baan, maar deze hulpbronnen niet helpen bij hun verdere loopbaanontwikkeling. Dit betekent dat deze hulpbronnen vooral belangrijk zijn bij de uitstroom uit de bijstand/intrede op de arbeidsmarkt maar niet zozeer voor langere termijn succes op de arbeidsmarkt.

In deze dissertatie hebben we buurtkenmerken gebruikt als indicatoren voor iemands sociale netwerk of omgeving. Tegelijkertijd kunnen deze indicatoren ook deels de effecten meenemen van onder andere de plaatselijke of regionale economische omstandigheden waaraan statushouders blootgesteld worden. De buurtindicatoren die we gebruiken – dus niet alleen die met betrekking tot de hoofdstukken over statushouders – zijn dus geen directe metingen van het sociale netwerk. We nemen niet direct waar met wie een persoon wel of geen relatie onderhoudt. Dit heeft twee specifieke gevolgen voor de interpretatie van de effecten hiervan: (1) buurtkenmerken kunnen maar één gedeelte van het sociale netwerk – buren – meten; en (2) we onderschatten het reële effect van sociale contacten in de buurt omdat het geschatte effect een gemiddelde is van buren die een persoon wel en niet kent. Over het algemeen trekken we de conclusie dat de omgeving van eerste huisvesting belangrijk is voor de intrede van

statushouders op de arbeidsmarkt en dat dit waarschijnlijk deels te maken heeft met hun sociale netwerken.

Beleidsimplicaties

De resultaten uit deze dissertatie hebben drie beleidsimplicaties. Ten eerste onderstrepen onze bevindingen dat het voor toekomstig beleid belangrijk is om statushouders te plaatsen in een omgeving waarin de arbeidsdeelname hoog is. Dit soort beleid kan statushouders helpen om de transitie naar de arbeidsmarkt te maken, maar helpt statushouders waarschijnlijk niet met hun verdere ontwikkeling op de arbeidsmarkt. Daarvoor lijkt ander beleid noodzakelijk te zijn.

Ten tweede blijkt uit onze bevindingen dat de kenmerken van het sociale netwerk van mensen zowel uitkeringsafhankelijkheid kunnen vergroten als verkleinen. Dit heeft mogelijk consequenties voor de schatting van de effecten van sociaal investeringsbeleid. Daarbij wordt uitgegaan van een *'social multiplier'*-effect: als beleid één persoon helpt bij het uitstromen uit een uitkering, dan kan deze persoon weer anderen in diens netwerk helpen om uit een uitkering te stromen. Tot nu toe wordt daarbij alleen rekening gehouden met netwerkmechanismes gerelateerd aan informatie en/of normen over het hebben van een uitkering. Dit kan tot een onderschatting van het *'social multiplier'*-effect leiden omdat, zoals wij in deze dissertatie hebben laten zien, ook netwerkmechanismes met betrekking tot informatie over (de kansen op) de arbeidsmarkt van belang zijn. Daardoor is het *'social multiplier'*-effect waarschijnlijk groter dan tot nu toe is gedacht.

De laatste beleidsimplicatie gaat over het relatieve belang van de kenmerken van sociale netwerken bij het verklaren van de in- en uitstroom uit een uitkering. Alles overziend blijkt dat de grootte van de netwerkeffecten even of minder groot zijn dan die van andere kenmerken zoals leeftijd, opleiding en huishoudenskenmerken. Ondanks dat de effecten beperkt zijn, kunnen sociale netwerken om andere redenen evengoed belangrijk zijn voor de levensuitkomsten van mensen: (1) sociale netwerken kunnen *'social multiplier'*-effecten in gang zetten; en (2) sociale netwerken hebben niet alleen effecten op uitkeringsafhankelijkheid maar ook op andere relevante aspecten van het leven van mensen. Het gaat dan bijvoorbeeld om gevoelens van verbondenheid met naasten, zelfredzaamheid, keuzevrijheid en hoe mensen denken over solidariteit en de bescherming die geboden wordt door de verzorgingsstaat. Dit soort aspecten beïnvloeden misschien niet direct of iemand een uitkering ontvangt maar zijn toch belangrijk voor de kwaliteit van het leven van mensen die afhankelijk zijn van een uitkering.

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Marcus H. Kristiansen was born in Oslo, Norway on December 17, 1991. He obtained his bachelor degree in "Social Sciences" at the Norwegian University of Science and Technology in Trondheim, Norway in 2013. In 2015, he completed his Research Master in "Sociology and Social Research" at Utrecht University (*cum laude*). Since September 2015, he has been working as a PhD-candidate at the Department of Sociology at Utrecht University and the Interuniversity Center of Social Science Theory and Methodology (ICS). He wrote his dissertation under the supervision of Prof. dr Cok Vrooman (UU | SCP) and Prof. dr Ineke Maas (UU | VU). In the winter of 2017-2018 he visited the Swedish Institute for Social Research (SOFI) at Stockholm University, were he was hosted by Prof. dr. Carina Mood.

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How do characteristics of social networks affect individuals' chances of relying on social, unemployment or disability benefits? How do these characteristics affect immigrants' benefit receipt and income development after benefit receipt? By combining insights from disparate yet related strands of research, this dissertation shed new light on these two questions. It does so by combining survey and administrative register data, and drawing on a natural experiment among refugees arriving to the Netherlands.

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