Empirical studies in labour and migration economics

Albrecht, S.

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This thesis consists of three empirical studies investigating topics in the fields of labour and migration economics, using a combination of administrative and survey data. The first study in labour economics seeks to answer the question of whether international differences in earnings inequality between skilled and (relatively) unskilled workers can be explained by differences in the relative supply of and demand for skilled and unskilled workers across countries. Both the second and third studies are placed in the field of migration economics, dealing with the topical issue of refugee migration. Specifically, they investigate the social and attitudinal effects of the reception of refugees on host communities. The second study employs a case study of a town in rural Australia to examine how a large influx of refugees to the township has impacted social capital among the native residents. The third study broadens the context by using refugee centre data from all of the Netherlands over a number of years to link exposure to refugees to changes in natives' attitudes to immigration. While the three studies differ (in parts) in the subfield, context and methodology of empirical microeconomics, a uniting factor is that they are motivated by important real-world problems, and that any conclusions drawn are based on the thorough analysis of a suitable dataset.

Sabina Albrecht holds a BA degree in Staatswissenschaften (economics, law and social sciences) from the University of Erfurt and an MPhil degree in Economics from the Tinbergen Institute. She wrote her PhD dissertation under the supervision of Hessel Oosterbeek and Erik Plug at the University of Amsterdam. Sabina is currently employed at Queensland University of Technology as a Postdoctoral Researcher in the area of education economics and behavioural economics.

Empirical Studies in Labour and Migration Economics Sabina Albrecht

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Sabina Albrecht

Empirical Studies in Labour and Migration Economics

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Empirical Studies in Labour and Migration Economics

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Promotiecommissie:

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Ten years ago, I was a high school graduate with no clear idea of what I should do with my life. At this time, I only knew that I wanted to learn more at a university, and I knew which fields did *not* interest me. Blessed with the freedom of choice and the emotional and financial support of my parents, I embarked on an exciting journey that eventually led me to become an economist. The social sciences fascinated me. I enrolled in a multidisciplinary programme at the University of Erfurt that combines economics with law and sociology/political science. At the end of it, I found the analytical perspective and quantitative methodology that economics had to offer most convincing, and I was impressed by the depth of topics covered by the modern-day economists. I realised that economics is truly a *social* science, as it is first and foremost concerned with the individuals' and the community's well-being. I also had learned about myself that I thoroughly enjoy academic work, despite the occasional slumps in motivation that any research student must experience with some regularity. Fast-forward to 2018. Having gone through the Tinbergen PhD programme, I have finally found out what interests me. This dissertation is a synthesis of it.

I would not have arrived at this point without the support of my supervisors, colleagues, friends and family, to all of whom I would like to express my warmest thanks.

My supervisor Hessel Oosterbeek was a source of inspiration from the first days of choosing a topic for my Master's thesis up to the last day of discussing the finishing touches on my latest work. I enjoyed bouncing off early ideas with you, Hessel, and exploring the potential for joint projects. Your critical thinking, the dedication to policy-relevant work and your calm, reflected approach to matters both academic and personal are values that I will carry with me for the rest of my life. My co-supervisor Erik Plug filled in with valuable advice whenever Hessel was in the Southern hemisphere. Erik, thanks for making me feel part of the UvA group right from the start, and for being a role model for that healthy mixture of 'fun and focus' that contributes to academic success and makes the job as a researcher so very enjoyable.

While my supervisors have helped me considerably in developing my own ideas and setting up interesting research, my co-authors David, Riccardo, Elena and Sigrid have taught me to be ambitious, to persevere, and to work as a team. The regular work-in-progress group meetings together with the formidable applied microeconometricians from the Vrije Universiteit have been the perfect environment to learn from a very talented group of people and to sharpen my own argument before presenting at international conferences. In that context I would like to thank Adam, Bas, Elisabeth, Lenny, Lydia, Mathilde, Nadine, Nienke, Nynke, Noemi, Paul, Pauline, Sandra, Sabien and Thomas for their helpful comments and the fun we had together at our social outings.

I spent most of my time on the sixth floor of the E-building at the University of Amsterdam, which wouldn't have been half as much fun without forays onto the seventh and third floor to share a coffee with my friends Andrej, Simin, Stephanie and many more. Silvia, Eszter, Laura and Martin infected me with their positive attitude when I started the PhD and created the friendliest work environment. The always smiling and helpful staff at both the UvA and the Tinbergen Institute, especially Robert, Wilma, Ester, Christina, Judith and Arianne, contributed no less to that environment.

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Sometimes, getting away from Amsterdam was the only way forward, and I am grateful to Hannah and Malte for recharging my batteries in various cities over the years. 'Going home' had the same effect. I am deeply indebted to my family: My parents had to make a bigger sacrifice by letting me go than I was aware of at times, and for that I appreciate their unwavering support for my academic ambitions all the more. My brothers Julian and Alexander were always interested in my research and provided emotional support whenever I needed it. Lastly, there is no one who has provided more encouragement and inspiration than David. The nearly six years of my PhD are also our history together, and while I am finishing this first chapter of my academic career, we have begun writing a new chapter of our growing family together.

Sabina Albrecht
Brisbane, 2018

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Chapter 1

Introduction

> It is a capital mistake to theorise before one has data. Insensibly one begins to twist facts to suit theories, instead of theories to suit facts.
>
> Sherlock Holmes in *A Scandal in Bohemia*
> by Arthur Conan Doyle

Economics is a social science, and as such concerned with the functioning of society and the wellbeing of its individuals. This thesis, like many others, is evidence of the fact that research in economics goes beyond studying markets and their actors. With hundreds of years of study, the economics science has developed views on a multitude of issues, comprising both macroeconomic and microeconomic perspectives and combining insights from related fields such as psychology, political science and sociology with the analytical rigour of mathematical models and statistical inference.

One subfield of economics in particular tries to build a bridge between the rigours of economic science and the realities of the problems that matter to society. That field, empirical microeconomics, is the one that I chose when pursuing my doctoral studies in economics. While many branches of economics start with an attempt to expand the theory and then later search for useful applications, at its essence, empirical microeconomics follows exactly the opposite approach. This is also the direction I have taken with the three chapters of this thesis: Each was motivated by an important, real-world problem that requires robust investigation. Consequently, the methodologies I employ in each chapter are not identical, but suited to the empirical question at hand. The uniting factor is that any conclusions drawn are based on the thorough analysis of a dataset.

Empirical economics is not without its challenges. Chief among them remains separating causality from correlation, a crucial element to coming to a conclusion that has the potential to guide and inform policy. While developments have been made in the area of identification, including the use of experiments, not all questions can be answered as easily as others. The toolkit of an empiricist is therefore as diverse as the subjects of her scrutiny. In the absence of true randomisation in the variable of interest, the empiricist can follow several routes to inference. One such route is to transform the predictions of a well-established theory into an empirical model, and test whether it matches the data at hand well. This is the route I took in Chapter 2. In Chapter 3, I make use of a so-called natural experiment that provides a setting in which there is a natural 'treated' and 'control' group, which I compare in their responses to an incentivised survey. On other occasions, advanced econometric techniques help to establish causality. Each applied micro-econometric method relies on one ore more identifying assumptions, which need to be thoroughly checked by the researcher. If they are difficult to test, demonstrating robustness of the results in a variety of ways is mandatory to support one's claim. This is the challenge I faced in Chapter 4.

Thesis overview

This thesis is based on three independent studies in the realms of labour and migration economics. The first study deals with a long-standing topic at the core of labour economics. The question "Where does economic inequality come from?" has concerned societies, researchers and policy makers for many decades, and it is currently more relevant to ask than ever. The second and third study contribute to our understanding of the integration of migrants (refugees) into host societies. If motivated by recent global events and debates, migration, too, is an age-old topic that has been scrutinised by the economics profession. The questions posed in this thesis add to the knowledge base by looking at the dimensions of social capital and attitudes in the host society, essentially lubricants to a successfully functioning, integrated society.

In Chapter 2, entitled **An international comparison of earnings inequality and skill distributions**, I enter the debate about whether international differences in earnings inequality between skill groups can be explained by the differences in the supply and demand for workers of different skill levels. The degree of earnings inequality between skill groups has a direct relation to overall earnings inequality, a topic that is as hotly debated in the public as in the academic sphere. Understanding why skill earnings inequality is higher in some countries than in others, as is for example the case in the United States or the United Kingdom compared to Scandinavian countries, may help policy makers in addressing problems in their own country. I make use of a dataset specifically collected with the intention to make 'skill' comparable across a wide range of countries. This dataset contains both classical proxies for skill, chiefly measures for years of education and work experience, but also direct measures of numeracy, literacy and computer skills of over 50,000 adults from 15 countries.

I draw on the canonical supply and demand model for skill, which relates net supply (i.e., supply minus demand) differences between workers of different skill groups to their earnings differences, and test the implied negative relationship empirically in the cross-country dataset. The results suggest that a sizeable proportion of the variation in skill earnings inequality across countries can be explained by this simple framework, which is particularly instructive in light of some arguments in the literature that dismiss the model outright for a cross-country comparison. Overall, the supply and demand framework explains around 30 percent of the international variation between skill groups' earnings, in spite of the fact that country-specific labour market institutions may work against this mechanism. A novel and interesting finding is that the relationship between earnings inequality and relative net supply is just as visible if it is based on a measure of computer skill (proficiency in using digital technology and communication tools for work purposes), which is a relatively recent dimension of skill that has not yet been studied with regards to how it contributes to earnings or, in relative terms, to earnings inequality.

Chapter 3, entitled **When refugees work: The social capital effects of resettlement on host communities**, leaves the domain of labour economics and shifts the focus of this dissertation to the timely issue of refugee migration. In what is joint work with David Smerdon, I examine the effect of refugee resettlement on social capital through a case study of a rural town in Australia. The resettlement was exogenous with respect to social indicators of the township and filled an unmet labour demand in the host community, which makes it an interesting case to look at. The former feature helps to establish a credible comparison between this town and other control towns that are similar in a variety of dimensions, except for the contact with refugees. The latter characteristic allows us to focus on the social impact of contact with refugees, without the confounding effects of increased labour market pressures or higher crime rates that are sometimes associated with refugee migration.

We combine trust data from an incentivised survey with repeated cross-sectional survey data from treatment and control towns to test conflicting theories of how migration affects social measures like trust and attitudes. Intriguingly, we find no evidence of negative effects on social capital from the resettlement on individuals in the host community. Residents, and in particular women in the treated town trusted refugees relatively more and showed significantly more favourable attitudes toward general refugee resettlement. A weighted synthetic control group analysis supports our findings. While our study only speaks about one specific case with its particular characteristics,we argue that the results of this case study need not be unique, but that further research is warranted into what makes resettlement successful.

Chapter 4, entitled **Exposure to refugees and attitudes to immigration**, continues the research on the effects of refugee resettlement on host communities, this time with a focus on attitudes. Together with my coauthors from Tilburg University, Riccardo Ghidoni, Elena Cettolin and Sigrid Suetens, I broaden the context by linking a detailed dataset of all refugee centres in the Netherlands, spanning a time frame of six years and including the recent 'refugee crisis', to a representative panel survey of individuals' attitudes towards immigration. Based on the location, dates of opening and occupancy numbers of refugee centres, we proxy different dimensions and intensities of exposure to refugees that individuals have experienced over time. Specifically, we investigate the duration of exposure, the number of refugees exposed to, and the degree of proximity to the individual for their effect on attitudes.

Distinguishing different dimensions of exposure and examining their impact individually, as well as in interaction with each other, is a novel contribution to the literature. In a difference-in-differences framework combined with individual fixed effects regressions, we find that exposure to small numbers of refugees has a significantly positive effect on attitudes if experienced for a long enough time. Exposure to large numbers of refugees significantly reduces attitudes, particularly with a short duration of exposure. Comparing the effects at the neighbourhood-level to the municipality-level shows that proximity matters: The effects dissipate for the whole municipality. Our estimates help to reconcile effects going in opposite directions that have been found in the literature to date, and provide useful insights for policy makers with regards to how best to organise the reception and distribution of asylum seekers.

Chapter 2

An international comparison of earnings inequality and skill distributions*

> The main force pushing toward reduction in inequality has always been the diffusion of knowledge and the diffusion of education.
>
> Thomas Piketty

2.1 Introduction

Countries differ in their degree of earnings inequality between skill groups. In some countries, such as the United States and the United Kingdom, the wage gap between higher-skilled and lower-skilled workers is much larger than in other countries. This chapter tests whether the observed variation stems from differences in supply of and demand for skill across countries. Using newly collected skill data from the OECD Programme for the International Assessment of Adult Competencies (PIAAC) on literacy, numeracy and computer skill, I apply the canonical supply and demand model for skill to a cross-section of 15 OECD countries.

Why do we care about skill earnings inequality, and why are supply and demand plausible candidates for an explanation? Skill earnings inequality has a direct relation to overall earnings inequality, a topic that has recently received much attention in the public debate as well as in the academic literature (Autor, 2014). The commonly cited causes for increasing levels of earnings inequality have disparate effects on workers of lower or higher skill. The revolution in information and communication technologies (ICT), for example, has shifted labour demand in favor of those who possess complementary skills. Similarly, off-shoring is

*This chapter is based on (Albrecht, 2017).

assumed to bear down on lower-skilled jobs. Both of these factors increase the pay gap between high- and low-skilled workers through a changed demand for skill. On the other hand, a generally higher-educated labour force affects the supply side of the market and works against increased wage dispersion (OECD, 2011). Factors like these vary in the extent to which they are at work in different countries –, so taken together, the supply of and demand for skill may constitute a useful tool for explaining differences in earnings inequality across countries.

The theory behind the canonical supply and demand model goes back to Tinbergen (1974, 1975) who coined the phrase 'race between technology and education', referring precisely to forces similar to the ICT revolution and an up-skilling of the labour force. While Tinbergen and much of the literature that followed suit were talking about the development of earnings inequality over time, Blau and Kahn (1996) and Leuven et al. (2004) applied his concept to differences in earnings inequality across countries. The two studies come to – at first glance – contradictory but plausible findings. Blau and Kahn (1996) reject the validity of a supply and demand model for skill in the international context and conclude that differences in institutions outweigh any influence of market forces on skill premia. Leuven et al. (2004), however, refute their argument by applying a measure of skill that is more suitable for comparisons across countries, obtained from one of PIAAC's predecessor studies. With data from the 1990s, they find that international differences in supply and demand for skill explain about a third of the differences in earnings inequality between countries. This provides a benchmark estimate and an important lesson for the current chapter: an accurate, internationally comparable measurement of skill is crucial for an analysis of skill earnings inequality across countries. Almost twenty years later, such new data including even further developed measures of cognitive skill that encompass the ability to successfully use a computer are now available and the subject of earnings inequality is as topical as ever.

The first studies to make use of the PIAAC data for analysing earnings and skill distributions are Hanushek et al. (2013), Paccagnella (2015) and Pena (2015). The data show that cognitive skills, as well as formal education, are rewarded differentially across OECD countries (Hanushek et al., 2013; Paccagnella, 2015).[1] Given these findings, a question that follows naturally is to what extent heterogeneous skill prices contribute to differences in earnings *inequality* across countries. To find an answer, Paccagnella (2015) and Pena (2015) decompose the gap in the 90/10-earnings differential between countries into an effect due to

[1]Hanushek et al. (2013) replace the traditional human capital measure of schooling by numeracy skill in a series of Mincer regressions. The estimated returns range from 12 to 28 percent for a one standard deviation increase in numeracy skill. Paccagnella (2015) complements the estimate of the average return with estimates at different quantiles of the earnings distribution and concludes that returns to human capital favour individuals at the upper end as compared to the lower end of the earnings distribution (i.e., the 90th percentile versus the 10th percentile).

the different skill distributions in the countries' populations, and an effect due to different returns to skill.[2] Both studies conclude that purely compositional differences in skill are far less important than disparities in the wage structure or unobservable factors, which is consistent with previous work.[3] Based on these results both studies tentatively conjecture that unobserved institutional factors play a bigger role for international differences in earnings inequality than (supply of and demand for) skill. However, despite the congruence of results across multiple studies, the interpretation offered is not straightforward. Skill supply and demand, as captured in the skill composition, and skill prices depend on each other (see Leuven et al., 2004, and even Paccagnella (2015) and Pena (2015) admit to this limitation of their approach). Because of this endogeneity, the decomposition techniques do not allow for a clean interpretation (or an unbiased estimate, for that matter) of the impact of market forces.

The analysis in this chapter avoids the pitfall by taking the data to an economic model that explicitly relates the relative supply and demand for skill to earnings inequality between groups of different skill. More specifically, each country's population is split into three groups – low-, medium- and high-skilled – based on cut-off values from the skill distribution in a baseline country. Supply of skill is then determined by the number of people in a certain skill group, including unemployed and employed workers. Demand for skill is constructed as an index commonly applied in the literature that takes employment numbers in specific industries and occupations into account. According to the canonical model, net supply (i.e. supply minus demand) of a certain skill group relative to another group correlates negatively with the earnings dispersion between those groups. I estimate this intuitive and simple model on pairwise combinations of countries in a series of regressions for various skill measures.

This provides two contributions to the literature. First, in contrast to earlier studies that use the PIAAC data, the present economic model accounts for the interdependence of skill prices and skill supply and demand, achieving a genuine estimate of the relevance of net supply differences for skill earnings inequality across countries. Second, this chapter updates and extends the work by Leuven et al. (2004). I revisit the previous findings relating to literacy and numeracy skill or to formal education with an expanded sample that now also includes women and unemployed people in the labour market. Additionally, I generate new results concerning the novel dimension of computer skill.

The main finding of this chapter is that supply and demand for skill remain important for understanding earnings inequality between people of different skill in the 21st century

[2] With a slightly different method, Pena (2015) additionally quantifies the contribution of unobservable factors, which in Paccagnella's method is included in the returns component.

[3] See Blau and Kahn (2005); Devroye and Freeman (2001); Fournier and Koske (2012) for earlier applications of the econometric decomposition techniques to similar contexts.

as well. Overall, the supply and demand framework explains around 30 percent of the international variation between skill groups' earnings, in spite of the fact that country-specific labour market institutions may work against this mechanism. The results are particularly pronounced for workers at the bottom of the skill distribution: For them, supply and demand account for almost 50 percent of the international differences in their relative earnings. Based on a measure of broad cognitive skill, the canonical model estimates that a 10 percent decrease in the relative net supply of low-skilled people (i.e. from a 0.33- to a 0.30-share of the population) increases their relative earnings by 1.5 percent. These results persist qualitatively under all measures of cognitive skill, such as literacy and numeracy or computer skill alone. The supply and demand model, however, shows no such correlation based on years of schooling and experience as skill measure as in Blau and Kahn (1996). This confirms the results of Leuven et al. (2004) and reinforces the argument that the indirect variables associated with skill are imperfect at best in describing the true skill level of an individual when it comes to an international comparison.

The remainder of this chapter is structured as follows. Section 2.2 presents the PIAAC data and points out their unique features. Section 2.3 discusses the supply and demand model of skill and translates the model into an empirical relationship, and Section 2.4 analyses the results. Section 2.5 offers concluding remarks.

2.2 Data

The data for the skill supply and demand analysis come from the recently conducted Survey of Adult Skills as part of the OECD Programme for the International Assessment of Adult Competencies (PIAAC). The PIAAC Survey makes internationally comparable skill data, earnings and a variety of background variables accessible for 24 countries, which makes the dataset highly suitable for comparative labour market research. It succeeds and extends the International Adult Literacy Survey (IALS, 1994 – 1998) and the Adult Literacy and Life Skills Survey (ALL, 2003 – 2006), the former having been an important data source for the cited literature (e.g. Blau and Kahn, 2005; Leuven et al., 2004).

Due to some data limitations the analysis concentrates on samples from 15 countries. These countries are Belgium (Flanders), the Czech Republic, Denmark, Estonia, Finland, Germany, Ireland, Japan, Korea, the Netherlands, Norway, Poland, the Slovak Republic, the United Kingdom (England and Northern Ireland) and the United States.[4] The data collection

[4]Precluded from the analysis are Australia and Cyprus because public-use files were not available at the time of writing; Austria, Canada and Sweden because of missing data on earnings; France, Italy and Spain because these countries have chosen not to participate in the computer skill assessment; and the Russian Federation because of a considerably smaller sample size than other countries.

extended over the period from August 2011 to April 2012 for these countries and took 230 days on average. The background information was collected in a computer-aided interview in the country's official language(s) that took about 30 to 45 minutes. In a second part of the interview the cognitive test was taken with no restriction on time.[5] The response rate was sufficiently high as judged by PIAAC's Technical Standards and Guidelines. All countries offered a modest (monetary or non-monetary) incentive to respondents in order to help reduce non-response bias.[6] The target population of the study comprised non-institutionalised adult residents between the age of 16 and 65, regardless of their citizenship, nationality or language. While some countries deviated from the PIAAC standard sampling design for the purpose of further national use of the data, strict quality controls by the PIAAC Consortium assured that the final probability-based samples were representative of the target population (OECD, 2013d, ch.10, 14).

This chapter restricts the sample to those active in the labour market. The sample includes 18 to 65 year old women and men who are either employed as wage and salary workers or are unemployed. Since the analysis is based on available background information about education, work experience, earnings, occupation and industry affiliation, respondents with missing observations in any of these variables are dropped from the sample. Overall, the per-country sample sizes range from 2597 (United States) to 4753 (Denmark) observations.

Definition of skill measures

The uniqueness of the PIAAC survey lies in assessing cognitive skills of the participants in three dimensions – literacy, numeracy and computer skill –, which contain the exact same information for every country. To give an impression of what these skill dimensions measure, I quote the definitions from the PIAAC Technical Report (OECD, 2013d):

Literacy (including reading components): understanding, evaluating, using and engaging with written texts to participate in society, to achieve one's goals, and to develop one's knowledge and potential;

Numeracy: the ability to access, use, interpret and communicate mathematical information and ideas, in order to engage in and manage the mathematical demands of a range of situations in adult life;

[5]The average time taken to complete the cognitive test was 50 minutes (see key fact sheet made available by the OECD (2013a)).

[6]The response rate was 50% or higher and without significant non-response bias for all countries. This was assessed in several Non-Response Bias Analyses (basic, extended or item-related), in which all countries were (at least) required to compare response rates for different subgroups and to compare the distribution of auxiliary variables (correlated with proficiency) for respondents and non-respondents For more detailed information see OECD (2013d), ch.16.3.

Computer skill: [7] using digital technology, communication tools and networks to acquire and evaluate information, communicate with others and perform practical tasks; more specifically, the ability to solve problems for personal, work and civic purposes by setting up appropriate goals and plans, and accessing and making use of information through computers and computer networks.

The domains literacy and numeracy are to a large extent based on the measurements in the previous OECD skill studies, whereas the computer skill domain introduces a new, modern dimension to skill measures.[8] The potential scale for all three measures ranges from 0 to 500 points. For the purpose of the supply and demand analysis I rely in turn on one skill measure comprising both literacy and numeracy (due to high correlations between the two, as outlined below), which is called S_{litnum}; on the computer skill measure alone, $S_{computer}$; and on an all-encompassing measure of cognitive skill, S_{PIAAC}.[9]

Internationally less comparable but more often available and used in the relevant literature are the measures of formal education, which translates acquired levels of education in ISCED classification into years of schooling, and work experience. Blau and Kahn (1996) and Leuven et al. (2004) combine the two in a weighted average, with weights stemming from a regression of wages on years of schooling, a second-order polynomial in work experience and country dummies. In order to assure comparability of my results with the previous literature and to assess the information content of this classical measure of human capital, results based on the Blau-Kahn measure, S_{BK}, are reported as well.[10] The Blau-Kahn measure is an indirect measure of skill as opposed to cognitive skill. Since my analysis compares skill across countries, the PIAAC-based measures may be superior to an indirect measure, given the large variety of school education and post-school training systems across the world. On the other hand, cognitive skills are not as easily observed by employers as diplomas and training certificates, with the consequence that earnings might not accurately reflect differences in those skills.

[7] Formally, this dimension is called ‘Problem solving in technology-rich environments’ (PSTRE). For reasons of clarity and brevity this chapter uses ‘computer skill’ in lieu thereof.

[8] Computer skill is measured only for those survey participants with at least some experience in using a computer. Missing values are imputed to retain a representative sample; Appendix 2.C provides further details and a critical discussion.

[9] All PIAAC-based measures are averages of the plausible values of the respective skill measure(s) in the dataset, scaled by the factor $\frac{1}{100}$. For example, S_{litnum} is constructed as the average of PVLIT1 to PVLIT10 and PVNUM1 to PVNUM10, divided by 100.

[10] The obtained weights from the worldwide regression are 0.092 for education, 0.372 for experience, -0.058 for the square of experience and 1.277 for the constant. Note that as in Blau and Kahn (1996) experience is scaled by a factor of $\frac{1}{10}$.

2.2.1 Some descriptive statistics

The supply and demand analysis of skill depends on observed differences in skill supply, relative earnings and the employment sector across countries. Looking at the descriptive statistics of certain key variables, some dissimilarities between countries become apparent. Table 2.A1 in Appendix 2.A shows averages and standard deviations by country.

Differences are most notable in the earnings measure. Earnings denote gross hourly earnings of wage and salary workers and are PPP-corrected for $US.[11] They range from a low of $US 9 to 10 in the Eastern European countries and Estonia to a high of $US 23 to 24 in Norway, Denmark and the United States. Similarly, their spread varies considerably across countries. The divergence in the second moment carries over to other aspects of the earnings distributions. Figure 2.1 shows common measures of overall earnings inequality. The log wage differential between the 90th and 10th percentile as well as the split into the two halves of the distribution are comparable to statistics reported by the OECD (2013b) and display Korea as the country with highest earnings inequality, regardless of the measure. Likewise, the Nordic countries and Belgium always form the group with lowest earnings inequality. Germany stands out as having a higher degree of inequality in the lower half of the distribution, whereas inequality is centred in the top half of the distribution for Japan.[12]

Countries differ also in their mean achievements in terms of cognitive skills and years of education and work experience. On average the survey respondents spent around 13 to 14 years in school and have acquired 18 to 19 years of work experience, with some exceptions. When translated into the Blau-Kahn measure of human capital, disparities with respect to the cognitive skill measures become obvious. Countries that score high on average in the classical measure based on schooling and experience are not necessarily those that do well on the cognitive scores.[13]

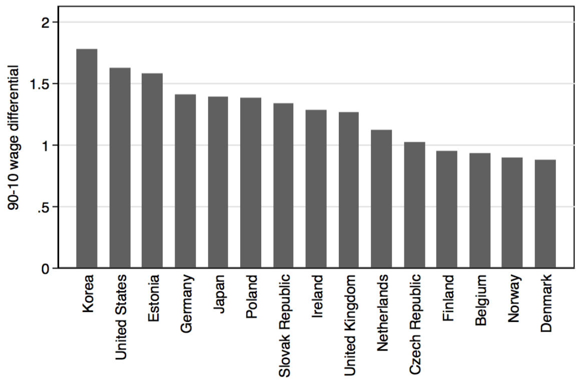
Within countries, the associations between the dimensions of cognitive skill are strong, as Table 2.1 reveals. For all countries literacy and numeracy are correlated with a coefficient around 0.9, which is the reason for combining the two dimensions into one measure. Computer skill is clearly positively correlated with literacy and numeracy, but to a lesser extent. This justifies the use of the pooled PIAAC measure S_{PIAAC} and at the same time leaves

[11]The data was collected from a set of questions that allowed respondents to choose the time interval for which they report their earnings. The PIAAC team combined all pieces of information into an hours-corrected earnings measure and performed quality checks by looking at the individual earnings distributions in the countries (OECD, 2013d).

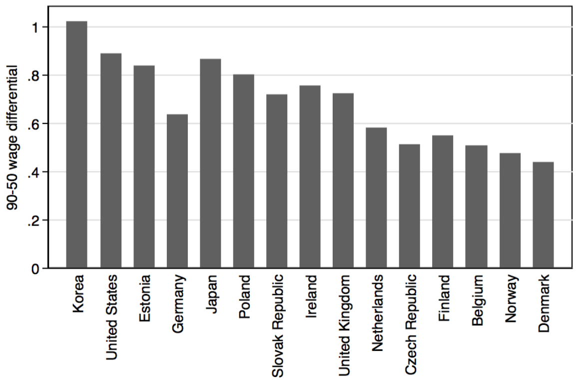
[12]For a detailed descriptive analysis of both earnings distributions and skill distributions see Paccagnella (2015).

[13]According to S_{PIAAC} Finland, the Netherlands and Norway come out on top of the ranking. These countries would take ranks 12, 5 and 2 respectively under a ranking based on years of schooling and experience. Similarly, the first three countries under S_{BK} are ranked 13th, 3rd and 12th under S_{PIAAC}. Spearman's rho gives a rank correlation of 0.129.

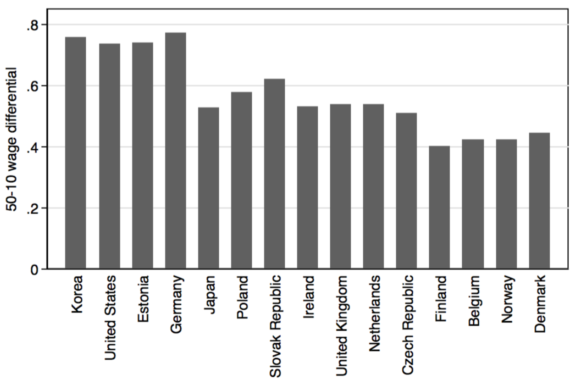
room for potential differences when looking at computer skill individually.



(a) 90-10 log wage differential



(b) 90-50 log wage differential



(c) 50-10 log wage differential

Figure 2.1: Pattern of log earnings inequality

Table 2.1: Correlations between skill measures

	literacy -numeracy	literacy - computer	numeracy - computer	literacy - S_{BK}	numeracy - S_{BK}	computer - S_{BK}	N
Belgium	0.910	0.622	0.642	0.368	0.395	0.216	2761
Czech Republic	0.837	0.492	0.546	0.249	0.334	0.099	2893
Denmark	0.912	0.643	0.621	0.304	0.375	0.181	4753
Estonia	0.881	0.505	0.552	0.213	0.289	0.102	4306
Finland	0.880	0.617	0.607	0.221	0.278	0.104	3411
Germany	0.906	0.556	0.629	0.280	0.342	0.098	3431
Ireland	0.896	0.547	0.552	0.396	0.400	0.308	3194
Japan	0.892	0.407	0.437	0.279	0.392	0.133	3301
Korea	0.913	0.521	0.565	0.342	0.399	0.282	3239
Netherlands	0.914	0.660	0.642	0.310	0.348	0.204	3227
Norway	0.922	0.648	0.655	0.271	0.338	0.122	3078
Poland	0.878	0.525	0.538	0.321	0.334	0.192	4464
Slovak Republic	0.879	0.453	0.465	0.233	0.326	0.154	2769
United Kingdom	0.891	0.544	0.607	0.295	0.308	0.137	4514
United States	0.922	0.704	0.700	0.507	0.530	0.384	2597

Note: Within-country correlations calculated using sampling weights.

2.2.2 Mincer earnings regressions

All human capital variables are related to hourly earnings in a meaningful way. Mincer-type of regressions (Mincer, 1974) show a coherent direction of association for all countries, but great variation in the magnitude of estimates. In order to assess the relative and individual importance of schooling versus the comprehensive measure of cognitive skill, I estimate three specifications of earnings regressions.

Table 2.2 reports the results in three separated sections. Earnings are generally concavely associated with work experience and positively with education or skill. In a classical Mincer regression of log earnings on education and a quadratic in work experience, one additional year of schooling is associated with an increase in earnings between 6.1 (Norway) and 10.1 (Germany) percent. The estimates are comparable in what they convey about relative effects across countries, but not identical to Hanushek et al. (2013)'s estimates due to a less restricted sample. Replacing education by S_{PIAAC} shows that earnings are also strongly related to cognitive skills. A one standard deviation increase in cognitive skill is associated with a 12.2 (Finland) to 22.4 (United States) percent increase in earnings.[14] Including both variables of human capital side by side proves that they each pick up some of the variation in earnings

[14]Note that these effects appear to be somewhat smaller than the effect of education since the within-country standard deviation of education lies around 2.5 years for all countries. However, as Paccagnella (2015) already notes, such direct comparisons have to be taken very cautiously because of the different metrics of the two variables.

Table 2.2: Earnings regressions on variables of human capital

Dependent variable: *log earnings*	educ.	(s.e.)	S_{PIAAC}	(s.e.)	educ.	(s.e.)	S_{PIAAC}	(s.e.)	N
Belgium	0.069	(0.003)	0.158	(0.007)	0.051	(0.003)	0.087	(0.008)	2679
Czech Republic	0.071	(0.005)	0.160	(0.014)	0.052	(0.006)	0.097	(0.015)	2607
Denmark	0.062	(0.003)	0.118	(0.011)	0.053	(0.003)	0.059	(0.011)	4467
Estonia	0.075	(0.004)	0.188	(0.009)	0.054	(0.004)	0.122	(0.010)	3960
Finland	0.062	(0.002)	0.122	(0.009)	0.054	(0.002)	0.058	(0.009)	3224
Germany	0.101	(0.005)	0.216	(0.011)	0.078	(0.005)	0.120	(0.012)	3286
Ireland	0.070	(0.005)	0.169	(0.016)	0.052	(0.005)	0.097	(0.017)	2774
Japan	0.078	(0.005)	0.179	(0.013)	0.058	(0.006)	0.122	(0.015)	3248
Korea	0.085	(0.004)	0.209	(0.013)	0.069	(0.005)	0.085	(0.016)	3103
Netherlands	0.080	(0.004)	0.155	(0.011)	0.065	(0.004)	0.081	(0.012)	3088
Norway	0.061	(0.003)	0.130	(0.009)	0.047	(0.004)	0.089	(0.009)	2984
Poland	0.093	(0.005)	0.195	(0.013)	0.076	(0.006)	0.090	(0.014)	3866
Slovak Republic	0.089	(0.005)	0.210	(0.012)	0.068	(0.006)	0.127	(0.013)	2505
United Kingdom	0.086	(0.005)	0.202	(0.010)	0.063	(0.005)	0.153	(0.012)	4209
United States	0.098	(0.005)	0.224	(0.018)	0.077	(0.006)	0.093	(0.021)	2350

Note: Least squares regressions weighted by sampling weights. Robust standard errors in parentheses. All coefficients are statistically significant at 1%. Regressions control for gender, experience, experience^2. S_{PIAAC} is standardised within each country. Each separated section corresponds to one specification.

and remain statistically significant, but reduce the importance of the other. At the same time, the overall fit increases. This points to the fact that the PIAAC scores and years of schooling at least partly impact earnings through different channels. When the comprehensive PIAAC score is split up into its three components (estimates are not reported here), the coefficients on education remain unchanged and the association of S_{PIAAC} is distributed across literacy, numeracy and computer skill. Due to the strong correlation between the dimensions not all coefficients are significant, but which type of skill dominates differs between the countries.[15]

The insight that emerges from all three specifications of Mincer regressions is that even though the estimated coefficients on education or cognitive ability do not reflect a causal relationship, they nevertheless are consistently positive and economically significant across countries and therefore predictive for average earnings. This provides the premise for examining both the indirect measure of skill based on education and experience and the PIAAC-based measures with respect to earnings inequality.

2.3 The supply and demand model of skill

The canonical model for the supply and demand of skill represents an appealing route to do so because of its simplicity and intuitive groundings. The supply and demand model for-

[15]For further analysis of earnings regressions including heterogeneous effects by age and other explanatory variables, see Hanushek et al. (2013).

malizes Tinbergen's race of education and technology. Tinbergen looked at the long-term movements in income inequality between graduate and other labour in developed countries as the "net balance of [...] conflicting effects" (Tinbergen, 1975, p.79) arising from demand and supply factors. The individual's supply behaviour originates from her utility maximisation and ultimately results in a certain share of a population obtaining higher education or skill. Demand for highly educated or skilled workers follows from the production function and increases with augmented capital and technological development, thus coining the phrase 'skill-biased technological change'. The interplay of demand and supply factors determines the income ratio between higher and lower skilled labour, and how income inequality between skill groups evolves is therefore decided in a race between technology and (the supply of) education. Intuitively, because workers of different skill groups are considered to be imperfect substitutes, higher relative net demand for one skill group versus the other results in relatively higher earnings for this skill group.

2.3.1 The empirical model

The competitive framework of the origination of skill group inequality was first applied by Blau and Kahn (1996) to explain the different levels of earnings inequality across countries.[16] Translating the mechanism into an international context requires the additional assumption that there are international barriers to the mobility of capital, labour or goods so that skill prices are not equalised. Then, a specific supply and demand structure for skill in one country implies distinct returns to skill levels as compared to another country. In order to empirically test this relationship, Blau and Kahn (1996) employ demand and supply indices for skill groups that are relative across two dimensions. First, supply and demand for skill in one country is always measured in reference to a *baseline country*. Second, the difference in supply and demand *between two skill groups* is used to explain their relative wages.

Leuven et al. (2004) develop Blau and Kahn's indices slightly further and I report their formulas here. Low, middle and high skill groups are defined by cut-off values from the skill distribution in the baseline country that split the population in the baseline country into three equally sized groups. This absolute perspective on skill creates variation in skill supply as the distributions in the countries differ. Appendix 2.B illustrates the skill group classification, as well as supply and demand indices, for the United States as the (arbitrarily chosen) baseline country.

The *skill supply index* in reference to the baseline country is a count of the representation of skill group k in the workforce (including currently employed and unemployed persons) of

[16] The cross-country methodology is an adaptation of the partial equilibrium framework developed by Katz and Murphy (1992) to study changes in skill earnings inequality over time.

country *j* relative to the baseline country *b* on a log scale.[17]

$$s_{k,j} = \ln\left(\frac{S_{k,j}}{S_{k,b}}\right)$$

with $S_{k,j}$ the share of skill group *k* in country *j*
$S_{k,b}$ the share of skill group *k* in baseline country *b* ($= \frac{1}{3}$ by construction).

The *skill demand index* in reference to the baseline country focuses on employed people and measures the degree to which the occupation-industry structure in one country *j* favours the skill group *k* relative to the baseline country (Blau and Kahn, 1996; Leuven et al., 2004). It sums over the weighted differences in employment in occupation-industry cells between two countries.[18] The differences in employment are measured regardless of the skill group affiliation of the employees. The relation to skill groups is introduced through the weights, which are constructed as the share of the skill group employed in the individual occupation-industry cells in the baseline country, scaled by the skill group's total share of employed in the baseline country. The final number is also transformed into its logarithm and centred around zero.

$$d_{k,j} = \ln\left(1 + \sum_o c_{ok}\frac{\Delta E_o}{E_{k,b}}\right)$$

with c_{ok} the share of skill group *k* of employed in occupation-industry cell *o* in the baseline country *b*
ΔE_o the difference in shares of total labour input employed in cell *o* between country *j* and *b*
$E_{k,b}$ the share of total labour input accounted for by skill group *k* in baseline country *b*.

Subtracting demand from supply gives the net supply of skill group *k* in country *j*.

$$NS_{k,j} = s_{k,j} - d_{k,j}$$

Finally, the difference between the net supply indices of two skill groups *k* and *l* gives their *relative net supply*.

$$NS_{k,j} - NS_{l,j} \tag{2.1}$$

For these two skill groups earnings differentials are calculated as the log of the ratio of

[17]Leuven et al. (2004) and Blau and Kahn (1996) focus on employed workers also in their supply indices. I deviate from this because I assume unemployed persons to be 'ready to be hired' and therefore being part of the supply of skill on the labour market. A robustness check shows that when the analysis is conducted on employed persons only, the results are very similar.

[18]Occupation-industry cells are determined by a 3x6-grid of three major groups of occupations (managers and professionals; clerical and sales workers; craft, trade, operators, assemblers, elementary (labourers), service workers) and six industries (agriculture; mining, manufacturing and construction; transportation, communication and public utilities; trade; finance, insurance, real estate and services; government).

average earnings in skill groups *k* and *l* in country *j*.

$$W_{k/l,j} = \ln\left(\frac{W_{k,j}}{W_{l,j}}\right)$$

In comparison to the baseline country *b* the *relative skill earnings differential* is then

$$W_{k/l,j} - W_{k/l,b} \tag{2.2}$$

The supply and demand model predicts that if one country has a larger relative net supply of one skill group as compared to the baseline country, the skill group should fare worse in terms of relative earnings in that country compared to the baseline country, i.e., the relative skill earnings differential should be negatively correlated to the relative skill net supply. To give an example, the larger the relative net supply of high skilled workers in Finland compared to the United States, the worse off are high skilled workers in relative terms in Finland than they are in the United States (possibly interpreted as bargaining power). The following regression equation, which combines equations (2.1) and (2.2), expresses this relationship:

$$W_{k/l,j} - W_{k/l,b} = \alpha + \beta(NS_{k,j} - NS_{l,j}) + \varepsilon_j \tag{2.3}$$

with β having a negative sign. In terms of Tinbergen's original idea, β can be thought of as the inverse of the elasticity of substitution between the two skill groups (Tinbergen, 1975, p. 85).

2.4 Results

I assess the validity of the theoretical relationship expressed in equation (2.3) in separate regressions for each pairwise comparison between two skill groups, i.e. high versus low, high versus middle and middle versus low skill, and in pooled regressions with all skill groups. The former estimations are informative on whether market forces influence earnings differentials only for certain parts of the skill distribution, whereas pooled regressions of all pairs of relative net supply and relative earnings differentials give an indication of how much variation the model explains overall. Importantly, I repeat this sequence of regressions for each measure of skill as defined in Section 3.3. On the one hand, this allows verifying to some extent the inference drawn from a single skill measure. On the other hand, it provides insight into whether individual dimensions of skill add to the explanatory power of the model in the international context.

Paccagnella (2015) and Pena (2015) both choose to look at earnings inequality in reference to one specific baseline country, which is in Paccagnella's case the United States, in Pena's case the United Kingdom. This is commonly done in the literature but remains an arbitrary choice that may influence the outcome. To avoid this potential bias the supply and demand indices are calculated with reference to each of the 15 countries in turn. The estimations of equation (2.3) therefore cluster standard errors at the country level in order to account for the underlying dependencies, are heteroskedasticity-robust and apply a small sample-correction factor as proposed in Cameron et al. (2008).[19]

Figure 2.A1 in Appendix 2.A complements the regression results with a graphical representation of the data. The supply and demand model deduces a negative correlation of earnings differentials and net supply differences between two countries. Any data point in the scatter plot is thus predicted to lie either in the second or fourth quadrant, and a fitted regression line has a negative slope. While the regression models yield estimates of the slope, the graphs show which quadrants the data points are scattered in.

2.4.1 Cognitive skill measures

Table 2.3: Regressions on net supply

	S_{litnum}			$S_{computer}$			S_{PIAAC}		
Dependent variable: *Earnings differentials*									
	Net supply	(s.e.)	R^2	Net supply	(s.e.)	R^2	Net supply	(s.e.)	R^2
high – low	-0.124	(0.028)***	0.405	-0.148	(0.021)***	0.322	-0.160	(0.021)***	0.466
med – low	-0.119	(0.023)***	0.477	-0.122	(0.032)***	0.239	-0.151	(0.026)***	0.317
high – med	-0.039	(0.025)	0.030	-0.062	(0.035)*	0.041	-0.052	(0.031)	0.044
pooled	-0.107	(0.023)***	0.299	-0.123	(0.022)***	0.206	-0.131	(0.023)***	0.292

Note: Robust standard errors that take clustering at the country level into account in parentheses. *,**,*** denote significance at 10%, 5% and 1%.

Table 2.3 displays the regression results under the three skill measures based on the cognitive test scores from PIAAC. Given the strong correlation between performance in the three dimensions, it is not surprising that they are very similar in nature. Under all PIAAC-based measures the estimated coefficient on relative net supply of any two skill groups is negative, *confirming* the prediction of the supply and demand model. In general, the supply and demand model holds consistently and is able to explain 20 to 30 percent of the overall variation in between-group inequality between any two skill groups (referring to the pooled regressions). The results are clearest, however, for low-skilled workers: a 10 percent decrease in relative net supply of low-skilled workers (i.e. from a 0.33- to a 0.30-share of the population)

[19] A world average could also serve as the baseline; the findings are robust to this exercise, but for reasons of comparability with Leuven et al. (2004) and generality of the model data points are constructed for every country with respect to every other country.

increases their relative earnings between 1.2 and 1.6 percent, and the explanatory power of the canonical model reaches up to 47 percent (depending on the skill measure and the comparison group). The – in absolute value – larger coefficients on net supply in the regressions with high- and low-skilled workers as compared to middle- and low-skilled workers are also in line with the model as they represent the lower degree of substitutability between the two skill groups that are farther apart.

One drawback is the non-significant estimate on the net supply of high- versus middle-skilled workers. While the sign goes in the right direction, it is clear by the R-squared that the canonical model fails in explaining earnings differences in this category. This could have to do with the fact that the type of skills elicited in the PIAAC test are not as important for what determines earnings at the higher end of the skill distribution. Instead, non-cognitive components such as managerial skills may play a bigger role.

Up to this point, the outcomes discussed applied to all three measures of cognitive skill. Looking at them separately reveals that both a measure only based on literacy and numeracy skills and a measure based on computer skill have explanatory power on their own. Especially S_{litnum} is suitable for explaining earnings differences for the middle-skilled versus the low-skilled, a finding consistent with Leuven et al. (2004). Computer skill seems to add information that is reflected in relative earnings particularly when contrasting high- to low-skilled workers. Here, a skill measure based on computer skill only is able to explain 32 percent of the international variation in earnings differentials (or, expressed differently, combining the literacy and numeracy measure with computer skill raises the explanatory power of the model roughly from 40 to 47 percent), which gives support to the hypothesis of an increased role of computer skill in the labour market (OECD, 2013c, p.3). In general, the R-squareds are lower as compared to S_{litnum} or S_{PIAAC}. This has perhaps to do with the fact that measuring skill solely through the ability to use computer technology is too unidimensional and not as relevant for certain types of jobs in lower occupations and certain industries; nevertheless, given the broad concordance of estimates, one cannot negate the importance of possessing computer skill in the labour market and their relative valuation in earnings.

The supply of and demand for cognitive skills are predictive for earnings inequality when it comes to international variation; but how useful are years of schooling and experience in that context?

2.4.2 Education and experience

When the less direct measure of skill, the composite of years of schooling and experience, is used to classify multiple countries into different skill groups, the picture changes drastically.

Table 2.4 shows the results.

Table 2.4: Blau and Kahn's measure

	S_{BK}		
Dependent variable: *Earnings differentials*			
	Net supply (s.e.)		R^2
high – low	0.059	(0.038)	0.038
med – low	0.031	(0.044)	0.008
high – med	0.003	(0.025)	0.000
pooled	0.035	(0.027)	0.015

Note: Robust standard errors that take clustering at the country level into account in parentheses. *,**,*** denote significance at 10%, 5% and 1%.

Under this measure the supply and demand model does not fit the data, having literally zero explanatory power. The regression model estimates no significant relationship between the net supply structure and earnings differentials between any two skill groups. If anything, the elasticity estimates carry a *positive* sign. This pattern is not reconcilable with the supply and demand model for skill but coincides with the finding in Blau and Kahn (1996), where a relative abundance of high- versus low-skilled workers in the United States positively correlates with higher relative wages than in other countries (p. 822).[20]

Years of education and experience appear to not be suitable for characterizing international differences in earnings inequality. This comes as no surprise given the international dissimilitude of (post-)educational systems. In contrast, the PIAAC measures are designed to assess basic competencies of the working population in every country "that are relevant to adults in many social contexts and work situations, and necessary for fully integrating and participating in the labour market, education and training, and social and civic life", as stated in a summarizing key fact sheet about the Survey of Adult Skills (OECD, 2013a). Given this objective and the findings in Tables 2.3 and 2.4, a plausible reading of the results is that the PIAAC cognitive scores succeed in making skills internationally comparable, both overall and especially so when describing the lower end of the skill distribution. For comparing only the top two skill groups, most likely other, more sophisticated skills are reflected in relative earnings as well, so that the prediction of the elasticity of substitution between the highest skill groups is obscured by the simplicity of the assessed tasks.

[20]Appendix 2.B elaborates on the direct comparison of the two datasets.

2.4.3 The roles of supply and demand

The data prove that the net supply situation of skill in a country (compared to a baseline country) is predictive for earnings inequality when skill is measured on an internationally comparable basis. For those skill measures it is instructive to look at the influence of supply and demand indices separately.

Table 2.5: Regressions on supply and demand

	S_{PIAAC}					
Dependent variable: *Earnings differentials*						
	Supply	(s.e.)	Demand	(s.e.)	R^2	H0: $\beta_S = -\beta_D$
high – low	-0.162	(0.025)***	0.181	(0.090)*	0.466	0.7948
med – low	-0.141	(0.031)***	0.003	(0.083)	0.348	0.0427
high – med	-0.051	(0.032)	0.038	(0.123)	0.044	0.9022
pooled	-0.129	(0.027)***	0.100	(0.097)	0.293	0.7047

Note: Robust standard errors that take clustering at the country level into account in parentheses. *,**,*** denote significance at 10%, 5% and 1%.

Table 2.5 shows coefficients for the split indices and adds p-values of two-sided hypothesis tests for the equality of coefficients in absolute terms.[21] As theory predicts, relative skill supply is negatively associated with relative earnings. All coefficients (except on the high- versus middle-skilled comparison) are significant at 1 percent and are quite similar to the estimates on net supply in Table 2.3. The coefficients on demand are positive and largely of a similar absolute size as the supply estimates, but statistically indistinguishable from zero. This is due to the fact that there is less variation in demand indices as compared to supply indices; the overall standard deviation of relative demand is 0.131 (mean: 0.007), whereas the overall standard deviation of supply is 0.477 (mean: -0.014).[22] This results in large standard errors for the demand estimates and non-significant results, but the hypothesis of equality of demand and supply estimates in absolute terms cannot be rejected in any of the cases except the middle- versus low-skilled comparison.[23] Significance issues aside, according to the data a 10 percent increase in the supply of middle- or medium-skilled workers decreases their earnings relative to low-skilled workers by 1.4 to 1.6 percent, inequality thus declines. A 10 percent higher relative demand for high-skilled workers, however, increases inequality between high- and low-skilled workers by 1.8 percent. Stronger demand for middle-skilled workers, in contrast, does not seem to affect the relative earnings of low-skilled competitors.

This tentative exercise in distinguishing the impacts of supply and demand completes the

[21] Results are only shown for the measure S_{PIAAC}; S_{litnum} and $S_{computer}$ yield similar estimates and all qualitative statements hold for these measures as well.

[22] For an example of supply and demand indices for one baseline country, see Table 2.B1 in Appendix 2.B.

[23] Note that under S_{litnum} and $S_{computer}$ this is not the case; there is still a probability of 0.2639 and 0.1995 of obtaining the observed difference in size of estimates, assuming the null hypothesis is true.

picture that the canonical model draws within the scope of this work.

2.5 Conclusion

Applying the canonical supply and demand model of skill to the PIAAC data shows that a substantial amount of earnings inequality between skill groups can be accommodated by a simple partial equilibrium framework. Depending on the measure of skill used to categorise the international population into groups, the conclusions about the validity as well as the power of the model differ. All skill measures based on the newly collected data from the OECD Programme of the International Assessment of Adult Competencies confirm the predictive power of supply of and demand for skill for relative earnings differentials. Up to 47 percent when looking at the relative earnings of the lowest skill group, or 30 percent of overall between-group inequality are explained. Persistent results based on the ability to make use of computer technology prove the importance of this ‘novel’ skill dimension for the labour market. As in Leuven et al. (2004) and Blau and Kahn (1996), the supply and demand model has no explanatory power under a skill measure based on years of schooling and experience, strengthening the argument that years of education and work experience cannot be easily compared across countries. The results are robust to various checks of the data and are not unequivocally driven by either supply or demand.

An interesting discovery is that the results are very much in line with what Leuven et al. (2004) estimate from data of the 1990s. This level of congruence is remarkable for several reasons. The first is that the countries included in the two studies were not the same. Only 8 countries overlap between the two samples, whereas 14 countries appear in only one of the two datasets. Amongst those 14 are countries from very different world regions such as South America (Chile in Leuven et al. (2004)) or Asia (Japan and Korea in this study). The accordance of estimates and R-squareds seems to suggest that regardless of which OECD countries are compared to each other, differences in skill net supply explain about a third of the differences in earnings inequality between skill groups.

The second reason why this finding is remarkable is because the canonical supply and demand model for skill has lately been criticised as being ‘too simplistic’ for modern economies. Starting with a widely-cited contribution by Autor et al. (2003), a literature has emerged that focuses on occupational tasks rather than skills in order to explain changes in earnings inequality (Acemoglu and Autor, 2011; Firpo et al., 2012; Goos et al., 2014, among others). The idea behind a task-based model is that skills are portable across tasks and that tasks are the unit that produces output. Changes in labour market conditions and technology therefore primarily influence the allocation of skills to tasks (Acemoglu and Autor, 2011).[24] This distinction allows technological change to be routine-biased (i.e. biased against routine tasks) rather than skill-biased (i.e. biased in favour of higher skill), and explains the job polarisation phenomenon that is observed in employment data of many countries (Autor, 2015; Goos and Manning, 2007; Goos et al., 2009; Michaels et al., 2010). Job polarisation describes changes in the employment structure over time and is therefore also related to inequality changes over time within one country. With its cross-sectional cross-country analysis, this chapter takes on a different perspective and it is not clear *a priori* how job polarisation (in some of the countries, or to differing degrees) should affect the outcome. However, in light of a generally more complex relationship between supply and demand for skill and skill wage inequality, achieving the same explanatory power with the canonical model as 20 years earlier is a noteworthy result.

While a more complex model could perhaps do an even better job at explaining the prevailing empirical patterns, the results speak for the fact that the canonical supply and demand model for skill provides a good benchmark for looking at skill earnings inequality, especially in a cross-country context. Consequently, policy makers should consider the insights that we gain from skill supply and demand as a tool for shaping (earnings) inequality (Autor, 2014). For example, by promoting educational programmes and providing broader access to post secondary education, policy can steer the supply of skill and work towards a moderation of skill earnings inequality. Alternatively, skill demand can be influenced through taxation and investment in such a way that it benefits skill groups that are currently oversupplied. Comparing one country's situation to, say, a neighbouring country should always take differences in supply and demand for skill between the countries into account. Only after these differences are removed, other factors such as labour market institutions and regulations may give insight into additional drivers of earnings inequality.

[24]See Gathmann and Schoenberg (2010) for an empirical measurement of the portability of skills across occupations.

Appendix 2.A Additional tables and figures

Table 2.A1: Mean (standard deviation) of key variables

	N	empl.	male	age		education		experience		earnings	
Belgium	2761	2679	0.52	41.09	(11.18)	12.99	(2.59)	19.60	(11.55)	20.24	(10.85)
Czech Republic	2893	2607	0.53	40.15	(11.33)	13.27	(2.52)	18.51	(11.68)	9.11	(7.84)
Denmark	4753	4467	0.50	41.09	(12.32)	13.01	(2.59)	21.29	(12.42)	24.06	(11.49)
Estonia	4306	3960	0.45	40.30	(12.43)	12.50	(2.60)	18.62	(12.49)	9.97	(9.51)
Finland	3411	3224	0.49	41.12	(12.40)	13.00	(2.88)	18.34	(12.41)	18.65	(7.98)
Germany	3431	3286	0.52	41.34	(11.98)	13.73	(2.54)	19.32	(12.48)	18.74	(13.11)
Ireland	3194	2774	0.49	37.93	(11.60)	15.36	(2.89)	16.40	(11.09)	22.19	(16.30)
Japan	3301	3248	0.56	41.51	(12.50)	13.32	(2.35)	18.68	(12.03)	16.60	(17.31)
Korea	3239	3103	0.57	39.27	(11.33)	13.38	(2.98)	12.66	(9.95)	17.58	(18.35)
Netherlands	3227	3088	0.52	39.68	(12.46)	13.61	(2.48)	18.60	(11.60)	20.87	(14.61)
Norway	3078	2984	0.49	39.74	(12.72)	14.44	(2.41)	18.26	(12.03)	24.43	(12.31)
Poland	4464	3866	0.53	38.60	(11.75)	13.37	(2.93)	15.63	(11.78)	9.71	(10.99)
Slovak Republic	2769	2505	0.52	40.22	(11.31)	13.59	(2.58)	18.14	(11.68)	10.00	(18.19)
United Kingdom	4514	4209	0.51	38.82	(12.23)	13.19	(2.29)	18.74	(12.12)	18.86	(14.55)
United States	2597	2350	0.50	median: 40-44		13.85	(2.92)	20.32	(12.33)	23.44	(21.01)

	S_{BK}		S_{PIAAC}		literacy		numeracy		computer skill	
Belgium	2.916	(0.281)	2.765	(0.509)	2.817	(0.424)	2.870	(0.451)	2.608	(0.828)
Czech Republic	2.923	(0.288)	2.686	(0.490)	2.758	(0.380)	2.772	(0.407)	2.527	(0.918)
Denmark	2.931	(0.304)	2.770	(0.484)	2.770	(0.414)	2.852	(0.450)	2.689	(0.756)
Estonia	2.843	(0.309)	2.680	(0.491)	2.787	(0.402)	2.761	(0.407)	2.493	(0.890)
Finland	2.886	(0.325)	2.888	(0.465)	2.971	(0.419)	2.914	(0.438)	2.780	(0.711)
Germany	2.968	(0.323)	2.686	(0.535)	2.733	(0.430)	2.764	(0.473)	2.561	(0.907)
Ireland	3.085	(0.304)	2.612	(0.522)	2.727	(0.420)	2.631	(0.465)	2.477	(0.910)
Japan	2.925	(0.274)	2.784	(0.547)	3.004	(0.352)	2.927	(0.398)	2.421	(1.186)
Korea	2.837	(0.315)	2.565	(0.575)	2.756	(0.378)	2.677	(0.410)	2.261	(1.174)
Netherlands	2.957	(0.289)	2.851	(0.475)	2.913	(0.430)	2.869	(0.448)	2.771	(0.702)
Norway	3.022	(0.310)	2.801	(0.497)	2.833	(0.427)	2.842	(0.490)	2.729	(0.733)
Poland	2.878	(0.324)	2.527	(0.584)	2.720	(0.427)	2.661	(0.449)	2.199	(1.136)
Slovak Republic	2.946	(0.295)	2.631	(0.536)	2.789	(0.337)	2.835	(0.394)	2.268	(1.145)
United Kingdom	2.914	(0.267)	2.751	(0.483)	2.820	(0.425)	2.728	(0.477)	2.705	(0.745)
United States	2.997	(0.334)	2.624	(0.584)	2.740	(0.479)	2.592	(0.551)	2.541	(0.879)

Note: Male stands for the fraction of males in the sample. Age is continuous and ranges from 18 to 65, with exception of the United States where it is grouped into 5 year-intervals from 20 to 65. Education refers to years of schooling. Experience denotes years worked. Earnings are gross hourly earnings in $US. All statistics are calculated using sampling weights.

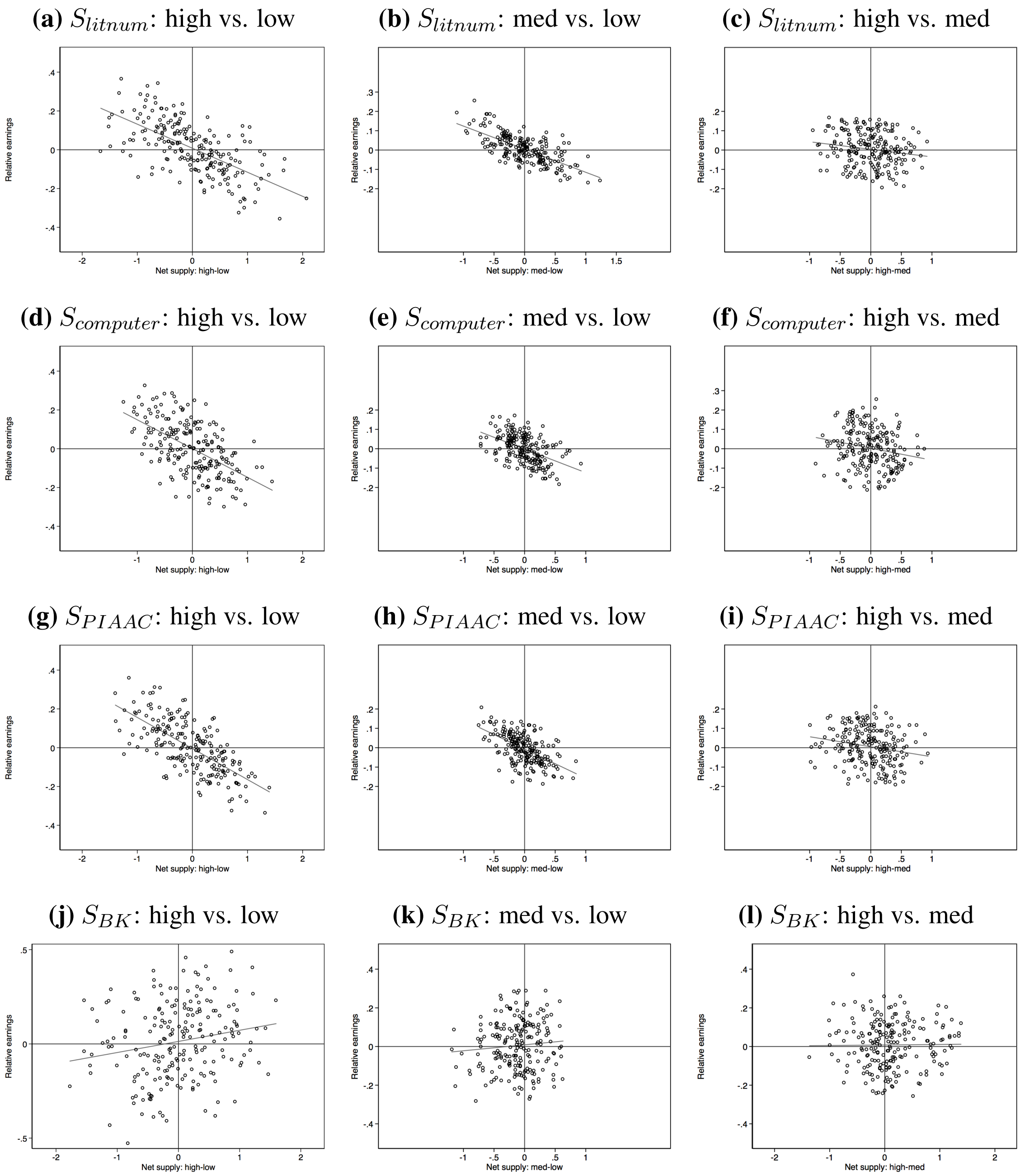


Figure 2.A1: Relative net supply and relative earnings differentials

Appendix 2.B Example of supply and demand indices

(a) U.S. skill distribution **(b)** Finland's skill distribution

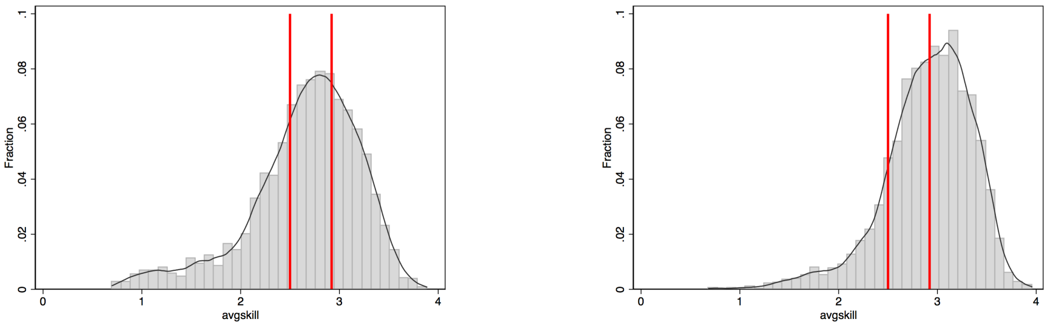


Figure 2.B1: Skill cut-offs according to the U.S. distribution as baseline

Using the United States as an example for the baseline country, Figure 2.B1 shows the categorisation of another country's population into absolute skill groups. The U.S. skill distribution (skill measure: S_{PIAAC}) in Panel (a) is split into three equal parts, where the vertical lines mark the cut-off values. When overlayed with the Finnish skill distribution in Panel (b), the low skill group is noticeably smaller and the high skill group larger. That means, when compared to the United States, Finland has a lower supply of low-skilled workers and a larger supply of high-skilled workers.

Table 2.B1: Supply, demand and net supply under S_{PIAAC}

	Supply			Demand			Net supply		
	low	med	high	low	med	high	low	med	high
Belgium	-0.327	-0.060	0.290	0.022	0.018	-0.038	-0.349	-0.078	0.328
CzechRep	-0.175	0.094	0.060	0.186	0.022	-0.229	-0.361	0.072	0.290
Denmark	-0.407	0.036	0.261	0.027	-0.004	-0.021	-0.434	0.040	0.282
Estonia	-0.138	0.103	0.021	0.102	0.005	-0.107	-0.241	0.098	0.128
Finland	-0.732	-0.034	0.440	0.084	-0.012	-0.069	-0.815	-0.022	0.509
Germany	-0.105	-0.031	0.122	0.139	0.014	-0.159	-0.244	-0.045	0.281
Ireland	-0.008	0.102	-0.105	0.098	0.003	-0.100	-0.106	0.099	-0.005
Japan	-0.283	-0.276	0.398	0.141	0.008	-0.155	-0.424	-0.285	0.553
Korea	0.042	0.027	-0.073	0.173	0.044	-0.238	-0.131	-0.017	0.165
Netherlands	-0.603	-0.072	0.420	-0.027	0.014	0.010	-0.577	-0.085	0.410
Norway	-0.480	-0.025	0.341	0.025	0.004	-0.027	-0.505	-0.029	0.368
Poland	0.189	-0.026	-0.201	0.133	0.023	-0.162	0.056	-0.049	-0.040
SlovakRep	-0.101	0.061	0.033	0.143	0.007	-0.156	-0.245	0.054	0.190
United Kingdom	-0.256	0.020	0.187	0.070	-0.005	-0.063	-0.326	0.025	0.250
United States					baseline country				

Note: Calculated using sample weights. Skill measure used is S_{PIAAC}.

Table 2.B1 shows supply, demand and net supply of skill groups for all countries compared to the United States. It becomes clear that most countries have fewer low-skilled

workers in terms of the U.S. skill distribution, but that demand for this skill group tends to be higher in these countries as compared to the United States. This results in negative net supply for all countries except Poland. Roughly the opposite holds for high-skilled workers; for the middle skill group differences to the United States are not as systematic.

The comparison of Table 2.B1 to Tables 5 – 7 in Blau and Kahn (1996), which also display demand, supply and net supply with reference to the United States, is interesting. There is little overlap in countries considered and the underlying samples are different with respect to gender and the exclusion of self-employed or unemployed, but nevertheless the big picture of demand indices is comparable, suggesting that the demand structure for skill has not changed that much over time. The supply indices, however, are diametrically opposed for the low and high skill groups. Applying the same skill measure as Blau and Kahn used in the current dataset shows that this is not a product of a change in supply structure over time, but entirely accounted for by the different measurement of skill. This finding provides a strong argument for the superiority of cognitive skill measures as opposed to a composite of years of education and experience in the international context.

Table 2.B2: Supply, demand and net supply under S_{BK}

	Supply			Demand			Net supply		
	low	med	high	low	med	high	low	med	high
Belgium	0.178	0.074	-0.319	-0.008	0.044	-0.037	0.186	0.030	-0.282
CzechRep	0.212	0.273	-0.808	0.135	0.118	-0.293	0.077	0.154	-0.515
Denmark	0.148	-0.007	-0.166	0.023	0.012	-0.033	0.125	-0.020	-0.132
Estonia	0.517	-0.327	-0.509	0.084	0.028	-0.114	0.433	-0.356	-0.395
Finland	0.348	-0.233	-0.235	0.072	0.031	-0.104	0.276	-0.264	-0.132
Germany	-0.013	0.220	-0.269	0.120	0.079	-0.218	-0.133	0.141	-0.051
Ireland	-0.222	-0.398	0.427	0.122	0.023	-0.151	-0.344	-0.421	0.578
Japan	0.203	0.091	-0.389	0.150	0.079	-0.258	0.053	0.012	-0.132
Korea	0.442	-0.088	-0.643	0.192	0.093	-0.338	0.250	-0.181	-0.305
Netherlands	0.076	0.004	-0.088	-0.017	0.012	0.003	0.093	-0.008	-0.091
Norway	-0.104	-0.207	0.253	0.039	0.008	-0.044	-0.142	-0.215	0.297
Poland	0.389	-0.112	-0.463	0.104	0.060	-0.173	0.285	-0.172	-0.290
SlovakRep	0.169	0.227	-0.583	0.107	0.071	-0.192	0.062	0.156	-0.391
United Kingdom	0.313	-0.091	-0.331	0.070	0.050	-0.122	0.243	-0.141	-0.208
United States					baseline country				

Note: Calculated using sample weights. Skill measure used is S_{BK}.

Appendix 2.C Imputation of missing computer skill scores

In light of the technological revolution that has reached both private life and the workplace it is especially interesting to study the influence of supply and demand of computer skill on inequality. However, part of the population does not possess the basic knowledge necessary to participate in a direct assessment of computer skills. This leads to missing computer skill scores for a substantial fraction of each country's population, which are displayed in Table 2.C1. The Technical Report (OECD, 2013d) names three reasons for a missing computer skill score:

1. The individual reports to not have any prior experience in using a computer.
2. The individual reports to have used a computer before but fails a basic ICT core test.[25]
3. The individual opts out of the computer-based assessment.[26]

Table 2.C1: Missing computer skill scores

	missing	no computer experience	failed ICT core test	opted out
Belgium	0.11	0.04	0.03	0.04
Czech Republic	0.20	0.06	0.02	0.12
Denmark	0.12	0.01	0.06	0.05
Estonia	0.26	0.06	0.03	0.16
Finland	0.11	0.01	0.03	0.07
Germany	0.13	0.05	0.03	0.05
Ireland	0.23	0.05	0.04	0.14
Japan	0.31	0.07	0.10	0.14
Korea	0.26	0.12	0.09	0.05
Netherlands	0.07	0.01	0.03	0.03
Norway	0.09	0.00	0.04	0.04
Poland	0.32	0.06	0.07	0.19
Slovak Republic	0.33	0.18	0.02	0.12
United Kingdom	0.08	0.02	0.04	0.03
United States	0.13	0.04	0.04	0.06

Note: All values are fractions of the total populations.

[25]The ICT core test assesses whether the subject commands the skills necessary to follow the actual test, i.e. scrolling, clicking and the like (OECD, 2013c, p. 88).

[26]Since the evaluation of computer skills was a voluntary component, 'opting out' need not be related to any reason in particular. It is likely, however, that individuals opt out because they do not feel comfortable enough taking the assessment.

Logit regressions in Table 2.C2 show that the probability of not having participated in the computer skill assessment is positively related to a higher age group and working in a lower type of occupation, and negatively associated with earnings, experience, education and higher cognitive scores in the other dimensions. These relations are stronger for the first category of missing than for the second, and partly disappear for the third category. The data are thus not missing at random. Leaving these observations out of a supply and demand analysis would distort the indices by imposing the assumption that these individuals play no role in the labour market.

Table 2.C2: Logit regressions of the probability of missing computer skill score

	(1) missing	(2) no exp	(3) fail ICT	(4) opt out	(5) missing	(6) no exp	(7) fail ICT	(8) opt out
earnings	-0.722***	-0.860***	-0.0854	-0.547***				
	(0.116)	(0.220)	(0.0871)	(0.122)				
age group	0.376***	0.588***	0.125***	0.168***	0.371***	0.578***	0.123***	0.169***
	(0.0303)	(0.0382)	(0.0277)	(0.0382)	(0.0313)	(0.0378)	(0.0278)	(0.0404)
male	0.186**	0.265***	0.339***	-0.139*	0.0201	0.0905	0.318***	-0.269***
	(0.0824)	(0.0826)	(0.0807)	(0.0754)	(0.0784)	(0.0620)	(0.0810)	(0.0682)
education	-0.0911***	-0.228***	0.0124	-0.0387	-0.123***	-0.234***	0.00278	-0.0621***
	(0.0277)	(0.0303)	(0.0162)	(0.0245)	(0.0253)	(0.0308)	(0.0176)	(0.0234)
experience	-0.224***	-0.286***	-0.184***	0.0383	-0.302***	-0.343***	-0.194***	-0.0215
	(0.0557)	(0.0766)	(0.0451)	(0.0695)	(0.0633)	(0.0661)	(0.0460)	(0.0784)
numeracy	-1.120***	-1.145***	-1.399***	-0.310	-1.167***	-1.116***	-1.420***	-0.371**
	(0.218)	(0.196)	(0.299)	(0.198)	(0.208)	(0.200)	(0.283)	(0.181)
occupation	0.497***	0.957***	0.239***	0.367***	0.612***	1.089***	0.231***	0.469***
	(0.0431)	(0.0456)	(0.0606)	(0.0626)	(0.0469)	(0.0801)	(0.0573)	(0.0631)
unemployed					0.0234	0.105	-0.210***	0.157**
					(0.0955)	(0.175)	(0.0548)	(0.0729)
Constant	1.585***	-0.855*	-0.292	-1.399***	0.335	-2.976***	-0.0577	-2.476***
	(0.526)	(0.494)	(0.512)	(0.473)	(0.520)	(0.734)	(0.542)	(0.486)
Observations	48,350	48,350	48,350	48,350	51,938	51,938	51,938	51,938

Note: Robust standard errors account for clustering by country. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

To avoid this problem computer skills are imputed for those missing in accordance with the stated reason for why they are missing. Since reasons 1. and 2. indicate with certainty that the subject has no computer skills (as defined by the scale of the assessment), the imputed score is 0. Category 3. is more ambiguous as the reason for opting out is unspecified. The score for computer skill is imputed as the prediction based on the variables education, experience, age, male, literacy, numeracy and an indicator for the lowest type of occupation, with weights being the estimated coefficients of a regression of observed computer skill on these variables.

The imputation allows to make use of the novel skill dimension to examine the importance of computer skills, as well as to construct a comprehensive measure of cognitive skill covering all three dimensions. It is important to note that while imputing computer skill scores introduces some uncertainty about the obtained results in the supply and demand analysis, the imputation does not affect the analysis based solely on literacy and numeracy skills or the formal measures of years of schooling and experience. Eyeballing the results in Table 2.3 proves that the obtained estimates for the computer skill measure are not unsound.

As an additional robustness check to the applied imputation mechanism the supply and demand model is replicated under two alternatives. These mechanisms are chosen in such a way that they provide one rather 'optimistic' and one rather 'pessimistic' estimation of missing computer skill scores.

Pessimistic imputation Individuals reporting no prior computer experience, as well as individuals who fail the basic ICT core test are assigned a zero score for their average computer skill value. Individuals who opt out of the computer-based assessment receive an extremely low score of 0.85[27]. This mechanism can be thought of as assuming that individuals who opt out of the computer-based assessment feel uncomfortable working with a computer because they have very poor computer skills.

Optimistic imputation All missing computer skill scores are imputed as out-of-sample predictions based on available information on age, gender, education, experience, average literacy and numeracy score and occupation. Coefficients come from a regression of observed average computer skill scores on the aforementioned characteristics. This mechanism can be thought of as a forward looking scenario, assuming that in the near future it will be less likely that individuals have no computer skills at all.

Table 2.C3 shows the estimation results for regressions of skill group earnings differentials on relative net supply applying the two alternative imputation mechanisms for missing computer skill scores. In the direct comparison with Table 2.3 a few observations are salient. First, under the 'pessimistic imputation' standard errors are slightly smaller, leading to significant estimates even for contrasting high- to middle-skilled workers. The explanatory power of the model for this category is greater but remains limited at 8 to 11 percent. Second, under the 'pessimistic imputation' the estimated net supply coefficients according to the measure $S_{computer}$ are smaller in absolute value and the explanatory power is generally lower, but qualitative nuances are the same. Changed imputed scores are of little consequence for the combined PIAAC measure S_{PIAAC}. Third, the 'optimistic imputation' results in similar estimates as the applied imputation method, with only slight differences in explanatory power shifted from the high- versus low-skilled contrast to the middle- versus low-skilled

[27]The value stems from OECD (2013b) where individuals with missing literacy or numeracy scores are treated as scoring 85 points on average.

comparison. In summary, elasticity estimates from the supply and demand model are reliable across a range of methods of imputation of missing values; regardless of the method, around 30 percent of overall earnings inequality between skill groups can be explained by the model.

Table 2.C3: Net supply regressions under alternative imputation mechanisms

Optimistic imputation						
	$S_{computer}$			S_{PIAAC}		
Dependent variable: *Earnings differentials*						
	Net supply	(s.e.)	R^2	Net supply	(s.e.)	R^2
high – low	-0.132	(0.025)***	0.260	-0.131	(0.028)***	0.376
med – low	-0.136	(0.028)***	0.350	-0.124	(0.025)***	0.450
high – med	-0.077	(0.035)**	0.064	-0.050	(0.028)*	0.043
pooled	-0.119	(0.023)***	0.203	-0.113	(0.024)***	0.277
Pessimistic imputation						
	$S_{computer}$			S_{PIAAC}		
Dependent variable: *Earnings differentials*						
	Net supply	(s.e.)	R^2	Net supply	(s.e.)	R^2
high – low	-0.085	(0.015)***	0.211	-0.123	(0.016)***	0.390
med – low	-0.051	(0.017)***	0.165	-0.101	(0.023)***	0.327
high – med	-0.073	(0.024)***	0.076	-0.071	(0.022)***	0.106
pooled	-0.071	(0.012)***	0.158	-0.108	(0.015)***	0.299

Note: Robust standard errors that take clustering at the country level into account in parentheses. *,**,*** denote significance at 10%, 5% and 1%.

Chapter 3

When refugees work: The social capital effects of resettlement on host communities*

> It is the perception that the new migration and cohesion are inversely correlated that currently drives public concern and policy making in this sphere.
>
> Roger Zetter, Fourth Director of the Refugee Studies Centre, University of Oxford

3.1 Introduction

It is estimated that there are over 65 million forcibly displaced people worldwide.[28] Since the onset of the Syrian civil war in 2011, there has been a rapid acceleration in the numbers of newly created asylum seekers. Many European states have received tens to hundreds of thousands of refugees annually during this period, which has resulted in significant political, economic and social pressures. Where and how best to resettle the arrivals are vital questions for the welfare of both host citizens and the refugees themselves, as well as for shaping the political discourse around the issue.

In this chapter we focus on the social capital effects of resettlement on host communities. This is an important aspect as social integration is arguably the most salient and dominant factor in the public debate. Social effects from shocks to ethnic diversity in general can manifest themselves in several measurable dimensions, such as trust, ethnocentrism, community

*This chapter is based on joint work with David Smerdon (Albrecht and Smerdon, 2017).

[28] United Nations High Commissioner for Refugees (2016).

involvement, feelings of safety and inter-group attitudes. Furthermore, it is empirically well-known that changes to social capital can have significant economic flow-on effects (Knack and Keefer, 1997). Despite this, there has been a dearth of quantitative evidence about the social impact of refugee resettlement. One reason for this is that it is difficult to untangle the ensuing increase in employment competition from the direct impact of resettlement on social and community cohesion. Another complication is endogeneity arising from a tendency to voluntarily resettle into communities with preexisting positive social characteristics.

The current chapter addresses these concerns by making use of a natural experiment in rural Australia. Our case study features a town that experienced a large refugee resettlement shock that was exogenous with respect to social indicators, for reasons we will argue below. The town is characteristic of many rural communities in Australia across several important dimensions: a small and declining population, low unemployment, and highly ethnically homogeneous (Anglo-Saxon). The resettlement shock has dramatically changed the demographic nature of the local community: from having no refugees in 2009, the town is now home to 200 (roughly 8%) refugees from Myanmar.

We conduct an incentivised survey in the field to measure natives' relative trust of refugees, which is our primary indicator of the impact on social capital.[29] We test relative trust by measuring the effect of resettlement on natives' trust towards refugees relative to their trust of other natives. To identify the social impact, we compare these incentivised measures to those from control towns that are similar along demographic, economic and geographic dimensions but host no refugees.

In addition to the data from the incentivised trust measures, we also elicit trust and other social indicators, in particular attitudes toward refugee resettlement in general, through non-incentivised survey questions. Finally, we validate both types of our collected data against existing survey data that was collected from both treated and control towns pre- and post-treatment. Combining the different sources and time periods of data allows us to study the direct social effects of the refugee shock in the absence of changes to labour competition.

We find no evidence from our case study that social capital is adversely affected by exposure to refugee resettlement. Our main finding is that the migration shock has increased relative trust in the town: natives in the treated town trust refugees relatively more. This is a surprising result that is robust to different specifications and weightings. Alternative explanations based on preexisting social capital differences, income effects, or selective migration are not supported by the data. In addition, the results from the survey measures indicate that natives in the treated town hold significantly more favourable attitudes towards refugee reset-

[29]The measure of relative trust in our study is similar in nature to the literature on ethnocentric trust, which measures the degree to which people prefer to trust their own ethnicity and is often associated with homophily and social fragmentation along ethnic lines.

tlement in Australia. We find evidence of substantial gender heterogeneity in these results, with females exhibiting significantly more pronounced positive treatment effects.

Our study is closely related to a large body of literature on ethnic diversity. Within this field, social capital is generally differentiated into 'bonding' capital (ties to in-group members) and 'bridging' capital (ties to out-group members) (Putnam, 2007). This distinction defines the three major theories that connect social capital to ethnic diversity. *Contact theory*, also known as inter-group theory, states that more contact with other ethnicities fosters out-group trust and solidarity (Allport 1954, Pettigrew and Tropp 2006 among others). However, broadly speaking it has received less empirical support than other theories. More popular in the literature is *Conflict theory*, which relies on competition over limited resources to predict a decline in bridging capital and concurrent rise in bonding capital as ethnic diversity increases (Leigh 2006). Alesina and La Ferrara (2002) find that racial fragmentation of a community (defined by five categories) is harmful to generalised trust, whereas a finer distinction by ethnic or national origin (defined by ten groups) does not correlate with trust. Roughly the same holds for participation in social activities and groups, which constitutes another component of social capital (Alesina and La Ferrara, 2000).

More recently, *Constrict theory* has risen to notoriety, largely on the back of Robert D. Putnam's 2006 Johan Skytte Prize lecture entitled 'E Pluribus Unum', in which he argues that increased ethnic diversity leads to individual isolation and anomie, resulting in lower bonding and bridging capital. Putnam's hypothesis is supported by empirical evidence from a large dataset from communities across the United States (Putnam, 2007). However, subsequent studies have found mixed results; the most notable critique of constrict theory is by Sturgis et al. (2011), who highlight, among other weaknesses, the unsubstantial (if statistically significant) effect sizes in Putnam's results. Moreover, negative effects of ethnic diversity on trust seem to be predominately found in older residents; among younger age cohorts, the effect has been found to disappear or even reverse (Stolle et al., 2008; Sturgis et al., 2014). Recent field work by Espinosa et al. (2015) also concludes that the impact of diversity on cooperation and efficiency is strongly context-dependent.

Three important caveats to this literature, all of which we address in the current study, are as follows. First, as Sturgis et al. (2014) notes, much of the empirical evidence from large populations conflates diversity with segregation, such that actual exposure as assumed in contact theory is minimal or absent. On the other hand, small rural towns, such as our case study, have high rates of intra-community contact and by their nature prevent the formation of segregated ethnic neighbourhoods. Secondly, the empirical results are subject to endogeneity in that the localised migration decision is likely to be correlated with localised social capital. Dahlberg et al. (2012) suggest that this correlation, as well as failing to account for omitted variables, can lead to biased estimates of causal effects. In our case study, the particular circumstances of the resettlement allow us to discount both self-selection and labour competition effects. Finally, the literature largely relies on self-reported survey measures of trust and trustworthiness, which can be susceptible to several well-documented drawbacks (see Sapienza et al. (2013) for a good summary). For example, respondents of non-incentivised questions about trust, particularly trust towards different ethnicities, may feel some influence towards answering closer to particular societal norms with regard to specific ethnicities.

A more robust measure of social capital is the trust game of Berg et al. (1995), popular in the experimental economics literature. This method has the advantages of being incentivised and therefore arguably eliciting an objective and more continuous measure of trust from participants. The trust game has been used extensively both in and outside the lab as a tool to test for discrimination based on ethnicity. Fershtman and Gneezy (2001) is a prominent example showing that the male Israeli Jewish society systematically mistrusts - and thereby discriminates against - male Jews of Eastern origin. In a similar experiment involving Australian students, Guillen and Ji (2011) find evidence of taste-based discrimination of (male) domestic students towards international students. In Europe, Falk and Zehnder (2013) find that Zurich's population trusts fellow citizens from certain districts less than others. The discrimination is found to be based on actual statistics, and decreases in the socio-economic status and increases in the degree of ethnic heterogeneity of the district. By their design and implementation method Falk and Zehnder (2013) is similar to our study as they mail-out surveys that include the trust game decision in a Western country. Most recently, Cox and Orman (2015) use a variant of the trust game to investigate bonding and bridging social capital in the US for first-generation immigrants and native-born Americans as a measure of immigrant assimilation. In a similar conclusion to Fershtman and Gneezy (2001), they find that both native-born and immigrant Americans send less to immigrants in the Moonlighting game.[30]

While both general and forced migration typically increase ethnic diversity in the receiving community, systematic differences with respect to education, wealth and other dimensions are potentially relevant to social capital. *A priori*, it is unclear to what extent the results of the ethnic diversity literature apply to refugee resettlement, although it seems a reasonable base on which to draw hypotheses if one considers refugees as a special case of first-generation immigrants. The literature has paid surprisingly little attention to the social effects of migration by this specific subpopulation. Dahlberg et al. (2012) exploit exogenous variation from a Swedish refugee placement policy between 1985 and 1994 to measure the effect on social preferences for redistribution, but their finding of a negative relationship has

[30]The Moonlighting game is similar to the trust game but allows for negative amounts to be 'sent' or 'returned'.

been contested by Nekby and Pettersson-Lidbom (2017).[31] Bell et al. (2013) look at the effect on crime of the influx of asylum seekers from Iraq, Afghanistan and Somalia to Great Britain in the late 1990s/early 2000s. They observe a small rise in property crime and attribute this to the fact that the asylum seekers had very limited labour market opportunities. The authors' conclusion highlights the importance of omitted variables such as labour conditions in isolating the direct social effects of refugee migration. Since in our case study the refugees relocated only after they had found employment in the host community, it remains an interesting question to see how social capital behaves.

The overview of the literature landscape suggests a need for a robust test of the direct social effects of refugee resettlement. Moreover, the methodological contentions of past studies of ethnic diversity motivate our choice of case study. We next present the background, design and procedure of our incentivised survey in Section 3.2 and introduce our data in Section 3.3. We then detail and discuss the results in Sections 3.4 and 3.5. Section 3.6 concludes.

3.2 Methodology

Our case study is Nhill, a small Australian town situated in rural, north-western Victoria. More than 350 km separate the town from the closest major cities of Melbourne and Adelaide (Figure 3.1). Nhill has a population of roughly 2,300 and is the administrative centre of its local government area (LGA), the Hindmarsh Shire (total population: 5,800). At the 2011 national census, unemployment was recorded as 3.1%, significantly below the state (5.4%) and national (5.6%) levels. In the two decades prior to the refugee resettlement, the Shire's population had experienced a declining trend of between 1-2% per year. Excluding refugees, 90% of the population are of Anglo-Saxon heritage and roughly the same proportion were born in Australia. The two major industries are agriculture (grain, meat) and health care.

The dual conditions of low unemployment and declining population are representative of many rural Australian towns of similar size. For Nhill, this led to an unfilled demand for low-skilled labour by the major employer, a large poultry business called Luv-a-Duck[32]. In 2009, after exhausting all local and interregional recruitment options, the employer established contact with *AMES Australia*[33], an NGO specialising in settlement services for newly-arrived refugees and migrants, with a view to employee recruitment. The NGO acted as an employment broker and approached the Karen refugee community in Melbourne.

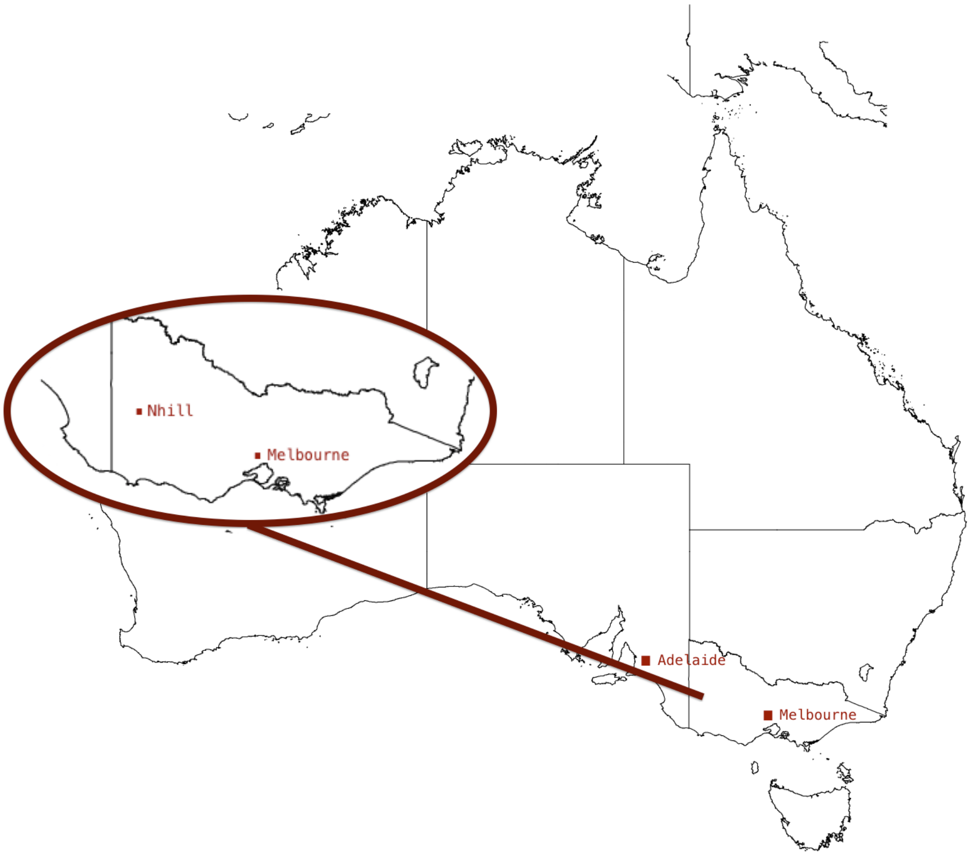
The Karen people are an ethnic minority from Myanmar. A prolonged, violent con-

[31] For the reply to the critique, see Dahlberg et al. (2013).

[32] See `http://www.luvaduck.com.au`.

[33] See `http://www.ames.net.au`.

Figure 3.1: Location of Nhill in Victoria, Australia



flict with the Burmese army has created around 400,000 Karen refugees, most of whom are housed in UN camps on the Myanmar-Thai border until they are resettled by other countries, such as Australia. In late 2009, *AMES Australia* invited Karen refugees in Melbourne to relocate to Nhill for jobs at Luv-a-Duck. There was immediately an oversupply of interest and in early 2010, five refugee workers and their families relocated to Nhill. Within the next two years, over fifty Karen took up jobs at the Nhill Luv-a-Duck factory, resulting in a total resettlement of approximately 150 refugees. At the time of writing, the Karen community numbers approximately 200 people, or about 8% of the township.

We argue that the conditions of the resettlement amount to a refugee migration shock that is exogenous with respect to social indicators of the local Nhill population. We support this claim by evidence from targeted interviews of all key stakeholders, from which the main findings are as follows:

- The initial recruitment proposal by the employer was driven by pure business motivations, as was verified by its initiator, the then-General Manager of the company. In an interview we conducted, he explained that the company's management did not consider the Nhill community especially "welcoming" or otherwise distinct from neighbouring townships, and its board initially had concerns as to how the relocation would be perceived by the local residents. Historical data of social indicators in Nhill and surrounding regions also support the claim that the residents are not unusual in any respect; we discuss this in more depth below in the description of the data.

- On the refugee side, the NGO's management confirmed that the Karen employees had had no exposure to the Nhill community before volunteering for the work relocation programme, and opined that the decision to resettle in Nhill was driven solely by the job opportunities for all refugees and their families.
- Interviews with the local Council and community leaders made clear that the extremely low unemployment situation in Nhill was such that all able-bodied residents who had wanted employment could have attained it, at least at the low-skilled level of the Luv-a-Duck factory. From this, we conclude that the refugee resettlement did not impinge on labour competition, and certainly not to the tangible levels usually assumed in the context of forced migration.[34]

The elements of the Luv-a-Duck worker programme lead us to conclude that the refugee resettlement treatment to Nhill was exogenous, conditional on the town's economic and demographic situation. In Section 3.4.3 we check the robustness of our results to the exogeneity assumption by weighting the control sample in such a way that pre-treatment differences in social indicators are minimised.

Our case study represents a rare natural shock that allows us to examine the social impact of refugee resettlement on host communities. We exploit the exogeneity of the migration shock to test for social capital effects in the absence of increased labour competition. We are particularly interested in changes of native inhabitants' relative trust towards refugees as a result of exposure to the treatment. To measure these effects, we administer an incentivised survey.

3.2.1 Design

The incentivised survey mimics the 2×2 design of a laboratory experiment, in that we administer it in both the treated and control towns and vary whether we elicit trust towards a

[34] A further insight from the interview investigations is that practically all local residents had had exposure to the refugees in the community chiefly due to the town's small size and geography. This may also explain the absence of segregation stemming from ethnic (or refugee) 'enclaves' that are often found in large cities.

refugee or towards an Australian resident from a respondent.[35] Control towns are selected from all rural towns in Victoria on their similarity to Nhill along levels of population, unemployment and per-capita gross regional product. As an incentivised measure of trust, we adopt the trust game of Berg et al. (1995). In the survey, respondents are informed that their answers will be paired with the answers of an anonymous partner from a different local government area (LGA).

The Trust Game incentivises the respondents to reveal their levels of trust towards their paired partner in the following way: All respondents play as the 'Sender' in the pairs and are informed whether their partner is an Australian resident or refugee.[36] The respondent is told that she and her partner are initially endowed with AUD \$40 each (AUD \$1 $\approx$ US \$0.75; henceforth all amounts in AUD). The respondent can choose to send some of her endowment to her partner, which is tripled by the experimenter. The 'Returner' then has the opportunity to send some amount back to the Sender, after which both players' accounts are closed and the game ends. Respondents are informed that one in ten participants will be paid out their Trust Game earnings plus \$100. This allows us to collect an incentivised measure of in-group and out-group trust from the sending choices of the respondents. In addition, we also collect the Senders' expectations about Returner behaviour.

Trust measures are compared between respondents, controlling for background characteristics. Our primary outcome of interest from the incentivised survey is relative trust, defined as the difference between natives' average trust levels toward refugees and their trust levels toward other Australians. The treatment effect is then obtained by taking the difference in relative trust across treatment and control towns. Conditional on background characteristics, our regression equation at the individual level takes the form:

$$Trust_i = \beta_0 + \beta_1 town + \beta_2 partner + \beta_3 (town \times partner) + \boldsymbol{\beta}.\boldsymbol{X_i} + \epsilon_i \tag{3.1}$$

where $Trust$ is the amount sent in the Trust Game, $town = 1$ for individuals from the treated town and 0 otherwise, $partner = 0$ for an Australian resident partner and 1 for a refugee partner, and $\boldsymbol{X_i}$ is a vector of exogenous demographic regressors. The interaction regressor β_3 provides an estimate of the treatment's effect on between-sample relative trust towards refugees.

The method mimics a standard difference-in-differences framework, with the particularity that instead of repeated measures over time we use relative outcomes for two groups. An advantage to the direct between-sample social capital comparisons is that the underlying

[35]'Australian resident' is an official residency category in Australia that includes either a citizen or permanent visa holder, and excludes refugees or others on humanitarian visas.

[36]For budgetary and power reasons, we collected a smaller number of responses from 'Returners' (both from Australians and refugees) for matching purposes only.

assumption for the above regression 'only' requires that relative trust towards refugees and Australians was not different in treated and control towns in the absence of the refugee resettlement. The treatment's effect on relative trust is also relevant as a test of the different theories of the relationship between ethnic diversity and social capital, detailed above.

Control towns were selected to be as similar as possible to the treated town along structural dimensions that drive the outcomes of interest, so that any difference in observed outcomes is attributable to the shock experienced through treatment (Abadie et al., 2015). Data on humanitarian visas provided by the Australian Department of Social Services allowed us to restrict our sample to those Victorian LGAs that housed no refugees at the time of the treatment. Rural location, population size and the economic situation are the structural variables that then determined our 'donor pool' of control towns, motivated by the trust game literature and on account of the conditionality of treatment allocation on these factors. In Section 3.3, we show that in addition to these structural determinants, the demographics across treated and control towns are reasonably balanced in terms of age distribution, family structure and education.

In the first step towards determining the donor pool of control towns, we minimised a weighted sum of squared differences in population size, unemployment rate and per-capita gross regional product (GRP) of all rural Victorian LGAs with respect to Hindmarsh (the LGA of Nhill).[37,38] In combination with the data on humanitarian visa holders, this mechanism selected control towns from the following rural LGAs: Buloke, Corangamite, Gannawarra, Indigo, Mansfield, Moyne, West Wimmera and Yarriambiack.

The selected control areas are among the smallest LGAs in population size and host no more than 16,000 residents (Moyne; 0.27% of the state). All are in similarly rural locations and agriculture is also the major industry in each of these areas. The GRP contribution to all of Victoria ranges from 0.1% (West Wimmera) to 0.35% (Corangamite) with average GRP per capita ratios around $50,000. Like Nhill, most areas have faced slight population decline over the last fifteen years and have low unemployment rates at around 4%.

The trust game is the standard tool for measuring social capital in experimental economics as it provides objective and incentivised measures for the researcher. Notwithstand-

[37]The Australian Bureau of Statistics produces annual numbers of the estimated resident population (ERP), the Department of Employment (federal government) models unemployment at a local level in every quarter, and the National Institute of Economic and Industry Research (NEIR) uses micro-simulation modelling to produce an estimate of local economic output. `http://economic-indicators.id.com.au` collects information from these data sources for every fiscal year.

[38]The three variables were standardised by the mean and standard deviation of the sample of Victorian LGAs in order to bring them on a comparable scale. A regression of pre-treatment social capital variables (at the LGA-level) on population size, unemployment and GRP per capita determined approximate weights that were given to these factors in the minimisation. The difference in population received a weight of 0.4, the difference in unemployment a weight of 0.4 and the difference in GRP per capita a weight of 0.2.

ing, there is some debate in the literature about its use compared to non-incentivised survey methods of trust (e.g. Glaeser et al., 2000; Sapienza et al., 2013). Consequently, we also administer a questionnaire with several standard survey questions on social capital and community involvement in order to compare the consistency of our incentivised measures with direct and non-incentivised survey measures indicators of trust. To cater to our specific context of refugee resettlement, the questionnaire also includes one question about a subject's attitude towards general refugee resettlement in Australia. Each subject completes the Trust Game and questionnaire together in what we call the 'survey', a copy of which is found in Appendix 3.B.

Being able to compare incentivised with non-incentivised trust measures provides an additional advantage in our setting. A key assumption of our design is that before the influx of refugees treated and control towns measured equally in terms of relative trust towards refugees. Having established a correlation between the incentivised and non-incentivised measures of trust, we can test for pre-treatment differences between treated and control towns by looking at survey data collected before the time of treatment. The Victorian Department of Health[39] makes such data, representative at LGA-level, available for the years 2008, 2011 and 2014 in the *Victorian Population Health Survey (VPHS)*. Using the data from all three years, we can thus both compare treated and control towns in terms of social capital before the influx of refugees, and substantiate our analysis by investigating longitudinal effects of the treatment in a classical difference-in-differences framework. Figure 3.2 shows the chronology of the datasets available to us in relation to the treatment period.

Lastly, in an effort to rule out channels through which treatment effects could potentially be mitigated, we collected a variety of data from other sources in addition to those mentioned above. We draw on data from the Crime Statistics Agency (CSA) who publish annual crime statistics for Victoria, and on regional internal migration estimates from the Australian Bureau of Statistics (ABS) in order to explore further potentially important impacts of the refugee resettlement.

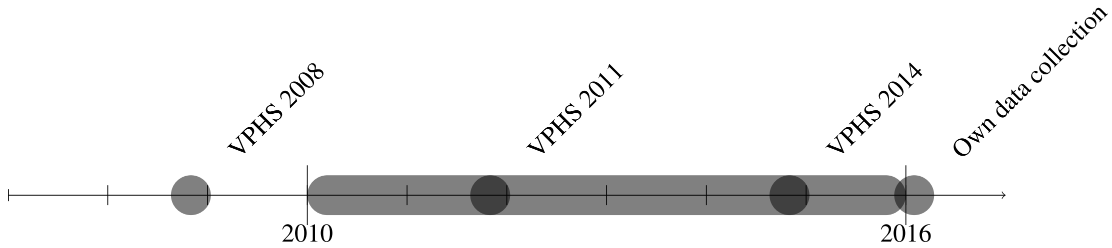
3.2.2 Procedure

Respondents were recruited by mail and in person by way of an invitation letter.[40] The letter invited the recipient to participate in a survey of trust in rural Victoria. It stated that the survey was anonymous, online and took ten to twenty minutes to complete. It also informed the householder that one in ten participants received a prize of between \$100 and \$260, with

[39] As of 2015, the Department for Health and Human Services.

[40] For an excellent discussion about representativeness and other features of mail-based procedures in experiments, see Holm and Nystedt (2005).

Figure 3.2: Chronology of data sets



Note: The shaded area between 2010 and 2016 represents the duration of the treatment affecting our estimation.

the exact amount depending on their score in the Trust Game. Each letter contained a link to the online survey as well as a personal access code. Recipients who did not have access to the internet could contact us to receive a paper version.

In addition, the letter accommodated several other features to pre-empt suspicions or concerns from rural householders receiving an unexpected letter from a European university. It included a statement that the householder's local council had been informed of the study and that the research was being assisted by a well-known Victorian NGO[41] and had ethics approval. We stressed the anonymity of their answers and clearly stated that their responses would be used only for scientific purposes. The letter also included a clipping from one of the local newspapers of the householder's area about the study, and contained email and (Australian) phone details so that the recipient could contact us.[42] Finally, the letter finished with a page of frequently asked questions that were again aimed at reassuring recipients of the legitimacy of the research. Appendix 3.B contains a copy of a typical invitation letter for an LGA.

The letters were mailed to random control and treatment town addresses sourced from a public residential address directory. About 10,000 letters of invitation were sent, of which at least 1,000 were not received by the householder.[43] From the delivered and opened letters, 397 individuals completed the survey. Despite several measures to assure householders of the study's legitimacy, we encountered a much lower response rate to our invitation letters than in similar mailed-out experiments in other countries.[44,45] We therefore also distributed the invitation letters by hand in some participating towns. The procedure for hand-distribution

[41] *AMES Australia*.

[42] Many householders made use of the contact details to assure themselves of the study's legitimacy.

[43] We received back approximately 1,000 letters marked 'Return to sender' either because of an incorrect address or deceased householder. An unknown proportion of letters were undelivered and not returned or were delivered and discarded without opening, and so the true response rate is indeterminate.

[44] E.g. Holm and Nystedt (2005) report a response rate of 33%; Falk and Zehnder (2013) report 25%. In contrast, our response rate to the mailed invitations was roughly 4%, both in the treated and control samples.

[45] We later learned that rural towns in Victoria had been targeted in past years by a number of scams, including scams by mail, and even one originating from Amsterdam. While this is unfortunate, there were no regional differences in response rates and so any selection effects should be mitigated between samples.

was to set up a table in the main street with signs advertising a survey of trust in rural Victoria with cash prizes. 103 surveys were completed from this distribution method. In total, we collected data from 500 Australian respondents, of which 472 played as Senders in the Trust Game. The data from the 28 Australian Returners, in addition to data from 119 refugee subjects, were used primarily for matching purposes. Returners indicated their Trust Game choices using the strategy method.

Although only a small proportion of respondents were given an invitation in person, the different methods of recruitment raise concerns about self-selection and experimenter-demand effects that we address now. All participating towns that we visited only had one main street, and our table was positioned outside a central landmark, such as the (only) supermarket or post office. We therefore argue that for these small communities, our invitations delivered in person went to a no less representative sample than the mailed letters. To minimise experimenter demand bias, if people asked questions on the street, we provided no substantial information further to what was contained in the invitation letters. We also first asked people whether they had received a mailed invitation, although naturally the possibility of a small number of dishonest ‘doubling up’ cannot be discounted. Finally, there were no significant differences in the results within each treatment between subjects who received their letters by mail or by hand, or between subjects who completed the survey online or on paper, or the combinations of these features. This is consistent with previous studies that have found no influence of these different methods on trust behaviour (Holm and Nystedt, 2008).

As mentioned, the survey consisted of two parts: the Trust Game and a questionnaire. First, respondents read an explanation of the game along with some examples. Next, they found out whether their randomly chosen partner was an Australian resident or a refugee, and then were asked to enter their chosen amount to send. They could send an amount between \$0 and \$40 in multiples of \$5. Respondents were also asked their expectation about the Returner’s choice. In the second section, respondents answered demographic questions, standard (non-incentivised) survey questions about trust and other social measures, and indicated their attitude towards refugee resettlement in Australia on a sliding scale from 0 to 100. Finally, respondents entered an email (or, for the paper surveys, a postal address) for the prize draw and indicated whether they wanted to be contacted for their results after we matched their answers to their partner’s choices. Prize winners could choose to be paid by check, bank transfer or PayPal.

3.3 Data

Because our research design exploits a natural shock, we first present balancing tables to compare our respondent samples along demographic variables as a check for selectivity along observable characteristics. We provide additional evidence of internal validity at the town level by examining pre-treatment population statistics on social indicators.

Table 3.1 gives an overview of the background characteristics of respondents in control and treated towns and shows the p-values for tests of equal means in the two samples. While there are a few differences between the samples, they are statistically not significant at $\alpha = .05$. The control sample is made up of more male respondents than the treated sample and has a higher median age group. We control for these differences in our analysis, as previous research suggests that age may be negatively correlated with trust in a context of high ethnic diversity (Stolle et al., 2008; Sturgis et al., 2014). In both control and treated towns approximately 90% of respondents report being born in Australia, while respondents from the treated town are on average slightly lower educated.

Apart from the gender and age group imbalances, the overall small differences in background characteristics between the treated and control towns are not exceptional when we compare the means to those in the population-adjusted VPHS dataset from 2014 (the most recent population-weighted dataset available). Moreover, this comparison validates that random sampling was successful and that our dataset is representative of the general population in the relevant areas.

To quantitatively substantiate the independence assumption of our case study, we briefly analyse the pre-resettlement VPHS data. Table 3.2 shows summary statistics of survey measures (non-incentivised) of social capital for both treated and control towns. Reported are the unconditional estimates of means and standard deviations using sampling weights, which account for the individual probability of being sampled and additionally restore representativeness at the LGA-level in terms of gender and age groups.[46] In addition, we show the p-value for a hypothesis test of equal means.

At the end of 2008, less than a year before the first refugees arrived in Nhill, 86.6% of the respondents from control towns and 87% of respondents from Nhill answered positively to the question ‘Do you agree that most people can be trusted?’. The statistical equivalence of the proportions in the two samples also holds when treating the general trust question as a continuous measure (4 answer categories: ‘no, not at all’, ‘not often’, ‘sometimes’, ‘yes,

[46] For control towns, using sampling weights gives an accurate depiction of the population in control areas since the entire LGAs were included. Nhill represents about half of the population of the Hindmarsh Shire, the LGA for which sampling weights assure representativeness. Given that we have no means to construct better weights and that Nhill constitutes by far the largest settlement in the area, we can assume that the use of sampling weights is still better than using no weights at all.

Table 3.1: Background characteristics

	Own data		
	Control	Nhill	p-value
male	0.465	0.382	0.131
	(0.499)	(0.488)	
age group (median)	55-64yo	45-54yo	0.051
born in Australia	0.877	0.902	0.459
	(0.329)	(0.299)	
single/couple with child(ren)	0.721	0.725	0.930
	(0.449)	(0.448)	
Education status			
less than Y12 or equivalent	0.246	0.314	0.186
	(0.431)	(0.466)	
completed high school	0.158	0.157	0.972
	(0.365)	(0.365)	
vocational qualification	0.236	0.255	0.699
	(0.425)	(0.438)	
university degree	0.359	0.275	0.094
	(0.480)	(0.448)	
Pearson's χ^2 (Education status)	$= 3.249$		0.355
Observations	398	102	

Note: Unconditional means, estimated population standard deviations in parentheses. The p-value corresponds to a Mann-Whitney test of equal medians for the variable age group and to a t-test of equal means for all other variables. The second-to-last row reports Pearson's χ^2-test of equal composition of education status.

definitely'). Unfortunately, the survey does not contain any measure of trust towards other ethnicities or refugees in particular, such that we cannot test for pre-treatment differences across samples for our main outcome. However, when being asked 'Do you think that multiculturalism makes life in your area better?', respondents from both samples again answer similarly positively and there is no statistical difference between treated and control towns.[47]

Other dimensions of social capital show either minimal or no difference between treated and control towns pre-resettlement. While residents from Nhill appear to have been slightly more active (volunteering, club membership and attendance of community events), this does not find expression in higher perceived or real community strength (feeling valued by society, having a say, help from neighbours, feeling safe at night). We further tested whether the social indicators prior to the resettlement of refugees were jointly significant for predicting the town of the respondent. Table 3.3 shows that this is not the case.

Despite a small number of statistically significant differences in matters related to so-

[47]The list of all variables used from each data source can be found in Table 3.A1 in Appendix 3.A.

Table 3.2: Social capital indicators before resettlement

	VPHS 2008		
	Control	Nhill	p-value
general trust	0.866	0.870	0.902
	(0.337)	(0.440)	
attitude towards multiculturalism	0.849	0.859	0.772
	(0.358)	(0.349)	
feel safe at night	0.894	0.883	0.689
	(0.304)	(0.423)	
volunteer	0.574	0.649	0.065
	(0.489)	(0.623)	
club membership (count)	1.299	1.484	0.032
	(1.128)	(1.380)	
feel valued by society	3.451	3.490	0.586
	(0.834)	(1.095)	
attend community event	0.741	0.829	0.004
	(0.433)	(0.491)	
feel have a say	3.283	3.358	0.323
	(0.944)	(1.144)	
help from neighbours	3.486	3.477	0.902
	(0.924)	(0.875)	
Observations	3575	187	

Note: Unconditional means using sampling weights, estimated population standard deviations in parentheses. The p-value corresponds to a t-test of the hypothesis that the two samples have equal means. For some variables the number of observations deviates due to missing or not applicable.

cial activity, the overall picture that emerges confirms our assumption that Nhill was by no means special in terms of social capital when the decision was made to attempt the refugee resettlement. Under this assumption our estimates are internally valid and can be considered causal. We take note of the potential concern, however, and provide an alternative estimation based on a synthetic control group as a robustness check in Section 3.4.3, from which the main conclusions remain unchanged.

3.4 Results

We present the results on social capital in two parts: the Trust Game measures on relative trust of refugees and the results from the direct survey questions. We then briefly describe checks for robustness using a synthetic control group.

Table 3.3: Test for joint significance

	VPHS 2008	
Treated town	β	s.e.
general trust	-0.003	(0.010)
attitude towards multiculturalism	0.002	(0.009)
feel safe at night	-0.002	(0.012)
volunteer	0.005	(0.008)
club membership (count)	0.002	(0.004)
feel valued by society	-0.002	(0.005)
attend community event	0.015*	(0.008)
feel have a say	-0.001	(0.004)
help from neighbours	-0.003	(0.004)
constant	0.038**	(0.017)
Observations	2,046	
R-squared	0.003	
F-statistic	1.537	
p-value	0.129	

Standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$
Note: Weighted linear regression accounting for survey design. Social indicators prior to the resettlement of refugees are not jointly significant for predicting the town of the respondent.

3.4.1 Relative trust of refugees

Across all respondents, Senders in the Trust Game sent an average of \$27.49 (sd: \$10.9) from their endowment of \$40. Respondents in the treated town sent on average less to Australian partners (\$25.30 (sd: \$10.5)) than did Senders in control towns (\$28.32 (sd: \$10.8)), and a slightly larger amount to refugee partners (\$27.50 (sd: \$11.3) versus \$27.31 (sd: \$10.9) in control towns)[48]. Between towns, the difference in trust of Australian partners is statistically marginally significant ($p = .08$); trust of refugees is statistically equivalent between towns ($p = .91$). This pattern in the data does not exclude the possibility of lower bonding capital after the treatment, but the equivalence of bridging capital is not consistent with conflict or constrict theories for social capital. Within town, the differences between trust to either partner type are not significant, though their directions are in line with a positive contact explanation.

Our main result is that treatment led to an increase in relative trust of refugees. This effect is contrary to the predictions of both conflict and constrict theories. After controlling for demographic characteristics, Nhill residents trust refugees with on average \$4.75 more than Australian partners, when compared to residents from the control towns. The marginal

[48]Figure 3.A1 in Appendix 3.A shows histograms of amounts sent by treatment.

effect of higher relative trust of refugees due to the refugee shock is in the order of 12%. A test of statistical significance of the difference-in-difference estimation is supportive of contact theory, which stipulates that exposure to different ethnicities, or in our case refugees, leads to higher relative trust of the out-group ($p = .03$). The effect is already there, yet not statistically significant, when controlling for fewer or no background characteristics of the respondents. Table 3.4 reports the results of the estimation of equation (3.1).[49]

Table 3.4: Marginal effects of selected regressors on trust

	OLS estimation					
Amount sent	(1)		(2)		(3)	
refugee partner × treated town	3.212	(2.449)	3.885	(2.366)	4.745**	(2.409)
refugee partner	-1.012	(1.129)	-1.265	(1.098)	-1.432	(1.112)
treated town	-3.024*	(1.744)	-3.242*	(1.695)	-3.656**	(1.742)
male			1.621*	(0.973)	2.036**	(1.036)
born in Australia			3.073**	(1.505)	3.469**	(1.506)
age group			-0.940***	(0.354)	-0.984**	(0.461)
single/couple with child(ren)			2.422**	(1.108)	1.710	(1.150)
Education level (base: less than Y12 or equivalent)						
completed high school			2.201	(1.577)	2.009	(1.597)
vocational qualification			0.885	(1.367)	1.181	(1.389)
university degree			6.002***	(1.261)	5.447***	(1.396)
constant	28.324***	(0.826)	24.919***	(2.716)	26.494***	(3.238)
additional controls					✓	
Observations	472		472		471	
R-squared	0.007		0.098		0.144	

Standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Note: OLS estimation. The estimate of *refugee partner × treated town* measures the increase in relative trust of refugees from the treatment. Additional controls include: occupation and industry dummies (see Tables 3.A2 and 3.A3 in Appendix 3.A for full regression details and alternative specifications).

[49] As is typical for trust games, there were focal points in sending behaviour at 50% and 100% of the endowments across all treatments; less typically, there were relatively few subjects opting for the Nash equilibrium choice of sending nothing. Following the related experimental literature, the bunching at \$40 motivates a Tobit estimation with right-censoring, which we report in Tables 3.A4 and 3.A5 in Appendix 3.A. Marginal effects from Tobit regressions confirm our results with higher statistical significance.

An alternative way of dealing with bunching at the top of the sending distribution is to test for differences in the probability of trusting the partner with the maximum amount or with half or more of the subject's endowment. Treated subjects are approximately 20% more likely to express maximum trust when paired with a refugee rather than with an Australian partner, compared to non-treated subjects. There is no difference, however, in sending half or more of one's endowment, so the positive treatment effect at the very top must be evened out by small negative effects along the rest of the distribution (see columns (1) to (4) of Table 3.A6 in Appendix 3.A).

Result 3.4.1. *(Relative trust) Treated subjects display relatively greater trust of refugees than other Australians.*

The regression and subsequent tests also indicate a significant gender effect. Our results are broadly consistent with previous research that has found that females typically send less in trust game experiments (e.g Rau, 2012), but Section 3.4.1 will show that this only holds true for the control sample. Our estimation also controls for other demographic factors, including whether the subject was born in Australia, their age group, their level of education, family structure, occupation and industry. Of these, a person's education, especially having obtained a university degree, is a significant positive predictor of trust, which is consistent with past trust game experiments (Glaeser et al., 2000; Uslaner, 2002). Being born in Australia is associated with higher levels of trust, whereas being older is associated with trusting less.

The negative coefficient on $town$ reflects the lower sent amounts by Nhill subjects, compared with control subjects, towards Australian partners. This at first is a puzzling result in light of the similar pre-treatment social capital between samples (see Tables 3.2 and 3.3). However, when we synthetically recreate the control group using these pre-treatment predictors of social capital in the robustness checks below, this significance disappears and the point estimate also decreases (Table 3.7). Nevertheless, given that our design does not cleanly randomise treatment, we cannot exclude from our data that resettlement led to both an increase in trust of refugees and a decrease in trust toward Australians outside of Nhill.[50]

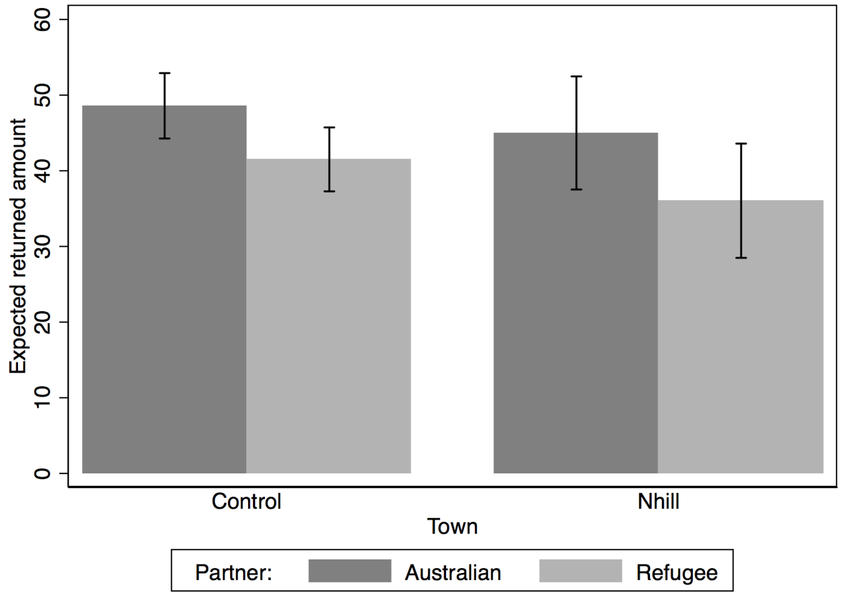
Preferences and beliefs

A criticism of the trust game is that sending amounts represent both the Sender's beliefs about the trustworthiness of her partner and her individual preferences. Sending behaviour has been found to be influenced by risk aversion (Karlan, 2005), reciprocity and altruism (Ashraf et al., 2006). Sapienza et al. (2013) show that while the trust game measures both belief- and preference-based trust, survey measures from the World Values Survey to a large extent measure only the former. They suggest measuring expectations of Returner behaviour in order to distinguish between the two concepts.

In our setting, we are interested in understanding whether the observed increase in relative trust is driven mainly by a higher belief in refugees' trustworthiness, or by a stronger preference for altruism. Figure 3.3 depicts that on average both control and treated samples

[50]Recall that in the Trust Game, subjects were told that their partner was either a refugee or an Australian *from a different area*.

Figure 3.3: Average expected return by treatment



Note: Means are of each Sender's expectations about the amount returned to them. Subjects exhibiting 'hypertrust' ($\frac{\text{Expected return}}{\text{Amount sent}} > 3$) are excluded. Overlaid error bars depict 95% confidence intervals.

expected less from refugee partners than Australian partners, and these differences are statistically significant ($p = .01$ and $.03$ for control towns and Nhill, respectively). Given the higher trust displayed towards refugees in our Nhill sample, this suggests that altruistic preferences are playing a strong role in driving our findings. This conclusion is supported when we compute the $\frac{\text{Expected return}}{\text{Sent amount}}$ ratios for each Sender who chose to send a positive amount.[51] The proportion of subjects with a ratio of less than 1 is highest by far in the Nhill-refugee treatment (23%). These subjects chose to send an amount to their refugee partner that would have led to negative earnings in the game if their expectations were realised. While we cannot rule out the role of beliefs in our data, the analysis is indicative that a preference for altruism is likely the dominant channel through which the treatment effect is operating.

Effect heterogeneity

The positive average treatment effect in Table 3.4 masks a considerable degree of heterogeneity between genders. This is best illustrated by the unconditional means of trusting behaviour by females and males with different partners (Table 3.5). The standard gender effect result in

[51] A small percentage of Senders (roughly 8%) reported expectations about return amounts that would require a partner to return back part or all of her own endowment in addition to all received earnings ($\frac{\text{Expected return}}{\text{Amount sent}} > 3$). The behaviour of these 'hypertrustors' cannot be explained by standard preferences and may be driven by comprehension errors; we discard them from a further analysis of the expectations data.

the literature whereby males are more trusting (as seen in the positive and significant gender coefficient in the results above) is only visible in the control sample, and there it does not matter whether subjects are paired with an Australian partner or a refugee. A surprising insight is that in the treated sample (i) males display a lower level of trust compared to males in the control sample but do not differ in terms of relative trust, and (ii) females show increased levels of trust of refugees when compared to females in the control sample, which does affect their overall relative trust. Judging by the unconditional means, the positive treatment effect on relative trust of refugees is therefore entirely driven by the women in Nhill. This is confirmed by repeating the estimation in samples split by gender in Table 3.6.[52] Controlling for background characteristics, treated females trust refugees with on average \$5.73 more than Australian partners, when compared to females in the control towns. They are also 35% more likely to trust a refugee with the maximum amount of \$40 rather than an Australian citizen in a sample of relatively high-trusting individuals (those who trust with half or more of their endowment; see columns (5) and (6) of Table 3.A6).

Table 3.5: Average trust by gender

Town:	Control		Nhill	
Partner:	Australian	Refugee	Australian	Refugee
female	26.98	26.65	25.48	29.67
(n)	(91)	(106)	(31)	(30)
male	29.82	28.06	25.00	24.25
(n)	(82)	(93)	(19)	(20)

Note: Means of amount sent in the Trust Game by gender and treatment group.

3.4.2 Survey indicators of social capital

All survey measures of general trust and trust of different groups[53] from our questionnaire are positively and significantly correlated with trust as measured in the Trust Game. Consistent with the results of the Trust Game (where average trust differences between towns are not significant), the survey measures of general trust and trust of different groups show no significant differences across treated and control towns. If the survey questions capture

[52] Tobit estimations of the split samples can be found in 3.A7 in Appendix 3.A. These specifications confirm that the treatment effect of increased trust of refugees can only be observed for females.

[53] These groups include ‘People you know personally’, ‘People you meet for the first time’, ‘People of another religion’ and ‘People of another nationality’.

Table 3.6: Heterogeneous treatment effects by gender

	OLS regression							
	female sample				*male sample*			
Amount sent	(1)		(2)		(3)		(4)	
refugee partner × treated town	4.510	(3.195)	5.725*	(3.158)	1.003	(3.811)	0.141	(3.709)
refugee partner	-0.327	(1.557)	-1.015	(1.544)	-1.753	(1.629)	-1.266	(1.582)
treated town	-1.494	(2.266)	-1.985	(2.238)	-4.817*	(2.738)	-4.859*	(2.649)
born in Australia			2.207	(2.105)			3.778*	(2.181)
age group			-0.867*	(0.488)			-1.177**	(0.521)
single/couple with child(ren)			3.006*	(1.539)			1.385	(1.644)
Education level (base: less than Y12 or equivalent)								
completed high school			1.427	(2.215)			3.620	(2.272)
vocational qualification			0.397	(1.928)			1.242	(1.949)
university degree			4.969***	(1.743)			7.552***	(1.846)
constant	26.978***	(1.142)	24.886***	(3.822)	29.817***	(1.188)	27.629***	(3.835)
Observations	258		258		214		214	
R-squared	0.010		0.085		0.029		0.149	

Standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Note: OLS regression results on trust, with the sample being split by gender. The estimate of *refugee partner × treated town* measures the increase in relative trust of refugees from the treatment.

mostly the belief-based component of trust as Sapienza et al. (2013) suggest, this finding corroborates our conclusion of Section 3.4.1 that altruistic preferences are a likely driver of the positive treatment effect on relative trust of refugees.

We do find significant differences on other survey indicators of social capital. Natives in the treated town are 8% more likely to volunteer at least once a month and are on average members of slightly more (0.5) community clubs and societies, and both of these results are significant.

However, a comparison with the VPHS 2008 data shows that these differences were to some extent also present before the resettlement (Table 3.2). To account for this fact, we therefore turn to the VPHS data and conduct a difference-in-differences analysis using social capital survey measures from pre-treatment (2008) and two available post-treatment waves (2012 and 2014). Controlling for time trends, there are no significant treatment effects on any of the survey measures with respect to general trust, volunteering, feelings of safety, and club membership up until 2014.[54] Figure 3.A2 in Appendix 3.A illustrates the differences between treated and control towns over time, and regressions confirm that treatment effects on measures of social capital lie around zero and are statistically not significant (Table 3.A8 in Appendix 3.A).

[54]Other indicators of community involvement as in Table 3.2 also show non-significant results.

Result 3.4.2. *(Survey indicators of social capital) Treatment does not affect reported levels of general trust, volunteering, feelings of safety, and community involvement significantly.*

The most striking result from our questionnaire is the measure of attitudes towards general refugee resettlement.[55] We asked subjects to answer the question "In general, how positive or favourable do you feel about resettled refugees in Australia?" on a scale from 0 (extremely negative) to 100 (extremely positive). Surprisingly (at least to us), both control and treated respondents indicated generally favourable attitudes towards refugee resettlement in Australia, with roughly 80% indicating a positive opinion on the scale (Figure 3.4). Subjects in Nhill reported significantly more favourable attitudes on average (72.3 versus 65.5; $p = .02$). The effect is strengthened when controlling for background characteristics, with a difference of 8.5 points on the scale ($p = .00$) and generally larger for women than for men, even though the gender difference is not significant. It is noteworthy that this result reflects broad views on national refugee resettlement, suggesting that the treatment has affected attitudes looking beyond a local level.[56]

Result 3.4.3. *(Attitudes) Treated subjects have significantly more favourable attitudes towards refugee resettlement in Australia.*

In sum, the combined results from both the Trust Game and survey questions point in the direction of a treatment effect favourable towards refugees (particularly for the female sub-population), and show no evidence of lower social capital across any of the investigated dimensions as a result of the resettlement.

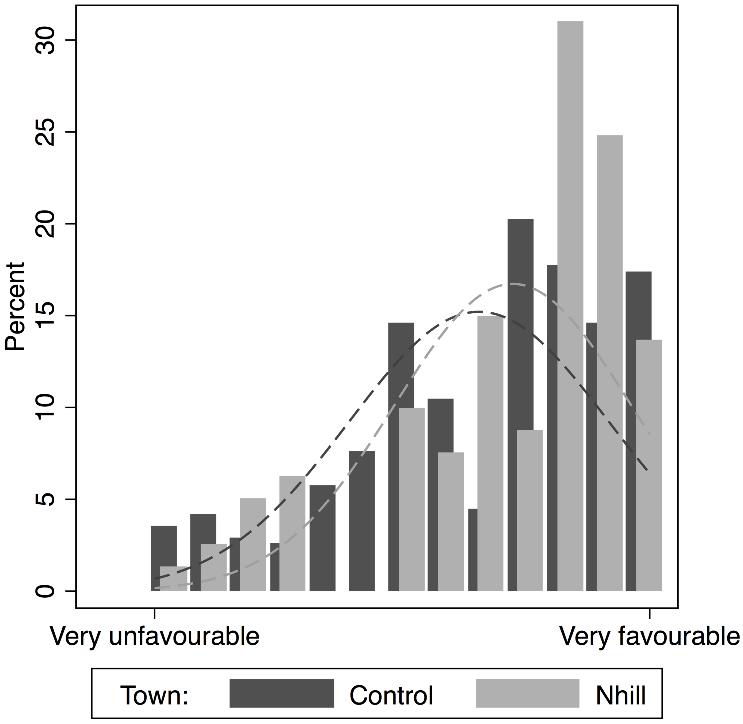
3.4.3 Robustness checks

Because the design of our study assumes that treatment is randomly allocated with respect to social indicators, our control towns were selected on the basis of demographic and economic

[55] Note that we added this question to cater to our specific context. It is not included in the VPHS surveys, and therefore allows only a post-treatment comparison.

[56] Several subjects from Nhill wrote in the feedback that they would have scored their attitude higher if it had pertained to the Nhill Karen refugees, (e.g. "our refugees"). This suggests both that the treatment difference would be significantly stronger if the question measured attitudes to local refugee resettlement, and that the treatment spillovers to broader attitudes are weaker than the localised effect.

Figure 3.4: Attitudes towards resettled refugees in Australia



Note: Histogram displays answers to the survey question "*In general, how positive or favourable do you feel about resettled refugees in Australia?*". Subjects marked a scale from 1 (very unfavourable) to 100 (very favourable). Control: $\bar{x} = 65.6, \sigma = 26.2$ ($n = 397$). Nhill: $\bar{x} = 72.4, \sigma = 23.8$ ($n = 101$). Normal density plots for each sample are overlaid. Nhill residents are significantly more favourable of resettlement ($p = .02$)

similarities to Nhill. At the time of our field data collection, the 2008 VPHS data on social measures was not available to us, but subsequent analysis revealed some small differences between the treated and control towns roughly twelve months before the resettlement (Table 3.2). While these differences are minor, jointly not predictive of treatment allocation and absent in the most important dimensions of social capital, we check for the robustness of our estimates to potential unbalancedness at the town level.

We adopt recent advances in comparative case study techniques and follow Abadie et al. (2010) in constructing a synthetic control group out of our 'donor pool' of control towns.[57] The weights obtained at the town level are as follows: Buloke 0.4022, Corangamite 0.0460, Dimboola 0.0482 (Dimboola is a town of the Hindmarsh Shire, where no refugees live), Gan-

[57]Note that because we have only one pre-treatment period available, we apply a reduced version of Abadie et al. (2010). We find weights for the nine donor control towns that minimise the difference between the weighted average of control towns and Nhill with respect to all social indicators in Table 3.2 and crime rates. (We include crime rate as an additional factor that is expected to influence trust in the population. It can also be regarded as an outcome potentially affected by the treatment. Figure 3.A4 in Appendix 3.A shows that there was no increase in crime rate after the treatment.)

nawarra 0.0797, Indigo 0.0441, Mansfield 0.0592, Moyne 0.0459, West Wimmera 0.0825, Yarriambiack 0.1922. While it is not possible to construct a perfect match of Nhill through weighting with the limited number of donor control towns, Table 3.A9 in Appendix 3.A, which is a reproduction of Table 3.2 for the synthetic control group, no longer exhibits any significant differences in pre-treatment social capital. Applying the obtained town weights to the regressions of our main outcomes confirms the robustness of our previous results (Table 3.7). In the main specification of the OLS model controlling for gender, age group, birth in Australia, education level and family status, the difference-in-differences effect of relative trust towards refugees between Nhill and the synthetic control is 5.94 (s.e. 3.57), a slight increase on our initial estimate. When the sample is split by gender, the effect for females is statistically significant at 5% with an estimate of 10.41 (s.e. 4.25) whereas the effect for males is practically zero (0.02, s.e. 5.53).[58]

Table 3.7: Marginal effects on trust with synthetic control

	OLS estimation (synthetic control)					
Amount sent	(1)		(2)		(3)	
refugee partner × treated town	5.944*	(3.573)	10.407**	(4.254)	0.023	(5.534)
refugee partner	-3.368	(2.766)	-5.331*	(3.166)	-0.081	(4.155)
treated town	-2.271	(2.422)	-1.575	(2.764)	-3.642	(3.862)
male	0.950	(1.759)				
born in Australia	3.875	(2.826)	-0.207	(2.895)	12.017***	(3.669)
age group	-0.608	(0.655)	-0.781	(0.729)	-0.507	(0.986)
single/couple with child(ren)	2.456	(2.072)	1.485	(2.385)	2.376	(2.603)
Education level (base: less than Y12 or equivalent)						
completed high school	0.647	(2.681)	1.546	(3.314)	2.220	(3.895)
vocational qualification	-0.829	(2.618)	-3.714	(2.966)	3.421	(3.957)
university degree	3.507	(2.272)	2.649	(2.439)	7.754**	(3.451)
constant	23.455***	(4.996)	29.031***	(5.520)	14.686**	(7.206)
Sample	full		female		male	
Observations	470		257		213	

Standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Note: OLS estimation using town weights for the composition of a synthetic control group. The estimate of *refugee partner × treated town* measures the increase in relative trust of refugees from the treatment. Columns (2) and (3) analyse the female and male subsamples of the full population.

[58] Table **??** in Appendix 3.A again confirms the robustness of this result to the estimation with a right-censored Tobit model.

3.5 Discussion

The results from our analysis and checks for robustness reveal a generally positive social impact of the resettlement on the host community, particularly on females. This is a surprising conclusion, especially in light of the previous research into the social effects of ethnic diversity. Controlling for economic influences as well as pre- and post-treatment sample differences, we find that locals in Nhill have developed more favourable relative trust, as well as attitudes, towards refugees as a result of exposure to the resettlement shock. Our results are in line with the predictions of contact theory of diversity and social capital. Although based on different outcomes, they also go in the same direction as a recent study that found that hosting refugees decreases community electoral support for anti-refugee political parties in Austria (Steinmayr, 2016).

One explanation for this could be that refugees may compose a systematically different population to those of regular internal or external migrations. For example, refugees are more likely to have recently been exposed to trauma and typically entail more of a short-term economic burden on host countries than economic migrants, though the long-term economic effect is positive (Parsons, 2013). As compared to other migrants, refugees are also more likely to be the subjects of media reports, though the influence of these accounts can be mixed. In addition to being refugees from war and persecution, the group resettled to Nhill is ethnically homogeneous.

It is important to stress that the positive effects found in our case study are the result of a broad treatment 'package' that comprises all of the elements of the Nhill resettlement programme. In particular, the economic environment of the town pre-treatment was such that the refugee migration did not result in increased competition for jobs. This allows us to interpret our results as pure social capital benefits in the absence of economic stress. Two arguments could be invoked against this conclusion: income gain as a driver for the observed social effects, and selective migration away from Nhill.

We briefly elaborate on these points below, and discuss further implications and limitations.

3.5.1 Evidence for contact theory

According to the interviews we conducted with a cross-section of community members, the differences in treatment effects between genders are consistent with contact theory. In Nhill women tend to mix more with the refugees on a social level (as in cooking/sewing classes or in school) rather than in a work context. We can also test whether other evidence points in the direction of this mechanism by checking an additional proxy for (presumably) more contact: residential distance. Refugees were initially temporarily housed in a large dwelling in Nhill as they searched for more permanent accommodation. Using the distance of treated subjects' homes from the temporary housing as an instrument for exposure, we test whether there is systematic variation in sending behaviour among Nhill residents towards refugees in the trust game. While our sample is substantially reduced, the evidence is consistent with a contact explanation. Nhill subjects who lived closer to the refugee housing send more to their (non-local) refugee partners in our experiment, while distance had no impact on sending behaviour towards other Australians. Regressions with and without controls confirm this direction, though the tests are too underpowered to provide statistical significance (p =0.17; see also Figure 3.A5 in Appendix 3.A).

3.5.2 Refugee characteristics

Hosting a homogeneous ethnic group of refugees constitutes a part of the treatment in our case study, and it is not obvious to what extent the homogeneity or ethnicity plays a role. To test for preexisting differences in social characteristics along these dimensions, we therefore collected data on social indicators from two refugee control groups: a sample of the same ethnic group (Karen) who did not relocate, and a sample of other refugees of mixed ethnicities[59] who also did not resettle to a rural community.

Table 3.A11 in Appendix 3.A summarises the results of this comparison. We find significant differences between the refugee control groups along some social indicators. Specifically, while control Karen are members of fewer social or community clubs, they feel safer, volunteer more and report higher general levels of trust (though this does not translate into higher trust of subpopulations). Under the assumption that the control Karen have not been affected by the Nhill Karen resettlement with respect to these indicators, we cannot rule out the possibility that pre-existent differences in social characteristics along ethnic dimensions affected the integration or reception of resettled refugees in our case study.

3.5.3 Threats to internal validity

Given the pre-treatment economic conditions of Nhill, the worker migration could have led to a potential income gain to the community through meeting the labour shortfall. While we cannot discount that treated individuals may have perceived the resettlement as reviving the community, we can look at pre- and post-treatment income data to measure realised individual income effects in these periods.

[59]The ethnic fractions of this sample are as follows: 28% Iraqi, 19% Hazara, 23% Sri Lankan, 6% Afghan, 4% Indian, 4% undisclosed, <2% from each of: Burundian, Congolese, Iranian, Italian, Japanese, Lebanese, (non-Karen) Burmese.

In 2014, *AMES Australia* and Deloitte Economic Access conducted a study about Nhill and estimated the economic benefit from the refugee influx in the four years since resettlement at a AU$41.49 million net contribution to GRP.[60] The estimate originates from an internal Regional General Equilibrium Model that takes the increase in low-skilled labour supply and the increase in demand for such labour into account (AMES DAE, 2015). The model makes clear that the contribution to GRP derives largely from benefits accrued to the primary employer Luv-a-Duck, with 54 out of 75 new employment positions created within the company. While this could have positive spillover effects that are noticeable for an individual in the host community, the model does not speak to the direct economic impact at the individual level.

Interviews conducted with inhabitants of the town revealed a strong perception that economic gains due to the refugee resettlement have not (yet) trickled down to the individual resident. This anecdotal evidence is supported by an analysis of income measures in the VPHS 2008 and 2014 data. While we do find that the income distribution in Nhill has changed considerably over time, similar changes are present for the income distributions in control areas. A regression of household income on year and town dummies and a treatment indicator, together with the usual control variables in a Mincer equation,[61] does not produce a significant effect of living in Nhill after the refugee resettlement. In contrast, both the year 2011 as well as 2014 have seen a significant increase in household income, implying that overall the income distribution has shifted to the right. This shift can also be observed in the changes in income categories in the Nhill and control sample over time (Appendix 3.A, Figure 3.A6).

Another threat to the validity of our estimates is the possibility that the Nhill population experienced selective migration away from (or towards) the town after the refugees had started resettling. Data on regional internal migration estimates from the ABS does not support this theory. There is no discernible change in trend in either net migration (Appendix 3.A, Figure 3.A7), arrival or departure rates for the Hindmarsh Shire since the resettlement programme, nor do migration trends in the treated area differ markedly from those in control areas. Our interviews with community members are consistent with these conclusions.

Overall, we find no evidence that either economic drivers or selective migration are influencing our results.

[60] Interestingly, official GRP trend data from the ABS do not show any increase over the four years since resettlement.

[61] We control for age, highest education achieved, gender, being native-born and employment status.

3.5.4 Limitations and implications

While the data from our experiment provide compelling evidence, we should be cautious in generalising the implications. A remark emblematic to case studies like ours is that the observed treatment effects may be limited to one community. This critique is not without merit, and we mention again that the treatment in our study is the resettlement programme in its entirety. We cannot say with certainty to what extent individual factors of the programme, such as community communication channels and Karen cultural norms, are significant to our findings. For instance, our case study involves a largely ethnically and religiously homogeneous group, and consequently we cannot control for the influence of these variables on our conclusions. It may be interesting for future studies to measure the impact of these factors on our observed relationship.[62]

Notwithstanding, several of the central elements of the treatment package are common to many rural communities in Australia and other host nations. Many countries that resettle refugees contain a large proportion of rural towns with declining populations and high labour demand, particularly at low-skilled levels. The fact that the mix of these demographic and economic characteristics is not uncommon to rural towns suggests that the results of this case study may generalise to other cases.

A chart from 2016 by The Economist[63] on how Germany could organise relocation of refugees shows that the pre-conditions that we identify in our case study can also be met in a country as densely populated and facing as large an influx of refugees as Germany. The graphic points to smaller rural districts, particularly those with shrinking populations, scattered around the country away from major cities, where labour demand for low-skilled workers is and will be high and where vacant housing or the space to build new capacities are available. Bevelander and Lundh (2007), an empirical paper on employment integration of refugees in Sweden, confirm that chances of successful economic integration are highest in small municipalities in the countryside.

The replicability of the Nhill case study to other potential host communities may depend on other elements in addition to those mentioned above. We conducted interviews with the Nhill local council, employers and the NGO that facilitated the resettlement, with a view towards identifying other ingredients for policy. Two principal components were identified from this qualitative research. The first is the presence of strong, centralised leadership in

[62]While most inhabitants and community leaders of Nhill who we interviewed suggested that religion was not influential for the resettlement, several proceeded to conjecture other puzzling physical characteristics as having contributed to integration success, such as the Karen's typically small physical stature.

[63]The Data Team, April 25th 2016, 17:26 `http://www.economist.com/blogs/graphicdetail/2016/04/daily-chart-8?fsrc=scn/tw/te/bl/ed/?fsrc=scn/fb/te/bl/ed/refugeesingermanymaybeseekingasyluminthewrongplaces`. Last accessed on May 2nd, 2016.

both host town and refugee communities. Direct communication channels between the Karen elders and various community leaders in Nhill (such as local council representatives and church personnel) have helped issues to be raised and resolved quickly, as well as facilitated the timely dissemination of information.

The second ingredient that emerged was a whole-of-family focus for integration. Refugee workers who resettle for employment reasons often feel that they have a place in the community through their jobs, as well as access to an immediate social network from the workplace. The same can often not be said of their non-worker family members. In Nhill there has been a conscious policy of putting in place networks and facilities to address this, including classes at local community learning centres, organised gatherings for local and Karen women, and the inclusion of Karen traditions in local festivals and events. Our investigations strongly suggest that this and the community leadership structure are important, if not necessary, conditions for resettlement programmes to generate the social capital benefits that Nhill has experienced.

3.6 Conclusion

The success of the Nhill resettlement programme is a rare positive story in an otherwise bleak topic. However, this chapter suggests that the results of this case study need not be unique. We find that the social capital benefits to Nhill do not seem to be due to any special, inimitable factor or condition. For rural towns whose employers and populations otherwise face decline, careful, guided resettlement has the potential to offer a welfare improvement to both refugees and host communities.

The replicability of the case study may depend on elements outside the scope of our analysis, so identifying other factors that facilitate refugee integration is critical. These may be less tangible but are without doubt no less important, and so mixed methods may be more suitable than the quantitative techniques conventional to economics in order to develop a policy-applicable recipe. As a first step, we have shown in this case study that there is strong empirical support for further research into the development of ‘smart resettlement’ programmes for host countries.

Appendix 3.A Additional tables and figures

Table 3.A1: List of variables

Source	Variable name	Description	Scale
own data	amount sent (trust)	The amount sent in the trust game in AUD	0 to 40, in steps of 5
own data, VPHS	general trust	Do you agree that most people can be trusted?	0 to 1; 0 = no; 1 = yes
VPHS	positive attitude towards multiculturalism	Do you think that multiculturalism makes life in your area better?	0 to 1; 0 = no; 1 = yes
own data, VPHS	feel safe at night	Do you feel safe walking alone down your street after dark?	0 to 1; 0 = no; 1 = yes
own data, VPHS	volunteer	Do you help out a local group as a volunteer?	0 to 1; 0 = no; 1 = yes
own data, VPHS	club membership (count)	Are you a member of a sports/religious/school/professional group or academic society/action group or any other community group?	0 to 5; count of categories
VPHS	feel valued by society	Do you feel valued by society?	1 to 4; 1 = no, not at all; 2 = not often; 3 = sometimes; 4 = yes, definitely
VPHS	attend community event	Did you attend a local community event in the past six months?	0 to 1; 0 = no; 1 = yes
VPHS	feel have a say	Do you feel there are opportunities to have a real say on issues that are important to you?	1 to 4; 1 = no, not at all; 2 = not often; 3 = sometimes; 4 = yes, definitely
VPHS	help from neighbours	Can you get help from neighbours when you need it?	1 to 4; 1 = no, not at all; 2 = not often; 3 = sometimes; 4 = yes, definitely
own data	trust: know personally	How much do you trust people from various groups? People you know personally	1 to 4; 1 = do not trust at all; 2 = do not trust very much; 3 = trust somewhat; 4 = trust completely
own data	trust: meet first time	How much do you trust people from various groups? People you meet for the first time	1 to 4; 1 = do not trust at all; 2 = do not trust very much; 3 = trust somewhat; 4 = trust completely
own data	trust: other nationality	How much do you trust people from various groups? People of another nationality	1 to 4; 1 = do not trust at all; 2 = do not trust very much; 3 = trust somewhat; 4 = trust completely
own data	trust: other religion	How much do you trust people from various groups? People of another religion	1 to 4; 1 = do not trust at all; 2 = do not trust very much; 3 = trust somewhat; 4 = trust completely

Table 3.A2: Marginal effects of selected regressors on trust, OLS estimation

	OLS estimation					
Amount sent	(1)		(2)		(3)	
refugee partner × treated town	3.212	(2.449)	3.885	(2.366)	4.745**	(2.409)
refugee partner	-1.012	(1.129)	-1.265	(1.098)	-1.432	(1.112)
treated town	-3.024*	(1.744)	-3.242*	(1.695)	-3.656**	(1.742)
male			1.621*	(0.973)	2.036**	(1.036)
born in Australia			3.073**	(1.505)	3.469**	(1.506)
age group			-0.940***	(0.354)	-0.984**	(0.461)
single/couple with child(ren)			2.422**	(1.108)	1.710	(1.150)
Education level (base: less than Y12 or equivalent)						
completed high school			2.201	(1.577)	2.009	(1.597)
vocational qualification			0.885	(1.367)	1.181	(1.389)
university degree			6.002***	(1.261)	5.447***	(1.396)
Industry (base: Agriculture, forestry, fishing and mining)						
Manufacturing and construction					2.625	(2.779)
Retail trade					0.509	(2.421)
Transportation, postal and warehousing					5.243	(4.011)
Public service					-0.302	(2.204)
Education					0.839	(2.043)
Health care and social assistance					-3.048	(2.077)
Hospitality and tourism					0.547	(3.400)
other/not currently employed					0.008	(2.039)
Occupation (base: Management and professional, incl. Farmers)						
Technicians and tradesworkers					-7.766***	(2.371)
Community and personal service workers					2.241	(2.019)
Clerical, administrative and sales workers					-1.704	(1.840)
Machine operators, drivers and labourers					-2.999	(2.904)
retired					-1.604	(1.970)
unemployed					-3.561	(3.414)
student					3.012	(4.181)
constant	28.32***	(0.826)	24.92***	(2.716)	26.49***	(3.238)
Observations	472		472		471	
R-squared	0.007		0.098		0.144	

Standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Note: OLS regression results on trust. The estimate of *refugee partner × treated town* measures the increase in relative trust of refugees from the treatment.

Table 3.A3: Marginal effects of regressors on trust, OLS estimation (continued)

	OLS estimation			
Amount sent	(4)		(5)	
refugee partner × treated town	3.658	(2.381)	4.650*	(2.421)
refugee partner	-1.140	(1.114)	-1.351	(1.128)
treated town	-3.020*	(1.719)	-3.486**	(1.762)
male	1.637*	(0.982)	2.062*	(1.050)
born in Australia	3.115**	(1.514)	3.501**	(1.514)
single/couple with child(ren)	2.216*	(1.181)	1.458	(1.206)
Age group (base: 16-24)				
25-34	1.205	(3.242)	1.483	(3.393)
35-44	-1.545	(3.017)	-0.942	(3.184)
45-54	-1.191	(2.842)	-0.570	(3.048)
55-64	-1.868	(2.791)	-1.862	(3.042)
65+	-3.816	(2.700)	-3.857	(3.129)
Education level (base: less than Y12 or equivalent)				
completed high school	2.271	(1.583)	2.089	(1.604)
vocational qualification	1.005	(1.375)	1.336	(1.401)
university degree	6.041***	(1.277)	5.584***	(1.412)
Industry (base: Agriculture, forestry, fishing and mining)				
Manufacturing and construction			2.387	(2.827)
Retail trade			0.365	(2.445)
Transportation, postal and warehousing			4.927	(4.036)
Public service			-0.625	(2.234)
Education			0.490	(2.077)
Health care and social assistance			-3.200	(2.091)
Hospitality and tourism			0.425	(3.427)
other/not currently employed			0.0845	(2.050)
Occupation (base: Management and professional, incl. Farmers)				
Technicians and tradesworkers			-7.832***	(2.389)
Community and personal service workers			2.152	(2.040)
Clerical, administrative and sales workers			-1.737	(1.852)
Machine operators, drivers and labourers			-3.128	(2.921)
retired			-1.340	(2.007)
unemployed			-3.763	(3.448)
student			3.748	(4.315)
constant	22.69***	(3.169)	23.88***	(3.662)
Observations	472		471	
R-squared	0.101		0.147	

Standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Note: OLS regression results on trust. The estimate of *refugee partner × treated town* measures the increase in relative trust of refugees from the treatment.

Table 3.A4: Marginal effects of selected regressors on trust, Tobit estimation

	Tobit estimation					
Amount sent	(1)		(2)		(3)	
refugee partner × treated town	5.063	(3.351)	5.960*	(3.201)	7.052**	(3.199)
refugee partner	-1.311	(1.548)	-1.800	(1.490)	-2.003	(1.479)
treated town	-4.087*	(2.365)	-4.533**	(2.274)	-5.037**	(2.290)
male			2.239*	(1.320)	2.876**	(1.379)
born in Australia			4.381**	(2.008)	4.889**	(1.972)
age group			-1.411***	(0.484)	-1.451**	(0.616)
single/couple with child(ren)			3.094**	(1.495)	1.978	(1.522)
Education level (base: less than Y12 or equivalent)						
completed high school			3.343	(2.122)	3.038	(2.114)
vocational qualification			0.966	(1.823)	1.347	(1.819)
university degree			8.435***	(1.716)	7.565***	(1.853)
Industry (base: Agriculture, forestry, fishing and mining)						
Manufacturing and construction					4.653	(3.767)
Retail trade					1.840	(3.225)
Transportation, postal and warehousing					7.418	(5.371)
Public service					0.896	(2.976)
Education					2.119	(2.751)
Health care and social assistance					-3.751	(2.748)
Hospitality and tourism					3.207	(4.632)
other/not currently employed					0.513	(2.702)
Occupation (base: Management and professional, incl. Farmers)						
Technicians and tradesworkers					-10.45***	(3.094)
Community and personal service workers					3.384	(2.755)
Clerical, administrative and sales workers					-2.521	(2.438)
Machine operators, drivers and labourers					-3.945	(3.855)
retired					-2.059	(2.611)
unemployed					-5.488	(4.488)
student					6.089	(5.865)
constant	30.954***	(1.144)	26.907***	(3.670)	28.395***	(4.282)
sigma	14.359***	(0.597)	13.603***	(0.563)	13.184***	(0.544)
Observations	472		472		471	

Standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Note: Regression results are of a right-censored Tobit model. The estimate of *refugee partner × treated town* measures the increase in relative trust of refugees from the treatment. Regression coefficients are marginal effects on the uncensored latent variable.

Table 3.A5: Marginal effects of regressors on trust, Tobit estimation (continued)

	Tobit estimation			
Amount sent	(4)		(5)	
refugee partner × treated town	5.700*	(3.208)	6.948**	(3.205)
refugee partner	-1.659	(1.508)	-1.937	(1.496)
treated town	-4.259*	(2.296)	-4.832**	(2.307)
male	2.278*	(1.327)	2.951**	(1.392)
born in Australia	4.461**	(2.010)	4.959**	(1.973)
single/couple with child(ren)	2.841*	(1.583)	1.652	(1.590)
Age group (base: 16-24)				
25-34	2.475	(4.462)	3.216	(4.569)
35-44	-2.364	(4.098)	-1.276	(4.215)
45-54	-1.725	(3.854)	-0.741	(4.016)
55-64	-3.200	(3.782)	-2.979	(4.006)
65+	-5.476	(3.660)	-5.137	(4.120)
Education level (base: less than Y12 or equivalent)				
completed high school	3.467	(2.122)	3.153	(2.114)
vocational qualification	1.139	(1.827)	1.549	(1.828)
university degree	8.537***	(1.729)	7.766***	(1.864)
Industry (base: Agriculture, forestry, fishing and mining)				
Manufacturing and construction			4.434	(3.815)
Retail trade			1.723	(3.244)
Transportation, postal and warehousing			6.851	(5.386)
Public service			0.460	(3.003)
Education			1.727	(2.780)
Health care and social assistance			-3.936	(2.757)
Hospitality and tourism			3.136	(4.638)
other/not currently employed			0.563	(2.706)
Occupation (base: Management and professional, incl. Farmers)				
Technicians and tradesworkers			-10.52***	(3.104)
Community and personal service workers			3.226	(2.766)
Clerical, administrative and sales workers			-2.551	(2.443)
Machine operators, drivers and labourers			-4.310	(3.863)
retired			-1.824	(2.644)
unemployed			-5.789	(4.519)
student			7.183	(5.975)
constant	23.467***	(4.281)	24.355***	(4.792)
sigma	13.579***	(0.562)	13.159***	(0.543)
Observations	472		471	

Standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Note: Regression results are of a right-censored Tobit model. The estimate of *refugee partner × treated town* measures the increase in relative trust of refugees from the treatment. Regression coefficients are marginal effects on the uncensored latent variable.

Table 3.A6: Sending the maximum

	OLS regression											
	(1)		(2)		(3)		(4)		(5)		(6)	
Amount sent:	40 (max)		40 (max)		40 (max)		20-40 (half or more)		40 (max)		40 (max)	
refugee partner × treated town	0.184*	(0.102)	0.203**	(0.099)	0.231**	(0.100)	-0.022	(0.082)	0.348**	(0.149)	0.069	(0.194)
refugee partner	-0.024	(0.047)	-0.037	(0.046)	-0.045	(0.046)	-0.009	(0.038)	-0.046	(0.073)	-0.028	(0.077)
treated town	-0.101	(0.073)	-0.110	(0.071)	-0.131*	(0.072)	-0.041	(0.059)	-0.106	(0.106)	-0.092	(0.134)
male			0.054	(0.041)	0.077	(0.043)	0.015	(0.034)				
born in Australia			0.126**	(0.063)	0.144**	(0.063)	0.104**	(0.052)	0.039	(0.105)	0.127	(0.115)
age group			-0.043***	(0.015)	-0.043**	(0.019)	-0.014	(0.012)	-0.048**	(0.023)	-0.056**	(0.027)
single/couple with child(ren)			0.059	(0.046)	0.032	(0.048)	0.082**	(0.039)	0.071	(0.075)	-0.019	(0.082)
Education level (base: less than Y12 or equivalent)												
completed high school			0.127*	(0.066)	0.114*	(0.066)	0.039	(0.055)	0.013	(0.107)	0.303***	(0.116)
vocational qualification			0.013	(0.057)	0.029	(0.058)	0.052	(0.048)	0.024	(0.093)	-0.038	(0.098)
university degree			0.216***	(0.053)	0.198***	(0.058)	0.105**	(0.044)	0.204**	(0.084)	0.228**	(0.091)
Industry (base: Agriculture, forestry, fishing and mining)												
Manufacturing and construction					0.199*	(0.116)						
Retail trade					0.146	(0.101)						
Transportation, postal and warehousing					0.237	(0.167)						
Public service					0.118	(0.092)						
Education					0.132	(0.085)						
Health care and social assistance					-0.062	(0.086)						
Hospitality and tourism					0.285**	(0.141)						
other / not currently employed					0.039	(0.085)						
Occupation (base: Management and professional, incl. Farmers)												
Technicians and tradesworkers					-0.275***	(0.099)						
Community and personal service workers					0.121	(0.084)						
Clerical, administrative and sales workers					-0.051	(0.077)						
Machine operators, drivers and labourers					-0.070	(0.121)						
retired					-0.006	(0.082)						
unemployed					-0.159	(0.142)						
student					0.304*	(0.174)						
constant	0.301***	(0.034)	0.231**	(0.114)	0.188	(0.135)	0.709***	(0.094)	0.367**	(0.184)	0.439**	(0.200)
Sample	full		full		full		full		female, sent 20-40		male, sent 20-40	
Observations	472		472		471		472		216		181	
R-squared	0.007		0.083		0.144		0.038		0.091		0.119	

Standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Note: Linear Probability Model with dependent variable $= 1$ if the Sender sends the maximum amount ($40) and 0 otherwise. Column (4) is an exception in which the dependent variable $= 1$ if the Sender sends at least half her endowment ($20). The absence of a treatment effect in this specification suggests that the treatment effect is concentrated at shifting moderate trustors to the maximum. The estimate of *refugee partner* × *treated town* measures the increase in relative trust of refugees from the treatment. Columns (5) and (6) analyse subsamples of the full population.

Table 3.A7: Heterogeneous treatment effects by gender, Tobit estimation

	Tobit estimation							
	female sample				*male sample*			
Amount sent	(1)		(2)		(3)		(4)	
refugee partner × treated town	7.139*	(4.314)	9.025**	(4.210)	1.840	(5.249)	0.225	(5.004)
refugee partner	-0.425	(2.080)	-1.501	(2.041)	-2.307	(2.280)	-1.721	(2.167)
treated town	-2.154	(3.005)	-3.075	(2.923)	-6.351*	(3.771)	-6.453*	(3.578)
born in Australia			2.830	(2.746)			5.596*	(2.923)
age group			-1.335**	(0.649)			-1.743**	(0.722)
single/couple with child(ren)			3.813*	(2.017)			1.647	(2.246)
Education level (base: less than Y12 or equivalent)								
completed high school			1.697	(2.882)			5.936*	(3.117)
vocational qualification			0.576	(2.518)			1.128	(2.605)
university degree			7.044***	(2.304)			10.517***	(2.543)
constant	29.115***	(1.535)	27.374***	(5.048)	32.987***	(1.690)	30.457***	(5.223)
sigma	14.118***	(0.783)	13.510***	(0.747)	14.403***	(0.903)	13.366***	(0.833)
Observations	258		258		214		214	

Standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Note: Regression results are of a right-censored Tobit model. The estimate of *refugee partner × treated town* measures the increase in relative trust of refugees from the treatment. Regression coefficients are marginal effects on the uncensored latent variable.

Table 3.A8: Marginal effects on survey indicators of social capital

	OLS regression							
	(1)		(2)		(3)		(4)	
Dependent variable:	general trust		volunteer		club membership (count)		feel safe	
post-2010 × treated town	0.025	(0.031)	-0.021	(0.052)	-0.092	(0.112)	-0.051	(0.045)
2011	0.021**	(0.010)	0.036**	(0.015)	-0.006	(0.033)	-0.006	(0.011)
2014	0.016	(0.013)	0.005	(0.017)	-0.097***	(0.036)	0.007	(0.012)
treated town	0.011	(0.026)	0.084**	(0.041)	0.264***	(0.081)	0.019	(0.031)
male	0.003	(0.009)	0.044***	(0.013)	0.033	(0.028)	0.127***	(0.009)
born in Australia	0.039***	(0.015)	0.130***	(0.021)	0.317***	(0.047)	-0.013	(0.015)
age group	0.009**	(0.004)	0.042***	(0.007)	0.091***	(0.012)	-0.033***	(0.004)
single/couple with child(ren)	-0.006	(0.012)	0.116***	(0.017)	0.366***	(0.034)	-0.005	(0.011)
Education level (base: less than Y12 or equivalent)								
completed high school	0.041**	(0.016)	0.044*	(0.023)	0.157***	(0.045)	0.033**	(0.015)
vocational qualification	0.040***	(0.012)	0.101***	(0.017)	0.263***	(0.033)	0.047***	(0.013)
university degree	0.088***	(0.012)	0.181***	(0.017)	0.926***	(0.039)	0.101***	(0.012)
constant	0.764***	(0.029)	0.144***	(0.042)	0.186**	(0.084)	0.866***	(0.029)
Observations	11,036		11,169		11,185		11,092	
R-squared	0.012		0.037		0.113		0.060	

Standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Note: Data source: VPHS. Regression results are of OLS regressions accounting for survey design of the VPHS surveys. The estimate of *post-2010 × treated town* is the difference-in-differences estimate of the treatment.

Table 3.A9: Social capital before resettlement in synthetic control

	VPHS 2008		
	Synthetic control	Nhill	p-value
general trust	0.878	0.870	0.746
	(0.327)	(0.338)	
positive attitude towards multiculturalism	0.855	0.859	0.907
	(0.352)	(0.349)	
feel safe at night	0.903	0.883	0.467
	(0.296)	(0.322)	
volunteer	0.622	0.649	0.518
	(0.485)	(0.479)	
club membership (count)	1.462	1.484	0.804
	(1.211)	(1.060)	
feel valued by society	3.498	3.490	0.910
	(0.821)	(0.841)	
attend community event	0.783	0.829	0.129
	(0.413)	(0.378)	
feel have a say	3.343	3.358	0.839
	(0.922)	(0.877)	
help from neighbours	3.543	3.477	0.359
	(0.894)	(0.875)	
Observations	3575	187	

Note: Unconditional means using synthetic control sampling weights. Estimated population standard deviations in parentheses (). The p-value corresponds to a t-test of the hypothesis that the two samples have equal means.

Table 3.A10: Marginal effects on trust with synthetic control, Tobit estimation

	Tobit estimation (synthetic control)					
Amount sent	(1)		(2)		(3)	
refugee partner × treated town	8.100*	(4.532)	13.823***	(5.114)	0.001	(7.800)
refugee partner	-3.855	(3.391)	-6.056*	(3.506)	0.101	(5.941)
treated town	-3.091	(3.058)	-1.837	(3.155)	-5.369	(5.434)
male	1.571	(2.331)				
born in Australia	4.896	(3.426)	-0.241	(3.564)	15.067***	(4.474)
age group	-0.948	(0.837)	-1.117	(0.933)	-1.011	(1.331)
single/couple with child(ren)	3.182	(2.541)	1.462	(2.849)	3.389	(3.384)
Education level (base: less than Y12 or equivalent)						
completed high school	0.454	(3.300)	1.022	(3.773)	3.338	(5.125)
vocational qualification	-1.150	(3.270)	-4.377	(3.431)	3.681	(5.475)
university degree	4.646	(3.077)	3.631	(3.234)	10.414**	(4.753)
constant	25.355***	(6.320)	31.949***	(6.974)	14.603***	(8.983)
sigma	14.265***	(0.853)	13.117***	(0.984)	16.008*	(1.470)
Sample	full		female		male	
Observations	470		257		213	

Standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

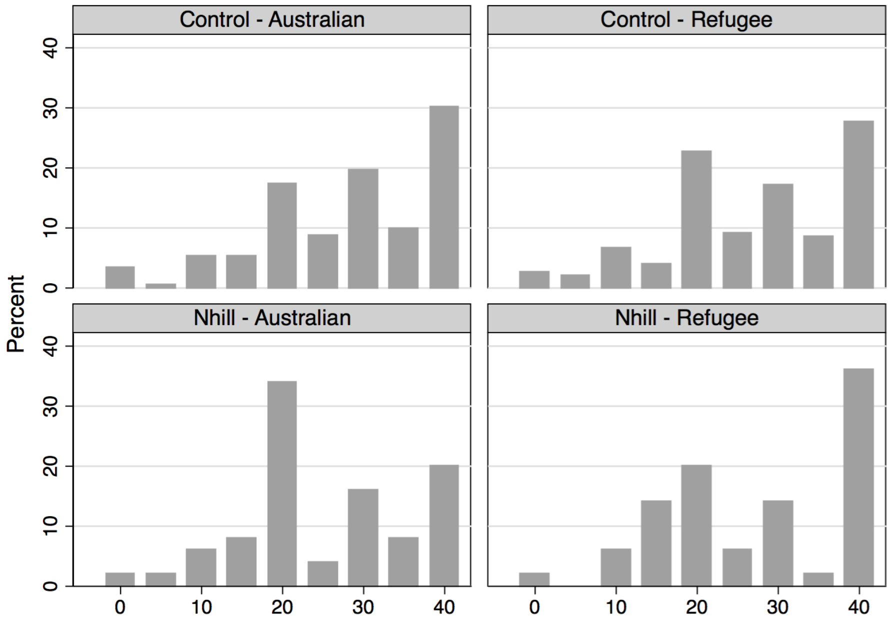
Note: Right-censored Tobit model using town weights for the composition of a synthetic control group. The estimate of *refugee partner* × *treated town* measures the increase in relative trust of refugees from the treatment. Regression coefficients are marginal effects on the uncensored latent variable. Columns (2) and (3) analyse the female and male subsamples of the full population.

Table 3.A11: Differences in social capital between refugee groups

	Control refugees			
	Mixed ethnicity	Karen	p-value	Obs. (non-missing)
safe	2.404	2.927	0.001	88
volunteer	2.317	3.326	0.000	84
general trust	0.477	0.977	0.000	87
trust: know personally	3.362	3.238	0.384	89
trust: meet for the first time	2.234	2.256	0.898	90
trust: other nationality	2.766	2.930	0.195	90
trust: other religion	2.894	2.814	0.512	90
clubs	1.304	0.800	0.017	91
attitude towards resettlement	79.511	88.533	0.001	92
Observations (Total)	47	46		

Note: Unconditional means of survey responses by refugee group. The p-value corresponds to a t-test of equal means.

Figure 3.A1: Distribution of sending behaviour by treatment



Note: Histogram samples: $n = 173$ (Control-Australians), $n = 199$ (Control-Refugees) $n = 50$ (Nhill-Australians and Nhill-Refugees).

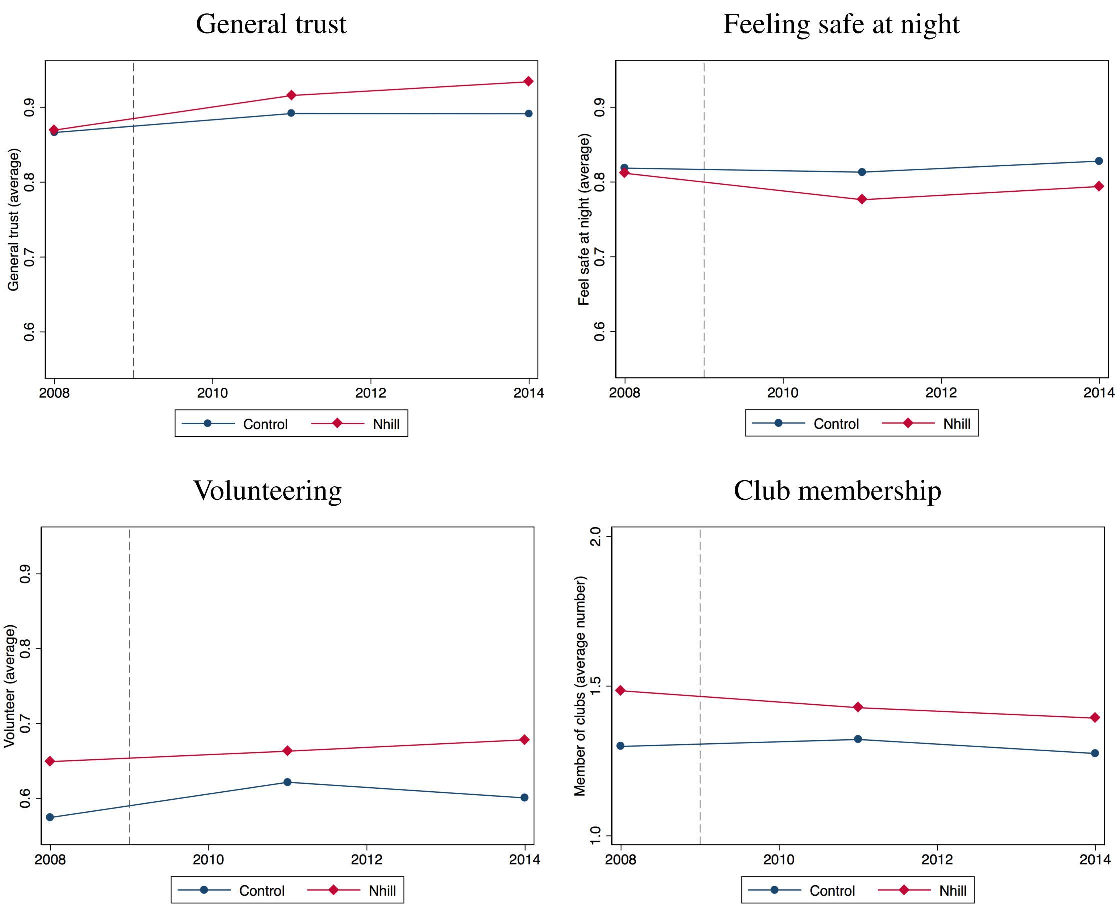
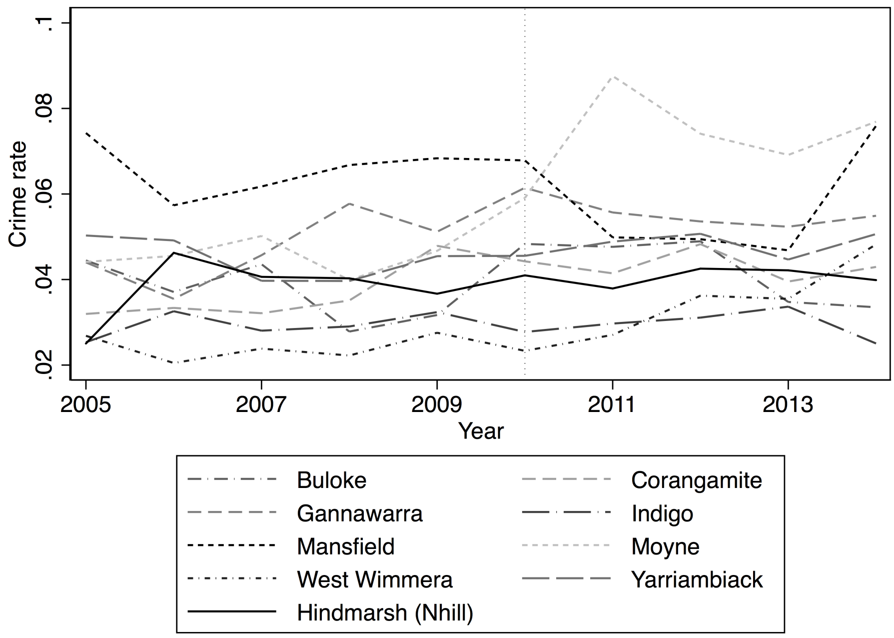


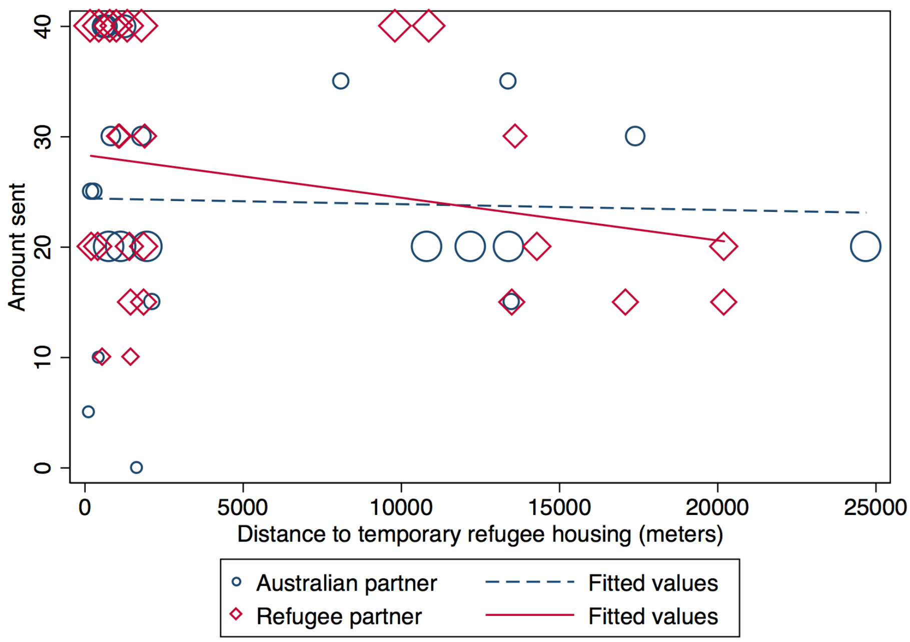
Figure 3.A2: Difference-in-differences in social capital over time (VPHS data)

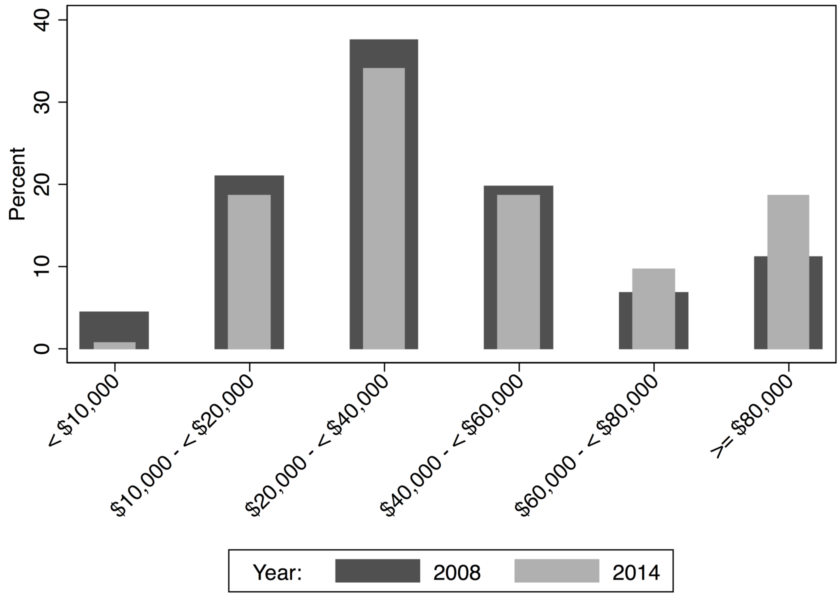
Figure 3.A4: Crime rate by LGA



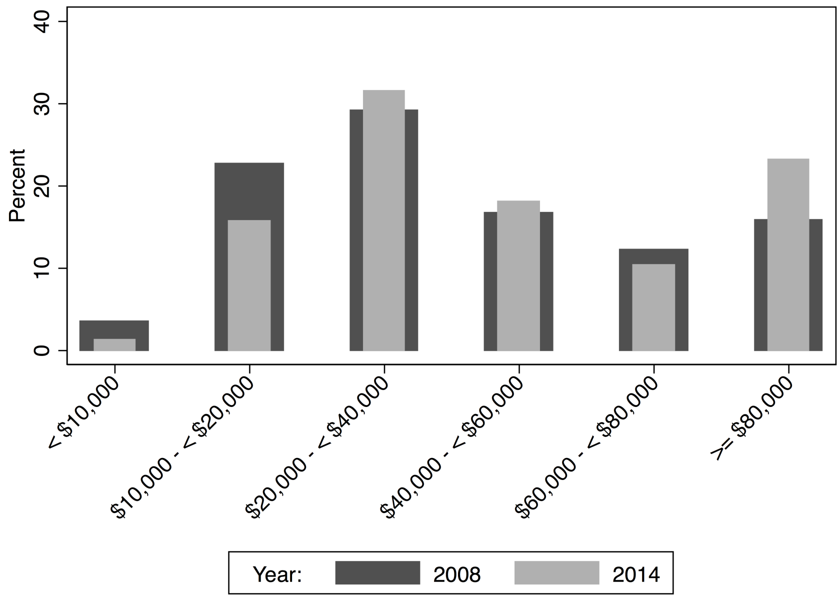
Note: The crime rate is calculated as the number of offences recorded divided by the estimated resident population in every given year.

Figure 3.A5: Trust by distance from temporary refugee housing





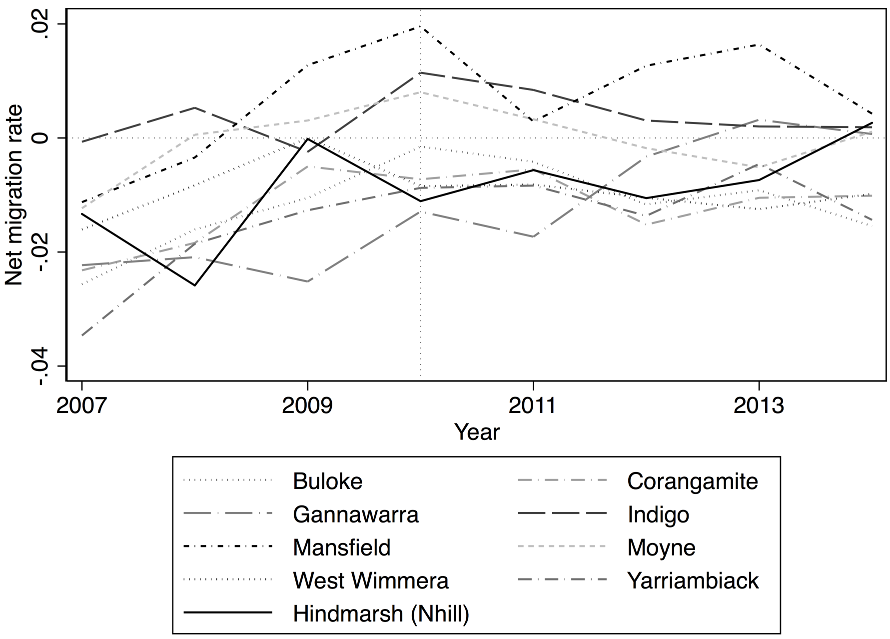
(a) Household income in Nhill



(b) Household income in Control towns

Figure 3.A6: Change in income distribution by categories

Figure 3.A7: Net migration rate by LGA



Note: The net migration rate is calculated as the numbers of arrivals minus departures, divided by the estimated resident population in every given year.

Appendix 3.B Instructions and invitation letter

UNIVERSITY OF AMSTERDAM

«FirstName» «LastName»
«ADDRESS»
«SUBURB» VIC «postcode»

Survey of trust in regional Victoria

«startdate»

Dear Buloke Shire Resident,

You have been selected to participate in a short survey on how people trust others in regional Victoria. You may have already read about the survey in various local newspapers (such as the *Gannawarra Times*) or on your community Facebook pages. Roughly 1000 Victorians, including at least 100 residents of the Buloke Shire, are taking part.

The survey is anonymous and only takes 10 minutes of your time to complete. There are two parts to the survey: A so-called Trust Game and a short questionnaire. **One out of every ten people who take part in the survey will win a cash prize**. Your prize depends on your score in the Trust Game, but is guaranteed to be between **$100 and $260.**

In addition to the good chance of winning a prize, your participation will help researchers to understand how trust functions in Australia. This survey is also supported by *AMES Australia* in the context of a broader understanding of migration in regional Australia.

The survey is hosted online by the University of Amsterdam. Participation is simple. Please carefully type the survey link below into your web browser's address bar and enter your personal access code.*

The deadline for completing the survey is «enddate».

Survey Link:	**«link_short»**
Your access code:	**«AccessCode»**

Should you have any concerns, we will be pleased to provide assistance. You can write us an email to: d.c.smerdon@uva.nl (replies within 24 hours), or phone us on 0467474485 from Monday, 1 February 2016 to «enddate» from 9.00am to 5.00pm. You can find more information via the 'News' section of the University of Amsterdam website: http://creedexperiment.nl/creed

Thank you for participating!

Sincerely,

Professor Joep Sonnemans
Department of Economics and Business
University of Amsterdam, The Netherlands

* If you do not have access to a computer or the internet, there are a number of paper surveys available. Please call us and we will arrange one for you.

Survey of trust in regional Victoria

Welcome!

The survey consists of two sections: a short game for which your decision will be paired with another random participant, and a basic questionnaire. By completing this survey, you will help researchers and policy makers to better understand how trust functions in Australia. In addition, your participation places you in a draw to win a cash prize. **One out of every ten participants will win a prize.** Your prize, if you are selected, depends on your score in the game, which is described below. Prize winners will win at least $100 each, but you have the chance to increase your potential prize to up to $260. To go into the draw for a prize, you must complete and return this survey in the enclosed, stamped return envelope by **Friday, February 12, 2016**.

In the first section, you will play a simple game known as the Trust Game. This is a standard game used by researchers to measure trust. In this game, you and another survey participant will each make a decision about how to allocate money across hypothetical 'accounts'. Your survey partner has been randomly chosen by a computer, and your decision in the game will be paired together with the decision of your partner to determine each person’s final account. The amount of dollars in your final account will be added to a guaranteed $100 and together this will determine your prize.

In the second section, you only need to fill out a short questionnaire. Please fill out the questionnaire from start to finish. You should not change your answers to section 1 after completion of section 2.

Anonymity guarantee
Your responses will be stored anonymously, and your confidentiality is completely assured. This study has approval by the Research Ethics Committee at the University of Amsterdam, and has also been approved by AMES Australia. The Victorian Department of Health and Human Services and the local councils and newspapers of all participating areas have been informed and are aware that we are conducting this survey.

In your invitation letter, we enclosed some frequently asked questions about the study and their answers. Should you have further questions, we will be pleased to provide assistance. You can write us an email to: d.c.smerdon@uva.nl (replies within 24 hours), or phone us on 0467474485 from Monday, January 25, to Friday, February 12, 2016 from 9.00am to 5.00pm.

On the next page, you will begin the first section: The Trust Game.

Section 1: The Trust Game

Approximately one thousand residents of Victoria are participating in this study. A computer has randomly divided the participants into pairs that are **not** in the same town or local government area. In each pair, one person has been randomly chosen to play the role of the **Sender** and the other to be the **Returner** for the game.

Pairs are anonymous; the only information you will know about your partner is whether they are an Australian citizen or a refugee.

In the Trust Game, you have to make a decision about money. You will use amounts of money that are in hypothetical 'accounts'. However, at the end of the survey we will draw 100 winners who will be paid out according to the choices they made in the Trust Game. Your decisions can therefore have real consequences.

The rules of the game are simple:

1. At the start, both the Sender and the Returner are given a 'game account' with **$40 each**.
2. Then, the Sender can choose to transfer some or all of the money in his or her account to the Returner.
3. Any money transferred is **tripled** by the computer and added to the Returner's account.
4. Finally, the Returner can then choose to transfer some of his or her money back to the Sender.

The accounts are then 'closed' and each person's potential prize is calculated as the amount of dollars in his or her final account, **plus $100**. And that's it!

On the next page, you can find some simple examples of how the game might work. Please note that these examples are completely made-up and only meant to help you understand how the game works. The numbers are not an indication of what your partner might do, and so they should not influence your own choices.

Before continuing, please carefully write your personalised Access Code in the box below. Without an access code, you will not be eligible to win a cash prize.

Access Code:

Example 1

Both people start with $40 in their game accounts.

The Sender chooses to transfer $30, which is tripled ($90) and then passed on to the Returner.

The Returner, who now has $130, chooses to return $60 to the Sender.

The game accounts are then closed.

The Sender's final account has: **$10 + $60 = $70**

The Returner's final account has: **$130 - $60 = $70**

(So in this example, both participants would earn a cash prize of $70 + $100 = $170 if they were drawn as winners.)

Example 2

Both people start with $40 in their game accounts.

The Sender chooses to transfer $20, which is tripled ($60) and then passed on to the Returner.

The Returner, who now has $100, chooses to return $15 to the Sender.

The game accounts are then closed.

The Sender's final account has: **$20 + $15 = $35**

The Returner's final account has: **$100 - $15 = $85**

(So in this example, the Sender would earn a cash prize of $35 + $100 = $135 and the Returner would earn a cash prize of $85 + $100 = $185 if they were drawn as winners.)

On the next page, you will find out your randomly allocated role and you can fill in your decisions.

Your Decision Sheet

The computer has made the following allocation for you:

Your role:	**SENDER**
Your partner:	**AUSTRALIAN**

As the Sender, your decision is how much to transfer to your partner, which will be tripled. Your final amount in your game account will equal $40 minus the amount you transfer to your partner plus any amount your partner chooses to return to you. You can choose to transfer an amount from $0 to $40, in steps of $5. Your partner can then return back to you any amount in whole dollars up to the limit of their account. Please fill the circle of your choice below.

TG1 How much do you wish to transfer to your partner?

❍ $0 (your partner will receive: $0)
❍ $5 (your partner will receive: $15)
❍ $10 (your partner will receive: $30)
❍ $15 (your partner will receive: $45)
❍ $20 (your partner will receive: $60)
❍ $25 (your partner will receive: $75)
❍ $30 (your partner will receive: $90)
❍ $35 (your partner will receive: $105)
❍ $40 (your partner will receive: $120)

We are also interested in your expectations about how much you will get back from your partner. This can be no larger than the total amount in your partner's account after your transfer, which is equal to $40 plus however much they received from your choice above.

TG2 Considering your transfer, please tell us how much that you expect your partner to send back to you by writing an amount in whole dollars.

I expect back: **$** [ ]

Section 2: Questionnaire

Please fill out this short questionnaire as truthfully as possible to finish your survey. You must submit a completed questionnaire to be placed in the draw for the prizes. The questionnaire answers will help inform our understanding of the results. Your answers will be stored completely anonymously.

Q1 What is your current age?

- ❍ 16 to 19
- ❍ 20 to 24
- ❍ 25 to 34
- ❍ 35 to 44
- ❍ 45 to 54
- ❍ 55 to 64
- ❍ 65 or over

Q2 What is your gender?

- ❍ Male
- ❍ Female

Q3 What is your highest completed level of education?

- ❍ Less than Year 12 or equivalent
- ❍ Year 12 or equivalent
- ❍ Vocational Qualification (e.g. TAFE)
- ❍ Bachelor Degree
- ❍ Master's Degree or higher

Q4 Please indicate your occupation.

- ❍ Management and professional (includes Farmers)
- ❍ Technicians and tradesworkers
- ❍ Community and personal service workers
- ❍ Clerical, administrative and sales workers
- ❍ Machine operators, drivers and labourers
- ❍ Retired
- ❍ Unemployed
- ❍ Student

Q5 In which industry are you currently employed?

- ❍ Agriculture, forestry, fishing and mining
- ❍ Manufacturing and construction
- ❍ Retail trade
- ❍ Transportation, postal and warehousing
- ❍ Public service
- ❍ Education
- ❍ Health care and social assistance
- ❍ Hospitality and tourism
- ❍ Other / Not currently employed

Q6 Please indicate your current family structure.

- ❍ Single without children
- ❍ Single with children
- ❍ Married/De facto relationship without children
- ❍ Married/De facto relationship with children

Q7 Were you born in Australia?

- ❍ Yes
- ❍ No

Q8 For roughly how many years have you been living in your local area? [ ]

Q9 Roughly how many clubs are you a member of? (e.g. sports, social clubs, political party etc) [ ]

Q10 If a federal election was held tomorrow, which party would you be most likely to vote for?

- ❍ Liberal/National Coalition
- ❍ Labor
- ❍ Greens
- ❍ Other ____________________

Q11 What is your religion? (Examples of "Other" include Judaism, Humanism and Taoism)

- ❍ Catholic
- ❍ Anglican (Church of England)
- ❍ Other Christian
- ❍ Islam
- ❍ Buddhism
- ❍ Hinduism
- ❍ No religion
- ❍ Other (please specify) ____________________
- ❍ Prefer not to say

Q12 Generally speaking, would you say that most people can be trusted or that you need to be very careful in dealing with people?

- ❍ Most people can be trusted
- ❍ You can never be too careful when dealing with others

Q13 How safe do you feel walking in your district at night?

- ❍ Very safe
- ❍ Fairly safe
- ❍ Fairly unsafe
- ❍ Very unsafe

Q14 How much do you trust people from various groups? Please indicate for each whether you trust people from each group completely, somewhat, not very much or not at all.

	Trust completely	Trust somewhat	Do not trust very much	Do not trust at all
People you know personally	❍	❍	❍	❍
People you meet for the first time	❍	❍	❍	❍
People of another religion	❍	❍	❍	❍
People of another nationality	❍	❍	❍	❍

Q15 How often, if at all, do you participate in volunteer work? Please choose the closest option.

❍ Never
❍ One or two times a year
❍ About once once a month
❍ About once a week

Q16 In general, how positive or favourable do you feel about resettled refugees in Australia? Mark an ‘X’ on the line below to represent your view.

	extremely negative				indifferent			extremely positive			
	0	10	20	30	40	50	60	70	80	90	100
I feel:											

Congratulations! You have finished both sections of the survey. We will match your answers with those of your partner and process the results as quickly as possible. After we have collected all responses from the survey, we will calculate each participant's potential cash prize, and if you are among the winners, we will contact you by 31 March 2016 to arrange payment of your prize. Please write either your email address or your postal address below. (Your details will not be used for any purpose other than to notify you about prizes.)

❍ Email: ___________________________________ or

❍ Postal address: ____

____

If you are interested in receiving an email about your partner's decision and consequently your final game account in the Trust Game regardless of whether you are selected for a prize, choose this option below. We will send you an email with these results after the completion of the survey.

❍ Please notify me of my results!

To submit your survey, please include all pages of this survey in the enclosed, stamped return envelope and post it by the deadline. Please make sure that you have answered all questions and included all pages.

We thank you for your participation!

Chapter 4

Exposure to refugees and attitudes to immigration*

Today, as yesterday, a nation is judged by its attitude towards refugees.

Elie Wiesel, Nobel Peace Laureate in 1986

4.1 Introduction

Are attitudes to immigration formed on the basis of exposure to immigrants in the society, and does it matter for how long and to how many immigrants we are exposed? We test whether experiencing the presence of asylum seekers[64] in one's neighbourhood or municipality influences individuals' attitudes towards immigration in a difference-in-differences framework and panel fixed effects regressions. Exposure to immigrants, refugees or asylum seekers is not a binary experience as it is often assumed in the literature, but plausibly depends on the duration of exposure, the numbers of immigrants exposed to, and the degree of proximity to the individual (among other aspects). Using a detailed dataset of the placement of asylum seekers in centres across the Netherlands between 2011 and 2016, we examine these aspects of exposure for a representative panel of the Dutch population.

In distinguishing effects by dimension and intensity of exposure, we provide a possible explanation for the diverging effects found in the literature to date. The recent literature on

*This chapter is based on (Albrecht et al., 2018).

[64]Throughout the paper we refer to refugees and asylum seekers interchangeably, in spite of the legal difference between the two concepts. In countries with individualized procedures, such as the Netherlands, an asylum seeker obtains refugee status only once the claim for asylum has been assessed. Since a substantial part of the asylum seekers in our study are Syrians who are practically guaranteed to obtain refugee status, we abstract from the distinction for ease of exposition.

the effects of exposure to refugees and attitudes to immigration expressed in far-right voting comes to seemingly contradictory conclusions. Vasilakis (2017) finds evidence of increased xenophobia on Greek islands that hosted large numbers of refugees.[65] Steinmayr (2018) and Vertier and Viskanic (2018), however, show that in Austrian and French communities with refugee accommodation the country-wide trends of voting for the far-right anti-immigration parties were dampened. We argue that these differences could be explained by the dimension and intensity of exposure in the affected communities.

Social theories about intergroup relations give useful guidance for why different dimensions of the intensity of exposure could yield different empirical findings. Realistic conflict theory (Campbell, 1965) posits that conflict or negative attitudes between groups (often in the context of ethnic diversity) can arise when the groups do not share the same goals and compete over limited resources. A different cultural background and large numbers of refugees could put a strain on the host population and be the trigger for worsening attitudes and xenophobia. On the other hand, contact with members of the other group can under certain conditions help to reduce prejudice and improve attitudes to each other, which is known as intergroup contact theory (Allport, 1954). For a beneficial effect of intergroup contact to emerge, the contact situation needs to be long enough and involve some interpersonal interactions. A longer duration of exposure and greater proximity to the refugees increase the likelihood of such interactions between the native population and the immigrants.

Our study finds evidence for both theories. Attitudes to immigration are affected by exposure to immigrants, but by and large only when it takes place in the immediate neighbourhood. Which direction the effect takes, depends on the duration of exposure and the numbers of refugees in the neighbourhood. Exposure to large numbers of refugees leads to significantly less favourable attitudes to immigration, which we see particularly when the refugee centre has been in the neighbourhood for less than half a year. In contrast, experiencing refugees in the neighbourhood for more than half a year significantly improves attitudes to immigration. Most of the effect comes from small refugee centres, which implies that these create the most favourable conditions for positive intergroup contact. However, a strong pattern that we find is that a longer duration of exposure (i.e. more than half a year) has a significantly more positive effect on attitudes than a short duration, regardless of centre size.

Our study is the first to separate the time, quantity and space dimension of exposure to immigrants. This contribution rests on the features of the datasets that we link for the analysis: We combine monthly data on the allocation of refugees to centres across the country

[65] Two other studies essentially study the same islands, Dinas et al. (2017) and Hangartner et al. (2017), and find a comparable increase in hostile attitudes and voting for the far-right anti-immigration party. However, they relate the results to the arrival (and transiting) of refugees rather than a more sustained form of exposure.

with annual individual-level panel data on attitudes towards immigration. The high level of detail in the refugee data allows us to quantify the number of months and refugees in the neighbourhood or municipality of an individual, which splits the sample into a treated and control group. Using annual panel data on individual attitudes as opposed to the more aggregate and less frequent voting behaviour has several advantages. Most importantly, it preserves the variation in exposure that would otherwise be averaged over time. Secondly, it gives a direct measure of attitudes that is arguably more accurate and stable as it is less affected by environmental factors such as a political smear campaign or media coverage and not influenced by other political considerations. Lastly, it permits us to examine whether the effects of exposure differ by gender, age and level of education of the exposed population, with interesting results that give further insight about the channels at work.

Combining a difference-in-differences approach with individual fixed effects regressions helps us to address the most significant concern about endogeneity. Whether or not a municipality is host to a refugee centre may – crucially – depend on the residents' attitudes towards immigration issues. While the individual-level fixed effects take care of a level difference in attitudes and other stable unobservable characteristics that might be related to both exposure and attitudes, we include time-varying characteristics of municipalities, neighbourhoods and individuals to control for potentially different trends between exposed and non-exposed individuals. For the difference-in-differences specification we show that there was no pre-existing trend in attitudes that differed between the exposed and non-exposed individuals.

There are further facts about the institutional set-up that support our argument for a credible effect of (dimensions of) exposure to refugees on attitudes to immigration. Whereas the decision to open a refugee centre in a municipality may depend on the residents' attitudes, dimensions such as the duration of opening and the actual number of refugees hosted are likely determined by factors outside the circle of influence of individuals. A critical factor for the duration dimension is the timing of the influx of asylum seekers into the country, which is driven by global developments; most of the variation that we exploit comes in fact from the Syrian 'refugee crisis', when unexpectedly large numbers of refugees reached Europe in the summer of 2015 and beyond. Both the number of refugees and the proximity to the individual (whether the refugee centre is located within one's immediate neighbourhood or within the wider municipality) depend on the availability of space and suitable buildings.

Our study adds to the more nuanced view of the impact of exposure to immigrants on attitudes or political outcomes that has started to emerge from the recent literature.[66] Closest

[66]Based on data from migration and refugee waves of the last few decades, there is overwhelming evidence for a negative effect of a higher share of immigrants at a certain local level on attitudes towards immigration, as proxied by an increased support for anti-immigration parties. This has been shown to hold for Austria (Halla et al., 2017); Germany (Otto and Steinhardt, 2014); the United Kingdom (Becker and Fetzer, 2016); Denmark (Dustmann et al., 2016); Italy (Barone et al., 2016) and France (Edo et al., 2017). While this literature is

to our study is Steinmayr (2018), whose primary interest is to study the effect of exposure to refugees on voting outcomes of hosting municipalities. Instrumenting the presence of refugee accommodation with the availability of buildings that may be used as group accommodation, he finds that hosting refugees reduces the vote share for the far-right anti-immigration party. Surveys suggest this has to do with a stronger feeling that the successful integration of refugees will be achievable, which points to a story of positive intergroup contact and reduced prejudice. Indeed, in the latest update of his paper, he finds further evidence for the contact hypothesis by contrasting the effect to municipalities at the German border that were mostly affected by refugees in transit to Germany. Without a longer basis for contact to emerge, border municipalities increase their vote share for the anti-immigration party relative to unexposed municipalities.

Due to data limitations, exposure is measured as a binary variable in Steinmayer's study. While the context may give some indications of different dimensions of exposure, it is unclear what drives the effects. Vertier and Viskanic (2018) use a similar instrumental variable approach and come to overall comparable conclusions for France, where hosting refugees reduced the vote share for the far-right anti-immigration party considerably. Again, the main focus is on a binary effect of exposure, but the authors tentatively explore two exposure dimensions as a side note: They find that the negative effect on vote shares for the far-right party dissipates spatially and when the refugee numbers cross a certain threshold. Our paper connects to precisely this observation and takes the analysis further, by investigating the effects of different dimensions (and intensities) of exposure systematically.

The structure of the paper is as follows. Section 4.2 defines and illustrates what exposure to refugees entails in our study. This includes an overview of the institutional background of the asylum process, as well as an introduction to the type of variation in dimensions of exposure that we see in our data. Section 4.3 discusses the estimation strategy and identifying assumptions that lie underneath. Section 4.4 contains a description of the data, the key variables and the sample that our estimations are based on. Section 4.5 presents the results along with tests of the identifying assumptions and checks for robustness. Section 4.6 offers a discussion of the implications and channels that the effects might operate through. Section 4.7 concludes.

convincing in itself, it does not speak to the underlying situations of intergroup interaction between immigrants and natives that are connected to a higher share of immigrants.

4.2 Exposure to refugees

Exposure to refugees – and more precisely the *proximity*, *duration* and the *numbers* of refugees exposed to –, are the treatment variables of our analysis. We measure variation along these dimensions by looking at the distribution of refugee centres across the country, which have increased drastically on account of the refugee crisis. Besides the number of centres, the institutional background of the asylum process is crucial to understanding what 'exposure' entails for the local population.

4.2.1 Institutional background

To illustrate the experience and features of reception centres, which are a key part of exposure, we briefly describe the typical progression of an asylum seeker within the Netherlands.[67]

When asylum seekers come to the Netherlands, they enter an administrative multi-step procedure of up to 15 months. It determines whether they will receive protection as a refugee and become a so-called status holder with a residence permit valid for five years, or whether they will have to return to their home country. The *Immigration and Naturalisation Service* (IND) of the Government makes the decision on the refugee status. The *Central Agency for the Reception of Asylum Seekers* (COA) is responsible for the reception and supervision of asylum seekers in refugee centres during this process, and assists with finding subsequent accommodation in case of a positive decision by the government.

Upon arrival in the Netherlands asylum seekers report to the aliens police for registration and identification. From that moment on COA is responsible for providing accommodation to the asylum seekers, while the IND starts the interview application process. After an initial recovery and registration period in a central reception location the asylum seeker is assigned to one of the regular refugee centres in the country. Which centre that is, depends on the phase of the administrative process and on the availability of space. Children who are travelling alone are hosted in separate, smaller facilities better equipped for their support and supervision.

Refugee centres are typically located in or nearby residential areas. A legal requirement is that public transport is within 500m and schools and stores are within 3km reach. During the first six months of waiting for a decision asylum seekers are not allowed to do paid work, but may participate in voluntary work. Asylum seekers receive weekly pocket money to buy groceries and other necessities if they do not have the means to provide for themselves.[68]

[67] Information about refugee centres and the asylum process comes from `https://www.coa.nl/` and `https://ind.nl/asiel/Paginas/Asielzoeker.aspx`.

[68] The amount of the weekly allowance is regulated by law and depends on the number of adults and children

They are insured for basic health care through the Dutch system. At the centre the asylum seekers learn about basic Dutch rules and cultural aspects and commence a language course. Children often attend local schools, play sports and learn to ride a bicycle. After six months the asylum seeker can ask for permission to work for up to 24 weeks per year, but COA does not assist in finding paid employment. Any money earned through work is deducted from the weekly allowance.

At the end of the application procedure a decision is made regarding the further stay in the Netherlands. In case of a rejection the asylum seeker has the chance to appeal, but otherwise must return to his or her home country within a month. In case the conditions for temporary asylum are met, the asylum seeker receives a residence permit for the Netherlands valid for five years. This allows the refugee to move out of the reception centre and take up work. The refugee must also participate in a civic integration course in their new municipality of residence. Dutch municipalities have an obligation to provide housing to status holders in proportion to their population. This means that after the IND's decision to grant a residence permit, COA allocates the status holder to a municipality typically in the same region as the refugee centre. Which municipality that is depends mostly on the outstanding quota, and can be guided by considerations of whether the refugee has close family, work or medical needs that require him or her to go to a particular municipality. The municipality then has to offer the refugee suitable accommodation, usually in the form of social housing, which the refugee is required to accept. His or her right for accommodation in a refugee centre is expired. Importantly for our analysis, after the asylum procedure is over, refugees are distributed evenly across the country, so that differences in exposure discontinue.

4.2.2 The development of refugee numbers in the Netherlands

Whereas the features of refugee centres and the asylum process constitute the 'filling' of the exposure treatment, it is the number of open centres and their distribution across the country that determine who is exposed.

The numbers of asylum seekers that come to the Netherlands determines the number of centres that COA operates all across the country. While there is always a certain number of active centres to cater for the baseline stock of asylum seekers, this number varies at any given point in time based on the expected (or actual) need for refugee accommodation. During the period that we observe, from January 2011 to December 2016, around 30 centres are open continually. The onset of the Syrian civil war and the subsequent waves of refugees streaming into Europe constitute the largest shock to the number of refugee centres in our

in the refugee household and whether a meal is provided by the centre. For a family of four with two under-age children who cares for their own meals, the weekly allowance is €167.30.

sample. The unanticipated inflow of Syrian refugees more than quadrupled the number of centres and, hence, the number of people exposed to these type of immigrants on short notice.

Figure 4.1 shows the development of the stock of asylum seekers and refugee centres over time in more detail. At the beginning of 2011 less than 20,000 asylum seekers were waiting for the IND's decision on their status in one of 48 refugee centres in the country.[69] At the time, refugees came predominantly from North-East Africa, Central Asia and the Middle East. Over the years 2012 and 2013 the stock of refugees in the Netherlands decreased to less than 15,000 spread over 35 open centres, which was due to a reduced inflow and the processing of the existing stock of refugee applications. Mid 2014 saw the first impact of the Syrian civil war and a return to about 50 active refugee centres. One year later, in September 2015, what is now termed the European 'refugee crisis' set in and the likelihood of living in the vicinity of a refugee centre rose greatly. COA then operated around 100 centres in the Netherlands. This number surged to about 120 in early 2016, even though refugee numbers were decreasing by then.

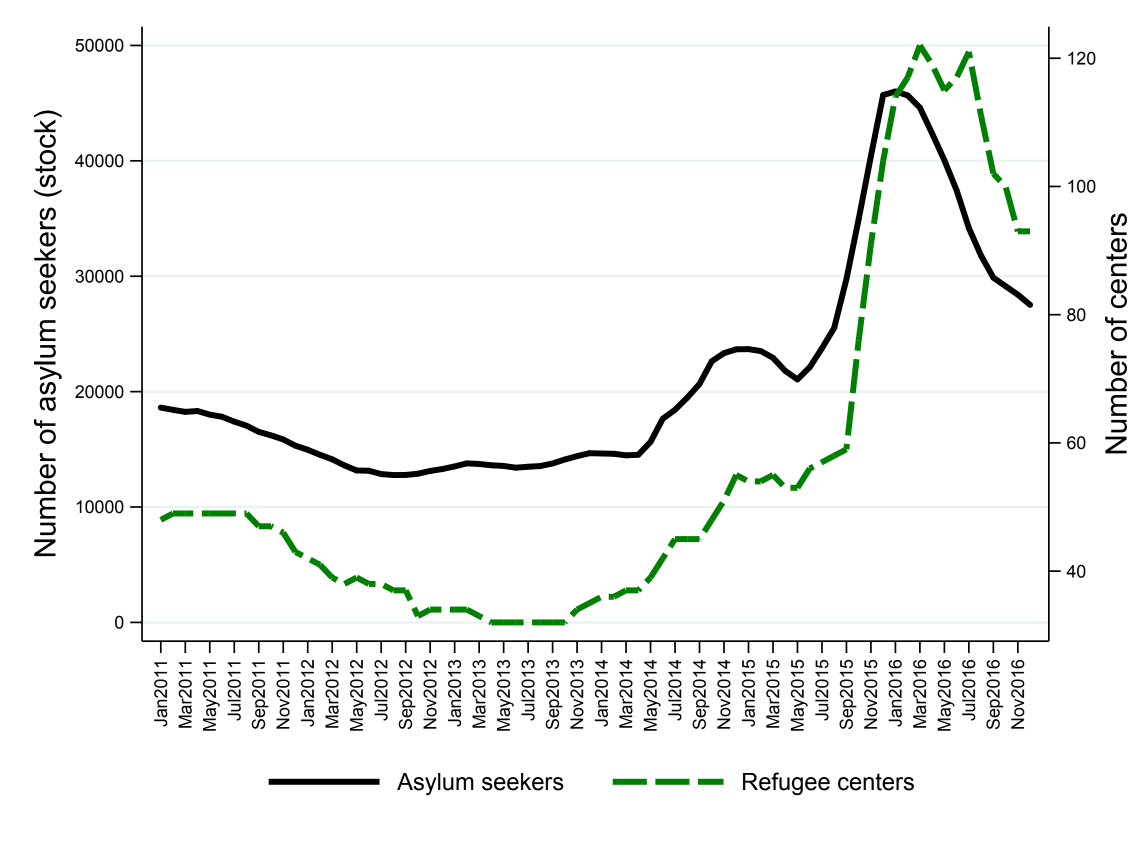
COA's decision to open a new centre, or to adjust the size or close an existing centre first and foremost depends on the demand for refugee accommodation, but the 'where' and 'when' of that decision are influenced by certain guiding principles and other actors. It is their explicit policy to aim for an even spread between regions to share the burden and facilitate integration. In addition to that, the flexibility, sustainability and quality that a particular centre offers as well as the financial situation factor into the decision.

Besides these rather independent guiding principles, the municipality where a refugee centre is (to be) located has its say in the decision process, and it is logical to assume that this introduces some degree of endogeneity to exposure. How much influence the municipality and its residents have, varies from case to case. In some cases, the availability of vacant government property such as former prisons or fallow private land have led to the creation of a refugee centre in a particular municipality.[70] In these cases, exposure to migrants is plausibly exogenous. In other cases, the local council decided to enter a contract with COA allowing them to operate a refugee centre on municipality premises.[71] The decision when and for how long to operate a refugee centre is more likely to be endogenous with respect to attitudes of the local residents; we will come back to this point when we talk about the empirical strategy in Section 4.3.

[69] We collapse multiple centres with the same six-digit postcode, as these are often recorded separately for administrative reasons, but are actually in the same location (street). We maintain this simplification throughout the paper.

[70] Examples are the centre in Heerlen in a former convent, or the one in Gilze en Rijen on the former area of the Air Force.

[71] This was the case for the re-opening of the centre in Aalten in 2015, for example.



Source: COA. Own calculations.

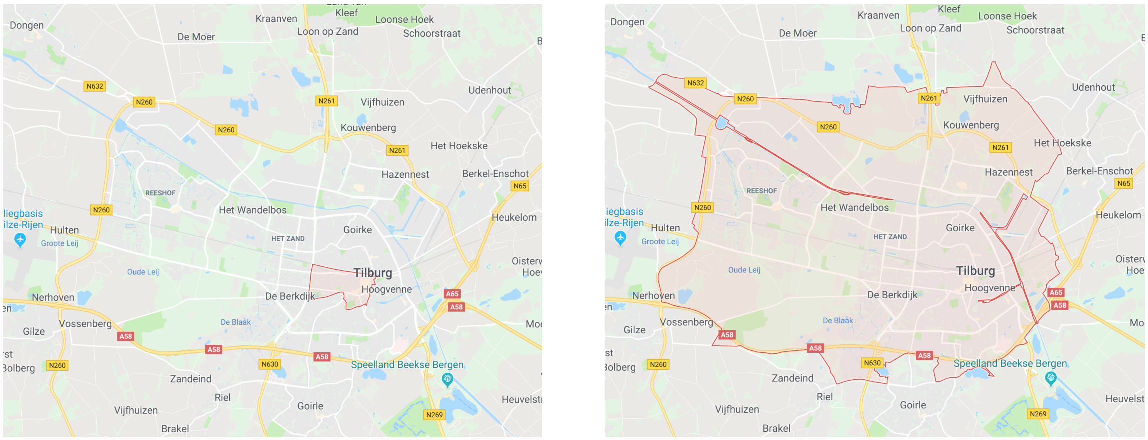
Figure 4.1: Refugee centres in the Netherlands

4.2.3 Three dimensions of exposure

The refugee centres differ in size and duration for which they are operated, which gives us variation along these two dimensions. The locations of the centres determine which individuals are exposed to refugees and how close they are geographically.

For our analysis we therefore distinguish three dimensions of exposure to refugees based on the characteristics of centres: (1) The degree of spatial *proximity* of the individual to the refugee location; (2) the *number* of refugees present in one location; and (3) the *duration* for which the refugees are present.

Proximity. Proximity varies based on where we draw the geographic line of exposure. We compare exposure in the same neighbourhood (defined by the postcode) to exposure in the municipality of a refugee centre. Figure 4.2 exemplifies the areas that the geographic boundaries comprise, which clearly shows the difference between these two definitions of exposure. In case (a), when only individuals living in the four-digit postcode of the refugee centre are considered exposed to refugees, the degree of proximity is high. The average size of a neighbourhood is roughly 2 km^2, with 4250 people registered there. The close vicinity



(a) The area of a postcode

(b) The area of a municipality

Source: Google Maps. Comparison of the boundaries of a four-digit postcode where a refugee centre is located and the boundaries of the corresponding municipality in the Netherlands, shown for the municipality of Tilburg as an example.

Figure 4.2: Exposure at different geographic levels

to the refugee centre implies a high likelihood of some kind of contact with the refugees. This could take place at local stores or in the streets, in passing by the centre on the way home, or through refugee children who attend local schools. Importantly, a high degree of proximity means both a higher likelihood of being positively or negatively affected by the presence of refugees.

In case (b), every individual living in the municipality where a refugee centre is located is considered exposed to refugees. Since a municipality comprises a much larger area, the degree of proximity is on average lower and the likelihood of actual contact decreases. However, it is conceivable that individuals do not spend all of their day in their own neighbourhood, and that sharing a common administrative jurisdiction with the refugees matters. Municipalities constitute the lowest level of government in the Netherlands. As such they are responsible for several public services and amenities such as the provision of social housing, local infrastructure, land, water and waste management, school buildings and the environment. Although municipalities with a refugee centre receive financial compensation from the government, the opening of a refugee centre may create extra costs to the municipality's finances and change the composition of other services delivered.

By comparing these two degrees of spatial proximity in the exposure to refugees, we capture different channels of how exposure to refugees affects individual attitudes towards immigration.

Numbers. A second characteristic of refugee centres is their occupancy rate. While there are different types of centres that also roughly correspond to size categories, every centre is unique in the availability of space (in that particular location and building). This creates a smooth distribution of occupancy rates across the country, ranging from very small centres with just 15 to 20 refugees to the largest centre with over 2000 occupants.

Figure 4.3 shows how the distribution of refugee numbers across municipalities changed from 2013, when the stock of refugees was smallest, to 2016. The maps establish that new centres that were opened as a result of the refugee crisis are spread over the whole country, and illustrate how the change in the number of asylum seekers also changed occupancy rates within a municipality. By looking at numbers of refugees exposed to as one dimension of exposure, we thus capture variation both in the extensive and intensive margin of exposure.[72]

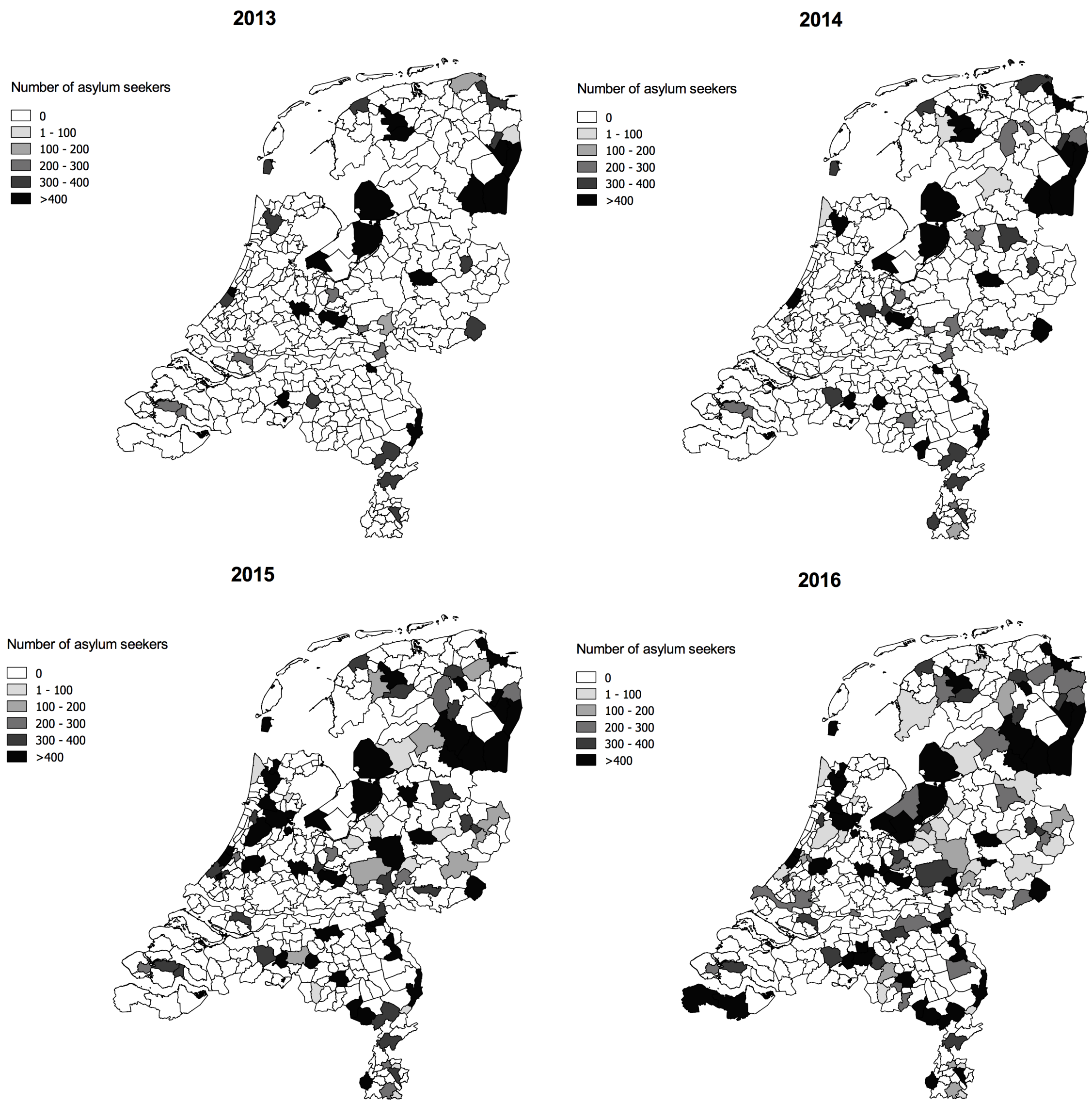
Whether the number of refugees has a positive or negative effect on attitudes to immigration is a priori unclear. On the one hand, a larger number may increase the chance of encounters and positive social interactions between locals and the immigrants, but it could just as well raise the level of disturbance or present a cultural shock to the locals.

Duration. The third dimension of exposure is its duration. Refugee centres open (and close) in different months during the year, and remain open for different lengths of time. This generates variation in the duration of exposure at the time when attitudes are elicited from the local population.

Figure 4.4 plots the timeline of opening for each centre. Every row in the graph represents one refugee centre, and within a row every dot corresponds to a month during which the centre hosted at least one refugee (with different colours indicating different categories of refugee numbers). The graph clearly shows that a baseline number of centres were continually open, that some centres closed for a number of months when there was less demand for refugee accommodation and later re-opened, and that many new centres were established in late 2015 at the height of the refugee crisis. We can also see that some of the new centres were open for just a few months. These were organised as short-term ‘emergency accommodation’ to cope with the sudden influx of large numbers of asylum seekers, and were typically operated for three to six months.

Similar to the numbers of refugees, the duration of exposure in any given year may push attitudes to immigration in different directions. A short duration of exposure within the year could mean that locals literally have not had the time to get to know this group of immigrants, so that their attitudes may be more reflective of preconceptions that they hold. On the other

[72]We explicitly choose to look at the absolute numbers of refugees in a neighbourhood or municipality, controlling for the size of the local population in the regressions, because it allows for a more direct interpretation and reflects the actual situation: Refugees are housed in one particular spot, where the absolute size of the location is apparent to the neighbours. Calculating shares of refugees relative to the local population would imply that refugees lives in locations evenly spread throughout a given neighbourhood or municipality. Nevertheless, Figure 4.A1 in Appendix 4.A shows that absolute refugee numbers are highly correlated with shares relative to population size.

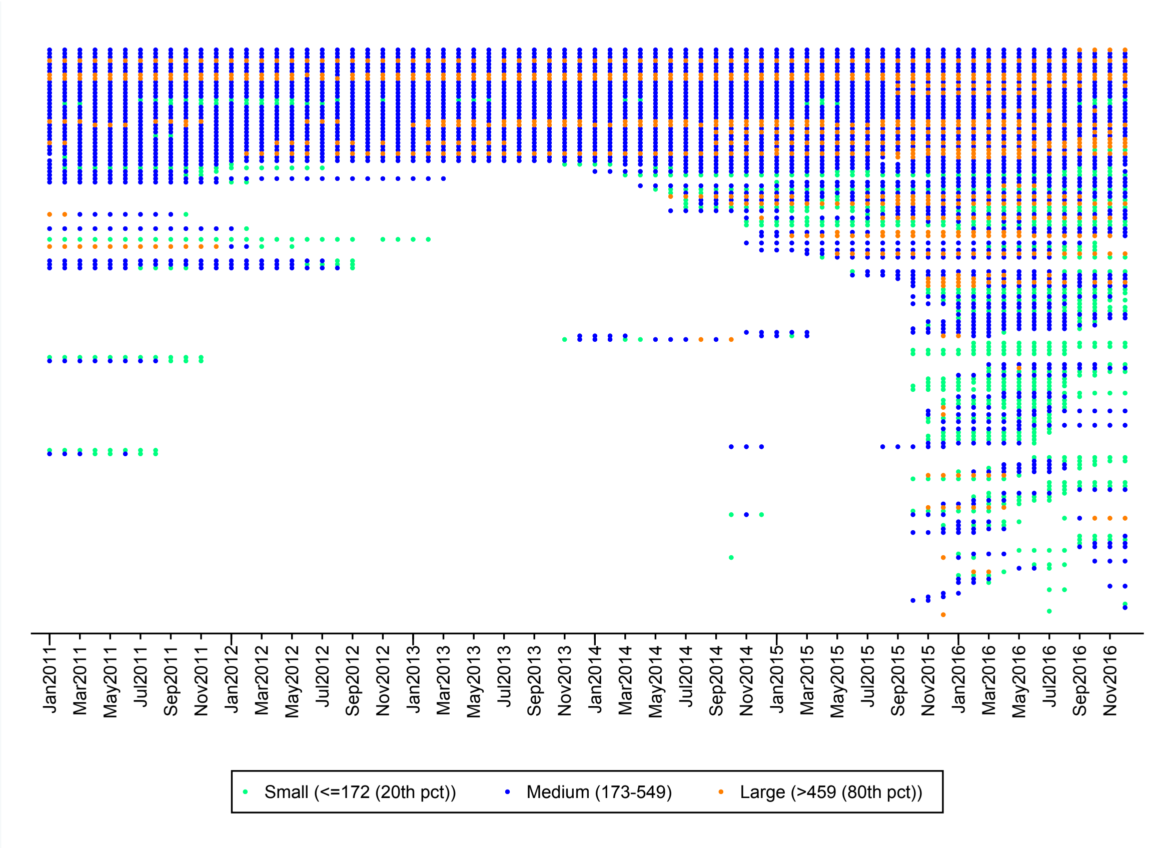


Source: COA. Own calculations. Every polygon corresponds to a municipality.

Figure 4.3: Distribution of refugees across municipalities

hand, it is less likely that conflicts would have emerged in a short period of time. A longer duration of exposure increases the likelihood of intergroup interactions. There is more time for problems and conflicts to come up, but also to be resolved. A long exposure period within the year can also shift the experience from a temporary one to a more permanent situation, which may be more likely to influence the individual's attitudes.

Interactions of dimensions. Importantly, as the colour-coding by size category in Figure 4.4 indicates, there may be interaction effects between the three dimensions. As an example of that, the effect of exposure could depend on whether individuals experience a small or a large number of refugees for a long time. A long duration of exposure to small numbers of refugees may be a more positive experience, perhaps because it is easier to establish contact



Source: COA. Own calculations. Every row represents one refugee centre in the Netherlands and the months during which it was open. Colour-coding indicates the size category that a centre fell in in a given month.

Figure 4.4: Timeline of open refugee centres

if the group size is less intimidating; with a large number of refugees, the negative effects of experiencing disturbances in and around the centre could dominate in the long run. Of course, both numbers and duration are expected to have different effects depending on the degree of proximity.

4.3 Empirical strategy

The empirical strategy to estimate the effect of dimensions of exposure is built around the panel structure of the dataset. Specifically, we combine two approaches that are both commonly used in the literature to exploit as much variation as possible in our data. The first is a classical difference-in-differences regression where we compare exposed to unexposed individuals over time. The second is individual fixed effects regressions that relate changes in attitudes to the dimensions and intensity of exposure.

The literature has made use of either difference-in-differences (Becker and Fetzer, 2016; Gehrsitz and Ungerer, 2016), panel fixed effects (Halla et al., 2017) or instrumental variable estimation (Halla et al., 2017; Steinmayr, 2018; Vertier and Viskanic, 2018) in comparable settings. A unique feature of our study is the unit of observation: With a direct measure of attitudes, we can include individual fixed effects in our regressions, and thereby control for stable unobservable differences that play a role for attitudes to immigration.

Difference-in-differences. Since the postcode or municipality defines whether an individual is exposed to refugees ('treated'), we look at several specifications of the difference-in-differences regression. We first examine whether we see a cross-sectional differential trend in attitudes after exposure by simply comparing the two groups of (at one point-) exposed and never-exposed individuals before and after the opening of the refugee centre in their area.[73] We next add area fixed effects to more accurately account for the fact that there may be level differences in attitudes between (exposed and unexposed) areas. Finally, we only use variation within individual by including individual fixed effects. Equation (4.1) presents the basic difference-in-differences specification.

$$Attitude_{iat} = \alpha_0 + \alpha_1 Exposed_a + \alpha_2 (Exposed \times Post)_{at} + \alpha_3 X_{iat} + \delta_t + \varepsilon_{iat} \tag{4.1}$$

$Exposed_a$ is a group identifier for areas that at one point have a refugee centre. $(Exposed \times Post)_{at}$ is an identifier for exposed areas after the opening the refugee centre, and thus the coefficient of interest in the difference-in-differences framework.[74] δ_t are year dummies. X_{iat} contains time-varying individual, postcode or municipality characteristics, which we alternate between specifications. Importantly, we always control for municipality size and the proportion of registered non-Western immigrants in the regressions. Since the exposure status varies by area, we cluster heteroskedasticity-robust standard errors at the area-level.

Fixed effects regressions. The panel regressions with individual fixed effects are our vehicle for estimating the effects of different dimensions and intensities of exposure on attitudes. Equation (4.2) presents the basic model for a panel fixed effects specification.

$$Attitude_{iat} = \beta_{0i} + \beta_2 ExposureDimension_{at} + \beta_3 X_{iat} + \tau_t + e_{iat} \tag{4.2}$$

β_{0i} are individual fixed effects that control for (unobservable) stable individual characteristics. $ExposureDimension_{at}$ denotes a variable that is either based on the numbers of refugees, the duration of exposure or the interaction of the two, measured in area a. τ_t are year fixed effects. As before, X_{imt} contains time-varying individual, postcode or municipality characteristics, which we alternate between specifications. Again, heteroskedasticity-

[73] Throughout the text, 'area' denotes either the postcode or municipality of the individual, depending on the degree of proximity that we adopt.

[74] Note that this variable ignores if a centre was closed at one point, to keep a clean distinction of the pre- and post-exposure periods. The post-indicator can thus be interpreted as measuring a lasting effect of exposure.

robust standard errors account for clustering at the area-level.

Similar to the difference-in-differences framework, the individual fixed effects model compares changes in attitudes within (exposed or unexposed) individual over time. The identification of the effect of exposure dimension therefore comes from both the opening of a centre, a pre-post-comparison, and the switching of exposure intensity during the observed periods of opening.

4.3.1 Identifying assumptions

If the identifying assumptions are met, both approaches provide credible estimates of the treatment effects. In the difference-in-differences framework, it is assumed that attitudes of exposed individuals would have followed a parallel trend to attitudes of the unexposed in the absence of treatment, conditional on the time-varying covariates included in the model. In the basic model without area or individual fixed effects, the common trend assumption should hold between the groups of exposed and unexposed individuals. Including area or individual fixed effects changes the comparison from a comparison at the group level to within-area or within-individual comparisons. Our panel of individuals is not balanced over the five years of observations, so that we prefer the tightest specification including individual fixed effects.

The individual fixed effects regressions rely on the conditional mean independence assumption. Conditional on individual fixed effects and the time-varying control variables, counterfactual attitudes in the absence of refugee centres should be independent of the degree of exposure. Formally,

$$E[Attitude_0|\beta_{0i}, X_{iat}, Exposure_{at}] = E[Attitude_0|\beta_{0i}, X_{iat}]$$

To test the validity of these assumptions, we provide two pieces of evidence in Section 4.5. For the difference-in-differences model we look at pre-treatment trends by estimating year-by-year differences between the exposed and unexposed individuals. Since in this model the exposure effect is identified from newly opened refugee centres, we should not see any significant differences in attitudes between the exposed and unexposed groups before 2015. To assert the effect of the dimensions of exposure, we simulate placebo treatments by randomly shuffling the exposure values for numbers and duration (i) among the whole sample, and (ii) among the treated individuals.

4.3.2 Endogeneity concerns

Both the difference-in-differences and the individual fixed effects approaches account for a level difference in attitudes between unexposed and exposed individuals. This removes the largest concern for endogeneity of exposure to attitudes, namely, that it is likely that municipalities (and by association the individuals in the municipalities) that enter a contract with COA to host a refugee centre in their municipality have a generally more positive attitude towards immigration, or that they have some unobserved stable individual characteristics that would bias the effect of exposure if they were omitted from the analysis.

What remains a concern, and which we try to address as thoroughly as possible, is that individuals in municipalities with a refugee centre may differ from the unexposed individuals in some time-varying way. In the difference-in-differences framework, where exposure is a binary variable that indicates whether a centre is or was present, the pre-trend analysis is the most logical way to test this concern about time-varying different trends. If there is no pre-trend in the binary case, we can safely attribute any difference after exposure to the fact that these individuals have been exposed to refugees.

Testing this for the individual fixed effects regressions is more challenging. Because values of exposure dimension can change for one individual in the post-exposure period (due to time-varying characteristics of the refugee centre), pre-dating the value of exposure dimension by one or two periods, as it is often done in settings where there are different intensities of treatment, is not informative. Instead, we address the problem by including time-varying observed control variables to mitigate the concern as much as possible. In addition to that, we run a robustness check removing centres that were open before the refugee crisis in 2015 from the sample. Since the refugee crisis hit the Netherlands quite suddenly, variation that we get from it comes closer to a natural experiment and suffers less from endogeneity concerns: There would have simply been less time for individuals to make themselves heard and influence the decision at the municipality-level.

As a last argument against endogeneity concerns: The fact that attitudes towards immigration is an individual-level variable, whereas the dimensions and intensity of exposure are determined by the decision process between COA and the municipality, helps to alleviate concerns about endogeneity. While individuals may have an influence on whether or not their municipality will open a centre, the dimensions of exposure are more likely to be exogenous with respect to individual attitudes. The date of opening depends to a large degree on the timing of the influx of asylum seekers and on how quickly a location may be ready to host them, so that the duration variable is likely determined by these outside factors. Similarly, the number of refugees exposed to depends on the type of building or facility that is available in the municipality. This, too, is less likely to be influenced by individual actors.

The degree of proximity (or whether an individual lives in a municipality with a refugee centre at all) can be influenced by moving to another neighbourhood or municipality, but we address (potentially) selective migration statistically by considering only the first neighbourhood an individual lived in (as recorded at the first observation in our dataset) as their place of residence.[75]

4.4 Data

The structure of our data sources is a unique feature that allows us to examine different dimensions of exposure. A crucial requirement for capturing dimensions is that there is variation in exposure between two attitude measurements. The dependent variable, individual attitudes to immigration, is recorded annually, whereas the independent variables on the dimensions and intensity of exposure are measured with monthly frequency at the level of the postcode or municipality. We link these two layers to obtain a yearly panel dataset at the individual level.

The data come from three sources.

(1) COA provides the monthly stock of occupants in all refugee reception centres in the country from 2011 to 2016. This is a unique data set that allows us to precisely determine when and to what intensity a municipality or a postcode was exposed to refugees.
(2) We match the geographic information on refugee centres with a representative panel study of the Dutch population called *Longitudinal Internet Studies for the Social sciences* (LISS) administered by CentERdata. The panel consists of 7000 individuals from 4500 households, and is based on a true probability sample of households drawn from the population register by Statistics Netherlands.[76] Besides collecting background information on the individuals, the LISS Core Study asks questions about political values every year in December, with the exception of 2014. We combine several of these survey questions into a single indicator of attitudes towards immigration.
(3) Yearly socio-demographic register data from *Statistics Netherlands* (CBS) on municipalities and neighbourhoods complete our dataset.

Dependent variable. The LISS Core Study contains several survey questions on political attitudes related to immigration. Averaged into one indicator, these six normative statements

[75]Throughout the six years covered by our sample, less than 12% percent of individuals move to a different postcode, which only half of the time corresponds to a move between municipalities. While this is not a large percentage, it should be noted that the treatment effect is an Intention-to-treat effect.

[76]Households that could not otherwise participate are provided with a computer and Internet connection.

measured on five-point Likert scales form the dependent variable, (positive) $Attitude$ towards immigration:[77,78]

It is good if society consists of people from different cultures.
It should be made easier to obtain asylum in the Netherlands.
Legally residing foreigners should be entitled to the same social security as Dutch citizens.
There are too many people of foreign origin or descent in the Netherlands.
It does not help a neighbourhood if many people of foreign origin or descent move in.
Some sectors of the economy can only continue to function because people of foreign origin or descent work there.

Independent variables. Apart from a binary indicator for exposure in the difference-in-differences models, the independent variables are different measures of dimensions of exposure. To match the exposure variables with the yearly attitudes, we collapse monthly information on refugee centres into yearly dimensions of exposure in the following way:

Numbers: The average number of refugees present during the open months in the year before the attitudes measurement.
Duration: The number of months a centre was open at the time of the attitudes measurement. We keep track of both the cumulative months since opening and the number of months within the year preceding the attitudes survey.

Since the treatment effect by intensity of exposure need not be linear, we additionally use dummies indicating different categories of treatment intensity in the regressions. For refugee numbers, we consider the median split, a tertile split, as well as more extreme comparisons of very small numbers of refugees exposed to (less than the 20th percentile of the occupancy distribution) versus very large numbers of refugees exposed to (more than the 80th percentile of the occupancy distribution).[79] For duration of exposure within year, we consider whether exposure lasted for less than half a year or (strictly) more than half a year before attitudes were elicited.

Proximity: We match the above exposure dimensions computed at the centre-level with the LISS panel in order to define the exposed group in one of two ways: (i) based

[77]Negatively framed questions are coded in reverse to construct the average. The results are robust to using the first component of a principal component analysis instead of the average.

[78]Table 4.A1 in Appendix 4.A shows pairwise correlation coefficients between the individual items and the constructed index. All six statements are significantly correlated with each other, and highly correlated with the computed index (correlation coefficients ranging from 0.61 to 0.81).

[79]Figure 4.A2 in Appendix 4.A shows the respective cut-off values for the occupancy distributions at both the postcode- and municipality-level.

on the same four-digit postcode (high proximity); (ii) based on the same municipality (low proximity).

Control variables. The opening of a refugee centre may depend on the capacity of a municipality to take in immigrants and its pre-existing share of migrants. It is therefore crucial to control for the size and the percentage of non-Western immigrants of the municipality. Similarly, the population and percentage of non-Western immigrants at the postcode-level might be associated with whether a refugee centre opens in a particular neighbourhood. These baseline regressors are always included. In some specifications we add other neighbourhood or municipality characteristics such as the population density and the percentage of 65+ year-old as a representation of the local community structure, and to control for time-varying trends at these higher levels. These covariates at the level of the exposure treatment matter for identification of the treatment effect. Including individual-level covariates may help to control for confounding trends in attitudes, as well as reduce the variance of the residual. If individual characteristics are included, we control for gender, age, highest level of education, occupation category, housing tenure, civil status and the number of children in the household.

4.4.1 Descriptive statistics

The yearly panel dataset at the individual level is, due to the nature of the LISS Core Study which regularly draws refreshment samples, not completely balanced. The sample consists of 6,890 individual panel members, of which 47% are observed every year over the entire period, hence, five times (2011-2016, excluding 2014). The remaining individuals are observed at least twice, and in various patterns over the years.[80] This gives us a total of 26,421 observations of attitudes towards immigration.

The panelists live in 2008 different neighbourhoods, spread over 383 municipalities.[81] Only 5 of the municipalities are not represented in every period. On average, a municipality in our sample comprises 5 four-digit postcodes (s.d. 6), and 14 panelists (s.d. 16). A postcode is represented by on average 3 panelists (s.d. 2) in the sample. Table 4.1 summarises information on the sample size by year.

[80]We conduct a robustness check using only the sample observed over all years, the results of which we report in Section 4.5.

[81]In 2016, the Netherlands had 390 municipalities. Because the number of municipalities is declining every year, we treated the 2016 municipalities as benchmark and aggregated statistics of former, merging municipalities in as far as possible. Three municipalities whose communities were split up and allocated to other municipalities were dropped from the sample.

Table 4.1: Estimation sample

Year	Panelists	ZIP4	Municipalities
2011	5159	1785	380
2012	5386	1822	381
2013	5371	1803	380
2015	5564	1845	382
2016	4941	1690	378
total	26421		
distinct	6890	2008	383

We next split our sample in (at some point-)exposed and never-exposed individuals and look at the balancing of background characteristics between them.[82] Table 4.2 lists these variables in two sub-tables, according to whether exposure is defined at the level of the postcode (high proximity) or municipality (low proximity). The averages draw on the first observation from each panelist.

Comparing panelists in exposed neighbourhoods or municipalities to those who have never been exposed shows that refugee centres are opened in neighbourhoods and municipalities with on average larger populations and larger pre-existing shares of (non-Western) migrants. In terms of individual characteristics of panelists, the subgroups are quite comparable, which is useful to know for the heterogeneity analysis in Section 4.5.

The sample consists of roughly 46% male panelists. If exposure is defined at the municipality-level, we see small differences in characteristics between the never exposed and exposed panelists that are likely due to the fact that all of the larger municipalities host refugee centres: On average, the exposed panelists are slightly younger (28% vs. 25% between 15-34), higher educated (33% vs. 29% with higher education) and less likely to be married (49% vs. 58%).

[82]Note that neither the difference-in-differences approach nor the individual fixed effects regressions require balancing of background characteristics.

Table 4.2: Descriptive statistics

Exposure at postcode-level	Never exposed	At some point exposed	p-value difference
Postcode characteristics			
Population	8253	9049	0.00
Migrants (%)	18.99	21.95	0.00
Non-Western (%)	9.13	10.64	0.01
Municipality characteristics			
Population	119030	128593	0.26
Migrants (%)	19.53	21.27	0.00
Non-Western (%)	10.47	11.09	0.18
Individual characteristics			
Male	0.46	0.45	0.80
15-34	0.26	0.24	0.22
35-54	0.35	0.38	0.19
55-65+	0.39	0.38	0.85
(Less than) secondary education	0.46	0.46	0.82
Vocational education	0.23	0.23	0.91
Higher education	0.31	0.31	0.88
Paid employment	0.46	0.46	0.83
Retired	0.18	0.20	0.43
Married	0.53	0.52	0.61
Observations	6464	426	

Exposure at municipality-level	Never exposed	At some point exposed	p-value difference
Postcode characteristics			
Population	7505	9008	0.00
Migrants (%)	14.07	23.70	0.00
Non-Western (%)	5.48	12.54	0.00
Municipality characteristics			
Population	41845	188461	0.00
Migrants (%)	14.45	24.23	0.00
Non-Western (%)	6.61	13.97	0.00
Individual characteristics			
Male	0.47	0.45	0.30
15-34	0.25	0.28	0.01
35-54	0.36	0.34	0.24
55-65+	0.40	0.38	0.21
(Less than) secondary education	0.47	0.45	0.07
Vocational education	0.24	0.22	0.03
Higher education	0.29	0.33	0.00
Paid employment	0.46	0.46	0.66
Retired	0.19	0.18	0.37
Married	0.58	0.49	0.00
Observations	3235	3655	

4.5 Results

With a clear understanding of the structure of the dataset and the contents of the exposure treatment, we now turn to the results of the analysis. Not surprisingly, we find noticeable differences between exposure defined at a higher or lower level of proximity: In general, the results are weaker at the municipality-level, and attitudes to immigration remain largely unaffected unless the individual lives in the immediate neighbourhood of the refugee centre. There, duration and numbers of refugees change attitudes to immigration in opposite directions around certain thresholds of exposure, and we examine how the effects are linked to the gender, age and education group of the exposed population. Our findings are robust to various changes in the sample, which address concerns about attrition, endogeneity and outliers. We further provide evidence in favour of the identifying assumption by looking at the pre-exposure trends in attitudes and through the use of placebo treatments.

4.5.1 Close proximity: Exposure at the postcode-level

Being exposed to refugees in the immediate neighbourhood has a significant impact on attitudes to immigration, which becomes visible once the dimensions and intensity of exposure are taken into account.

Exposure as a binary variable. Table 4.3, which presents the results of the difference-in-differences models, shows that on average individual attitudes do not change after the opening of a refugee centre in their neighbourhood. While the baseline comparison of (at some point-)exposed and never-exposed individuals suggests that attitudes worsen slightly after being exposed to refugees (columns (1)-(3)), the comparisons within-postcode (columns (4)-(6)) or within-individual (columns (7)-(9)) do not support this conclusion. Controlling for unobserved postcode and individual characteristics reduces the estimated effect of -0.09 (15% of the standard deviation of attitudes) by an order of magnitude, to practically zero (3% of the within-individual standard deviation of attitudes). Controlling for additional characteristics of neighbourhoods or individuals only strengthens this result.

What is noticeable from Table 4.3 – and this relates to the general perception in the Netherlands after the onset of the refugee crisis –, is that attitudes to immigration have worsened considerably over time for all individuals. Compared to the base year 2011, attitudes in 2015 and 2016 are between 0.02 and 0.06 points lower (on the scale from 1 to 5), but there is no differential trend for those actually exposed to the refugees.

Dimensions of exposure. The distinction of different dimensions of exposure proves that the difference-in-differences estimate does not tell the full story: The intensity of exposure dimensions matters.

Table 4.3: Difference-in-differences: Exposure as binary variable (exposure at postcode-level)

	Pooled			ZIP4 FE			Indidvidual FE		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Treated ZIP4	0.0098	0.0115	0.0036						
	(0.0376)	(0.0392)	(0.0347)						
Post treatment	-0.0949*	-0.0673	-0.0815*	-0.0192	-0.0197	-0.0261	-0.0079	-0.0080	-0.0044
	(0.0532)	(0.0532)	(0.0490)	(0.0341)	(0.0342)	(0.0317)	(0.0279)	(0.0281)	(0.0274)
2012	0.0074	0.0056	0.0003	0.0067	0.0045	0.0009	0.0029	-0.0003	-0.0013
	(0.0065)	(0.0066)	(0.0065)	(0.0066)	(0.0068)	(0.0068)	(0.0056)	(0.0059)	(0.0060)
2013	0.0046	0.0029	-0.0103	0.0028	-0.0006	-0.0091	0.0038	-0.0015	-0.0043
	(0.0076)	(0.0081)	(0.0079)	(0.0081)	(0.0088)	(0.0089)	(0.0067)	(0.0073)	(0.0075)
2015	-0.0081	-0.0120	-0.0387***	-0.0184*	-0.0236**	-0.0408***	-0.0322***	-0.0408***	-0.0460***
	(0.0095)	(0.0104)	(0.0101)	(0.0105)	(0.0119)	(0.0121)	(0.0084)	(0.0097)	(0.0103)
2016	-0.0196*	-0.0219*	-0.0531***	-0.0301**	-0.0364**	-0.0554***	-0.0429***	-0.0534***	-0.0606***
	(0.0102)	(0.0116)	(0.0114)	(0.0124)	(0.0141)	(0.0144)	(0.0100)	(0.0118)	(0.0125)
Mean dep.var.	2.900						2.900		
S.d. dep.var.	0.621						0.265		
ZIP4.-lvl controls		X	X		X	X		X	X
Ind.-lvl controls			X			X			X
ZIP4 FE				X	X	X			
Ind. FE							X	X	X
#Panelists							6890	6890	6890
#Observations	26421	26421	26421	26421	26421	26421	26421	26421	26421
Adjusted R2	0.0096	0.0189	0.0950	0.3214	0.3215	0.3567	0.0047	0.0050	0.0077

Standard errors in parentheses are clustered at ZIP4-level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Note: All regressions control for overall population and non-western population. Additional controls at ZIP4-level include percentage of 65 or older and population density. Individual-level controls include gender, level of education, occupation, age, number of children in household, type of dwelling, and civil status.

Table 4.4 focuses on the numbers dimension. Exposure to very large numbers of refugees is associated with worsening attitudes. Interestingly, a linear measure of refugee numbers has no effect on attitudes (column (1)), and we also do not see a clear difference for exposure to below or above median numbers of refugees (column (2)). A pattern starts to emerge moving further to the extremes of the occupancy distribution (columns (3) and (4)): Very large numbers of refugees have a sizeable and significant negative impact on attitudes, corresponding to a reduction of roughly 30% of the within-individual standard deviation. Contrary to the very large numbers, the coefficient for very small numbers moves towards a positive estimate, though not statistically significant. The opposite signs for individuals near refugee centres of different sizes explain why there is no effect on average.

Table 4.4: Exposure dimensions: Numbers of refugees (exposure at postcode-level)

	Occupancy numbers			
	(1) Continuous (scale: 100)	(2) Median split (large: >355)	(3) small: <=33rd pct large: >67th pct.	(4) small: <=20th pct large: >80th pct.
Number of refugees	-0.0055			
	(0.0046)			
Small		0.0047	0.0090	0.0444
		(0.0293)	(0.0316)	(0.0362)
Medium			0.0352	-0.0225
			(0.0381)	(0.0343)
Large		-0.0070	-0.0447	-0.0848***
		(0.0345)	(0.0404)	(0.0319)
Mean dep.var.	2.900			
S.d. dep.var.	0.265			
ZIP4-lvl controls	X	X	X	X
Ind.-lvl controls	X	X	X	X
Ind. FE	X	X	X	X
#Panelists	6890	6890	6890	6890
#Observations	26421	26421	26421	26421
Adjusted R2	0.0078	0.0076	0.0077	0.0078

Standard errors in parentheses are clustered at ZIP4-level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Note: Column (1) tests a linear relationship. Column (2) distinguishes below and above median occupancy. Column (3) splits the occupancy distribution in three even parts (33rd pct.<=285; 67th pct.>430). Column (4) distinguishes the top and the bottom of the distribution (20th pct.<=172; 80th pct.>549).
All regressions control for overall population and non-western population. Additional controls at ZIP4-level include percentage of 65 or older and population density. Individual-level controls include gender, level of education, occupation, age, number of children in household, type of dwelling, and civil status.

The pattern is weaker, but overall comparable, for the duration dimension of exposure

(Table 4.5). Neither the cumulative number of months nor the months of opening within the year before the attitudes elicitation are linearly related to attitudes towards immigration (columns (1) and (2)).[83] However, we take note that it seems to make a difference whether the refugee centre was open for less or more than half a year at the time of the attitude measurement: Only with a longer time to experience the presence of refugees in one's neighbourhood, attitudes to immigration may improve compared to those never-exposed.

Table 4.5: Exposure dimensions: Duration of exposure (exposure at postcode-level)

	Cumulative months since opening	Months open within year	
	(1) continuous months	(2) continuous months	(3) short: <=6 months long: 7-12 months
Months open	0.0009	0.0038	
	(0.0021)	(0.0023)	
Short exposure			-0.0332
			(0.0392)
Long exposure			0.0463*
			(0.0236)
Mean dep.var.	2.900		
S.d. dep.var.	0.265		
ZIP4-lvl controls	X	X	X
Ind.-lvl controls	X	X	X
Ind. FE	X	X	X
#Panelists	6751	6890	6890
#Observations	25877	26421	26421
Adjusted R2	0.0075	0.0077	0.0079

Standard errors in parentheses are clustered at ZIP4-level.*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$
Note: Column (1) is based on cumulative months of opening since 2011. The sample in column (1) excludes centers open in January 2011 because the exact month of opening is unknown. Columns (2)-(3) are based on the number of months of opening in the year preceding the attitudes measurement.
All regressions control for overall population and non-western population. Additional controls at ZIP4-level include percentage of 65 or older and population density. Individual-level controls include gender, level of education, occupation, age, number of children in household, type of dwelling, and civil status.

In fact, the evidence for an improvement in attitudes is stronger when the long duration of exposure coincides with small numbers of refugees (Table 4.6). Comparing the estimates for all possible interactions of the discrete categories of refugee numbers and duration reveals a

[83]Note that column (1) excludes postcodes that already had an open centre in January 2011, because we cannot perfectly calculate the cumulative number of months.

strikingly persistent pattern that holds under different cut-offs of the occupancy distribution: For every category of refugee numbers, the effect of a longer duration of exposure is significantly more positive than for a short duration of exposure within the year. In the case of long exposure to a small number of refugees, this leads to a positive difference in attitudes compared to unexposed individuals. The effect of a short duration of exposure is negative when the refugee numbers are large, but with a longer duration attitudes revert back to levels similar to those of the never-exposed.[84]

The evidence at the neighbourhood-level is consistent with the contact hypothesis. One of the conditions for a positive effect of intergroup contact to emerge is that there is sufficient time to make contact and reduce prejudice (Allport, 1954). The persistent pattern of long versus short duration of exposure within the year speaks for this condition playing an important role.[85]

[84] The six different treatment dimension intensities in column (2) are jointly different from zero (p-value = 0.008 on the F-test).

[85] If we hypothesise about why the overall effect is more negative in the presence of many refugees, we think that the condition of a shared goal between the native and immigrant group may be compromised with large numbers of refugees. (We interpret the 'shared goal' of refugees and native neighbours as living peacefully side-by-side, or the successful integration into the local society.) For one thing, locals may experience more nuisance in the neighbourhood, which could make them less sympathetic to the group of immigrants. Another explanation could be that an overwhelming number of immigrants might make cultural differences more salient.

Table 4.6: Exposure dimensions: Numbers and duration (exposure at postcode-level)

	Interaction of occupancy and duration (within year)	
	(1) small: <=33rd pct large: >67th pct.	(2) small: <=20th pct large: >80th pct.
Small × Short exposure	-0.0150 (0.0452)	0.0224 (0.0492)
Small × Long exposure	0.0526 (0.0338)	0.0865** (0.0410)
Medium × Short exposure	0.0044 (0.0678)	-0.0844 (0.0624)
Medium × Long exposure	0.0642* (0.0375)	0.0285 (0.0273)
Large × Short exposure	-0.1034 (0.0848)	-0.1089* (0.0586)
Large × Long exposure	0.0111 (0.0419)	-0.0386 (0.0447)
Mean dep.var.	2.900	
S.d. dep.var.	0.265	
ZIP4-lvl controls	X	X
Ind.-lvl controls	X	X
Ind. FE	X	X
#Panelists	6890	6890
#Observations	26421	26421
Adjusted R2	0.0079	0.0080

Standard errors in parentheses are clustered at ZIP4-level.*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Note: Short exposure refers to <=6 months, long exposure to 7-12 months within the year preceding the attitudes measurement. Occupancy categories change by column.

All regressions control for overall population and non-western population. Additional controls at ZIP4-level include percentage of 65 or older and population density. Individual-level controls include gender, level of education, occupation, age, number of children in household, type of dwelling, and civil status.

4.5.2 Low proximity: Exposure at the municipality-level

In contrast to exposure at the neighbourhood-level, hosting refugees in one's municipality does not have a strong impact on attitudes to immigration.

Expanding the geographic proximity to the refugee centre to the level of the municipality has the potential to tell us something about the channels of the effects. On the one hand, the likelihood of actual contact with the refugees decreases as compared to the situation when the refugee centre location is directly in your postcode. Similarly, the likelihood that one experiences nuisances due to the presence of the refugees is much lower. On the other hand, administrative decisions are not taken at the postcode-level but at the municipality-level. If hosting a refugee centre in the municipality has implications for the municipalities' finances, social housing or the provision of other amenities, it is reasonable to expect everyone in the municipality – and not just those living in the postcode of the refugee centre – to potentially alter their opinion on immigration matters.

Tables 4.A2-4.A4 in Appendix 4.A and Table 4.7 repeat the preceding analysis at the municipality-level. It is quite striking that while at the municipality-level considerably more individuals are exposed (implying more statistical power to identify effects), the increase in exposed individuals washes out any effect measured at the postcode-level. In the between-group comparison, the marginally significant difference in the post-treatment period disappears with an estimate of roughly half the size. We no longer find a clear effect on categories of occupancy rates, nor on short or long exposure to refugees.[86] A notable difference to exposure at the neighbourhood-level is a significantly negative effect on long exposure to large numbers of refugees (Table 4.7). While the coefficient was negative at the neighbourhood-level, it was smaller in size and not significant. Column (3) shows that this effect is indeed fully supported by those exposed in the municipality who do not live in the neighbourhood of the refugee centre. This 'spillover' effect may be explained by a concern about changes in services at the municipality-level, which is not counter-balanced by a positive effect of long duration of exposure at close proximity.

[86] The estimate on cumulative months of exposure in the continuous specification is negative and significant, but of a negligible size (-0.0009).

Table 4.7: Exposure dimensions: Numbers and duration (exposure at municipality-level)

	Interaction of occupancy and duration (within year)		
	(1)	(2)	(3)
	small: <=33rd pct large: >67th pct.	small: <=20th pct large: >80th pct.	small: <=20th pct large: >80th pct. excl. exposed at ZIP4-lvl.
Small × Short exposure	-0.0142	-0.0228	-0.0270
	(0.0165)	(0.0209)	(0.0230)
Small × Long exposure	0.0066	0.0231*	0.0246*
	(0.0120)	(0.0132)	(0.0130)
Medium × Short exposure	-0.0037	0.0001	-0.0048
	(0.0245)	(0.0163)	(0.0174)
Medium × Long exposure	0.0107	-0.0018	-0.0101
	(0.0130)	(0.0139)	(0.0143)
Large × Short exposure	0.0230	0.0170	0.0383
	(0.0223)	(0.0271)	(0.0236)
Large × Long exposure	-0.0130	-0.0531**	-0.0512**
	(0.0156)	(0.0220)	(0.0228)
Mean dep.var.	2.900		
S.d. dep.var.	0.265		
Muni.-lvl controls	X	X	X
Ind.-lvl controls	X	X	X
Ind. FE	X	X	X
#Panelists	6890	6890	6464
#Observations	26421	26421	24833
Adjusted R2	0.0074	0.0081	0.0078

Standard errors in parentheses are clustered at municipality-level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Note: Short exposure refers to <=6 months, long exposure to 7-12 months within the year preceding the attitudes measurement. Occupancy categories change by column. Column (3) excludes the postcodes in which the refugee centre is located.

All regressions control for overall population and non-western population. Additional controls at municipality-level include percentage of 65 or older and population density. Individual-level controls include gender, level of education, occupation, age, number of children in household, type of dwelling, and civil status.

4.5.3 Effect heterogeneity by individual characteristics

The average effects of dimensions and intensity of exposure in close proximity are intriguing, but it is possible that individuals are not equally affected. One of the advantages of the individual-level survey measure of attitudes is that we can explore this possibility. We focus on the broad categorisation of individuals by gender, three age groups and education levels, as those are (for the most part) stable over the sample period. Based on the previous literature on the determinants of attitudes towards immigration, we know that higher educated individuals tend to hold more favourable attitudes to immigration (Huber and Oberdabernig, 2016; Margaryan et al., 2017), while attitudes become less favourable with age (Huber and Oberdabernig, 2016; O'Rourke and Sinnott, 2006). There is no clear correlation with gender. It remains an empirical question whether these sub-groups of exposed individuals respond differently to different dimensions of exposure.

Although there are no clear-cut heterogeneities overall, three observations are worth pointing out. First, an interesting finding is that women drive the negative effect of a short duration of exposure to large numbers of refugees, and that their attitudes reverse if exposed to large numbers for a long duration. In contrast, this reversal does not occur for men's attitudes if exposed to large numbers of refugees (Table 4.A5). An explanation for this would be the (fear of) increased competition for men on the marriage market, since men constitute the majority of refugees. A second observation is that young people have a pronounced positive reaction to small numbers of refugees, regardless of the duration exposed to. The older generations, like men, hold more negative attitudes if exposed to large numbers of refugees for a long duration (Table 4.A6). Lastly, individuals with completed secondary education (or less) appear generally less sensitive to the duration dimension of exposure than to the sheer numbers. They show a consistently positive change in attitudes when exposed to small numbers of refugees, and a consistently negative change in attitudes when exposed to large numbers of refugees (Table 4.A7). This could have to do with potential labour market competition with immigrants, a channel repeatedly identified by the literature to influence attitudes towards immigration (Facchini and Mayda, 2012; Mayda, 2006; O'Rourke and Sinnott, 2006; Ortega and Polavieja, 2012; Scheve and Slaughter, 2001).

4.5.4 Evidence in favour of the identifying assumptions

Since we compare exposed to never-exposed individuals over time in both the difference-in-differences and individual fixed effects specifications, it is crucial to verify that we do not see pre-existing different trends in attitudes between these individuals, and that the effects of the dimensions of exposure can clearly only be observed for the periods and individuals that actually experienced the treatment.

Pre-trend. The pre-trend analysis relates to the difference-in-differences specifications, and provides a test of the common trend assumption. Tables 4.A8 and 4.A9 in Appendix 4.A interact the indicators for exposed individuals (at postcode- or municipality-level) with year dummies to show that there was no differential trend in attitudes for the exposed individuals. Regardless of the fact that the difference-in-differences analysis did not show a significant effect of exposure, it is reassuring that this holds for every year in the sample.

Placebo treatments. To verify that the effects of the dimensions of exposure are only truly related to the intensity of these dimensions experienced, we conduct two exercises which are similar in spirit to placebo treatments.

We first examine how often we would observe similar effects if the entire exposure package (keeping the corresponding values of refugee numbers and duration of exposure together) was randomly assigned to individuals in the panel ('random exposure'). Taking the years in which we observe individual panel members into account, we randomly shuffle the exposure values among all individuals, keeping the total number of treated individuals constant. For each generated placebo treatment we run the same fixed effects regression of the interaction of numbers and duration categories, and look at the distribution of estimates after 2000 simulations.[87] Figure 4.5 plots the estimated coefficients of two treatment categories from the simulations as histograms. In both cases, the actual estimates (from Table 4.6, column (2); plotted as red lines) are significantly different from the means of the simulated coefficients, which are centred around zero. The graphs show that it is very unlikely that we would observe similar effects 'by chance'.[88]

The second test keeps the identity of exposed individuals as in the original sample, but randomly re-assigns the intensity of exposure in both the numbers and duration dimensions ('random exposure intensity'). Figure 4.6 plots the distributions of the same estimated coefficients from 2000 simulations. With the true estimates lying very far in the tails of these distributions, it is highly unlikely that the estimated effects of different exposure dimensions would be observed in other constellations among the exposed individuals. We are thus confident that we are picking up effects that are truly related to these dimensions of exposure, and not simply the result of unobservable (time-varying) differences between the exposed and non-exposed individuals.

[87]This exercise comes close to randomisation inference (after Fisher (1935)), which is increasingly applied to make causal inference in (natural) experiments with small samples or clustered randomisation. Whereas the method is used in experiments to compute p-values without making assumptions about random sampling from the population, we use it to estimate the relationship between attitudes and exposure under counterfactual treatments. Schindler and Westcott (2017) is a recent example of a similar application.

[88]Since we did not exclude individuals that actually were exposed to the treatment package from the randomisation, it is logical that some of the random exposure treatments come close to the actual distribution.

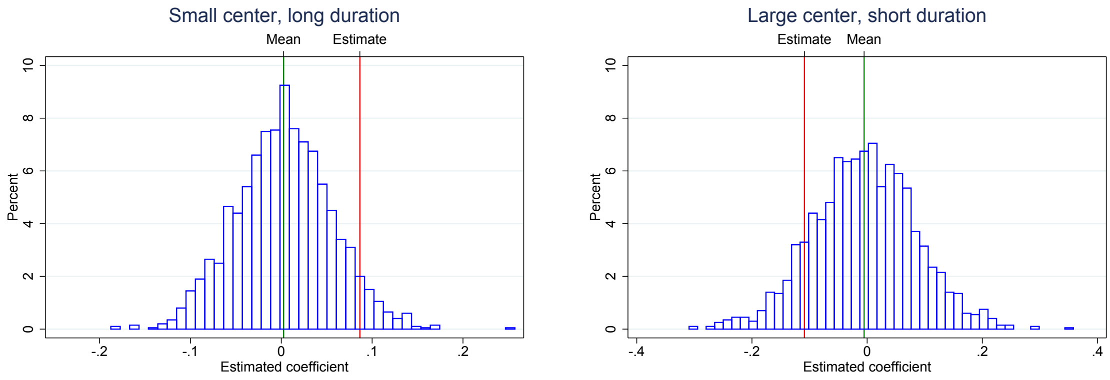


Figure 4.5: Simulations of the interaction regression under 'random exposure'

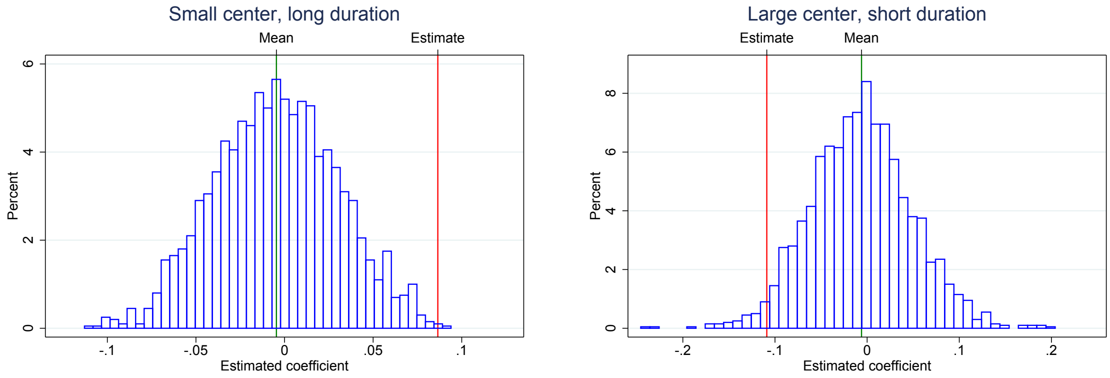


Figure 4.6: Simulations of the interaction regression under 'random dimension intensity'

4.5.5 Robustness checks

To further support our claim of a true effect of duration, numbers and proximity of exposure, we test the robustness of the interaction specification to various changes in the sample.

Balanced panel. As a first important check, we test whether our estimated effects may be driven by sample attrition. The LISS Core Study gives us a complete panel for roughly half of the individuals in the sample. It is difficult to test whether sample attrition is related to exposure to refugees,[89] but we can assert whether we estimate the same effects based on a sample that we observe over the whole sample period. Table 4.A10 in Appendix 4.A shows that this is by and large the case. While some estimates are a bit closer to zero (the negative effect of large numbers of refugees per se, or the positive effect for long exposure to small numbers of refugees), the overall picture is very similar.

Excluding exposure before the refugee crisis. Another relevant check for the fixed effects regressions is to exclude centres that were open before the refugee crisis. As explained in

[89] The LISS Core Study contains many other questionnaires unrelated to immigration, which all may contribute to the fact that individuals drop out of the sample.

Section 4.3, centres that opened after 2013 were created as a response to the sudden influx of Syrian refugees. Excluding postcodes that were exposed before achieves two things: First, it removes the potential confounding effects of those that were always exposed but changed treatment intensity, and of those that experienced the closure of a centre for a prolonged period; second, it focuses solely on variation that is arguably closer to a natural experiment. Ruling out any exposure before the refugee crisis yields very comparable results (4.A11 in Appendix 4.A, column (1)).

Excluding the biggest cities or the largest centres. A common concern in the literature is that estimates of exposure to immigrants may be driven by a few outliers in the distribution. In our setting, there are two types of outlier observations that could potentially influence the results.

The first type concerns the pool of municipalities. Amsterdam, Rotterdam and Den Haag stand out among Dutch municipalities as the only three municipalities with more than half a million people, and as being known for their international (expat) communities. Similar to Becker and Fetzer (2016) who exclude the greater area of London for parts of their analysis, we check whether the results that we find are driven by attitudes in the three biggest cities. Reassuringly, all results are qualitatively the same, with small changes to the level of statistical significance (4.A11 in Appendix 4.A, column (2)).

The three largest cities are outliers in terms of population size, but there are also outliers in terms of refugee numbers. Four centres house over 1000 refugees, which is a clear abnormality in the distribution of centre size.[90] When we drop the respective postcodes from the analysis, the results are very similar; the coefficient on short duration of exposure to large numbers of refugees becomes even stronger, indicating that this estimate was not driven by these outlier observations (4.A11 in Appendix 4.A, column (2)).

4.6 Discussion

In light of the diverging effects found in the literature to date, our results along different dimensions of exposure are intriguing. A closer inspection of the circumstances of exposure in those other cases shows that our results have the potential to reconcile all findings. Furthermore, separating dimensions of exposure gives a better idea of what might be happening 'inside the black box'.

Unifying observations. Our results show that exposure to refugees in close proximity can affect attitudes to immigration in opposite directions, depending on the interplay of proximity, duration and numbers of refugees exposed to. This is in line with the results reported in

[90] These centres are located in the municipalities Katwijk, Vlagtwedde, Dronten, and Heumen.

Steinmayr (2018), who uncovers a difference between municipalities that hosted refugees for a (presumably) longer duration and those that were in the transit zone for refugees migrating to Germany. According to our findings, two factors could contribute to this difference: Both the reduced duration of exposure and (or in combination with) the potentially much larger numbers coming through these towns could have triggered the more negative result in terms of attitudes.

The same factors would also explain that several studies on attitudes and voting behaviour in Greece find that exposure to refugees increases hostile attitudes towards immigrants and vote shares for the far-right anti-immigration party Golden Dawn (Dinas et al., 2017; Hangartner et al., 2017; Vasilakis, 2017). The three papers draw on much of the same variation in exposure to refugees, but stress different aspects of it. Vasilakis (2017) examines the numbers dimension by measuring the share of refugees on the islands, and notes that the Greek islands were exposed to masses of refugees. Dinas et al. (2017) and Hangartner et al. (2017) emphasise the short-lasting duration of exposure as the Greek islands often only serve as the point of arrival in Greece, with refugees soon making their way to the Greek mainland. Both interpretations of the type of exposure on the Greek islands are in line with our findings on numbers and duration.

The reduction in vote shares for the far-right anti-immigration party as a result of exposure to refugees in French communities (Vertier and Viskanic, 2018) equally matches our findings. The particular type of exposure to refugees studied in this paper was the result of the 'dismantling' of the 'Jungle of Calais', an illegal squatter camp in close vicinity to the tunnel and port connecting France with the United Kingdom. When the camp was dissolved, the refugees were distributed across France into very small-scale reception centres, with typically no more than a few dozen refugees.[91] A generally positive impact of small numbers of refugees on attitudes is in line with our findings, particularly if exposure lasts for more than half a year.

Channels. The different effects by dimensions and intensities of exposure shed some light on potential channels at work. While some of these dimensions have been explored in the previous literature, it is especially useful to be able to examine all of them in the setting of one country, where the broader context is comparable across municipalities.

A lot of the evidence points towards a positive type of interpersonal contact that requires some time to develop as the main driver of attitudes towards immigration. The ability to search for work after six months into the process may further facilitate contact and mixing, as well as a positive image, with the locals. Interestingly, the positive effect on long duration of exposure to a small number of refugees in the neighbourhood appears to be strongly driven

[91] The authors mention that 6400 refugees were dissipated over 200-400 centres; different data sources appear to list different figures.

by the question about one's opinion on foreigners in the neighbourhood.[92,93] This explains why the effect dissipates at the municipality-level, and provides further evidence for the contact hypothesis.

In contrast, the (more persistently) negative effect of exposure to very large numbers of refugees may be a sign of the potential for conflict between immigrants and natives. The fact that this effect is particularly pronounced for men and those with a low education level points to competition as the source of conflict. Young men with on average lower education make up the largest proportion of refugees. If large numbers of refugees stay longer in any given neighbourhood, they may pose a threat to the local men on the marriage market and compete with low educated natives for jobs.

Implications. Gaining a better understanding of whether attitudes to immigration are formed on the basis of exposure to immigrants or refugees has important (economic and political) consequences. Not only are attitudes of the local population a driver underlying voting behaviour, they are also a channel that could either facilitate or impede the successful integration of migrants in a society (Dustmann and Preston, 2001). Based on our results, the implications for refugee resettlement policy are to focus on many small, rather than fewer large reception centres for asylum seekers, and to allow for some continuity in a particular neighbourhood.

4.7 Conclusion

In this chapter, we have shown that there are different dimensions to exposure to refugees that produce effects on attitudes towards immigration in opposite directions. While a large number of refugees produces the most significant negative shift in attitudes, which affects not only the immediate neighbourhood but the whole municipality where the refugees are located, a longer duration of exposure within a given year has been consistently shown to improve attitudes compared to exposure that lasts for less than half a year. The combination of the two dimensions stipulates that exposure has the most positive effects on attitudes if it is sustained 'in small doses'.

Our analysis provides the missing link in the literature that reconciles diverging results found for different countries, and draws attention to the importance of what exposure entails. While there may be other contextual factors that shape attitudes towards immigration one way or the other, the clearly defined dimensions of proximity, duration and numbers of

[92]"It does not help a neighbourhood if many people of foreign origin or descent move in."

[93]The results from separate regressions of exposure dimensions on components of the attitudes index for exposure at the postcode-level can be found in Table 4.A12 in Appendix 4.A. The majority of estimates confirm the patterns that we find for the aggregate measure of attitudes, but are less precise.

migrants provide a benchmark for exposure that can be compared across different scenarios and give policy makers an indication of the consequences of resettlement policies.

The differences between the effects of dimensions and intensities of exposure, coupled with insights to which subgroups of the population they particularly apply, suggest certain channels that the effects operate through. A lot of the evidence points towards positive interpersonal contact, with potential for conflict situations to arise with very large numbers of refugees, but finding out what type of facilities, infrastructures or institutional details exactly promote the effects, should be the focus of future research.

Postscript: The link between Chapter 3 and Chapter 4

Chapter 4 is closely related to Chapter 3: Both study the impact of refugee (re-)settlement on the population in a Western host country. In both chapters the outcome variables belong to a class of soft measures that are linked to the integration of refugees. Social capital is defined by the OECD as "networks together with shared norms, values and understandings that facilitate co-operation within or among groups" (OECD, 2001). Trust within and between groups is a key component, but also civic engagement, cooperative norms and non-discrimination are comprised in the concept. Attitudes towards refugee resettlement as elicited in Chapter 3, and attitudes towards immigration more broadly as measured in Chapter 4, can be seen as measurements of such cooperative norms (Scrivens and Smith, 2013).

In many ways, Chapter 4 provides a more general analysis than Chapter 3, thereby creating a complementarity between the two studies. While Chapter 3 is a case study of a particular refugee resettlement programme, Chapter 4 broadens the perspective by using data on all refugee centres in the Netherlands over a six year time period. The estimation of the effect of exposure to refugees includes smaller and larger municipalities and is based on a panel study of attitudes from a representative sample of the Dutch population. At the same time, the two chapters are joint by contextual linkages. Australia and the Netherlands are two countries that are relatively similar culturally and along economic measures. OECD data show comparable GDP per capita for both countries (around US$ 50,000), and low to moderate unemployment rates of 4.8% in the Netherlands and 5.6% in Australia.[94] Interestingly, the lower unemployment rate in the Netherlands is closer to the conditions that prevail in Nhill and control towns. Both countries host relatively low numbers of refugees or asylum seekers (0.6% relative to the Dutch population, and 0.3% relative to the Australian population) and

[94] See `https://data.oecd.org/australia.htm` and `https://data.oecd.org/netherlands.htm` for comparative data. Last accessed on April 4th, 2018.

have experienced the same trend in recent years.[95]

Combining the conclusions from both studies gives a more comprehensive picture of the social capital impact of refugee resettlement on host communities. By tackling the same issue with a variety of methodologies, outcome measures and contexts, we can draw stronger insights when broad results point in the same direction – as is the case here. This is particularly promising for policy makers dealing with immigration issues, but also for researchers wishing to further our understanding of intergroup contact. Exposure to a small to moderate number of refugees in a localised context and for a longer time appears to have either a positive or no measurable impact on social capital directed towards the outgroup. In Nhill, roughly 200 refugees settled in the township within a short period of time and remained up to the point of our data collection a few years later. This number comes close to the threshold of 'small numbers of refugees' that was seen in Chapter 4, which was 172 refugees within a neighbourhood. Particularly for small numbers of refugees, but also on the aggregate for a longer duration, exposure to refugees led to more positive attitudes towards immigration in the host population. While the average neighbourhood in our Dutch sample is considerably larger than the township of Nhill, the fact that we do not observe the same positive effect at the even larger level of the municipality speaks to a localised impact of contact or exposure to the outgroup on social capital.

In both chapters we find suggestive evidence that positive results are driven by the experience of intergroup contact. In Chapter 4 it is the notable pattern of positive, non-significant and negative findings along different dimensions of exposure that point towards a higher likelihood that the conditions of Allport (1954)'s contact hypothesis are being met. In Chapter 3 a strong gender difference in combination with anecdotal evidence on the type of contact also suggest that bridging social capital can be formed on the basis of exposure. For the Netherlands, we do not observe the same stark contrast between men and women. This may have to do with the particular circumstances of exposure for women in Nhill. What is similar, though, is that women show more persistent positive change in attitudes after longer exposure as compared to men. Alternatively, the different outcome measure could explain the difference: While we find a strong gender effect on trust, attitudes towards refugee resettlement in general are not significantly different between men and women in Nhill.

Read together, the similar insights from the two studies have important policy implications. Most of all, policy makers should carefully consider the local context and conditions of where refugees (and other migrants) settle. If (re-)settlement can be organised in a controlled way, particularly focusing on a 'small numbers' approach and embedding the refugees in the

[95]See `https://data.worldbank.org/indicator/SM.POP.REFG?end=2016&locations=AU-NL&start=2011` and `http://popstats.unhcr.org/en/overview`. Last accessed on April 4th, 2018.

local community, intergroup contact between natives and the new members of society can lead to positive outcomes for social capital and need not be disruptive to the host population.

The OECD summarises succinctly why social capital, – and in particular trust and cooperative norms, the variables studied in this thesis –, are important dimensions of the functioning of society and the wellbeing of its individuals that I alluded to in the introduction to this thesis: As a public good, social capital contributes to both collective and individual wellbeing. Individual happiness and health, as well as democratic participation, social stability and economic production all benefit from cooperative norms and trust within a society. What is more, trust and positive attitudes are relatively stable attributes with potentially lasting influence across generations (Scrivens and Smith, 2013). Considering the impact of refugee resettlement on social capital is therefore vital for developing sustainable policies in this area. The joint research of Chapters 3 and 4 is a first step in this direction.

Appendix 4.A Additional tables and figures

Table 4.A1: Pairwise correlations of outcomes

	Positive attitude	anti foreigners	anti foreign neighborh.	pro multi-culturalism	pro asylum	pro same social security
anti foreigners	-0.8082*					
anti foreign neighborhood	-0.6687*	0.5535*				
pro multiculturalism	0.7263*	-0.5357*	-0.3685*			
pro asylum	0.7047*	-0.5160*	-0.4312*	0.3706*		
pro same social security	0.6545*	-0.3942*	-0.2626*	0.4124*	0.3398*	
pro foreign labor	0.6128*	-0.3497*	-0.2044*	0.3803*	0.3075*	0.2944*

Table 4.A2: Difference-in-differences: Exposure as binary variable (exposure at municipality-level)

	Pooled			Muni. FE			Indidvidual FE		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Treated muni.	0.0537	0.0522*	0.0377						
	(0.0329)	(0.0293)	(0.0266)						
Post treatment	-0.0522	-0.0401	-0.0364	-0.0070	-0.0066	-0.0014	-0.0067	-0.0065	-0.0057
	(0.0345)	(0.0303)	(0.0267)	(0.0257)	(0.0257)	(0.0247)	(0.0107)	(0.0108)	(0.0107)
2012	0.0072	0.0107	0.0042	0.0063	0.0084	0.0015	0.0027	0.0034	0.0018
	(0.0069)	(0.0074)	(0.0071)	(0.0075)	(0.0097)	(0.0094)	(0.0055)	(0.0068)	(0.0070)
2013	0.0043	0.0115	-0.0039	-0.0002	0.0043	-0.0120	0.0035	0.0050	0.0010
	(0.0078)	(0.0102)	(0.0089)	(0.0092)	(0.0148)	(0.0143)	(0.0065)	(0.0100)	(0.0105)
2015	0.0019	0.0136	-0.0194	-0.0224*	-0.0139	-0.0450*	-0.0315***	-0.0285*	-0.0363**
	(0.0129)	(0.0177)	(0.0155)	(0.0119)	(0.0271)	(0.0261)	(0.0088)	(0.0162)	(0.0171)
2016	-0.0075	0.0085	-0.0301*	-0.0362***	-0.0262	-0.0624**	-0.0420***	-0.0385*	-0.0485**
	(0.0151)	(0.0204)	(0.0181)	(0.0136)	(0.0322)	(0.0312)	(0.0099)	(0.0198)	(0.0209)
Mean dep.var.	2.900						2.900		
S.d. dep.var.	0.621						0.265		
Muni.-lvl controls		X	X		X	X		X	X
Ind.-lvl controls			X			X			X
Muni. FE				X	X	X			
Ind. FE							X	X	X
#Panelists							6890	6890	6890
#Observations	26421	26421	26421	26421	26421	26421	26421	26421	26421
Adjusted R2	0.0100	0.0142	0.0916	0.0876	0.0876	0.1517	0.0047	0.0046	0.0073

Standard errors in parentheses are clustered at municipality-level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Note: All regressions control for overall population and non-western population. Additional controls at municipality-level include percentage of 65 or older and population density. Individual-level controls include gender, level of education, occupation, age, number of children in household, type of dwelling, and civil status.

Table 4.A3: Exposure dimensions: Numbers of refugees (exposure at municipality-level)

	Occupancy numbers			
	(1) Continuous (scale: 100)	(2) Median split (large: >355)	(3) small: <=33rd pct large: >67th pct.	(4) small: <=20th pct large: >80th pct.
Number of refugees	-0.0021			
	(0.0019)			
Small		-0.0004	-0.0019	0.0025
		(0.0100)	(0.0108)	(0.0130)
Medium			0.0107	0.0038
			(0.0126)	(0.0115)
Large		0.0059	-0.0017	-0.0313*
		(0.0133)	(0.0149)	(0.0184)
Mean dep.var.	2.900			
S.d. dep.var.	0.265			
Muni.-lvl controls	X	X	X	X
Ind.-lvl controls	X	X	X	X
Ind. FE	X	X	X	X
#Panelists	6890	6890	6890	6890
#Observations	26421	26421	26421	26421
Adjusted R2	0.0074	0.0073	0.0073	0.0076

Standard errors in parentheses are clustered at municipality-level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Note: Column (1) tests a linear relationship. Column (2) distinguishes below and above median occupancy. Column (3) splits the occupancy distribution in three even parts (33rd pct.<=290; 67th pct.>438). Column (4) distinguishes the top and the bottom of the distribution (20th pct.<=219; 80th pct.>549).
All regressions control for overall population and non-western population. Additional controls at municipality-level include percentage of 65 or older and population density. Individual-level controls include gender, level of education, occupation, age, number of children in household, type of dwelling, and civil status.

Table 4.A4: Exposure dimensions: Duration of exposure (exposure at municipality-level)

	Cumulative months since opening	Months open within year	
	(1) continuous months	(2) continuous months	(3) short: <=6 months long: 7-12 months
Months open	-0.0009***	0.0003	
	(0.0003)	(0.0009)	
Short exposure			-0.0024
			(0.0136)
Long exposure			0.0040
			(0.0099)
Mean dep.var.	2.900		
S.d. dep.var.	0.265		
Muni.-lvl controls	X	X	X
Ind.-lvl controls	X	X	X
Ind. FE	X	X	X
#Panelists	6751	6890	6890
#Observations	25877	26421	26421
Adjusted R2	0.0076	0.0073	0.0073

Standard errors in parentheses are clustered at municipality-level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Note: Column (1) is based on cumulative months of opening since 2011. The sample in column (1) excludes centers open in January 2011 because the exact month of opening is unknown. Columns (2)-(3) are based on the number of months of opening in the year preceding the attitudes measurement.

All regressions control for overall population and non-western population. Additional controls at municipality-level include percentage of 65 or older and population density. Individual-level controls include gender, level of education, occupation, age, number of children in household, type of dwelling, and civil status.

Table 4.A5: Heterogeneous effects by gender (exposure at postcode-level)

	Female	Male
	(1) Occupancy 20th-80th pct.	(2) Occupancy 20th-80th pct.
Small × Short exposure	0.0490	-0.0192
	(0.0412)	(0.1153)
Small × Long exposure	0.0660	0.1228**
	(0.0502)	(0.0624)
Medium × Short exposure	-0.0950	-0.0685
	(0.0661)	(0.0660)
Medium × Long exposure	0.0546	0.0144
	(0.0432)	(0.0354)
Large × Short exposure	-0.1253***	-0.1080
	(0.0435)	(0.0741)
Large × Long exposure	0.0861	-0.1578**
	(0.0588)	(0.0648)
Mean dep.var.	2.939	2.855
S.d. dep.var.	0.254	0.276
ZIP4-lvl controls	X	X
Ind.-lvl controls	X	X
Ind. FE	X	X
#Panelists	3728	3166
#Observations	14094	12327
Adjusted R2	0.0065	0.0118

Standard errors in parentheses are clustered at ZIP4-level.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$
Note: All regressions control for overall population and non-western population. Additional controls at ZIP4-level include percentage of 65 or older and population density. Individual-level controls include level of education, occupation, age, number of children in household, type of dwelling, and civil status.

Table 4.A6: Heterogeneous effects by age group (exposure at postcode-level)

	15-34 years old	35-54 years old	55+ years old
	(1) Occupancy 20th-80th pct.	(2) Occupancy 20th-80th pct.	(3) Occupancy 20th-80th pct.
Small × Short exposure	0.3722*** (0.0974)	-0.0963 (0.0738)	-0.0052 (0.0509)
Small × Long exposure	0.3647*** (0.0478)	0.0271 (0.0782)	0.0544 (0.0465)
Medium × Short exposure	-0.4193*** (0.1170)	-0.0237 (0.1038)	-0.1516** (0.0753)
Medium × Long exposure	-0.0882 (0.0709)	0.1119* (0.0639)	-0.0683 (0.0433)
Large × Short exposure	-0.2911*** (0.0787)	-0.2574*** (0.0688)	0.1115 (0.1044)
Large × Long exposure	0.1980 (0.1459)	-0.1344 (0.1398)	-0.1084** (0.0462)
Mean dep.var.	2.944	2.887	2.914
S.d. dep.var.	0.300	0.259	0.251
ZIP4-lvl controls	X	X	X
Ind.-lvl controls	X	X	X
Ind. FE	X	X	X
#Panelists	924	2233	3038
#Observations	2587	6800	10382
Adjusted R2	0.0235	0.0067	0.0126

Standard errors in parentheses are clustered at ZIP4-level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Note: All regressions control for overall population and non-western population. Additional controls at ZIP4-level include percentage of 65 or older and population density. Individual-level controls include gender, level of education, occupation, number of children in household, type of dwelling, and civil status.

Table 4.A7: Heterogeneous effects by level of education (exposure at postcode-level)

	(Less than) Secondary education	Vocational education	Higher education
	(1)	(2)	(3)
	Occupancy 20th-80th pct.	Occupancy 20th-80th pct.	Occupancy 20th-80th pct.
Small × Short exposure	0.1102	-0.0416	-0.0635
	(0.0912)	(0.0606)	(0.0506)
Small × Long exposure	0.1321**	0.0111	0.0665
	(0.0565)	(0.0791)	(0.0555)
Medium × Short exposure	-0.0331	-0.1359	-0.1341**
	(0.0665)	(0.1049)	(0.0641)
Medium × Long exposure	-0.0459	0.0824*	0.0477
	(0.0499)	(0.0428)	(0.0640)
Large × Short exposure	-0.1119***	-0.1275***	0.0110
	(0.0273)	(0.0421)	(0.1257)
Large × Long exposure	-0.1708*	0.1917***	-0.0197
	(0.0913)	(0.0742)	(0.0968)
Mean dep.var.	2.799	2.793	3.115
S.d. dep.var.	0.271	0.264	0.245
ZIP4-lvl controls	X	X	X
Ind.-lvl controls	X	X	X
Ind. FE	X	X	X
#Panelists	3202	1706	2347
#Observations	11584	6257	8580
Adjusted R2	0.0063	0.0120	0.0150

Standard errors in parentheses are clustered at ZIP4-level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Note: All regressions control for overall population and non-western population. Additional controls at ZIP4-level include percentage of 65 or older and population density. Individual-level controls include gender, occupation, age, number of children in household, type of dwelling, and civil status.

Table 4.A8: Trend analysis for difference-in-differences (exposure at postcode-level)

	Trend analysis		
	(1)	(2)	(3)
	Pooled	ZIP4 FE	Ind. FE
Treated ZIP4	-0.0404		
	(0.0403)		
2012	0.0004	0.0015	-0.0002
	(0.0067)	(0.0070)	(0.0061)
2013	-0.0117	-0.0105	-0.0052
	(0.0081)	(0.0092)	(0.0078)
2015	-0.0398***	-0.0408***	-0.0459***
	(0.0103)	(0.0124)	(0.0106)
2016	-0.0556***	-0.0562***	-0.0614***
	(0.0114)	(0.0146)	(0.0126)
2012 × Treated ZIP4	-0.0021	-0.0083	-0.0184
	(0.0282)	(0.0346)	(0.0251)
2013 × Treated ZIP4	0.0224	0.0282	0.0171
	(0.0309)	(0.0303)	(0.0278)
2015 × Treated ZIP4	-0.0106	-0.0041	-0.0046
	(0.0366)	(0.0280)	(0.0353)
2016 × Treated ZIP4	-0.0109	0.0000	0.0101
	(0.0372)	(.)	(0.0336)
ZIP4-lvl controls	X	X	X
Ind.-lvl controls	X	X	X
ZIP4 FE		X	
Ind. FE			X
#Panelists			6890
#Observations	26421	26421	26421
Adjusted R2	0.0947	0.3566	0.0077

Standard errors in parentheses are clustered at ZIP4-level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Note: All regressions control for overall population and non-western population. Additional controls at ZIP4-level include percentage of 65 or older and population density. Individual-level controls include gender, highest level of education, occupation, age, number of children in household, type of dwelling, and civil status.

Table 4.A9: Trend analysis for difference-in-differences (exposure at municipality-level)

	Trend analysis		
	(1)	(2)	(3)
	Pooled	Muni. FE	Ind. FE
Treated muni.	0.0312		
	(0.0261)		
2012	0.0187	0.0178	0.0154
	(0.0224)	(0.0242)	(0.0181)
2013	0.0206	0.0150	0.0136
	(0.0253)	(0.0281)	(0.0227)
2015	0.0238	0.0036	0.0238
	(0.0318)	(0.0381)	(0.0307)
2016	-0.0196	-0.0501	-0.0085
	(0.0353)	(0.0441)	(0.0346)
2012 × Treated muni.	-0.0094	-0.0039	0.0155
	(0.0137)	(0.0202)	(0.0160)
2013 × Treated muni.	-0.0158	-0.0105	0.0172
	(0.0150)	(0.0184)	(0.0148)
2015 × Treated muni.	-0.0336*	-0.0241	-0.0135
	(0.0187)	(0.0148)	(0.0137)
2016 × Treated muni.	-0.0141	0.0000	0.0000
	(0.0207)	(.)	(.)
Muni.-lvl controls	X	X	X
Ind.-lvl controls	X	X	X
Muni. FE		X	
Ind. FE			X
#Panelists			6890
#Observations	26421	26421	26421
Adjusted R2	0.0912	0.1517	0.0076

Standard errors in parentheses are clustered at municipality-level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Note: All regressions control for overall population and non-western population. Additional controls at municipality-level include percentage of 65 or older and population density. Individual-level controls include gender, highest level of education, occupation, age, number of children in household, type of dwelling, and civil status.

Table 4.A10: Dimensions of exposure on balanced panel (exposure at postcode-level)

	Balanced panel (2011 – 2016)		
	(1) Occupancy (20th-80th pct.)	(2) Duration (within year)	(3) Interaction of occupancy and duration
Small	0.0577 (0.0371)		
Medium	0.0062 (0.0516)		
Large	-0.0553 (0.0422)		
Short exposure		0.0150 (0.0466)	
Long exposure		0.0343 (0.0313)	
Small × Short exposure			0.0577 (0.0514)
Small × Long exposure			0.0586 (0.0389)
Medium × Short exposure			-0.0090 (0.0940)
Medium × Long exposure			0.0247 (0.0443)
Large × Short exposure			-0.0920** (0.0464)
Large × Long exposure			-0.0233 (0.0678)
Mean dep.var.			
S.d. dep.var.			
ZIP4-lvl controls	X	X	X
Ind.-lvl controls	X	X	X
Ind. FE	X	X	X
#Panelists	3365	3365	3365
#Observations	16656	16656	16656
Adjusted R2	0.0116	0.0114	0.0114

Standard errors in parentheses are clustered at ZIP4-level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Note: All regressions control for overall population and non-western population. Additional controls at ZIP4-level include percentage of 65 or older and population density. Individual-level controls include gender, level of education, occupation, age, number of children in household, type of dwelling, and civil status.

Table 4.A11: Dimensions of exposure on different samples (exposure at postcode-level)

	Interaction of occupancy (20th-80th pct.) and duration (within year)		
	(1) Exposed after 2013	(2) Excluding largest cities	(3) Excluding largest centres
Small × Short exposure	0.0453	-0.0254	0.0223
	(0.0461)	(0.0358)	(0.0492)
Small × Long exposure	0.1217**	0.0817*	0.0861**
	(0.0497)	(0.0425)	(0.0411)
Medium × Short exposure	-0.1241*	-0.0807	-0.0856
	(0.0668)	(0.0648)	(0.0624)
Medium × Long exposure	-0.0053	0.0292	0.0269
	(0.0346)	(0.0276)	(0.0272)
Large × Short exposure	-0.1068*	-0.0831	-0.2078***
	(0.0565)	(0.0579)	(0.0301)
Large × Long exposure	0.0186	-0.0365	-0.0392
	(0.0815)	(0.0451)	(0.0476)
Mean dep.var.	2.903	2.889	2.900
S.d. dep.var.	0.265	0.264	0.265
ZIP4-lvl controls	X	X	X
Ind.-lvl controls	X	X	X
Ind. FE	X	X	X
#Panelists	6745	6345	6856
#Observations	25854	24616	26287
Adjusted R2	0.0080	0.0082	0.0080

Standard errors in parentheses are clustered at ZIP4-level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Note: Short exposure refers to <=6 months, long exposure to 7-12 months within the year preceding the attitudes measurement. The columns are based on different samples to test for robustness.

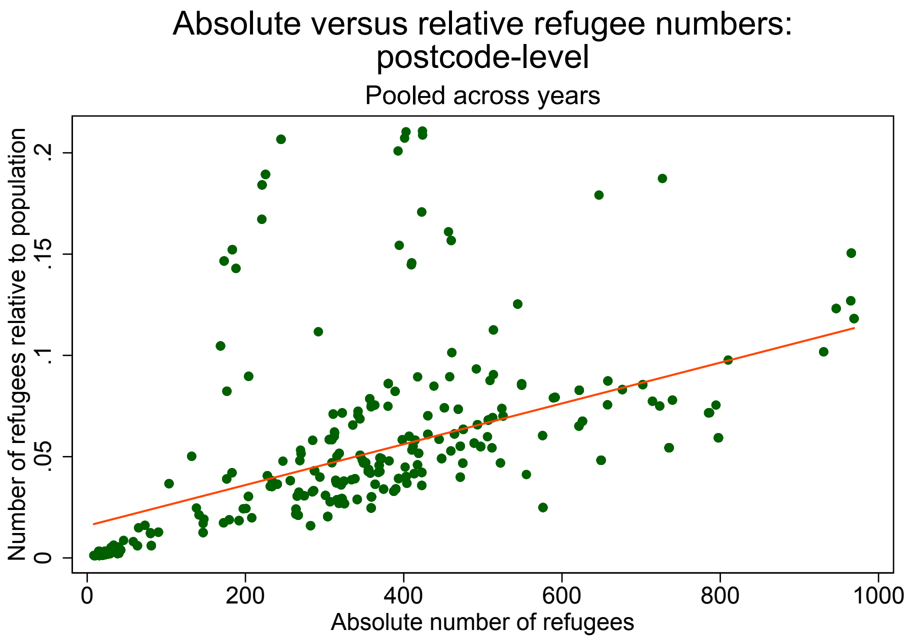
All regressions control for overall population and non-western population. Additional controls at ZIP4-level include percentage of 65 or older and population density. Individual-level controls include gender, level of education, occupation, age, number of children in household, type of dwelling, and civil status.

Table 4.A12: Components of the attitude index
(exposure at postcode-level)

	Negative statements		Positive statements			
	(1) Too many foreigners	(2) Anti diverse neighborhood	(3) Pro diversity	(4) Easier asylum	(5) Same social security	(6) Foreign labor good
Small × Short exposure	-0.1457*	0.0225	0.0145	-0.0522	-0.0335	0.0825
	(0.0864)	(0.1336)	(0.0642)	(0.0757)	(0.0745)	(0.0655)
Small × Long exposure	-0.1949**	-0.1247*	0.1088	0.0696	-0.0410	0.0617
	(0.0824)	(0.0689)	(0.0729)	(0.0952)	(0.0831)	(0.1205)
Medium × Short exposure	0.0141	0.1103	-0.0739	0.0453	-0.1394*	-0.2137
	(0.0756)	(0.0841)	(0.0589)	(0.1135)	(0.0839)	(0.1590)
Medium × Long exposure	0.0102	-0.0262	0.0726	0.0592	0.0031	0.0201
	(0.0665)	(0.1123)	(0.0879)	(0.0860)	(0.0913)	(0.0617)
Large × Short exposure	0.2728	0.2587	-0.0587	-0.2618	0.0876	0.1110
	(0.1963)	(0.2734)	(0.1231)	(0.2109)	(0.0652)	(0.1504)
Large × Long exposure	0.0334	0.0813	-0.1060	-0.0241	0.0622	-0.0489
	(0.1156)	(0.1661)	(0.1719)	(0.1200)	(0.1661)	(0.1275)
Mean dep.var.	3.234	3.599	3.535	2.199	3.419	3.081
S.d. dep.var.	0.510	0.524	0.450	0.517	0.601	0.575
ZIP4-lvl controls	X	X	X	X	X	X
Ind.-lvl controls	X	X	X	X	X	X
Ind. FE	X	X	X	X	X	X
#Panelists	6890	6890	6890	6890	6890	6890
#Observations	26421	26421	26421	26421	26421	26421
Adjusted R2	0.0040	0.0028	0.0048	0.0136	0.0073	0.0084

Standard errors in parentheses are clustered at ZIP4-level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: All regressions control for overall population and non-western population. Additional controls at ZIP4-level include percentage of 65 or older and population density. Individual-level controls include gender, level of education, occupation, age, number of children in household, type of dwelling, and civil status.



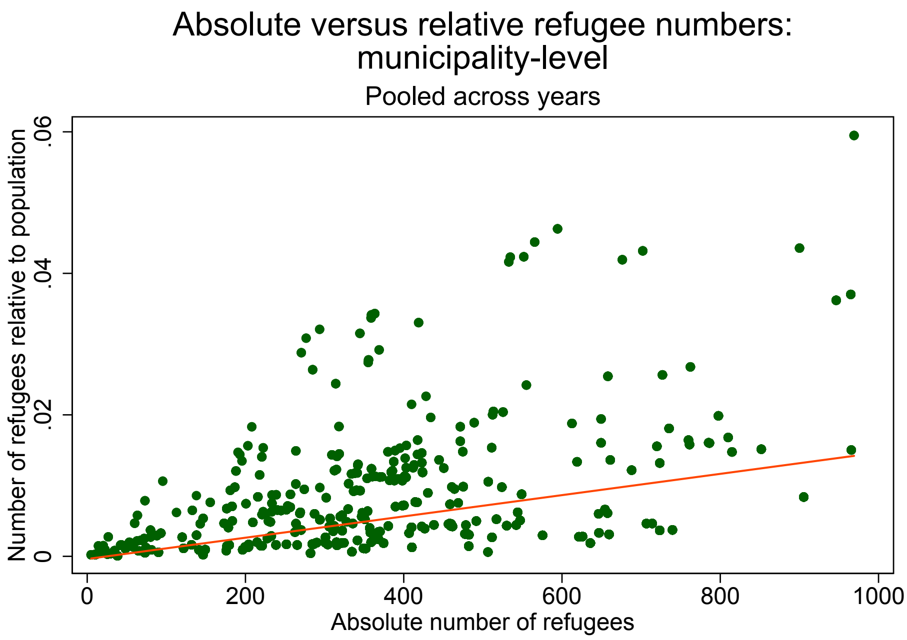


Figure 4.A1: Absolute versus relative numbers of refugees

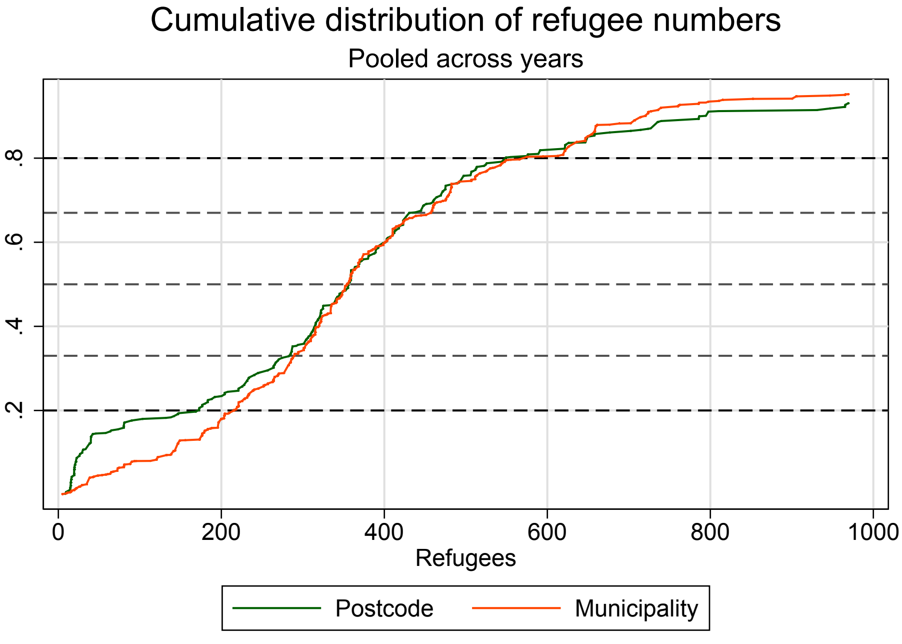


Figure 4.A2: Cut-offs from the distribution of refugee numbers

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Summary: Empirical Studies in Labour and Migration Economics

This thesis consists of three empirical studies investigating topics in the fields of labour and migration economics. The summary provides an overview of the research questions addressed, the methodologies used to answer those questions, and the conclusions from this research.

Chapter 2 falls in the realm of labour economics. At the heart of this chapter stands the question of whether international differences in earnings inequality between skilled and (relatively) unskilled workers can be explained by differences in the relative supply of and demand for skilled and unskilled workers across countries. The workhorse model for this analysis is the canonical supply and demand model for skill. The empirical implication of the model is a negative relationship between relative net supply (i.e., supply minus demand) differences and earnings inequality: the larger the net supply of, for example, high skilled workers relative to others, the smaller the degree of earnings inequality between high skilled workers and others. The study makes use of a dataset that measures skill on a comparable scale across a wide range of countries to test this relationship. The dataset contains direct measures of numeracy, literacy and computer skills of over 50,000 adults from 15 countries. Using these measures to divide the sample into three skill groups (high, medium and low), the results suggest that, overall, the simple supply and demand framework explains roughly 30 percent of the international variation between skill groups' earnings. This is a sizeable proportion, and in spite of the fact that country-specific labour market institutions may work against this mechanism. The results are particularly pronounced for workers at the bottom of the skill distribution, where supply and demand account for almost 50 percent of the international differences in relative earnings. The estimated relationship across countries states that a 10 percent lower relative net supply of low-skilled workers is associated with 1.5 percent higher relative earnings. This study is the first to look at computer skills in that framework and to consider a complete labour market that includes women and unemployed people in the sample.

Both Chapter 3 and Chapter 4 are placed in the field of migration economics, dealing with the topical issue of refugee migration. Chapter 3 examines one particular aspect of refugee migration that has received little attention in the economics literature: How does the resettlement of refugees into a small township affect social capital among the natives? Specifically, how does the local population's level of trust and attitudes to refugees change due to exposure to refugees? These questions are addressed by means of a case study in rural Australia, in which the locals of a small country town experienced a large influx of refugees over the course of a few years. The resettlement was exogenous with respect to social indicators of the township and filled an unmet labour demand in the host community. The former feature helps to establish a credible comparison between this town and other control towns that are similar in a variety of dimensions, except for the contact with refugees. The latter characteristic allows to focus on the social impact of contact with refugees, without the confounding effects of increased labour market pressures or higher crime rates that are sometimes associated with refugee migration. The study combines self-collected data on trust towards refugees and natives from an incentivised survey (using the trust game) with repeated cross-sectional survey data on other indicators of social capital from treatment and control towns. A difference-in-differences approach shows that the resettlement has left social capital unchanged in most dimensions, but has had a positive effect on trust to refugees relative to natives particularly for the female population. Furthermore, residents of the treated town developed more favourable views on refugee resettlement in general. A weighted synthetic control group analysis supports these findings, which are a promising first step in advancing research into what could make refugee resettlement beneficial for both sides.

Chapter 4 broadens the perspective on the impact of exposure to refugees on the host population by focusing on attitudes to immigration as a more general outcome measure in a country-wide analysis. This study uses administrative data on all refugee centres in the Netherlands over a six year period, which allows to distinguish between different dimensions and intensities of exposure to refugees. Specifically, the duration of exposure, numbers exposed to and proximity to the individual may influence attitudes in different ways. Looking at these dimensions separately and in interaction with each other is a novel contribution to the literature that has the potential to reconcile the prevailing diverging results. To show this, the study combines the data on exposure to refugees with individual-level panel data on attitudes to immigration. Individual fixed effects regressions reveal that exposure to small numbers of refugees has a significantly positive effect on attitudes if experienced for a long enough time. Exposure to large numbers of refugees significantly reduces attitudes, particularly with a short duration of exposure. Overall, the results only hold when individuals are exposed to refugees in close proximity (i.e., at the level of the neighbourhood as opposed to the municipality). These findings complement the insights from Chapter 3 and help to reconcile differences across several studies; as a first systematic approach to what ‘exposure to refugees’ entails, they provide useful advice to policymakers.

Samenvatting: Empirische Studies in Arbeids- en Migratie-Economie

Dit proefschrift bestaat uit drie empirische studies die onderwerpen op het gebied van arbeids- en migratie-economie onderzoeken. De samenvatting geeft een overzicht van de onderzoeksvragen, de methodologieën die gebruikt zijn om deze vragen te beantwoorden en de conclusies van dit onderzoek.

Hoofdstuk 2 valt in het gebied van de arbeidseconomie. Centraal in dit hoofdstuk staat de vraag of internationale verschillen in inkomensongelijkheid tussen geschoolde en (relatief) ongeschoolde werknemers kunnen worden verklaard door verschillen in het relatieve aanbod van en de vraag naar geschoolde en ongeschoolde werknemers in de verschillende landen. Het model voor deze analyse is het canonieke vraag- en aanbodmodel voor vaardigheden. De empirische implicatie van het model is een negatief verband tussen de verschillen in het relatieve netto aanbod (d.w.z. aanbod minus vraag) en inkomensongelijkheid: hoe groter het netto aanbod van bijvoorbeeld hooggeschoolde werknemers ten opzichte van anderen, hoe kleiner de mate van inkomensongelijkheid tussen hooggeschoolde werknemers en anderen. De studie maakt gebruik van een dataset die in een groot aantal landen vaardigheden op een vergelijkbare schaal meet om deze relatie te testen. De dataset bevat directe metingen van rekenvaardigheid, leesvaardigheid en computervaardigheid van meer dan 50.000 volwassenen uit 15 landen. Met behulp van deze maten word de steekproef in drie vaardigheidsgroepen (hoog, gemiddeld en laag) opgedeeld. Op basis van deze groepering suggereren de resultaten dat het eenvoudige aanbod- en vraagmodel in totaal ongeveer 30 procent van de internationale variatie tussen de verdiensten van de vaardigheidsgroepen verklaart. Dit is een aanzienlijk deel, en ondanks het feit dat landspecifieke arbeidsmarktinstituties tegen dit mechanisme kunnen werken. De resultaten zijn met name duidelijk voor werknemers aan de onderkant van de vaardigheidsverdeling, waar vraag en aanbod bijna 50 procent van de internationale verschillen in relatieve inkomsten uitmaken. De geschatte relatie tussen landen geeft aan dat een 10 procent lager relatief netto aanbod van laaggeschoolde werknemers wordt geassocieerd met 1,5 procent hogere relatieve inkomsten. Deze studie is de eerste om computervaardigheden in het kader van het vraag- en aanbodmodel voor vaardigheden te onderzoeken en om een complete arbeidsmarkt te overwegen die vrouwen en werklozen in de steekproef omvat.

Zowel Hoofdstuk 3 en Hoofdstuk 4 vallen in het gebied van migratie-economie en gaan om de actuele kwestie van vluchtelingenmigratie. Hoofdstuk 3 onderzoekt een bepaald aspect van vluchtelingenmigratie dat weinig aandacht heeft gekregen in de economische literatuur: Hoe beïnvloedt de plaatsing van vluchtelingen in een kleine gemeente het sociale kapitaal onder de lokale bevolking? En specifiek, hoe verandert het niveau van vertrouwen en de houding van de lokale bevolking ten opzichte van vluchtelingen als gevolg van contact met vluchtelingen? Deze vragen worden behandeld door middel van een casus op het platteland van Australië waar de inwoners van een kleine stad te maken kregen met een grote instroom van vluchtelingen gedurende een aantal jaren. De hervestiging was exogeen met betrekking tot sociale indicatoren van de stad en vulde een behoefte aan werknemers in de gemeenschap. Het eerste kenmerk helpt bij het vaststellen van een betrouwbare vergelijking tussen deze stad en andere controlesteden die in verschillende dimensies vergelijkbaar zijn maar met uitzondering van het contact met vluchtelingen. Het laatste kenmerk laat toe om te focussen op de sociale impact van het contact met vluchtelingen, zonder de verstorende effecten van toegenomen arbeidsmarktdruk of hogere criminaliteitscijfers die soms geassocieerd worden met vluchtelingenmigratie. De studie combineert zelf-verzamelde gegevens over vertrouwen in vluchtelingen en autochtonen van een geincentiveerde enquête (gebruik makend van het 'trust game') met herhaald cross-sectioneel onderzoek over andere indicatoren van sociaal kapitaal van zowel de 'behandelde' stad en controlesteden. Een 'difference-in-differences' analyse toont aan dat de plaatsing van vluchtelingen het sociale kapitaal in de meeste dimensies onveranderd heeft gelaten, maar een positief effect heeft gehad op het vertrouwen in vluchtelingen ten opzichte van autochtonen, en dit vooral voor de vrouwelijke bevolking. Bovendien ontwikkelden inwoners van de behandelde stad gunstigere opvattingen over hervestiging van vluchtelingen in het algemeen. De vergelijking met een (gewogen) synthetische controlegroep ondersteunt deze bevindingen, die een veelbelovende eerste stap zijn in het bevorderen van onderzoek naar wat de hervestiging van vluchtelingen voordelig zou kunnen maken voor beide partijen.

Hoofdstuk 4 verbreedt het perspectief op de impact van contact met vluchtelingen op de opvangende bevolking door te focussen op de houding ten opzichte van immigratie als een algemenere uitkomstmaat in een landelijke analyse. Deze studie maakt gebruik van administratieve gegevens over alle opvanglocaties in Nederland gedurend een periode van zes jaar, waardoor een onderscheid kan worden gemaakt tussen verschillende dimensies en intensiteiten van blootstelling aan vluchtelingen. Concreet kan de duur, getallen en nabijheid bij de vluchtelingencentra op verschillende manieren houdingen beïnvloeden. Deze dimensies afzonderlijk en in interactie met elkaar bekijken is een nieuwe bijdrage aan de literatuur die het potentieel heeft om de heersende, uiteenlopende resultaten met elkaar te verzoenen. Om dit aan te tonen, combineert het onderzoek de gegevens over blootstelling aan vluchtelingen met paneldata op individueel niveau over houdingen ten opzichte van immigratie. Indivdiual fixed effects regressies laten zien dat blootstelling aan kleine aantallen vluchtelingen een significant positief effect heeft op de houdingen als dit lang genoeg duurt. Blootstelling aan grote aantallen vluchtelingen vermindert de houdingen significant, vooral met een korte duur van blootstelling. In het algemeen zijn de resultaten alleen geldig wanneer personen worden blootgesteld aan vluchtelingen in de onmiddellijke nabijheid (d.w.z. op het niveau van de buurt in tegenstelling tot de gemeente). Deze bevindingen bieden een aanvulling op de inzichten uit Hoofdstuk 3 en helpen om verschillen tussen diverse onderzoeken met elkaar te verzoenen; en als een eerste systematische benadering van wat 'blootstelling aan vluchtelingen' inhoudt, bieden ze nuttig advies aan beleidsmakers.

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Two chapters are the result of collaborations with the following co-authors:

- Chapter 3, entitled “When refugees work: The social capital effects of resettlement on host communities”, is co-authored by dr. David Smerdon, University of Queensland. The study has received additional financial support from the *Behavioural Priority Area* at the University of Amsterdam and the *C. Willems Stichting*.
  The work is based on the equal contributions of (then) two PhD candidates at the time of designing the study, collecting primary and secondary data, analysis and writing.

- Chapter 4, entitled “Exposure to refugees and attitudes to immigration”, is based on joint work with dr. Elena Cettolin, dr. Riccardo Ghidoni and prof. dr. Sigrid Suetens, all Tilburg University.
  While all of the results and writing in this chapter have been produced by the author of this thesis, the research question, empirical strategy and presentation of the data go back to many joint discussions and careful data analysis by all co-authors. The data used in this chapter would not be accessible without the efforts of my co-authors.

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