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Sick of Your Poor Neighborhood?*

Quasi-Experimental Evidence on Neighborhood Effects on Health

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Does living in a low-income neighborhood have negative health consequences? We document causal neighborhood effects on health by exploiting a Spatial Dispersal Policy that quasi-randomly resettled refugees across neighborhoods and apartment buildings from 1986 to 1998. Refugees allocated to low-income neighborhoods had a 12 percent higher risk of having developed a lifestyle related disease 8 to 15 years after immigration compared with those allocated to high-income neighborhoods. Our results suggest that interaction with neighbors and the characteristics of the immediate environment are important determinants for health outcomes. Our results further suggest that differences in health care access, ethnic networks, and individual labor market outcomes are not the main drivers behind the neighborhood effects on health.

JEL Classification: J15, I12, I14, I31
Keywords: Health inequality, Refugee Dispersal Policy, lifestyle related diseases, neighborhood effects

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Lifestyle related diseases are responsible for more than 70 percent of deaths worldwide each year, and more than a third of these deaths occur between ages 30-69 (WHO (2018)). Such diseases not only lead to higher mortality rates, but are also associated with life-long decreased life quality. At the same time, a larger share of people living in low-income areas suffer from these types of diseases, creating substantial inequality in health across neighborhoods (Chetty et al., 2016b).

But why do people living in low-income areas have poorer health? Potential explanations include differential access to or quality of health care, income effects on health through differences in labor market opportunities, exposure to crime and disparities in education or health knowledge, among others. Another potential explanation is that low-income areas contribute to unhealthy lifestyle choices, such as physical inactivity, poor diets, and higher use of tobacco and alcohol, either because local amenities do not support healthy living or because such behaviors are reinforced through social interaction with neighbors. In other words, living in a low-income area can affect health negatively.

However, observing that residents in poorer areas have worse health does not necessarily imply that neighbors' lifestyle choices or the characteristics of the local area actually affect residents' health. It could simply be explained by selection, since individuals with poor health may only be able to afford housing in low-income neighborhoods. One could also imagine that individual income determines both neighborhood choice and health, and thus explains the observed neighborhood income impact on health. Moreover, neighborhoods may also affect the individual's earnings prospects, which could directly impact health. These points highlight that establishing a causal relationship between residential location and health is notoriously difficult.

In this paper, we exploit quasi-random assignment of refugee families to apartment buildings and neighborhoods in Denmark to overcome these challenges, and we document significant causal impacts of neighborhoods on a wide range of lifestyle related diseases. Moreover, we explore the potential mechanisms behind neighborhood effects on health and document causal place effects on health across locations as small as apartment buildings. This provides new evidence suggesting that place effects on health operate through interaction with neigh-

bors and very local area characteristics – as opposed to labor market conditions and health care access or quality.

For identification, we exploit a natural experiment created by the Danish Spatial Dispersal Policy in effect from 1986 to 1998, that quasi-randomly assigned refugee families to different apartment buildings located in different types of neighborhoods upon arrival to Denmark.[1] The neighborhoods in our analysis are parishes, which historically have delineated small communities and, in recent years, have been home to around 5,000 inhabitants. Recently, local lockdowns have been targeted to parishes to prevent the spread of covid-19. In order to measure neighborhood quality we divide all neighborhoods into three equally sized groups in each year based on the median household disposable income per adult household member in the neighborhood one year prior to the refugees' arrival. Our results show that refugees placed in low-income and thus more disadvantaged neighborhoods experience significantly worse health outcomes in the following years.

We regard median household disposable income as a simple summary measure of neighborhood quality, since neighborhood income is correlated with other neighborhood characteristics, such as employment, crime and poverty rates.[2] This correlation also implies that the estimated effect of neighborhood income on health does not necessarily reflect the partial impact of neighborhood income. To account for characteristics at the larger geographical level, we compare refugees allocated to neighborhoods within the same municipality and we control for time-varying municipality characteristics, such as proxies for health care access, local labor demand, and the size of the potential network.

We base our analysis on longitudinal administrative registers, where we observe residential locations, income, hospital diagnoses and other individual characteristics. By studying hospital diagnoses we only capture the most severe conditions. As a complement, we therefore also show that there are no significant differences in the number of visits to GPs and psychologists, which limits concerns that refugees in richer neighborhoods are diagnosed earlier when the health conditions are potentially less severe.

[1] A number of papers use this natural experiment to study other questions. See Damm and Dustmann (2014); Foged and Peri (2016); Dustmann et al. (2018, 2023) among others.

[2] In a similar spirit, studies of the *Moving to Opportunity* experiment have used neighborhood poverty rates as a summary measure of neighborhood quality, see for example Kling et al. (2007).

Our analysis is comprised of two different parts. First, we show that being assigned to the poorest third of neighborhoods increases the risk of suffering from a lifestyle related disease by 12.7 and 12 percent relative to assignment to middle- or top-income neighborhoods, respectively. On average, we find no significant impact on mental health diagnoses. Moreover, we show that the negative health effects of being assigned to the poorest third of neighborhoods are larger for females and older refugees.

In the second part of our analysis we take a step towards understanding the documented neighborhood effect on health. A neighborhood may influence its residents' physical and mental health in multiple ways, for example, through access to health care, labor market opportunities, transmission of behavior from neighbors (e.g., health habits), pollution levels, and the area's local amenities (e.g., recreational areas or grocery store options).[3] All these factors could potentially affect lifestyle choices and thus the development of lifestyle related diseases (Patienthåndbogen, 2017). Since some of these factors may affect mental health, we include mental health diagnoses in our analysis.

The universal health care system in Denmark ensures that, in general, any differences in access to and quality of health care across geographical areas are relatively modest.[4] Furthermore, in our empirical analysis we compare individuals in different neighborhoods within the same municipality, who are subject to the same local health authorities.

Moreover, we show that the estimated neighborhood impact on health is not a result of more advantageous labor market outcomes for individuals placed in higher income neighborhoods. Our results show that there are no significant differences in labor market outcomes across neighborhood income levels. This finding is in line with previous work studying neighborhood effects, that documents that there is no association between a local area's quality and labor market outcomes for residents (see Damm (2014), Sanbonmatsu et al. (2011); Kling et al. (2007); Oreopoulos (2003) among others). Therefore, we attribute the estimated health effects to neighborhood quality rather than to individual income.[5] In addition, we find that in

[3]We refer to Sanbonmatsu et al. (2011) and Chyn and Katz (2021) for a complete overview of potential mechanisms through which neighborhoods may influence mental and physical health.

[4]While geographic access is relatively uniform, evidence suggests that the quality of care may vary by patients' socioeconomic status (e.g., Prior et al. (2022)). In our setting, this is less likely to explain the neighborhood effect on health outcomes, since the refugees in our sample generally face similar socioeconomic conditions.

[5]Similarly, we do not find differences in receipt of transfer income.

richer neighborhoods, more refugees obtain a vocational education. As discussed by Cutler and Lleras-Muney (2006), education may affect health through multiple channels. While we can test some of these mechanisms in our setting, others remain unobservable. For these reasons, we cannot rule out that the observed differences in education affect refugees' health outcomes. However, we find, that the neighborhood effect on health is driven primarily by older refugees, for whom we find no corresponding neighborhood effect on education.

Peer health behaviors and access to certain local amenities are not directly observed in our study. To partially capture the influence of these unobserved factors, we examine variation across very small local environments. In particular, we compare individuals living in different types of apartment buildings within the same municipality or even the same parish. Defining neighborhoods as these small geographical units changes how well we capture features of the immediate environment, including potential peer groups and the character of the immediate neighborhood.[6] We find that the very local geographical area in which the refugees live, is more predictive of health outcomes than the characteristics of the larger geographical area. This suggests that transmission of behaviors from neighbors and local amenities are part of the mechanisms through which neighborhoods affect residents' health.

We contribute to the literature on neighborhood effects in two ways. First, we document the existence of strong and significant long-term causal effects of neighborhood assignment on a wide range of lifestyle related diseases. To the best of our knowledge, we are the first to show that such effects do not only exist within municipalities or parishes, but also within small local environments, such as apartment buildings.

Second, we examine potential mechanisms behind these effects. While prior work has emphasized the role of health care delivery in explaining geographical variation in health outcomes (e.g., Cutler et al. (2019); Badinski et al. (2023)), our findings point to the importance of the very local environment in shaping neighborhood effects on health. Our paper suggests that these effects are likely driven by access to local amenities, such as healthy food options and opportunity for physical activity, as well as interaction with neighbors. This points to a distinct

[6]Throughout the analysis we use the term 'apartment building' to describe individuals living in an apartment building where the apartments share the same stairway. In some cases apartment buildings have multiple stairways and in this case we use 'apartment building' to refer to a smaller unit than the actual apartment building.

channel for neighborhood effects on health that operates independently of formal health care provision.

Our study relates to a large literature on neighborhood effects. An important contributor to the knowledge on neighborhood effects has been the randomized controlled trial *Moving to Opportunity* experiment, which was carried out from 1994 to 1998 in five big American cities, see, for example Katz et al. (2001), Kling et al. (2007) or Chetty et al. (2016a). The experiment shows that moving to a low-poverty neighborhood significantly improves subjective well-being (Ludwig et al. (2012)), decreases the risk of an extreme body mass index and elevated blood sugar levels (Ludwig et al. (2011)), and improves adult mental health (Kling et al. (2007)), while Pollack et al. (2019) find no significant effect on adult hospital utilization. The literature also includes non-experimental evidence on neighborhood effects on health, for example on mental health, proxied by purchases of psychotropics, among social housing clients (Boje-Kovacs et al. (2018)) and on life expectancy among the elderly (Finkelstein et al. (2019)).

Furthermore, our work relates to studies of refugees' health outcomes. White et al. (2016) consider the development of diabetes among refugees in deprived neighborhoods. Grönqvist et al. (2012) show that income inequality within neighborhoods does not impact the risk of hospitalization. We show that neighborhood income affects health outcomes independently of neighborhood income inequality. Finally, a study by Hamad et al. (2020) documents an association between neighborhood deprivation and cardiovascular risk factors among refugees in Denmark. Relative to this paper, we go beyond correlations and estimate causal neighborhood impacts, considering only refugees who were quasi-randomly allocated under the ordinary Danish Refugee Dispersal Policy.[7] Contrary to our work, these studies do not consider the impacts on mental health nor the potential mechanisms behind the effects. Compared with previous work, we also show that the adverse health effects are more pronounced when comparing very small geographical units, namely apartment buildings as opposed to municipalities or parishes.

Because of this finding, our paper also relates to the literature on spillovers in health within smaller networks. This includes, for example, Eisenberg et al. (2013) who find no or small

[7]In Hamad et al. (2020) a third of the sample originates from former Yugoslavia. This is a large group that arrived during the Balkan wars. They were not subject to the ordinary dispersal policy, and their locations were influenced by selective migration (Damm, 2005). Therefore, we follow earlier studies, that uncovered causal relationships, by excluding this group.

contagious effects of mental health between college roommates, Christakis and Fowler (2007) who document an increased risk of obesity within social networks if a person in that network becomes obese, and Fadlon and Nielsen (2019) who find spillovers in health behaviors among family members and coworkers.

In the remainder of the paper we first describe the Spatial Dispersal Policy that dispersed individuals quasi-randomly to Danish neighborhoods, which lays the foundation for our identification strategy (Section I). In this section we also describe the data sources, sample selection and the definition of our main variables of interest. In Section II we spell out the identifying assumptions, discuss potential threats to identification and provide validity tests supporting the identifying assumptions. Then we present our empirical model in Section III. Section IV provides an overview of our results which show an increased risk of developing lifestyle related diseases as a consequence of living in a low-income neighborhood. In Section V we investigate a number of potential mechanisms and show the importance of the very local environment. Finally, Section VI concludes the paper.

I Institutional Background and Data

A The Danish Spatial Dispersal Policy, 1986 to 1998

From 1986 to 1998 the Danish Refugee Council (DRC) was in charge of Danish integration efforts targeted at newly arrived refugees. Among other things, this meant that the DRC was responsible for finding permanent housing for refugees. Prior to 1986 refugees were mainly housed in the largest cities, but in 1986 the DRC adopted a Spatial Dispersal Policy (SDP) designed to spread refugees evenly across Denmark.[8] In this section we highlight the features of the policy that created exogenous variation in the allocation of refugees across municipalities, parishes and apartment buildings.

Once the Danish government had granted asylum to an asylum seeker, the newly recognized refugee filled out a questionnaire with some basic information on age, ethnicity and family

[8]See Danish Refugee Council (1991) and Danish Refugee Council (1996) for a description of the Spatial Dispersal Policy.

size.[9] We will refer to this information as 'questionnaire observables'. This questionnaire contained all the information about the refugee that was available to the DRC at the time of allocation. The DRC used the questionnaire to assign the refugees to municipalities and to start looking for suitable housing using the information about family size to find housing of an appropriate size.[10] Information about ethnicity was used to create ethnic clusters at the municipality level, which was believed to ease integration. The questionnaire information was collected from all refugees, also within the same household. However, members of the same household who got asylum at the same time were also resettled together.

Importantly for our research design, the allocation decision was based on the questionnaire alone and did not involve any personal meeting between the allocation unit and the refugee prior to allocation. Once allocated to a municipality, the housing officers in the DRC used the questionnaire to look for suitable housing. Effectively, this meant that the DRC resettled refugees independently of other individual characteristics, and the policy design therefore creates random variation in refugees' initial housing location, conditional on the questionnaire observables. This means that we can compare health outcomes for individuals who, based on questionnaire observables, were similar but were allocated to neighborhoods with different income levels to estimate the impact on health of neighborhood quality.

The practical implementation of the Spatial Dispersal Policy was influenced by a simultaneous housing shortage.[11] Specifically, the DRC struggled to find enough affordable housing of a suitable size, considering the relatively low income levels of the newly arrived refugees.[12] This shortage is best illustrated by waiting times for permanent housing, which were six months, on average, but could be up to two years.[13] The effort needed to find permanent housing options is also illustrated by the DRC's need to employ special housing officers (distinct from

[9]The questionnaire did not involve any questions on personal characteristics, such as education, prior job experience or health.

[10]In practice, the distribution of refugees was carried out in three steps: First, refugees were distributed proportionally to the number of inhabitants in each of the fifteen counties in Denmark. Next, the refugees were allocated to municipalities within counties proportionally to the number of inhabitants in each municipality. In a third and final step the DRC found permanent housing for the resettled refugees within the assigned municipality.

[11]See Danish Refugee Council (1991) and Danish Refugee Council (1996).

[12]The DRC was not allowed to buy real estate and rent it to refugees and thus relied solely on rental opportunities.

[13]See Damm (2005) for statistics on waiting times. While waiting for the DRC to find permanent housing, the refugee moved to temporary housing in the municipality that he/she was assigned to within approximately ten days of being granted asylum, see Damm and Dustmann (2014).

the refugee's case-worker) who worked full-time on finding housing. The housing shortage implied that the DRC's demand for permanent housing always exceeded the available housing options, and this effectively created queues of individuals with the same questionnaire observables waiting for permanent housing. This meant that whenever the DRC found a permanent housing opportunity, the DRC offered it to the next refugee in line whose questionnaire observables matched the housing. This prevented the DRC from placing refugees in a selective manner.

B Data

Our analysis is based on rich administrative data from Statistics Denmark, covering 1985 to 2017, which allows us to link individual records from several registers and track individuals over time. We define our main outcomes of analysis using The National Patient Registry ("LPR"), The Health Insurance Register ("SYSI", "SSSY"), The Integrated Database for Labor Market Research ("IDA") as well as the Income Register ("IND"). We supplement these longitudinal data sets with the Population Register ("BEF"), which includes information on the refugees' first address on January 1st after immigration, and we include information on country of emigration and date of settlement in a Danish municipality from the Migration Register ("VNDS"), as well as mobility within Denmark ("BEFBOP", "BEFADR"), and police records of criminal convictions ("KRAF"). Combining these data sets provides us with key demographic variables, such as age, gender, origin country and address, and it allows us to identify both relatives and neighbors.

In order to study individuals subject to the Refugee Spatial Dispersal Policy, we consider a sample of refugees who arrived between 1986 and 1998. The Migration Register does not carry information on the type of residence permit granted to immigrants in this time period. Instead we define a refugee as someone who emigrated from one of nine refugee-sending countries: Afghanistan, Ethiopia, Iran, Iraq, Lebanon, Palestine,[14] Sri Lanka and Vietnam in 1986 to 1998, and Somalia 1989 to 1998.[15] Yugoslavia was also considered a refugee-sending country

[14] Stateless refugees.

[15] See Dustmann et al. (2023); Eckert et al. (2022); Foged and Peri (2016); Damm and Dustmann (2014); Damm (2009) among others for a similar approach. We note that there is some variation across studies related to sample selection. Some studies include additional source countries, some focus on the 1986-1993 cohorts, some

in that time period, but due to the large influx of this particular group the Danish government designed a special dispersal policy for them, and they are not included in our analysis. We exclude individuals who were married to a non-refugee partner at arrival and refugees married to a refugee partner who had arrived on any earlier date.[16] This prevents the inclusion of individuals who arrived in Denmark as a result of family-reunification – individuals we do not want to include, since they would be living with their spouse instead of being allocated to a municipality through the dispersal policy. Furthermore, we restrict the sample to those aged 18-64 at arrival.

These steps leave us with a sample of 21,965 refugees whose average age at arrival is 31 years. 38 percent of them are female while more than half are married (59 percent). The average family size is 2.2, since many arrive with children, and the two largest ethnic groups in our sample are Iraqi and Somali nationals, followed by people from Lebanon and Iran. We observe the educational level at arrival in the registers for 63 percent of the sample. Of those, 48 percent have basic schooling or less, 24 percent have vocational education, while 27 percent arrive with a higher education, c.f. Table 1.

Our main outcomes in the empirical analysis are diagnoses from inpatient and outpatient hospital visits based on the National Patient Registry, which contains information about all hospital contacts reported to the Ministry of Health by the staff at the hospital where the patient received treatment. The register includes comprehensive information about every contact between patients and hospitals. Besides information about the type of care, date of contact etc., the register provides very detailed information about the condition for which the patient received treatment. We use this information about the diagnoses associated with hospital contacts to construct our main diagnosis variables, capturing the occurrence of any diagnosis within 2-15 years since immigration and the occurrence of diagnosis within 8-15 years since immigration. The differences in health outcomes typically arise 8-15 years after immigration (see Online Appendix Figure A.1). In these measures we include both primary and secondary diagnoses. The diagnoses follow the International Classification of Diseases (ICD) from World Health Or-

focus on the working age population, some studies focus on refugee men, and some studies consider the children of refugees.

[16] A non-refugee partner refers to partners who did not immigrate from any of the nine refugee sending countries in the year intervals defined above.

ganization, which contains a very fine level of detail.[17] First, we aggregate the diagnoses that we include in our analysis into two main groups: lifestyle related diseases and mental disorders. We define lifestyle related diseases as illnesses for which certain lifestyle behaviors may increase risk of diagnoses.

The lifestyle related diseases consist of circulatory diseases,[18] nutritional/endocrine/metabolic (referred to as nutritional) diseases,[19] chronic obstructive pulmonary disease (COPD), hip arthrosis and alcohol related diseases. The lifestyle related diseases we include are the most common lifestyle related diseases (Patienthåndbogen (2017)), and they account for a large share of deaths worldwide (WHO (2018)). The mental disorders considered in our analysis are disorders due to psychoactive substance use, schizophrenic disorders, mood disorders (such as depression) and neurotic disorders.[20]

We study neighborhood effects on lifestyle related diseases because the risk of developing lifestyle related diseases is influenced by individual behavior. That means that if we expect neighborhoods to influence individual behavior by altering diet or exercise habits, then we would also expect neighborhoods to affect the risk of developing these diseases. Neighborhoods could influence these behaviors through, for example, the availability of healthy grocery stores or recreational areas and also through the behavior, attitudes, and appearances of other inhabitants.[21]

Our health measure has the advantage of being very detailed and available for the full population, since health care is universal and provided free of charge to Danish residents, including refugees. However, we do expect under-detection of diseases because not every condition is diagnosed or requires a visit to a hospital, although patients can be diagnosed with multiple (and

[17] ICD-8 structure prior to 1994 and thereafter the ICD-10 structure.

[18] Hypertension, ischaemic heart diseases, pulmonary diseases, other forms of heart disease, cerebrovascular diseases and arterial diseases.

[19] Diabetes, obesity and elevated cholesterol levels. Note that we cannot distinguish between type I and type II diabetes in the data, and we therefore include both types of diabetes, even though only the risk of type II is influenced by lifestyle behaviors.

[20] More specifically, we study mental and behavioral disorders due to psychoactive substance use, schizophrenia, schizotypal and delusional disorders, mood (affective) disorders, neurotic, stress-related and somatoform disorders, behavioral syndromes associated with physiological disturbances and physical factors, and disorders of adult personality and behavior. See Online Appendix Section B for a full overview of the grouping of diagnoses.

[21] See Christakis and Fowler (2007) for examples on how the risk of obesity can be influenced by obese social contacts or Sanbonmatsu et al. (2011) for an overview of how neighborhoods may influence both mental and physical health.

less severe) conditions when visiting the hospital. For less severe conditions individuals may just receive treatment from their GP and not get referred to hospital specialists and for some conditions individuals may never see a health professional.[22] The detection rate may depend on neighborhood income levels since correlational evidence suggests that individuals with lower income generally utilize health services to a lesser extent than their more affluent counterparts (Bago d'Uva and Jones, 2009)). This may bias our estimates towards zero. Under-detection of illness could also show up as random measurement error, which will affect precision, but will not create a bias. As a complement to the hospital diagnoses, we study mortality which does not suffer from potential issues of under-detection. Furthermore, we study the number of visits to GPs and psychologists.[23]

Lastly, it is relevant to note that our health data capture the incidence, but not the prevalence, of the diseases considered in our study. For measures of outcomes at the refugee level this does not matter as we study health outcomes over a fixed time period. However, it complicates the measurement of health status in the assigned neighborhoods, particularly the very small ones, since incidence in a single year may poorly reflect the overall health of its residents.

Second, we study several labor market outcomes to analyze whether our estimated health effects are a result of differences in employment probabilities, earnings or types of occupations across neighborhoods using a combination of the Integrated Database for Labor Market Research and the Income Register. Using these data we measure employment as the fraction of a full working year. This measure takes the value one if the worker was a full-time employee during the whole year. The fraction is less than one and measures the share of a full-time equivalent if the individual was either a part-time employee or not employed in some periods throughout the year. As a measure of labor market income, we use information on annual gross earnings deflated using the consumer price index from Statistics Denmark (with the year 2000 as base year) and converted to USD using the exchange rate from the Danish Central Bank on March 27, 2020.

The information about earnings stems from annual individual-level tax returns in the In-

[22] We refer to Nielsen (2016) for a elaborate discussion of the pros and cons of using either administrative or survey data to measure the latent variable health in a Danish context.

[23] Unfortunately, there are no diagnoses codes from these data sources.

come Register which contains data on all income sources, including earnings, pensions payouts, transfers etc. From this register we also include information on all public transfers and public pensions as well as disability insurance. Almost all data in this register is third-party reported by employers, government agencies etc., and what is more, tax evasion is low and the data are, therefore, of very high quality (see Kleven et al. (2011); Alstadsæter et al. (2019) among others).

In order to characterize occupations according to their task content, we use the ratio of communication and cognitive tasks relative to manual tasks in a job. The task content is from the O*NET database (US Bureau of Labor Statistics) merged to Danish register data using the International Standard Classification of Occupation. We measure the task content of occupations for those who were employed at the end of November each year.

As previously described, we define a neighborhood as a parish in our baseline specifications, and we will use both phrases interchangeably. For historical reasons, a parish revolves around a church and thus describes smaller neighborhood entities quite well. Moreover, more recently local lockdowns were done at the parish level to prevent the spread of covid-19. The individuals in our sample were assigned to 1,008 different parishes, which had, on average, 4,665 inhabitants during the period of the refugee dispersal policy. We study the importance of small local areas by varying the neighborhood level using a very fine level, considering households living in the same apartment building. A parish is a subset of a municipality, whereas an apartment building is a subset of a parish. During the period of the dispersal policy, refugees in our sample were distributed across 237 different municipalities and 8,369 different apartment buildings. Disregarding the refugees, the municipalities had an average of 23,754 inhabitants, whereas an apartment building only had 15 inhabitants, on average, during the period.[24]

For each year we characterize the geographical areas by the median level of household disposable income from the Income Register (deflated by the consumer price index to 2000 level) by dividing all neighborhoods into three equally sized income groups: Bottom-, middle- and top-income neighborhoods.[25] The bottom income group consists of neighborhoods below

[24]In Online Appendix Figure A.2 we include histograms showing the distribution of neighborhoods by size (measured as the number of residents, excluding refugees subject to the dispersal policy) for the different types of geographical areas.

[25]We measure household disposable income as the household disposable income per adult household member

the 33th percentile of median income. The middle income group consists of neighborhoods in the 34th to 66th income percentile, and the top income neighborhoods are those in 67th to 100th percentile. We calculate these groups for each year and assign all neighborhoods to one of the three groups, regardless of whether the DRC found housing for any refugee nor not. This approach implies that a neighborhood's income group may vary across refugee cohorts.

We regard median household disposable income as a simple summary measure of neighborhood quality, since neighborhood income is correlated with neighborhood characteristics, such as employment, health of its residents, poverty rates, neighborhood income inequality, and crime rates, as illustrated by Table 2 and Online Appendix Table A.1. In a similar spirit, studies of the *Moving to Opportunity* experiment have used neighborhood poverty rates as a summary measure of neighborhood quality, see for example Kling et al. (2007). Since income differences between neighborhoods are small, other characteristics correlated with income may play an important role in explaining the association between assignment neighborhood income group and health outcomes of the refugees.

The neighborhood income characteristics are supplemented with additional neighborhood variables, such as the number of general practitioners per capita in the municipality, the number of co-nationals, urban/rural parish, health care utilization and incidences of lifestyle related diseases and mental disorders among the non-refugee residents in the municipality. All these characteristics are defined in the same way as individual refugee characteristics, and they are measured one year prior to arrival of each refugee. Furthermore, we measure the number of local sports clubs and sports facilities in the neighborhood (parish) based on firms' industry codes reported in the Integrated Database for Labor Market Research. We refer to Table 2, Online Appendix Table A.2 and Online Appendix Table A.3 for the summary statistics of neighborhood characteristics.

II Identification

We argue that the design of the Spatial Dispersal Policy made the allocation of individuals random across housing options, conditional on the observables from the questionnaire. This

to account for differences in household size.

provides us with the variation used for identification. Previous studies have exploited the same natural experiment, arguing that the allocation of refugees was random across municipalities (Damm and Dustmann (2014)) and at the clustered hectare level (Damm (2014)). Our main definition of a neighborhood, namely a parish, lies somewhere in between these two in terms of the geographical area it spans. In our analysis we also consider smaller geographical units, namely apartment buildings.

For our identification strategy to be valid, we must rule out selection of individuals across neighborhoods. We expect selection of individuals to be based on the questionnaire observables across neighborhood types, because the DRC allocated individuals based on these observables. But, once we take this selection into account, we assume that there was no selection into top-, middle- or bottom-income neighborhoods based on other criteria, such as individuals' health or educational attainment at arrival, which were not included in the questionnaire: i.e., that the income level of the allocated neighborhood was independent of the refugee's individual characteristics not observed by the DRC. We do not assume that the number of individuals allocated to a certain parish or apartment building was random, since the supply of affordable housing likely varied across neighborhood income types.

This means that we assume that two individuals who were of similar age, gender, ethnicity and family size were equally likely to find housing in a low-, middle- or top- income parish, independent of any other potential differences between them. We make a completely parallel assumption for selection into apartment buildings. We argue that these assumptions are valid because individuals were assigned to permanent housing based solely on the questionnaire.

Three concerns that could invalidate the design arise in this context: *i)* the DRC selectively allocated certain types of individuals to certain types of neighborhoods, *ii)* neighborhoods tried to select refugees through lobbying for/against specific individuals, *iii)* individuals self-selected into neighborhoods. Below, we address each of these concerns carefully. We will address these concerns with a parish in mind as this is the neighborhood level we use throughout most of our specifications. However, a much similar line of reasoning applies to apartment buildings. In Section II.A we present empirical tests to further address these concerns.

The scope for the DRC to place individuals in a selective manner was very limited since

the housing officer already searched for housing based on information from the questionnaire before the person moved into the municipality. Furthermore, the contemporaneous shortage of housing meant that whenever the DRC found a housing opportunity, there was always a queue of individuals with similar observables waiting for the same type of housing. Therefore, the housing option was simply offered to the next person in line. Thus, it seems unlikely that the DRC systematically placed specific types of individuals in certain types of neighborhoods.[26]

A second concern is that neighborhoods, e.g., through lobbying, tried to affect which types of refugees were allocated to that area. This is a potential issue at all neighborhood levels. At the municipality level the scope for selection was limited due to the short time frame (approximately ten days) from the time asylum was granted until resettlement took place in the municipality. Once allocated to a municipality, the different parishes could perhaps lobby for/against certain refugees. However, contrary to the municipality, the parishes or residents of apartment buildings did not have a formal administrative unit to organize such lobbying, therefore, it seems unlikely that it took place.

Finally, one could worry that the individuals somehow managed to self-select into specific types of neighborhoods. We do not directly observe the actual housing offers made to the refugees but only their first address. It is therefore crucial for our identification strategy that the acceptance rate of housing offers was high. In the previously mentioned interview with the former housing officer, she could not recall that refugees declined a housing offer. The explanation for this is threefold. First, the person only received one housing offer, and if the individual declined that offer, he/she had to move out of the temporary accommodation. This means that there was no bargaining over housing offers and that the cost of declining the offer was high. Second, following the acceptance of a housing offer, the refugee was free to move whenever he/she wanted to. Finally, the difficulty of finding affordable housing was probably even greater for refugees themselves, since they would mostly be without network connections and lack knowledge of the Danish housing market in general. Damm (2009) shows that the take up rate was above 90 percent, which is remarkably high compared to the *Moving to Opportunity*

[26]In an interview, the former DRC head of housing stated that she found it very unlikely that housing officers would have been able to selectively allocate individuals across neighborhoods due to the constant lack of affordable yet large enough housing options in the housing market (interview with Bente Bondebjerg on October 22, 2019).

experiment in which the acceptance rate was between 48 and 62 percent (Katz et al. (2001)).

A Validity Tests

To further support our identifying assumptions, we run a set of balancing tests of neighborhood characteristics on several individual characteristics that were not observed by the DRC housing officer at the time of assignment, but are available to us in the administrative data. At the time of allocation the DRC did not know the educational level and health status of the refugees, which, therefore, should not correlate with any characteristics of the neighborhood they were assigned to. Thus, to test whether the individuals were distributed randomly across neighborhoods, we regress several neighborhood characteristics on the characteristics of the individual refugee known and unknown to the DRC at the time of allocation. We run the following linear regressions:

$$
\begin{aligned}
y_{n,t-1} = {} & \alpha + \beta_1 unknown\_educ_{it} + \beta_2 basic\_educ_{it} + \beta_3 academic\_educ_{it} \\
& + \beta_4 circulatory\_disease_{it} + \beta_5 nutritional\_disease_{it} + \beta_6 neurotic\_disorder_{it} \\
& + X_{it}\gamma + T_t + \varepsilon_{it}.
\end{aligned} \tag{1}
$$

The neighborhood characteristics, $y_{n,t-1}$, are indicator variables for the poorest, middle or richest third of neighborhoods, the share of residents suffering from a lifestyle related disease, the number of GPs per capita, the population share, the employment rate among all residents, and the employment rate among immigrants. X_{it} summarizes the individual characteristics known from the questionnaire: age, country of origin, gender, marital status and family size at immigration, and T_t are year of arrival fixed effects. We use vocational education as the reference group for the education dummies.

Table 3 presents the results from these balancing tests. They show that refugees' educational attainments acquired prior to immigration and health at immigration have no significant prediction power of the neighborhood income level, employment rates, population size, neighbors' health conditions in the neighborhood or the number of GPs per capita in the initial placement municipality.[27] All, but one, of the estimated coefficients are not statistically different from

[27] Appendix Table A.4 shows that these tests also hold if we condition on municipality fixed effects. Note that

zero at conventional significance levels, and an F-test of joint insignificance of the education and health variables cannot reject that they are jointly equal to zero, see Table 3. Furthermore, similar regression tests across apartment buildings also suggest that there is no selection on initial education and health status to neighborhoods (Online Appendix Table A.5).[28]

We follow previous studies on the Danish Refugee Dispersal Policy (see for example, Damm and Dustmann (2014); Foged and Peri (2016); Dustmann et al. (2018, 2023)) by defining the assignment neighborhood based on the refugees' address observed on January 1st in the year after immigration to ensure that we capture their permanent resettlement neighborhood instead of temporary refugee accommodation. We therefore supplement our balancing tests with balancing tests of refugees' mobility within Denmark in the year of immigration. Online Appendix Table A.6 shows that mobility within the year of immigration is limited and mobility rates are balanced across neighborhood income groups.

Based on the balancing tests and the arguments posed in Section II, we argue that the initial neighborhood placement was quasi-random and that we can rule out selection across neighborhoods. The balancing tests underline the importance of conditioning on observables available from the questionnaire. The tests show that having more adults in the household (e.g., spouses arriving together and adult children) and having small children increases the probability of placement in a high-income neighborhood.[29] This could reflect differences in space needs across family compositions and availability of affordable housing units of the required size and characteristics across neighborhoods.

III Empirical Model

The main question posed in this paper is how living in a low-income neighborhood impacts health outcomes. We can use the natural experiment described in Section I.A for identification of causal neighborhood effects in a reduced form approach. We estimate the health effects of

the conditions of neighbors' health in the placement parish is measured as the share of residents diagnosed with a lifestyle related disease in the year of a refugee's arrival (yearly incidences).

[28]Note that two of the 144 coefficients tested are significant at the 5 percent level for the association between a neighborhood characteristic and refugees' initial education or health in the four tables with balancing tests. This may simply arise by chance, because we are testing multiple hypotheses.

[29]Note that individuals arriving alone can be registered as married, so this measure does not necessarily reflect family size.

assignment to a neighborhood of a certain type using Ordinary Least Squares. Specifically, we estimate the impact on an individual's health outcome $y_{i,t+r}$:

$$y_{i,t+r} = \alpha + \sum_{k=2}^{3} \beta_k \cdot \mathbb{1}[incomegroup_{n,t-1} = k] + \boldsymbol{X_{it}\gamma} + \boldsymbol{T_t} + \boldsymbol{A_m} + \boldsymbol{M_{m,t-1}\kappa} + \eta C_{i,t-1} + \varepsilon_{i,t+r}. \tag{2}$$

In model (2), $y_{i,t+r}$ denotes the health outcome of individual i, r years after arrival year t placed in neighborhood n. $incomegroup_{n,t-1}$ denotes the income group of the assignment neighborhood one year prior to arrival $t-1$. We control for the information available from the questionnaire to the DRC: age, country of origin, gender, marital status and family size at immigration summarized in $X_{i,t}$. We also include year of arrival fixed effects, T_t. Furthermore, we condition on municipality fixed effects, A_m, capturing local conditions at the larger geographical area.

Finally, we condition on the municipal employment rate (log-transformed), the number of GPs per inhabitant in the municipality (log-transformed), the population share in the municipality (all summarized in $M_{m,t-1}$), as well as the share of co-nationals in the assigned municipality ($C_{i,t-1}$) to account for local labor market opportunities, health care access, and the size of the potential network at the larger geographical level. Additional area characteristics are included as controls in Section IV.B.

In the baseline specification we do not include controls at the neighborhood (parish) level since we regard neighborhood income as a summary measure of neighborhood quality, similar to the *Moving to Opportunity* literature (see for example Kling et al. (2007); Ludwig et al. (2012) where the neighborhood poverty rate is used as a marker for the collection of correlated characteristics).[30]

It is important to note that the inclusion of municipality fixed effects means that all municipality level controls are identified from within-municipality variation over time. This warrants caution when interpreting the coefficients on these controls, as the remaining variation may

[30]This is different relative to other related studies that isolate the partial effect of a neighborhood characteristic by conditioning on additional neighborhood covariates, see for example Damm (2009); Damm and Dustmann (2014); Dustmann et al. (2023).

be limited and potentially noisy. For the same reason, we do not include parish fixed effects in our baseline specification. Including them would restrict identification of the neighborhood income effect to the subset of parishes that changed income group classification between 1986 and 1998.

The coefficients β_k denote the increased risk of diagnosis y if assigned to a middle- or top-income neighborhood relative to being assigned to the poorest neighborhoods. Thus, a negative estimate of β_2 and β_3 means that the risk of being diagnosed with y is lower in a top- and middle-income neighborhood than in a low-income neighborhood. The parameters identify the causal impact of being assigned to a certain type of neighborhood if the allocation of individual i to neighborhood n is random, conditional on the set of included individual characteristics and fixed effects. As we argue in Section II, this assumption of independence is satisfied, since the Spatial Dispersal Policy allows us to rule out selection of individuals into specific neighborhoods if we condition on observables from the questionnaire guiding the allocation.

IV Main Results

In this section we present our main findings on neighborhood effects on health, including evidence showing that these effects differ across gender and age.

A Average Effects

Allocation to the poorest third of neighborhoods increases the risk of developing a lifestyle related disease 2 to 15 years after immigration by 1.9 percentage points relative to allocation to the richest third of neighborhoods, see Panel a of Table 4. This amounts to a 10.6 percent increase in risk relative to the sample mean. The effect is driven by an increase in the risk of developing hypertensive diseases. Hypertensive diseases is a subgroup of circulatory diseases, which are some of the most common lifestyle related diseases. We do not observe any significant differences in average mental health outcomes across neighborhood income types. This differs from the *Moving to Opportunity* studies (Kling et al. (2007); Ludwig et al. (2012)). One difference between our study and earlier work, is that the *Moving to Opportunity* studies are

based on screenings of psychological distress in the past month and lifetime depression and anxiety, while our study is based on psychiatric diagnoses from hospitals. Therefore, our study likely captures the most severe cases.

Online Appendix Figure A.1 shows that the effect on lifestyle related diseases emerges slowly, which is consistent with lifestyle related diseases gradually developing over time as a result of health behaviors. Furthermore, the individuals are relatively young at arrival (31 years on average) and the risk of developing lifestyle related diseases generally increases with age. Most of the effects on health arise 8 to 15 years after immigration, which is why we focus on this time horizon in Panel b of Table 4.[31] This shows that the risk of developing a lifestyle related disease increases by 1.9 and 1.8 percentage points following allocation to the poorest third of neighborhoods relative to a middle- or top-income neighborhood, respectively.

Table 4 shows that there are no significant differences in the number of visits to GPs and psychologists between refugees placed in the different neighborhoods. If anything, the parameter estimates suggest that refugees in the poorest neighborhoods visit their GP more often, which may be a further indication of worse health outcomes for this group.

It is natural to ask whether the increased risk of suffering from a lifestyle related disease in low-income neighborhoods translates into higher mortality rates. We find that individuals placed in low-income neighborhoods have a higher mortality rate than those placed in top-income neighborhoods, but the difference is only statistically significant at the 10 percent level for men, see the last column of Table 6.

B Robustness of Main Results

Our findings in Table 4 are robust to the choices made in the baseline specification and we present these tests in Online Appendix Table A.7. We find similar results using average income (column (1)) instead of median neighborhood income or based on the most common neighborhood income group over three years prior to arrival (column (2)). Furthermore, we show that the effects are not an artifact of the linear probability model; a probit regression yields the same qualitative effect column (3)), and we show that the results are similar to the baseline

[31] This resembles the time horizon in Ludwig et al. (2011) who study health outcomes 10 to 15 years after assignment to a low-poverty neighborhood.

specification if we consider only primary diagnoses as outcomes (column (4)).

As a placebo test, we study some health outcomes that should not be affected by neighborhood income, namely congenital disorders. Since these disorders may already be known at the time of arrival, we also include schizophrenia as an outcome, given its strong genetic component with typically a later onset (Ripke et al., 2022). These tests reveal precise null-effects, confirming that the significant impact on lifestyle related diseases does not simply seem to arise by chance. Furthermore, the placebo tests suggest that the observed neighborhood health effects do not arise due to differences in measurement of health outcomes across neighborhoods. These robustness checks and placebo tests can be found in Online Appendix Table A.7.

Moreover, we find that there are no significant differences in outmigration rates from Denmark across neighborhoods within the first 15 years, and our main conclusions remain the same if we study a balanced panel of individuals who do not die or leave the country during the study period.[32] In addition, refugees were free to move within Denmark after assignment, and subsequent mobility may affect the interpretation of the estimated effects if moving is selective and correlated with neighborhood disadvantage at assignment.[33] Appendix Table A.8 illustrates that there is no difference in relocation rates within the first 15 years after immigration between refugees assigned to the richest third of neighborhoods versus those assigned to the poorest third of neighborhoods (column (1)). Refugees assigned to the poorest third of neighborhoods were more likely to move out of their initial neighborhood compared with refugees allocated to the middle third of neighborhoods (column (1)). However, there are no systematic differences in the types of neighborhoods that they moved to dependent on initial assignment neighborhood income group (columns (2)-(4)), and the refugees placed in the poorest neighborhoods accumulated significantly more exposure to poorer neighborhoods than refugees placed elsewhere (column (5)).

In our baseline specification we compare parishes within the same municipality and control for a number of municipality characteristics related to the size of the potential network, local labor market conditions as well as health care access in the municipality surrounding

[32] These results are available upon request.

[33] Subsequent mobility is well-documented in earlier studies by Dustmann et al. (2023); Damm (2014) among others.

the neighborhood. Thus, our baseline results should not be driven by such differences at the broader geographical area. In fact, excluding all area level controls does not affect the main results (Table 5, column (1)). For reference, we also report the unadjusted (raw) correlation in column (2), which reflects both the causal effect and any selection by the assignment officer. The raw correlations are very similar to the conditional estimates.

We next consider alternative ways of measuring municipality-level characteristics. In column (3) of Table 5 we add an additional control for health status in the municipality by controlling for the log share with lifestyle related diseases in the municipality, and in column (4) we include the log health expenditure in the municipality. This does not affect the estimated effects in the baseline specification.

In the baseline specification we do not condition on covariates measured at the neighborhood (parish) level, since median household disposable income in the neighborhood serves as a proxy for neighborhood disadvantage. As illustrated by Table 2 and Online Appendix Table A.1, neighborhoods with low income are generally characterized by a number of factors associated with neighborhood disadvantage, such as lower employment rates and higher immigrant shares. The main results encompass the impact from these characteristics on individual health outcomes, and the results are robust to including additional controls for the quality and the size of the network in the neighborhood, measured by the share employed and the number of immigrants in the neighborhood as well as the log average household income among immigrants (columns (5)-(8)). As an additional measure of a neighborhood's quality related to healthy behavior, we include the number of sports facilities in the neighborhood in column (9). Furthermore, the results are robust to controlling for urbanity of the neighborhood (column (10)).

In column (11), we control for neighborhood income inequality, measured as the Gini coefficient in the placement neighborhood. Consistent with Grönqvist et al. (2012), who find no effect of within neighborhood income inequality on health outcomes, our results are robust to including this control. In column (12) we add an additional control for low income in the neighborhood, namely the poverty rate. This affects the parameter estimates and reduces precision, since the two income measures are strongly correlated, see Online Appendix Table A.1.

Finally, we include all the additional control variables measured at both the municipality level and at the neighborhood (parish) level simultaneously in column (13). This does not reduce our estimates and their precision compared to the baseline as much. In summary, our results are robust to including control variables at the neighborhood level, and we stress that the results should not be interpreted as the partial impact of neighborhood income, but rather as reflecting adverse health effects of neighborhood disadvantage correlated with neighborhood income.

C Heterogeneous Effects

The evidence on neighborhood effects on adults' health outcomes from the *Moving to Opportunity* experiment is based on a predominantly female sample (Ludwig et al. (2011) only study women, and in Kling et al. (2007); Ludwig et al. (2012) 98 percent of the adult sample are females). It is, therefore, informative to study whether there are heterogeneous effects by gender on health outcomes.

In our study, we find that the impact on health of placement neighborhood income type varies significantly by gender. Table 6 shows that females experience a larger increase in the risk of developing lifestyle related diseases 8 to 15 years after immigration – in particular nutritional disorders – if they are placed in the poorest third of neighborhoods as opposed to placement in a middle- or top-income neighborhood compared with males placed in similar neighborhoods. In other words, female health is more adversely affected by living in the poorest neighborhoods. Women placed in the poorest neighborhoods have a 3 percentage points higher risk of developing a lifestyle relate disease and a 2.6 percentage points higher risk of developing a nutritional disease than men placed in similar neighborhoods 8-15 years after immigration, relative to placement in the richest third of neighborhoods. In our sample, a larger share of women than men are diagnosed with nutritional or lifestyle related diseases, and our estimations indicate that the larger neighborhood effects for females might contribute to this difference. One potential explanation for the differential impact by gender could be that women are more affected by their immediate local environment because they have lower rates of labor force participation and spend more time at home compared with men.

Previous literature shows that economic assimilation of refugees differs substantially across

origin countries. In particular for the case of Denmark, refugees from Asia have higher employment rates and earnings than refugees originating from Middle Eastern and African countries (Foged et al., 2024). We therefore, also examine whether the estimated neighborhood effects on health differ by region of origin.[34] Our estimates reveal small and statistically insignificant differences in neighborhood effects across these groups, while there are some differences in contacts with GPs and psychologists as well as mortality, see Table 7. Taken together, the evidence indicates that the estimated neighborhood effects on health are broadly similar across origin groups, despite substantial differences in their labor market assimilation patterns.

As the risk of developing a lifestyle related disease increases with age (see, e.g., Prince et al. (2015)), the impact of neighborhood group may be more pronounced among older refugees in our sample. On the other hand, younger individuals may be more influenced by their assigned neighborhood – for example, if their health behaviors are more susceptible to peer effects or the amenities available in the local environment. To explore whether the impact on health of the type of neighborhood depends on age at immigration, we therefore explore how the impact of neighborhoods on health varies below and above the average age at immigration (31 years old). The results are reported in Table 8. Our findings indicate that the risk of developing a lifestyle related disease is smaller for younger refugees assigned to a low-income neighborhood compared to older refugees assigned to a similar neighborhood. For mental health outcomes, we find no significant heterogeneity by age at immigration. However, these patterns do not necessarily reflect permanent differences in responsiveness to neighborhood type, as the younger refugees may simply not yet have reached the age at which lifestyle related diseases are typically diagnosed.

V Mechanisms Behind the Neighborhood Effects

Next, we investigate some of the potential explanations behind the documented neighborhood income impact on health.[35] First, we explore how allocation to a given type of neighborhood affects different individual outcomes that in turn might affect the individual's health. Second, we

[34] We group refugees from Sri Lanka and Vietnam into one region of origin and all others into a second group.
[35] See Sanbonmatsu et al. (2011) for an overview of potential channels.

examine the importance of the very local environment and immediate neighbors by varying the size of the neighborhood. We conclude the section by discussing other potential mechanisms that we are not able to test directly.

A Individual Outcomes

We consider how initial neighborhood allocation affects the individuals' performance in the labor market, public transfer income and their educational attainments after immigration. Differential changes in these outcomes across neighborhoods could potentially contribute to the differences in health outcomes. For example, improved labor market opportunities for individuals in high-income neighborhoods could potentially affect health by increasing life satisfaction and/or by increasing the individuals' income levels.

Labor market and public transfers. Interestingly, persons allocated to the poorest third of neighborhoods by the Spatial Dispersal Policy do not experience different labor market outcomes than those allocated to top- or middle-income neighborhoods, see Table 9. This implies that the differences in health outcomes are not driven by differential labor market outcomes as a result of initial placement. We estimate precise zero effects on different measures of employment and income: After 15 years in Denmark the cumulative difference in the number of years with any employment is 0.03 to 0.05 years across the different types of neighborhoods, and it is not statistically significant.[36] Similarly for earnings, we observe differences of less than a typical monthly salary in the cumulative income over 15 years across neighborhoods.

Our findings are consistent with the findings in Damm (2014) who documents that living in socially deprived neighborhoods does not impact the labor market outcomes of refugee men. It is also in line with evidence from the *Moving to Opportunity* experiment. See for example Katz et al. (2001), Kling et al. (2007), Sanbonmatsu et al. (2011) or Ludwig et al. (2012) who find no effects on employment, earnings or welfare receipt probability. Finally, we examine differences in total public transfers received as well as public pensions and disability insurance income, since income differences could also arise from different uptake rates of e.g., disability

[36] In general, the group of refugees have very weak labor market attachment. The average number of years with any employment during the period considered is 3.23 years.

insurance across assignment neighborhoods.[37] However, we find small and statistically insignificant differences across neighborhood income groups. Thus, we can rule out any income effects of being placed in a bottom, medium, or top income neighborhood.[38]

Education. We document significant differences in educational outcomes across placement neighborhoods. Panel a of Table 10 shows that being placed in a top- or middle-income neighborhood increases the probability of completing an education in Denmark by 2.1 and 1.4 percentage points, respectively, compared with those placed in the poorest third of neighborhoods.[39] The table also shows that these results are primarily driven by completion of vocational education. The combination of Panels a and b shows that the differences in educational attainment across neighborhoods occur within the first eight years after arrival, which is before the observed differences in health outcomes across neighborhoods arise. These observed differences in education could therefore influence later health outcomes.

It cannot directly be inferred from Table 10 whether the increased educational level decreases the risk of developing lifestyle related diseases. To guide interpretation, we draw on the conceptual framework in Cutler and Lleras-Muney (2006), who outline several mechanisms through which education may influence health. Some of these can be directly assessed in our setting, while others are unobservable.

First, education may improve *labor market outcomes*, leading to better working conditions, higher earnings, or access to health insurance. This mechanism is testable in our data. As shown in Table 9, the increased educational level among individuals placed in richer neighborhoods does not translate into higher employment rates or earnings. We also find that occupations do not differ systematically in task complexity across neighborhoods, see Table 9.[40] This suggests that it is unlikely that better working conditions explain our findings. Moreover, due to the universal health care in Denmark, better health care insurance is less relevant in this context.

Second, education and health may be linked trough education's impact on *information and*

[37] Such differences could also suggest that that neighborhood network effects (see e.g., Dahl et al. (2014)) in receiving such transfers could be driving our results.

[38] The labor market results are robust to studying a sample aged 18-49 at arrival, who do not reach retirement age in the first 15 years.

[39] The results are very similar if we study enrollment instead of completion.

[40] We define occupations by their manual, cognitive and communicative task content. Our results show that there are no significant differences in each of these task contents or a combined index of the three.

cognitive skills, since education may improve individuals' ability to understand health information and adhere to medical advice. We cannot test these mechanisms directly, but we explore if refugees obtained health information directly through their obtained education. Table 10 shows no increase in the probability of completing a health-specific education across neighborhoods, suggesting that the direct health knowledge is not differentially affected across neighborhood income groups.

Third, education may itself alter individuals' *time preferences* or their *valuation of the future*, potentially making them more forward-looking and more inclined to invest in their long-term health. Fourth, education might affect *social standing or rank*, which itself has been linked to health outcomes. Similarly, education could improve *social networks*, which may offer emotional, informational, and financial support, and shape health behaviors. Finally, education could improve general *well-being* or self-esteem, which may influence health behaviors over the life course. These latter mechanisms are not directly testable in our data but may be at play for the population we study given the timing of educational completion.

To further investigate the relationship between our findings on education and health – both of which may be influenced by age at immigration – we analyze heterogeneity in education outcomes by age at immigration. Online Appendix Table A.9 shows that the positive effect of higher-income neighborhood placement on educational attainment is concentrated among refugees who arrived before age 31, with no significant effect for older arrivals. Combined with the age heterogeneity of the somatic health results discussed above, this pattern is consistent with life-cycle timing: older refugees may have aged out of schooling, while younger refugees may not yet have reached the age at which lifestyle related diseases are typically diagnosed. Therefore, these results do not allow us to conclude whether the education obtained by younger refugees will affect their future health outcomes. However, differences in education obtained in Denmark do not appear to be the main driver of the health differences we observe in this study.[41]

[41]It is possible that the increased educational level did not causally affect the refugees' health. Previous research on education reforms in Sweden (Meghir et al. (2018)) and twin studies in Denmark (Behrman et al. (2011)) does not find a causal impact of education on health. However, these studies study the health impact of education in a very different setting compared to ours.

B Varying the Neighborhood Size

Taking one step further, we explore the mechanisms behind the results by varying the neighborhood size. Specifically, if the neighborhood effects on health outcomes are driven by interaction with peer groups, we would expect the characteristics of smaller neighborhood units to be more predictive of health outcomes than larger geographic areas, as the measurement of peer groups becomes more accurate. We therefore include an additional measure of neighborhood income at the apartment building level – more specifically, a particular stairway of an apartment complex. Measuring neighborhood median income at the apartment building level rather than at the parish or municipality level should bring us closer to the income levels of peers as the population becomes smaller and the probability of interaction is increased.

Therefore, we estimate the increased probability of developing lifestyle related diseases within 15 years upon assignment to the poorest third of municipalities, parishes and apartment buildings. To test if the local environment or close peers are important for health outcomes, we estimate a model including all three indicators at the same time. In column (1) of Table 11 we compare the impact of being assigned to the poorest third of apartment buildings, holding constant the impact on health of being assigned to the poorest third of parishes and the poorest third of municipalities. That is, we examine if being assigned to the poorest third of apartment buildings has health implications over and above the health implications of assignment to the poorest third of municipalities and parishes. In this specification we do not include municipality fixed effects because including both municipality fixed effects and the indicator for assignment to a low-income municipality would mean that the effect of the latter is identified only from the small number of municipalities that changed income group between 1986 and 1998. This exercise shows that the income group of the assigned apartment building is more strongly associated with the risk of developing a lifestyle related disease than the income group of the parish, which in turn is more predictive than that of the municipality. Notably, the coefficient for being placed in a bottom income apartment building is statistically significant at the 5 percent level, whereas the corresponding coefficients for bottom income parishes and municipalities are not significant at the 10 percent level.

When we let apartment buildings define neighborhoods, we are able to compare the health of individuals allocated to the poorest third of apartment buildings to individuals in richer apartment buildings within the same parish. Therefore, we include parish fixed effects to control for time-invariant parish characteristics in column (2) of Table 11. These time-invariant characteristics may capture the access to outdoor recreational areas, parks, pollution and permanent sports facilities, such as public swimming pools and soccer fields, within the parish. It is less likely that the fixed effects capture the presence of local sports clubs and fast food stores, because these places open and close quite frequently over time.[42] The inclusion of parish fixed effects does not affect the magnitude of the estimated effect on health much – possibly because some of these time-invariant characteristics are captured by the municipality fixed effects in the baseline specification.

Similar to our main specification, where neighborhoods are defined at the parish level, we investigate the robustness of the results to different area level control variables. In Table 11 we show that at the apartment building level, the estimated effects on health are not sensitive to different municipality and parish level characteristics, such as the share of neighbors with a lifestyle related disease, the share of employed neighbors, the number of sports facilities, the urbanity of the neighborhood, neighborhood income inequality, or the poverty rate.

Since apartment buildings can be small units with relatively few residents (excluding the refugees assigned through the dispersal policy), we examine whether our findings are driven by apartment buildings with very few non-refugee residents. First, we restrict the sample to refugees assigned to apartment buildings with at least ten other residents. Second, to study whether the results are driven by within-family variation in health we re-estimate the model using only the oldest household member in each refugee family.[43] In both cases the results remain robust, as shown in columns (15) and (16), respectively, in Table 11.

Importantly, we do not include apartment building fixed effects in any of the models reported in Table 11. The estimated effects therefore reflect comparisons between refugees assigned to different types of apartment buildings, or between refugees assigned to the same

[42] Our data show that there is considerable variation in the number of restaurants, shops and sports clubs within parishes over time.

[43] When all members are of the same age we use the male household member.

building if the apartment building changes income category over time.

In summary, Table 11 suggests that the characteristics of the very local neighborhood are important factors for determining health outcomes. This may be due to a transmission of health behaviors from the immediate neighbors and the exposure to the characteristics of a very small geographical area, such as local recreational facilities and food store options.

C Remaining Explanations

What are the remaining differences between the poorest and richest neighborhoods once we sum up the results from Section V? Some of the effects may be due to different educational outcomes for refugees. Our findings suggest that both individual income effects and municipality level differences across neighborhoods as well as the presence of ethnic networks are not the main explanations. This may reflect that what matters most for the health outcomes we study are the characteristics of the very local neighborhood, such as the characteristics and behaviors of the immediate neighbors, along with the supply of fast food/grocery stores and immediate recreational areas. Using the income of the immediate neighbors as a proxy for the very local neighborhood quality, our results from Section IV.B indicate that such characteristics of the very local environment are important.

Given our results, especially amenities related to diet and exercise or behavior of immediate neighbors could potentially be very important, since both diet and exercise matter for the risk of developing lifestyle related diseases. Neighborhood characteristics such as traffic noise or air pollution may be less important determinants of the health outcomes studied in our setting. Our findings show that there are significant place effects on health when comparing apartment buildings within the same neighborhood where pollution levels are possibly similar. However, since we do not have data on pollution levels at the small local level, we cannot test how this influences health outcomes.

Furthermore, we cannot assess the quality of primary care directly. Although, we find that the number of visits to GPs are similar across neighborhoods, there may be differences in GP quality which could affect health outcomes of refugees. The inclusion of parish fixed effects in Table 11 (column (2)) captures average GP quality in the neighborhoods when comparing

refugees in different apartment buildings, and the corresponding results suggest that health differences are not entirely explained by differences in neighborhood level primary care quality.

Finally, since we do not control for the quality of the apartments that the DRC assigned the individuals to, it is possible that we capture apartment effects on health as opposed to neighborhood effects, i.e., that it is in fact the low quality apartments in the poorest neighborhoods that we measure the effect of. We do not observe the quality of the assigned apartments, but since we can rule out individual income effects, we can rule out large differences in apartment rents, which, in general, we would expect to correlate with quality. The small income differences between refugees imply that the apartment quality could only be reflected in prices to a limited extent and still be within the refugees' budget. On top of that, we only compare health outcomes of refugees assigned to different neighborhoods within the same municipality which in itself limits the differences in apartment quality across neighborhoods within the refugees' budget.

VI Concluding Remarks

We study a Spatial Dispersal Policy in force from 1986 to 1998 that quasi-randomly resettled individuals in different neighborhoods. This natural experiment allows us to rule out selection of individuals into neighborhoods and provides causal estimates of the impacts of neighborhoods on residents' health. Specifically, we characterize neighborhoods by their median income levels to study how the risk of developing a number of lifestyle related diseases depends on the quality of the neighborhood in which the person was resettled.

We document that there are long term negative health consequences of living in a low-income neighborhood. Individuals who were resettled in the poorest third of neighborhoods have a 12 percent higher risk of suffering from a lifestyle related disease within the first 8-15 years upon arrival compared to those who were resettled in richer neighborhoods. This is a substantial impact in comparison with the economically small and insignificant impacts of neighborhoods on adult economic self-sufficiency found in earlier studies. However, it seems likely that neighborhood effects on health could be even larger in countries without universal

health care and with larger income differences between neighborhoods than the Danish neighborhoods.

Our study contributes to the understanding of neighborhood effects on health by examining a number of potential mechanisms. While the neighborhood impact on health cannot be explained by differences in individuals' employment or earnings across neighborhoods, we document that individuals assigned to the richest neighborhoods are more likely to obtain a vocational non-health related education post-immigration. Our findings suggest that the impacts on health outcomes are not caused by differences in health care access, employment opportunities, or the size of the ethnic network. We provide new evidence suggesting that neighborhood effects on health can operate through interaction with neighbors and very local area characteristics. We find that the income level of immediate neighbors living in the same apartment building is more important for health outcomes than the income levels of those living in the same parish or municipality.

Thus, studying how immediate neighbors' exercise, diet and smoking habits and access to local recreational areas affect residents' behavior could provide a better understanding of the neighborhood effects on health documented in this paper. Such an understanding can serve as a guideline for policy interventions aimed at improving health conditions in the poorest neighborhoods.

Furthermore, our results have implications for the design of spatial dispersal policies for refugees. Policymakers should take into account the long-term negative health consequences when resettling refugees in the most disadvantaged neighborhoods. Especially, considering that this is a group already at significant disadvantage at arrival.

Table 1: Summary Statistics for the Population of Refugees

	All Mean	Bottom Mean	Middle Mean	Top Mean
Characteristics at Immigration				
Age	30.69	29.97	31.11	30.69
Female	0.38	0.37	0.38	0.37
Married	0.59	0.62	0.60	0.57
Number of Family Members	2.23	2.05	2.26	2.28
Number of Children	0.75	0.64	0.76	0.79
Origin Country				
Iraq	0.21	0.25	0.20	0.20
Lebanon	0.18	0.12	0.16	0.21
Somalia	0.19	0.27	0.19	0.16
Iran	0.16	0.10	0.15	0.19
Sri Lanka	0.12	0.13	0.13	0.11
Vietnam	0.09	0.06	0.12	0.08
Afghanistan	0.04	0.04	0.03	0.04
Ethiopia	0.02	0.02	0.02	0.02
Education				
Basic Education	0.48	0.47	0.49	0.48
Vocational Education	0.24	0.25	0.24	0.24
Higher Education	0.27	0.28	0.27	0.28
Education Unknown	0.37	0.38	0.38	0.37
N	21,965	3,887	6,838	11,240

Notes: Summary statistics for the full sample of refugees and by parish income groups. The sample consists of refugees between 18-64 years of age who arrived to Denmark between 1986 to 1998 from Iraq, Lebanon, Somalia, Iran, Sri Lanka, Vietnam, Afghanistan, Ethiopia and Palestine. We do not include family-reunification arrivals. All refugee characteristics are measured at year of immigration. Column "All" presents the mean of characteristics among all refugees in our sample irrespective of parish income group. "Bottom" refers to characteristics among refugees assigned to the bottom third of parishes measured by median disposable income in a given year. Similarly, "Middle" and "Top" refer to characteristics among refugees assigned to the middle and top third of parishes measured by disposable income, respectively. The parish income groups are defined among all parishes, irrespective of any refugee assignment. We define income group of assignment parish one year prior to immigration by median disposable income among all inhabitants aged 18 or above. Data is from administrative registers provided by Statistics Denmark.

Table 2: Summary Statistics for Initial Placement (Parish)

	Bottom Mean	Middle Mean	Top Mean
Characteristics of Residents			
Age	46.52	46.96	45.60
Median Household Income	13,978.23	14,626.28	16,020.96
Employment Rate	0.63	0.68	0.74
Prevalence of Lifestyle Related Diseases	0.09	0.08	0.07
Inhabitants	4,059.41	4,501.83	5,372.10
Co-Nationals	17.31	12.65	9.02
Poverty Rate	0.10	0.07	0.05
Gini Coefficient	0.22	0.20	0.20
Crime Rate	0.02	0.01	0.01
Parish Type			
Urban Area (Near City)	0.58	0.50	0.71
Urban Area (Away from City)	0.04	0.20	0.15
Rural Area (Near City)	0.09	0.10	0.09
Rural Area (Away from City)	0.29	0.20	0.05
Characteristics of Municipality			
General Practioners per 1,000 Inhabitants	0.47	0.43	0.46
Incidences of Lifestyle Related Diseases per 1,000 Inhabitants	33.14	29.40	26.18
Health and Social Expenditure per Capita	4,028.28	4,107.94	4,036.23
N	646	1,374	2,645

Notes: Summary statistics for parishes in which refugees were resettled. "Bottom", "Middle" and "Top" refer to parish characteristics of parishes in the bottom, middle and top third of parishes measured by median parish disposable income in a given year. We calculate the median income of each parish including all inhabitants in each parish aged 18 or above and define the income groups among all parishes, irrespective of any refugee assignment. All parish characteristics are measured one year prior to immigration. Employment rate is the share of the population with any employment between the ages of 18-64. The crime rate is the share of persons convicted of a criminal offense (excl. traffic related offenses), age 15 or older. Prevalence of lifestyle related diseases is measured as all incidences over the previous 8 years and thus only defined for refugee cohorts arriving after 1993. Health and social expenditure per capita and median household income is measured in USD. Observations are parish-year. Data on "Health and Social Expenditure per Capita" stems from Statistikbanken, (REG1, REG1R and REG11). Parish types are defined by Ministeriet for By, Bolig og Landdistrikter (2013). All other data are from administrative registers provided by Statistics Denmark.

Table 3: Balancing Tests

	(1) Bottom Income Group	(2) Middle Income Group	(3) Top Income Group	(4) Lifestyle Related Disease	(5) GPs	(6) Population Share	(7) Employment Rate	(8) Employment Rate Immigrants
Unobserved at Time of Allocation								
Unknown Education	-0.010	0.006	0.004	0.000	-0.000	0.000	0.000	0.001
	(0.008)	(0.011)	(0.011)	(0.000)	(0.003)	(0.000)	(0.001)	(0.003)
Basic Education	-0.010	0.007	0.003	-0.000	-0.003	-0.000	0.000	-0.001
	(0.008)	(0.010)	(0.011)	(0.000)	(0.003)	(0.000)	(0.001)	(0.003)
Higher Education	0.001	0.001	-0.002	0.000	0.001	-0.000	0.001	0.005
	(0.009)	(0.011)	(0.012)	(0.000)	(0.003)	(0.000)	(0.002)	(0.003)
Circulatory Disease	-0.002	-0.027	0.029	0.000	0.014	0.000	-0.002	-0.007
	(0.024)	(0.030)	(0.032)	(0.000)	(0.008)	(0.000)	(0.004)	(0.009)
Nutritional Disease	-0.008	-0.025	0.033	0.001	0.005	-0.000**	-0.000	0.004
	(0.032)	(0.040)	(0.043)	(0.001)	(0.012)	(0.000)	(0.006)	(0.010)
Neurotic Disorder	-0.083	0.038	0.045	0.001	-0.016	-0.000	0.005	-0.007
	(0.051)	(0.074)	(0.080)	(0.001)	(0.021)	(0.000)	(0.010)	(0.019)
Observed at Time of Allocation								
Age 30-49 Years	-0.005	0.002	0.002	-0.000	-0.004	-0.000***	0.003**	0.002
	(0.006)	(0.008)	(0.008)	(0.000)	(0.002)	(0.000)	(0.001)	(0.002)
Age 50-64 Years	-0.022**	0.036**	-0.014	-0.000	0.002	0.000	-0.003	-0.004
	(0.010)	(0.014)	(0.015)	(0.000)	(0.004)	(0.000)	(0.002)	(0.003)
Female	-0.002	-0.004	0.006	0.000***	0.007***	0.000***	-0.001	0.002
	(0.005)	(0.006)	(0.006)	(0.000)	(0.002)	(0.000)	(0.001)	(0.002)
Number of Adults	-0.013	-0.012	0.025**	-0.000**	-0.014***	-0.000	0.004**	0.006**
	(0.011)	(0.010)	(0.012)	(0.000)	(0.003)	(0.000)	(0.001)	(0.002)
Number of Children 0-2 Years Old	-0.014	0.028**	-0.015	-0.000**	-0.010***	-0.000	-0.000	-0.003
	(0.010)	(0.012)	(0.013)	(0.000)	(0.004)	(0.000)	(0.002)	(0.004)
Number of Children 3-17 Years Old	-0.006	0.004	0.002	-0.000**	-0.004***	-0.000***	0.001	0.001
	(0.003)	(0.004)	(0.004)	(0.000)	(0.001)	(0.000)	(0.001)	(0.001)
Married	0.016**	0.004	-0.020**	0.000***	0.004	0.000***	-0.004***	-0.001
	(0.007)	(0.008)	(0.009)	(0.000)	(0.002)	(0.000)	(0.001)	(0.002)
Sample Mean	0.18	0.31	0.51	0.03	0.45	0.00	0.69	0.49
Year of Immigration FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country of Origin FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	No	No	No	No	No	No	No	No
N	21,965	21,965	21,965	21,965	21,965	21,965	21,965	21,965
F	1.01	0.38	0.38	1.05	1.25	1.87	0.12	1.14
Pr > F	0.41	0.89	0.89	0.39	0.28	0.08	0.99	0.34

Notes: Balancing tests for parishes using linear regressions. Robust standard errors in parentheses clustered at the household level. $^{**}p < 0.05, ^{***}p < 0.01$. F denotes the F-statistic for joint insignificance of the educational attainment dummies and pre-existing health conditions. Vocational education is the reference group for the education dummies. Each column represents a different balancing test testing whether refugees with certain characteristics (column farthest to the left) are more likely to be placed in parishes with specific characteristics (dependent variables). The dependent variables in (1)-(3) are dummies for assignment to a bottom third income parish (1), middle third income parish (2) or top third income parish (3). In column (4) the dependent variable is the incidence (as a share of inhabitants) of lifestyle related diseases. In column (5) the dependent variable is the number of GPs per capita in the municipality. In columns (6)-(8) the dependent variable is the population share, the employment rate or the employment rate among immigrants in the parish. The controls are individual characteristics observed by the DRC at time of assignment and characteristics that the DRC does not observe at time of assignment: initial education and health. We measure all individual characteristics at year of immigration. We measure all parish characteristics one year prior to immigration. The sample mean denotes the mean of the dependent variable.

Table 4: Main Results

	Lifestyle Related	Circulatory	Nutritional	Hypertension	Diabetes	Mental Disorder	Neurotic	Visits GP	Visits Psychologist	Died
(a) Within 15 years after immigration										
Middle	-0.018**	-0.017**	-0.010	-0.011**	-0.008	0.003	0.004	-0.638	-0.006	-0.008
	(0.008)	(0.007)	(0.007)	(0.005)	(0.006)	(0.008)	(0.006)	(3.552)	(0.418)	(0.006)
Top	-0.019**	-0.014*	-0.007	-0.006	-0.006	0.012	0.009	-1.433	-0.384	-0.008
	(0.009)	(0.008)	(0.007)	(0.005)	(0.006)	(0.009)	(0.007)	(3.998)	(0.427)	(0.007)
Sample Mean	0.18	0.11	0.09	0.04	0.06	0.13	0.08	152.65	3.65	0.06
(b) Within 8-15 years after immigration										
Middle	-0.019**	-0.012**	-0.012*	-0.010**	-0.007	-0.002	-0.002	-2.392	0.047	-0.005
	(0.008)	(0.006)	(0.006)	(0.004)	(0.006)	(0.007)	(0.005)	(2.845)	(0.301)	(0.004)
Top	-0.018**	-0.009	-0.008	-0.004	-0.005	0.004	0.001	-1.619	-0.245	-0.004
	(0.009)	(0.007)	(0.007)	(0.005)	(0.006)	(0.008)	(0.006)	(3.191)	(0.308)	(0.004)
Sample Mean	0.15	0.09	0.08	0.04	0.06	0.10	0.06	101.56	2.63	0.03
N	21,965	21,965	21,965	21,965	21,965	21,965	21,965	21,965	21,965	21,965
Municipality Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Robust standard errors clustered at parish $\times$ immigration year level. $^{*}p < 0.10,^{**}p < 0.05,^{***}p < 0.01$. Estimates from a linear probability model testing the impact of assignment parish income group on the probability of being diagnosed with each of the diseases or having died in the top panel. The estimates show the the increased risk if assigned to the middle third or top third income neighborhoods compared to a bottom third income neighborhood. In Panel (a) the dependent variable is an indicator for being diagnosed with the disease considered or the number of visits to GPs and psychologists or dying 2-15 years after immigration. In Panel (b) the dependent variable is a dummy for being diagnosed with the considered disease or the number of visits to GPs and psychologists or dying 8-15 years after immigration. We measure parish income groups one year prior to arrival based on median disposable income in each parish among all parishes in Denmark in a given year. We control for individual characteristics observed at time of assignment by including controls for gender, marital status, family size, and country of origin as well as age and year of arrival fixed effects. The municipality controls are the population share, the share of co-nationals, the logarithm of the employment rate and the logarithm of the number of GPs per inhabitants in the municipality of assignment. In addition, we condition on municipality of assignment fixed effects. The sample mean denotes the share of refugees diagnosed with the disease or dying or the number of visits in the different year intervals.

Table 5: Estimated Impact on Lifestyle Related Diseases

	Baseline	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
(a) Diagnosed within 15 years after immigration														
Middle	-0.018**	-0.017**	-0.016*	-0.018**	-0.019**	-0.023***	-0.019**	-0.020**	-0.018**	-0.018**	-0.015*	-0.016*	-0.013	-0.018*
	(0.008)	(0.008)	(0.009)	(0.008)	(0.008)	(0.009)	(0.009)	(0.009)	(0.009)	(0.008)	(0.008)	(0.009)	(0.009)	(0.010)
Top	-0.019**	-0.019***	-0.022***	-0.019**	-0.019**	-0.028***	-0.022**	-0.021**	-0.018*	-0.019**	-0.016*	-0.016*	-0.011	-0.021*
	(0.009)	(0.007)	(0.008)	(0.009)	(0.009)	(0.011)	(0.009)	(0.010)	(0.010)	(0.009)	(0.008)	(0.010)	(0.011)	(0.012)
Log Share with				0.007										0.003
Lifestyle Related Diseases				(0.036)										(0.036)
Log Health					0.107									0.093
Expenditure					(0.069)									(0.070)
Log Employment Rate						0.064*								0.102*
in Parish						(0.037)								(0.057)
Number of Refugees							-0.000**							-0.000*
							(0.000)							(0.000)
Number of Refugees							0.000*							0.000**
Squared							(0.000)							(0.000)
Immigrant Share								-0.046						0.444*
								(0.150)						(0.228)
Immigrant Share								-0.008						-1.146**
Squared								(0.371)						(0.501)
Log Average									-0.002					0.018
Immigrant Household Income									(0.021)					(0.025)
Number of Sports										-0.001				-0.001
Facilities										(0.000)				(0.000)
Neighborhood												0.134		
Inequality												(0.152)		
Log Share Below													0.190*	0.309**
Poverty Line													(0.114)	(0.154)
(b) Diagnosed 8-15 years after immigration														
Middle	-0.019**	-0.019***	-0.017**	-0.019**	-0.020**	-0.022**	-0.019**	-0.021**	-0.018**	-0.019**	-0.016**	-0.017**	-0.013	-0.015
	(0.008)	(0.007)	(0.008)	(0.008)	(0.008)	(0.009)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.009)	(0.010)
Top	-0.018**	-0.021***	-0.021***	-0.018**	-0.018**	-0.024**	-0.020**	-0.020**	-0.016*	-0.018**	-0.017**	-0.015*	-0.009	-0.013
	(0.009)	(0.007)	(0.008)	(0.009)	(0.009)	(0.010)	(0.009)	(0.009)	(0.009)	(0.009)	(0.008)	(0.009)	(0.010)	(0.012)
Log Share with				-0.021										-0.024
Lifestyle Related Diseases				(0.033)										(0.033)
Log Health					0.121*									0.098
Expenditure					(0.066)									(0.066)
Log Employment Rate						0.038								0.076
in Parish						(0.036)								(0.056)
Number of Refugees							-0.000							-0.000
							(0.000)							(0.000)
Number of Refugees							0.000							0.000*
Squared							(0.000)							(0.000)
Immigrant Share								0.096						0.461*
								(0.166)						(0.243)
Immigrant Share								-0.382						-1.412**
Squared								(0.490)						(0.658)
Log Average									-0.015					0.011
Immigrant Household Income									(0.021)					(0.024)
Number of Sports										-0.000				-0.000
Facilities										(0.000)				(0.000)
Neighborhood												0.140		-0.400
Inequality												(0.153)		(0.331)
Log Share Below													0.215*	0.626**
Poverty Line													(0.131)	(0.294)
N	21,965	21,965	21,965	21,965	21,963	21,965	21,965	21,965	21,923	21,965	21,965	21,965	21,965	21,921
Municipality Controls	Yes	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes
Parish Type FE	No	No	No	No	No	No	No	No	No	No	Yes	No	No	No

Notes: Robust standard errors in parentheses clustered at parish $\times$ immigration year level. $^{*}p < 0.10,^{**}p < 0.05,^{***}p < 0.01$. The table presents variations of model (2) with different sets of controls. In column (Baseline) we replicate the estimates from Table 4. In (1) we exclude all municipality controls. In (2) we exclude all municipality and individual refugee controls. In (3) we include the logarithm of the number of incidences of lifestyle related diseases in the assignment municipality. In (4) we include the logarithm of health and social expenditure per capita in the municipality. In column (5) we include the logarithm of the employment to population rate in the parish. In (6) we control for the number of inhabitants in the neighborhood originating from any of the refugee sending countries in our sample. In (7) we include the share of immigrants and the squared share of immigrants. In (8) we include the logarithm of average disposable household income among immigrants in the neighborhood. In (9) we include the number of sports facilities in the neighborhood. In (10) we replace municipality fixed effects with parish type fixed effects. The parish type fixed effects are indicators for urban areas close to big cities, urban areas away from big cities, rural areas close to big cities and rural areas away from big cities. In (11) we include the neighborhood Gini coefficient. In (12) we include the neighborhood poverty rate. In (13) we include the controls simultaneously. In Panel (a) the dependent variable is an indicator for being diagnosed with a lifestyle related disease 2-15 years after immigration. In Panel (b) the dependent variable is an indicator for being diagnosed with a lifestyle related disease 8-15 years after immigration. In all regressions, except for (2), we control for individual characteristics observed at time of assignment. The description of individual controls, municipality controls and parish income groups is presented in Table 4. Municipality health expenditure is missing for a few observations in (4), and immigrant income cannot be calculated for a few parishes without immigrants prior to refugees' arrival in (8).

Table 6: Estimated Heterogeneous Effects by Gender

	Lifestyle Related	Circulatory	Nutritional	Hypertension	Diabetes	Mental Disorder	Neurotic	Visits GP	Visits Psychologist	Died
(a) Within 15 years after immigration										
Middle	-0.015	-0.024***	-0.002	-0.008	-0.015**	0.003	0.002	-0.377	-0.206	-0.015*
	(0.010)	(0.008)	(0.008)	(0.006)	(0.007)	(0.010)	(0.007)	(3.754)	(0.454)	(0.008)
Top	-0.010	-0.016*	0.002	-0.000	-0.009	0.013	0.007	-2.001	-0.572	-0.011
	(0.010)	(0.009)	(0.008)	(0.006)	(0.008)	(0.010)	(0.008)	(4.173)	(0.457)	(0.008)
Middle ×	-0.010	0.016	-0.020	-0.008	0.017*	-0.000	0.005	-0.634	0.521	0.019*
Female	(0.016)	(0.013)	(0.013)	(0.009)	(0.010)	(0.013)	(0.011)	(6.467)	(0.802)	(0.010)
Top × Female	-0.022	0.007	-0.024*	-0.015*	0.009	-0.002	0.004	1.534	0.499	0.006
	(0.015)	(0.012)	(0.012)	(0.008)	(0.010)	(0.012)	(0.010)	(6.119)	(0.765)	(0.009)
(b) Within 8-15 years after immigration										
Middle	-0.008	-0.010	-0.002	-0.005	-0.013*	0.000	-0.004	0.098	-0.046	-0.008
	(0.009)	(0.008)	(0.007)	(0.005)	(0.007)	(0.009)	(0.007)	(2.987)	(0.324)	(0.005)
Top	-0.007	-0.007	0.002	0.002	-0.008	0.007	0.001	-0.583	-0.344	-0.004
	(0.010)	(0.008)	(0.008)	(0.005)	(0.007)	(0.009)	(0.007)	(3.311)	(0.324)	(0.005)
Middle ×	-0.029*	-0.005	-0.025**	-0.012	0.014	-0.006	0.006	-6.437	0.243	0.007
Female	(0.016)	(0.012)	(0.013)	(0.008)	(0.010)	(0.013)	(0.010)	(5.053)	(0.580)	(0.007)
Top × Female	-0.030**	-0.005	-0.026**	-0.015*	0.008	-0.008	0.002	-2.707	0.264	-0.000
	(0.015)	(0.011)	(0.012)	(0.008)	(0.009)	(0.012)	(0.010)	(4.822)	(0.550)	(0.006)
N	21,965	21,965	21,965	21,965	21,965	21,965	21,965	21,965	21,965	21,965
Municipality Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Robust standard errors in parentheses clustered at parish × immigration year level. $^{*}p < 0.10,^{**} p < 0.05,^{***} p < 0.01$. The table shows estimates from a linear probability model testing gender differences in the impact of assignment parish income group on the probability of being diagnosed with each of the diseases in the top panel. In Panel (a) the dependent variable is an indicator for being diagnosed with the disease considered or the number of visits to GPs and psychologists or dying 2-15 years after immigration. In Panel (b) the dependent variable is a dummy for being diagnosed with the considered disease or the number of visits to GPs and psychologists or dying 8-15 years after immigration. We measure parish income groups one year prior to arrival based on median disposable income in each parish among all parishes in Denmark in a given year. In all regressions we control for individual characteristics observed at time of assignment by including controls for gender, marital status, family size, and country of origin as well as age and year of arrival fixed effects. The municipality controls are the population share, the share of co-nationals, the logarithm of the employment rate and the logarithm of the number of GPs per inhabitants in the municipality of assignment. In addition, we condition on municipality of assignment fixed effects.

Table 7: Estimated Heterogeneous Effects by Origin Region

	Lifestyle Related	Circulatory	Nutritional	Hypertension	Diabetes	Mental Disorder	Neurotic	Visits GP	Visits Psychologist	Died
(a) Within 15 years after immigration										
Middle	-0.019**	-0.015**	-0.013*	-0.008	-0.011*	-0.002	0.001	-1.485	0.339	-0.012*
	(0.009)	(0.008)	(0.007)	(0.005)	(0.006)	(0.009)	(0.007)	(4.046)	(0.473)	(0.007)
Top	-0.018*	-0.010	-0.011	-0.003	-0.008	0.011	0.008	-3.602	-0.093	-0.012*
	(0.010)	(0.008)	(0.008)	(0.006)	(0.007)	(0.010)	(0.008)	(4.468)	(0.476)	(0.007)
Middle × Asia	0.001	-0.012	0.018	-0.018	0.014	0.025	0.013	5.520	-1.892**	0.024*
	(0.018)	(0.016)	(0.014)	(0.011)	(0.013)	(0.016)	(0.012)	(7.617)	(0.810)	(0.013)
Top × Asia	-0.005	-0.020	0.019	-0.017	0.013	0.010	0.008	11.325	-1.664**	0.021*
	(0.017)	(0.014)	(0.014)	(0.011)	(0.013)	(0.015)	(0.011)	(7.295)	(0.796)	(0.012)
(b) Within 8-15 years after immigration										
Middle	-0.018**	-0.011	-0.015**	-0.007	-0.009	-0.005	-0.004	-3.555	0.294	-0.006
	(0.009)	(0.007)	(0.007)	(0.005)	(0.006)	(0.008)	(0.006)	(3.250)	(0.343)	(0.004)
Top	-0.017*	-0.006	-0.010	-0.000	-0.006	0.003	0.000	-3.937	-0.048	-0.005
	(0.010)	(0.008)	(0.008)	(0.005)	(0.006)	(0.009)	(0.007)	(3.578)	(0.343)	(0.005)
Middle × Asia	-0.006	-0.009	0.016	-0.016	0.008	0.017	0.008	7.177	-1.348**	0.005
	(0.018)	(0.015)	(0.014)	(0.011)	(0.013)	(0.014)	(0.010)	(5.986)	(0.577)	(0.008)
Top × Asia	-0.008	-0.016	0.013	-0.019*	0.007	0.006	0.005	12.264**	-1.136**	0.004
	(0.017)	(0.014)	(0.013)	(0.011)	(0.013)	(0.014)	(0.010)	(5.758)	(0.564)	(0.007)
N	21,965	21,965	21,965	21,965	21,965	21,965	21,965	21,965	21,965	21,965
Municipality Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Robust standard errors in parentheses clustered at parish × immigration year level. $^{*}p < 0.10,^{**}p < 0.05,^{***}p < 0.01$. The table shows estimates from a linear probability model testing origin region differences in the impact of assignment parish income group on the probability of being diagnosed with each of the diseases in the top panel. In Panel (a) the dependent variable is an indicator for being diagnosed with the disease considered or the number of visits to GPs and psychologists or dying 2-15 years after immigration. In Panel (b) the dependent variable is a dummy for being diagnosed with the considered disease or the number of visits to GPs and psychologists or dying 8-15 years after immigration. We measure parish income groups one year prior to arrival based on median disposable income in each parish among all parishes in Denmark in a given year. In all regressions we control for individual characteristics observed at time of assignment by including controls for gender, marital status, family size, and country of origin as well as year of arrival fixed effects. The municipality controls are the population share, the share of co-nationals, the logarithm of the employment rate and the logarithm of the number of GPs per inhabitants in the municipality of assignment. In addition, we condition on municipality of assignment fixed effects. We group refugees into two origin regions: Asia (Sri Lanka and Vietnam) and other.

Table 8: Estimated Heterogeneous Effects by Age at Immigration

	Lifestyle Related	Circulatory	Nutritional	Hypertension	Diabetes	Mental Disorder	Neurotic	Visits GP	Visits Psychologist	Died
(a) Within 15 years after immigration										
Middle	-0.042***	-0.038***	-0.024*	-0.031***	-0.031***	-0.001	0.003	-3.962	-0.421	-0.003
	(0.016)	(0.014)	(0.012)	(0.010)	(0.012)	(0.012)	(0.010)	(5.688)	(0.649)	(0.009)
Top	-0.029*	-0.026**	-0.011	-0.018*	-0.018	0.014	0.004	-8.082	-0.692	-0.002
	(0.015)	(0.013)	(0.012)	(0.010)	(0.012)	(0.012)	(0.010)	(5.771)	(0.637)	(0.009)
Middle ×	0.038**	0.033**	0.022	0.031***	0.036***	0.006	0.002	5.117	0.650	-0.007
Below Mean Age	(0.018)	(0.015)	(0.014)	(0.010)	(0.012)	(0.015)	(0.013)	(6.149)	(0.760)	(0.011)
Top × Below	0.016	0.019	0.007	0.018*	0.019	-0.003	0.007	10.430*	0.480	-0.010
Mean Age	(0.017)	(0.014)	(0.013)	(0.010)	(0.012)	(0.014)	(0.012)	(5.638)	(0.714)	(0.010)
(b) Within 8-15 years after immigration										
Middle	-0.037**	-0.030**	-0.025**	-0.025***	-0.030***	-0.007	-0.005	-6.555	-0.235	-0.009
	(0.015)	(0.013)	(0.012)	(0.010)	(0.011)	(0.011)	(0.009)	(4.530)	(0.459)	(0.007)
Top	-0.028*	-0.020	-0.012	-0.011	-0.018	0.004	-0.003	-8.431*	-0.434	-0.006
	(0.014)	(0.012)	(0.011)	(0.009)	(0.011)	(0.011)	(0.009)	(4.606)	(0.443)	(0.007)
Middle ×	0.028*	0.027**	0.020	0.024**	0.035***	0.008	0.004	6.445	0.443	0.007
Below Mean Age	(0.017)	(0.014)	(0.013)	(0.010)	(0.012)	(0.013)	(0.011)	(4.917)	(0.547)	(0.008)
Top × Below	0.015	0.017	0.006	0.011	0.021*	-0.001	0.006	10.674**	0.294	0.002
Mean Age	(0.015)	(0.012)	(0.012)	(0.009)	(0.011)	(0.012)	(0.011)	(4.554)	(0.506)	(0.007)
N	21,965	21,965	21,965	21,965	21,965	21,965	21,965	21,965	21,965	21,965
Municipality Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Robust standard errors in parentheses clustered at parish × immigration year level. $^{*}p < 0.10,^{**}p < 0.05,^{***}p < 0.01$. The table shows estimates from a linear probability model testing age differences in the impact of assignment parish income group on the probability of being diagnosed with each of the diseases in the top panel. In Panel (a) the dependent variable is an indicator for being diagnosed with the disease considered or the number of visits to GPs and psychologists or dying 2-15 years after immigration. In Panel (b) the dependent variable is a dummy for being diagnosed with the considered disease or the number of visits to GPs and psychologists or dying 8-15 years after immigration. We measure parish income groups one year prior to arrival based on median disposable income in each parish among all parishes in Denmark in a given year. In all regressions we control for individual characteristics observed at time of assignment by including controls for gender, marital status, family size, and country of origin as well as age and year of arrival fixed effects. The municipality controls are the population share, the share of co-nationals, the logarithm of the employment rate and the logarithm of the number of GPs per inhabitants in the municipality of assignment. In addition, we condition on municipality of assignment fixed effects.

Table 9: Estimated Impact on Labor Market and Public Transfer Outcomes

	Employment>0	Employment	Labor Income	Business Income	Task Complexity	Public Transfers	Public Pensions and Disability Insurance
				(a) Cumulative within 15 years after immigration			
Middle	0.03	0.03	-150.13	-142.44	0.00	-2,530.34	-314.60
	(0.09)	(0.08)	(2,770.20)	(2,823.68)	(0.02)	(2,094.76)	(1,097.29)
Top	0.05	-0.01	-642.20	-1,249.99	-0.00	131.83	35.54
	(0.10)	(0.08)	(2,956.20)	(3,029.12)	(0.03)	(2,424.95)	(1,264.82)
Sample Mean	3.23	2.23	69,950.18	75,064.97	-0.01	142,085.63	19,385.65
				(b) Cumulative 8-15 years after immigration			
Middle	0.01	0.01	-539.99	-435.32	-0.01	-2,239.69	-478.64
	(0.07)	(0.06)	(2,127.72)	(2,170.90)	(0.03)	(1,469.76)	(871.75)
Top	0.02	-0.03	-1,278.99	-1,622.77	0.00	66.99	249.56
	(0.07)	(0.06)	(2,281.07)	(2,349.40)	(0.03)	(1,709.14)	(1,021.92)
Sample Mean	2.18	1.60	51,478.80	55,940.86	-0.02	75,105.65	15,728.99
N	21,965	21,965	21,965	21,965	10,217	21,965	21,965
Municipality Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Robust standard errors in parentheses clustered at parish $\times$ immigration year level. $^{*}p < 0.10,^{**}p < 0.05,^{***}p < 0.01$. The estimates show how refugees' labor market outcomes 2-15 years after immigration (Panel (a)) and 8-15 years after immigration (Panel (b)) are affected by placement neighborhood type using linear regression. The dependent variables are: (1) cumulative years with any employment, (2) cumulative years of employment (full time equivalents), (3) cumulated labor income in USD (deflated to 2000-level), (4) cumulated business income in USD (deflated to 2000-level), (5) average task complexity if employed. Task complexity is the average value of cognitive and communicative task intensities relative to manual task intensity based on occupations merged to the O*NET skill index. (6) cumulated public transfers in USD (deflated to 2000-level), and (7) cumulated public pensions and disability insurance in USD (deflated to 2000-level). We measure parish income groups one year prior to arrival based on median disposable income in each parish among all parishes in Denmark in a given year. In all regressions we control for individual characteristics observed at time of assignment by including controls for gender, marital status, family size, and country of origin as well as age and year of arrival fixed effects. The municipality controls are the population share, the share of co-nationals, the logarithm of the employment rate and the logarithm of the number of GPs per inhabitants in the municipality of assignment. In addition, we condition on municipality of assignment fixed effects. The sample mean denotes the mean of the outcome (listed in the top panel) in the different year intervals.

Table 10: Estimated Impact on Education Outcomes

	All Education	Basic	Vocational	Higher	Health Education
	(a) Within 15 years after immigration				
Middle	0.014*	0.000	0.014**	0.004	-0.002
	(0.008)	(0.002)	(0.006)	(0.006)	(0.005)
Top	0.021**	-0.000	0.019***	0.006	0.001
	(0.008)	(0.003)	(0.006)	(0.007)	(0.006)
Sample Mean	0.15	0.01	0.09	0.07	0.05
	(b) Within 8 years after immigration				
Middle	0.014*	0.000	0.014**	0.004	-0.002
	(0.008)	(0.002)	(0.006)	(0.006)	(0.005)
Top	0.021**	-0.001	0.019***	0.006	0.001
	(0.008)	(0.003)	(0.006)	(0.007)	(0.006)
Sample Mean	0.15	0.01	0.09	0.07	0.05
N	21,965	21,965	21,965	21,965	21,965
Municipality Controls	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes

Notes: Robust standard errors in parentheses clustered at parish $\times$ immigration year level. $^{*}p < 0.10,^{**}p < 0.05,^{***}p < 0.01$. The regressions test if the probability of completing any of the education types after immigration is dependent on initial neighborhood income group. The dependent variables are dummies indicating whether the refugee completed the formal education of the type considered within 15 years after immigration (Panel (a)), and within 8 years after immigration (Panel (b)). We measure parish income groups one year prior to arrival based on median disposable income in each parish among all parishes in Denmark in a given year. In all regressions we control for individual characteristics observed at time of assignment by including controls for gender, marital status, family size, and country of origin as well as age and year of arrival fixed effects. The municipality controls are the population share, the share of co-nationals, the logarithm of the employment rate and the logarithm of the number of GPs per inhabitants in the municipality of assignment. In addition, we condition on municipality of assignment fixed effects. The sample mean denotes the mean of the outcome (listed in the top panel) in the different year intervals.

Table 11: Estimated Impact on Lifestyle Related Diseases within 15 Years After Immigration (Apartment Building Level)

	Baseline	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Placed in Bottom	0.017***	0.013**	0.015**	0.014**	0.017***	0.017***	0.017***	0.017***	0.017***	0.016***	0.017***	0.015**	0.016***	0.015**	0.014**	0.024***	0.015**
Income Apartment Building	(0.006)	(0.006)	(0.007)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.007)	(0.009)	(0.006)
Placed in Bottom		0.011															
Income Municipality		(0.011)															
Placed in Bottom		0.012															
Income Parish		(0.008)															
Log Share with					0.016										0.027		
Lifestyle Related Diseases					(0.043)										(0.045)		
Log Health						0.097									0.077		
Expenditure						(0.078)									(0.081)		
Log Employment Rate							-0.001								0.253*		
in Parish							(0.035)								(0.144)		
Number of Refugees								-0.000							-0.000		
								(0.000)							(0.000)		
Number of Refugees								0.000							0.000**		
Squared								(0.000)							(0.000)		
Immigrant Share									0.054						0.785		
									(0.165)						(0.711)		
Immigrant Share									-0.117						-1.832		
Squared									(0.429)						(1.290)		
Log Average										-0.010					-0.029		
Immigrant Household Income										(0.024)					(0.050)		
Number of Sports											-0.001*				-0.001		
Facilities											(0.000)				(0.000)		
Neighborhood													0.298*		-1.269		
Inequality													(0.161)		(0.773)		
Log Share Below														0.275**	0.814		
Poverty Line														(0.117)	(0.508)		
N	18,031	18,031	17,914	18,031	18,031	18,029	18,031	18,031	18,031	17,994	18,031	18,031	18,031	18,031	17,875	8,765	15,701
Municipality Controls	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Parish Type FE	No	No	No	No	No	No	No	No	No	No	No	Yes	No	No	No	No	No
Parish FE	No	No	Yes	No	No	No	No	No	No	No	No	No	No	No	Yes	No	No

Notes: Robust standard errors in parentheses clustered at parish × immigration year level. $^{*}p < 0.10,^{**}p < 0.05,^{***}p < 0.01$. The table presents modified versions of model (2) with only two neighborhood income groups (bottom vs. rest) and different sets of controls, using apartment building instead of parish level income groups. Column (Baseline) shows the baseline coefficients from a modified model (2) with apartment building level income groups. In (1) we control for placement in the poorest third of municipalities and placement in the poorest third of parishes. In (2) we replace the municipality fixed effects with parish fixed effects. Some singleton observations are dropped in this case. The control variables included in columns (3)-(14) are described in Table 5. In column (15) we consider apartment buildings with at least 10 non-refugee residents. In column (16) we consider only the oldest household member. In all columns the dependent variable is an indicator for being diagnosed with a lifestyle related disease 2-15 years after immigration. In all regressions we control for individual characteristics observed at time of assignment. The individual controls and municipality controls are described in Table 4. Municipality health expenditure is missing for a few observations in (5), and immigrant income cannot be calculated for a few parishes without immigrants prior to refugees' arrival in (9).

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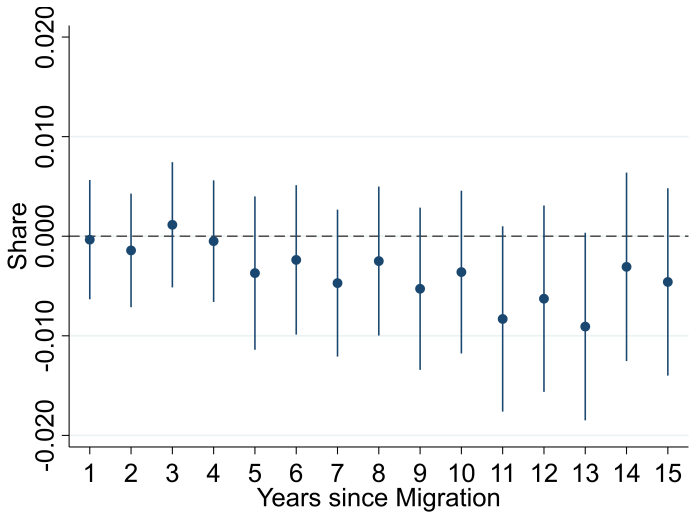
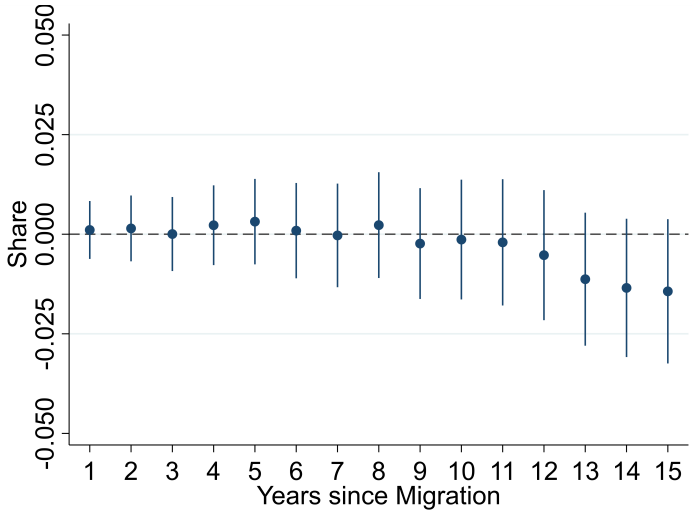
A Online Appendix

A Additional Tables and Figures



(a) Yearly Incidents



(b) Cumulative

Figure A.1: Development of Lifestyle Related Diagnoses

Notes: Robust standard errors clustered at parish × immigration year level. 95 percent confidence intervals. The graphs plot the development of lifestyle related diseases over time. The coefficients plotted show the increased probability of being diagnosed with a lifestyle related disease if initially assigned to a top-income neighborhood compared to a bottom-income neighborhood. In Panel (a) we show the coefficients from 15 different regression, one for each year plotted, in which the dependent variable is a dummy for being diagnosed with a lifestyle related disease in the year considered. In Panel (b) the coefficients also stem from 15 different regressions but the dependent variable in this panel is a dummy for being diagnosed in the year considered or any year before that since year of immigration. We measure parish income groups one year prior to arrival based on median disposable income in each parish among all parishes in Denmark in a given year. The estimation equation is described in Model 2.

Table A.1: Correlation Matrix for Neighborhood Characteristics in 1986 to 1998

	(1) Median Household Income	(2) Employment Rate	(3) Prevalence of Lifestyle Related Diseases	(4) Co-Nationals	(5) Poverty Rate	(6) Gini Coefficient	(7) Crime Rate
Median Household Income	1.00						
Employment Rate	0.51***	1.00					
Prevalence of Lifestyle Related Diseases	-0.29***	-0.35***	1.00				
Co-Nationals	-0.03*	-0.26***	0.02	1.00			
Poverty Rate	-0.58***	-0.53***	0.18***	-0.02	1.00		
Gini Coefficient	-0.17***	-0.42***	0.19***	-0.04***	0.77***	1.00	
Crime Rate	-0.23***	-0.43***	0.11***	0.17***	0.18***	0.09***	1.00

Notes: The table shows the correlation matrix for neighborhood characteristics in 1986 to 1998, among the neighborhoods where refugees were resettled. The definition of characteristics are described in Table 2 and Section I.B.

Table A.2: Summary Statistics for Initial Placement (Apartment Building)

	Bottom Mean	Middle Mean	Top Mean
Characteristics of Residents			
Age	40.27	39.28	38.36
Median Household Income	13,643.36	14,221.65	14,743.72
Employment Rate	0.47	0.53	0.57
Prevalence of Lifestyle Related Diseases per 1,000 Inhabitants	69.84	63.86	52.72
Inhabitants	20.37	13.53	13.51
Co-Nationals	1.30	0.94	0.75
Poverty Rate	0.11	0.10	0.09
Crime Rate	0.05	0.04	0.04
Parish Type			
Urban Area (Near City)	0.78	0.61	0.74
Urban Area (Away from City)	0.05	0.22	0.16
Rural Area (Near City)	0.04	0.06	0.07
Rural Area (Away from City)	0.13	0.11	0.03
Characteristics of Municipality			
General Practioners per 1,000 Inhabitants	0.50	0.44	0.46
Incidences of Lifestyle Related Diseases per 1,000 Inhabitants	34.64	28.84	25.78
Health and Social Expenditure per Capita	3,963.78	4,111.43	4,082.90
N	1,906	3,571	5,702

Notes: Summary statistics for apartment buildings in which refugees were resettled. An apartment building refers to the group of households living in the same building sharing a stairway. "Bottom", "Middle" and "Top" refer to characteristics of apartment buildings in the bottom, middle and top third of apartment buildings measured by median apartment building disposable income in a given year. We calculate the median income of each apartment building including all inhabitants aged 18 or above and define the income groups among all apartment buildings, irrespective of any refugee assignment. We define income groups and all apartment building characteristics one year prior to immigration. Prevalence of lifestyle related diseases is measured as all incidences over the previous 8 years and thus only defined for refugees arriving after 1993. Employment rate is the share of the population with any employment between the ages of 18-65. The crime rate is the share of persons convicted of a criminal offense (excl. traffic related offenses), age 15 or older. Observations are apartment building-year. Health and social expenditure per capita and median household income are measured in USD. Data on "Health and Social Expenditure per Capita" stems from Statistikbanken, (REG1, REG1R and REG11). Parish types are defined by Ministeriet for By, Bolig og Landdistrikter (2013). All other data are from administrative registers provided by Statistics Denmark.

Table A.3: Summary Statistics for Initial Placement (Municipality)

	Bottom Mean	Middle Mean	Top Mean
Characteristics of Residents			
Age	47.77	47.53	46.00
Median Household Income	14,632.81	14,692.50	15,938.28
Employment Rate	0.67	0.69	0.73
Inhabitants	33,151.86	20,171.66	23,997.75
Co-nationals	68.90	41.04	29.87
Poverty Rate	0.08	0.07	0.06
Crime Rate	0.01	0.01	0.01
Parish Type			
Urban Area (Near City)	0.20	0.25	0.59
Urban Area (Away from City)	0.10	0.30	0.22
Rural Area (Near City)	0.17	0.13	0.11
Rural Area (Away from City)	0.52	0.32	0.07
Characteristics of Municipality			
General Practioners per 1,000 Inhabitants	0.38	0.36	0.41
Incidences of Lifestyle Related Diseases per 1,000 Inhabitants	32.15	29.38	24.72
Health and Social Expenditure per Capita	3,693.70	3,640.13	3,570.68
N	172	520	1,014

Notes: Summary statistics for municipalities in which refugees were resettled. "Bottom", "Middle" and "Top" refer to characteristics of municipalities in the bottom, middle and top third of municipalities measured by median municipality disposable income in a given year. We calculate the median income of each municipality including all inhabitants aged 18 or above and define the income groups among all municipalities, irrespective of any refugee assignment. We define income groups and all municipality characteristics one year prior to immigration. Employment rate is the share of the population with any employment between the ages of 18-65. The crime rate is the share of persons convicted of a criminal offense (excl. traffic related offenses), age 15 or older. Observations are municipality-year. Health and social expenditure per capita and median household income are measured in USD. Data on "Health and Social Expenditure per Capita" stems from Statistikbanken, (REG1, REG1R and REG11). Parish tsypes are defined by Ministeriet for By, Bolig og Landdistrikter (2013). All other data are from administrative registers provided by Statistics Denmark.

Table A.4: Balancing Tests, Conditional on Municipality Fixed Effects

	(1) Bottom Income Group	(2) Middle Income Group	(3) Top Income Group	(4) Lifestyle Related Disease	(5) GPs	(6) Population Share	(7) Employment Rate	(8) Employment Rate Immigrants
Unobserved at Time of Allocation								
Unknown Education	-0.007	0.002	0.005	0.000	-0.001	0.000	0.000	0.002
	(0.007)	(0.009)	(0.009)	(0.000)	(0.001)	(0.000)	(0.001)	(0.002)
Basic Education	-0.006	0.002	0.004	0.000	-0.001	-0.000	0.001	-0.000
	(0.007)	(0.009)	(0.008)	(0.000)	(0.001)	(0.000)	(0.001)	(0.002)
Higher Education	0.001	0.001	-0.003	0.000	-0.000	-0.000	0.001	0.002
	(0.008)	(0.010)	(0.009)	(0.000)	(0.001)	(0.000)	(0.001)	(0.002)
Circulatory Disease	0.004	-0.043	0.039	0.000	0.004	0.000	0.000	-0.000
	(0.021)	(0.026)	(0.026)	(0.000)	(0.003)	(0.000)	(0.004)	(0.007)
Nutritional Disease	-0.004	-0.017	0.021	0.000	-0.001	-0.000	0.005	0.012
	(0.026)	(0.033)	(0.032)	(0.001)	(0.003)	(0.000)	(0.005)	(0.008)
Neurotic Disorder	-0.009	0.022	-0.012	0.002	0.000	-0.000	-0.009	-0.022
	(0.051)	(0.071)	(0.062)	(0.001)	(0.006)	(0.000)	(0.008)	(0.016)
Observed at Time of Allocation								
Age 30-49 Years	-0.006	0.008	-0.002	-0.000	-0.001	-0.000***	0.001	0.002
	(0.005)	(0.007)	(0.006)	(0.000)	(0.001)	(0.000)	(0.001)	(0.002)
Age 50-64 Years	-0.023**	0.033***	-0.010	-0.000	-0.001	0.000	-0.002	-0.000
	(0.009)	(0.013)	(0.012)	(0.000)	(0.001)	(0.000)	(0.002)	(0.003)
Female	-0.001	0.012**	-0.011**	0.000***	0.000	0.000***	-0.002***	0.002
	(0.004)	(0.005)	(0.005)	(0.000)	(0.001)	(0.000)	(0.001)	(0.001)
Number of Adults	0.004	-0.011	0.007	-0.000	-0.001	-0.000	0.001	0.004
	(0.008)	(0.009)	(0.008)	(0.000)	(0.001)	(0.000)	(0.001)	(0.002)
Number of Children 0-2 Years Old	-0.018**	0.022**	-0.004	-0.000**	-0.001	-0.000	-0.001	-0.005
	(0.008)	(0.010)	(0.009)	(0.000)	(0.001)	(0.000)	(0.001)	(0.003)
Number of Children 3-17 Years Old	-0.008***	0.002	0.006	-0.000***	0.001	-0.000	0.000	-0.001
	(0.003)	(0.003)	(0.003)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)
Married	0.012**	-0.001	-0.011	0.001***	0.001	0.000***	-0.002**	0.003
	(0.006)	(0.007)	(0.007)	(0.000)	(0.001)	(0.000)	(0.001)	(0.002)
Sample Mean	0.18	0.31	0.51	0.03	0.45	0.00	0.69	0.49
Year of Immigration FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country of Origin FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	21,965	21,965	21,965	21,965	21,965	21,965	21,965	21,965
F	0.39	0.58	0.68	1.26	1.01	2.04	0.50	1.08
Pr > F	0.89	0.75	0.66	0.27	0.41	0.06	0.81	0.37

Notes: Balancing tests for parishes using linear regressions. Robust standard errors in parentheses clustered at the household level. $^{**}p < 0.05, ^{***}p < 0.01$. F denotes the F-statistic for joint insignificance of the educational attainment dummies and pre-existing health conditions. Vocational education is the reference group for the education dummies. Each column represents a different balancing test testing whether refugees with certain characteristics (column farthest to the left) are more likely to be placed in parishes with specific characteristics (dependent variables). The dependent variables in (1)-(3) are dummies for assignment to a bottom third income parish (1), middle third income parish (2) or top third income parish (3). In column (4) the dependent variable is the incidence (as a share of inhabitants) of lifestyle related diseases. In column (5) the dependent variable is the number of GPs per capita in the municipality. In columns (6)-(8) the dependent variable is population share, the employment rate or the employment rate among immigrants in the parish. The controls are individual characteristics observed by the DRC at time of assignment and characteristics that the DRC does not observe at time of assignment: initial education and health. We measure all individual characteristics at year of immigration. We measure all parish characteristics one year prior to immigration. The sample mean denotes the mean of the dependent variable.

Table A.5: Balancing Tests, Apartment Building Level

	(1) Bottom Income Group	(2) Middle Income Group	(3) Top Income Group	(4) Lifestyle Related Disease	(5) GPs	(6) Population Share	(7) Employment Rate	(8) Employment Rate Immigrants
Unobserved at Time of Allocation								
Unknown Education	-0.016	0.018	-0.001	-0.002	0.001	-0.000	0.029***	0.001
	(0.012)	(0.012)	(0.009)	(0.002)	(0.003)	(0.000)	(0.008)	(0.003)
Basic Education	0.001	0.013	-0.014	0.000	-0.002	-0.000	0.011	-0.001
	(0.012)	(0.011)	(0.008)	(0.002)	(0.003)	(0.000)	(0.008)	(0.003)
Higher Education	-0.001	-0.001	0.002	-0.000	0.004	-0.000	0.016	0.005
	(0.013)	(0.013)	(0.009)	(0.002)	(0.003)	(0.000)	(0.009)	(0.003)
Circulatory Disease	-0.003	-0.012	0.016	-0.004	0.013	0.000	0.003	-0.009
	(0.035)	(0.034)	(0.026)	(0.004)	(0.009)	(0.000)	(0.022)	(0.009)
Nutritional Disease	-0.014	-0.048	0.062	0.000	0.018	0.000	0.029	0.009
	(0.045)	(0.044)	(0.037)	(0.006)	(0.012)	(0.000)	(0.031)	(0.010)
Neurotic Disorder	-0.049	-0.048	0.096	0.001	-0.040	-0.000	0.033	-0.004
	(0.093)	(0.083)	(0.077)	(0.016)	(0.025)	(0.000)	(0.067)	(0.022)
Observed at Time of Allocation								
Age 30-49 Years	0.004	-0.009	0.004	-0.002	-0.007***	-0.000	-0.017***	0.003
	(0.009)	(0.009)	(0.006)	(0.001)	(0.002)	(0.000)	(0.006)	(0.002)
Age 50-64 Years	-0.054***	0.053***	0.001	-0.001	-0.003	-0.000	-0.010	-0.001
	(0.016)	(0.016)	(0.012)	(0.002)	(0.004)	(0.000)	(0.010)	(0.004)
Female	-0.064***	0.046***	0.018***	0.002	0.004**	-0.000	0.030***	0.004**
	(0.007)	(0.007)	(0.005)	(0.001)	(0.002)	(0.000)	(0.005)	(0.002)
Number of Adults	-0.045***	0.006	0.039***	0.003	-0.013***	-0.000***	0.009	0.006**
	(0.012)	(0.012)	(0.010)	(0.002)	(0.004)	(0.000)	(0.008)	(0.003)
Number of Children 0-2 Years Old	-0.011	0.006	0.005	-0.002	-0.012***	-0.000	0.019**	-0.001
	(0.014)	(0.014)	(0.011)	(0.002)	(0.004)	(0.000)	(0.010)	(0.004)
Number of Children 3-17 Years Old	-0.008	-0.001	0.009**	0.000	-0.004***	-0.000***	-0.004	0.001
	(0.005)	(0.005)	(0.004)	(0.001)	(0.001)	(0.000)	(0.003)	(0.001)
Married	-0.036***	0.038***	-0.002	0.001	0.003	-0.000	0.017***	-0.002
	(0.009)	(0.009)	(0.006)	(0.002)	(0.002)	(0.000)	(0.006)	(0.002)
Sample Mean	0.51	0.36	0.13	0.02	0.46	0.00	0.47	0.48
Year of Immigration FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country of Origin FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	No	No	No	No	No	No	No	No
N	18,031	18,031	18,031	18,031	18,031	18,031	18,031	18,031
F	0.60	0.91	1.74	0.61	1.91	0.45	2.38	1.00
Pr > F	0.73	0.48	0.11	0.72	0.08	0.84	0.03	0.42

Notes: Balancing tests for apartment buildings using linear regressions. Robust standard errors in parentheses clustered at the household level. $^{**}p < 0.05$,$^{***}p < 0.01$. F denotes the F-statistic for joint insignificance of the educational attainment dummies and pre-existing health conditions. Vocational education is the reference group for the education dummies. Each column represents a different balancing test testing whether refugees with certain characteristics (column farthest to the left) are more likely to be placed in apartment buildings with specific characteristics (dependent variables). The dependent variables in (1)-(3) are dummies for assignment to a bottom income apartment building (1), middle income apartment building (2) or top income apartment building (3). In column (4) the dependent variable is the incidence (as a share of inhabitants) of lifestyle related diseases. In column (5) the dependent variable is the number of GPs per capita in the municipality. In columns (6)-(8) the dependent variable is population share, the employment rate or the employment rate among immigrants in the parish. The controls are individual characteristics observed by the DRC at time of assignment and characteristics which the DRC does not observe at time of assignment: initial education and health. We measure all individual characteristics at year of immigration. We measure all apartment building characteristics one year prior to immigration. The sample mean denotes the mean of the dependent variable.

Table A.6: Mobility within Year of Immigration

	(1) Stayed in Initial Neighborhood	(2) Stayed in Initial Neighborhood	(3) Number of Moves	(4) Number of Moves
Middle	0.032	0.005	-0.042	0.004
	(0.023)	(0.022)	(0.030)	(0.029)
Top	0.015	0.014	-0.022	-0.013
	(0.022)	(0.025)	(0.029)	(0.034)
Sample Mean	0.66	0.66	0.44	0.44
N	21,965	21,965	21,965	21,965
Municipality Controls	No	Yes	No	Yes
Municipality FE	No	Yes	No	Yes

Notes: Robust standard errors clustered at parish $\times$ immigration year level. $^{*}p < 0.10,^{**} p < 0.05,^{***} p < 0.01$. Estimates from a linear probability model testing the impact of assignment parish income group on the probability of remaining in the initial neighborhood within the year of immigration (columns (1)-(2)), and the number of moves across neighborhoods within year of immigration (columns (3)-(4)). We measure parish income groups one year prior to arrival based on median disposable income in each parish among all parishes in Denmark in a given year. We control for individual characteristics observed at time of assignment by including controls for gender, marital status, family size, and country of origin as well as age and year of arrival fixed effects. The municipality controls are the population share, the share of co-nationals, the logarithm of the employment rate and the logarithm of the number of GPs per inhabitants in the municipality of assignment. In addition, we condition on municipality of assignment fixed effects in columns (2) and (4). The sample mean denotes the mean of the dependent variable.

Table A.7: Robustness Checks and Placebo Tests

	Panel A: Robustness of Lifestyle Related Diseases					Panel B: Placebo Tests		
	Baseline	(1)	(2)	(3)	(4)	Congenital Abnormalities	Congenital Metabolic Disorders	Schizophrenia
(a) Diagnosed within 15 years after immigration								
Middle	-0.018**	-0.019**	-0.020*	-0.017**	-0.012	-0.000	-0.004	0.003
	(0.008)	(0.009)	(0.012)	(0.008)	(0.008)	(0.003)	(0.003)	(0.004)
Top	-0.019**	-0.014	-0.026**	-0.020**	-0.015*	-0.002	-0.000	0.003
	(0.009)	(0.009)	(0.012)	(0.009)	(0.008)	(0.004)	(0.004)	(0.005)
(b) Diagnosed 8-15 years after immigration								
Middle	-0.019**	-0.024***	-0.020*	-0.018**	-0.015**	-0.002	-0.002	0.001
	(0.008)	(0.008)	(0.011)	(0.008)	(0.007)	(0.003)	(0.003)	(0.004)
Top	-0.018**	-0.019**	-0.025**	-0.019**	-0.017**	-0.002	0.002	0.001
	(0.009)	(0.009)	(0.012)	(0.009)	(0.008)	(0.003)	(0.003)	(0.004)
N	21,965	21,965	15,322	21,757	21,965	21,965	21,965	21,965
Municipality Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Income Type	Disposable	Disposable	Disposable	Disposable	Disposable	Disposable	Disposable	Disposable
Moment	Median	Mean	Median 3 Years	Median	Median	Median	Median	Median
Method	OLS	OLS	OLS	Probit	OLS	OLS	OLS	OLS

Notes: Robust standard errors in parentheses clustered at parish × immigration year level. $^{*}p < 0.10,^{**} p < 0.05,^{***} p < 0.01$. All estimates in Panel A show the impact of assignment parish on the probability of being diagnosed with a lifestyle related disease in different setups. In Panel B we use congenital disorders (congenital abnormalities and congenital metabolic disorders) and schizophrenia as placebo outcomes which should not be affected by neighborhood characteristics. Column (Baseline) replicates the main results from Table 4. Column (1) shows the same estimation where income groups instead are based on the mean parish income. Colum (2) shows estimations where income groups are based on the most common income group observed over three years prior to arrival. Column (3) shows the estimated neighborhood effects from a probit model. Column (4) shows results using the baseline specification while only considering primary diagnoses as the outcome. In Panel (a) the dependent variable is an indicator for being diagnosed with a disease 2-15 years after immigration. In Panel (b) the dependent variable is an indicator for being diagnosed with a disease 8-15 years after immigration. We measure parish characteristics one year prior to arrival. In all regressions we control for individual characteristics observed at time of assignment by including controls for gender, marital status, family size, and country of origin as well as age and year of arrival fixed effects. The municipality controls are the population share, the share of co-nationals, the logarithm of the employment rate and the logarithm of the number of GPs per inhabitants in the municipality of assignment. In addition, we condition on municipality of assignment fixed effects.

Table A.8: Mobility within 15 Years After Immigration

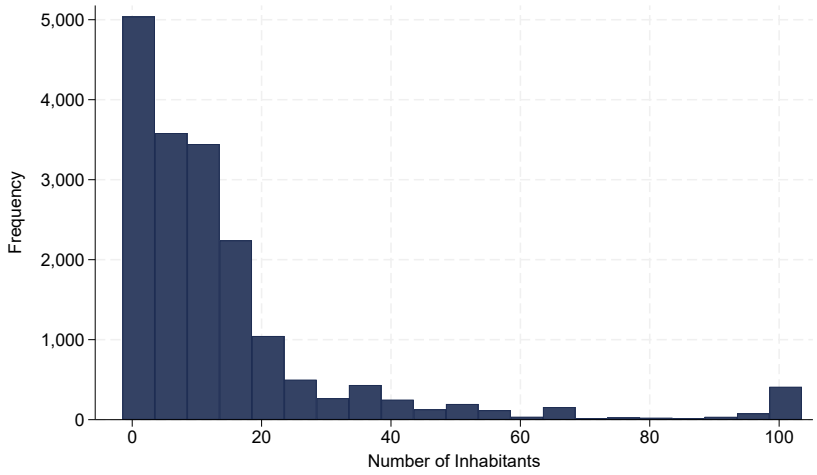
	(1) Stayed in Initial Neighborhood	(2) Moved to Bottom Income Neighborhood	(3) Moved to Middle Income Neighborhood	(4) Moved to Top Income Neighborhood	(5) Years in Bottom Income Neighborhoods
Middle	0.023**	-0.006	-0.002	-0.002	-1.594***
	(0.011)	(0.014)	(0.014)	(0.014)	(0.155)
Top	-0.006	0.017	0.011	0.006	-2.509***
	(0.012)	(0.015)	(0.015)	(0.015)	(0.161)
Sample Mean	0.14	0.57	0.57	0.58	4.68
N	21,965	21,965	21,965	21,965	21,965
Municipality Controls	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes

Notes: Robust standard errors clustered at parish $\times$ immigration year level. $^{*}p < 0.10,^{**}p < 0.05,^{***}p < 0.01$. Estimates from a linear probability model testing the impact of assignment parish income group on the probability of staying in the initial neighborhood for all 15 years (column (1)), ever moving to different types of neighborhoods within 15 years after immigration (columns (2)-(4)) and the cumulative number of years spent in a bottom income neighborhood (column (5)). We measure parish income groups one year prior to arrival based on median disposable income in each parish among all parishes in Denmark in a given year. We control for individual characteristics observed at time of assignment by including controls for gender, marital status, family size, and country of origin as well as age and year of arrival fixed effects. The municipality controls are the population share, the share of co-nationals, the logarithm of the employment rate and the logarithm of the number of GPs per inhabitants in the municipality of assignment. In addition, we include municipality of assignment fixed effects. The sample mean denotes the mean of the dependent variable.

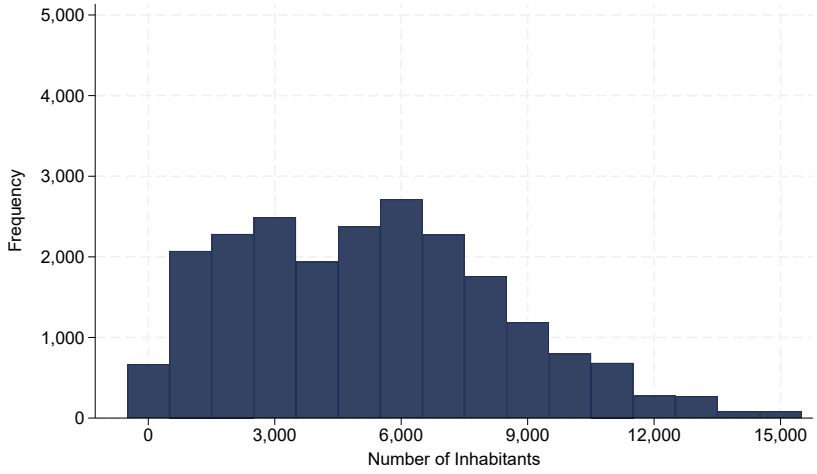
Table A.9: Estimated Impact on Education Outcomes by Age at Immigration

	All Education	Basic	Vocational	Higher	Health Education
	(a) Within 15 years after immigration				
Middle	0.002	0.000	0.001	-0.000	-0.000
	(0.009)	(0.002)	(0.006)	(0.007)	(0.006)
Top	-0.009	-0.002	0.002	-0.012*	-0.004
	(0.010)	(0.002)	(0.007)	(0.007)	(0.006)
Middle ×	0.019	-0.000	0.021**	0.007	-0.002
Below Mean Age	(0.013)	(0.003)	(0.009)	(0.010)	(0.008)
Top × Below	0.047***	0.003	0.027***	0.028***	0.007
Mean Age	(0.012)	(0.003)	(0.009)	(0.009)	(0.007)
Sample Mean	0.15	0.01	0.09	0.07	0.05
	(b) Within 8 years after immigration				
Middle	0.002	0.000	0.002	-0.000	-0.000
	(0.009)	(0.002)	(0.006)	(0.007)	(0.006)
Top	-0.009	-0.002	0.003	-0.012*	-0.004
	(0.010)	(0.002)	(0.007)	(0.007)	(0.006)
Middle ×	0.019	-0.000	0.018**	0.006	-0.003
Below Mean Age	(0.013)	(0.003)	(0.009)	(0.010)	(0.008)
Top × Below	0.047***	0.003	0.024***	0.028***	0.007
Mean Age	(0.012)	(0.003)	(0.008)	(0.009)	(0.007)
Sample Mean	0.15	0.01	0.09	0.07	0.05
N	21,965	21,965	21,965	21,965	21,965
Municipality Controls	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes

Notes: Robust standard errors in parentheses clustered at parish × immigration year level. $^{*}p < 0.10,^{**} p < 0.05,^{***} p < 0.01$. The regressions test if the probability of completing any of the education types after immigration is dependent on initial neighborhood income group and age at immigration. The dependent variables are dummies indicating whether the refugee completed the formal education of the type considered within 15 years after immigration (Panel (a)), and within 8 years after immigration (Panel (b)). We measure parish income groups one year prior to arrival based on median disposable income in each parish among all parishes in Denmark in a given year. In all regressions we control for individual characteristics observed at time of assignment by including controls for gender, marital status, family size, and country of origin as well as age and year of arrival fixed effects. The municipality controls are the population share, the share of co-nationals, the logarithm of the employment rate and the logarithm of the number of GPs per inhabitants in the municipality of assignment. In addition, we condition on municipality of assignment fixed effects. The sample mean denotes the mean of the outcome (listed in the top panel) in the different year intervals.



(a) Apartment Buildings



(b) Parishes

Figure A.2: Frequency of Geographical Areas by Area Population Size

Notes: The graphs show the frequency of geographical areas by area size measured as the number of inhabitants. Panel (a) shows the distribution of apartment buildings by apartment building population size, and Panel (b) shows the distribution of parishes by parish population size.

B Diagnoses with ICD Codes

The first parentheses indicate (ICD-10) diagnoses codes from 1994 and onwards and the second parentheses indicate (ICD-8) diagnoses codes before 1994. Diagnoses in bold correspond to the groups we use in our regression analysis.

Lifestyle related diseases:

- **Circulatory diseases:**
  - **Hypertensive diseases** (referred to as hypertension): (I10), (400-401)
  - Ischaemic heart diseases: (I20, I22, I24, I25), (411-414)
  - Pulmonary diseases: (I26-I28), (426, 450, 514)
  - Other forms of heart diseases: (I30-I52), (393-398, 420-429)
  - Cerebrovascular diseases: (I60-I67, I69), (430-438)
  - Arterial diseases: (I70-I72,I74), (440-442, 444)
- **Endocrine, nutritional and metabolic diseases** (referred to as nutritional diseases):
  - **Diabetes**: (E10-E14), (250)
  - Obesity: (E66), (277)
  - Metabolic disorders (high cholesterol): (E78), (272)
- Chronic obstructive pulmonary diseases (COPD): (J44), (490, 491, 492)
- Hip arthrosis: (M16), (710.2)
- Alcohol related diseases:
  - Alcohol induced acute pancreatitis: (K85.2), (577.0),
  - Alcoholic liver disease: (K70), (571.0)
  - Alcoholism: (No ICD10 code), (303)

Mental disorders:

- Mental and behavioral disorders due to psychoactive substance use: (F10-F19), (291, 294.3, 309.1, 29430, 29438, 29439, 30919)
- Schizophrenia, schizotypal and delusional disorders: (F20-F29), (295)
- Mood [affective] disorders: (F30-F39), (296)
- **Neurotic, stress-related and somatoform disorders:** (F40-F48), (300)
- Behavioral syndromes associated with physiological disturbances and physical factors: (F50-F59), (305)
- Disorders of adult personality and behavior: (F60-F69), (301, 302)

Congenital disorders:

- **Congenital abnormalities:** (Q00-Q99), (740-759)
- **Congenital metabolic disorders:** (E70-E77, E79-E90), (270-271, 273-276, 278-279)