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ABSTRACT

Sick of Your Poor Neighborhood? Quasi-Experimental Evidence on Neighborhood Effects on Health*

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 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. Differences in health care access, ethnic networks, and individual labor market outcomes cannot explain our findings.

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, see 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, see for example Chetty et al. (2016b).

But why do people living in low-income areas have poorer health? A potential explanation is that low-income areas induce unhealthy lifestyle choices, such as lack of physical activity, unhealthy diets and the use of tobacco and alcohol, because, for example, amenities in low-income areas do not support healthy lifestyle choices or because unhealthy behaviors are transmitted between 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 gradient in health. Moreover, neighborhood income 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 local areas in Denmark to overcome these challenges, and we document significant causal impacts of neighborhoods on a wide range of lifestyle related diseases. Moreover, to the best of our knowledge, we are the first to explore the potential mechanisms behind neighborhood effects in health and to document causal neighborhood effects on health across neighborhoods as small as apartment buildings. To do so, we exploit a natural experiment created by a Danish Spatial Dispersal Policy in effect from 1986 to 1998, that quasi-randomly assigned refugee families to differ-

ent 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 and poverty rates.[2] 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 health care access, local labor demand, and the size of the potential network.

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.

In the second part of our analysis we take a step towards understanding the documented neighborhood income gradient in 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), 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, see

[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).

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

Patienthåndbogen (2017). Since some of these factors may also affect mental health, we also 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 small. Furthermore, in our empirical analysis we compare individuals in different neighborhoods within the same municipality, who are subject to the same local health authorities and local labor market.

Moreover, we show that the estimated income gradient in health is not a result of more advantageous labor market outcomes for individuals placed in higher income neighborhoods. Our results consistently show that there are no significant differences in any labor market outcome 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 can rule out any income effects of neighborhood placement, and this allows us to attribute the estimated health effects to neighborhood quality rather than to individual income. In addition, we find that in richer neighborhoods, more refugees obtain a vocational education, but previous evidence does not find any causal impact of education on health outcomes in Sweden or Denmark (Meghir et al. (2018); Behrman et al. (2011)).

There are some mechanisms that we cannot measure and test directly. These are factors such as health behaviors of peers and some local amenities. However, we take a step in that direction by documenting the importance of the very local environment. We do this by studying a smaller neighborhood level, namely households living in the same apartment building, which changes how well we capture potential peer groups and the character of the immediate neighborhood.[4] 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.

[4]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.

We base our analysis on longitudinal administrative registers, which allows us to observe annual residential locations, income, hospital diagnoses and other individual characteristics. In spite of the high quality of our data, it is likely that our estimates capture a lower bound of the size of the true effect due to varying detection rates across areas. Correlational evidence shows that a larger share of residents in richer neighborhoods visit their general practitioner (GP) or dentist in a given year, see Panels g-h in Figure A.1 in the Online Appendix and Bago d'Uva and Jones (2009). This may result in lower detection rates in poorer neighborhoods which will bias our estimates towards zero.

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). However, because of data limitations, the *Moving to Opportunity* experiment only provides limited evidence on neighborhood effects on health. 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)).

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. A different approach is taken by Grönqvist et al. (2012) who show that income inequality within neighborhoods does not impact the risk of hospitalization. The health impact of income inequality within a neighborhood may be different from the impact of income differences between neighborhoods. The poorest neighborhoods may have little income inequality, while still having adverse health impacts. Finally, a study by Hamad et al. (2020) documents an interesting 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.[5] Contrary to our work, these studies do not consider the impacts on mental health nor the potential mechanisms behind the effects. Compared with any 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 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.

We contribute to the literature on neighborhood effects in two ways. The first part of our contribution is to document the existence of strong and significant causal long-term neighborhood effects on a wide range of lifestyle related diseases. To the best of our knowledge, we are the first to document that these effects do not only exist within municipalities or parishes, but also within small local environments, such as apartment buildings. The existing literature does not provide much evidence for why neighborhood effects on health exist. The second part of our contribution is to fill part of this gap by ruling out a number of likely mechanisms and documenting the importance of the very local environment in causing neighborhood effects on health. Our paper suggests that these effects on health are likely caused by access to local amenities, such as healthy food options and opportunity for physical activity, as well as interaction with neighbors.

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). We carefully spell out the identifying assumptions, discuss potential threats to identification and provide balancing tests supporting the identifying assumptions in this section. Then we present our empirical model in Section II. In Section III we describe the

[5]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.

data sources, sample selection and the definition of our main variables of interest. Following that, 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 Identification

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.[6] 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 size.[7] 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.[8] Information about ethnicity was used to create ethnic clusters at the municipality level, which was believed to ease integration.

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

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

[8] 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.

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.[9] Specifically, the DRC struggled to find enough affordable housing of a suitable size, considering the relatively low income levels of the newly arrived refugees.[10] This shortage is best illustrated by waiting times for permanent housing, which were six months, on average, but could be up to two years.[11] The effort needed to find permanent housing options is also illustrated by the DRC's need to employ special housing officers (distinct from 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 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

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

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

[11] 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).

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 will 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 I.C 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. 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.[12] Thus, it seems unlikely that the DRC systematically placed specific types of individuals in certain types of neighborhoods.

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

[12] Interview with Bente Bondebjerg on October 22, 2019.

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

C Balancing 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.[13] We use vocational education as the reference group for the education dummies.

Table 1 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'

[13] We refer to Section II for the definition of the neighborhood income groups.

health conditions in the neighborhood or the number of GPs per capita in the initial placement municipality.[14] All, but one, of the estimated coefficients are not statistically different from 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 1. 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.2).[15]

Based on the balancing tests and the arguments posed in Section I.B, 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. They show that larger families were more likely to be assigned to richer neighborhoods. This could be a result of larger families being assigned to cities, in which income was generally higher, and where it was easier to find bigger yet affordable apartments.

II Empirical Model

The main question posed in this paper is how living in a low-income neighborhood impacts health outcomes. To answer this question we divide all neighborhoods into three equally sized income groups based on their median disposable household income: Bottom-, middle- and top-income neighborhoods. We calculate these groups for each year in our sample and assign all neighborhoods to one of the three groups, regardless of whether the DRC found housing for any individual in a given neighborhood in a given year. This approach implies that a neighborhood's income group may vary across refugee cohorts.

We can use the natural experiment described in Section I for identification of causal neighborhood effects in a reduced form approach. We estimate the health effects of assignment to a neighborhood of a certain type using Ordinary Least Squares. Specifically, we estimate the

[14] Appendix Table A.1 shows that these tests also hold if we condition on municipality fixed effects. Note that 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).

[15] 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.

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).[16]

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

[16]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).

and fixed effects. As we argue in Section I.B, 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.

In addition, to be sure that the estimated long-term health effect is a result of neighborhood quality, and not due to differences in labor market opportunities, we must rule out effects on individual income. For example, if we observe that individuals who were initially placed in neighborhoods with higher median income have better health outcomes 15 years after immigration, and these individuals at the same time experienced higher income growth, we do not know whether to attribute the improved health outcomes to neighborhood quality or individual income changes. We test this empirically and provide evidence of the absence of any individual income effects in Section IV.A.

III 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 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"). 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,[17] Sri Lanka and Vietnam in 1986 to

[17] Stateless refugees.

1998, and Somalia 1989 to 1998.[18] Yugoslavia was also considered a refugee-sending country 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.[19] 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 2.

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.

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

[19] 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.

The differences in health outcomes typically arise 8-15 years after immigration (see Online Appendix Figure A.2). In these measures we include both primary and secondary diagnoses. The diagnoses follow the International Classification of Diseases (ICD) from World Health Organization, which contains a very fine level of detail.[20] First, we aggregate the diagnoses that we include in our analysis into two main groups: lifestyle related diseases and mental disorders. The lifestyle related diseases consist of circulatory diseases,[21] nutritional/endocrine/metabolic (referred to as nutritional) diseases,[22] 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.[23]

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.[24]

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

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

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

[22] Diabetes, obesity and elevated cholesterol levels.

[23] 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.

[24] 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.

(and 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.[25] The detection rate may depend on neighborhood income levels since correlational evidence suggests that inhabitants in low-income areas generally utilize health services to a lesser extent than their more affluent counterparts (see Panels g and h of Appendix Figure A.1 and 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.

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 Income Register which contains data on all income sources, including earnings, pensions payouts, transfers etc. 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

[25] 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.

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, recent 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. 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). We measure household disposable income as the household disposable income per adult household member to account for differences in household size. 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 and poverty rates, see Table 3. 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). 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 3, Table A.5 and Table A.6 for the summary statistics of neighborhood characteristics.

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.

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.2 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.[26] 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

[26]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.

third of neighborhoods relative to a middle- or top-income neighborhood, respectively.

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. We find similar results using average income instead of median neighborhood income. Furthermore, we show that the effects are not an artifact of the linear probability model; a probit regression yields the same qualitative effect. As a placebo test, we study some health outcomes that should not be affected by neighborhood income, namely congenital disorders. These tests reveal precise null-effects, confirming that the significant impact on lifestyle related diseases does not simply seem to arise by chance. These robustness checks and placebo tests can be found in Online Appendix Table A.3. 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.[27] 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.[28] Appendix Table A.4 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 ini-

[27] These results are available upon request.

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

tial 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 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)). However, there are multiple ways we could measure these elements. In column (2) 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 (3) 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 3, 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, such as the share employed and the number of immigrants in the neighborhood (columns (4)-(7)). 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 (8). Furthermore, the results are robust to controlling for urbanity of the neighborhood (column (9)). In column (10) 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. Finally, we include all the additional control variables measured at both the municipality level and at the neighborhood (parish) level simultaneously in column (11). 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.

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.

V Mechanisms Behind the Neighborhood Effects

Next, we investigate some of the potential explanations behind the documented neighborhood income gradient in health.[29] 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 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 measure.

A Individual Outcomes

We consider how initial neighborhood allocation affects the individuals' performance in the labor market 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. 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 7. This implies that the differences in health outcomes are not driven by differential labor market outcomes as a result of initial placement. We estimate very 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.[30] Similarly for earnings, we observe differences of less than a typical monthly salary in the cumulative income over 15 years across neighborhoods. This is consistent with the findings in Damm (2014) who documents that living in socially deprived neighborhoods

[29] See Sanbonmatsu et al. (2011) for an overview of potential channels.

[30] 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.

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. Thus, we can rule out any income effects of being placed in a bottom, medium or top income neighborhood.[31]

Education. We document a significant difference in educational outcomes across placement neighborhoods. Panel a of Table 8 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.[32] 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.

It cannot directly be inferred from Table 8 whether the increased educational level decreases the risk of developing lifestyle related diseases. More education might lead to higher employment probabilities and also higher wages, which in turn might affect health directly and indirectly. However, Table 7 shows that the increased educational level among individuals placed in richer neighborhoods does not translate into more employment or higher earnings, on average. Second, increased educational levels may increase knowledge about health related topics. However, Table 8 shows that the probability of completing a health specific education does not differ across neighborhoods. Third, even though earnings are not affected, higher educated individuals may be employed in jobs that are less detrimental to health, for example by finding employment in less physically demanding jobs. The last column in Table 7 shows that the occupations where the individuals are employed do not differ in task complexity across neighborhoods.[33] Fourth, more education can increase general knowledge and the ability to follow and understand general health guidelines and advice from health professionals and au-

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

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

[33]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.

thorities. Finally, obtaining an education could improve self-esteem or impact the formation of social networks, which in turn might improve general well-being and thus possibly health outcomes in the long term. Based on the timing of completion of education, the two latter explanations may be at play for the population we study. However, 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.

B Varying the Neighborhood Size

Taking one step further, we explore the mechanisms behind the results by varying the neighborhood size. Specifically, if the 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 9 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. This exercise shows that the income group of the assigned apartment building is more important for the risk of developing a lifestyle related disease than the income group of the parish or the municipality.

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 9. These time-invariant characteristics may capture the access to outdoor recreational areas, parks 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.[34] 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 9 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 or the poverty rate.

In summary, Table 9 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. We can, among other things, rule out both individual income effects and municipality level differences across neighborhoods as well as the presence of ethnic networks as important explanations. This may reflect that what matters most for the health outcomes

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

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 diseases such as diabetes.[35]

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 and mental disorders

[35]Note that our measure of lifestyle related diseases does not include asthma. However, air pollution or traffic noise may be indirectly linked to any disease caused by factors such as stress, happiness etc.

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. Furthermore, we show that exposure to the poorest neighborhoods is particularly harmful for women. On average, mental health is not affected by the neighborhood type.

Our study contributes to the understanding of neighborhood effects on health by examining a number of potential mechanism that have not been tested previously. While the neighborhood income gradient in 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. We find no evidence that the impacts on health outcomes are caused by differences in health care access, employment opportunities, or the size of the ethnic network. Remaining explanations for the observed income gradient include differences in other neighborhood amenities and the health behaviors of residents, and we provide evidence that what matters most for neighborhood effects on health is the very local neighborhood. 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.

Table 1: 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)
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.

Table 2: 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 and Ethiopia. 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 3: 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	16.78	13.44	9.11
Poverty Rate	0.10	0.07	0.05
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. 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 4: Main Results

	Lifestyle Related	Circulatory	Nutritional	Hypertension	Diabetes	Mental Disorder	Neurotic	Died
				(a) Diagnosed within 15 years after immigration				
Middle	-0.018**	-0.017**	-0.010	-0.011**	-0.008	0.003	0.004	-0.008
	(0.008)	(0.007)	(0.007)	(0.005)	(0.006)	(0.008)	(0.006)	(0.006)
Top	-0.019**	-0.014*	-0.007	-0.006	-0.006	0.012	0.009	-0.008
	(0.009)	(0.008)	(0.007)	(0.005)	(0.006)	(0.009)	(0.007)	(0.007)
Sample Mean	0.18	0.11	0.09	0.04	0.06	0.13	0.08	0.06
				(b) Diagnosed 8-15 years after immigration				
Middle	-0.019**	-0.012**	-0.012*	-0.010**	-0.007	-0.002	-0.002	-0.005
	(0.008)	(0.006)	(0.006)	(0.004)	(0.006)	(0.007)	(0.005)	(0.004)
Top	-0.018**	-0.009	-0.008	-0.004	-0.005	0.004	0.001	-0.004
	(0.009)	(0.007)	(0.007)	(0.005)	(0.006)	(0.008)	(0.006)	(0.004)
Sample Mean	0.15	0.09	0.08	0.04	0.06	0.10	0.06	0.03
N	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
Municipality FE	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 dying 2-15 years after immigration. In Panel (b) the dependent variable is a dummy for being diagnosed with the considered disease 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 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)
(a) Diagnosed within 15 years after immigration												
Middle	-0.018**	-0.017**	-0.018**	-0.019**	-0.023***	-0.019**	-0.020**	-0.018**	-0.018**	-0.015*	-0.013	-0.018*
	(0.008)	(0.008)	(0.008)	(0.008)	(0.009)	(0.009)	(0.009)	(0.009)	(0.008)	(0.008)	(0.009)	(0.010)
Top	-0.019**	-0.019***	-0.019**	-0.019**	-0.028***	-0.022**	-0.021**	-0.018*	-0.019**	-0.016*	-0.011	-0.021*
	(0.009)	(0.007)	(0.009)	(0.009)	(0.011)	(0.009)	(0.010)	(0.010)	(0.009)	(0.008)	(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)
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***	-0.019**	-0.020**	-0.022**	-0.019**	-0.021**	-0.018**	-0.019**	-0.016**	-0.013	-0.017*
	(0.008)	(0.007)	(0.008)	(0.008)	(0.009)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.009)	(0.009)
Top	-0.018**	-0.021***	-0.018**	-0.018**	-0.024**	-0.020**	-0.020**	-0.016*	-0.018**	-0.017**	-0.009	-0.016
	(0.009)	(0.007)	(0.009)	(0.009)	(0.010)	(0.009)	(0.009)	(0.009)	(0.009)	(0.008)	(0.010)	(0.011)
Log Share with			-0.021									-0.025
Lifestyle Related Diseases			(0.033)									(0.033)
Log Health				0.121*								0.104
Expenditure				(0.066)								(0.066)
Log Employment Rate					0.038							0.081
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					0.494**
							(0.166)					(0.244)
Immigrant Share							-0.382					-1.383**
Squared							(0.490)					(0.663)
Log Average								-0.015				0.008
Immigrant Household Income								(0.021)				(0.024)
Number of Sports									-0.000			-0.000
Facilities									(0.000)			(0.000)
Log Share Below											0.215*	0.329**
Poverty Line											(0.131)	(0.146)
N	21,965	21,965	21,965	21,963	21,965	21,965	21,965	21,923	21,965	21,965	21,965	21,921
Municipality Controls	Yes	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No	Yes	Yes
Parish Type FE	No	No	No	No	No	No	No	No	No	Yes	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 include the logarithm of the number of incidences (share of inhabitants above 18) of lifestyle related diseases in the assignment municipality. In (3) we include the logarithm of health and social expenditure per capita in the municipality. In column (4) we include the logarithm of the employment to population rate in the parish. In (5) we control for the number of refugees by including the number of inhabitants in the neighborhood originating from any of the refugee sending countries in our sample. In (6) we include the share of immigrants and the squared share of immigrants. In (7) we include the logarithm of average disposable household income among immigrants in the neighborhood. In (8) we include the number of sports facilities in the parish. In (9) 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 (10) we include the poverty rate in the neighborhood. In (11) 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 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 (3), and immigrant income cannot be calculated for a few parishes without immigrants prior to refugees' arrival in (7).

Table 6: Heterogeneous Effects by Gender

	Lifestyle Related	Circulatory	Nutritional	Hypertension	Diabetes	Mental Disorder	Neurotic	Died
				(a) Diagnosed within 15 years after immigration				
Middle	-0.015	-0.024***	-0.002	-0.008	-0.015**	0.003	0.002	-0.015*
	(0.010)	(0.008)	(0.008)	(0.006)	(0.007)	(0.010)	(0.007)	(0.008)
Top	-0.010	-0.016*	0.002	-0.000	-0.009	0.013	0.007	-0.011
	(0.010)	(0.009)	(0.008)	(0.006)	(0.008)	(0.010)	(0.008)	(0.008)
Middle ×	-0.010	0.016	-0.020	-0.008	0.017*	-0.000	0.005	0.019*
Female	(0.016)	(0.013)	(0.013)	(0.009)	(0.010)	(0.013)	(0.011)	(0.010)
Top × Female	-0.022	0.007	-0.024*	-0.015*	0.009	-0.002	0.004	0.006
	(0.015)	(0.012)	(0.012)	(0.008)	(0.010)	(0.012)	(0.010)	(0.009)
				(b) Diagnosed 8-15 years after immigration				
Middle	-0.008	-0.010	-0.002	-0.005	-0.013*	0.000	-0.004	-0.008
	(0.009)	(0.008)	(0.007)	(0.005)	(0.007)	(0.009)	(0.007)	(0.005)
Top	-0.007	-0.007	0.002	0.002	-0.008	0.007	0.001	-0.004
	(0.010)	(0.008)	(0.008)	(0.005)	(0.007)	(0.009)	(0.007)	(0.005)
Middle ×	-0.029*	-0.005	-0.025**	-0.012	0.014	-0.006	0.006	0.007
Female	(0.016)	(0.012)	(0.013)	(0.008)	(0.010)	(0.013)	(0.010)	(0.007)
Top × Female	-0.030**	-0.005	-0.026**	-0.015*	0.008	-0.008	0.002	-0.000
	(0.015)	(0.011)	(0.012)	(0.008)	(0.009)	(0.012)	(0.010)	(0.006)
N	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
Municipality FE	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 a dummy for being diagnosed with a lifestyle related disease or dying 2-15 years after immigration. In panel (b) the dependent variable is a dummy for being diagnosed with a lifestyle related disease 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: Labor Market Outcomes

	Employment>0	Employment	Labor Income	Business Income	Task Complexity
			(a) Cumulative within 15 years after immigration		
Middle	0.03	0.03	-176.24	-167.21	0.00
	(0.09)	(0.08)	(3,251.98)	(3,314.75)	(0.02)
Top	0.05	-0.01	-753.88	-1,467.38	-0.00
	(0.10)	(0.08)	(3,470.33)	(3,555.93)	(0.03)
Sample Mean	3.23	2.23	82,115.43	88,119.75	-0.01
			(b) Cumulative 8-15 years after immigration		
Middle	0.01	0.01	-633.90	-511.03	-0.01
	(0.07)	(0.06)	(2,497.76)	(2,548.45)	(0.03)
Top	0.02	-0.03	-1,501.43	-1,905.00	0.00
	(0.07)	(0.06)	(2,677.77)	(2,758.00)	(0.03)
Sample Mean	2.18	1.60	60,431.63	65,669.70	-0.02
N	21,965	21,965	21,965	21,965	10,217
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 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. 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 8: 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 9: 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)
Placed in Bottom	0.017***	0.016***	0.015**	0.014**	0.017***	0.017***	0.017***	0.017***	0.017***	0.016***	0.017***	0.015**	0.015**	0.014**
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)	(0.007)
Placed in Bottom		0.028*												
Income Municipality		(0.015)												
Placed in Bottom		0.009												
Income Parish		(0.009)												
Log Share with					0.016									0.021
Lifestyle Related Diseases					(0.043)									(0.045)
Log Health						0.097								0.085
Expenditure						(0.078)								(0.082)
Log Employment Rate							-0.001							0.268*
in Parish							(0.035)							(0.146)
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					0.752
									(0.165)					(0.726)
Immigrant Share									-0.117					-1.649
Squared									(0.429)					(1.320)
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)
Log Share Below													0.275**	0.158
Poverty Line													(0.117)	(0.304)
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	17,875
Municipality Controls	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No	Yes	No
Parish Type FE	No	No	No	No	No	No	No	No	No	No	No	Yes	No	No
Parish FE	No	No	Yes	No	No	No	No	No	No	No	No	No	No	Yes

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 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 the remaining columns are described in Table 5. 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

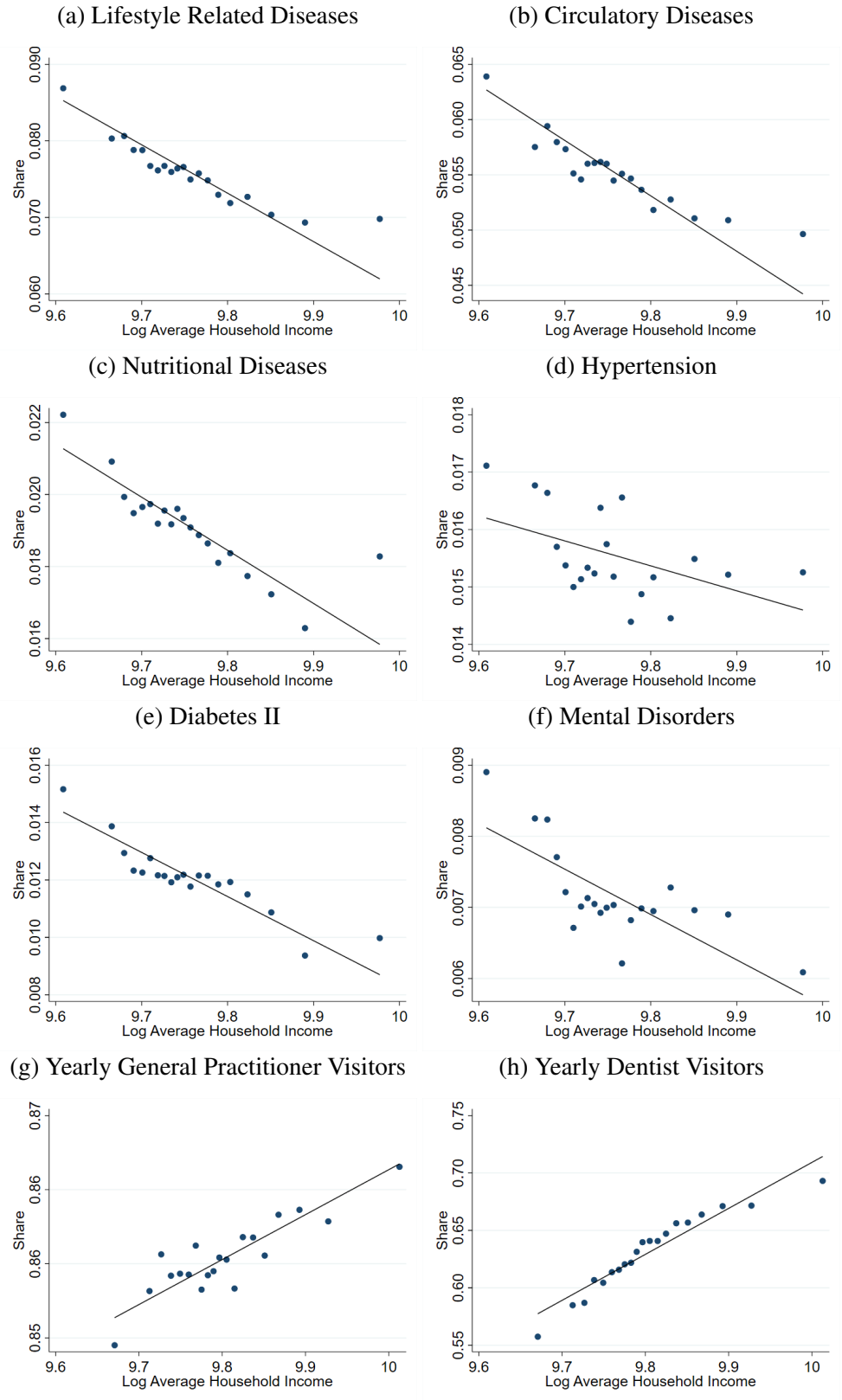


Figure A.1: Association Between Health and Neighborhood Income

Notes: The figures illustrate the association between health, health behaviors and income between parishes. Panels (a)-(f) plot the average share in a parish diagnosed with the disease in question against the parish median disposable income, averaged over 1991-2017. Panels (g)-(h) plot the average share of inhabitants in a parish that visited their GP or dentist, respectively, against the parish median disposable income, averaged over 1991-2017. These unconditional correlations do not account for any selection or differences in inhabitant composition such as age or gender across parishes. The data are administrative data provided by Statistics Denmark from 1991-2017 for the full Danish population above 18 years of age.

Table A.1: 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)
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

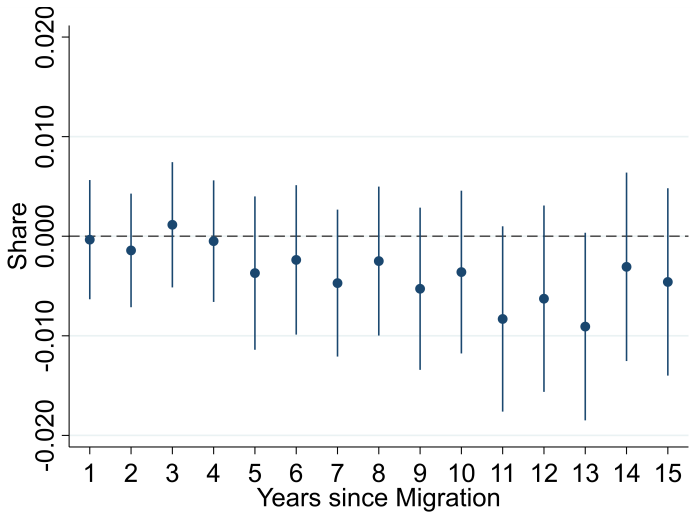
Notes: Balancing tests for parishes using linear regressions. Robust standard errors in parentheses clustered at the household level. $^{**}p < 0.05, ^{***}p < 0.01$. F denotes the F-statistic for joint insignificance of the educational attainment dummies and pre-existing health conditions. Vocational 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.

Table A.2: 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)
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.

(a) Yearly Incidents



(b) Cumulative

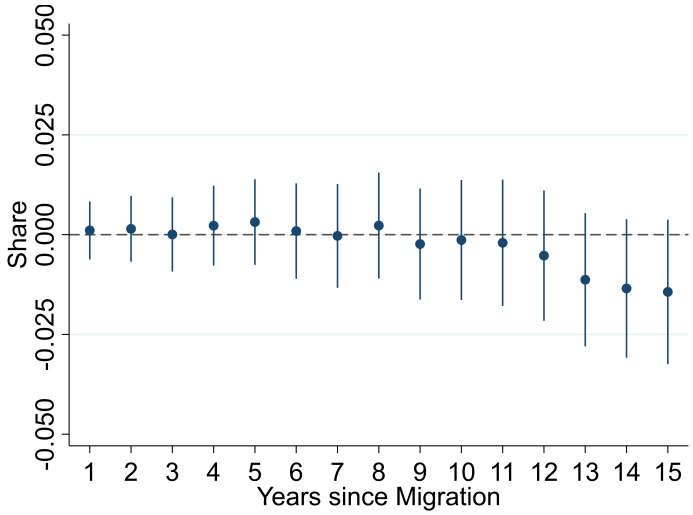


Figure A.2: Development of Lifestyle Related Diagnoses

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.3: Robustness Checks and Placebo Tests

	Panel A: Robustness of Lifestyle Related Diseases			Panel B: Placebo Test of Congenital Disorders	
	Baseline	(1)	(2)	Abnormalities	Metabolic
	(a) Diagnosed within 15 years after immigration				
Middle	-0.018**	-0.019**	-0.017**	0.003	-0.006
	(0.008)	(0.009)	(0.008)	(0.004)	(0.005)
Top	-0.019**	-0.014	-0.020**	0.001	-0.003
	(0.009)	(0.009)	(0.009)	(0.005)	(0.005)
	(b) Diagnosed 8-15 years after immigration				
Middle	-0.019**	-0.024***	-0.018**	-0.002	-0.002
	(0.008)	(0.008)	(0.008)	(0.003)	(0.003)
Top	-0.018**	-0.019**	-0.019**	-0.002	0.002
	(0.009)	(0.009)	(0.009)	(0.003)	(0.003)
N	21,965	21,965	21,757	21,965	21,965
Municipality Controls	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes
Income Type	Disposable	Disposable	Disposable	Disposable	Disposable
Moment	Median	Mean	Median	Median	Median
Method	OLS	OLS	Probit	OLS	OLS

Notes: Robust standard errors in parentheses clustered at parish $\times$ 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) 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. Column (2) shows the estimated neighborhood effects from a probit model. 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.4: 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 ever moving to different types of neighborhoods within 15 years after immigration (columns (1)-(3)), staying in the initial neighborhood for all 15 years (column (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 condition on municipality of assignment fixed effects. The sample mean denotes the mean of the outcome variable.

Table A.5: 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	1.26	0.90	0.76
Poverty Rate	0.11	0.10	0.09
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. 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.6: Summary Statistics for Initial Placement (Municipality)

	Bottom Mean	Middle Mean	Top Mean
Characteristics of Residents			
Age	47.90	47.51	45.97
Median Household Income	14,619.07	14,714.44	15,944.96
Employment Rate	0.67	0.69	0.73
Inhabitants	26,706.01	21,611.65	24,329.57
Co-nationals	49.48	35.06	30.98
Poverty Rate	0.08	0.07	0.06
Parish Type			
Urban Area (Near City)	0.16	0.27	0.60
Urban Area (Away from City)	0.10	0.29	0.23
Rural Area (Near City)	0.16	0.14	0.11
Rural Area (Away from City)	0.58	0.30	0.06
Characteristics of Municipality			
General Practioners per 1,000 Inhabitants	0.38	0.37	0.41
Incidences of Lifestyle Related Diseases per 1,000 Inhabitants	32.17	29.21	24.72
Health and Social Expenditure per Capita	3,643.20	3,645.20	3,575.85
N	183	521	1,002

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. 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 types are defined by Ministeriet for By, Bolig og Landdistrikter (2013). All other data are from administrative registers provided by Statistics Denmark.

B Diagnoses with ICD Codes

The first parentheses indicate (ICD-10) diagnoses codes from 1994 and onwards and 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)