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Sick of Your Poor Neighborhood?*

Quasi-Experimental Evidence on Neighborhood Effects on Health

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Does living in a low-income neighborhood have negative health consequences? We document causal neighborhood effects on health by exploiting a Spatial Dispersal Policy that quasi-randomly resettled refugees across neighborhoods from 1986-1998. The risk of developing a lifestyle related disease before 2018 increased by 5.1 percent for those allocated to the poorest third of neighborhoods compared with those in the richest third of 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 prematurely.[1] 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.[2] 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.

[1] More than a third of deaths caused by lifestyle related diseases, such as cardiovascular diseases, diabetes, some cancers and chronic respiratory diseases occur between ages 30-69, see WHO (2018).

[2] See, for example, Chetty et al. (2016) who document the association between income and life expectancy in the United States. See also Panels a-f in Figure A.1 in the Appendix, which show a negative correlation between median local area (parish) income and the share of inhabitants diagnosed with a number of different lifestyle related diseases in Denmark.

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.[3] Moreover, to the best of our knowledge, we are the first to explore the potential mechanisms behind neighborhood effects on health. 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 different neighborhoods upon arrival to Denmark.[4] The neighborhoods in our analysis are parishes, which historically have delineated small communities and, in recent years, have been home to around 3,000 inhabitants. We divide all neighborhoods into three equally sized groups based on the median household income in the neighborhood one year prior to the refugees' arrival. Our results show that refugees placed in low-income neighborhoods experience significantly worse health outcomes in the following years.

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 4.6 and 5.1 percent relative to assignment to middle- or top-income neighborhoods, respectively. On average, we find no significant impact on mental health outcomes. Using an instrumental variables strategy, we show that for each year spent in the poorest third of neighborhoods, the risk of developing a lifestyle related disease increases by 0.5 percentage points.[5] This is primarily due to an increased risk of developing hypertensive

[3]There exists only limited evidence on neighborhood effects on health. See Ludwig et al. (2011) and Ludwig et al. (2012) for evidence on body mass index, elevated blood sugar levels and subjective well-being.

[4]A number of existing papers study this natural experiment. See Damm (2009), Damm and Dustmann (2014), Foged and Peri (2015) and Dustmann, Vasiljeva, and Damm (2018) among others.

[5]We instrument the number of years spent in the poorest third of neighborhoods with a dummy for placement in the poorest third of neighborhoods.

diseases along with endocrine and nutritional diseases, such as diabetes and obesity. 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 transmission of behavior (e.g., health habits), its local amenities (e.g., recreational areas or grocery store options), labor market opportunities, or through the local institutions, such as health care access.[6] All these factors could potentially affect lifestyle choices and thus the development of lifestyle related diseases.[7] Since some of these factors may also affect mental health, we include mental health diagnoses in our analysis.

Interestingly, 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, Liebman, and Katz (2007) or Oreopoulos (2003) among others).[8] Therefore, we can rule out any income effects of neighborhood placement, and this allows us to attribute the estimated health effects to neighborhood income rather than

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

[7]See Patienthåndbogen (2017).

[8]Damm (2014) documents that refugees located in socially deprived areas do not experience worse labor market outcomes than those placed elsewhere. Similarly, the randomized controlled trial *"Moving To Opportunity"* literature does not suggest any long term effects on labor market attachment, economic self-sufficiency or income levels, see Sanbonmatsu et al. (2011) and Kling, Liebman, and Katz (2007).

to individual income.[9]

Next, we show that controlling for a number of neighborhood characteristics and resident composition does not affect the income gradient. 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. Including additional controls for health care access in the municipality also leaves the income gradient in health unaffected. Furthermore, controlling for institutional differences between municipalities, differences between rural and urban parishes as well as the presence of ethnic networks and sports facilities does not affect the income gradient. Thus, these mechanisms do not appear to be important determinants of neighborhood health. Our findings indicate that the share of residents suffering from a lifestyle related disease can explain part of the variation in the newcomers' health outcomes across neighborhood income groups.

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 varying the level of a neighborhood using both a more aggregated level (municipalities) and a more disaggregated level (households living in the same apartment complex), which changes how well we capture potential peer groups and the character of the immediate neighborhood. When we compare the resulting income gradients from these estimations, we find that the closer we get to immediate neighbors (and the very local geographical area in which the refugees live), the larger the estimated coefficients

[9]We show that in richer neighborhoods, more refugees obtain a vocational education, but there is no significant difference in the share obtaining a health-related education across neighborhoods. In addition, there are no differences in the task complexity of occupations, conditional on employment. Moreover, previous evidence shows that there is no causal impact of education on health outcomes in Sweden or Denmark (Meghir, Palme, and Simeonova (2018) and Behrman et al. (2011)).

become. This suggests that transmission of behaviors from neighbors and local amenities are part of the mechanisms through which neighborhoods affect residents' health. We find further support for this explanation by comparing individuals placed in the poorest third of apartment buildings to individuals in richer apartment buildings within the same parish, thus controlling for time-invariant parish characteristics, such as access to recreational areas.[10] This nearly halves the neighborhood income gradient in health. Thus, parish amenities appear to be important for health outcomes, but they cannot fully explain the differences in health outcomes across apartment buildings.

We base our analysis on administrative registers covering 31 years, 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 Appendix.[11] 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, Kling, and Liebman (2001), Kling, Liebman, and Katz (2007) or Chetty, Hendren, and Katz (2016). However, because of data limitations, the *Moving to*

[10]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.

[11]See Bago d'Uva and Jones (2009) for evidence on the association between health care utilization and income.

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, Liebman, and Katz (2007)).[12]

The literature also includes non-experimental evidence on neighborhood effects on health, for example in mental health among social housing clients, in life expectancy among the elderly, and in diabetes among refugees (see Boje-Kovacs, Greve, and Weatherall (2018), Finkelstein, Gentzkow, and Williams (2019), and White et al. (2016), respectively).[13]

We contribute to the literature on neighborhood effects on health 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 since such evidence on neighborhood effects on health is scarce. Furthermore, since previous studies do not provide knowledge on the mechanisms, the second part of our contribution is to push forward the understanding of these neighborhood effects by ruling out a number of likely mechanisms and pointing to the importance of the nature of very local environments.

Because of this finding, our paper also relates to the literature on spillovers

[12]Ludwig et al. (2012) also document non-significant improvements in two indices of mental and physical health. In Ludwig et al. (2011) elevated blood sugar level is included as an indication of untreated diabetes.

[13]Boje-Kovacs, Greve, and Weatherall (2018) study the impact on mental health of living in a socially deprived neighborhood for vulnerable residents in the capital of Denmark. They find an impact on mental health based on purchases of psychotropics (anti-depressants, anti-psychotics etc.). Finkelstein, Gentzkow, and Williams (2019) show that moving to a neighborhood with higher life expectancy increases the newcomer's life expectancy among Medicare recipients in the US by comparing movers from the same origin. White et al. (2016) show that neighborhood deprivation increases the risk of developing diabetes using a Swedish refugee dispersal policy similar to the one we use for identification.

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.

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, describing a reduced form approach and an instrumental variables strategy. In Section III we describe the 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 test 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. 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.[14] 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.[15] Information about ethnicity was used to create ethnic clusters at the municipality level, which was believed to ease integration.

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

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

[15]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.

neighborhood income.

The practical implementation of the Spatial Dispersal Policy was influenced by a simultaneous housing shortage.[16] Specifically, the DRC struggled to find enough affordable housing of a suitable size, considering the relatively low income levels of the newly arrived refugees.[17] This shortage is best illustrated by waiting times for permanent housing, which were six months, on average, but could be up to two years.[18] 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 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

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

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

[18] 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).

(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 (apartment buildings) and municipalities.

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 municipality – independent of any other potential differences between them. This conditionally random allocation of individuals between municipalities is important even when we let parishes define a neighborhood, because it allows us to compare health outcomes of individuals assigned to parishes in different municipalities.

In a similar way, we assume that once allocated to a municipality, two individuals with the same questionnaire observables had the same probability of finding housing in a low-, middle- or top-income parish independent of any

other (un)observable characteristics. 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 and municipalities.

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.[19] 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 af-

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

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

Finally, one could worry that the individuals somehow managed to self-select into specific types of neighborhoods. We do not directly observe the actual housing offers made to the refugees but only their first address. It is therefore crucial for our identification strategy that the acceptance rate of housing offers was high. In the previously mentioned interview with the former housing officer, she could not recall that refugees declined a housing offer. The explanation for this is threefold. First, the person only received one housing offer, and if the individual declined that offer, he/she had to move out of the temporary accommodation. This means that there was no bargaining over housing offers and that the cost of declining the offer was high. Second, following the acceptance of a housing offer, the refugee was free to move whenever he/she wanted to. Finally, the difficulty of finding affordable housing was probably even greater for refugees themselves, since they would mostly be without network connections and lack knowledge of the Danish housing market in general. Damm (2009) shows that the take up rate was above 90 percent, which is remarkably high compared to the *Moving to Opportunity* experiment in which the acceptance rate was between 48 and 62 percent.[20]

[20]See Katz, Kling, and Liebman (2001) for statistics on the take up rates in the *Moving to Opportunity* experiment.

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:

(1)

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

The neighborhood characteristics, $y_{n,t-1}$, are indicator variables for the poorest, middle or richest third of neighborhoods, and the share of residents suffering from a lifestyle related disease. 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 FE.[21]

Table 3 presents the results from these balancing tests. They show that refugees' educational attainments acquired prior to immigration have no significant prediction power of the neighborhood income level or neighbors' health

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

conditions in the initial placement neighborhood.[22] If we use health diagnoses in the year of arrival as proxies for refugees' initial health conditions, we find no significant association between initial health and neighborhood income level or neighborhood health.[23] None of the estimated coefficients are statistically different from zero at conventional significance levels, and an F-test of joint insignificance of the education and initial health variables cannot reject that they are jointly equal to zero, see Table 3. Furthermore, we find no evidence of selection on health and education across apartment buildings or municipalities using similar regression tests.[24]

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 and women 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.

[22]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).

[23]Unfortunately, we have no ex ante data on refugees' health. However, we do not expect neighborhood quality to have an immediate impact on health. Instead, we expect lifestyle related diseases to build up gradually over time. Thus, any difference in the risk of suffering from a lifestyle related disease must be attributed to pre-existing health conditions. One drawback of this measure is that the detection risk may depend on neighborhood of assignment. One could worry that the detection risk is lower in the low-income neighborhoods.

[24]See Appendix Tables A.1 and A.2. Note that one coefficient is significant at the 5 percent level for the association between municipality level median income and refugees reporting that their highest completed education was basic schooling. This may reflect an imbalance in how refugees' educational attainment was surveyed across municipalities (the survey took place at Danish language training facilities), or it may simply arise by chance, because we are testing multiple hypotheses.

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.

We can use the natural experiment described in Section I for identification of causal neighborhood effects in both a reduced form approach and in an instrumental variables (IV) setup. In the reduced form approach we simply estimate the health effects of assignment to a neighborhood of a certain type using OLS. In the IV setup we use the exact same conditionally random variation in assignment neighborhood to instrument the number of years the individual spent in the poorest neighborhoods using 2SLS.[25] This allows us to estimate the average impact of spending one additional year in a low-income neighborhood.

Reduced form strategy. Specifically, in the reduced form setup we estimate the impact on an individual's health outcome $y_{i,t+r}$:

$$(2) \quad y_{i,t+r} = \alpha + \sum_{k=2}^{3} \beta_k \cdot \mathbb{1}[incomegroup_{n,t-1} = k] + \boldsymbol{X_{it}\gamma} + \boldsymbol{T_t} + \varepsilon_{i,t+r}.$$

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

[25] This is similar in spirit to Angrist and Krueger (1991) who use quarter of birth as an instrument for years of schooling, or Acemoglu and Angrist (2000) who instrument years of schooling using state of birth.

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.

The coefficients β_k denote the increased risk of diagnosis y if assigned to a middle- or top-income neighborhood relative to being assigned to the poorest neighborhoods. Thus, a negative estimate of β_2 and β_3 means that the risk of being diagnosed with y is lower in a top- and middle-income neighborhood than in a low-income neighborhood. The parameters identify the causal impact of being assigned to a certain type of neighborhood if the allocation of individual i to neighborhood n is random, conditional on the set of included individual characteristics and fixed effects. As we argue in Section 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 income level, we must rule out individual income effects. For example, if we observe that individuals who were initially placed in neighborhoods with higher income have better health outcomes 19 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 or individual income changes. We test this and provide evidence of the absence of any individual income effects in Section IV.A.

Instrumental variables strategy. We can also use the natural experiment to quantify the health impact of spending one additional year in the poorest neighborhoods. The Spatial Dispersal Policy did not prevent individuals from moving once allocated to a neighborhood, and the decision to relocate most likely de-

pends on individual (unobserved) characteristics along with the amenities of neighborhoods. Therefore, we instrument the number of years spent in the poorest neighborhoods with assignment neighborhood type. This means that we exploit the variation in the years of exposure to a neighborhood type which is induced by the initial allocation. This approach yields an estimate which is a weighted average of a series of local average treatment effects (LATE) of one additional year spent in the poorest third of neighborhoods.[26]

A discussion of the assumptions behind our IV strategy is warranted. The Spatial Dispersal Policy provides us with quasi-random variation in initial neighborhoods, conditional on observables, such that the independence assumption is satisfied.[27] Moreover, the initial placement only affected health outcomes through the number of years an individual lived in a specific type of neighborhood, which implies that we can comfortably assume that the exclusion restriction holds.[28] Finally, the income group of the placement neighborhood is a relevant instrument if there is persistence in the type of neighborhood the individual lives in over time. In other words, if the number of years the individual is exposed to a bottom income neighborhood depends on the placement neighborhood income type, our instrument is relevant and has prediction power in the first stage regression.[29] Lastly, we assume monotonicity, i.e., that being placed in a bottom income neighborhood always increases years of exposure to bottom

[26]We refer to Angrist and Pischke (2008) for the interpretation of LATE in the case of a multivalued endogenous regressor.

[27]This is discussed in detail in Section I.B.

[28]As already discussed, in Section IV.A we show that the initial allocation of individuals did not impact their labor market outcomes. Table 7 shows very precise null-effects on employment and earnings.

[29]The instrumental variables estimation approach also deals with the situation in which the individual does not move but the neighborhood changes its rank in the income distribution over time, for example, if an individual is initially placed in a bottom income neighborhood and that neighborhood evolves into a middle income neighborhood over time. We document that the income rank of neighborhoods is highly stable over time (see Appendix Figure A.3).

income neighborhoods.

These assumptions allow us to scale the neighborhood effects on health by the number of years spent in the poorest third of neighborhoods. We implement the strategy by estimating the following equations with 2SLS:

(3)

$$First\ stage: \quad x_{i,t+r} = \alpha_1 + \tilde{\beta} \cdot bottomincomegroup_{n,t-1} + \boldsymbol{X_{it}\gamma_1} + \boldsymbol{T_t} + \tilde{\varepsilon}_{i,t+r}$$

$$Second\ stage: \quad y_{i,t+r} = \alpha_1 + \beta \cdot \hat{x}_{i,t+r} + \boldsymbol{X_{it}\gamma_1} + \boldsymbol{T_t} + \varepsilon_{i,t+r}.$$

The predicted number of years an individual has spent r years after immigration t in a bottom income neighborhood is denoted by $\hat{x}_{i,t+r}$, and the controls $\boldsymbol{X_{it}}$ and $\boldsymbol{T_t}$ are the same as in equation (2). Thus, β denotes the increased risk of being diagnosed with y following one additional year of exposure to the poorest third of neighborhoods.

III Data

Our analysis is based on rich administrative data from Statistics Denmark 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") and 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,[30] Sri Lanka and Vietnam in 1986 to 1998, and Somalia 1989 to 1998.[31] We exclude individuals who were married to a Dane or a non-refugee immigrant spouse, along with refugees married to a refugee spouse arriving more than a year earlier. 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 25,738 refugees whose average age at arrival is 30 years. 40 percent of them are female while more than half are married (61 percent). The average family size is 2.4, since many arrive with children, and the two largest ethnic groups in our sample are Iraqi and Lebanese nationals, followed by people from Somalia and Iran. Upon arrival 30 percent of the sample were surveyed by a statistical agency about their educational level obtained abroad.[32] Of those, 56 percent report basic schooling or less, 21 percent have vocational education, while 23 percent arrive with a higher education, c.f. Table 1.

Our main outcomes in the empirical analysis are diagnoses from hospitals based on the National Patient Registry, which contains information about all

[30] Stateless refugees.

[31] See Dustmann, Vasiljeva, and Damm (2018), Foged and Peri (2015), Damm and Dustmann (2014) or Damm (2009) among others for a similar approach. 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.

[32] The information was used for the purposes of national statistics in an anonymized format, and it was not collected by the DRC.

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. The diagnoses follow the International Classification of Diseases (ICD) from World Health Organization, which contains a very fine level of detail.[33] 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,[34] nutritional/endocrine/metabolic (referred to as nutritional) diseases,[35] 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 and neurotic disorders.[36]

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

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

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

[35]Diabetes, obesity and elevated cholesterol levels.

[36]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 Appendix Section A for a full overview of the grouping of diagnoses.

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.[37]

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.[38] 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. 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.[39] This may bias our estimates towards zero.[40]

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 Labor Market Research Register and the Income Register. Using this 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

[37] 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.

[38] Even though patients can be diagnosed with multiple (and less severe) conditions when visiting the hospital.

[39] See Panels g and h of Figure A.1 and Bago d'Uva and Jones (2009).

[40] Under-detection of illness could also simply show up as random measurement error. This will affect precision, but will not create a bias.

the individual was either a part-time employee or not employed in some periods throughout the year. As a measure of 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.[41] 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.[42] 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. The individuals in our sample were assigned to 1,055 different parishes, which had, on average, 3,126 inhabitants during the study period. We study the importance of small local areas by varying the neighborhood level using a more aggregate level (municipality) and a very fine level considering households living in the same building (apartment building level). A parish is a subset of a municipality, whereas an apartment building is a subset of a parish. During the period of the study, refugees in our sample were distributed across 255 different municipalities and 9,405 different apartment build-

[41] See Kleven et al. (2011) and Alstadsæter, Johannesen, and Zucman (2019).

[42] The task content is from the O*NET database (US Bureau of Labor Statistics) merged to Danish register data using the International Standard Classification of Occupation.

ings. Disregarding the refugees, the municipalities had an average of 15,424 inhabitants, whereas an apartment building only had 12 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.[43] The neighborhood income characteristics are supplemented with additional neighborhood variables, such as the number of general practitioners per capita, the number of co-nationals, urban/rural area, health care utilization and incidences of lifestyle related diseases and mental disorders among the non-refugee residents. 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 based on firms' industry codes reported in the Integrated Database for Labor Market Research. We refer to Table 2, Table A.11 and Table A.12 for the summary statistics of neighborhood characteristics.

IV Main Results

In this section we present our main findings on neighborhood effects on health. We start by presenting the neighborhood effects from the reduced form approach, including evidence showing that these effects differ across gender. We then proceed to present evidence on the health impacts of spending an additional year in a low-income neighborhood using an IV strategy.

A Reduced Form Approach

Allocation to the poorest third of neighborhoods increases the risk of developing a lifestyle related disease before 2018 by 1.8 percentage points relative to allocation to the richest third of neighborhoods. The risk of developing a lifestyle related disease is 1.6 percentage points higher if the individual was allocated to

[43] Deflated by the consumer price index (2000 level).

the poorest third of neighborhoods compared to allocation to a middle-income neighborhood, see Panel a of Table 4. This amounts to a 5.1 and 4.6 percent increase in risk relative to the sample mean, respectively. These effects are driven by increases in the risk of developing diabetes and hypertensive diseases. Diabetes and hypertensive diseases are subgroups of nutritional and 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.

Figure 1 shows that the effect 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 (the mean is 30 years old) and the risk of developing lifestyle related diseases generally increases with age. Most of the effects on health arise 8-15 years after immigration, which is why we focus on this time horizon in Panel b of Table 4. This shows that the risk of developing a lifestyle related disease increases by 1.5 and 1.8 percentage points following allocation to the poorest 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 middle- or top-income neighborhoods, but the difference is small in magnitude and not statistically significant at a 5 percent level, see Appendix Table A.4.[44]

Our findings in Table 4 are very robust to the choices made in the base-

[44]In addition, there are no significant differences in outmigration rates across neighborhoods in years 1 to 19, 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.

line specification. We find similar results using mean income instead of median neighborhood income. Using a continuous income measure instead of income group dummies shows that a one standard deviation decrease in median neighborhood income causes an increase in the risk of suffering from a lifestyle related disease of 0.008 percentage points. Finally, 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. The full set of robustness checks and placebo tests can be found in Appendix Table A.3.

a Heterogeneous Effects

The impact on health of placement neighborhood income type varies significantly by gender. Table 5 shows that females experience a larger increase in the risk of developing lifestyle related diseases and nutritional disorders compared to males if they are placed in the poorest third of neighborhoods as opposed to placement in a middle- or top-income neighborhood. In other words, female health is more adversely affected by living in the poorest neighborhoods. More than 19 years after immigration, women placed in the poorest neighborhoods have a 1.7 percentage point higher risk of developing a nutritional disease than men placed in similar neighborhoods, relative to placement in the richest third of neighborhoods.[45] Since we do not observe any differential impacts on diabetes for women, this difference is primarily driven by obesity, which is the other large component of the nutritional diagnoses. In our sample, a larger share of women than men are diagnosed with nutritional or lifestyle related diseases be-

[45]The estimate is not significant over the full time period, but statistically significant 8-15 years after immigration.

fore 2018, 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 to men.

B Instrumental Variables Approach

In this subsection we turn to the results from the IV approach. First, we learn from Appendix Figure A.2 that there is substantial persistence in the type of neighborhoods that people live in. After 19 years, those placed in the poorest third of neighborhoods have spent almost 10 years, on average, in that type of neighborhood (Panel a).[46] The behavior for those placed in a middle- or top-income neighborhood is similar, although slightly less persistent (Panels b to c). Furthermore, the graphs reveal that the individuals placed in the poorest neighborhoods have spent significantly more time in a bottom income neighborhood than those placed in a middle or top income neighborhood.[47] This implies that we have a relevant instrument and a very strong first stage (see Table 6).

When we instrument total exposure to the poorest third of neighborhoods, we find that each additional year spent in the lowest income neighborhoods increases the risk of suffering from a lifestyle related disease by 0.5 of a percentage point. The effects are mainly driven by the occurrence of diabetes and hypertension, see Table 6.[48] The findings are qualitatively similar if we instead instrument average income in all neighborhoods that the individual lived in (See Appendix Table A.7 and Appendix Section C for a description of this approach). It is important to instrument the number of years an individual has

[46] After 19 years, 40 percent live in their initial assignment neighborhood.

[47] Appendix Figure A.3 shows that there is substantial persistence in the ranking of neighborhoods in the income distribution.

[48] See Appendix Table A.4 for the dynamics of diagnosed lifestyle related diseases.

spent in the poorest neighborhoods, because there is a significant self-selection of less healthy individuals into poorer neighborhoods after the initial allocation. Appendix Table A.8 shows that the income gradient in health is larger if endogenous moving is not taken into account.

The results from the IV approach must be interpreted with more cautiousness than the reduced form results, since the former are subject to more assumptions. Nevertheless, both set of results point towards significant negative health consequences of living in the poorest third of neighborhoods.

V Mechanisms Behind the Neighborhood Effects

Next, we investigate some of the potential explanations behind the documented neighborhood income gradient in health using the reduced form setup. First, we explore how allocation to a given type of neighborhood affects different individual outcomes that in turn might affect their health outcomes. Second, we investigate how the observed income gradient in health depends on the characteristics of the neighborhoods and the composition of residents. Each refugee outcome considered in the first approach and each control variable included in the second approach tests a different potential explanation, and they capture some of the most obvious, yet measurable, ways in which neighborhoods may affect residents' health outcomes. Taken together, all these tests provide no evidence in support of a number of plausible explanations behind the negative neighborhood income gradient in health. Finally, 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 more than 19 years in Denmark the cumulative difference in the number of years with any employment is between 0.01 and 0.10 years across the different types of neighborhoods, and it is not statistically significant.[49] Similarly for earnings, we observe differences of less than a typical monthly salary in the cumulative income over 19 years across neighborhoods. This is consistent with the findings in Damm (2014) who documents that living in socially deprived neighborhoods does not impact the labor market outcomes of refugee men. It is also in line with evidence from the *Moving to Opportunity* experiment. See for example Katz, Kling, and Liebman (2001), Kling, Liebman, and Katz (2007), Sanbonmatsu et al. (2011) or Ludwig et al. (2012) who find no effects on em-

[49] 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 4.17 years.

ployment, earnings or welfare receipt probability. Thus, we can rule out any income effects of being placed in a bottom, medium or top income neighborhood.

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 2.4 and 1.5 percentage points, respectively, compared with those placed in the poorest third of neighborhoods.[50] 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. Column (5) in Table 7 shows that the occupations where

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

the individuals are employed do not differ in task complexity across neighborhoods.[51] Fourth, more education can increase general knowledge and the ability to follow and understand general health guidelines and advice from health professionals and authorities. 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, Palme, and Simeonova (2018)) and twin studies in Denmark (Behrman et al. (2011)) document that there is no causal impact of education on health in these countries.

B Neighborhood Characteristics and Residents

Neighborhood characteristics. Turning to the characteristics of the neighborhood, we show that the income gradient in health is not driven by differences between urban and rural areas or local institutions at the municipality level (see Table 9). The Danish health care system is universal and provided free of charge to all residents, including refugees. This makes it unlikely that the differential health outcomes are driven by differences in access to health care. Moreover, all residents have access to medical treatment of virtually the same quality. However, there might be minor differences in health care access and quality across geographical areas. Residents in rural areas may have restricted access to the health care system because they generally travel longer distances to visit their GP or local hospital. The characteristics of the neighborhood can also differ

[51] 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.

systematically between rural and urban areas in terms of education possibilities, leisure activities, air pollution etc. However, we find no evidence that such differences between rural and urban areas explain the income gradient.

By including municipality fixed effects we further control for such differences between areas. Comparing neighborhoods within the same municipality allows us to compare neighborhoods that are subject to the same local authorities. Even though hospitals and the overall health policy was run by the counties throughout the period, municipalities could still affect access to health care in areas such as rehabilitation offers or health preventive actions. The local authorities might also differ in their tax rates and service levels (such as spending per pupil, policemen/inhabitant ratio or cultural investments). Moreover, characteristics of health care professionals may also differ between municipalities but less so within municipalities.[52] We find no evidence that the income gradient can be attributed to differences across municipalities even though our estimates become less precise when including municipality fixed effects, due to the lack of statistical power.

As an alternative to including municipality fixed effects, we include the number of general practitioners per inhabitant in the municipality as a control variable, which supports the conclusion that differences in access to health care does not explain the income gradient.[53]

Furthermore, we study whether differences in the availability of local sports clubs and sports facilities can explain the differences in health outcomes across neighborhoods. Table 9 shows that the income gradient in health is not affected

[52]Especially in large municipalities they might also differ within municipalities.

[53]The conclusion remains if we control for the number of general practitioners per capita along with municipality expenditure on social and health services, see Appendix Table A.5. However, municipality expenditure on health services may reflect both the quality of health services and the health conditions of inhabitants.

by controlling for the number of sports clubs and facilities in the neighborhood.

Neighborhood residents. As previously discussed, it is likely that health behaviors are transmitted to the resettled refugees from their peer groups in the neighborhood. This implies that the neighborhood income gradient in health is possibly explained by the characteristics of neighborhood peers. To explore this potential explanation we control for the characteristics of the residents of the different neighborhoods and analyze how these controls affect our baseline results.

First, we include the number of co-nationals as a control variable in Column (4) of Table 9. In this case, the neighborhood income gradient in health is almost unchanged, which suggests that the presence of ethnic networks is not an important factor behind the results.[54]

Another relevant peer group for refugees may be inhabitants with the lowest income levels, since the refugees themselves have very low income levels. Therefore, we include the poverty rate in the neighborhood as a control variable in Column (5) of Table 9, but it does not have much explanatory power with regards to the income gradient from the baseline results, nor does the poverty rate in itself significantly impact the risk of developing a lifestyle related disease.

Finally, we investigate how the share of inhabitants diagnosed with a lifestyle related disease impacts the income gradient in health. Unfortunately, we do not have a reliable measure of neighbors' health status at the parish level upon the

[54]Ideally, we want to measure the income levels among co-nationals, but this is not feasible because the number of co-nationals prior to immigration is very low in a number of neighborhoods. As an alternative to including the number of co-nationals, we use the number of individuals from refugee-sending countries and the share of all immigrants in the neighborhood in Appendix Table A.5. We also include the average income among immigrants in the neighborhood as a control, but this does not affect the estimates much either.

refugees' arrival.[55] Instead, we measure this at the municipality level.[56] We find that allocation to a neighborhood in which a larger share of the municipality's residents are diagnosed with a lifestyle related disease significantly increases the risk of developing a lifestyle related disease. However, since municipality and parish level income are not strongly correlated, the income gradient is not affected in Column (6) of Table 9. If we instead define the neighborhood income groups based on rankings of municipality level income and control for the health status in the exact same way, the neighborhood income gradient in health is notably reduced and it is no longer significant at the 5 percent level, see Appendix Table A.6 for this exercise.[57] This indicates that part of the income gradient in health can be explained by differences in health status and the transmission of health behaviors between peers in neighborhoods.

C 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 effects to become larger in magnitude as the measurement of peer groups becomes more accurate. Thus, measuring median income at the parish level rather than at the municipality level should bring us closer to the income levels of peers as the population becomes smaller and the probability of interaction is increased. The same argument goes for measuring median income levels in the apartment building (more specifically, a particular stairway of an apartment complex) rather than measuring

[55] We do not have a good measure of the prevalence of lifestyle related diseases before 1994 at the parish level because of limited data availability. Moreover, the number of inhabitants diagnosed in a given year fluctuates relatively much due to low numbers of inhabitants in some parishes, which means that incidences of lifestyle related diseases in the parish are quite noisy.

[56] The municipalities are sufficiently large to reduce the uninformative yearly variation in incidence of lifestyle related diseases.

[57] Appendix Table A.6 replicates Table 9 with income groups defined at the municipality level instead of parish level income groups.

income levels at the parish level.

We therefore estimate the increased probability of developing lifestyle related diseases upon assignment to the poorest third of municipalities, parishes and apartment buildings, respectively. The results are presented in the first three columns of Table 10. The table shows that the neighborhood effect on health becomes larger when the neighborhood size becomes smaller. The neighborhood effect on health is larger at the parish level compared with the municipality level, while the neighborhood effect on health is even larger at the apartment building level compared with the parish level.[58] Moreover, the neighborhood effect on health is most precisely estimated when we let an apartment building define a neighborhood.

In the fourth column of Table 10 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 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. We conduct this analysis by including dummies for being assigned to the poorest third of municipalities, parishes and apartment buildings simultaneously. 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. The latter both reveal small and statistically insignificant estimates. In fact, the

[58]The results are similar when estimating the specification in Model (2) instead of using only one dummy variable. We also find similar results when conducting the same exercise using our instrumental variables approach to instrument exposure to the poorest third of neighborhoods, see Appendix Table A.9. Five of seven coefficients become larger in magnitude when moving from the municipality or parish to the apartment building level. One estimate in particular is large and precisely estimated in this case; the impact on diabetes, which consistently seems to be driving impacts on nutritional diagnoses.

apartment building income group is as important as the municipality and parish income group combined.[59]

Similar to Table 9, we show that also at the apartment building level, the neighborhood effects on health do not depend on different municipality and parish level characteristics, such as the number of GPs per capita, sports facilities and the poverty rate, see Table 11. However, 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 (8) of Table 11. 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.[60] While the inclusion of parish fixed effects does reduce the magnitude of the income gradient in health substantially, there is still a 1.3 percentage point higher risk of developing lifestyle related diseases in the poorest third of apartment buildings after taking account of time-invariant parish characteristics.[61] Thus, differences in neighbors' health behavior may also explain a significant part of the differences

[59] In an alternative approach we simultaneously instrument the number of years spent in a bottom income parish and a bottom income apartment building. This exercise shows that an additional year spent in a bottom income apartment building increases the risk of diabetes by 0.9 of a percentage point whereas this estimate is 0.0 percentage points and not significant for time spent in a low-income parish (see Appendix Table A.10). Moreover, the risk of developing a lifestyle related disease increases by 1.9 percentage points for an additional year in a low-income apartment building, while there is no extra effect of an additional year in a low-income parish.

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

[61] The difference is significant at the 10 percent level.

in health outcomes across neighborhoods.

In summary, this 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 parks and public permanent sports facilities.

D Remaining Explanations

What are the remaining differences between the poorest and richest neighborhoods once we sum up the results from Section IV.B and Section IV.A? 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 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 fastfood/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.C indicate that such characteristics of the very local environment are important.

Given our results, it is especially amenities related to diet or exercise or behavior of immediate neighbors that 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.[62]

[62]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.

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 be correlated with quality. That is, the apartment quality could only be reflected in prices to a limited extent and still be within the refugees' budget. Yet, the price for quality may vary across the country such that individuals in rural areas far away from the capital got better quality apartments for the same rent as those placed in cities. However, the neighborhood income gradient persists even when we compare individuals placed in the same municipality and control for parish types, i.e., we compare rural parishes with rural parishes in the same municipality. Thus, we do not believe this is the main explanation behind our results.

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 depends on the income 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 5.1 percent higher risk of suffering from a lifestyle related disease compared with 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 provide evidence that the risk of developing a lifestyle related disease increases with the number of years spent in a low-income neighborhood, and this is primarily driven by an increased risk of suffering from diabetes and hypertension. 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 across municipalities, nor is it caused by the presence of ethnic networks or differences in poverty rates or availability of sports clubs. 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: Summary Statistics for the Population of Refugees

	All Mean	Bottom Mean	Middle Mean	Top Mean
Characteristics at Immigration				
Age	30.58	30.06	30.96	30.79
Female	0.40	0.40	0.41	0.40
Married	0.61	0.64	0.62	0.61
Number of Family Members	2.36	2.20	2.38	2.45
Number of Children	0.84	0.74	0.85	0.90
Origin Country				
Iraq	0.20	0.24	0.19	0.19
Lebanon	0.20	0.13	0.18	0.22
Somalia	0.18	0.27	0.18	0.15
Iran	0.17	0.11	0.15	0.19
Sri Lanka	0.12	0.13	0.14	0.11
Vietnam	0.08	0.06	0.11	0.08
Afghanistan	0.03	0.04	0.03	0.04
Ethiopia	0.02	0.02	0.01	0.02
Education Surveyed				
Basic Education	0.56	0.54	0.58	0.55
Vocational Education	0.21	0.22	0.20	0.21
Higher Education	0.23	0.25	0.22	0.24
Education Not Surveyed	0.70	0.70	0.70	0.70
N	25,738	4,288	7,654	12,406

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. Basic, vocational and academic education is only available for those who were surveyed. Column "All" presents the mean of characteristics among all refugees in our sample irrespective of parish income group. "Bottom" refers to characteristics among refugees assigned to the bottom third of parishes measured by median disposable income in a given year. Similarly, "Middle" and "Top" refer to characteristics among refugees assigned to the middle and top third of parishes measured by disposable income, respectively. The parish income groups are defined among all parishes, irrespective of any refugee assignment. We define income group of assignment parish one year prior to immigration by median disposable income among all inhabitants aged 18 or above. Data is from administrative registers provided by Statistics Denmark.

Table 2: Summary Statistics for Initial Placement (Parish)

	Bottom Mean	Middle Mean	Top Mean
Characteristics of Residents			
Age	46.48	46.85	45.61
Median Household Income	13,953.39	14,602.77	16,017.42
Employment Rate	0.63	0.68	0.74
Prevalence of Lifestyle Related Diseases	0.09	0.08	0.07
Inhabitants	3,987.00	4,351.20	5,311.90
Co-Nationals	17.49	12.30	8.79
Poverty Rate	0.09	0.07	0.05
Parish Type			
Urban Area (Near City)	0.45	0.43	0.68
Urban Area (Away from City)	0.04	0.19	0.16
Rural Area (Near City)	0.09	0.10	0.08
Rural Area (Away from City)	0.30	0.21	0.05
Characteristics of Municipality			
General Practioners per 1,000 Inhabitants	0.46	0.43	0.46
Incidences of Lifestyle Related Diseases per 1,000 Inhabitants	33.01	29.31	26.11
Health and Social Expenditure per Capita	4,016.16	4,112.72	4,022.29
N	683	1,456	2,773

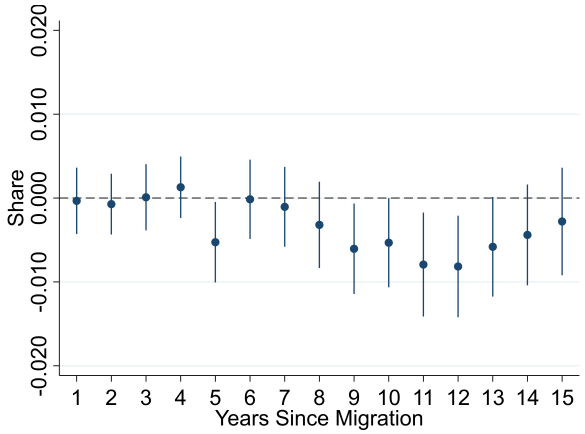
Notes: Summary statistics for parishes in which refugees were resettled. "Bottom", "Middle" and "Top" refer to parish characteristics of parishes in the bottom, middle and top third of parishes measured by median parish disposable income in a given year. We calculate the median income of each parish 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 refugees arriving after 1993. Health and social expenditure per capita and median household income is measured in USD. Observations are parish-year. Data on "Health and Social Expenditure per Capita" stems from Statistikbanken, (REG1, REG1R and REG11). Parish types are defined by Ministeriet for By, Bolig og Landdistrikter (2013). All other data are from administrative registers provided by Statistics Denmark.

Table 3: Balancing Tests

	(1) Bottom Income Group	(2) Middle Income Group	(3) Top Income Group	(4) Lifestyle Related
Unobserved at Time of Allocation				
Unknown Education	0.000	0.008	-0.008	-0.000
	(0.010)	(0.013)	(0.014)	(0.000)
Basic Education	-0.001	0.024	-0.023	-0.000
	(0.011)	(0.014)	(0.015)	(0.000)
Higher Education	0.011	0.003	-0.014	0.000
	(0.013)	(0.016)	(0.018)	(0.000)
Circulatory Disease	-0.001	-0.027	0.027	0.000
	(0.022)	(0.028)	(0.030)	(0.000)
Nutritional Disease	-0.002	-0.017	0.019	0.001
	(0.031)	(0.038)	(0.041)	(0.001)
Neurotic Disorder	-0.086	0.044	0.042	0.001
	(0.049)	(0.073)	(0.078)	(0.001)
Observed at Time of Allocation				
Age 30-49 Years	-0.003	-0.002	0.005	-0.000
	(0.006)	(0.007)	(0.008)	(0.000)
Age 50-64 Years	-0.022**	0.031**	-0.009	0.000
	(0.009)	(0.013)	(0.014)	(0.000)
Female	-0.003	-0.006	0.010	0.000
	(0.004)	(0.006)	(0.006)	(0.000)
Number of Adults	-0.015	-0.011	0.026**	-0.000***
	(0.010)	(0.009)	(0.011)	(0.000)
Number of Children 0-2 Years Old	-0.002	0.022**	-0.020	-0.000**
	(0.009)	(0.011)	(0.012)	(0.000)
Number of Children 3-17 Years Old	-0.007**	0.003	0.004	-0.000***
	(0.003)	(0.003)	(0.004)	(0.000)
Married	0.013**	0.002	-0.015	0.000***
	(0.006)	(0.008)	(0.008)	(0.000)
Year of Immigration FE	Yes	Yes	Yes	Yes
Country of Origin FE	Yes	Yes	Yes	Yes
N	24,348	24,348	24,348	24,484
F	0.74	1.02	0.80	1.33
Pr > F	0.62	0.41	0.57	0.24

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. 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 the 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. 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 initial health. As a proxy for initial health we use diagnoses within the first year after arrival, but measure all other individual characteristics at year of immigration. We measure all parish characteristics one year prior to immigration.

(a) Yearly Incidents



(b) Cumulative

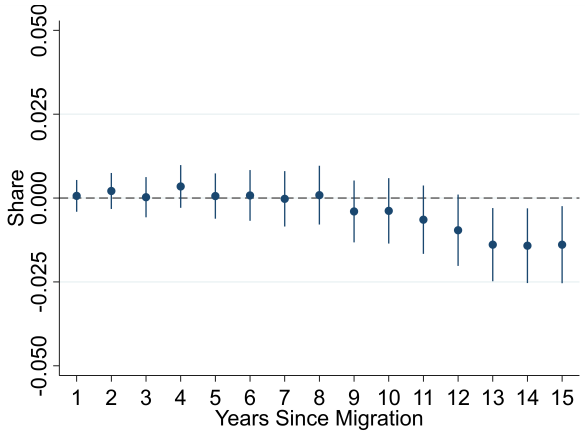


Figure 1: Development of Lifestyle Related Diagnoses

Notes: Robust standard errors clustered at parish $\times$ immigration year level. 90 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 lifestyle related diseases 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.

Table 4: Main Results

	Lifestyle Related	Circulatory	Nutritional	Hypertension	Diabetes	Mental Disorder	Neurotic
				(a) Ever diagnosed			
Middle	-0.016*	-0.023***	-0.015**	-0.018***	-0.012*	-0.006	0.002
	(0.009)	(0.008)	(0.007)	(0.006)	(0.007)	(0.009)	(0.008)
Top	-0.018**	-0.016**	-0.017**	-0.019***	-0.015**	-0.011	-0.003
	(0.008)	(0.008)	(0.007)	(0.006)	(0.006)	(0.008)	(0.007)
				(b) Diagnosed 8-15 years after immigration			
Middle	-0.015**	-0.006	-0.012**	-0.005	-0.007	0.004	0.004
	(0.007)	(0.006)	(0.006)	(0.004)	(0.005)	(0.006)	(0.005)
Top	-0.018***	-0.004	-0.013**	-0.003	-0.010**	0.003	0.005
	(0.007)	(0.005)	(0.005)	(0.004)	(0.005)	(0.006)	(0.005)
Sample Mean	0.35	0.25	0.20	0.12	0.13	0.24	0.16
N	24,348	24,348	24,348	24,348	24,348	24,348	24,348

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 in the top panel. The estimates show the the increased probability of being diagnosed with each of the considered diseases 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 at some point from year of arrival until the end of 2017. In Panel (b) the dependent variable is a dummy for being diagnosed with the considered disease 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 fixed effects. The sample mean denotes the share of refugees diagnosed with the disease before 2018.

Table 5: Heterogeneous Effects by Gender

	Lifestyle Related	Circulatory	Nutritional	Hypertension	Diabetes	Mental Disorder	Neurotic
				(a) Ever diagnosed			
Middle	-0.012	-0.025**	-0.018*	-0.016**	-0.021**	-0.003	0.005
	(0.011)	(0.011)	(0.009)	(0.008)	(0.008)	(0.011)	(0.009)
Top	-0.010	-0.015	-0.010	-0.011	-0.017**	-0.011	-0.005
	(0.011)	(0.010)	(0.009)	(0.007)	(0.008)	(0.010)	(0.009)
Middle ×	-0.009	0.005	0.006	-0.004	0.022*	-0.007	-0.008
Female	(0.018)	(0.015)	(0.016)	(0.012)	(0.013)	(0.015)	(0.014)
Top × Female	-0.020	-0.003	-0.017	-0.018*	0.005	0.000	0.004
	(0.017)	(0.014)	(0.016)	(0.011)	(0.012)	(0.014)	(0.013)
				(b) Diagnosed 8-15 years after immigration			
Middle	-0.005	-0.005	-0.003	-0.002	-0.011*	0.008	0.003
	(0.008)	(0.007)	(0.006)	(0.005)	(0.006)	(0.008)	(0.006)
Top	-0.007	-0.003	-0.003	0.001	-0.011*	0.006	0.003
	(0.008)	(0.006)	(0.006)	(0.004)	(0.006)	(0.007)	(0.005)
Middle ×	-0.026*	-0.002	-0.024*	-0.007	0.008	-0.009	0.003
Female	(0.015)	(0.011)	(0.012)	(0.008)	(0.009)	(0.012)	(0.009)
Top × Female	-0.028*	-0.003	-0.027**	-0.010	0.003	-0.006	0.004
	(0.014)	(0.010)	(0.012)	(0.007)	(0.009)	(0.011)	(0.009)
N	24,348	24,348	24,348	24,348	24,348	24,348	24,348

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 at some point from year of arrival until the end of 2017. In panel (b) the dependent variable is a dummy for being diagnosed with a lifestyle related disease 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 fixed effects.

Table 6: IV Estimates

	Lifestyle Related	Circulatory	Nutritional	Hypertension	Diabetes	Mental Disorder	Neurotic
Years of Exposure to Bottom Parish	0.005**	0.005**	0.004**	0.005***	0.004**	0.002	0.000
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
N	24,348	24,348	24,348	24,348	24,348	24,348	24,348

Notes: Robust standard errors in parentheses clustered at parish $\times$ immigration year level. $^{*}p < 0.10,^{**}p < 0.05,^{***}p < 0.01$. The table shows the increased risk of being diagnosed with one of the diseases in the top panel following an additional year of exposure to a bottom income neighborhood. We use initial placement neighborhood income group as an instrument in the first stage. The F-statistic from the first stage regression for years of exposure to a bottom income neighborhood is 293.80, and the estimated coefficient from the first stage is $\tilde{\beta} = 3.77$. 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 fixed effects.

Table 7: Labor Market Outcomes

	Employment>0	Employment	Labor Income	Business Income	Task Complexity
			(a) Cumulative since immigration		
Middle	0.01	-0.03	-2,237.89	-2,713.35	-0.01
	(0.11)	(0.09)	(3,949.49)	(4,080.61)	(0.02)
Top	0.10	0.04	1,488.98	1,169.13	-0.00
	(0.10)	(0.09)	(3,686.75)	(3,819.31)	(0.02)
			(b) 8-15 years after immigration		
Middle	-0.02	-0.02	-1,292.96	-1,435.74	-0.02
	(0.06)	(0.05)	(2,149.64)	(2,209.96)	(0.02)
Top	0.07	0.04	1,964.53	1,540.18	0.01
	(0.06)	(0.05)	(2,028.41)	(2,095.66)	(0.02)
Sample Mean	4.17	2.99	113,662.96	122,807.43	-0.03
N	24,348	24,348	24,348	24,348	11,182

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 from year of arrival to 2017 (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. The sample mean denotes the mean of the outcome considered in the top panel from year of immigration until 2018. 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 fixed effects.

Table 8: Education Outcomes

	All Education	Basic	Vocational	Higher	Health Education
			(a) Ever		
Middle	0.015**	-0.000	0.017***	0.000	-0.001
	(0.007)	(0.002)	(0.005)	(0.005)	(0.004)
Top	0.024***	0.001	0.021***	0.004	0.003
	(0.007)	(0.002)	(0.005)	(0.005)	(0.004)
			(b) Within 8 years after immigration		
Middle	0.015**	-0.000	0.017***	-0.000	-0.001
	(0.007)	(0.002)	(0.005)	(0.005)	(0.004)
Top	0.025***	0.001	0.021***	0.004	0.004
	(0.007)	(0.002)	(0.005)	(0.005)	(0.004)
Sample Mean	0.15	0.01	0.09	0.07	0.05
N	24,348	24,348	24,348	24,348	24,348

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 from year of arrival until 2017 (Panel (a)), and within the first 8 years after arrival (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 fixed effects. The sample mean denotes the mean of the outcome considered in the top panel from year of immigration until 2018.

Table 9: Mechanisms, Lifestyle Related Diseases

	Baseline	(1)	(2)	(3)	(4)	(5)	(6)	(7)
				(a) Ever diagnosed				
Middle	-0.016*	-0.019*	-0.016*	-0.016*	-0.016*	-0.016*	-0.016	-0.015*
	(0.009)	(0.010)	(0.010)	(0.009)	(0.009)	(0.009)	(0.010)	(0.009)
Top	-0.018**	-0.019**	-0.013	-0.018**	-0.018**	-0.020**	-0.018*	-0.015*
	(0.008)	(0.009)	(0.010)	(0.009)	(0.008)	(0.009)	(0.010)	(0.009)
				(b) Diagnosed 8-15 years after immigration				
Middle	-0.015**	-0.016**	-0.012	-0.015**	-0.016**	-0.016**	-0.012	-0.014**
	(0.007)	(0.008)	(0.008)	(0.007)	(0.007)	(0.007)	(0.008)	(0.007)
Top	-0.018***	-0.018**	-0.012	-0.017***	-0.018***	-0.019***	-0.013*	-0.015**
	(0.007)	(0.007)	(0.008)	(0.007)	(0.007)	(0.007)	(0.008)	(0.007)
N	24,348	22,948	24,340	24,345	24,233	24,348	24,348	24,348
Parish Type FE	No	Yes	No	No	No	No	No	No
Municipality FE	No	No	Yes	No	No	No	No	No
Control	No	No	No	GP/Capita	Sports Facilities	Co-Nationals	Poverty	Health

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 tests of potential mechanisms behind the estimated neighborhood effects by estimating model (2) with different sets of controls. In column (Baseline) we replicate the estimates from Table 4. In column (1) we include 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 (2) we include municipality fixed effects, in (3) we include the number of GPs per capita in the municipality of assignment as a control. In column (4) we include the number of sports facilities in the parish. In column (5) we include the number and squared number of co-nationals in the neighborhood, and in column (6) we include the poverty rate in the neighborhood as a control. In column (7) we include the logarithm of the number of incidences (share of inhabitants above 18) of lifestyle related diseases in the assignment municipality as a control. All municipality and neighborhood characteristics are measured one year prior to immigration. The coefficients on the controls in (3), (5) and (6) are positive or virtually zero and insignificant. Only the controls in (4) and (7) are significant with an estimated coefficient of -0.001 in Panel (a) and Panel (b) in column (4), and 0.031 in Panel (a) and 0.027 in Panel (b) in column (7). In Panel (a) the dependent variable is an indicator for being diagnosed with a lifestyle related disease at some point from year of arrival until 2017. In Panel (b) the dependent variable is an indicator for being diagnosed with a lifestyle related disease 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. All other parish characteristics are also measured 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 fixed effects.

Table 10: Neighborhood Effects on Health Using Different Neighborhood Definitions

	Lifestyle Related	Lifestyle Related	Lifestyle Related	Lifestyle Related
Placed in Bottom Income Municipality	0.012			0.007
	(0.010)			(0.010)
Placed in Bottom Income Parish		0.018**		0.013
		(0.009)		(0.009)
Placed in Bottom Income Apartment Building			0.022***	0.020***
			(0.007)	(0.007)
Sample Mean	0.36	0.36	0.36	0.36
N	19,625	19,625	19,625	19,625

Notes: Robust standard errors in parentheses clustered at parish $\times$ immigration year level. $^{*}p < 0.10,^{**}p < 0.05,^{***}p < 0.01$. The table shows the increased probability of being diagnosed with a lifestyle related disease following initial assignment to a bottom income neighborhood using different definitions of a neighborhood. The bottom income municipality, parish and apartment building group refer to the bottom third of all municipalities, parishes and apartment buildings, respectively. We measure income groups one year prior to arrival based on median disposable income in each year. In the first three columns the impact of assignment to the different neighborhood levels are estimated separately. In the last column the three dummies for assignment to the poorest third of municipalities, parishes and apartment buildings are included simultaneously. 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 fixed effects. The sample mean denotes the share of refugees diagnosed with a lifestyle related disease before 2018.

Table 11: Mechanisms, Lifestyle Related Diseases (Apartment Building Level)

	Baseline	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Placed in Bottom	0.023***	0.020***	0.019***	0.023***	0.021***	0.022***	0.021***	0.022***	0.013*
Income Apartment Building	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.008)
N	19,782	18,581	19,771	19,780	19,539	19,625	19,625	19,782	19,650
Parish Type FE	No	Yes	No	No	No	No	No	No	No
Parish FE	No	No	No	No	No	No	No	No	Yes
Municipality FE	No	No	Yes	No	No	No	No	No	No
Control	No	No	No	GP/Capita	Sports Facilities	Co-Nationals	Poverty	Health	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 tests of potential mechanisms behind the estimated neighborhood effects by estimating model (2) with different sets of controls, using apartment building instead of parish level income groups. Column (Baseline) shows the baseline coefficients from model (2) with apartment building level income groups. In column (1) we include 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 (2) we include municipality fixed effects, in (3) we include the number of GPs per capita in the municipality of assignment as a control. In column (4) we include the number of sports facilities in the parish. In column (5) we include the number and squared number of co-nationals in the parish, and in column (6) we include the poverty rate in the parish as a control. In column (7) we include the logarithm of the number of incidences (share of inhabitants above 18) of lifestyle related diseases in the assignment municipality as a control. In column (8) we include parish fixed effects. All municipality and parish characteristics are measured one year prior to immigration. The coefficients on the controls in (3), (5), (6) and (7) are positive or virtually zero and insignificant. Only the controls in (4) are significant with an estimated coefficient of -0.001. In all columns the dependent variable is an indicator for being diagnosed with a lifestyle related disease at some point from year of arrival until 2017. We measure apartment building income groups one year prior to arrival based on median disposable income in each apartment building among all apartment buildings in Denmark in a given year. All parish characteristics are also measured 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 fixed effects.

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A Appendix: Additional Tables and Figures

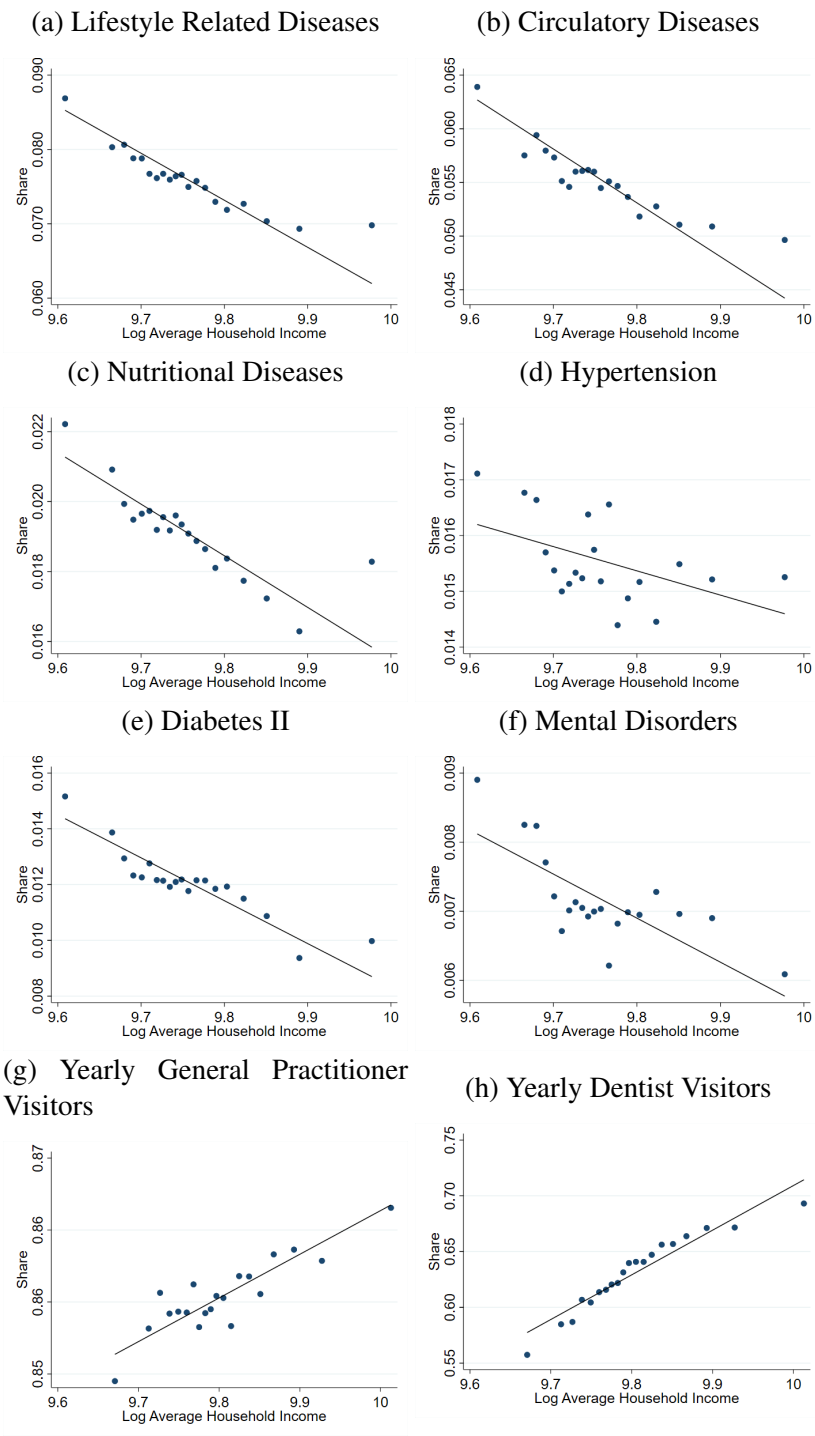

Figure A.1: Association Between Health and Neighborhood Income

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, Apartment Building Level

	(1) Bottom Income Group	(2) Middle Income Group	(3) Top Income Group	(4) Lifestyle Related
Unobserved at Time of Allocation				
Unknown Education	0.006	0.002	-0.009	0.000
	(0.015)	(0.015)	(0.011)	(0.002)
Basic Education	0.012	0.007	-0.019	0.003
	(0.016)	(0.016)	(0.012)	(0.002)
Higher Education	0.017	-0.012	-0.005	0.003
	(0.019)	(0.018)	(0.014)	(0.003)
Circulatory Disease	-0.008	-0.003	0.011	-0.002
	(0.032)	(0.033)	(0.024)	(0.004)
Nutritional Disease	-0.043	-0.014	0.056	-0.001
	(0.042)	(0.042)	(0.035)	(0.005)
Neurotic Disorder	-0.010	-0.074	0.085	-0.001
	(0.090)	(0.079)	(0.073)	(0.015)
Observed at Time of Allocation				
Age 30-49 Years	0.003	-0.013	0.011	-0.002
	(0.009)	(0.008)	(0.006)	(0.001)
Age 50-64 Years	-0.070***	0.063***	0.007	-0.001
	(0.014)	(0.015)	(0.011)	(0.002)
Female	-0.061***	0.041***	0.020***	0.003***
	(0.006)	(0.006)	(0.004)	(0.001)
Number of Adults	-0.031***	0.003	0.029***	0.003
	(0.011)	(0.011)	(0.009)	(0.002)
Number of Children 0-2 Years Old	-0.022	0.014	0.008	-0.001
	(0.013)	(0.013)	(0.010)	(0.002)
Number of Children 3-17 Years Old	-0.014***	0.003	0.011***	-0.000
	(0.004)	(0.004)	(0.003)	(0.001)
Married	-0.040***	0.037***	0.003	0.001
	(0.009)	(0.009)	(0.006)	(0.001)
Year of Immigration FE	Yes	Yes	Yes	Yes
Country of Origin FE	Yes	Yes	Yes	Yes
N	20,804	20,804	20,804	20,806
F	0.37	0.40	1.28	0.83
Pr > F	0.90	0.88	0.26	0.55

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. 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. 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 initial health. As a proxy for initial health we use diagnoses within the first year after arrival, but measure all other individual characteristics at year of immigration. We measure all apartment building characteristics one year prior to immigration.

Table A.2: Balancing Tests, Municipality Level

	(1) Bottom Income Group	(2) Middle Income Group	(3) Top Income Group	(4) Lifestyle Related
Unobserved at Time of Allocation				
Unknown Education	0.004	0.017	-0.021	-0.000
	(0.009)	(0.014)	(0.013)	(0.000)
Basic Education	0.007	0.028	-0.036**	-0.000
	(0.010)	(0.015)	(0.014)	(0.000)
Higher Education	0.006	0.018	-0.024	0.000
	(0.012)	(0.017)	(0.017)	(0.000)
Circulatory Disease	-0.002	0.020	-0.018	0.000
	(0.020)	(0.030)	(0.029)	(0.000)
Nutritional Disease	-0.013	0.058	-0.045	0.000
	(0.027)	(0.039)	(0.036)	(0.000)
Neurotic Disorder	-0.086	0.055	0.032	-0.001
	(0.050)	(0.074)	(0.075)	(0.001)
Observed at Time of Allocation				
Age 30-49 Years	-0.013**	0.011	0.002	-0.000
	(0.005)	(0.008)	(0.007)	(0.000)
Age 50-64 Years	-0.024***	0.032**	-0.008	-0.000
	(0.008)	(0.013)	(0.013)	(0.000)
Female	-0.009**	-0.012**	0.021***	-0.000
	(0.004)	(0.006)	(0.005)	(0.000)
Number of Adults	-0.002	-0.012	0.014	-0.000***
	(0.006)	(0.010)	(0.010)	(0.000)
Number of Children 0-2 Years Old	0.013	-0.005	-0.008	-0.000
	(0.008)	(0.012)	(0.011)	(0.000)
Number of Children 3-17 Years Old	-0.002	0.003	-0.001	-0.000***
	(0.002)	(0.003)	(0.003)	(0.000)
Married	0.013**	-0.010	-0.003	-0.000
	(0.005)	(0.008)	(0.008)	(0.000)
Year of Immigration FE	Yes	Yes	Yes	Yes
Country of Origin FE	Yes	Yes	Yes	Yes
N	25,738	25,738	25,738	25,738
F	0.64	1.22	1.52	1.00
Pr > F	0.70	0.29	0.17	0.42

Notes: Balancing tests for municipalities 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. 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 municipalities with specific characteristics (dependent variables). The dependent variables in (1)-(3) are dummies for assignment to a bottom income municipality (1), middle income municipality (2) or top income municipality (3). In column (4) the dependent variable is the incidence (as a share of inhabitants) of lifestyle related diseases. 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 initial health. As a proxy for initial health we use diagnoses within the first year after arrival, but measure all other individual characteristics at year of immigration. We measure all municipality characteristics one year prior to immigration.

A 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)

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)	(3)	Abnormalities	Metabolic
			(a) Ever diagnosed			
Middle	-0.016*	-0.014		-0.015*	0.001	-0.003
	(0.009)	(0.009)		(0.009)	(0.004)	(0.004)
Top	-0.018**	-0.015*		-0.018**	-0.001	-0.005
	(0.008)	(0.009)		(0.009)	(0.004)	(0.004)
Standardized Median Income			-0.008**			
			(0.003)			
			(b) Diagnosed 8-15 years after immigration			
Middle	-0.015**	-0.018**		-0.016**	-0.002	-0.001
	(0.007)	(0.007)		(0.007)	(0.002)	(0.002)
Top	-0.018***	-0.019***		-0.019***	-0.003	0.000
	(0.007)	(0.007)		(0.007)	(0.002)	(0.002)
Standardized Median Income			-0.007***			
			(0.002)			
N	24,348	24,348	24,348	24,348	24,348	24,348
Income Type	Disposable	Disposable	Disposable	Disposable	Disposable	Disposable
Moment	Median	Mean	Continuous	Median	Median	Median
Method	OLS	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) demonstrates the estimated effects using a standardized continuous income measure, and column (3) shows the estimated neighborhood effects from a probit model. In Panel (a) the dependent variable is an indicator for being diagnosed with a disease at some point from year of arrival to the end of 2017. 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 if origin as well as age and year fixed effects.

Table A.4: Impact on Mortality

	Within 8 Years Since Migration	Within 15 Years Since Migration	Before 2018
Middle	-0.003	-0.006	-0.005
	(0.005)	(0.006)	(0.006)
Top	-0.008*	-0.010*	-0.009
	(0.004)	(0.005)	(0.006)
Sample Mean	0.04	0.07	0.10
N	24,348	24,348	24,348

Note: Robust standard errors in parentheses clustered at parish $\times$ immigration year level. $^{*}p < 0.10,^{**} p < 0.05,^{***} p < 0.01$. The estimates show the increased probability of death if assigned to a middle- or a top-income neighborhood compared to a bottom income neighborhood. In the first column the dependent variable is a dummy for dying within the first 8 years after immigration, in the second column the dependent variable is dummy for dying within the first 15 years since immigration. In the last column the dependent variable is a dummy for dying before 2018. 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 fixed effects. The sample mean denotes the mean of the outcome considered in the top panel from year of immigration until 2018.

Table A.5: Mechanisms, Lifestyle Related Diseases

	Baseline	(1)	(2)	(3)	(4)
			(a) Ever diagnosed		
Middle	-0.016*	-0.015*	-0.015*	-0.016*	-0.016*
	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)
Top	-0.018**	-0.017*	-0.018**	-0.019**	-0.019**
	(0.008)	(0.009)	(0.009)	(0.009)	(0.009)
			(b) Diagnosed 8-15 years after immigration		
Middle	-0.015**	-0.014**	-0.015**	-0.015**	-0.015**
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Top	-0.018***	-0.016**	-0.017**	-0.018***	-0.018**
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
N	24,348	24,345	24,348	24,348	24,265
Parish Type FE	No	No	No	No	No
Municipality FE	No	No	No	No	No
Control	No	Health Expenditure	Number Refugees	Immigrant Share	Immigrant Income

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 tests of potential mechanisms driving the estimated neighborhood effects by estimating the increased probability of being diagnosed with a lifestyle related disease following assignment to a middle- or top-income neighborhood compared to bottom-income neighborhoods with different sets of controls. In column (Baseline) we repeat the estimates from Table 4. In column (1) we include the control "Health Expenditure", which refers to the inclusion of the logarithm of the number of GPs per capita in the municipality and the logarithm of health and social expenditure per capita in the municipality. In column (2) 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 as a control. In column (3) we include the share of immigrants and the squared share of immigrants as a control. In column (4) we include the logarithm of median disposable income among immigrants in the neighborhood. We measure all these neighborhood controls one year prior to arrival. In Panel (a) the dependent variable is a dummy for being diagnosed with a lifestyle related disease before 2018, and in Panel (b) the dependent variable is a dummy for being diagnosed with a lifestyle related disease between 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. All other parish characteristics are also measured 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 fixed effects.

Table A.6: Mechanisms, Lifestyle Related Diseases (Municipality Level)

	Baseline	(1)	(2)	(3)	(4)	(5)	(6)	(7)
				(a) Ever diagnosed				
Middle	-0.013	-0.017*	-0.019	-0.013	-0.014	-0.013	-0.012	-0.010
	(0.009)	(0.010)	(0.014)	(0.009)	(0.009)	(0.009)	(0.009)	(0.010)
Top	-0.015	-0.019*	-0.025	-0.015	-0.014	-0.015	-0.013	-0.008
	(0.010)	(0.011)	(0.020)	(0.010)	(0.010)	(0.010)	(0.010)	(0.011)
				(b) Diagnosed 8-15 years after immigration				
Middle	-0.013*	-0.016**	-0.020*	-0.013*	-0.013*	-0.012*	-0.010	-0.010
	(0.007)	(0.008)	(0.012)	(0.007)	(0.007)	(0.007)	(0.007)	(0.008)
Top	-0.017**	-0.020**	-0.032**	-0.017**	-0.017**	-0.018**	-0.013	-0.011
	(0.008)	(0.009)	(0.016)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
N	24,541	23,141	24,533	24,538	24,233	24,348	24,348	24,541
Parish Type FE	No	Yes	No	No	No	No	No	No
Municipality FE	No	No	Yes	No	No	No	No	No
Control	No	No	No	GP/Capita	Sports Facilities	Co-Nationals	Poverty	Health

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 tests of potential mechanisms behind the estimated neighborhood effects by estimating model (2) with different sets of controls, using municipality instead of parish level income groups. Column (Baseline) shows the baseline coefficients from model (2) with municipality level income groups. The controls in columns (1) to (7) are exactly identical to the controls in Table 9, but the income groups are based on the median income in the municipality. The coefficients on the controls in (3), (5) and (6) are positive or virtually zero and insignificant. Only the controls in (4) and (7) are significant. In column (4) the controls are significant with an estimated coefficient of -0.001 in Panel (a) and Panel (b). In (7) the controls are significant at the 10 percent level with an estimated coefficient of 0.030 in Panel (a), and 0.023 in Panel (b). In Panel (a) the dependent variable is an indicator for being diagnosed with a lifestyle related disease at some point from year of arrival until 2017. In Panel (b) the dependent variable is an indicator for being diagnosed with a lifestyle related disease 8-15 years after immigration. We measure municipality income groups one year prior to arrival based on median disposable income in each municipality among all municipalities in Denmark in a given year. All other characteristics are also measured 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 fixed effects. In Column (2) the income gradient in health becomes larger, when controlling for time-invariant municipality characteristics. This suggests that the municipality fixed effects capture some unobserved area components which are positively correlated with the income level in the municipality and the share of refugees diagnosed with a lifestyle related disease. One possible explanation behind this is that the detection probability of lifestyle related diseases is higher in richer municipalities.

B Instrumental Variables Strategy

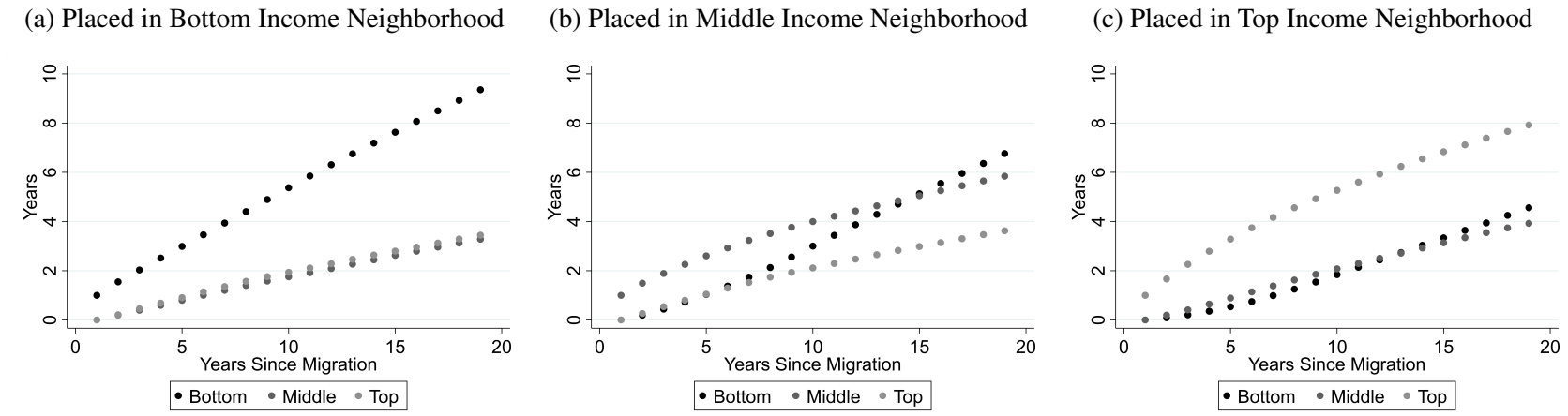

Figure A.2: Cumulative Exposure to Neighborhoods by Years Since Immigration

Notes: The figure plots the cumulative exposure to bottom, middle and top third income neighborhoods conditional on type of initial placement neighborhood against years since immigration. Panel (a) shows the cumulative exposure to each neighborhood type among those refugees initially placed in the bottom third income neighborhoods. Similarly, Panel (b) and Panel (c) show the cumulative exposure to the different neighborhood types among those initially placed in the middle third or top third income neighborhoods, respectively. 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.

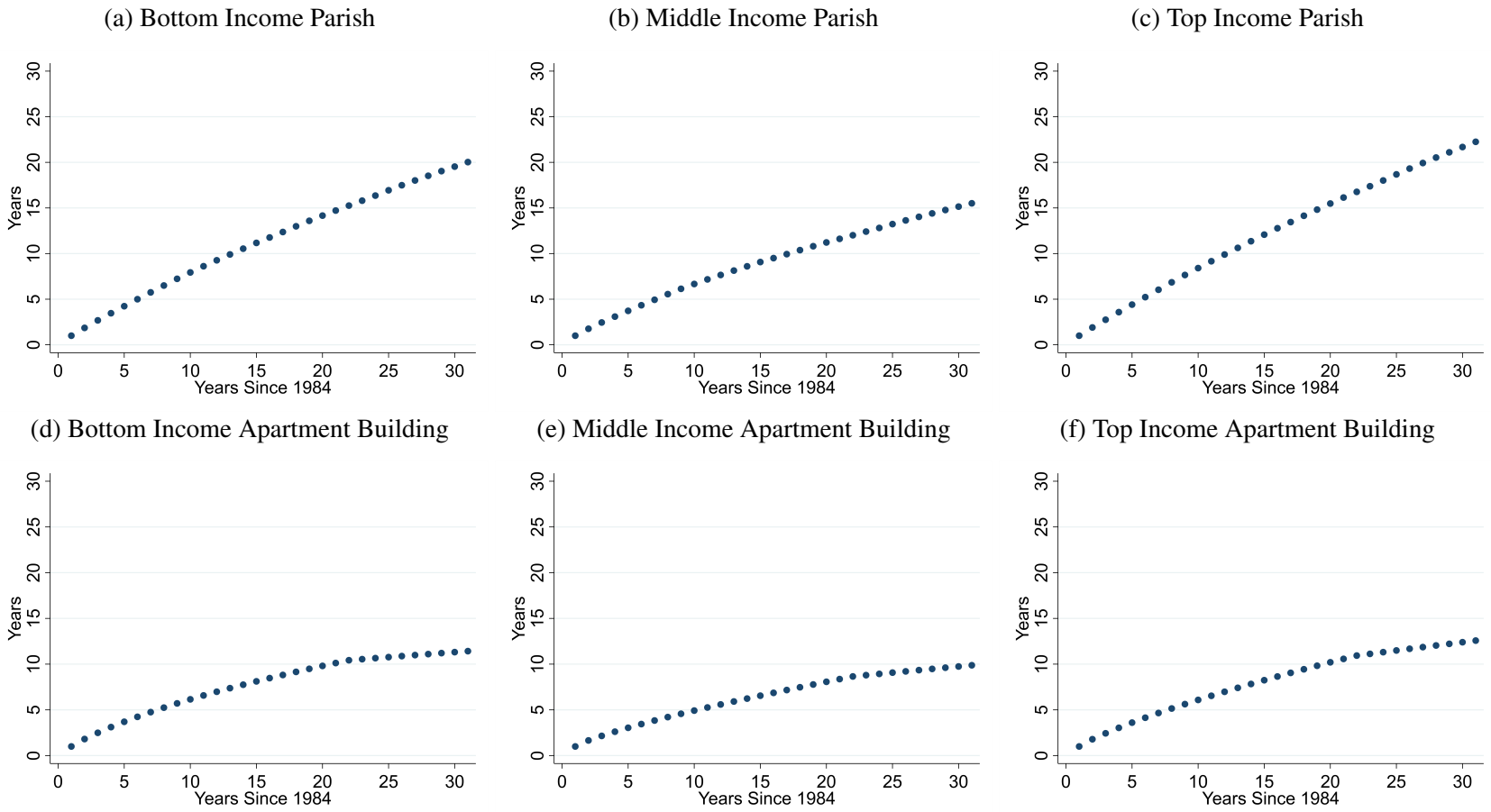

Figure A.3: Persistence in Neighborhood Classifications

Notes: Panel (a) shows the cumulative number of years that a parish that belongs to the bottom third income group of parishes in 1984 belongs to the bottom third income group of parishes until 2017 measured by median disposable income among adults in each parish in each year. Similarly, Panels (b) and (c) show this for parishes originally classified as the middle or top third income groups of parishes, respectively. Panels (d), (e) and (f) show the exact same for apartment buildings.

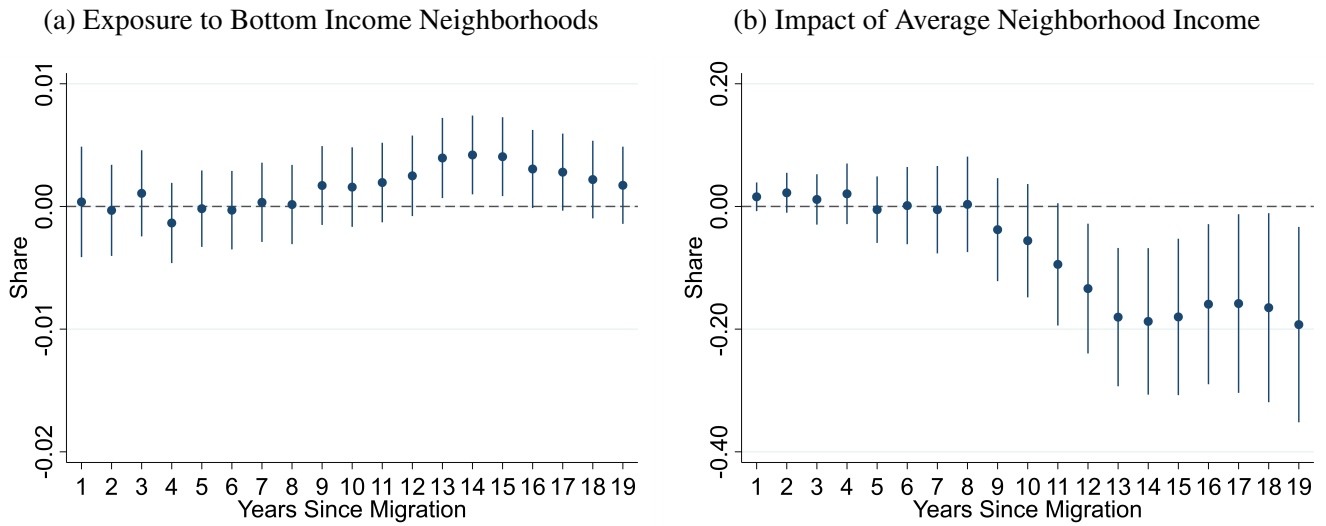

Figure A.4: Dynamics of Lifestyle Related Diagnoses, IV Estimates

Notes: In Panel (a) cumulative years of exposure to bottom income neighborhoods is instrumented by placement neighborhood income group. In Panel (b) the average income in all neighborhoods lived in until year $t + r$ is instrumented by the average income in the first placement neighborhood. Robust standard errors clustered at parish $\times$ immigration year level. 90 percent confidence intervals. 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 fixed effects.

C Alternative Instrumental Variables Strategy

Another approach to taking endogenous moving into account is to instrument the average income level that the refugee was exposed to over the r years since arrival. As an instrument we use the initial income level in the placement neighborhood one year prior to arrival. We then estimate the effect of experiencing a higher average neighborhood income level since arrival. In this approach we calculate the average income level of all neighborhoods that the refugee lived in during the r years after arrival: $\bar{x}_{i,t+r} \equiv \frac{\sum_{r=0}^{r} income_{n,t+r}}{r}$. We instrument this average using the income level of the placement neighborhood at time $t-1$. Again, this instrument is relevant if there is some persistence in neighborhood income levels experienced after arrival. If this is fulfilled we can estimate:

$$Second\ stage: \quad y_{i,t+r} = \alpha_1 + \beta_1 \hat{\bar{x}}_{i,t+r} + \boldsymbol{X_{it}\gamma_1} + \boldsymbol{T_t} + \varepsilon_{i,t+r}. \tag{4}$$

In model (4), $income_{n,t-1}$ denotes income in initial placement neighborhood one year prior to arrival and $\hat{\bar{x}}_{i,t+r}$ denotes the average neighborhood income level experienced over the r years since arrival. All other inputs are the same as in models (2) and (3). The coefficient β_1 can be interpreted as the increased risk of being diagnosed with y when living in neighborhoods with one percent higher income for r years.

Table A.7: IV Results, Average Neighborhood Income Level

	(1) Lifestyle Related	(2) Circulatory	(3) Nutritional	(4) Hypertension	(5) Diabetes	(6) Mental Disorder	(7) Neurotic
Average Income	-0.198*	-0.086	-0.104	-0.162**	-0.139*	-0.215**	-0.086
	(0.103)	(0.087)	(0.089)	(0.072)	(0.080)	(0.098)	(0.083)
N	24,348	24,348	24,348	24,348	24,348	24,348	24,348

Notes: Robust standard errors in parentheses clustered at parish $\times$ immigration year level. $^{*}p < 0.10,^{**}p < 0.05,^{***}p < 0.01$. The table shows the increased probability of being diagnosed with each of the diseases considered in the top panel following an increase in average income in neighborhoods lived in since immigration of 1 percent. The average neighborhood income level in all neighborhoods lived in since immigration is instrumented by the median neighborhood income among adults of age 18 and above in the first placement neighborhood. 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 fixed effects. F-statistics from first stage regression for average income in parishes lived in is = 131.30.

D Exposure to the Poorest Neighborhoods, Without IV

Table A.8: Exposure to the Poorest Neighborhoods, OLS

	Lifestyle Related	Circulatory	Nutritional	Hypertension	Diabetes	Mental Disorder	Neurotic
Years of Exposure	0.007***	0.004***	0.006***	0.003***	0.004***	0.007***	0.005***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
N	24,541	24,541	24,541	24,541	24,541	24,541	24,541
Method	OLS	OLS	OLS	OLS	OLS	OLS	OLS

Notes: Robust standard errors in parentheses clustered at parish $\times$ immigration year level. $^{*}p < 0.10,^{**} p < 0.05,^{***} p < 0.01$. The table shows the increased risk of being diagnosed with one of the diseases in the top panel following an additional year of exposure to a bottom income bottom neighborhood. 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 fixed effects.

E Neighborhood Definition

Table A.9: IV Estimates, Different Definitions of Neighborhoods

	Lifestyle Related	Circulatory	Nutritional	Hypertension	Diabetes	Mental Disorder	Neurotic
				(a) Bottom Municipality			
Years of Exposure to	0.003	0.000	0.005**	0.001	0.001	-0.002	0.000
Bottom Municipality	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)
				(b) Bottom Parish			
Years of Exposure to	0.004**	0.004**	0.004**	0.004**	0.003*	0.003	0.001
Bottom Parish	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
				(c) Bottom Apartment Building			
Years of Exposure to	0.019***	0.011**	0.013***	0.006	0.010**	0.003	-0.001
Bottom Apartment Building	(0.006)	(0.005)	(0.005)	(0.004)	(0.004)	(0.005)	(0.005)
N	19,782	19,782	19,782	19,782	19,782	19,782	19,782

Notes: Robust standard errors in parentheses clustered at parish $\times$ immigration year level. $^{*}p < 0.10,^{**} p < 0.05,^{***} p < 0.01$. The table shows the increased probability of being diagnosed with one of the diseases in the top panel following an additional year living in a bottom neighborhood using different neighborhood definitions. In Panel (a) we let a municipality define a neighborhood and measure the increased probability of being diagnosed with the disease considered following an additional year spent in a bottom income municipality. We use initial municipality income group as instrument for the years spent in a bottom income municipality in the first stage. Completely parallel to that we let a parish define a neighborhood in Panel (b) using initial parish income group as an instrument in the first stage. Similarly, in Panel (c) we let an apartment building define a neighborhood and use initial apartment building income group as instrument in the first stage. In all neighborhood definitions we define neighborhood income groups based on median disposable income among adults of age 18 and above one year prior to arrival. The bottom income municipality, parish and apartment building group refer to the bottom third of all municipalities, parishes and apartment buildings, respectively. We measure income groups one year prior to arrival based on median disposable income in each 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 fixed effects. F-statistics from first stage regressions for years of exposure to bottom income municipality = 133.34, bottom income parish = 299.54, and bottom income apartment building = 89.21.

Table A.10: IV Results

	Lifestyle Related	Circulatory	Nutritional	Hypertension	Diabetes	Mental Disorder	Neurotic
Years of Exposure to	-0.003	0.000	-0.000	0.001	-0.000	0.002	0.002
Bottom Parish	(0.004)	(0.003)	(0.003)	(0.002)	(0.002)	(0.003)	(0.003)
Years of Exposure to	0.019***	0.010	0.013**	0.006	0.009*	0.002	-0.002
Bottom Apartment Building	(0.007)	(0.006)	(0.006)	(0.005)	(0.005)	(0.006)	(0.005)
N	19,625	19,625	19,625	19,625	19,625	19,625	19,625

Notes: Robust standard errors clustered at parish $\times$ immigration year level. $^{*}p < 0.10,^{**}p < 0.05,^{***}p < 0.01$. The table shows the increased probability of being diagnosed with one of the diseases in the top panel following an additional year spent in a bottom income neighborhood using different neighborhoods at the parish and apartment building level simultaneously. We use initial parish and apartment building income groups as instruments for years spent in a bottom income parish and apartment building in the first stage. In both neighborhood definitions we define neighborhood income groups based on median disposable income among adults of age 18 and above one year prior to arrival. The bottom income parish and apartment building group refer to the bottom third of all parishes and apartment buildings, respectively. We measure income groups one year prior to arrival based on median disposable income in each 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 fixed effects. F-statistics from first stage regression for years of exposure to a bottom parish and apartment building is = 149.09.

Table A.11: Summary Statistics for Initial Placement (Apartment Building)

	Bottom Mean	Middle Mean	Top Mean
Characteristics of Residents			
Age	40.28	39.57	38.81
Median Household Income	13,563.90	14,221.27	14,855.93
Employment Rate	0.48	0.55	0.59
Prevalence of Lifestyle Related Diseases per 1,000 Inhabitants	69.90	67.45	47.69
Inhabitants	20.92	11.32	13.48
Co-Nationals	1.26	0.83	0.77
Poverty Rate	0.13	0.10	0.10
Parish Type			
Urban Area (Near City)	0.45	0.43	0.68
Urban Area (Away from City)	0.04	0.19	0.16
Rural Area (Near City)	0.09	0.10	0.08
Rural Area (Away from City)	0.30	0.21	0.05
Characteristics of Municipality			
General Practioners per 1,000 Inhabitants	0.46	0.43	0.46
Incidences of Lifestyle Related Diseases per 1,000 Inhabitants	33.01	29.31	26.11
Health and Social Expenditure per Capita	4,016.16	4,112.72	4,022.29
N	683	1,456	2,773

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.

Table A.12: Summary Statistics for Initial Placement (Municipality)

	Bottom Mean	Middle Mean	Top Mean
Characteristics of Residents			
Age	47.91	47.46	45.98
Median Household Income	14,610.13	14,678.44	15,936.67
Employment Rate	0.67	0.69	0.73
Inhabitants	30,743.78	20,078.51	22,332.97
Co-nationals	54.05	37.06	27.10
Poverty Rate	0.08	0.07	0.06
Urban Area (Near City)	0.15	0.23	0.58
Urban Area (Away from City)	0.09	0.29	0.22
Rural Area (Near City)	0.18	0.14	0.11
Rural Area (Away from City)	0.54	0.31	0.06
Characteristics of Municipality			
General Practioners per 1,000 Inhabitants	0.37	0.36	0.41
Incidences of Lifestyle Related Diseases per 1,000 Inhabitants	32.40	29.24	24.65
Health and Social Expenditure per Capita	3,562.66	3,648.68	3,552.23
N	199	511	1,021

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.