



Income Inequality and Health: Lessons from a Residential Assignment Program

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May 16, 2011

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ABSTRACT

This paper investigates how income inequality affects health. Although a large literature has shown that inhabitants in areas with greater income inequality suffer from worse health, past studies are severely plagued by inadequate data, non-random residential sorting and reverse causality. We address these problems using longitudinal population hospitalization data coupled with a settlement policy where Swedish authorities distributed newly arrived refugee immigrants to their initial area of residence. The policy was implemented in a way that provides a source of plausibly random variation in initial location. Our empirical analysis reveals no statistically significant effect of income inequality on the probability of being hospitalized. This finding holds also when investigating subgroups more vulnerable to negative health influences and when studying different types of diseases. There is however some indications of a detrimental effect on older persons' health; but the magnitude of the effect is small. Our estimates are precise enough to rule out large effects of income inequality on health.

Keywords: Income inequality; Immigration; Quasi-experiment

JEL: I10; J15

* The authors acknowledge financial support from NORFACE (Grönqvist), the Swedish Council for Working Life and Social Research (Grönqvist and Johansson) and Jan Wallander and Tom Hedelius Stiftelser (Grönqvist and Niknami). Part of this work was done while Grönqvist and Niknami visited CReAM at University College London. The authors are grateful to the faculty and staff, in particular Christian Dustmann, for their hospitality. The paper has benefitted from suggestions by seminar participants at UCL, IZA, SOLE 2011 (Vancouver), Migration: Economic Change and Social Challenges (London), SOFI, and comments by Anders Björklund, Christian Dustmann, Per Molander, Anders Stenberg and Eskil Wadensjö.

1. INTRODUCTION

This paper investigates how income inequality affects health. An enormous literature in several disciplines has shown that inhabitants in areas with greater income inequality suffer from worse health and higher mortality rates (see reviews by e.g. Deaton 2003; Leigh, Jencks and Smeeding 2009; Wilkinson and Pickett 2006). The magnitude of the estimates in some of these studies is strikingly large. For instance, Lynch et al. (1998) find that the annual loss of lives from income inequality in the US is comparable to the combined loss of lives from lung cancer, diabetes, motor vehicle crashes, HIV, suicide and homicide. If valid, the results suggest that the rising levels of income inequality witnessed in many industrialized countries during the past decades (Gottschalk and Smeeding 2000) may have far reaching consequences for public health and that policies to combat inequality can bring major health benefits to society. This may especially be true for many minority groups living in areas characterized by high levels of income inequality (Deaton and Lubotsky 2002).

There are two theories linking income inequality to health. The first is the strong income inequality hypotheses which states that inequality itself matters, regardless of an individual's own income level. Several explanations have been suggested for why inequality might matter at all income levels. One potential pathway works through political influence. Well off individuals are more likely to participate in political activities (Benebou 2000). In unequal societies rich individuals pay more to the government in terms of taxes than the transfers and services they receive. They may therefore support policies that favour less public spending. Among other things this could result in worse health care (Kawachi and Kennedy 1997). It has also been suggested that inequality erodes social capital (cf. interpersonal trust) and increases the

social distance between people, which in turn has been posited to influence health through psychosocial stress, self-destructive behaviour and civic involvement (Kaplan et al. 1996).

The weak income inequality hypothesis (also called the relative deprivation hypothesis) states that it is an individual's income relative to his reference group that matters. In this framework individuals are assumed to compare themselves to others who are more advantaged while ignoring those who are less advantaged. Being relatively more disadvantaged is believed to raise psychosocial stress and thereby adversely impact health (e.g. Wilkinson 1997; Marmot et al. 1991). There is plenty of evidence in the biological literature that links relative social status to both physical and mental health.¹

Despite being somewhat vague about the pathways through which inequality may influence health, the hypothesis that income inequality matters *per se* is supported by a large empirical literature. Most studies are based on comparisons across countries or US states. The studies have been carefully reviewed by e.g. Deaton (2003); Judge, Mulligan and Benxeval (1998); Leigh, Jencks and Smeeding (2009); Lynch et al. (2004); and Wilkinson and Pickett (2006). The general conclusion is that inequality adversely influences health and mortality. For instance, Waldmann (1992) finds that greater cross-country inequality is associated with significantly higher infant mortality rates. Kaplan et al. (1996) show that US states characterized by high levels of inequality have higher mortality rates. Only a few studies find no effect. For example, Leigh and Jencks (2007) show that the top decile income share does not affect population health in a panel of developed countries.

¹ Deaton (2001) and Eibner and Evans (2005) cite several studies. These papers also explicitly analyze the relationship between relative deprivation and health.

Individual level studies are scarce due to data limitations. Overall, the results in individual level studies are weaker than the aggregated area studies (Deaton 2003). Lochner et al. (2001), Fiscella and Franks (1997) and Soobander and Leclere (1999) find only a small effect of income inequality on mortality and self-reported health. A few studies excel in terms of better data or more convincing identification strategy. Gerdtham and Johannesson (2004) use perhaps the richest data to date and are able to discriminate between the effect of own income, relative income and income inequality. They find that mortality decreases significantly as individual income increases, but there is no evidence that relative income differences or income inequality matters for mortality in Sweden. Mellor and Milyo (2002) are able to control for unobserved regional characteristics using panel data from the US on self-reported health. After adjusting for household income and regional level fixed effects they no longer find any evidence that inequality affects health.

There are at least three reasons to be concerned about the results in past empirical studies. First, if individual health is a concave function of income, there will be a mechanical correlation at the aggregate level between inequality and health even if inequality has no effect on health (e.g. Miller 2001). To measure the effect of inequality on health it is therefore necessary to adjust for individual income. Since individual level data is rare most previous studies have relied on aggregated data and have therefore not been able to control for own income. Second, in spite of the advantages with individual level studies one problem is that the regional inequality measures used often have been estimated from few individual observations. It is possible that this approach generates measurement error which biases the estimator downwards (Deaton 2003). Third, and perhaps most importantly, the relationship between inequality and health may be spuriously driven by non-random sorting of individuals across regions. Alternatively,

health could also affect inequality if people with worse health are less able to work and therefore have lower earnings (e.g. Cutler, Lleras-Muney and Vogl 2010). Most previous studies control for potential confounders but in the absence of a controlled randomized experiment it is impossible to rule out that the observed relationship is not driven by omitted variables or reverse causality.

We circumvent these methodological problems using rich register data coupled with a Swedish refugee placement policy where authorities during the years 1985–1994 assigned newly arrived refugee immigrants to their initial area of residence. The institutional setup generates a setting in which it is plausible to assume that initial exposure to income inequality is randomly determined conditional on a few key individual characteristics. The policy has been used in several previous studies to investigate peer and neighborhood effects among immigrants (e.g. Edin, Fredriksson and Åslund 2003; Åslund and Fredriksson 2009; Åslund et al. 2011).

Our data originate from administrative records and cover the entire Swedish population age 16–65. The data contain the exact diagnosis on all individuals admitted to Swedish hospitals from 1987 to 2004, as well as a wide range of standard individual characteristics, income measures, and geographic identifiers. There is also information on year of death. We measure income inequality at the municipality level using disposable income. We employ several measures: the Gini Coefficient; the Coefficient of Variation; the (log) 90 to 10 percentile income ratio. Even though Sweden has a compressed income distribution our analysis focuses on a period in which the country was hit by a significant economic recession due to a major banking crisis (e.g. Englund

1999). The cross-municipal cross-year variation in our data is therefore large and its range spans the average Gini Coefficient in countries like the US and the UK.^{2 3}

Our work offers several innovations over the existing literature. Most importantly, this is the first study that uses a source of plausibly random variation in exposure to inequality to uncover the causal effect on health. The most convincing studies to date have instead relied on panel data to control for regional differences that may correlate with inequality and health (e.g. Mellor and Milyo 2002).

Another advantage is our data. The use of population registers minimizes the risk of measurement error both in health and in inequality. To the best of our knowledge only a handful of datasets exist that link hospital records to population registers and this is the first time that such data are used to study the relationship between income inequality and health.⁴

This is also the first paper to explicitly study immigrants. A common feature in many immigrant communities is that its members on average suffer from worse health and also reside areas with greater income inequality (e.g. Loue 1988). Even though this association does not necessarily imply a causal relationship it suggests one potentially important determinant of immigrant health.

A fourth improvement is that we are able to study whether the potential effect of inequality differs across subgroups of the population that may be more or less susceptible to negative health influences. We investigate groups that differ in terms of education, gender and age. Due to sample size restrictions no previous study has been able to do this.

² The Gini Coefficient in our data varies between .185 and .500. The within municipality variation in inequality amounts to about 40 percent of the overall variation.

³ In the year 2000, for instance, the Gini Coefficient in the US and the UK was about 35 and 32, respectively (OECD 2005).

⁴ Grönqvist (2009) uses similar data to study the effect of segregation on health.

Last, many countries, including the US, have in different ways tried to influence the settlement decision of their inhabitants. Some of the interventions have been evaluated in terms of their health consequences; e.g. the Moving to Opportunity Project (Kling, Katz and Liebman 2007) or the Gautreaux Assisted Housing Program (Vortuba and Kling 2009). Knowledge of how such interventions affect individuals' general well-being is of course important from a policy perspective. Our paper also adds to this literature.

The results suggest that a one standard deviation increase in any of our inequality measures increases the probability of being hospitalized by between .03 and 1.8 percent. This estimate corresponds to between 1/200 and 1/20 of the health gap between individuals with compulsory education versus university education. Although not statistically significant our estimates are precise enough to rule out that a one standard deviation increase in inequality raises the probability of being admitted to hospital by more than between 1.9 and 7 percent (between 1/20 and 1/5 of the educational health gap). We further find no significant effects investigating different types of diagnoses. In most subgroups that we study there is no evidence that inequality affects the risk of being hospitalized. There is however some weak evidence of a significant adverse effect on older persons' health; although the magnitude of the effect is small.

The paper unfolds as follows. Section 2 explains the institutional background surrounding the placement policy and the Swedish health care system. Section 3 describes our data and empirical strategy. Section 4 contains the results and Section 5 gives concluding remarks.

2. INSTITUTIONAL BACKGROUND

This section discusses institutional facts surrounding the settlement policy. We also briefly outline the Swedish health care system.

*2.1 Migration to Sweden and the settlement policy*⁵

Sweden's has a relatively large share of immigrants — about 14 percent of its 9 million residents are foreign-born. Since the 1970s the majority of the immigrants arriving are either refugees or family reunification migrants. Over the past decades, the economic performance of the migrants has been trending downwards to the extent that Sweden today has one of the largest immigrant-native labor market gaps among the OECD countries (OECD 2007). There is also a significant health gap between immigrants and natives. The probability of being hospitalized was for instance in 1994 almost 9 percent higher among immigrants.

As a way of reducing a strong geographic concentration of immigrants, the Swedish government enacted in 1985 a policy to assign newly arrived refugees to an initial municipality of residence. Because of the large inflow of asylum seekers in the late 1980s, the number of receiving municipalities was increased from 60 to include 277 of Sweden's 284 municipalities in 1989. The original idea was to put people in locations with good opportunities for providing work or education. However, at the time the housing market was booming, which meant that available public housing essentially came to determine the location. The policy encompassed all refugees who arrived during the period 1985–1994, except for family reunification migrants.

Following arrival, asylum seekers were placed in refugee centres, while waiting for the Immigration Board's ruling on whether or not to grant a residence permit. The centres were distributed all over Sweden and there was no link between the port of entry

⁵ This section draws heavily on Åslund et al. (2011).

to Sweden and the location of the center. In general, it took between three and twelve months to be approved. Upon admission, municipal placement usually occurred immediately. Refugee preferences were in some cases considered in the assignment, but because of the housing market boom individual requests were in practice given very little weight. The refugees were allowed to move if they found housing in another location but were still required to take part in an 18-month introduction program in their assigned municipality. During the introduction period the migrants received social assistance. Eight year after arrival about 50 percent were still living in their assigned municipality. The dispersal policy was later abolished due to large increases in the number of asylum seekers. In section 3.2 we discuss the arguments for why the placement policy provides exogenous variation in initial location.

2.2 The Swedish health care system⁶

The local county councils are the major financiers and providers of Swedish health care. There are 25 county councils and each council is obliged to provide its residents with equal access to health services and medical care. Health care is mostly financed through local taxes. Each county council sets its own patient fees but a national ceiling limits the total amount that a patient pays during a 12-month period (out-of-pocket). Thus, patient fees only account for about 3 percent of the total revenues. The daily fee for staying at a hospital is about USD 15. There is free choice of provider but referral is required in some cases, particularly when patients seek specialized care, or when they choose health care in another county. The county councils are allowed to contract private providers but the majority of the health care is performed by public agents. In their contacts with health care providers immigrants are entitled to an interpreter free of charge.

⁶ This brief outline of the Swedish health care system draws on the Swedish Association of Local Authorities and Regions (2005).

3. DATA AND EMPIRICAL STRATEGY

3.1 Data and sample selection

Our empirical analysis exploits micro data originating from administrative registers. The dataset, collected and maintained by Statistics Sweden, covers the entire Swedish population aged 16–65 during the period 1987–2000, and individuals aged 16–74 from 2001 through 2004. It contains annual information on a wide range of educational and demographic characteristics as well as different income sources.

Information on hospitalizations was provided by the National Board of Health and Welfare and covers all inpatient medical contacts at public hospitals from 1987 through 1996. This is no major restriction since virtually all medical care in Sweden at that time was performed by public agents. From 1997 and onwards the register also includes privately operated health care. In order for an individual to be registered with an health impairment (s)he must have been admitted to a hospital. As a general rule, this means that (s)he has to spend the night at the hospital. However, starting in 2002 the registers also cover outpatient medical contacts in the specialized care.

An important feature of the data is that it contains the cause of each admission. The diagnoses, made by physicians, are classified according to the World Health Organization's International Statistical Classification of Diseases and Related Health Problems (ICD). ICD is a four digit coding of diseases and signs, symptoms, abnormal findings, complaints, and external causes of injury or diseases.⁷ In our analysis we focus on several common diseases: ischemic heart disease, respiratory diseases, cancer, mental health problems and diabetes. Table A.1. outlines the different types of diagnoses and

⁷ The underreporting conditional on having been in contact with health care providers is very low and estimated to be less than one percent each year.

the way they have been constructed. Although the data include possible co-morbidities we only use the main diagnosis in our analysis.

Income is measured using disposable income (in 1990 year's prices), i.e. the universe of net income from work and capital combined with social benefits and transfers. The unit of analysis is the individual.⁸ We measure inequality for all individuals aged 25–65 using three distinct measures: (i) the Gini Coefficient; (ii) the Coefficient of Variation; (iii) the (log) 90 to 10 percentile income ratio. These measures represent some of the most commonly used ways to quantify inequality (e.g. Atkinson 1970). The Gini coefficient varies between 0 (complete equality) and 1 (complete inequality). It has several attractive properties one of which is that it is sensitive to income disparities throughout the distribution. The coefficient of variation is simply the standard deviation divided by its mean. Also this measure incorporates all data throughout the distribution. Although each measure has its own shortcomings together they should well portray local income inequality.

We compute these measures for each municipality and year.⁹ As discussed by Deaton (2003), in doing so we implicitly assume that people only compare themselves with individuals living in the same municipality. Even though alternative reference groups have been suggested (e.g. age, race or education as in Eibner and Evans 2005) the standard approach in the literature is to use geographically constrained groups.¹⁰

⁸ An alternative solution is to use household income. However, we cannot observe co-habitants in the data if the co-habiting couple does not have any children in common. Since co-habiting is frequent in Sweden this strategy would introduce measurement error. Moreover, using the individual as the unit of analysis is not as restrictive in Sweden as it may be in other countries because of the high female labor force participation rate.

⁹ The average municipality hosts about 30,000 inhabitants.

¹⁰ One alternative would be to measure inequality within municipalities across ethnic groups (e.g. Bertrand, Luttmer and Mullainathan 2000; Edin, Fredriksson and Aslund 2003). However for small source countries this would mean that our analysis relies on very few observations and that our measure of inequality therefore becomes too noisy.

Table A.2 displays descriptive statistics for our inequality measures and other selected variables.

We extract all immigrants aged 25–60 that arrived from a refugee sending country during the years 1990 to 1994.¹¹ Some countries have been aggregated due to confidentiality. The rationale for starting our analysis in 1990 is that this is when information on disposable income first becomes available. We exclude individuals with a spouse, child or parent already living in Sweden at the time of immigration since family reunification migrants were exempted from the placement policy.

While our data provide an objective measure of health that is not plagued by self-report bias and measurement error, one potential problem is that we only have information on health for individuals who have been hospitalized. First of all, this means that our analysis less likely extends to less severe morbidities. Potentially more serious is however that the likelihood of being admitted to hospital, conditional on health, may be correlated with local income inequality. This is true if doctors in municipalities with greater income inequality are less/more likely to admit patients, or if the inhabitants are less/more likely to seek medical care. In this case our estimator may be biased.^{12 13} In section 4.2 we discuss how we deal with this issue.

3.2 Using the settlement policy to identify the effect of inequality on health

¹¹ The placement policy was most strictly enforced in the period 1987 to 1991. In a sensitivity analysis we excluded cohorts who arrived after 1991 (results are available on request). Although the statistical precision decreases due to the smaller number of observations it is reassuring to find that the estimates are relatively stable and do not alter the conclusions in this paper.

¹² This can of course also be a problem in studies using data on self-reported health status if greater inequality for instance generates higher stress levels and thereby decreases an individual's possibilities to correctly assess his health.

¹³ The direction of the bias is ambiguous and depends on the correlation between true health, observed health, and inequality.

To estimate the effect of income inequality on health we exploit the Swedish refugee placement policy where authorities assigned newly arrived refugees to their initial location of residence. The policy has been carefully documented and used to examine the relationship between neighborhoods and immigrants' socioeconomic outcomes (e.g. Edin, Fredriksson and Åslund 2003; Åslund and Fredriksson 2009, Åslund and Rooth 2007; Åslund et al. 2011). We refer to these studies for a more comprehensive treatment of the policy.

As previously mentioned, the institutional arrangement implied that refugees were to be assigned their initial municipality of residence. Past studies provide convincing evidence that the policy actually created a geographic distribution that was independent of unobserved individual characteristics. For instance, Edin, Fredriksson and Åslund (2003) show that the residential area of those placed clearly differed from the location choices made by migrants arriving from the same regions shortly before the reform.

Despite this evidence it is important to note that local placement officers had information on the refugee and may have tried to match refugees to specific locations. Another issue is that refugees were allowed to state residential preferences. There are three arguments for why it still is possible to consider initial location as exogenous with respect to the unobserved characteristics of the individual. First, there was no direct interaction between the local placement officers and individual refugees so any selection by placement officers must have been on observed characteristics. Since our administrative registers contain the same set of information on the refugee as was available to the officer (age, education, gender, marital status, family size, country of origin) we are able to control for potential cream skimming. Second, the housing market boom severely restricted any residential preferences from being realized (e.g.

Fredriksson and Åslund 2009). Finally, the timing of the receipt of the residence permit must have coincided perfectly with the arrival of a housing vacancy in the preferred location in order for preferences to be realized. Since placement occurred rapidly after having received the permit the joint probability of these two events to occur at the same time must be considered as extremely low.¹⁴

It is difficult to test for random assignment since it requires a variable that was not observed by the officer (or at least unexploited). Instead we provide results which illustrate the differences in how well individual characteristics correlate with properties of the local area in the year of arrival versus five years after placement. During this period individuals will have had time to change residential area. Consequently, one would expect to find a stronger link between the individual and the municipality characteristics. Table 1 presents estimates from regressions where the dependent variable is some feature of the municipality measured in the year of arrival and then five years later. When looking at the results for year of arrival in Panel A we find only 3 out of 48 estimates significant at the five percent level. This is just slightly more than what we would expect to find by pure chance. When municipality characteristics instead are observed five years after placement we can see that 21 out of the 48 estimates are significant. These results clearly highlight the importance of accounting for non-random selection to uncover the causal effect of income inequality on health.

To take advantage of the plausibly exogenous variation in initial inequality we run regressions of following form by type of diagnosis

$$(1) \text{Hospitalized}_{ikt} = \alpha + \beta \text{Inequality}_{kt} + \gamma' \mathbf{X}_i + \delta' \mathbf{Z}_{kt} + \theta_k + \mu_t + \varepsilon_{ikt}$$

¹⁴ Oreopoulos (2003) use a similar argument when studying the effect of neighborhoods on adult outcomes for individuals who were assigned to different housing projects in Toronto.

where i denotes individual, k municipality, and t year of arrival. \mathbf{X}_i is a vector of individual characteristics controlling for dummies of: age, gender, marital status, educational attainment (6 levels) and country of origin; and linearly for disposable income (and its square) and number of children. \mathbf{Z}_{kt} represents a vector of time-varying municipality characteristics controlling for: (the log of) population size, share of university educated, and the unemployment rate. θ_k represents municipality fixed effects which absorbs all persistent municipal characteristics that may be related to health; e.g. access to fitness centers or environmental characteristics of the area. μ_t denotes year of arrival fixed effects. ε_{ikt} is by assumption conditionally independent of the covariates in the regression model, we however allow them to be correlated across individuals in the same municipality. We estimate models where the outcome is a dummy equal to one if the individual has been hospitalized at least once during a five years period after arrival. To ensure that our inequality measures are not suffering from non-random residential mobility they are dated in the year of immigration.

4. EMPIRICAL ANALYSIS

4.1 Main results

This section provides the results from our empirical analysis. Our baseline specification, given by equation (1), relates the probability of being hospitalized in five years following arrival to inequality in the assigned municipality. Throughout, estimates are reported for three inequality measures: the Gini Coefficient; the Coefficient of Variation; the (log) 90 to 10 income percentile ratio. To conserve space we suppress the estimates for the control variables (available on request). In general, these show a reduced risk of hospitalization for highly educated individuals, as well as for individuals

with more children, married people, younger individuals, and males. Since unobserved local factors are quite stable within municipalities over time we estimate the standard errors by clustering at the municipality level (Bertrand, Duflo and Mullainathan 2004).

Table 2 presents our main results. Numbers in brackets provide the effect of a one standard deviation increase in inequality on the probability of being hospitalized expressed in percent. Estimates are shown for all individuals in our sample (Panel A) and by population subgroup (Panels B to D). We focus on groups defined by highest completed level of education, gender and age at immigration. The reason is that we wish to investigate whether some groups are better or worse at coping with living in a high inequality area. Although it is well known that individuals with certain background characteristics such as low education are more susceptible to health insults (e.g. Cutler and Lleras-Muney and Vogl 2009) previous studies have not been able to investigate whether some groups respond differently to inequality.

In Panel A we can see that there is no statistically significant effect of inequality on the probability of being hospitalized for any of our inequality measures. The point estimate in column (1) suggests that a one standard deviation increase in the Gini Coefficient (.031) raises the probability of being hospitalized in five years after arrival by .5 percentage points ($.163 \times .031$). In relation to the mean of the dependent variable this translates into an increase in the order of 1.8 percent. The estimate in column (2) suggests that a one standard deviation increase in the Coefficient of Variation increases the likelihood of being admitted to hospital by .08 percentage points ($.002 \times .389$), or stated differently, by .03 percent. The corresponding numbers for the (log) 90 to 10 percentile income ratio are .04 percentage points ($.027 \times .147$) and 1.4 percent.

To interpret the magnitude of these estimates it is useful to compare the coefficients with the educational health gap. The educational gradient has repeatedly

been documented in many different countries and contexts (e.g. Cutler and Lleras-Muney 2010). In our sample individuals who have completed at least two years of university education are 9.5 percentage points less likely to be admitted to hospital in five years after arrival compared to individuals that only have finished compulsory school. Our estimates therefore suggest that a one standard deviation increase in our inequality measures corresponds to only roughly between 1/200 and 1/20 of the educational health gap.

Although not statistically significant, the estimates are precise enough to be able to rule out large effects. The upper limit of the 95 percent confidence intervals for our inequality measures is: .634, .014 and .144. These intervals suggest that a one standard deviation increase in inequality increases the probability of being hospitalized by at most between 1.9 and 7.6 percent. This constitutes about 1/20 to 1/5 of the educational health gap.

Before proceeding with the analysis note that we study a minority group that faces a considerable economic disadvantage.¹⁵ This is important since the theory suggests that any detrimental health effects are likely to be more pronounced for the least well off in society. Recall that the refugees in our sample were required to take part in an introductory program for 18 months during which time they received social assistance. Since most of the refugees therefore do not have other incomes than social assistance we cannot explicitly investigate whether relative income differences

¹⁵ OECD (2007) reports that Sweden is one of the countries with the largest native-immigrant gaps in the labor market.

matters.^{16 17} Yet, given the population of study our estimates are likely to capture both the strong and the weak income inequality hypotheses.

It is however possible to investigate whether the effect is stronger for individuals with lower income potential as proxied by low education. Panel B displays estimates by highest completed level of education. We can see that that there is no statistically significant effect of inequality on the probability of being hospitalized for individuals that at most have completed high school. Neither is there a significant effect for individuals with university education. As for the total sample the estimates are precise which makes it possible to rule out large effects.

Panel C shows results by gender. As we can see, there are no indications that income inequality affects the probability of being hospitalized in five years after arrival when for men or women.

In Panel D we split the sample by age at immigration using 40 as cut-off. Since young individuals are overrepresented in our sample we choose not to set a higher age limit. The results show that there is weak evidence that greater inequality increases the risk of being hospitalized among individuals who were at least age 40 when immigrating. The point estimate on the Gini Coefficient is statistically significant at the 10 percent level. The estimate suggests that a one standard deviation increase in the Gini Coefficient raises the probability of being hospitalized by about 2.8 percentage points ($.031 \times 911$). In relation to the mean of the dependent variable this means an 8 percent ($.028 / .346$) increase in the probability of being admitted to hospital. For the Coefficient

¹⁶ A related issue is that we observe annual disposable income but not the exact date the placement occurred. This means that refugees who were placed late (early) in the year mechanically will have lower (higher) recorded incomes.

¹⁷ Gerdtham and Johannesson (2004) are able to discriminate between the weak and strong income inequality hypotheses using Swedish data. They find no evidence for any of the hypotheses.

of Variation and the (log) 90 to 10 percentile income ratio the estimates are positive and just marginally insignificant at the 10 percent level.

Our data also allows us to separately investigate different diagnoses. We focus on some common illnesses which have been highlighted in the past literature as more likely linked to inequality (e.g. Wilkinson 1996; 1997). Table 3 presents the results from this analysis. It is clear that there is no statistically significant effect. Since the incidence of these diagnoses is low the precision of the estimates is not as good as in Table 2. For instance, the upper limit of the 95 percent confidence interval suggests that a one standard deviation increase in the Gini Coefficient raises that likelihood of being diagnosed with respiratory diseases by no more than 29 percent. Interesting is however that in some cases the sign on the coefficients is actually negative.

4.2 Robustness tests

Table 4 presents results from other sensitivity tests. Panel A asks whether the results are sensitive to how we have specified our regression model. One concern is that, even though we have plausibly exogenous variation in initial location, inequality could be correlated with other properties of the municipality that affect health. It is however important to note that our baseline model controls for all permanent differences across municipalities that may correlate with inequality and health. This raises the question if there are regional characteristics that change over time that may confound our results. To assess whether our results are likely to be driven by unobserved developing local factors we drop our set of time-varying municipal covariates: population size, unemployment rate and the share university educated. Presumably these variables are among the strongest factors linked to inequality and health. Interestingly is therefore that our baseline results (in Panel A in Table 2) are relatively stable when dropping these

controls. This suggests that other less important unobserved factors are not likely to explain our results.

Another way to investigate whether the results are sensitive to unobserved local shocks is to include county-by-year fixed effects in the regressions. This approach absorbs unobserved evolving factors that affect all individuals in a given county; for instance, changes in the quality of the local health care.¹⁸ The strategy is quite demanding in that it only relies on variation across municipalities within counties to identify the effect of inequality. It is reassuring to find that the estimates are stable when adding county-by-year fixed effects to our baseline model.

Remember that our regressions control for individual income and its square. Past studies have demonstrated the importance of controlling for own income to account for the fact that the aggregated measures of inequality can be mechanically linked to inequality (e.g. Miller 2001). In our setting this is not as vital since refugees only account for a small fraction of the municipality's total disposable income. Still, it is comforting to find that dropping controls for own income reveal no major changes in the point estimates.

As discussed earlier, one potential concern is that we only have health measures for individuals who were admitted to hospital. If there is systematic selection into medical care based on local inequality the results may be biased. Fortunately, the institutional setting is such that this problem may not be so severe, especially considering that we have access to detailed data. The Swedish health care system calls for the local county councils to provide its residents with equal access to medical care to

¹⁸ Note that we cannot include municipality by year fixed effects since this would remove all variation used to identify our parameter of interest.

very low fees. This is likely to weaken the financial incentives for selection into medical care.

There may still be some problem with the selection process if not captured by our covariates. Our estimation strategy is in this respect quite persuasive. The municipality fixed effects account for permanent differences in the quality of the local health care as well as the possibility that inhabitants may be more or less likely to seek medical care. Ethnic group fixed effects control for potential discrimination by the health care system towards specific ethnic groups in addition to any group specific differences in the propensity to seek medical care. The year fixed effects absorb annual shocks that are common for all individuals and correlates with health and inequality. Even though we believe that this is a rather convincing way of dealing with the potential problem one could still be concerned that there may be systematic selection into medical care based on unobserved local shocks. It is therefore reassuring that our analysis in Panel A showed that the estimates are not sensitive to controlling for annual shocks at the county level. Since Swedish health care policy is decided at the county level this finding suggests that such selection is not likely to be a problem.

To further show that selection into health care is no cause of concern we use two alternative health indicators that are less likely to be plagued by this potential problem. The first is the probability of taking long-term sick leave (> 13 days). Sick leave is not a perfect proxy for health since there could also be other factors influencing sick leave, for instance social norms (e.g. Hesselius, Johansson and Nilsson 2009). Nevertheless, in order for an individual in Sweden to receive sick pay it is necessary to see a doctor on the seventh day of job absence. Since a doctor's certificate is required it is reasonable to treat sick leave as a health indicator. And because an individual has economic incentives to go to the doctor to get the certificate it is less likely that an individual neglects visiting

a physician in the case of illness. We have information on sick leave starting in 1993. For that reason we cannot observe the outcome over a five year period as we have done so far. Instead we investigate the effect of initial inequality on the probability of taking out sick-leave in year five after arrival. The results in Panel B show no statistically significant effect of inequality on the probability of taking sick leave. These results support our earlier findings.

Because mortality is not likely to be influenced by selection into health care we also examine this health indicator. The dependent variable is defined as the probability of dying in five years after arrival. Also here we find no statistically significant effect. Note however that the mean of the dependent variable is quite low making the estimates imprecise.

We have also experimented with using the average number of days admitted to hospital in a five year period after arrival as an outcome. The concern was that we may lose valuable information by only examining health at the extensive margin. It is evident from the results that there is no statistically significant effect of inequality on the number of days spent in hospital care. Note however the negative sign on two of the coefficients.

One possible objection to our results is that it may take some time for immigrants to learn about the nature of inequality in their municipality of residence. To investigate if this is the case we have examined if the effect gets stronger later during our observation period (results available on request). The results from this exercise suggest that this is not the case.

We also investigated whether there is a non-linear effect of inequality on health by adding squared terms to our regression model. Panel C shows no evidence of such a relationship.

The past literature has raised the question what geographic unit is appropriate to measure inequality for (e.g. Deaton 2003). To examine whether our results are sensitive to the level of aggregation we experimented with measuring inequality at the parish level. There are about 1,500 parishes in Sweden on average hosting about 6,000 inhabitants. We ran the same set of regressions as in Table 2. The estimates from this exercise are very similar to our main results.

Theory does not teach us what income concept should be used. It is possible that social status is more strongly linked to income from work. We investigated this by re-estimating our models using gross labor earnings to measure inequality. It turns out that the results were very similar.

4.3 Estimating the effect of long-term exposure to inequality

So far, the aim of this paper has been to estimate the “reduced form” (or intention to treat) effect of assigned inequality on health. This parameter is especially important for policy makers trying to weight the costs and benefits of similar settlement policies. To the extent that initial inequality provides a good proxy for individuals’ actual exposure over a longer period our estimates also incorporate the impact of long-term exposure to inequality. Of course, this is only true as long as individuals do not change residential area over time. Even though many of the theoretical foundations of the income inequality hypothesis seem to be more applicable for long-term exposure the focus in the previous literature has exclusively been on single year measures of inequality and health.

To estimate the effect of actual long-term exposure to inequality we use assigned inequality as an instrument for average exposure over a five year period after arrival. As before, our outcome of interest is the probability of being hospitalized in a five year

period after arrival. One concern with the instrumental variable (IV) approach is however that it requires that initial exposure has no direct effect on health other than through average exposure. Heckman (2007) and others have argued that inputs in the health production functions at different points in time are complementarities. If initial exposure to inequality affects the dynamic accumulation of health capital the IV strategy is no longer valid. This is true if initial exposure to inequality causes poor health which in turn increases the risk of subsequent co-morbidities.

With this caveat in mind Table 5 presents our IV estimates. As is common in IV analyses the statistical precision is rather poor so we are no longer able to rule out large effects of inequality on health. If we instead focus on the point estimates we can see that a one standard deviation increase in inequality raises the probability of being hospitalized in the full sample by between 1.7 and 4.5 percent depending of the inequality measure used. The estimates are similar to those in our main results. Also when looking at different subgroups the IV estimates resembles our reduced form estimates. This is natural since the first-stage relationships are quite strong. For example, the coefficient of initial inequality is just below .4. The statistical precision of the first-stage relationship is good and discards any potential concerns of weak instruments.¹⁹ To summarize, even though there is a great deal of uncertainty in our IV estimates the coefficients are quite in line with our previous analysis suggesting small health effects.

5. CONCLUDING REMARKS

A large number of studies have shown that greater income inequality is associated with adverse health outcomes. Investigations of this question are complicated due to the requirements of high quality individual level data and methods to account for non-

¹⁹ All estimates are available from the authors upon request.

random residential sorting and reverse causality. In this study we address these problems using rich administrative hospitalization data together with a settlement policy where Swedish authorities distributed newly arrived refugee immigrants to an initial area of residence. The policy provides a source of plausibly exogenous variation in exposure to inequality that makes it possible to uncover the causal effect on health. In contrast to most previous studies we can rule out large effects from inequality on health.

Given that the findings contradict those in many past studies it is relevant to ask whether our results are an artifact of the specific context in which our analysis is preformed. In comparison with other countries Sweden has traditionally been considered as an egalitarian country (e.g. Aaberge et al. 2002). The country has an extensive welfare state, which among other things, encompasses publicly financed health care, schools, pensions, elder care, and social services. There are also many different forms of income support. Could this institutional setting compensate for the potential detrimental effect of inequality on health?

In this respect it is important to remember that while other studies focus on the total population within a community, we study a group that is significantly socioeconomic disadvantaged. Many of the theoretical predictions suggest that less affluent groups should be more affected by inequality. It is therefore remarkable that we find so limited evidence that inequality affects health. Equally noteworthy is that there is no effect of inequality on health even when studying individuals with worse socioeconomic status (i.e. less education) within this disadvantaged group.

Still, even though the results are internally valid it is important to bear in mind that refugees represent a special group of individuals which might limit the scope of generalizing the results to a larger population. One issue for instance is that income inequality in the country of origin might have been considerably larger than the level of

inequality experienced in Sweden. It is not implausible to think that individuals that have been subject to high levels of inequality might not respond to the relatively lower levels they are exposed to in Sweden. A related question is that refugees may lack a reference group. It is likely that refugees at least initially do not compare themselves to native Swedes in their municipality of residence; for instance due to language barriers. Even though we examine exposure to inequality over a five year period after arrival this time need not be sufficient to allow for an adjustment in the choice of reference group. Although it is beyond the scope of this paper it would certainly be interesting to investigate the link between assimilation and potential changes in reference group.

It is also conceivable that inequality does not matter in a setting where equality of opportunity is large. Compared to for instance the US, Sweden has significantly higher intergenerational mobility (e.g. Björklund and Jäntti 1997).²⁰ Although this is undeniably an interesting question to explore it is beyond the scope of this paper.

It is often claimed that income inequality imposes negative external effects on society (e.g. Wilkinson 1996). These concerns have been used to motivate the introduction or expansion of various kinds of redistributive policies. Although there indeed may be some forms of external costs associated with income inequality our findings suggest that health is not likely to be one of them.

²⁰ Among immigrants in Sweden there is evidence that the intergenerational transmission of education is only slightly higher compared to natives (Niknami 2010).

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Table A.1 Classification of diagnoses included in the analysis

Type of diagnosis	ICD classification	Common diagnoses included in the category
Respiratory diseases	J00–J99	Asthma, pneumonia
Mental diseases	F00–F99	Psychosis
Cancer	C00–D48	
Ischemic heart conditions	I20–I25	Myocardial infarction
Diabetes	E10–E14	

Table A.2 Summary statistics for selected variables

Variable	Mean	Std. dev.	Min	Max
<i>Individual characteristics</i>				
Hospitalized in five years after arrival	.283	.451		
Female	.469	.499		
Married	.723	.447		
Age at immigration	35.80	8.70	25	60
Number of children	1.30	1.47	0	12
Compulsory school	.282	.450		
At most two years high school	.138	.345		
At least two years high school	.255	.436		
At most two years university	.134	.340		
At least two years university	.172	.377		
<i>(Initial) Regional characteristics</i>				
Gini Coefficient	.244	.031	.185	.500
Coefficient of Variation	.732	.389	.347	6.122
log(P90/P10)	1.069	.147	.820	2.019

Notes: The sample consists of refugee migrants age 25–60 at arrival that immigrated 1990–1994 (N=66,871). If not stated otherwise all variables are measured in the year of immigration. Summary statistics on education is conditional on that information is available.

Table 1 OLS estimates from balancing tests regressing initial municipal properties on individual characteristics

	Dependent variable					
	Gini	CV	log (P90/P10)	log(Pop. size)	log(Unem. rate)	log(Univ. share)
	(1)	(2)	(3)	(4)	(5)	(6)
	<u>A. Year of arrival</u>					
Age at immigration*10 ³	.002 (.004)	.032 (.130)	.004 (.018)	.007 (.009)	.030 (.035)	.002 (.009)
Female*10 ³	-.033 (.046)	.443 (1.532)	-.397 (.206)	-.243* (.101)	-.319 (.382)	.115 (.112)
Married*10 ³	-.217 (.175)	-1.407 (3.691)	-.953 (.946)	-.239 (.271)	.666 (.921)	.274 (.191)
Number of children*10 ³	.004 (.037)	1.013 (.604)	.039 (.194)	-.189 (.082)	-.305 (.885)	-.051 (.068)
Ref. Compulsory school						
At most two years high school*10 ³	.367 (.192)	10.94* (5.00)	1.122 (.831)	.538 (.348)	1.395 (1.025)	.296 (.218)
At least two years high school*10 ³	.161 (.120)	.214 (3.79)	.452 (.558)	.402 (.219)	.522 (.935)	.245 (.177)
At most two years university*10 ³	.279* (.135)	6.471 (4.408)	.556 (.633)	.225 (.288)	1.462 (.898)	.016 (.198)
At least two years university*10 ³	.190 (.112)	.619 (3.579)	.509 (.496)	.305 (.197)	-.305 (.885)	.201 (.184)
	<u>B. Five years after arrival</u>					
Age at immigration*10 ³	-.018 (.030)	-.844 (.993)	-.020 (.114)	1.179 (.936)	.480* (.110)	.197 (.180)
Female*10 ³	-.319 (.274)	-6.645 (8.326)	-1.378 (1.099)	-11.2 (10.47)	-1.605 (.963)	3.710* (1.877)
Married*10 ³	-827* (.406)	-10.14 (14.51)	-4.933* (1.641)	-40.69* (16.031)	-7.581* (2.332)	-7.100* (3.263)
Number of children*10 ³	-.397* (.134)	-11.33* (4.029)	-.973 (.616)	-14.69* (5.334)	1.441 (.755)	-3.636* (1.118)
Ref. Compulsory school						
At most two years high school*10 ³	-1.032 (.647)	-2.852 (20.24)	-4.706 (2.591)	-27.11 (22.60)	-1.3917 (2.395)	-11.53* (4.858)
At least two years high school*10 ³	.627 (.513)	3.104 (16.40)	2.671 (2.127)	22.41 (18.71)	-1.2301 (2.039)	10.19* (3.631)
At most two years university*10 ³	1.074* (.483)	12.81 (15.56)	4.663* (2.014)	54.01* (17.91)	1.167 (2.311)	24.09* (4.196)
At least two years university*10 ³	2.434* (.483)	8.038 (14.56)	8.952* (2.172)	74.00* (17.96)	1.249 (2.543)	34.81* (5.501)

Notes: Each column represents a separate regression. All coefficients and its standard errors have been multiplied by 10³. The sample consists of refugee migrants age 25–60 at arrival that immigrated 1990–1994 (N=66,871). All regressions control for municipality, year of arrival and ethnic group fixed effects. Standard errors are clustered at the municipality level in parentheses. * = significant at 5 % level

Table 2 OLS estimates of the effect of initial inequality on the probability of being hospitalized in five years after arrival

Sample	Inequality measure		
	Gini (1)	CV (2)	log(P90/ P10) (3)
A. Total sample (N=66,871; Outcome mean .283)	.163 (.240) [1.8%]	.002 (.006) [.03%]	.027 (.059) [1.9%]
B. <i>Education</i>			
University (N=18,273; Outcome mean .245)	.034 (.520) [.5%]	-.001 (.015) [.2%]	.067 (.105) [4.2%]
High school or less (N=48,598; Outcome mean .298)	.176 (.261) [1.8%]	.003 (.007) [.4%]	.012 (.064) [.6%]
C. <i>Gender</i>			
Females (N=31,369; Outcome mean .316)	.088 (.378) [1.7%]	-.002 (.010) [-.02%]	.068 (.081) [2.8%]
Males (N=35,502; Outcome mean .254)	.206 (.308) [2.5%]	.005 (.009) [.7%]	-.010 (.075) [-.6%]
D. <i>Age at immigration</i>			
Less than 40 (N=48,484; Outcome mean .260)	-.078 (.247) [-.9%]	-.004 (.008) [-.6%]	-.032 (.064) [-1.7%]
At least 40 (N=18,387; Outcome mean .346)	.911* (.520) [8.2%]	.023 (.016) [1.9%]	.174 (.112) [7.1%]
Municipality FE:s	Yes	Yes	Yes
Contry of origin FE:s	Yes	Yes	Yes
Year of arrival FE:s	Yes	Yes	Yes

Notes: Each cell represents a separate regression. Inequality is measured at the (initial) municipality level using disposable income. The sample consists of refugee migrants age 25–60 at arrival that immigrated 1990–1994. The regressions controls with dummies for age at immigration, educational attainment (five levels), gender, marital status, missing values, and linearly for disposable income (and its square) and family size. The regressions include municipality level controls for the unemployment rate, population size, and share of university educated; all entered in logs. Standard errors clustered at the municipality level in parentheses. ** = significant at 5 % level; * = significant at 10 % level.

Table 3 OLS estimates of the effect of initial inequality on the probability of being hospitalized in five years after arrival by type of diagnosis

Dependent variable = 1 if diagnosed with	Inequality measure		
	Gini (1)	CV (2)	log (P90/P10) (3)
Respiratory diseases (Outcome mean .026)	-.081 (.083) [-9.7%]	-.003 (.002) [-4.5%]	-.016 (.018) [-9.0%]
Mental disorders (Outcome mean .026)	.102 (.095) [12.1%]	.005 (.003) [7.5%]	.013 (.022) [7.4%]
Cancer (Outcome mean .017)	.001 (.076) [.2%]	-.002 (.001) [-4.6%]	.013 (.016) [11.2%]
Ischemic heart diseases (Outcome mean .009)	.064 (.052) [22%]	.001 (.002) [4.3%]	.013 (.011) [21.3%]
Diabetes (Outcome mean .006)	-.034 (.051) [-17.6%]	.001 (.002) [6.5%]	-.006 (.010) [-14.7%]
Municipality FE:s	Yes	Yes	Yes
Contry of origin FE:s	Yes	Yes	Yes
Year of arrival FE:s	Yes	Yes	Yes

Notes: Each cell represents a separate regression. Inequality is measured at the (initial) municipality level using disposable income. The sample consists of refugee migrants age 25–60 at arrival that immigrated 1990–1994 (N=66,871). The regressions controls with dummies for age at immigration, educational attainment (five levels), gender, marital status, missing values, and linearly for disposable income (and its square) and family size. The regressions include municipality level controls for the unemployment rate, population size, and share of university educated; all entered in logs. Standard errors clustered at the municipality level in parentheses. ** = significant at 5 % level; * = significant at 10 % level.

Table 4 Sensitivity analysis of the effect of initial inequality on the probability of being hospitalized in five years after arrival (OLS estimates)

	Inequality measure		
	Gini Coefficient (1)	Coefficient of Variation (2)	log (P90/P10) (3)
Baseline estimate (as in Panel A of Table 2)	.163 (.240)	.002 (.006)	.027 (.059)
<i>A. Change in specification</i>			
Removing regional level controls	.076 (.238)	.002 (.006)	.003 (.057)
Including county*year FE:s	.042 (.331)	.001 (.006)	-.047 (.090)
Removing control for own income	.116 (.241)	.001 (.006)	.018 (.060)
<i>B. Change in outcome</i>			
Pr(Sick leave in year five after arrival) (Outcome mean .059)	.045 (.163)	.007 (.005)	-.030 (.044)
Pr(Died in five years after arrival) (Outcome mean .008)	-.008 (.046)	.000 (.001)	.000 (.010)
Average number of days hospitalized (Outcome mean 3.62)	-13.46 (20.38)	.488 (.479)	-5.02 (5.29)
<i>C. Non-linear effects</i>			
Inequality	.284 (.897)	-.007 (.014)	-.066 (.302)
Inequality squared	-.203 (1.423)	.003 (.003)	.037 (.115)
Municipality FE:s	Yes	Yes	Yes
Contry of origin FE:s	Yes	Yes	Yes
Year of arrival FE:s	Yes	Yes	Yes

Notes: Each cell represents a separate regression. Inequality is measured at the (initial) municipality level using disposable income. The sample consists of refugee migrants age 25–60 at arrival that immigrated 1990–1994 (N=66,871). The regressions controls with dummies for age at immigration, educational attainment (five levels), gender, marital status, missing values, and linearly for disposable income (and its square) and family size. The regressions include municipality level controls for the unemployment rate, population size, and share of university educated; all entered in logs. Standard errors clustered at the municipality level in parentheses. ** = significant at 5 % level; * = significant at 10 % level.

Table 5 IV estimates of the effect of average exposure to inequality on the probability of being hospitalized within a five year period after arrival

Sample	Inequality measure		
	Gini (1)	CV (2)	log(P90/ P10) (3)
A. Total sample (N=66,871; Outcome mean .283)	.413 (.596) [4.5%]	.010 (.031) [1.7%]	.067 (.146) [3.4%]
<i>B. Education</i>			
University (N=18,273; Outcome mean .245)	.089 (1.358) [1.2%]	-.008 (.081) [-1.7%]	.175 (.266) [10.7%]
High school or less (N=48,598; Outcome mean .298)	.441 (.643) [4.4%]	.016 (.035) [2.0%]	.030 (.155) [1.4%]
<i>C. Gender</i>			
Females (N=31,369; Outcome mean .316)	.226 (.956) [2.2%]	-.012 (.055) [-1.9%]	.171 (.207) [7.7%]
Males (N=35,502; Outcome mean .254)	.524 (.783) [6.4%]	.027 (.047) [4.1%]	-.025 (.186) [-1.4%]
<i>D. Age at immigration</i>			
Less than 40 (N=48,484; Outcome mean .260)	-.209 (.661) [-2.4%]	-.020 (.045) [-3.4%]	-.080 (.160) [-4.4%]
At least 40 (N=18,387; Outcome mean .346)	2.118* (1.179) [19.0%]	.118 (.091) [17.3%]	.430 (.280) [17.6%]
Municipality FE:s	Yes	Yes	Yes
Contry of origin FE:s	Yes	Yes	Yes
Year of arrival FE:s	Yes	Yes	Yes

Notes: Each cell represents a separate regression. The endogenous variable is an individual's average exposure to inequality starting in the year of arrival and ending five years later. The excluded instrument is the assigned inequality level. Inequality is measured at the municipality level using disposable income. The sample consists of refugee migrants age 25–60 at arrival that immigrated 1990–1994. The regressions controls with dummies for age at immigration, educational attainment (five levels), gender, marital status, missing values, and linearly for disposable income (and its square) and family size. The regressions include municipality level controls for the unemployment rate, population size, and share of university educated; all entered in logs. Standard errors clustered at the municipality level in parentheses. ** = significant at 5 % level; * = significant at 10 % level.