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Causes of Inequality in Health and Economic Resources:

Empirical Essays in Economics

Ph.D. dissertation

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Advisors: Niels Johannesen & Asger Lau Andersen

Submitted: November 30, 2020

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Acknowledgements

Writing the introduction to this thesis made me realize how much I have learned during my time as Ph.D. student and yet how little I know. I am grateful that I got this opportunity to spend three years on becoming a better economist and immerse myself into topics of my interest.

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Mia Herløv Jørgensen
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Introduction

This dissertation considers some of the causes of inequality in income, wealth and health. The dissertation consists of three self-contained chapters which all study different but important dimensions of inequality in developed economies. The chapters therefore naturally differ both in the research question and their empirical approach. However, common to all the chapters is that they apply modern micro-econometric techniques on high quality administrative data to provide explanations of why some individuals have better health or are more affluent than others.

The first chapter studies the health impacts on health of living in a low-income neighborhood. Specifically, we examine if living in a low-income neighborhood increases the risk of developing a lifestyle related disease such as diabetes or hypertension. To answer this question we exploit that the Danish Spatial Dispersal Policy resettled refugees quasi-randomly across neighborhoods from 1986 to 1998. This natural experiment allows us to isolate the causal health impact of living in a low-income neighborhood from self-selected settlement patterns. We find that refugees who were placed in the poorest neighborhoods were more likely to develop lifestyle related diseases and that the impact was stronger among women. This cannot be explained by employment and income differences across neighborhoods and we find no evidence that access to health care or the presence of ethnic networks explain the negative health consequences of living in a low-income neighborhood. Our findings suggest that health behavior of close neighbors or the characteristics of the very local area are important to understanding how neighborhoods impact the health of their residents. The chapter is co-authored with Linea Hasager.

In the second chapter we study if there is a connection between individuals' debt and their mental health. Specifically we examine if individuals with high leverage are more likely to develop mental health problems upon an adverse shock. To answer this we study mental health problems following a somatic inpatient hospitalization using an event study model, where we allow the mental health response to depend on the individual's leverage prior to the hospitalization. Using this model we show that individuals with higher leverage are more likely to suffer from mental health problems after they experience a health shock than individuals with lower leverage. We also show that following an adverse health shock individuals with high leverage are more likely to be in arrears on their loans despite experiencing the same income loss as individuals with lower leverage. This indicates that there exists a link from debt to mental health, where the mechanism is that since individuals with high leverage have more difficulty in mitigating income losses by acquiring new debt, they are at higher risk of experiencing financial distress associated with negative emotions such as stress or anxiety. Thus, our findings suggest that rising household debt potentially has implications for public mental health. The chapter is co-authored with Asger Lau Andersen, Rajkamal Iyer, Niels Johannesen and José-Luis Peydró.

The third chapter is concerned with how monetary policy affects income and wealth across the income distribution. To isolate the impact of monetary policy from other macroeconomic shocks we exploit exogenous monetary policy rate changes determined by the Danish currency peg. We document that softer monetary policy creates income and asset value gains for all income groups but that gains disproportionately benefit the top income groups. Common to all income groups, however, is that the income gains are small in comparison with the asset value gains. In the chapter we examine the direct and indirect channels of monetary policy and how components of income and wealth contribute to the income gradient in the gains created by softer monetary policy. Our results show that the income gradient reflects differences between income groups both in the composition of income and wealth but also in the individual component's response to a monetary softening. Our estimations also suggest that softer monetary policy is associated with an unequal distribution of consumption gains. Our findings are important for understanding both the distributional consequences, the transmission mechanism, and the aggregate effects of monetary policy changes. The chapter is co-authored with Asger Lau Andersen, Niels Johannesen and José-Luis Peydró.

For convenience the three abstracts are listed below.

Part 1: Sick of Your Poor Neighborhood? Quasi-Experimental Evidence on Neighborhood Effects in Health

with Linea Hasager

Does living in a low-income neighborhood have negative health consequences? We document neighborhood effects in health by exploiting a Spatial Dispersal Policy that resettled refugees quasi-randomly across neighborhoods from 1986 to 1998, which allows us to separate causal impacts from selection into neighborhoods. We show that the risk of developing a lifestyle related disease before 2018 increases by 5.1 percent relative to the sample mean for individuals who were allocated to the poorest third of neighborhoods compared to allocation to the richest third of neighborhoods. In particular, among women the impact of neighborhood income on health is larger. We find no evidence that our results can be explained by differences across neighborhoods in access to health care, presence of ethnic networks, employment or individual income growth differences. Instead, our results suggest that interaction with immediate neighbors and the characteristics of the very local environment are important for understanding neighborhood effects in health.

Part 2: Household Debt and Mental Health

with Asger Lau Andersen & Rajkamal Iyer & Niels Johannesen & José-Luis Peydró

Does debt have negative consequences for mental health? We answer this question by comparing mental health responses to adverse shocks across individuals with different ex ante levels of debt. The increase in the incidence of mental health problems following a somatic health shock is 45% larger for individuals who are initially more levered than for those with low debt. Differences between high and low debt individuals are especially strong for outcomes indicating severe depression. We present evidence consistent with the hypothesis that these differences are due to high debt amplifying the negative financial consequences of health shocks.

Part 3: Monetary Policy and Inequality

with Asger Lau Andersen & Niels Johannesen & José-Luis Peydró

We analyze the *distributional effects* of monetary policy on income, wealth and consumption. For identification, we exploit administrative household-level data covering the entire population in Denmark over the period 1987-2014, including detailed information about income and wealth from tax returns, in conjunction with exogenous variation in the Danish monetary policy rate created by a long-standing currency peg. Our results consistently show that all income groups gain from a softer monetary policy, but that the gains are monotonically increasing in the ex-ante income level. Over a two-year horizon, a decrease in the policy rate of one percentage point raises disposable income by less than 0.5% at the bottom of the income distribution, by around 1.5% at the median income and by around 5% at the top. Moreover, the effects on asset values through increases in house prices and stock prices are larger than the effects on disposable income by more than an order of magnitude and exhibit a similar monotonic income gradient. We show how all these distributional effects reflect systematic differences in the exposure to the direct (e.g. leverage) and indirect (e.g. business income) channels of monetary policy. Consistent with the main results for disposable income and asset values, we also find that the effects on net wealth and consumption (car purchases) increase monotonically over the ex-ante income distribution. Our estimates imply that softer monetary policy increases income inequality by raising income shares at the top of the income distribution and reducing them at the bottom.

Introduktion

Denne afhandling undersøger forskellige årsager til ulighed i indkomst, formue og sundhed. Afhandlingen består af tre selvstændige kapitler, der undersøger vigtige aspekter af ulighed i udviklede lande. Hvert kapitel koncentrerer sig derfor om et selvstændigt forskningsspørgsmål relateret til ulighed og de anvender derfor forskellige empiriske metoder. De tre kapitler har dog det tilfælles, at de anvender moderne mikroøkonometriske metoder på højkvalitets administrative data til at give forklaringer på, hvorfor nogle mennesker har et bedre helbred eller er mere velhavende end andre.

I det første kapitel undersøger vi, om det påvirker helbreddet at bo i et lavindkomstområde. Mere specifikt undersøger vi, om det at bo i et lavindkomstnabolag øger sandsynligheden for at udvikle livsstilsrelaterede sygdomme såsom diabetes og forhøjet blodtryk. For at kunne undersøge dette udnytter vi, at Dansk Flygtningehjælp fandt boliger til flygtninge kvasi-tilfældigt på tværs af nabolag fra 1986 til 1998. Dette naturlige eksperiment gør det muligt for os at isolere de kausale helbredseffekter af at bo i et lavindkomstnabolag fra selvvalgte bosætningsmønstre. Vores resultater viser, at flygtninge, der blev boligplaceret i den fattigste tredjedel af alle nabolag, havde en øget risiko for at udvikle livsstilsrelaterede sygdomme, og at effekten var stærkere blandt kvinder. Dette skyldes ikke, at deres indkomstudvikling eller beskæftigelse blev påvirket af, hvor de blev boliplaceret. Vores resultater tyder desuden ikke på, at den øgede risiko for livsstilsrelaterede sygdomme skyldes dårligere adgang til sundhedsvæsenet eller tilstedeværelsen af etniske netværk i de fattigste nabolag. Derimod peger vores resultater i retning af, at det især er sundhedsadfærd fra de nærmeste naboer og nærmiljøets karakteristika, der er afgørende for at forstå, hvordan nabolag påvirker deres indbyggeres sundhed. Kapitlet er skrevet i samarbejde med Linea Hasager.

I det andet kapitel undersøger vi, om der findes en sammenhæng mellem en individers gæld og deres mentale sundhed. Helt konkret undersøger vi, om individer med høj gældsfaktor er mere tilbøjelige til at udvikle mentale helbredsproblemer, når de oplever et økonomisk chok. For at besvare dette følger vi individers mentale sundhed før og efter en somatisk indlæggelse i en eventstudie model, hvor vi tillader, at effekten på mental sundhed afhænger af individets gældsfaktor før indlæggelsen. Ved at anvende denne model viser vi, at individer med en høj gældsfaktor er mere tilbøjelige til at udvikle mentale helbredsproblemer, efter de oplever et helbredschok end individer med en lavere gældsfaktor. Vi viser også, at individer med høj gældsfaktor efter helbredschokket oftere er i restance på deres lån på trods af, at de oplever indkomsttab i samme omfang som individer med lavere gældsfaktor. Dette indikerer, at der eksisterer et link fra gæld til mental sundhed, hvor mekanismen er, at da individer med høj gældsfaktor har sværere ved at afbøde indkomsttab ved at opnå ny gæld, har de større risiko for at komme i økonomisk vanskeligheder, der er forbundet med negative følelser som stress eller angst. Således tyder vores resultater på, at voksende husholdningsgæld potentielt kan have konsekvenser for den mentale folkesundhed. Kapitlet er skrevet i samarbejde med Asger Lau Andersen, Rajkamal Iyer, Niels Johannesen og José-Luis Peydró.

Det tredje kapitel i afhandlingen undersøger, hvordan pengepolitik påvirker husholdningers indkomst og formue på tværs af indkomstfordelingen. I kapitlet isolerer vi effekten af pengepolitik fra andre makroøkonomiske effekter ved at udnytte eksogene ændringer i den pengepolitiske rente som følge af den danske fastkurspolitik. Vores undersøgelser viser, at en pengepolitisk lempelse øger indkomsten og giver formueværdistigninger for alle indkomstgrupper, men at gevinsterne er størst i toppen af indkomstfordelingen. Fælles for alle indkomstgrupper er dog, at indkomststigningerne er små i sammenligning med formueværdistigningerne. I kapitlet undersøger vi de direkte og indirekte pengepolitiske kanaler, og hvordan indkomst- og formuekomponenter bidrager til indkomstgradienten i gevinsterne ved en pengepolitisk lempelse. Vores resultater viser, at indkomstgradienten både afspejler forskelle mellem indkomstgrupperne i indkomst- og formuesammensætningen, men også i hvordan hver af de enkelte komponenter bliver påvirket af en pengepolitisk lempelse. Vores undersøgelse indikerer desuden, at en ekspansiv pengepolitik er forbundet med ulige fordelte forbrugsgevinster. Vores resultater bidrager med vigtig viden i forhold til at forstå de fordelingsmæssige konsekvenser, den pengepolitiske transmissionsmekanisme og den samlede effekt af pengepolitiske ændringer. Kapitlet er skrevet i samarbejde med Asger Lau Andersen, Niels Johannesen og José-Luis Peydró.

Chapter 1

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

Sick of your poor neighborhood? *

Quasi-experimental evidence on neighborhood effects in health

Linea Hasager[†] Mia Jørgensen[‡]

November 26, 2020

Abstract

Does living in a low-income neighborhood have negative health consequences? We document neighborhood effects in health by exploiting a Spatial Dispersal Policy that resettled refugees quasi-randomly across neighborhoods from 1986 to 1998, which allows us to separate causal impacts from selection into neighborhoods. We show that the risk of developing a lifestyle related disease before 2018 increases by 5.1 percent relative to the sample mean for individuals who were allocated to the poorest third of neighborhoods compared to allocation to the richest third of neighborhoods. In particular, among women the impact of neighborhood income on health is larger. We find no evidence that our results can be explained by differences across neighborhoods in access to health care, presence of ethnic networks, employment or individual income growth differences. Instead, our results suggest that interaction with immediate neighbors and the characteristics of the very local environment are important for understanding neighborhood effects in health.

JEL Classification: J15, I12, I14, I31

Keywords: Health inequality, Refugee Dispersal Policy, lifestyle related diseases, neighborhood effects

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I Introduction

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 do 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. For example, because amenities in low-income areas do not support healthy lifestyle choices or because unhealthy behaviors are transmitted between neighbors. In other words, living in a low-income area can affect health negatively.

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

In this paper, we exploit quasi-random assignment of refugee families to local areas in Denmark to overcome these challenges and 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 in health. To do so, we exploit a natural experiment created by a Danish Spatial Dispersal Policy in act from 1986 to 1998, which 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

[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 Appendix, which shows 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.

[3] There exists only limited evidence on neighborhood effects in 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.

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 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.[6] 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. 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, which 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] We can therefore rule out any income effects of neighborhood placement, and this allows us to attribute the

[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.

[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).

estimated health effects to neighborhood income rather than 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 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 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 characterization 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 become. This suggests that transmission of behaviors from neighbors and local amenities are part of the mechanisms through which neighborhoods affect residents' health.

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 true effect size due to varying detection rates across areas. Correlational evidence shows that a larger share of residents in richer neighborhoods visit their GP or dentist in a given year, see Panels g-h in Figure A.1 in Appendix.[10] This may result in lower detection rates in poorer neighborhoods which will lead to a downward bias in our estimates.

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

[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)).

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

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 Opportunity* experiment only provides limited evidence on neighborhood effects in 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)).[11]

The literature also includes non-experimental evidence on neighborhood effects in health, for example in mental health among social housing clients, in life expectancy among elderly, and in diabetes among refugees (see Boje-Kovacs, Greve, and Weatherall (2018), Finkelstein, Gentzkow, and Williams (2019), and White et al. (2016), respectively).[12]

We contribute to the literature on neighborhood effects in health in two ways. The first part of our contribution is to document the existence of strong and significant causal long-term neighborhood effects in a wide range of lifestyle related diseases, since such evidence on neighborhood effects in 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 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 II. 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 III, describing a reduced form approach and an instrumental variables strategy. In Section IV we describe the data sources, sample selection and the definition of our main variables

[11]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.

[12]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.

of interest. Following that, Section V provides an overview of our results which shows an increased risk of developing lifestyle related diseases as a consequence of living in a low-income neighborhood. In Section VI we test a number of potential mechanisms and show the importance of the very local environment. Finally, Section VII concludes the paper.

II 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 had mainly found housing 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 stairways of 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.[13] We will refer to this information as 'questionnaire observables'. This questionnaire contained all 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 housing opportunities using the information about family size to find housing of a suitable size.[14] 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 housing opportunities. 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 on questionnaire observables were similar, but were allocated to neigh-

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

[14] 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 refugee within the assigned municipality.

borhoods with different income levels to estimate the impact on health of neighborhood income.

The practical implementation of the Spatial Dispersal Policy was influenced by a simultaneous housing shortage.[15] Specifically, the DRC struggled to find enough affordable housing of a suitable size, considering the relatively low income levels of the newly arrived refugees.[16] This shortage is best illustrated by waiting times for permanent housing which were on average six months, but could be up to two years.[17] 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 thus 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 whom it matched in terms of questionnaire observables. 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 (2014)) and at the clustered hectare level (Damm (2014)). Our main definition of a neighborhood, 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 (stairways of 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 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 allocation 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 stairway was random, since the supply of

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

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

[17] See Damm (2004) for numbers 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).

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 stairways of apartment buildings. We argue that these assumptions are valid because individuals were assigned to permanent housing solely based on the questionnaire.

Three concerns arise in this context which could invalidate the design: *i)* the DRC selectively allocated certain types of individuals to certain types of neighborhoods, *ii)* neighborhoods tried to select refugees trough lobbying against/for 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 goes for stairways and municipalities.

The scope for the DRC to place individuals in a selective manner was very limited since the housing officer searched for housing based on information from the questionnaire already 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. Interviewing the former DRC head of housing, she found it very unlikely that housing officers were able to selectively allocate individuals across neighborhoods due to the constant lack of affordable yet large enough housing options in the housing market.[18] Thus, it seems unlikely that the DRC systematically placed specific types of individuals in certain types of neighborhoods.

A second concern is that neighborhoods, e.g. through lobbying, tried to affect which types of refugees were allocated to that area. This is a potential issue at all neighborhood levels. At the municipality level the scope for selection was limited due to the short time frame (approximately ten days) from asylum was granted until resettlement took place in the municipality. Once allocated to a municipality, the different parishes could perhaps lobby against/for certain refugees. However, contrary to

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

the municipality, the parishes or stairways did not have a formal administrative unit to organize such lobbying and it therefore 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 from 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.[19]

C Balancing tests

To further support our identifying assumptions, we run a set of balancing tests of neighborhood characteristics on several individual characteristics which 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 should therefore 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 individual refugee characteristics known and unknown to the DRC at the time of allocation. We run the following linear regressions:

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

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

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

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.[20]

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 conditions in the initial placement neighborhood.[21] If we use health diagnoses in the first year upon arrival as proxies for refugees' initial health conditions, we find no significant association between initial health and neighborhood income level or neighborhood health.[22] 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 stairways of apartment buildings or municipalities using similar regression tests.[23]

Based on the balancing tests and the arguments posed in Section II.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.

III 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.

[20] We refer to Section III for the definition of the neighborhood income groups.

[21] Neighbors' health conditions 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).

[22] 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.

[23] 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.

We can use the natural experiment described in Section II 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.[24] This allows us to estimate the average impact of spending one additional year in a low-income neighborhood.

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

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

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

The coefficients β_k denote the increased risk of diagnosis y if assigned to a middle- or top-income neighborhood relative to being assigned to the poorest neighborhoods. Thus, a negative estimate of β_2 and β_3 means that the risk of being diagnosed with y is lower in a top- and middle-income neighborhood than in a low-income neighborhood. The parameters identify the causal impact of being assigned to a certain type of neighborhood if the allocation of individual i to neighborhood n is random, conditional on the set of included individual characteristics and fixed effects. As we argue in Section II.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.

On top of that, 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.

[24]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.

We test this and provide evidence of the absence of any individual income effects in Section V.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 depends 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.[25]

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.[26] 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.[27] 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.[28] Lastly, we assume monotonicity – i.e. that being placed in a bottom income neighborhood always increases years of exposure to bottom income neighborhoods.

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

$$\begin{aligned} &First\ stage: \;\; x_{i,t+r} = \alpha_1 + \tilde{\beta} \cdot bottomincomegroup_{n,t-1} + \boldsymbol{X_{it}\gamma_1} + \boldsymbol{T_t} + \tilde{\varepsilon}_{i,t+r} \\ &Second\ stage: \;\; y_{i,t+r} = \alpha_1 + \beta \cdot \hat{x}_{i,t+r} + \boldsymbol{X_{it}\gamma_1} + \boldsymbol{T_t} + \varepsilon_{i,t+r} \end{aligned} \tag{3}$$

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

[26] This is discussed in detail in Section II.B.

[27] As already discussed, in Section V.A we show that the initial allocation of individuals did not impact their labor market outcomes. Table 8 shows very precise null-effects on employment and earnings.

[28] The instrumental variables estimation approach also handles if 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.2).

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.

IV 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[29], Sri Lanka and Vietnam in 1986 to 1998, and Somalia 1989 to 1998.[30] 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 to 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 persons from Somalia and Iran. Upon arrival 30 percent of the sample were

[29] Stateless refugees.

[30] 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.

surveyed by a statistical agency about their educational level obtained abroad.[31] Of those, 56 percent report basic schooling or less, 21 percent have vocational education while 23 percent arrive with an academic 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 hospital contacts reported to the Ministry of Health by the staff at the hospital where the patient receives 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 receives 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 carry an extreme level of detail.[32] First, we aggregate the diagnoses, we include in our analysis, into two main groups: lifestyle related diseases and mental disorders. The lifestyle related diseases consist of circulatory diseases[33], nutritional/endocrine/metabolic (referred to as nutritional) diseases[34], 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.[35]

We study neighborhood effects in lifestyle related diseases because the risk of developing lifestyle related diseases is influenced by individual behavior. That means, that if we expect neighborhoods to influence individual behavior by altering diet or exercise habits, then we would also expect neighborhoods to affect the risk of developing these diseases. Neighborhoods could influence these behaviors for example through the availability of healthy grocery stores or recreational areas but also through the behavior, attitudes, and appearances of other inhabitants.[36]

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. How-

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

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

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

[34] Diabetes, obesity and elevated cholesterol levels.

[35] 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.

[36] 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.

ever, we do expect under-detection of diseases because not every condition is diagnosed or requires a visit to a hospital.[37] 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 level since correlational evidence suggests that inhabitants in low-income areas generally utilize health services to a lesser extent than their more affluent counterparts.[38] This may bias our estimates towards zero.[39]

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 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 the annual gross earnings deflated using the consumer price index from Statistics Denmark (with 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 at the same time tax evasion is small and the data is therefore of very high quality.[40] 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.[41] 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 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 (stairway level). A parish is a subset

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

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

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

[40] See Kleven et al. (2011) and Alstadster, Johannesen, and Zucman (2019).

[41] 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.

of a municipality, whereas a stairway is a subset of a parish. During the period we study, refugees in our sample were distributed across 255 different municipalities and 9,405 different stairways. On average, disregarding the refugees, the municipalities had 15,424 inhabitants whereas a stairway only had 12 inhabitants during the period. For each year we characterize the geographical areas by the median level of household disposable income from the Income Register.[42] 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. We refer to Table 2, Table A.10 and Table A.11 for the summary statistics of neighborhood characteristics.

V Main results

In this section we present our main findings on neighborhood effects in 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 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

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

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.3.

Our findings in Table 4 are very robust to the choices made in the baseline specification. We find similar results using the 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 Table 5.

a Heterogeneous effects

The impact on health of placement neighborhood income type varies significantly by gender. Table 6 shows that females experience a larger increase in the risk of developing lifestyle related diseases 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 points higher risk of developing a nutritional disease than men placed in similar neighborhoods, relative to placement in the richest third of neighborhoods.[43] 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.

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

In our sample a larger share of women than men are diagnosed with nutritional or lifestyle related diseases before 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 Figure 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). 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.[44] This implies that we have a relevant instrument and a very strong first stage (see Table 7).

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 percentage points. The effects are mainly driven by the occurrence of diabetes and hypertension, see Table 7.[45] The findings are qualitatively similar if we instead instrument average income in all neighborhoods that the individual lived in (See Appendix Table A.6 and Appendix Section C for a description of this approach). It is important to instrument the number of years an individual has 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.7 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 negative health consequences of living in the poorest third of neighborhoods.

[44] Appendix Figure A.2 shows that there is substantial persistence in the ranking of neighborhoods in the income distribution.

[45] See Appendix Table A.3 for the dynamics of diagnosed lifestyle related diseases.

VI 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 neighborhoods' characteristics and the composition of residents. Each refugee outcome considered in the first approach and each control variable included in the second approach is testing 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 which 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 their 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 8. 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.[46] 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

[46]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.

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

Education. We document a significant difference in educational outcomes across placement neighborhoods. Panel a of Table 9 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 to those placed in the poorest third of neighborhoods.[47] 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 9 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 8 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 9 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 8 shows that the occupations where the individuals are employed do not differ in task complexity across neighborhoods.[48] 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

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

[48] 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.

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 10). The Danish health care 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 systematically between rural and urban areas in terms of education possibilities, spare time 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 overall health policy was run by the counties throughout the period, municipalities could still affect access to health care such as rehabilitation offers or health preventive actions. The local authorities might also differ in their tax rate and service level (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.[49] 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.[50]

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

[50]The conclusion remains if we control for the number of general practitioners per capita along with municipality expenditures on social and health services, see Appendix Table A.4. However, municipality expenditures on health services may reflect both the quality of health services and the health conditions of inhabitants.

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 different neighborhood resident characteristics 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 10. 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.[51]

Another relevant peer group for refugees may be inhabitants with the lowest income levels, since the refugees themselves have very low income levels. We therefore include the poverty rate in the neighborhood as a control variable in Column (5) of Table 10, but it does not have much explanatory power neither 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 refugees' arrival.[52] Instead, we measure this at the municipality level.[53] 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 10. 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 becomes insignificant at the 5 percent level, see Appendix Table A.5 for this exercise.[54] 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.

[51] 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.4. We also include the average income among immigrants in the neighborhood as a control, but this does not affect the estimates much either.

[52] We do not have a good measure of 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.

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

[54] Appendix Table A.5 replicates Table 10 with income groups defined at the municipality level instead of parish level income groups.

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 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 stairways, respectively. The results are presented in the first three columns of Table 11. The table shows that the neighborhood effect in health becomes larger when the neighborhood size becomes smaller. The neighborhood effect in health is larger at the parish level compared to the municipality level, while the neighborhood effect in health is even larger at the stairway level compared to the parish level.[55] Moreover, the neighborhood effect in health is most precisely estimated when we let a stairway define a neighborhood.

In the fourth column of Table 11 we compare the impact of being assigned to the poorest third of stairways, holding constant the impact on health of being assigned to poorest third of parishes and municipalities. That is, we examine if being assigned to the poorest third of stairways 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 stairways simultaneously. This exercise shows that the income group of the assigned stairway 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 stairway income group is as important as the municipality and parish income group combined.[56]

[55]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.8. Five of seven coefficients become larger in magnitude when moving from the municipality or parish to the stairway level. Especially one estimate is large and precisely estimated in this case; the impact on diabetes, which consistently seems to be driving impacts on nutritional diagnoses.

[56]In an alternative approach we simultaneously instrument the number of years spent in a bottom income parish and a bottom income stairway, respectively. This exercise shows that an additional year spent in a bottom income stairway increases the risk of diabetes by 0.9 percentage points, whereas this estimate is 0.0 percentage points and not significant for time spent in a low-income parish (see Appendix Table A.9). Moreover, the risk of developing a lifestyle related disease increases by 1.9 percentage points for an additional year in a low-income stairway, while there is no extra effect of an additional year in a

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.

D Remaining explanations

What are the remaining differences between the poorest and richest neighborhoods once we sum up the results from Section V.B and Section V.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 food/grocery store options, immediate recreational areas and local sports clubs. Using the income of the immediate neighbors as a proxy for the very local neighborhood quality, our results from Section V.C indicate that such characteristics of the very local environment are important.

Given our results, especially amenities related to diet or exercise or behavior of immediate neighbors could potentially be very important, since both diet and exercise matter for the risk of developing lifestyle related diseases. Neighborhood characteristics such as traffic noise or air pollution may be less important determinants of diseases such as diabetes.[57]

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 in 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 we in general would expect to be correlated with quality. That is, the apartment quality could only to a limited extent be reflected in prices 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

low-income parish.

[57] 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.

our results.

VII Concluding remarks

We study a Spatial Dispersal Policy in act from 1986 to 1998 which quasi-randomly resettled individuals in different neighborhoods. This natural experiment allows us to rule out selection of individuals into neighborhoods and provide 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 individuals who were resettled in the poorest third of neighborhoods are at higher risk of suffering from a lifestyle related disease compared to those who were resettled in richer neighborhoods. 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. Furthermore, we show that exposure to the poorest neighborhoods is particularly harmful for women. On average, mental health is not affected by the neighborhood type.

We contribute to the understanding of neighborhood effects in health by examining a number of potential mechanism that have not been tested previously. The neighborhood income gradient in health cannot be explained by differences in individuals' employment or earnings across neighborhoods, but we document that persons 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. Remaining explanations for the observed income gradient include differences in neighborhood amenities and the health behaviors of residents, and we provide evidence that what matters most for neighborhood effects in health is the very local neighborhood. The income level of immediate neighbors living in the same stairway of an 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 the access to local recreational areas affect residents' behavior could provide a better understanding of the neighborhood effects in 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
Academic 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 arrivals based on family-reunifications. 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 Expenditures 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 expenditures per capita and median household income is measured in USD. Observations are parish-year. Data on ”Health and Social Expenditures per Capita” stems from Statistikbanken, (REG1 and REG11). Parish types are defined by Ministry for Cities, Housing and Rural Areas (2013). All other data is 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)
Academic 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	-0.002	0.022**	-0.020	-0.000**
0-2 Years Old	(0.009)	(0.011)	(0.012)	(0.000)
Number of Children	-0.007**	0.003	0.004	-0.000***
3-17 Years Old	(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. 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 if 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 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 upon arrival, but measure all other individual characteristics at year of immigration. We measure all parish characteristics one year prior to immigration.

(a) Yearly Incidents

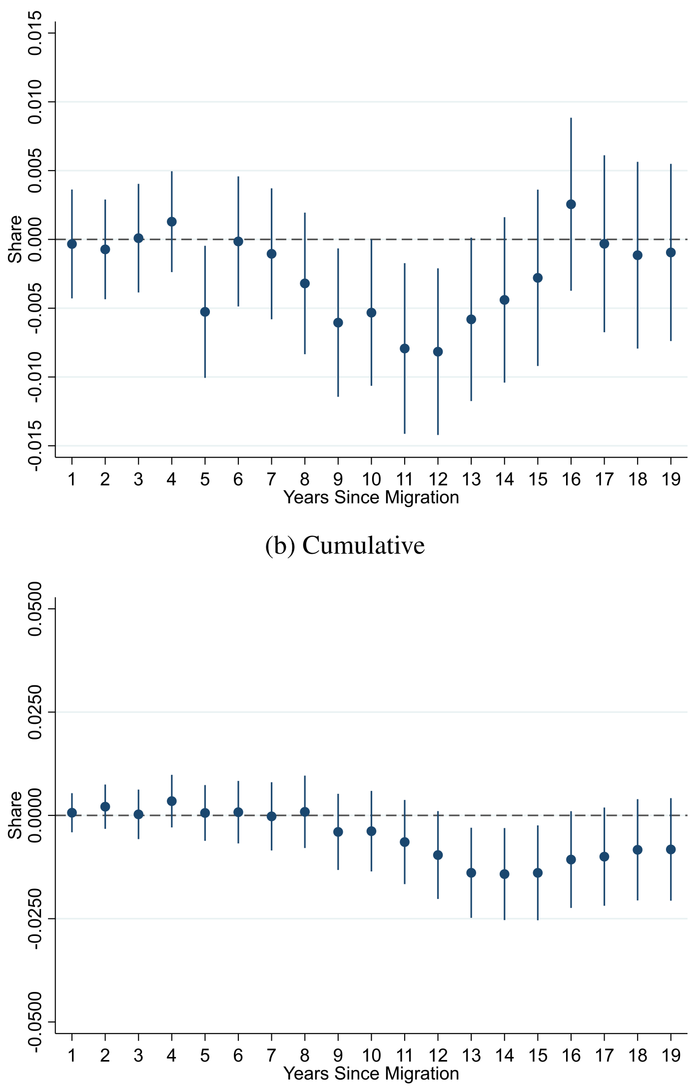


Figure 1: Development of Lifestyle Related Diagnoses

Notes: Standard errors in parentheses clustered at parish × 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 19 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 19 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: 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 before 2018. In Panel (b) the dependent variable is a dummy for being diagnosed with the disease considered disease 8-15 years after immigration. We measure parish income groups one year prior to arrival based on median disposable 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: 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

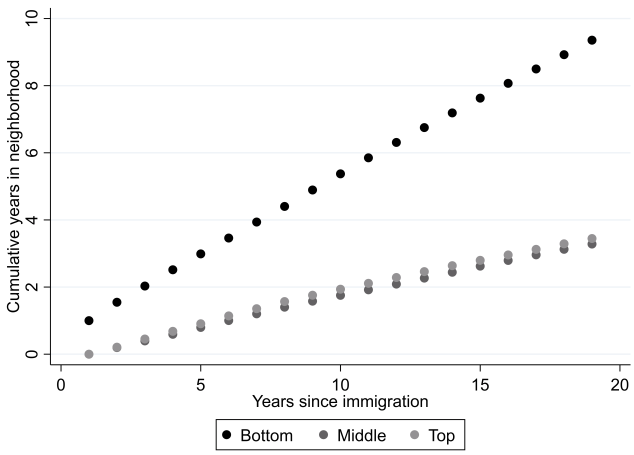
Notes: Standard errors in parentheses clustered at parish $\times$ immigration year level. $^{*}p < 0.10,^{**} p < 0.05,^{***} p < 0.01$. All estimates in Panel A show the impact of assignment parish on the probability of being diagnosed with a lifestyle related disease in different setups. In Panel 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) show the same estimation where income groups instead are based 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 before 2018. 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 6: Heterogeneous Effects by Gender

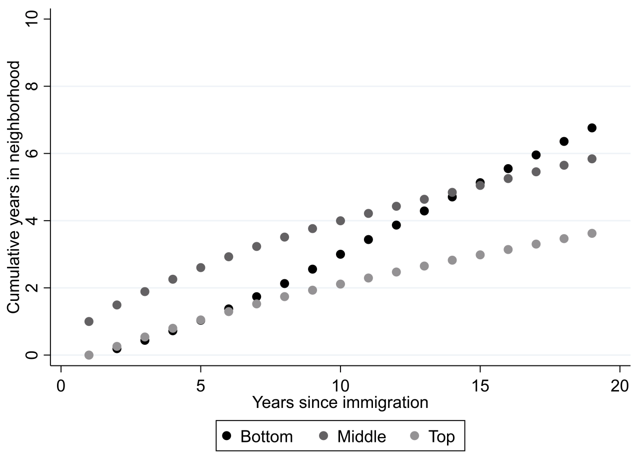
	Lifestyle Related	Circulatory	Nutritional	Hypertension	Diabetes	Mental Disorder	Neurotic
				(a) Ever diagnosed			
Middle	-0.012	-0.025**	-0.018*	-0.016**	-0.021**	-0.003	0.005
	(0.011)	(0.011)	(0.009)	(0.008)	(0.008)	(0.011)	(0.009)
Top	-0.010	-0.015	-0.010	-0.011	-0.017**	-0.011	-0.005
	(0.011)	(0.010)	(0.009)	(0.007)	(0.008)	(0.010)	(0.009)
Middle × Female	-0.009	0.005	0.006	-0.004	0.022*	-0.007	-0.008
	(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 × Female	-0.026*	-0.002	-0.024*	-0.007	0.008	-0.009	0.003
	(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: 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 before 2018. 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 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.

(a) Placed in Bottom Income Neighborhood



(b) Placed in Middle Income Neighborhood



(c) Placed in Top Income Neighborhood

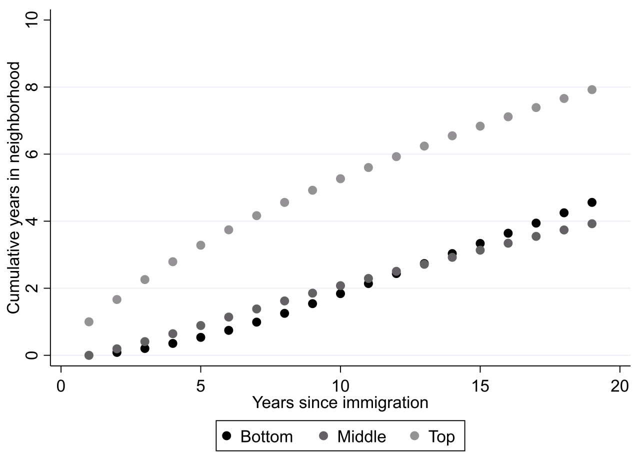


Figure 2: Cumulative Exposure to Neighborhoods by Years Since Immigration

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 in each parish among all parishes in Denmark in a given year.

Table 7: 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: 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 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 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: 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: 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 year upon immigration (Panel (b)), is 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 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 9: Education Outcomes

	All Education	Basic	Vocational	Academic	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: 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 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 upon arrival (Panel (b)). We measure parish income groups one year prior to arrival based on median disposable 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 10: Mechanisms, Lifestyle Related Diseases

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

Notes: Standard errors in parentheses clustered at parish $\times$ immigration year level. $^{*}p < 0.10, ^{**}p < 0.05, ^{***}p < 0.01$. The table tests 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 and squared number of co-nationals in the neighborhood, and in column (5) we include the poverty rate in the neighborhood as a control. In column (6) 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)-(6) are positive or virtually zero and insignificant. Only the controls in (6) are significant with an estimated coefficient of 0.031 in Panel (a) and 0.027 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 parish income groups one year prior to arrival based on median disposable 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 11: Neighborhood Effects in Health Using Different Neighborhood Definitions

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

Notes: Notes: 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 stairway group refer to the bottom third of all municipalities, parishes and stairways, 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 stairways 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.

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A Appendix: Additional Tables and Figures

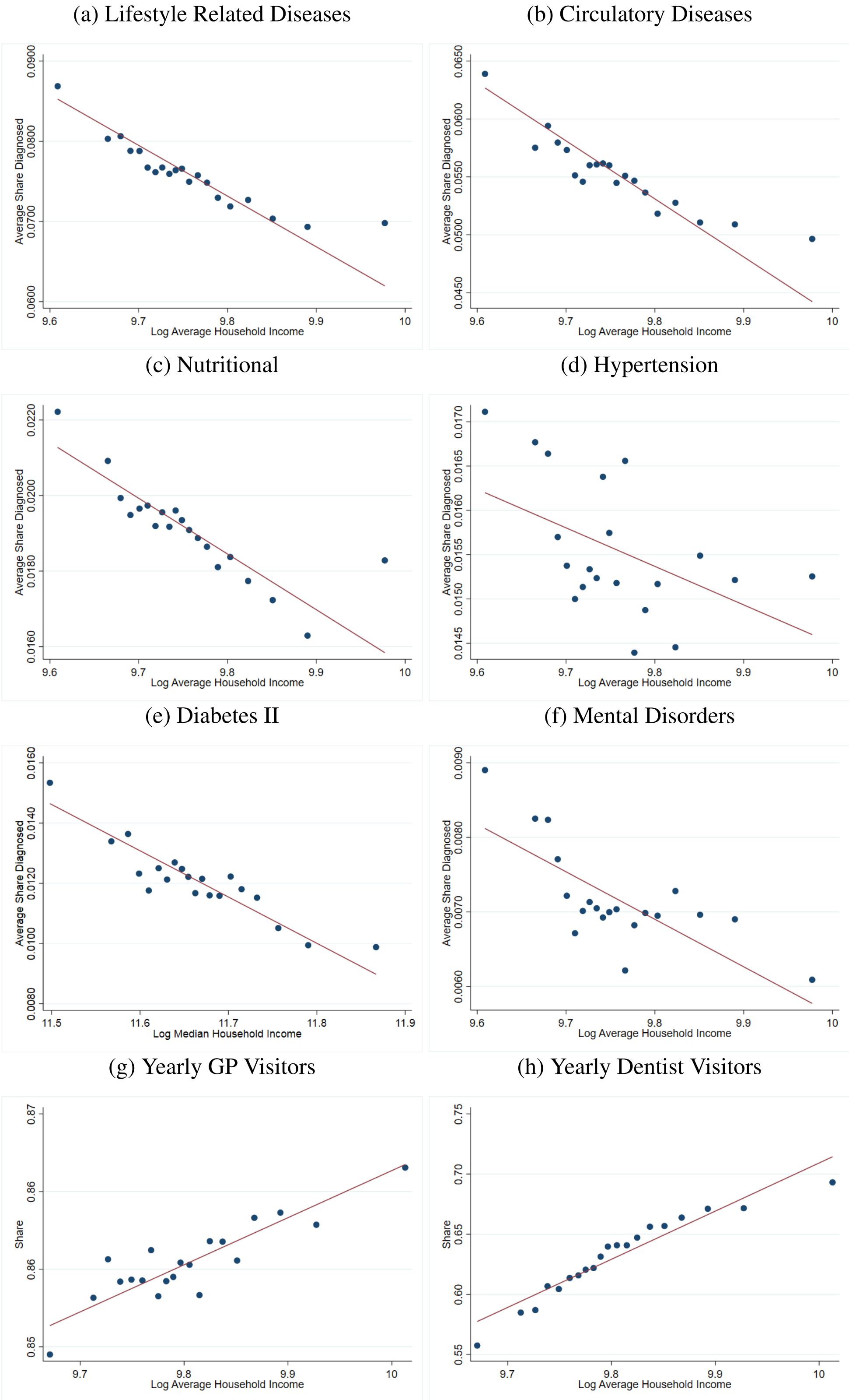

Figure A.1: Association Between Health and Neighborhood Income

Notes: The figures illustrate the association between health, health behavior 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. Data stems from administrative data provided by Statistics Denmark from 1991-2017 for the full Danish population above 18 year of age.

Table A.1: Balancing Tests, Stairway 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)
Academic 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	-0.022	0.014	0.008	-0.001
0-2 Years Old	(0.013)	(0.013)	(0.010)	(0.002)
Number of Children	-0.014***	0.003	0.011***	-0.000
3-17 Years Old	(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 stairways using linear regressions. 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 if refugees with certain characteristics (column farthest to the left) are more likely to be placed in stairways with specific characteristics (dependent variables). The dependent variables in (1)-(3) are dummies for assignment to a bottom income stairway (1), middle income stairway (2) or top income stairway (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 upon arrival, but measure all other individual characteristics at year of immigration. We measure all stairway 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)
Academic 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	0.013	-0.005	-0.008	-0.000
0-2 Years Old	(0.008)	(0.012)	(0.011)	(0.000)
Number of Children	-0.002	0.003	-0.001	-0.000***
3-17 Years Old	(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. 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 if 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 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 upon 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 with 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: Impact on Mortality

	Within 8 YSM	Within 15 YSM	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: 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.4: 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: Standard errors in parentheses clustered at parish $\times$ immigration year level. $^{*}p < 0.10, ^{**}p < 0.05, ^{***}p < 0.01$. The table tests 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 logarithmic number of GPs per capita in the municipality and the logarithmic health and social expenditures 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 logarithmic 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.5: Mechanisms, Lifestyle Related Diseases (Municipality Level)

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

Notes: Standard errors in parentheses clustered at parish $\times$ immigration year level. $^{*}p < 0.10, ^{**}p < 0.05, ^{***}p < 0.01$. The table tests potential mechanisms behind the estimated neighborhood effects by estimating model (2) with different sets of controls but with municipality instead of parish level income groups. Column (Baseline) shows the baseline coefficients from model (2) with municipality level income groups. The controls in column (1) to (6) are exactly identical to the controls in Table 10, but the income groups are defined based on the median income in the municipality. The coefficients on the controls in (3)-(6) are positive or virtually zero and insignificant. Only the controls in (6) 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 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

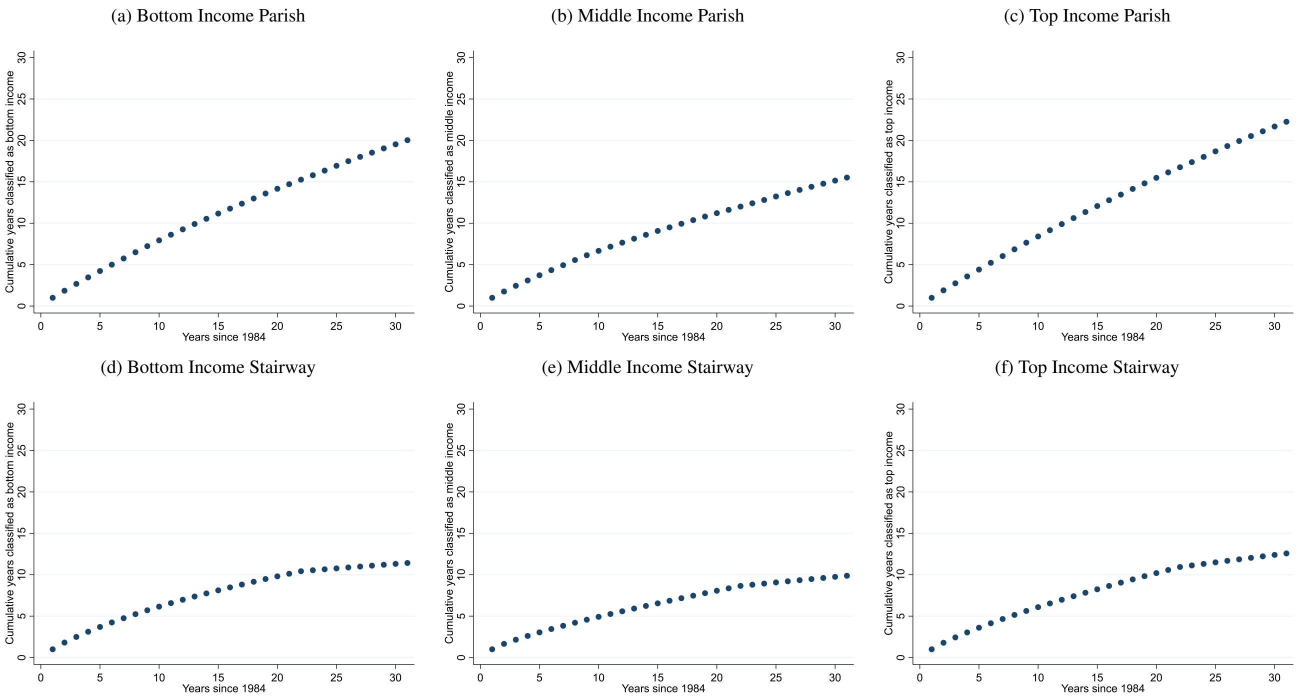

Figure A.2: Persistence in Neighborhood Classifications

Notes: Panel (a) shows the cumulative number of years a parish, which 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 stairways.

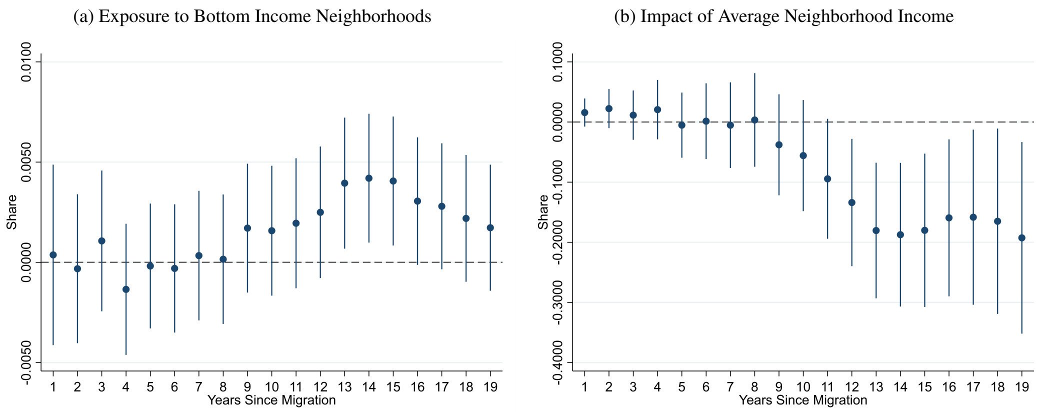

Figure A.3: Dynamics of Lifestyle Related Diagnoses, IV Estimates

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. 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 which 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 with 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.6: 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: Standard errors in parentheses clustered at parish $\times$ immigration year level. $^{*}p < 0.10, ^{**}p < 0.05, ^{***}p < 0.01$. The tables 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 pct. 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.7: 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: 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 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.8: 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 Stairway				
Years of Exposure to	0.019***	0.011**	0.013***	0.006	0.010**	0.003	-0.001
Bottom Stairway	(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: 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 a stairway (households living in the same building) define a neighborhood and use initial stairway 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 stairway group refer to the bottom third of all municipalities, parishes and stairways, 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 stairway = 89.21.

Table A.9: IV Results

	Lifestyle Related	Circulatory	Nutritional	Hypertension	Diabetes	Mental Disorder	Neurotic
Years of Exposure to Bottom Parish	-0.003	0.000	-0.000	0.001	-0.000	0.002	0.002
	(0.004)	(0.003)	(0.003)	(0.002)	(0.002)	(0.003)	(0.003)
Years of Exposure to Bottom Stairway	0.019***	0.010	0.013**	0.006	0.009*	0.002	-0.002
	(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: Standard errors clustered at parish × 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 stairway level simultaneously. We use initial parish and stairway income groups as instruments for years spent in a bottom income parish and stairway 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 stairway group refer to the bottom third of all parishes and stairways, respectively. We measure income groups one year prior to arrival based on median disposable 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 stairway is = 149.09.

Table A.10: Summary Statistics for Initial Placement (Stairway)

	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 Expenditures per Capita	4,016.16	4,112.72	4,022.29
N	683	1,456	2,773

Notes: Summary statistics for stairways in which refugees were resettled. A stairway refers to the group of households living in the same building. ”Bottom”, ”Middle” and ”Top” refer to stairway characteristics of stairways in the bottom, middle and top third of stairways measured by median stairway disposable income in a given year. We calculate the median income of each stairway including all inhabitants in each stairway aged 18 or above and define the income groups among all stairways, irrespective of any refugee assignment. We define income groups and all stairway 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 stairway-year. Health and social expenditures per capita and median household income is measured in USD.

Table A.11: 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 Expenditures 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 municipality 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 in each municipality 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 expenditures per capita and median household income is measured in USD.

Chapter 2

Household Debt and Mental Health

Household Debt and Mental Health

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Abstract

Does debt have negative consequences for mental health? We answer this question by comparing mental health responses to adverse shocks across individuals with different ex ante levels of debt. The increase in the incidence of mental health problems following a somatic health shock is 45% larger for individuals who initially have high debt than for those with low debt. Differences between high- and low-debt individuals are especially strong for outcomes indicating severe depression. We present evidence consistent with the hypothesis that these differences are due to high debt amplifying the negative financial consequences of health shocks.

1 Introduction

Household debt has risen dramatically over recent decades in most developed countries (André (2016). While the potential benefits of access to credit are substantial, studies have also shown that excessive household borrowing can have negative economic consequences for society at large (e.g. Mian, Rao, and Sufi (2013); Mian, Sufi, and Verner (2017); Eggertsson and Krugman (2012)), as well as for the individual borrower (e.g. Melzer (2011); Jappelli, Pagano, and Maggio (2013)).

In addition to economic costs, high debt may also lead to emotional distress and deteriorating mental health for the borrower. A contractual feature of a debt contract is that the borrower has to make a steady stream of payments. Failure to do so can have severe consequences as lenders seek to repossess and liquidate assets held by the borrower. In effect, when individuals with a high level of debt experience adverse shocks that diminish their ability to service the debt, they may feel emotions such as stress or anxiety for not being able to repay, which could trigger mental health problems.

In this paper, we study the role of debt in shaping mental health outcomes following such adverse shocks. Our main analysis focuses on shocks to an individual's health that require somatic inpatient hospitalization. Health shocks of this type have important economic consequences (Dobkin et al. (2018)), which - through the mechanism described above - could exacerbate any direct effects on mental health for individuals with high debt. We test this hypothesis by comparing changes in mental health outcomes around the time of the shock for individuals with individuals with different ex ante levels of debt.

To conduct the analysis, we use a unique data set with detailed information on the health of all individuals in Denmark over the period 2000-11. Assembled from various government administrative registers, the data set contains records of each interaction between a person and the health care system. This allows us to accurately identify somatic health shocks as well as mental health problems for a large sample of individuals.

We characterize an individual as suffering from mental health problems if he/she has any contact with a mental health professional, including at psychiatric hospitals, or receives hospital treatment for illness related to depression. Thus, our main outcome is a comprehensive measure capturing both mild and severe cases of mental health problems.[1]

[1] A potential worry is that mild cases may go untreated and therefore also undetected with our approach to measurement. However, the fact that health care, including psychiatric treatment, is universal and provided free of charge in Denmark alleviates this concern.

We combine the health data with data from tax returns and other administrative records that provide accurate and detailed information on an individual's debt, wealth, income, labor market attachment, and a range of demographic characteristics. Using this information, we characterize individuals as having high debt if their loan-to-value ratio - calculated as the value of their debt relative to the value of their home - belongs in the top quartile of the loan-to-value distribution in the year prior to the health shock. Individuals with loan-to-value ratios below this threshold are characterized as low-debt individuals. Focusing on homeowners who experience a somatic health shock, we then use an event study design to analyse the impact of the shock on mental health and examine whether the response is different for high-debt individuals than for otherwise similar low-debt individuals.

We find significantly stronger mental health effects of somatic health shocks for high-debt individuals than for those with low debt. The share of individuals who receive some form of mental health care rises sharply for both groups following the shock, but significantly more so for high-debt individuals, and the difference between the two groups remains significant in the seven years following the health shock. Cumulated over this period, the mental health effect is 45% larger for high-debt individuals than for low-debt individuals. The difference is especially clear when we look at indicators of severe mental health problems, such as psychiatric hospital care.

Consistent with our proposed channel linking household debt, adverse shocks, and mental health, we find that the health shock constitutes a negative earnings shock for high debt as well as low-debt individuals. While the initial drop in earnings is of similar size in the two groups, we find a sharp rise in the incidence of loan arrears among high-debt individuals, indicating mounting financial difficulties, but only a modest increase for those with low debt. Over time, the two groups' earnings trajectories also diverge: While the earnings of low-debt individuals stabilize, they continue to fall for individuals with high ex ante leverage, suggesting a potential feedback mechanism from mental health problems to labor market income.

We find further support for the proposed channel when we compare individuals experiencing the health shock during the financial crisis in 2008-09 to those who experience it in other years. The results from this exercise suggest that the amplifying role of debt is especially strong in times of financial turmoil when access to new credit becomes tighter and existing liabilities are more likely to lead to binding constraints.

We also provide evidence that our findings are not limited to somatic health shocks in particular since we find qualitatively similar results studying other types of adverse shocks.

Specifically, we study the mental health effects of fatal and non-fatal health shocks to spouses as well as job losses during mass layoffs and find stronger responses for individuals with high debt ex ante.

The main concern with the analysis reported above is that individuals with high level of debt may be of particular type that is more susceptible to mental health problems when faced with an adverse shock.

We address this potential issue in two ways. First, we control extensively for observable characteristics that correlate with ex ante debt and could influence mental health responses to shocks. Thus, our estimates are based on comparing mental health responses across individuals with different ex ante debt levels who are *similar* in other observable dimensions (e.g. age, gender, income...) and experience the *same* type of health shock. We demonstrate that our results are insensitive to including controls that significantly raise the model's explanatory power, suggesting a limited role for unobservables in explaining the estimated results (Altonji, Elder, and Taber (2005).

Second, we present results from a supplementary analysis in which we instrument the ex ante loan-to-value ratio using the number of years that have passed from the time since the individual first purchased a home. The idea behind this approach is that loan-to-value ratios decrease systematically with the number of years since loan origination as borrowers pay down their debt, a pattern we confirm in our data. Identification effectively comes from comparing individuals who purchased a home early in life vs. those who purchased later, thus cleansing the estimates from the influence of later - potentially endogenous - decisions regarding borrowing and debt repayment. We find that, qualitatively, our findings also hold with this alternative approach, suggesting that our results do in fact reflect a causal effect of debt on individuals' mental health following adverse shocks.

Overall, our results suggest that a high level of debt can leave individuals susceptible to mental health problems in the face of adverse shocks. These results speak to the public health implications of growing household debt levels that have been a common feature in many economies across the world. With high debt levels, aggregate shocks like financial crises and recessions could have long-lasting consequences for the mental well-being of large parts of the population, which could in turn amplify their initial impact on the economy.

From a health practitioners perspective, our findings also illustrate a potential scope for targeted preventive mental health care following somatic illness, as indebtedness should be seen as a serious risk factor for developing severe mental illness. In addition to the direct benefits of preventing such illness, it may also improve recovery rates as mental illness is associated with increased mortality risk (Kisely et al. (2005); Chang et al. (2011)).

Our paper contributes to an existing literature examining the relationship between debt and mental health problems. Many studies have found a positive association between the two (e.g. Brown, Taylor, and Wheatley Price (2005); Bridges and Disney (2010); Gathergood (2012); Drentea and Reynolds (2012); Hojman, Miranda, and Ruiz-Tagle (2016) - see Sweet et al. (2013) for a review). However, they typically rely on surveys - many of them with small sample sizes - and self-reported measures of both debt and mental health. Moreover, findings are most often correlational in nature, documenting a positive association in the cross-section - which we also find in our data - or over time within-subject.[2] This opens the door to problems of reverse causality. Intuitively, a positive correlation between debt and mental health problems may arise due to at causal effect of the former on the latter, but it could also be driven by mentally ill persons taking out loans to compensate for lost income or due to problems of self-control. Exploiting the panel nature of our data, our research design alleviates such concerns by studying the relationship between ex ante debt and subsequent *changes* in mental health problems. Moreover, by focusing on the effect of debt working through the impact of adverse shocks, we are able to study a specific causal mechanism through which debt can affect mental health outcomes.

We also contribute to a broader literature on the relationship between household financial circumstances and health in general. Richardson, Elliott, and Roberts (2013) provide a literature review and meta-analysis of the effects of unsecured debt and find that it is related to a broad range of undesirable health outcomes, both mental and physical. Currie and Tekin (2015) and Tsai (2015) focus on foreclosures and find positive associations with unscheduled hospital visits and a broad range of negative health outcomes, respectively. Ramsey et al. (2016) document higher mortality risk among lung cancer patients filing for bankruptcy. Using a research design similar to ours, Morrison et al. (2013) document higher mortality rates following cancer diagnoses among individuals with more debt.

Finally, the paper ties to the literature that examines the vicious cycle of poverty and why people find it very difficult to get out of a debt trap (e.g. Mani et al. (2013)

In the remainder of the paper we first describe the data we use in our analysis in Section 3 and then discuss our empirical strategy in Section 4. Having explained our empirical model we

[2]Gathergood (2012) is a notable exception. Using data from the British Household Panel Survey, he uses local house price changes as a source of exogenous variation in the severity of financial distress, he finds a negative effect of entering into mortgage arrears on self-reported psychological health.

provide characteristics of both debt groups in Section **??**. In the following Section **??** we first discuss our main findings and how they relate to the proposed mechanism linking mental health and debt. Finally, Section 7 concludes the paper.

2 Institutional background

Health care in Denmark The Danish health care system is universal and financed almost entirely through general taxes. All residents have equal access to services, which are generally provided free of charge.

Most health care services are provided by five regional governments that are responsible for hospitals, including psychiatric, and for reimbursing GPs and specialists for services provided in private practice. [3]

When in need of non-acute health care, the general practitioner is the patient's primary contact, and the GP acts as gatekeeper between the primary health care system and more specialized treatment. If a health problem requires specialist treatment, the GP can refer the patient to treatment in hospitals or non-hospital specialist clinics. In clinics operated by medical doctors, such as psychiatrists, treatment is provided free of charge for the patient.

In contrast, non-hospital specialist services performed by other types of health professionals, such as psychologists, require partial or full payment by the patient, depending on whether the patient has a referral from a GP or not. In the former case, the regional government pays a 60% subsidy.[4]

Household debt and borrowing Danish households are among the world's most indebted but also own large assets (OECD (2020)). This is in large part due to the Danish mortgage financing system, which grants homeowners easy and cheap access to credit, thereby also increasing house prices (Campbell (2012)).

Mortgage debt accounts for about 70% of total household debt in Denmark. Mortgage loans are offered only by specialized mortgage banks and are financed by covered bond issues. At origination, all mortgage borrowers face the same interest rates determined by current rates on the covered bond market.

[3]Non-hospital health clinics typically function as privately owned companies that, based on collective agreements between the regions and the practitioners, get a specific reimbursement for each service they provide. However, there are also health clinics run directly by the regional governments.

[4]GPs can refer the patient to psychologist treatment under specific circumstances. These include cases where the patient has had serious illness, lost a relative, suffered from mild to moderate depression or anxiety, or attempted suicide.

Homeowners can borrow up to 80% of their home value on the mortgage market. The proceeds from loans can be used for any purpose, including consumption. In addition, households may borrow from non-specialized retail banks that offer a wide range of credit facilities. Loans from such banks account for about 25% of total household debt, while the remaining 5% mostly consist of student loans, store credit, and other debt to non-financial companies.

3 Data sources and sample selection

We use data from several government administrative registers covering the entire Danish population. Common to all registers is a unique personal identifier that all Danish citizens receive at birth or date of first residence. This allows us to link information from the various sources at the level of the individual.

For health outcomes, we rely on three registers: First, information on hospitalizations comes from the National Patient Register, which contains detailed records of all contacts between hospitals and their patients. The information in this register is recorded by staff at the treating hospital and reported to the Ministry of Health for accounting and monitoring purposes. Each record contains information on the type of care provided (inpatient, outpatient or emergency room), the type of hospital (somatic or psychiatric), and a diagnosis indicating the disease or condition for which the patient received treatment (ICD-10 classification). We use this information to identify individuals who experience somatic health shocks or receive hospital treatment for mental health problems.

Second, we use data on consultations with psychiatrists and psychologists from the Health Insurance Register. The data in this register come from primary healthcare providers who, for reimbursement purposes, report information to regional governments about the services they have provided to patients.

Third, the Cause of Death Register contains information about the date and cause of death for deceased individuals. We use this to identify individuals who commit suicide as well as those who experience the loss of a spouse.

We combine the health data with individual-level data on income, debt, and assets from annual tax returns. This data is highly reliable because it is almost completely based on third-party reported information from employers, government agencies and financial institutions, and evasion is minimal (Kleven et al. (2011); Alstadsæter, Johannesen, and Zucman (2019)). On the asset side, we have information on bank account deposits, financial securities, and the value of all homes owned by the individual as assessed by the tax authority for taxation purposes.

Data on liabilities include all debt owed to financial institutions, and to the government (e.g. student loans). Financial institutions and other credit providers must also report details about delinquent loans to the tax authority, and we use this data to construct an end-of-year indicator for loan arrears for each individual.

Finally, we add individual background characteristics from a number of registers provided by Statistics Denmark. From the population register, we extract data on municipality of residence, gender, and age. This register also contains information on household structure, thus allowing us to link individuals to their spouses in the data. From the Integrated Database for Labour Market Research (IDA), we add information linking employees to employers. The data base also contains information on individuals' main source of income and employment status.

From these sources, we compile an individual-level data set with annual observations for each person in the Danish population in the years 2000-11. We then construct a number of indicators for mental health problems. Our main outcome variable is a comprehensive measure that indicates whether the individual in a given year has any consultations with a psychiatrist or psychologist, gets treatment of any kind at somatic hospitals for a diagnosis related to depression, or receives any type of care at psychiatric hospitals. We also construct separate indicator variables for each of these separate outcomes. Finally, we generate an indicator for whether the individual committed suicide or received hospital treatment of any kind for attempted suicide or intentional self-harm.

We measure an individual's *leverage* by the loan-to-value (LTV) ratio, defined as the individual's total debt divided by the tax value of their home(s). Figure 1 illustrates the raw correlation between an individual's within-year rank on this measure among all working-age homeowners and our comprehensive measure of mental health problems described above. Consistent with the existing literature, we find a strong positive correlation between indebtedness and mental health problems in this population.

We impose a number of sample restrictions to obtain our analysis sample. Most importantly, we focus exclusively on individuals who experience some adverse shock. In our main analysis, we focus on *health shocks* and the description below explains how we select our sample for this analysis. However, we also report results from supplementary analyses where we focus on other adverse shocks. The sample selection procedures for these analyses are described in **??**.

We define individuals experiencing *health shocks* in a given year as those who i) were admitted to a somatic hospital for inpatient care due to a non-mental, non-pregnancy-related disease or condition in that year, and ii) had not received inpatient care at a somatic hospital in the three previous years. We then limit our sample to individuals who experience such a shock in some year between 2003 and 2011. Further, since our interest is in homeowners in the adult working-age population, we also require that individuals are between 30 and 60 years old and that they own at least one home in each of the three years prior to the year of hospitalization

We follow each individual for up to seven years before and after the health shock. This produces a baseline sample of 546,750 individuals with a total of 4,631,685 individual-year observations.

A key feature in our analysis is the distinction between individuals with high vs. low debt before the health shock. We define the high-debt individuals as individuals whose LTV ratio in the year before the event places them in the top 25% among all homeowners in the full population in that year. Conversely, low-debt individuals are defined as those with LTV ratios below the 75th percentile threshold in the year before the event.

Table 1 shows descriptive statistics for high vs. low-debt individuals in our sample, measured in the year before the health shock. By construction, high-debt individuals have more debt than low-debt individuals. They are also younger and more likely to have children, which is consistent with younger homeowners being more levered because they have only recently entered the housing market. There is virtually no difference in income between the two groups but high-debt individuals are about four times more likely to be in arrears on at least one of their loans. That is unsurprising, given their higher leverage.

Despite these differences, the two groups are reasonably similar in terms of mental health outcomes before the health shock. For example, the share of individuals who consult a psychiatrist in the year before the shock is exactly 1% in both groups. For the comprehensive measure, there is a difference of 0.5 percentage points in the share of people receiving some type of mental health care. Thus, as in the full population, we find a modest *ex ante* correlation between debt and mental health problems within our sample.[5]

4 Empirical strategy

The aim of our main analysis is to study the role of debt in shaping mental health outcomes following adverse health shocks. To do that, we estimate a standard event study model allowing for heterogeneous responses across individuals with different levels of indebtedness.[6] Specifically,

[5]Controlling for differences in observable characteristics between high and low-debt individuals, as explained in the following section, reduces the ex ante difference to 0.2 percentage points.

[6]Dobkin et al. (2018) and Fadlon and Nielsen (2019) use similar designs to study responses to hospital admissions and health shocks to family members, respectively, but do not focus explicitly on response heterogeneity.

we estimate the following model:

$$\begin{aligned} y_{it} = & \sum_{j \neq -1} \mathbb{1}[e_{it} = j] \times (\lambda_j + \beta_j HighDebt_i + \boldsymbol{X_i}\alpha_j) \\ & + \delta HighDebt_i + \boldsymbol{X_i}\mu_j + \gamma_t + \varepsilon_{it} \end{aligned} \tag{1}$$

where i indexes individuals, t indexes years, y_{it} is an outcome of interest, $HighDebt_i$ is the indicator for high debt before the event, $\boldsymbol{X_i}$ is a vector of individual-specific controls, γ_t is a year fixed effect, and ϵ_{it} is an error term.

The variable e_{it} measures the number of years since the event year, with negative values indicating that the health shock has not yet occurred in year t. Thus, the term $\sum_{j \neq -1} \mathbb{1}[e_{it} = j]$ denotes a set of indicators for event time with $e_{it} = -1$, the year before the event, as the omitted category. To allow different responses to the health shock for individuals with different ex ante leverage, we interact these indicators with the indicator for high debt. The high-debt indicator also appears uninteracted with event time to capture any level difference between individuals with high vs. low debt.

We estimate Model (1) with ordinary least squares, with standard errors clustered at the level of the individual. The outcomes of main interest are the measures of mental health problems described in section 3, but we also consider a number of economic outcomes to explore potential mechanisms behind the mental health responses.

For each outcome, the λ_j and α_j coefficients jointly summarize the dynamic response to adverse health shocks for low-debt individuals. The coefficients of main interest to us, however, are the β_j, which summarize the *difference* in outcome responses to the adverse health shock between high and low-debt individuals. For example, β_2 expresses the change in the outcome variable from one year before to two years after the event for high-debt individuals, *over and above* the corresponding change for low-debt individuals.

We take several steps to address potential concerns about whether the β_j reflect the *causal* effect of high debt on mental health responses to health shocks. First, to address the concern that high- and low-debt individuals would have had different mental health developments even in the absence of the somatic health shock, we follow individuals for several years before the shock arrives to assess directly the two groups display parallel pre-trends. In practice, this amounts to checking whether the β_j coefficients are small and statistically insignificant for negative values of j.

In another context, Kleven, Landais, and Søgaard (2019) use a design similar to ours to study gender differences in the impact of children on labor market outcomes.

Second, to address concerns about omitted variable bias, we control non-parametrically for a broad range of potential confounders. The vector $\boldsymbol{X_i}$ contains categorical control variables for gender, children, income (decile group), municipality of residence (98 categories) and age (31 categories), all measured in the year before the event. We also control for the nature of the health shock in the event year, as indicated by the diagnosis recorded in the National Patient Register.[7] Importantly, we include all these controls uninteracted as well as interacted with the event time indicators so as to capture differences in mental health responses to somatic health shocks across each dimension. This has important implications for the interpretation of the β_j coefficients. For example, adding controls for age and diagnosis type implies that the β_j capture the difference in mental health responses between individuals who have different levels of debt but have the *same age* and experience the *same type of health shock*.

Third, we apply instrumental variable estimation techniques to address any remaining endogeneity concerns. Exploiting the fact that leverage declines mechanically over time as households pay back their mortgages, we instrument the indicator for high debt in the year before the event with the number of years between the individual's first purchase of a home and the arrival of the adverse health shock. In constructing this instrument, we make use of the fact that information on income and assets from tax records goes as far back as 1987. Thus, for most individuals in our sample, we can pinpoint the exact year when they first became homeowners.[8,9]

With controls for age at the time of the health shock included, identification in the IV regression comes from comparing mental health responses of individuals who are the same age when they experience the adverse health shock but purchased their first home at different ages. All other sources of variation in leverage, including subsequent decisions on debt repayment or new borrowing, are disregarded. This is desirable, since such decisions could in principle be influenced by emerging mental health problems that are not yet observable in the year before the health shocks.

However, one may still worry that the instrument correlates with subsequent mental health

[7]Specifically, we apply the 99-grouping in the ICD-10 classification and include a dummy for each of the 99 categories (except for a reference category). Appendix Table H.1 shows the distribution of diagnosis types (aggregated into 18 groups) among high-debt and low-debt individuals within our sample.

[8]For individuals who experience the health shock in 2003, the measured number of years since first purchase is right-censored at 16. To ensure uniformity across individuals with different event years, we therefore censor the instrument at this value for everyone in the sample.

[9]To illustrate the mechanics of the instrument, we regress the indicator for high debt in event year -1 on the control variables in $\boldsymbol{X_i}$ and indicator variables representing the number of years since the first home purchase. The results, illustrated in Appendix Figure H.7, show that the share of high-debt individuals does indeed fall monotonically with time since first purchase among individuals in our sample, conditional on our baseline set of controls.

problems through some other channel than leverage. In particular, one could speculate that individuals who purchase a home at a young age have different character traits (e.g. matureness, groundedness) than those who are older when they purchase first their home, and that it is these differences in character, rather than their lower debt, that make them more mentally robust in the face of adverse health shocks. As a supplement to the IV regression with the baseline set of controls, we therefore also estimate a version where we sacrifice controlling for age at the time of the health shock and instead control for age at the time of the first home purchase.[10] In this version, identification comes from comparing mental health responses between individuals who purchased their first home at the same age but experience the same type of health shock at different ages.

Fourth and finally, we perform a number of auxiliary tests of the hypothesis that debt causally influences the mental health impact of somatic health shocks. One such test involves analysing the impact on economic variables. If debt-induced stress is the cause of a differential response across high and low-debt individuals, we should expect to see a larger increase in indicators of financial distress for the former group. Another test involves studying whether the difference between high and low-debt individuals varies over time. If excess leverage is the root cause of this difference, we should expect it to be larger in years with tight credit supply where high initial leverage is more likely to lead to binding constraints and financial distress.

5 Main Results

In this section, we present the results from our estimations of Model (1) for health shocks. Results for other types of shocks are presented in section 6.

5.1 Mental health outcomes

Figure 2 shows estimation results for Model (1) with our comprehensive summary measure of mental health problems as the outcome variable. We illustrate the results in two ways: In the left panel, we plot the average dynamic effects of the health shock for high vs. low-debt individuals.[11] The vertical distances between the two graphs correspond exactly to the estimated β_j coefficients, which we plot in the right panel.

[10] Note that we cannot simultaneously control for age at both points in time, since age at purchase, age at the time of the shock, and the number of years from purchase to shock are perfectly collinear.

[11] Note from equation (1) that the partial effect of event time indicator j is individual-specific and equal to $\lambda_j + \boldsymbol{X_i}\alpha_j$ for low-debt individuals and $\lambda_j + \beta_j + \boldsymbol{X_i}\alpha_j$ for high-debt individuals. To construct the left panel of Figure 2, we compute averages of both these values across all individuals in the sample and plot them for each value of $j \neq -1$.

Starting in the left panel, the figure shows that individuals in both groups experience a sharp and persistent increase in mental health problems in the year of the health shock. The share receiving treatment for such problems increases by 1.2 percentage points in the year of the shock, rising to 1.4 percentage points in the next year for low-debt individuals. Five years after the health shock, the share with mental health problems is still elevated by around half a percentage point. Compared to the baseline share of 3% (Table 1) in the year before the event, these are large effects.

However, the effects are even larger for high-debt individuals. At its peak in year 1 after the shock, the share of individuals experiencing mental health problems is 1.8 percentage points above the pre-shock level. As shown in the right panel, this 0.4 percentage point difference compared to low-debt individuals is highly statistically significant, and it remains large in the subsequent years. Cumulated over event years 0-7, the effect of the health shock is 45% larger for high-debt individuals than for low-debt individuals.[12]

Figure 2 also shows that the share of individuals with mental health problems is on an upward trend before the health shock for both groups. One reason for this may be that the hospitalization in the event year in some cases reflects a culmination following a protracted period of somatic illness, rather than acute disease. In such cases, it is possible that mental health problems would have become more prevalent even in the absence of the shock, although the steepness of the increase in the event year suggests that this is not the full story.[13]

Most important for our purposes, however, is the fact that high and low-debt individuals display almost completely parallel trends before the event, after which they diverge. Thus, conditional on our controls, we find no significant differences in mental health developments between individuals with high vs. low debt *before* the shock. This strongly suggests that the subsequent divergence between the two groups does indeed reflect differences in the causal effect of the health shock.

Figure 3 shows results for the various indicators of mental health problems underlying our main comprehensive measure. We find qualitatively similar results across all outcomes, but with some variation in magnitude and statistical strength. Panel A shows a clear increase in the share of individuals who consult a psychologist, peaking in year 1 at 0.9 percentage point

[12]The cumulated effect over event years 0-7 is 5.9 percentage points for low-debt individuals and 8.6 percentage points for high-debt individuals.

[13]Appendix Figure H.4 shows results for a subsample of individuals who are hospitalized with specific circulatory diseases characterized by their acute nature. Consistent with the interpretation above, we find no upward trends in mental health outcomes prior to the event year when we limit the sample to such cases, but sharp and persistent increases in the event year. The differences between high and low-debt individuals are qualitatively the same as in the full sample but statistically weaker due, in part, to the smaller number of observations.

above the pre-event level for low-debt individuals. For high-debt individuals, the increase is 0.2 percentage points higher, and the difference is statistically significant at the 5% level. Panel B shows similar results for psychiatrist consultations, but here the difference between the two groups is smaller and statistically insignificant.

Turning to the outcomes indicating severe mental health problems, we find sharp increases in the share of people receiving hospital treatment for depression or psychiatric hospital care, as shown in panels C and D, respectively. For both outcomes, the response to the health shock is significantly stronger for high-debt individuals, and the difference compared to low-debt individuals is even clearer than in panels A and B. This is especially true for psychiatric hospital care, where the difference in year 1 is nearly 0.3 percentage points. In line with these results, we also find a stronger responses in mental health problems among high-debt individuals if we consider the strongest indicators for severe mental health problems: suicides, suicide attempts and intentional self-harm (see Appendix Figure H.5).

5.1.1 Controlling for potential confounders

A potential concern with our results is that the difference in mental health responses between high and low-debt individuals could be driven by confounding factors other than leverage. As we explain in section 4, we address this issue by interacting the event time indicators with a set of controls for potential confounders. Table 2 explores how our main result varies with the exact composition of this set. To present this information in a compact way, we estimate simplified versions of Model (1) where we have replaced the full set of event time dummies with a binary indicator, $post_{it}$, that takes the value one if $e_{it} \geq 0$. Table 2 reports the coefficient on the interaction term between this indicator and $HighDebt_i$. We include observations from event years -3 to 2 i the estimations. Thus, the model allows us to estimate the average increase in mental health problems in years 0-2 after the health shock, relative to the three preceding years, and the reported coefficient captures the *difference* in this response between high and low-debt individuals, conditional on the included controls.

We begin with a simple version with no controls in column (1) and then gradually add controls until we reach our baseline set, shown in column (4). The coefficient is remarkably stable across columns and always significant at the 1 percent level, demonstrating that our main result is insensitive to the composition of the set of controls.

In columns (5) to (8), we go beyond our baseline. A specific concern is that the stronger mental health response for high-debt individuals could be due to higher financial vulnerability in some broader sense, rather than leverage per se. For example, one could imagine that self-employed individuals, who often invest heavily in their own businesses and face considerable income risk, are both more indebted and more vulnerable to adverse shocks than wage earners. Similarly, high-debt individuals are plausibly more likely to have insufficient liquid assets to smooth fluctuations in income, and it may be this feature, rather than their indebtedness, that makes them mentally vulnerable to adverse shocks. To address these concerns, we sequentially add controls capturing various dimensions of financial vulnerability that may correlate with indebtedness: Self-employment, low liquid assets, and a high share of unsecured debt. All are measured in the year before the event.[14] In all three cases, the coefficient on $post_{it} \cdot HighDebt_i$ is largely unaffected by the addition of the extra control variable. This corroborates the view that the observed differences between high and low debt really do reflect differences in ex ante leverage, rather than a correlated dimension of financial vulnerability.

Finally, we add individual fixed effects to our model to capture time-invariant characteristics affecting mental health. This raises the model explanatory power substantially without affecting the key coefficient much.

5.1.2 Instrumental variable estimation

Columns (9) and (10) report results from IV regressions in which we instrument the indicator for high debt with the number of years since first home purchase, as described in section 4. In column (9), we use our baseline set of controls, which includes the age at the time of the health shock. The coefficient estimate on $post_{it} \cdot HighDebt_i$ is larger than in our baseline OLS regressions but so is the standard error. Thus, while the coefficient is statistically significant at the 10 percent level (p-value of 0.06), we cannot reject the null that it is equal to the coefficient from our baseline specification shown in column (4).

In column (10), we sacrifice controlling for age at the time of the health shock in order to control for age at the time of the first home purchase. Compared to column (9), this increases the precision of the key estimate considerably. The coefficient estimate is strongly significant and even larger than in our OLS regressions. It suggests that the share of individuals suffering from mental health problems increases by a full percentage point *more* for high debt than for low-debt individuals following an adverse health shock.

[14]The indicator for self-employment is based on the individual's primary source of income. Individuals are defined as having low liquid assets if their end-of-year bank deposit balances are less than 1/6 of their annual disposable income. We define individuals as having a high share of unsecured debt if the balances on their non-mortgage loans exceeds 20% of their total debt.

5.1.3 Other robustness checks

Our main results described above are also robust to a number of other variations of our baseline specification. Appendix Figure H.1 shows that they do not hinge on the specific choice of cut-off between high and low-debt individuals. Appendix Figure H.2 shows that we can change the sample restriction on age with no change in results. Finally, Appendix H.3 shows that the results are insensitive to controlling for the occurrence of a second hospitalization in the event year: In our main specification, we control for the nature of the first hospitalization in the year, as indicated by the diagnosis type. However, an individual may be re-admitted later in the year with the same or a different diagnosis. We therefore add controls capturing the diagnosis type of any second hospitalization and note that this does not change our main result.

5.2 Economic outcomes

To explore possible mechanisms behind our main result, we now turn our attention to the dynamic effects of health shocks on a range of economic variables.

Existing literature has shown that health shocks are costly both in terms of direct health expenditures and indirectly via income losses(Mohanan (2013); Fadlon and Nielsen (2020);Dobkin et al. (2018). Since health insurance coverage is universal in Denmark, the direct costs are of minor importance in our context. Income losses, on the other hand, could be substantial, not only due to foregone earnings while hospitalized, but also in the longer term due to, for example, lost promotions or reduced productive capacity. To the extent that such income losses lead to financial distress, this could have adverse consequences for mental health.

We explore this potential channel in Figure 4. Panel A shows results from estimating Model (1) with labor market earnings (in DKK) as the outcome. Both high and low-debt individuals are on a clear downward earnings trend in the years before the health shock. With a drop of about DKK 11,000 (USD 1,750), the earnings decline clearly accelerates in the year when the shock occurs, and then flattens out. There is virtually no difference between the two groups until year 2 after the shock, suggesting that the initial impact of the health shock on income is the same for high and low-debt individuals.

Interestingly, however, the earnings paths of the two groups then diverge, with continued declines for high-debt individuals and stable or even moderately increasing earnings for low-debt individuals. One explanation could be a feedback mechanism from mental health problems: As our main results show, the share of individuals suffering from severe mental health problems rises more sharply after the shock among those with high debt, and this may be causing the continued decline in earnings for this group.

Panel B shows results for loan arrears. Here, we see strikingly different patterns between high vs. low-debt individuals. For the low-debt group, the share of individuals who are in arrears on their loans barely moves in the year of the health shock, and then increases moderately in subsequent years. In stark contrast, the share of delinquent borrowers among high-debt individuals increases considerably in the year of the health shock and continues to do so in the following years, reaching a level more than 2 percentage points above the pre-event baseline after 7 years.[15]

Together, these results suggest a mechanism through which high debt amplifies the adverse consequences of somatic health shocks for mental health: While high and low-debt individuals initially suffer similar income losses due to the health shock, the implications of these losses in terms of loan arrears are much more severe for high-debt individuals. Experiencing financial distress can be associated with negative emotions such as stress, anxiety, and guilt (Tsai (2015); Ramsey et al. (2016)). In addition to the emotional stress coming from the health shock itself, such emotions may plausibly explain the larger increase in mental health problems for individuals with high ex ante debt.

5.3 Heterogeneous effects

High debt can become an emotional stress factor when it turns into a binding constraint on consumption possibilities or the ability to stay in one's home. This is more likely to happen during times when credit supply is tightened, such as during a financial crisis.[16] We should therefore expect larger debt-related differences in mental health responses to health shocks in 2008-09 when the Danish financial sector was hit by the international crisis than in other years.

Figure 5 documents such a pattern. The difference in mental health responses between high and low-debt individuals is 2-3 times larger among those who suffered a health shock in 2008 or 2009 compared to other years. [17]

Figure 5 also illustrates results from heterogeneity analyses across individuals with vs. without children (Panel B) and gender (Panel C). We find no significant differences in either dimension. This suggests that the amplifying effect of debt on mental health problems following

[15] Morrison et al. (2013) similarly find larger increases in mortgage defaults, foreclosures and bankruptcy filings for high-debt homeowners than for low-debt homeowners following a cancer diagnosis.

[16] Indeed, existing evidence suggests that household leverage played a key role in explaining the sharp drop in consumption in the U.S. during the Great Recession of 2007-09 (Mian and Sufi (2010); Mian, Rao, and Sufi (2013)).

[17] The β_1 and β_2 estimates in the two graphs are significantly different at the 10 and 5 percent levels, respectively.

somatic health shocks is present for both men and women, and among individuals with or without children.

6 Results for other adverse shocks

In this section, we show that our results are not specific to somatic health shocks but hold across different types of adverse shocks.

First, rather than shocks to the individual's own health, we focus on adverse health shocks for the spouse. To do that, we redefine our sample to include individuals whose spouse experiences an adverse health shock - defined in the exact same way as in the main analysis. All other sample restrictions are unchanged. We then estimate Model 1 with our usual comprehensive measure of mental health problems as the outcome, only now with e_{it} denoting the number of years since the *spousal* health shock.

The results from this analysis are shown in Panel A of Figure 6. As in the case of own health shocks, we see a sharp increase in the share of people suffering from mental health problems at the time of the shock for individuals with low debt, but an even sharper increase for high-debt individuals. The relative difference between the two groups' estimated responses is about 30% but it is only borderline statistically significant, as seen from the right graph.

The results are even stronger in Panel B where we look at mental health responses to experiencing the death of a spouse. Here, the sample consists of people who have lost their spouse in some year between 2003 and 2011, with all other sample restrictions unchanged. The share of people who receive some mental health treatment rises by no less than 10 percentage points for both groups in the year of the shock. For high-debt individuals, it increases even further in the next year at which point the impact is again about 30% larger than for low-debt individuals. These large increases are primarily due to a sharp increase in the share of people consulting a psychologist. If we remove this particular outcome from our comprehensive measure, we find increases of 0.8 and 1.1 for low and high-debt individuals, respectively.

Finally, we explore the effects on mental health when the individual loses her job during a mass layoff event. We define a mass layoff event as a year where at least 30% of the employees at a workplace leave from one year to the next.[18] We then confine our sample to homeowners who become unemployed after experiencing such an event at some point between 2003 and 2011,

[18] More precisely, we define that an individual experiences a mass layoff in year t if i) he/she works for an employer that employs at least 50 people in year t-1, ii) at least 30% of the employees leave the employer from year t-1 to year t, and iii) no more than 50% of those who left the employer move on to work for the same new employer.

while not having experienced any unemployment during the three preceding years. Compared to health shocks, the number of people experiencing mass layoff events is much lower, and we obtain a sample of 3,831 individuals between 30 and 60 years of age. Perhaps because of the small number of observations, the mental health responses are not statistically significant for either group, but - consistent with our other results - the point estimates do suggest a sizeable differential increase of 1 percentage point in the high-debt group.

Summing up, the results presented in this section suggest that the mental health consequences of other types of adverse shocks are also more severe for high-debt individuals. The statistical power of this evidence is generally lower than for the case of adverse shocks to the individual's own health but, overall, the results are consistent with and corroborate our main finding that high debt poses a risk factor for mental health problems when individuals face difficult circumstances.

7 Concluding remarks

In this paper we provide novel evidence on the link between debt and mental health. We study the effects of various adverse shocks on mental health and find an amplifying effect of having a high initial level of debt. Focusing on adverse health shocks requiring hospitalization, we show that the increase in the share of individuals who receive some type of treatment for mental health problems is 45% larger for individuals with high ex ante debt than for those with low debt. Similar effects appear for spousal health shocks and job loss related to mass layoffs. Consistent with debt-induced financial distress lying at the root of these findings, we show that individuals with high debt experience a much larger increase in loan arrears than low-debt individuals following an adverse health shock, despite initially similar declines in income.

Our findings provide lessons for financial regulators, social insurance policymakers, and health care professionals. Financial regulators should be aware of the potentially enormous personal costs of excessive borrowing when designing regulation governing households' access to credit. Social insurance policymakers should pay attention to trends in household debt levels when assessing the costs and benefits of programs that insure households against the financial consequences of adverse shocks. Finally, health care professionals must recognize that financial leverage is a relevant risk factor for developing mental health problems following somatic disease.

Table 1 – Pre-event characteristics Notes: The table shows the sample means of basic characteristics for our estimation sample, measured in the year prior to the event. Gross income and housing wealth are measured at 2015 price level and winsorized at the 2.5 and 97.5 percentiles within each year.

	Low debt	High debt	All
Sample mean at t-1			
Age	50.683	42.616	48.604
Children	0.436	0.635	0.487
Gross income, 1000 DKK	389	394	391
Household debt, 1000 DKK	760	1215	877
Loan arrears	0.006	0.027	0.012
Consultation with psychiatrist	0.010	0.010	0.010
Consultation with psychologist	0.010	0.014	0.011
Depression treatment	0.002	0.003	0.003
Psychiatric hospital	0.010	0.010	0.010
Any mental health treatment	0.028	0.033	0.030
N	405,832	140,918	546,750

Table 2 – **Sensitivity to controls** Notes: The table reports results from simplified versions of Model 1 in which the set of indicators for event time has been replaced by a single post-event dummy. The estimate shown in each column of the Table is the coefficient on the interaction term between this dummy and the indicator for high debt. In all columns, the dependent variable is our comprehensive measure of mental health problems. Observations from years -3 to +2 relative to the event year are included in the estimation. Columns (1) to (8) report OLS estimates with different sets of controls. Columns (9) and (10) report results from IV regressions where we instrument the dummy for high debt with the number of years since first real estate purchase. In columns (9) and (10) the F-statistic refers to the cluster robust Kleibergen-Paap rk Wald F-statistic of weak identification. Std. errors are estimated allowing for clustering at the level of the individual.

	(1)	(2)	(3)	Baseline (4)	(5)	(6)	(7)	(8)	(9)	(10)
High debt X post event	0.00324***	.00298***	0.00328***	0.00288***	0.00283***	0.00256***	0.00272***	0.00313***	0.01446*	0.00980***
	(0.00059)	(0.00061)	(0.00062)	(0.00061)	(0.00061)	(0.00062)	(0.00064)	(0.00061)	(0.00783)	(0.00293)
Controls:										
Year fixed effects	X	X	X	X	X	X	X	X	X	X
Gender X post		X	X	X	X	X	X	X	X	X
Age at health shock X post		X	X	X	X	X	X	X	X	
Age at purchase X post										X
Children X post		X	X	X	X	X	X	X	X	X
Municipality X post			X	X	X	X	X	X	X	X
Income X post			X	X	X	X	X	X	X	X
Diagnosis X post				X	X	X	X	X	X	X
Self-employed X post					X					
Low liquidity X post						X				
High unsecured debt X post							X			
Indvidual fixed effects								X		
Estimation method	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	IV	IV
First stage F-statistic	-	-	-	-	-	-	-	-	1314.72	9086.28
R2	0.00229	0.00723	0.01313	0.02588	0.02600	0.02589	0.02589	0.49420	-	-
N	2,409,549	2,409,549	2,408,442	2,408,442	2,407,965	2,408,442	2,408,442	2,385,654	2,234,554	2,234,554

Figure 1 – Correlation between mental health problems and leverage Notes: The figure shows a binned scatter plot of our comprehensive measure for mental health problems against within-year percentile ranks of the loan-to-value ratio. The sample is homeowners aged 30-60 years with non-zero debt.

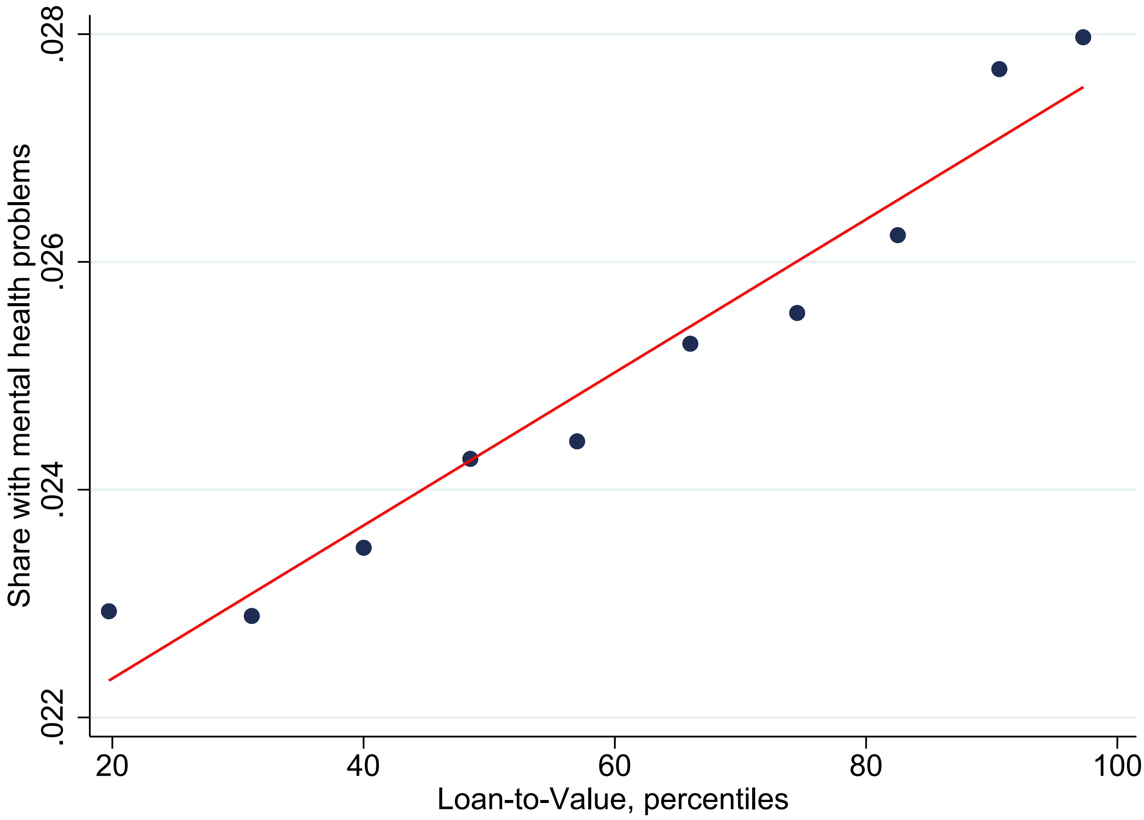


Figure 2 – Mental health responses to health shocks, high- vs. low-debt individuals
Notes: The figure shows the impact of an inpatient hospitalization on mental health problems for individuals with different ex ante degrees of leverage. The dependent variable is our comprehensive measure of mental health problems. The left graph shows dynamic responses separately for high- and low-debt individuals. These are constructed by estimating Model (1) and plotting sample averages of $\hat{\lambda}_j + \boldsymbol{X_i}\hat{\alpha}_j$ (red graph) and $\hat{\lambda}_j + \hat{\beta}_j + \boldsymbol{X_i}\hat{\alpha}_j$ (blue graph) for each value of $j \in \{-7; 7\}$. The right graph shows the difference in responses between high- and low-debt individuals, corresponding to the estimated $\hat{\beta}_j$ from Model (1). Vertical bars indicate 95% confidence intervals. Standard errors are clustered at the level of the individual.

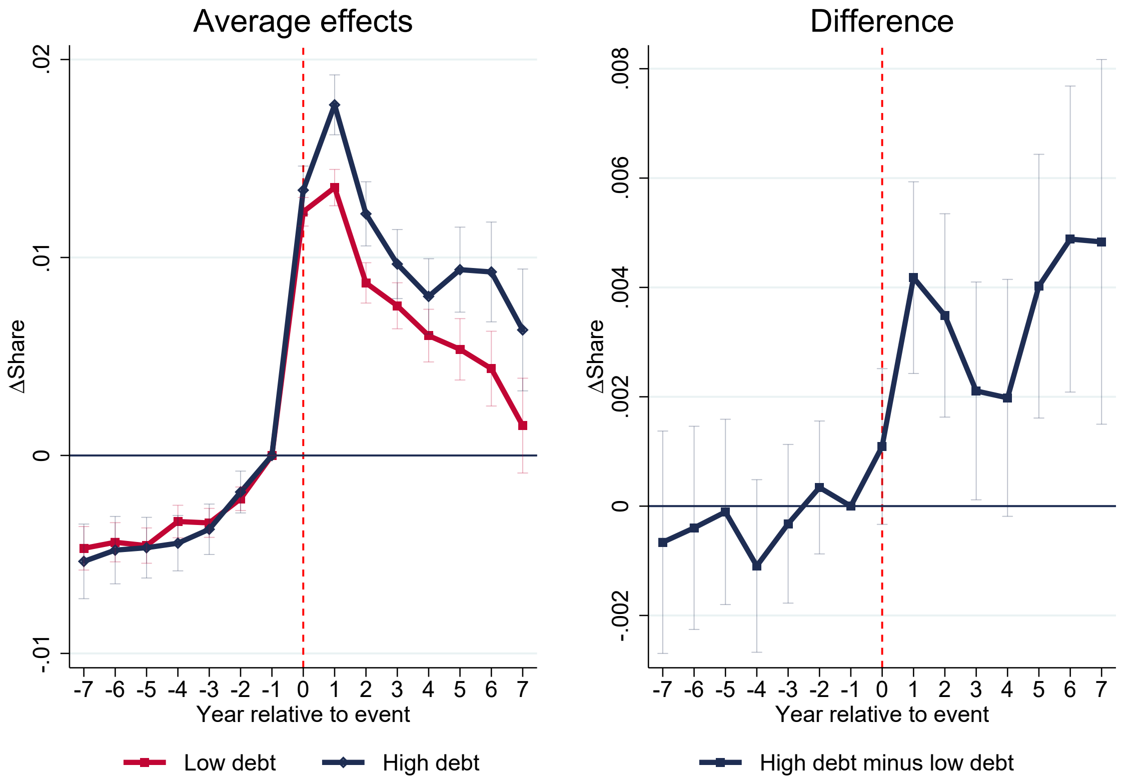


Figure 3 – Mental health components Notes: The figure shows the impact of an adverse health shock requiring hospitalization on various measures of mental health problems. The dependent variables are indicators for whether the individual had any consultations with a psychologist (Panel A); had any consultations with a psychiatrist (Panel B); received hospital treatment for depression (Panel C); received any type of care at a psychiatric hospital (Panel D). Graphs on the left show dynamic responses separately for high- and low-debt individuals while graphs on the right show differences between the two groups, corresponding to the estimates of the β_j in Model (1). Vertical bars indicate 95% confidence intervals. Standard errors are clustered at the level of the individual.

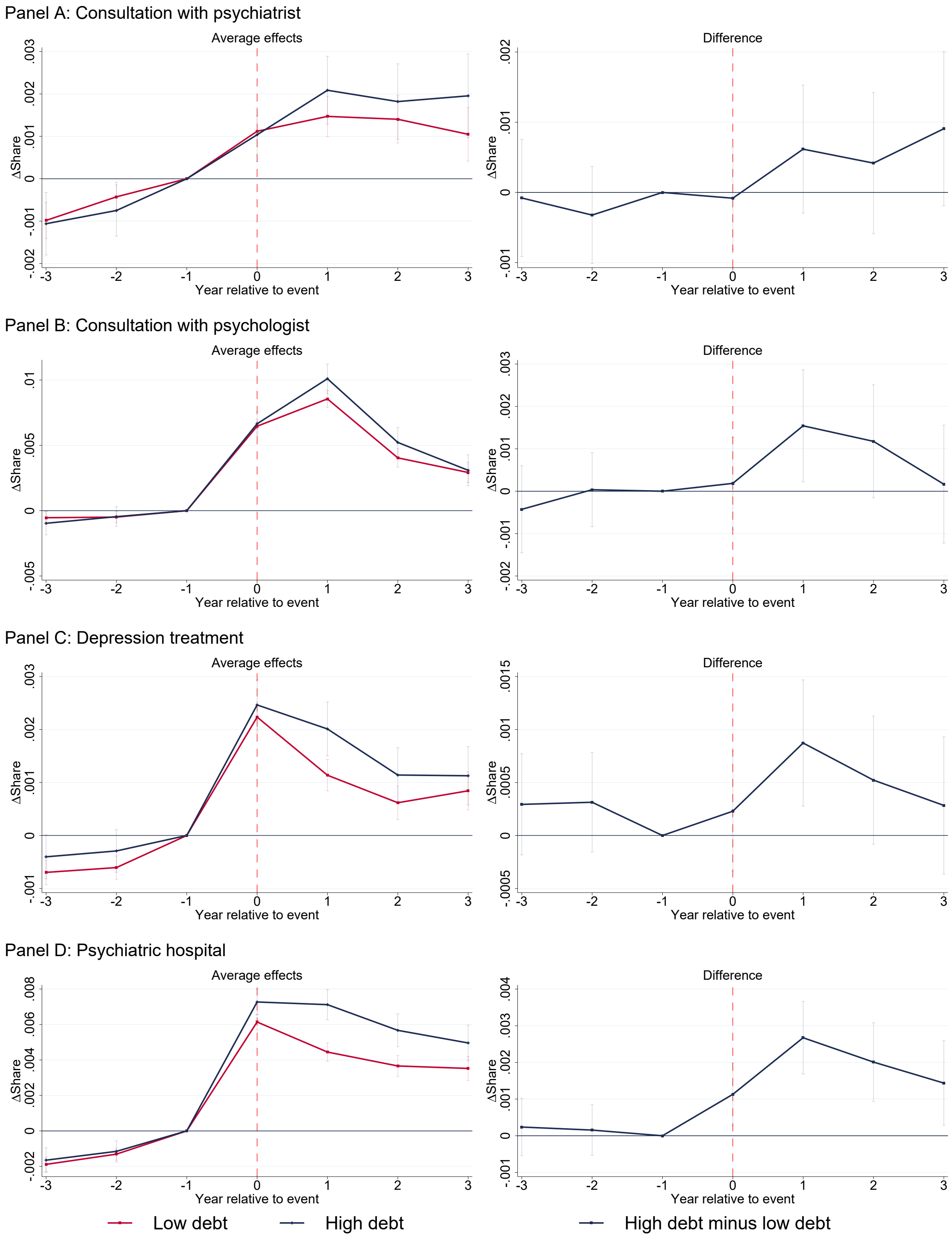


Figure 4 – Economic consequences of a health shock Notes: The figures shows the impact of an inpatient hospitalization on earnings (panel A), and on the share of individuals in loan arrears (panel B). Graphs on the left show dynamic responses separately for high and low-debt individuals while graphs on the right show differences between the two groups, corresponding to the estimates of the β_j in Model (1). Vertical bars indicate 95% confidence intervals. Std. errors are clustered at the individual level.

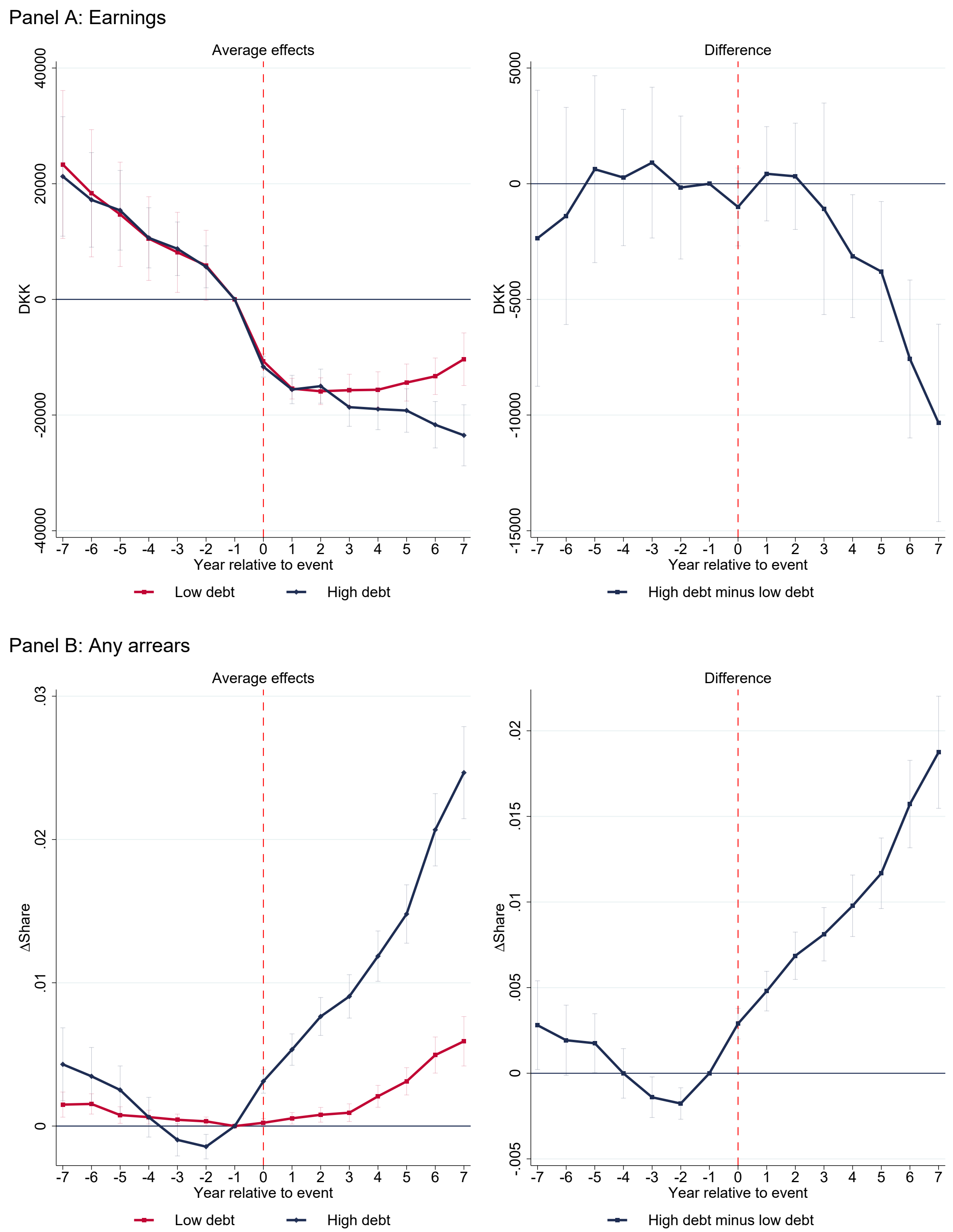


Figure 5 – Heterogeneous effects of debt on mental health responses to health shocks
Notes: The figure shows results from split-sample estimations of Model (1) for adverse health shocks. The dependent variable is our comprehensive measure of mental health problems. Each graph shows differences in effects between high and low-debt individuals, corresponding to the estimates of the β_j. In Panel A, the sample is split by whether the event happens during the financial crisis in 2008-09 or not. In Panel B, the sample is split by whether the individual has children in the year before the event. In Panel C, the sample is split by gender. Std. errors are clustered at the level of the individual.

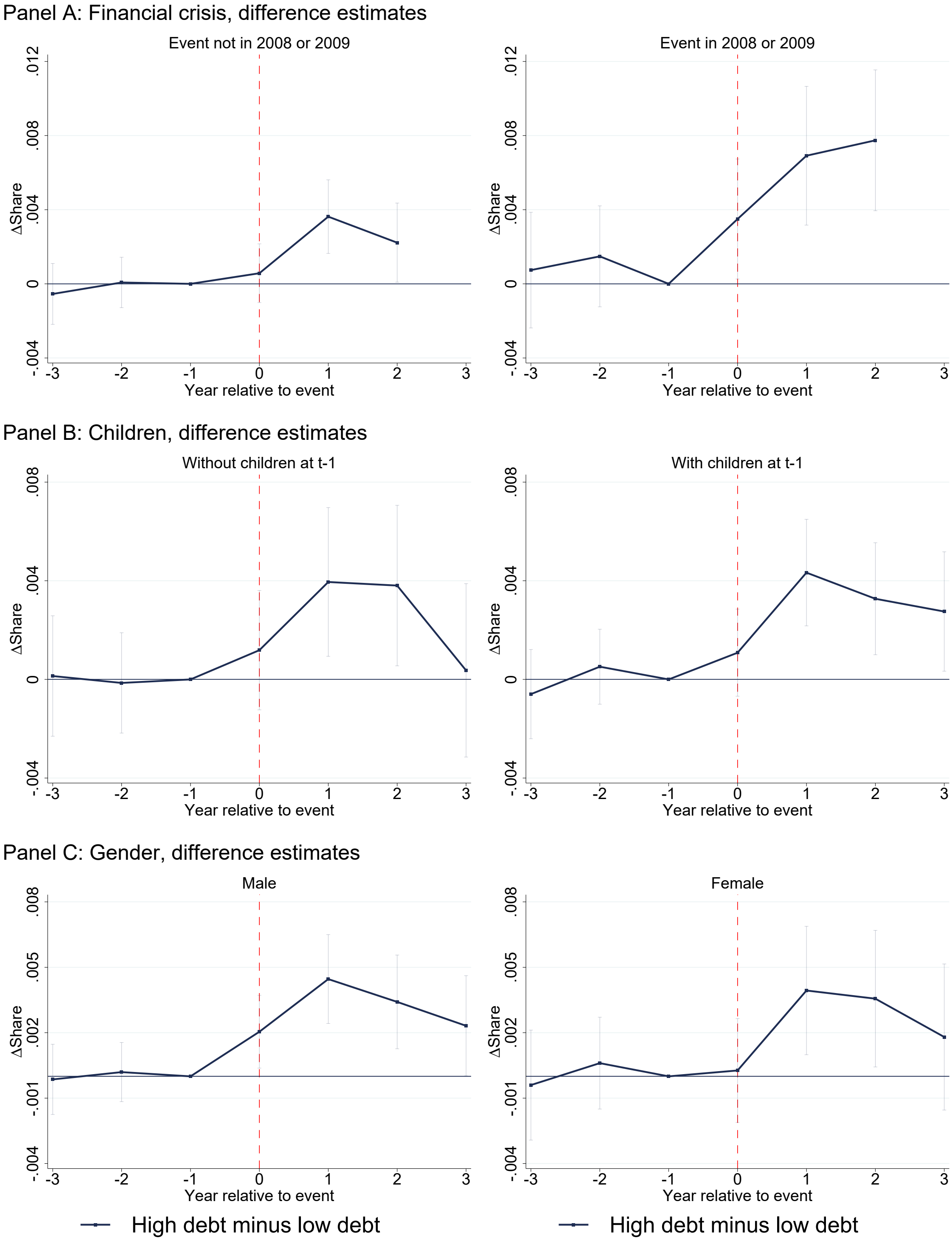
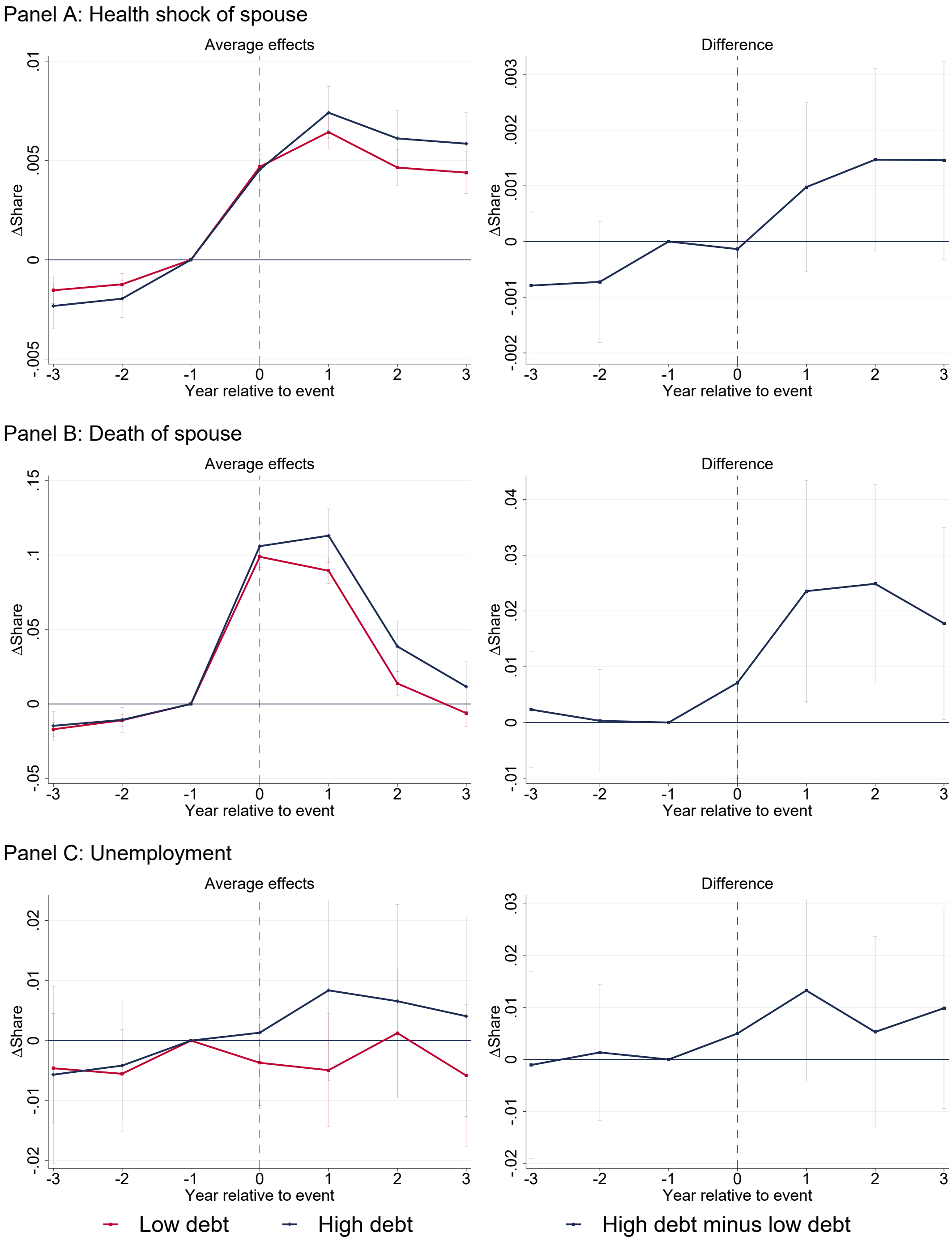


Figure 6 – Mental health responses to other adverse shocks Notes: The figure shows dynamic responses for our comprehensive mental health measure around the time of various adverse shocks. Graphs on the left show responses separately for high- and low-debt individuals, while graphs on the right show differences between the two groups. The adverse shocks are: a somatic health shock to the individual's spouse (Panel A), the death of a spouse (Panel B), and unemployment following a mass layoff event (Panel C). In panel (a) and (b) we include the same controls as in Model (1) except for the controls capturing the type of diagnosis. In panel B, we pool the small municipalities Læsø and Frederikshavn to one municipality in the estimation to calculate the average effects. In panel C, we control for age and gender interacted with event time, and year fixed effects. Std. errors are clustered at the level of the individual in all panels.



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8 Appendix: Additional Tables and Figures

Table H.1 – Distribution of diagnosis types at inpatient hospitalization event Note: The table shows the diagnosis type associated with the first inpatient hospitalization in the event year.

	Low debt		High debt	
	Share	*Frequency*	*Share*	*Frequency*
Infectious and parasitic diseases	0.02	8,117	0.02	3,100
Neoplasms	0.09	37,337	0.06	8,314
Endocrine, nutritional and metabolic diseases	0.02	8,928	0.02	3,382
Diseases in blood and blood-forming organs	0.01	2,029	0.01	705
Diseases in nervous system	0.03	11,363	0.03	3,664
Diseases of the eye and adnexa	0.01	3,652	0.01	986
Diseases of the ear and the mastoid process	0.01	3,247	0.01	986
Diseases of the circulatory system	0.13	53,976	0.10	13,387
Diseases of the respiratory system	0.05	20,697	0.06	7,891
Diseases of the digestive system	0.10	41,801	0.11	15,078
Diseases of the genitourinary system	0.09	35,713	0.09	12,964
Diseases of the skin and subcutaneous tissue	0.02	8,117	0.03	3,523
Diseases of the muschuloskeltal system and connective tissue	0.11	43,424	0.10	13,669
Congenital malformations, deformations and chromosal abnormalities	0.01	2,029	0.01	705
Symptoms and undefined conditions	0.11	45,859	0.13	17,756
Broken bones and joint damages	0.07	27,597	0.07	9,442
Lesions, wounds and other traumas (external causes)	0.09	34,902	0.10	14,092
Examination, preventive care etc. w/o symptoms or diagnosis	0.04	17,451	0.08	11,273
N	1	405,832	1	140,918

Figure H.1 – Robustness: Measurement of high and low debt Notes: The figure shows the impact on mental health of a health shock by various definitions of ex ante high and low debt. In panel (a) high-debt individuals are home-owners in the top 10% of the LTV distribution in the year prior to event and low debt are the remaining home-owners. In panel (b) the high-debt individuals are home-owners who are both in the top 25% of the distribution of LTV and DTI in the year prior to the event and low-debt individuals are the other home-owners. In panel (c) we define high-debt individuals as the homewoners in the top 25% of the LTV distribution and low-debt individuals as home-owners in the bottom 25% of the LTV distribtion. Std. errors are clustered at the individual level.

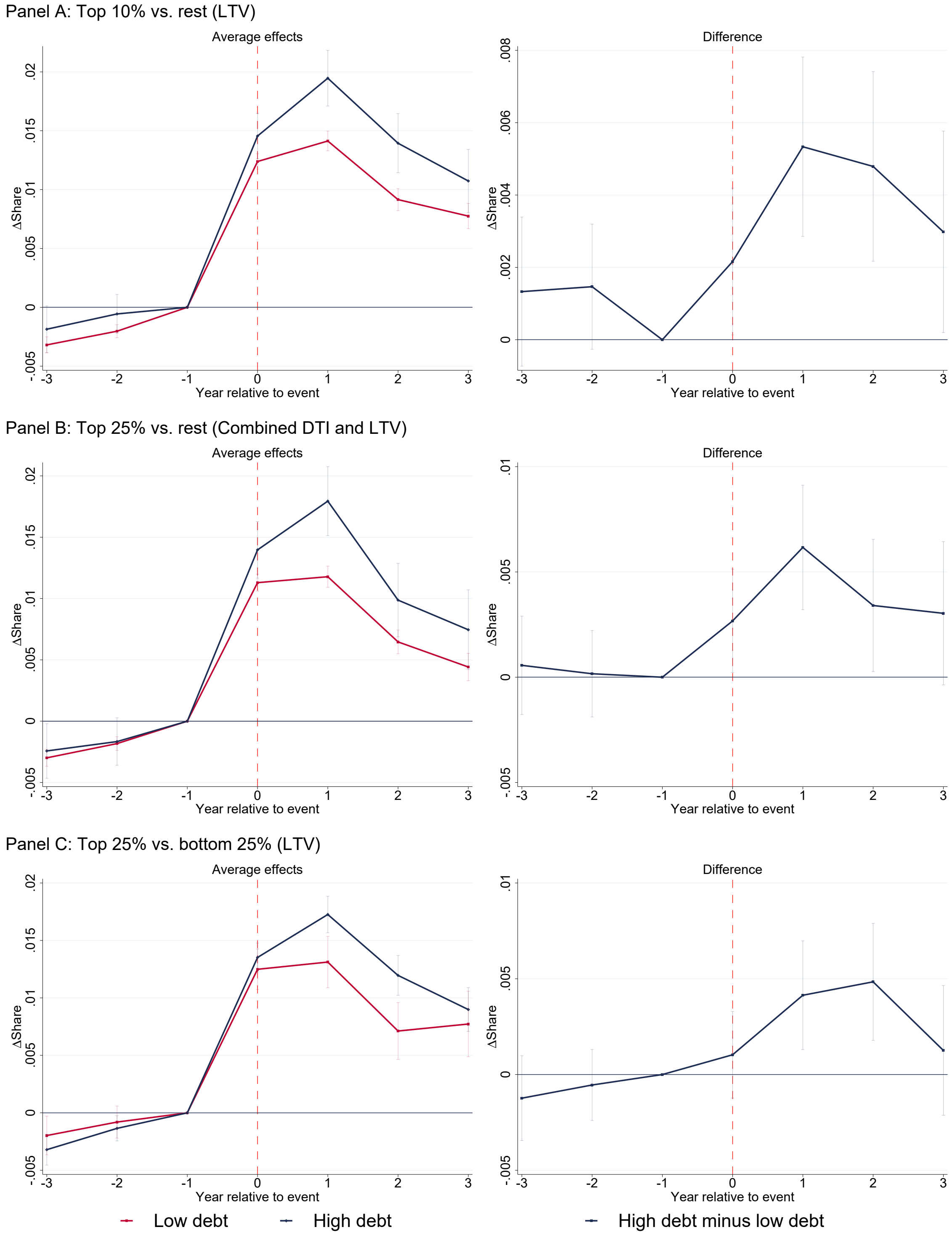


Figure H.2 – Robustness: Different age restrictions on sample Notes: The figure shows the impact on mental health of a health shock by ex ante leverage for different age restrictions on the sample. Std. errors are clustered at the individual level. Individuals are included in the estimation when they are within 30-70 years of age (panel A), 30-80 years of age (panel B), 40-60 years of age (panel C) and 50-70 years of age (panel D). Std. errors are clustered at the individual level

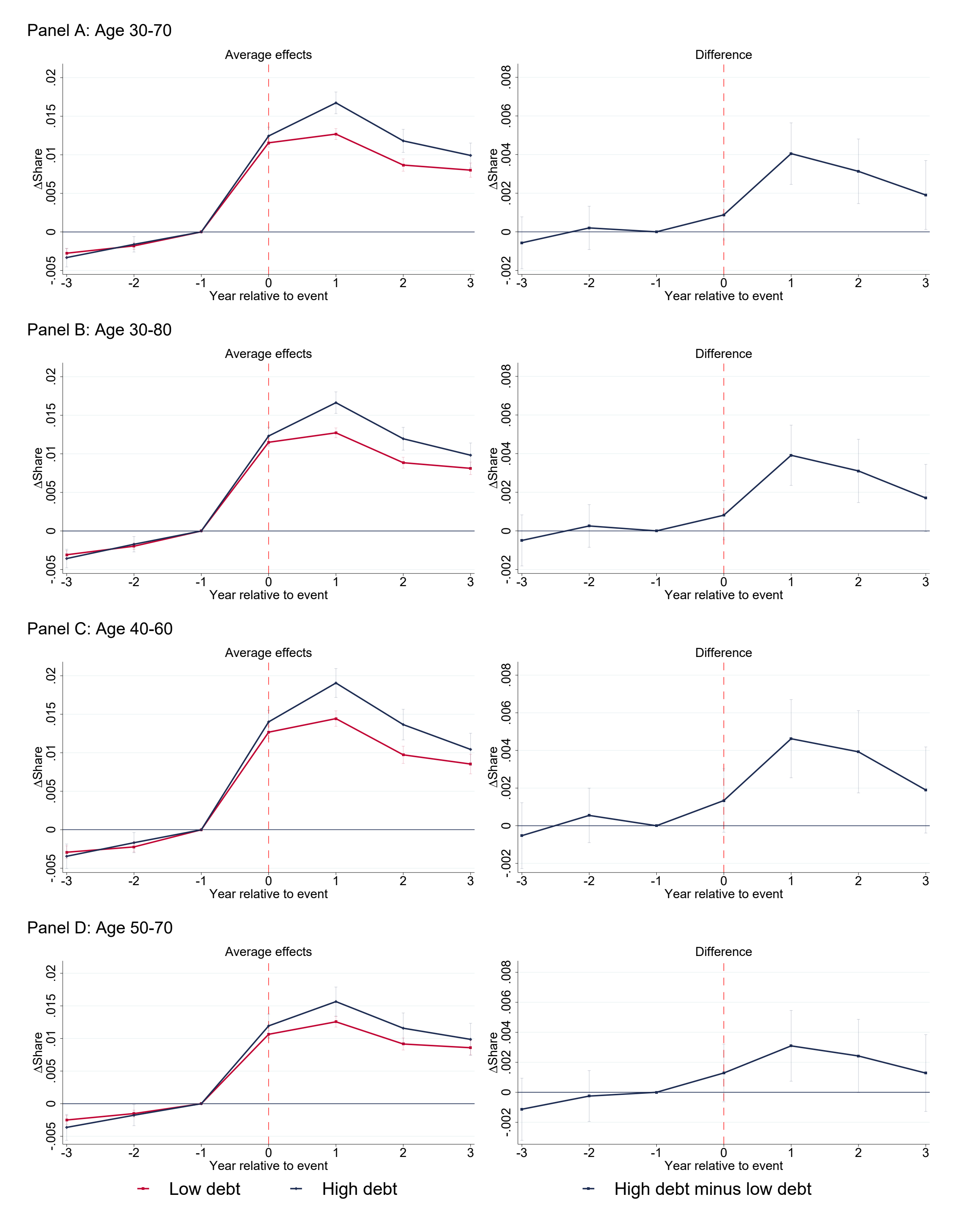


Figure H.3 – Robustness: Further controls for diagnosis Notes: The figure shows the impact on mental health of a health shock by ex ante leverage including controls for all diagnoses associated with subsequent inpatient hospitalizations in the event year. Std. errors are clustered at the individual level.

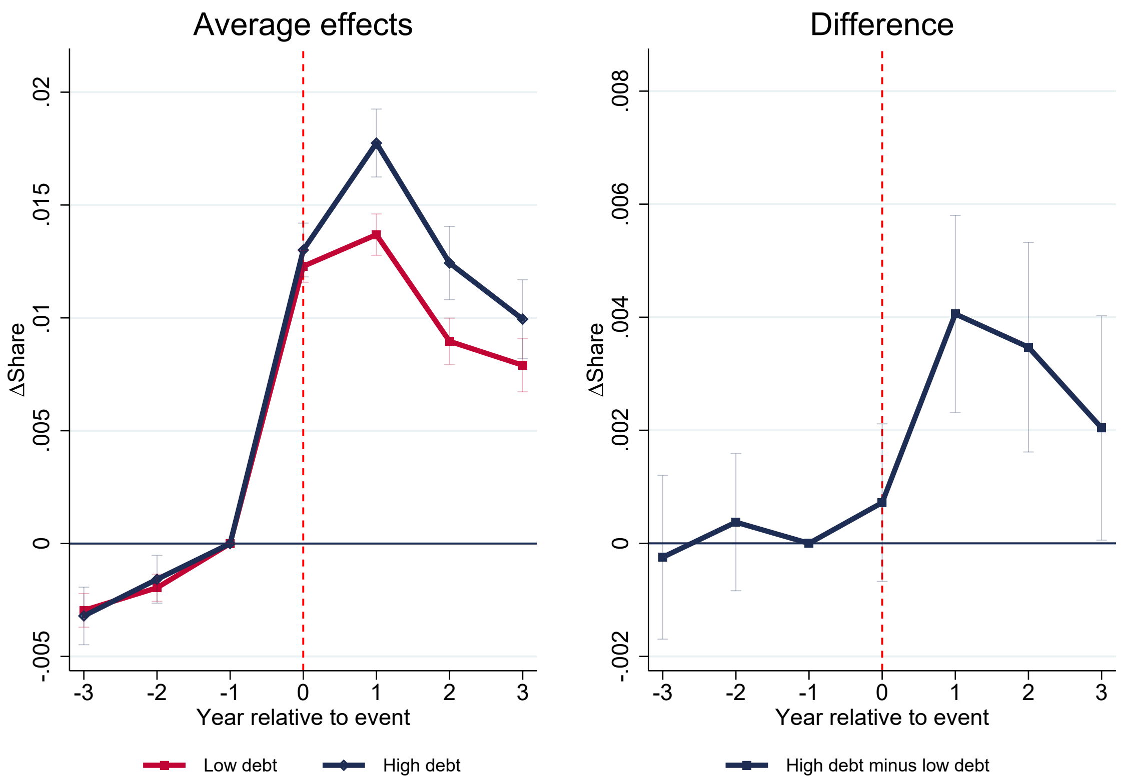


Figure H.4 – Impact of specific circulatory diseases on mental health by ex ante leverage Notes: The figures shows the effect on mental health and components of our comprehensive mental health measure of a more specific health shock by ex ante leverage. In this figure a health shock is limited to inpatient hospitalizations with the diagnoses: Acute myocardial infarction , other ischaemic heart diseases, symptomatic heart disease, other heart diseases, and cerebrovascular diseases corresponding to ICD-10 codes: I20-I69. The dependent variable is a dummy for suffering from mental health problems (Panel A), having any consultations with a psychologist (Panel B), and having any contact with a psychiatric hospital (Panel C). Std. errors are clustered at the individual level.

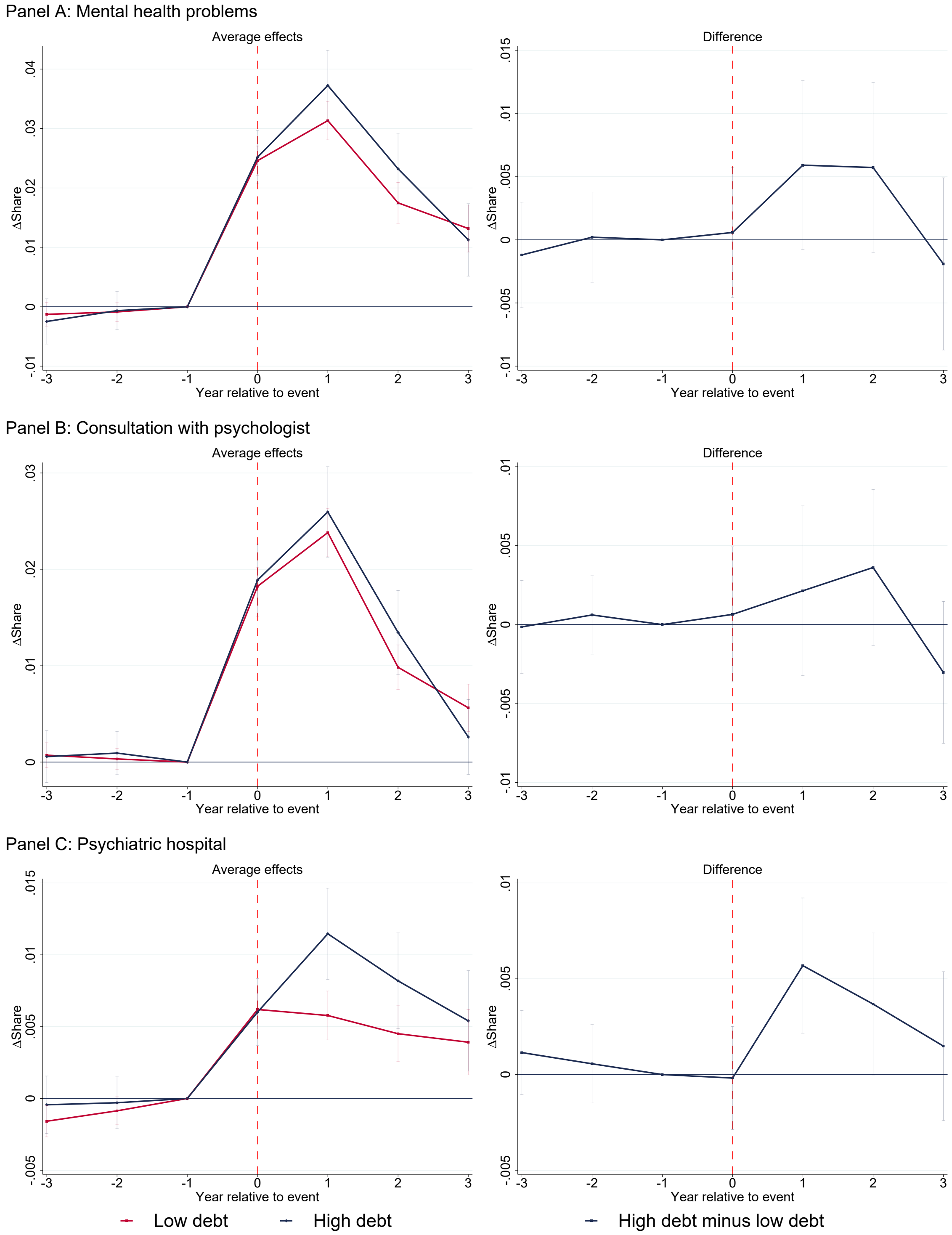


Figure H.5 – Impact of a health shock on suicide and intentional self-ham by ex ante leverage Notes: The figures shows the effect on suicides (attempts with and without death) and intentional self-ham of a health shock by ex ante leverage. The dependent variable is a dummy that takes the value one if the individual has committed suicide, attempted to commit suicide or committed other intentional self-harm time t and zero otherwise. Suicide attempts and intentional self-harm are defined by the ICD-10 codes X60-X87. Std. errors are clustered at the individual level.

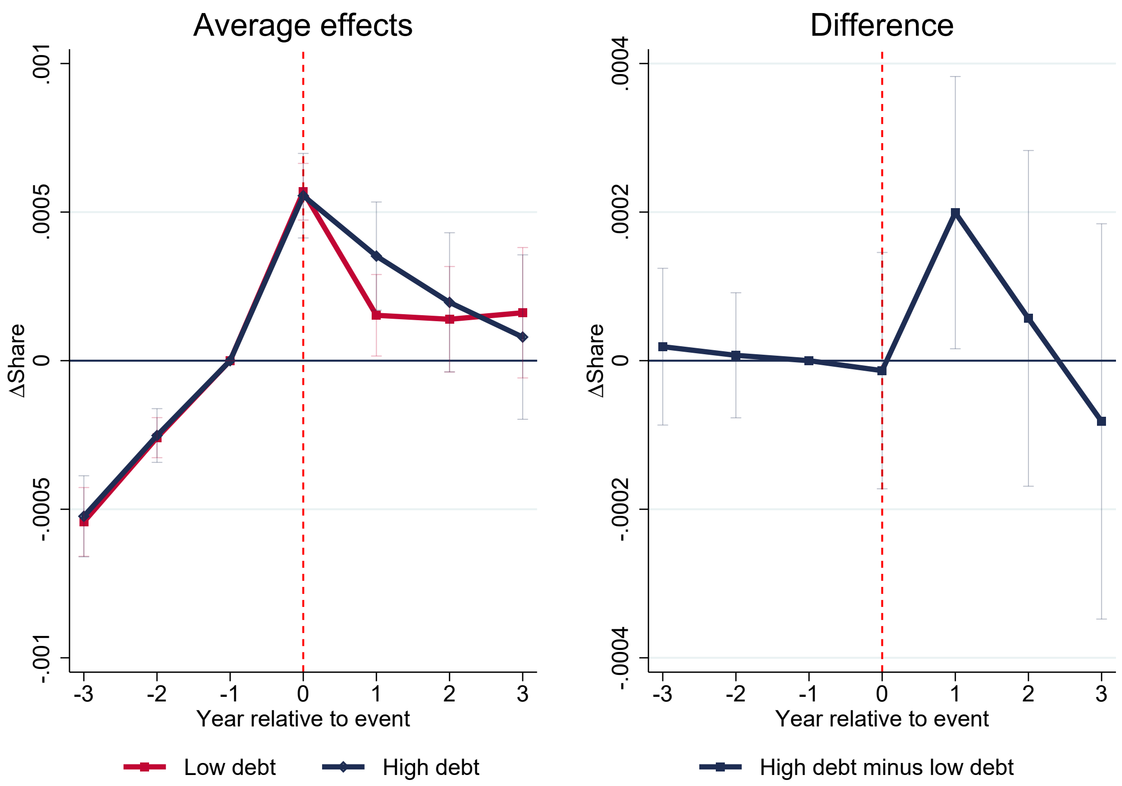


Figure H.6 – Impact of a health shock mental health (not including psychologist consultations) by ex ante leverage Notes: The figures shows the effect on mental health problems of a health shock by ex ante leverage. In this figure we do not include psychologist consultations in the measurement of mental health problems. Std. errors are clustered at the individual level.

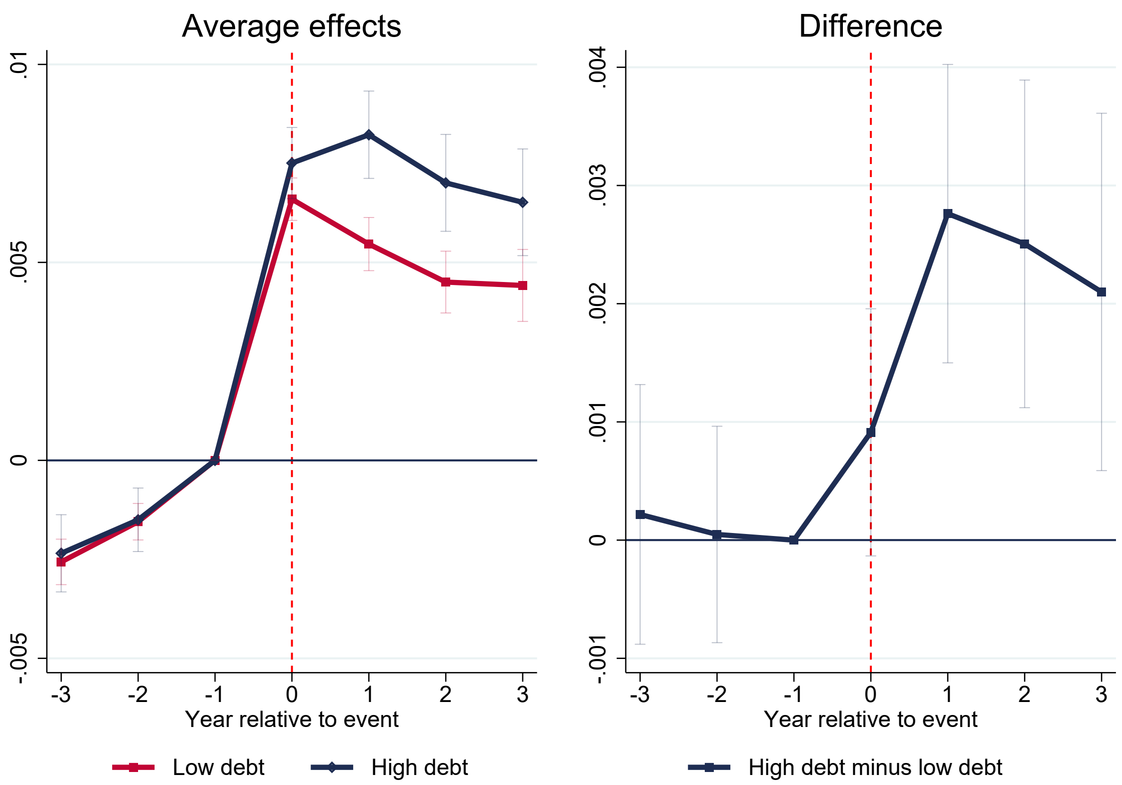
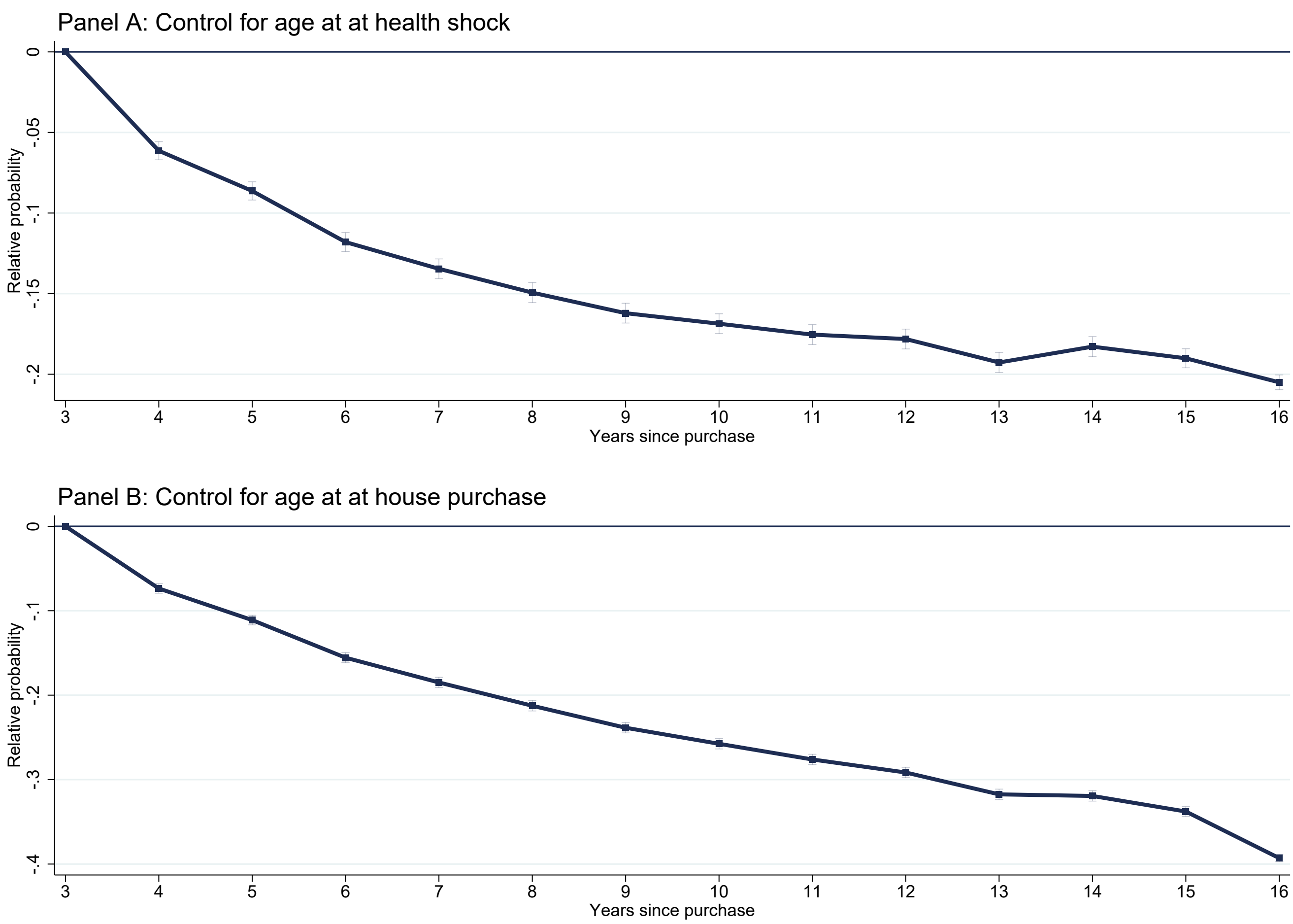


Figure H.7 – Years since first purchase and probability of having high debt Notes: The figure illustrates the relevance of the instrument used in the IV regressions reported in Table 2. To produce the graphs, we regress the indicator of high debt in the year before the event on a set of indicator variables for the number of years between the first home purchase and the somatic health shock (right-censored at 16 years), as well as a set of controls. In each panel, we plot the estimated coefficients on the indicators for time since first purchase, with 3 years as the omitted category. Panel A shows results for a specification including the same controls as in Model 1, including age at the time of the health shock. In Panel B, we instead control for age at the time of the first home purchase. Both panels show that individuals who bought their first home only a few years before they experience the somatic health shock are more likely to have high debt in the year before the shock than individuals who purchased their first home many years before the shock. Std. errors are clustered at the level of the individual.



Chapter 3

Monetary Policy and Inequality

Monetary Policy and Inequality*

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November 30, 2020

Abstract

We analyze the *distributional effects* of monetary policy on income, wealth and consumption. For identification, we exploit administrative household-level data covering the entire population in Denmark over the period 1987-2014, including detailed information about income and wealth from tax returns, in conjunction with exogenous variation in the Danish monetary policy rate created by a long-standing currency peg. Our results consistently show that all income groups gain from a softer monetary policy, but that the gains are monotonically increasing in the ex-ante income level. Over a two-year horizon, a decrease in the policy rate of one percentage point raises disposable income by less than 0.5% at the bottom of the income distribution, by around 1.5% at the median income and by around 5% at the top. Moreover, the effects on asset values through increases in house prices and stock prices are larger than the effects on disposable income by more than an order of magnitude and exhibit a similar monotonic income gradient. We show how all these distributional effects reflect systematic differences in the exposure to the direct (e.g. leverage) and indirect (e.g. business income) channels of monetary policy. Consistent with the main results for disposable income and asset values, we also find that the effects on net wealth and consumption (car purchases) increase monotonically over the ex-ante income distribution. Our estimates imply that softer monetary policy increases income inequality by raising income shares at the top of the income distribution and reducing them at the bottom.

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1 Introduction

While macroeconomics has traditionally focused on the *aggregate effects* of monetary policy (e.g. Woodford, 2003; Gali, 2015), both scholars and policymakers increasingly highlight the role of heterogeneous households in monetary policy transmission (e.g. Slacalek, Tristani and Violante, 2020). From this point of departure, we set out to estimate the *distributional effects* of monetary policy: the effect on household income, wealth and consumption at each position of the income distribution. This endeavor is important for at least three reasons.

First, it informs recent heterogeneous-agent models of monetary policy where the relative importance of direct and indirect channels of monetary policy is key (Kaplan, Moll and Violante, 2018; Auclert, 2019). The distributional effects of monetary policy arise because households have different balance sheets and occupations and therefore also different exposure to the various channels. By studying how changes in, for instance, interest expenses, salary income and housing prices contribute to the overall effects of monetary policy, we provide direct evidence on the importance of each channel.

Second, the distributional effects of monetary policy have important implications for the aggregate effects. Since the marginal propensity to consume is lower for less affluent households (Japelli and Pistaferri, 2014), a given increase in aggregate income creates a larger increase in aggregate demand if it accrues to households at the bottom of the income distribution.

Third, our analysis sheds new light on how monetary policy shapes inequality. Some argue that softer monetary policy primarily helps the low-skilled by creating new jobs (Draghi, 2016), but others emphasize that the well-to-do also benefit through increasing asset prices, which makes the net effect on inequality ambiguous (Bernanke, 2015). While existing papers have studied the effect of monetary policy on summary measures of inequality constructed from top-coded survey data (Coibon et al., 2017), our analysis traces the effect of monetary policy to each position of the income distribution and highlights the important role of the top 1%.

Our analysis draws on rich, administrative micro-data from Denmark. The main data source is population-wide tax records with detailed information about income and balance sheets for the entire period 1987-2014: more than 70 million individual-year observations. In the tax records, we observe all major components of households' disposable income (e.g. salaries, dividends and interest expenses) as well as the main balance sheet components (e.g. housing, stocks and debt). This information is generally reported by third parties such as employers and financial institutions and mismeasurement due to tax evasion is therefore limited (Alstadsæter et al, 2019). Matching observations on unique personal identifiers, we combine the tax records with a number of other administrative data sources: the auto register with information on car purchases, an important component of durable consumption, as well as the population register with information on demographics, place of residence and household structure.

This granular information allows us to estimate how monetary policy differentially affects the income, wealth and consumption dynamics of households at different income levels and over different time horizons. Specifically, we consider 21 income groups corresponding to different positions of the income distribution (p0-5,...,p95-99, top1%). Our dependent variable is always the change in a household-level outcome from the *ex ante* level to some future period scaled by disposable income. This approach is reminiscent of the local projections method in empirical macroeconomics (Jordà, 2005).

A key challenge is the potential endogeneity of monetary policy to local economic conditions. We address this challenge by exploiting the long-standing commitment of the Danish monetary authorities to exchange rate stability: for more than three decades the Danish Krone has been pegged to the German Mark (1987-1998) and the Euro (1999-2019) and the exchange rate has been virtually fixed throughout this period. As central banks cannot use the policy rate to control demand for currency and at the same time use it to pursue other policy objectives (Mundell, 1963; Fleming, 1962), Denmark typically imports its monetary policy stance from Frankfurt: when the European Central Bank changes its leading interest rate, the Danish Central bank generally changes its rate by the same amount on the same day to restore the interest rate differential that is consistent with a fixed exchange rate. This introduces a source of exogenous variation in the Danish policy rate that we exploit for identification.[1]

Our main explanatory variable is the change in the Danish policy rate interacted with indicators of the household's position in the *ex ante* income distribution. We address the endogeneity concern by using the change in the German/Euro policy rate as an instrument for the change in the Danish policy rate while at the same time controlling for the German/Euro business cycle. This implies that the identifying variation comes from the German/Euro monetary policy shock in the sense of Christiano et al. (1999). In robustness tests, we include time fixed effects that absorb all aggregate shocks (including the aggregate effect of monetary policy) and expand the set of macro controls in several dimensions.

Our first set of results concerns the effects of monetary policy on disposable income. We find that softer monetary policy increases disposable income at all income levels. However, the gains are highly heterogeneous and monotonically increasing in the income level: a decrease in the

[1]A recent paper uses the same source of variation to estimate the effect of monetary policy on real GDP growth (Jordà et al., 2020)

policy rate of one percentage point raises disposable income by less than 0.5% at the bottom of the income distribution, by around 1.5% at the median income level and by around 5% for the top-1% over a two-year horizon.[2] We identify the key economic channels underlying this gradient by estimating the effect on each component of disposable income separately. Consistent with theory and the perception of policymakers (Draghi, 2016), we find that softer monetary policy has the largest effect on salary income at relatively low income levels (around the 25th percentile) reflecting a sizeable increase in employment for this group. However, most other components of disposable income contribute to a positive income gradient. First, gains in the form of higher business income and stock market income (indirect channels) are highly concentrated at the top of the income distribution. Second, gains in the form lower interest expenses (direct channel) are increasing in income reflecting, at least partly, that higher-income households have more debt relative to their disposable income.[3] Summing up all the channels, the gains from a lower monetary policy rate are increasing over the income distribution.[4]

Our second set of results concerns the effects of monetary policy on asset values through changes in housing prices and stock prices.[5] We find that softer monetary policy creates capital gains for all income groups with a pronounced positive income gradient: a one percentage point decrease in the policy rate increases asset values by around 20% of disposable income at the bottom of the income distribution over a two-year horizon and by around 75% of disposable income at the top. Comparing to the previous set of results, this suggests that the gains created by softer monetary policy in the form of appreciation of assets are generally much larger, more than an order of magnitude at all income levels, than the gains in the form of a higher disposable income. The gradient is largely explained by the fact that households at higher income levels hold more assets relative to their disposable income. Expressed relative to total asset values, the estimated effects range from around 6% at the bottom to around 8% at the top.

We conduct a range of tests to probe the robustness of these core results. We show that the income gradient survives when we include time fixed effects that absorb the (average) effect

[2]The positive income gradient in the effects of monetary on disposable income is consistent with earlier evidence that high-income households are more exposed to aggregate fluctuations in the economy (Parker and Vissing-Jorgensen, 2009).

[3]The positive correlation between household income and leverage also emerges in other countries, including the United States (Kuhn et al., 2015).

[4]Changes in government transfers and taxes tend to mitigate the income gradient suggesting that economies with less fiscal redistribution than Denmark may exhibit an even steeper income gradient in the effects of monetary policy.

[5]Specifically, the outcome in these regressions is the *ex ante* value of the household's assets multiplied by the change in the relevant market price index (i.e. a local house price index for real estate and the market index for stocks) measured relative to disposable income.

of *any* macro shock. It remains largely unchanged when we add *ex ante* macro forecasts to the set of controls to account for the (heterogeneous) effect of expectations, and it becomes only slightly flatter when we further control for *ex post* Danish exports to account for the (heterogeneous) effect of monetary policy in Frankfurt through external demand for Danish products. It is robust to controlling for serial correlation in the policy innovations and to modifying the instrument to account for the zero lower bound. Finally, the income gradient remains similar when we augment the baseline model with household fixed effects to control for time-invariant household-level characteristics.

As a first extension of the core analysis, we investigate the role of household debt in the transmission of monetary policy. Concretely, we estimate an augmented model where the effect of changes in the policy rate is allowed to vary with leverage at each income level. We find that, when we compare households with similar leverage, the estimated effect on disposable income and housing wealth is very similar across income groups. The most notable exception is the top 1% who benefit much more from softer monetary policy than any other income group at each level of leverage.[6]

Second, we study the distributional effects of monetary policy on consumption and wealth accumulation. Theoretically, the gains created by softer monetary, whether in the form of disposable income or asset values, must be either consumed or added to the household's wealth due to the intertemporal budget constraint. However, by changing market interest rates, monetary policy also changes the trade-off between consumption and savings more broadly as captured by the intertemporal elasticity of substitution. Accounting for both of these channels of monetary policy as well as others (e.g. through portfolio choices), we use household-level changes in net wealth and in new cars as outcomes. The results indicate that the wealth and consumption gains of softer monetary policy are both highly unequally distributed. The effects on net wealth, ranging from 20-30% of disposable income below the median income to almost 80% of disposable income at the top, are strikingly similar to the estimated price effects on asset values, which is consistent with an important role for "saving by holding" (Fagereng et al., 2019).

Third, as exposure to the various channels of monetary policy varies systematically over the life cycle, we also examine the distributional effects of monetary policy in the age dimension. The analysis employs a modified version of the baseline model where the change in the policy rate is interacted with indicators of age rather than indicators of income. We find that effects on disposable income are hump-shaped in age, largest for the middle-aged and almost zero

[6]The estimated effects on stocks observe the opposite pattern: the effect is larger for households with low or no debt and generally much larger for households in the top-1% within groups with similar leverage.

for the young and the elderly. This pattern reflects a host of differences, most importantly that the middle-aged are more often self-employed and have more debt than other age groups and therefore benefit more from higher business income and lower interest expenses when the policy rate is lowered. By contrast, the effect on asset values is monotonically increasing in age reflecting that average portfolios of stocks as well as housing assets are increasing in age. In sum, softer monetary policy creates the largest benefits for the middle-aged through income and for the elderly through asset prices while the young benefit very little through either channel.

Finally, to relate our findings to the broader literature examining trends in inequality (e.g. Piketty, 2014), we undertake a simulation exercise to summarize the distributional implications of our estimates. The results suggest that softer monetary policy unambiguously increases income inequality by raising the income shares at the top of the income distribution and lowering them at the bottom. Specifically, holding other factors constant, reducing the policy rate by one percentage point raises the share of aggregate disposable income by around 3% for the top-1% over a two-year horizon and lowers it by around 1.5% for the bottom income group.

A key contribution of the paper is to inform theory models about the various direct and indirect channels of monetary policy (e.g. Kaplan, Moll and Violante, 2018). For instance, our results highlight that non-labor channels (e.g. net interest income, business income, asset values) contribute importantly to both the aggregate and distributional effects of monetary policy. We also contribute to the emerging literature on monetary policy and inequality. Contrary to our findings, three recent studies find that lower policy rates are associated with less inequality (Coibon et al., 2017; Mumtaz and Theophilopoulou, 2017; Ampudia et al., 2018). Our methodology differs substantially from these papers in many respects: in particular, we estimate the effects of monetary policy on household-level outcomes rather than summary measures of inequality and we use administrative data covering the entire population rather than top-coded survey data. Further, we contribute to the broader literatures using micro-data to study the heterogeneous effects of monetary policy on firms (e.g. Kashyap and Stein, 2000; Jimenez et al 2012, 2014) and the effects of pass-through from policy rates to market interest rates on household consumption (Di Maggio et al., 2017; Flodén et al., 2019; Cloyne et al., 2019). Most similar in terms of the empirical approach is a paper that studies the heterogeneous effects of monetary policy across households with different liquidity using micro-data from Norway (Holm et al., 2020).

The rest of the paper is structured in the following way. Section 2 lays out the institutional framework underlying monetary policy in Denmark. Section 3 describes the data sources, the sample and offers descriptive statistics. Section 4 develops the empirical model and discusses identification. Sections 5-7 present the results. Section 8 concludes.

2 Background

The monetary policy rule in Denmark is simpler than in many other countries such as the U.S., the U.K., the Euro Area and Japan (Taylor, 1993). For more than three decades the Danish Krone has been pegged to the German Mark and the Euro and exchange rate stability is the overriding objective of monetary policy. In the words of Bodil Nyboe Andersen, former Governor of the Danish Central Bank: "[Our] aim is to ensure that the krone's rate against the euro is stabilized close to the central rate within ERM II, and the exchange rate is the sole basis for our monetary policy deliberations".[7]

This institutional fact is a key part of our identification strategy. Theory tells us that to keep the exchange rate fixed in an open economy, the central bank must use the policy rate to control the demand for local currency and therefore cannot at the same time use it to control other local economic conditions (Mundell, 1963; Fleming, 1962). It follows that the Danish policy rate is not decided in response to local macroeconomic outcomes such as inflation, income growth and unemployment.

While the Danish policy rate is still potentially endogenous to local economic conditions, notably because there is some alignment with the business cycles in the Euro Area, the currency peg introduces a source of exogenous variation in Danish monetary policy that we will exploit in the empirical analysis. Given the importance of the institutional framework for our identification strategy, we describe the background in some detail in this section.

2.1 Currency pegs

Denmark participated in the first attempts to reduce exchange rate volatility in Europe after the collapse of the Bretton Woods system in 1973. This cooperation evolved into the Exchange Rate Mechanism (ERM) in 1979 with exchange rates between European currencies floating within relatively narrow bands. In this period, Denmark suffered from high inflation rates and repeatedly devalued the target value of the Danish Kroner within the ERM to restore its competitiveness and reduce external imbalances. Consequently, interest rates were high because investors required a premium to compensate them for the expected future depreciation of the

[7]Speech at the annual meeting of the Danish Bankers Association on 4 December 2002, quoted in Abildgreen (2010).

Krone.

In a sharp and lasting policy reversal, the center-right government coming into office in 1982 almost immediately announced that it was firmly committed to a fixed exchange rate. The exchange rate target was first stated in terms of ECU, a weighted average of the currencies participating in ERM, and, in 1987, restated in terms of German Mark. The economic rationale of the policy was to reduce inflation by shifting market expectations. If two countries permanently maintain a fixed exchange rate, their inflation rates should eventually converge. To the extent that markets perceived the commitment to a fixed exchange rate with Germany to be credible, expectations about German inflation should anchor expectations about Danish inflation.

When twelve members of the European Union adopted the Euro in 1999, Denmark remained outside the monetary union: a popular vote in Denmark had rejected the treaty introducing the Euro and a political compromise allowed Denmark to opt out while the other ERM countries proceeded toward a common currency. The monetary collaboration between Denmark and the Eurozone was formalized in ERM II: the peg was restated in terms of Euro with a target exchange rate that was an exact conversion of the existing target in Mark.

2.2 Exchange rates and policy rates

Figure 1 illustrates the exchange rates of Danish Kroner relative to German Mark, Euro and US dollar. Between the collapse of Bretton Woods to the announcement of the fixed exchange rate regime (1973-1982), Kroner depreciated quickly against Mark losing almost half of its value in less than a decade. In the first years of the fixed exchange rate regime (1982-1987), Kroner was pegged to ECU and continued to depreciate slightly against Mark although at a much slower pace: as other ERM countries occasionally devalued their currencies against Mark, a fixed rate against ECU implied a depreciation against Mark.

In the period where Kroner was pegged to Mark (1987-1998), the fluctuations around the target rate were generally small and well within the acceptable bands of 2.25%. The only notable exception was the temporary depreciation in August 1993 in the context of massive volatility in European currency markets with speculative attacks forcing a number of European countries to adjust or even give up their currency pegs.[8] In the past two decades (1999-2020), Kroner

[8] A major attack on Kroner occurred in February 1993 when a center-left government came into office on a policy agenda emphasizing job creation and growth. The new government quickly affirmed its commitment to the fixed exchange rate and the Danish central bank successfully defended the peg to Mark with interventions in currency markets and temporary increases in the policy rate. However, when the entire ERM came under attack and it was agreed to expand the fluctuation bands from 2.25% to 15%, Kroner depreciated temporarily against Mark. The U.K. famously left the ERM as a consequence of this currency attack.

has been pegged to Euro and the exchange rate between the two currencies has been almost completely fixed. The highly volatile exchange rate of Kroner against US Dollar serves as a useful comparison.

Figure 2 illustrates the leading monetary policy rates in Denmark, Germany and the Eurozone. The spread between Denmark and Germany increased significantly after the first oil crisis in 1973 and generally hovered around 10 percentage points in the late 1970s and early 1980s reflecting the pronounced difference in inflation rates and the expected depreciation of Kroner. At the announcement of the fixed exchange rate regime in 1982, the spread quickly dropped to around 5 percentage points and, as the commitment gained credibility, continued to decrease in the following years. Since the early 1990s, the Danish policy rate has generally tracked the policy interest rate of the Bundesbank (until 1998) and the European Central Bank (from 1999) closely. The policy rate spread has been fairly constant and close to zero throughout the whole period.

The figures tell us that Danish monetary policy is conducted in accordance with standard theory for an economy with a fixed exchange rate outlined above. In normal times, Denmark effectively imports its monetary policy stance from Frankfurt: when the European Central Bank changes its leading interest rate to pursue some policy objective for the Eurozone, the Danish Central bank generally changes its rate by the same amount on the same day to restore the interest rate differential that is consistent with a fixed exchange rate. On rare occasions, however, the exchange rate objective requires that the policy rate is changed unilaterally. The Danish policy rate was temporarily raised far *above* the German / European rate when uncertainty in global currency markets caused capital to flow out of Kroner: during the speculative attacks in 1992-1993; when Mexico and Russia abandoned their currency pegs in 1994 and 1998; and when Lehman Brothers failed in 2008. More recently, the Danish policy rate has often been slightly *below* the European rate to prevent excessive capital flows into Kroner: during the European debt crisis in 2011 and when Switzerland suddenly abandoned its peg to the Euro in 2015.

In sum, by committing to exchange rate stability, Danish monetary authorities have voluntarily renounced on the ability to mitigate domestic shocks by adjusting the policy rate. In most periods, keeping the exchange fixed is achieved by mimicking the interest rate decisions made in Frankfurt; in rare periods with turmoil in global markets, it requires a temporary adjustment of the interest rate spread.

3 Data

This section describes the micro-data on income, wealth and consumption used in the main analysis. We specify the sample, describe the data sources and provide summary statistics of the key variables. The micro-data come from different administrative registers and are matched with a unique personal identifier.

3.1 Sources, variables and sample

The main source of micro-data is the Danish tax register, which contains annual information about taxable income and wealth at the individual level for the period 1987-2014. The information derives from tax returns and since tax filing is compulsory for all individuals with primary residence in Denmark, the dataset covers the entire adult population. The information is generally reliable as it is overwhelmingly reported by third parties like employers and financial institutions rather than by taxpayers themselves (Kleven et al., 2011).[9]

The tax register contains information about total taxable income as well as its various positive components (income) and negative components (deductions). The most important positive components are salary income, business income (from self-proprietorships), stock market income (dividends and realized capital gains), interest income (from deposits and bonds), government transfers (including public pensions) and private pension income (payouts from private pension accounts). The most important negative component is interest expenses. We define *disposable income* as the sum of the income components minus interest expenses and tax liabilities.[10]

The tax register also contains information about important categories of assets and liabilities.[11]. Specifically, we observe the value of deposits, listed stocks and loans as reported by financial institutions as well as the value of real estate as assessed by the tax authorities for the purpose of property taxation. As the tax value of real estate often understates the market value, we use transaction prices retrieved from the real estate register to construct local market price indeces, which allows us to approximate capital gains on real estate, including properties that do not change hands in a given period, at market value (see details in the Online Appendix). The main wealth components for which no information is available on the tax return are loans

[9] A recent paper reinforces this point by showing that tax evasion, the systematic misreporting of income on the tax return, is negligible, around 3%, in the aggregate (Alstadsæter et al., 2019).

[10] There is a small residual income category "other income", which is the sum of a large number of rare and highly diverse income types that do not fit any of the other categories. While we include other income in overall disposable income, we do not study this income component separately.

[11] While this information previously served to levy a tax on net wealth, most of it remained on the tax return when the net wealth tax was abolished in 1997 (Jakobsen et al., 2020)

from private persons and foreign banks (without a presence in Denmark); unlisted stocks; and savings in tax favored pension accounts.[12]

To study household-level consumption, we retrieve information about registration of cars from the auto register. We do not observe car values and therefore use the number of new cars registered in a given year as our key measure of consumption. This approach has several advantages relative to other consumption measures used in the literature: it has population-wide coverage and includes cars paid without external financing as opposed to measures based on auto loan balances obtained from financial institutions (e.g. Di Maggio et al, 2017) and it is not mechanically related to income and wealth as opposed to imputed measures of consumption (e.g. Holm et al., 2020).[13] However, our approach also has limitations: given that information on purchase prices is not available, we are not able to make quantitative comparisons between, say, income gains and consumption gains.

Finally, the population register provides information on age and place of residence and defines households, which is the unit of analysis throughout the paper. Two adults are considered to belong to the same household if they are married, registered partners or cohabiting partners. For variables such as income and wealth, we always take averages over adults belonging to the same household to ensure comparability across households with one and two adults. We define household age as the age of the oldest household member.

We limit the sample to households where the oldest adult member is at least 25 years old. Young households with low incomes are often students with high life-time incomes receiving considerable financial support from their parents (Andersen et al., 2020), so income rankings are not a good measure of economic resources for this group. We also exclude a small number of households with annual disposable income below a threshold of $10,000 (in 2015 prices), which presumably reflects that true income is not measured well.[14]

[12]Since the abolition of the net wealth tax in 1997, taxpayers are no longer required to complement the information reported by domestic banks with self-reported information on loans from other sources. Similarly, taxpayers are not required to provide estimated values of unlisted stocks. Tax favored pension accounts are similar to 401k in the United States: the accounts are personal and managed either by the individuals themselves or by private pension funds. Access to assets in pension accounts prior to pension age is possible, but triggers a significant penalty.

[13]A number of recent papers impute consumption from income and wealth data based on the accounting identity *consumption = disposable income + net capital gains - change in net wealth* (Browning and Leth-Petersen, 2003; Jensen and Johannesen, 2017; Eika et al., 2020). While we could, in principle, use imputed consumption as an outcome, we are concerned that systematic imputation errors may bias the results in the present context. For instance, a decrease in the policy rate causes the market value of fixed-rate mortgage loans to increase and the (unobserved) capital loss increases the imputed measure of consumption.

[14]To be precise, we use the threshold 60,000 kroner, which is lower than social benefits at the lowest rate. Measurement problems could arise due to work in the informal sector, unreported emigration or other similar reasons.

3.2 Descriptive statistics

The main goal of the analysis is to estimate how the effects of monetary policy vary with the position in the income distribution. We capture positions in the income distribution by ranking households *within* age cohorts according to a three-year average of their total income and assigning them to income groups based on the rank. We prefer ranking within age cohorts as income, wealth and consumption change systematically over the life-cycle (Friedman, 1957; Modigliani, 1964).

To provide a basis for understanding the various channels through which monetary policy may differentially affect households at different income levels, we describe the composition of income and net wealth by income group in Table 1. For expositional simplicity, the table employs only 7 groups, each corresponding to 20% of the population except that the top group is further split into three subgroups to highlight the pronounced heterogeneity at the top. Our regressions generally employ 21 groups, each corresponding to 5% of the population except that the top group is further split into two subgroups.[15]

Panel A summarizes the relative importance of the various types of income and expenses by income group. Each item is scaled by disposable income so that the sum of the income components minus expenses equals 100% within each group (except for rounding). Net government transfers are defined as transfers *from* the government in the form of pensions and benefits net of transfers *to* the government in the form of income taxes. For the bottom income group (bottom 20%), salaries and government transfers are the main income components whereas business income, stock market income, interest income and private pension income are negligible. Moving up the income distribution, the importance of salary income increases until the 90th percentile and then decreases whereas the importance of business income, stock market income and, to a lesser extent, interest income increases throughout the income distribution. In the top income group (top 1%), business income is almost as important as salary income and stock market income makes up a substantial part of disposable income. Reflecting the redistributive effects of government intervention, net government transfers decrease steeply as income increases. Interest expenses account for an increasing share of disposable income throughout the income distribution reflecting, as we shall see below, that household leverage tends to increase with income.

The differences in the composition of income is suggestive of how the quantitative effects

[15] In some regressions, we also study heterogeneous effects of monetary policy by age and thus group households by age rather than by income ranks.

of monetary policy may differ over the income distribution. For instance, if softer monetary policy increases salaries at the same rate for all income groups, it will create the largest relative increases in disposable income for the middle class; and if it lowers interest expenses at the same rate for all income groups, the relative increase in disposable income will generally be larger at higher income levels. While these considerations are instructive, they also have clear limitations. First, the price effects need not be homogeneous across households: wage rates may increase more and interest rates may decrease less for some households than for others depending on the industries they work in and the type of loans they hold. Second, monetary policy may also have heterogeneous non-price effects: unemployed workers may find jobs and start earning salary income when the business cycle is improving and households may decide to take more loans when the market interest rate is falling and the magnitude of these effects could differ markedly across groups. Our regression results generally differ from the mechanical inference based on the Table 1 by accounting for heterogeneous price and non-price effects.

Panel B summarizes the value of the various types of assets and liabilities by income group.[16] Each item is scaled by disposable income so that summing across asset classes and subtracting debt gives the ratio of net wealth to disposable income (except for rounding). Balance sheets are quite similar for the three lowest income groups (bottom 60%): net wealth amounts to around two times disposable income, real estate is by far the most important asset and financial assets are almost exclusively in the form of deposits.[17] Moving higher up in the income distribution, net wealth increases monotonically and reaches almost seven times disposable income in the top income group (top 1%). All three types of assets increase through the income distribution but not in the same proportions: deposits and housing roughly double (relative to disposable income) when moving from the bottom to the top income group, while the value of stocks increases more than twenty times. Debt also increases almost monotonically through the income distribution: it roughly doubles (relative to disposable income) when moving from the bottom to the top income group. This is tightly related to homeownership: households in higher income groups are more likely to own their home, which typically involves a significant degree of debt financing. The positive correlation between household income and leverage also emerges in other countries,

[16]When comparing the balance sheet components to the corresponding income components, one should account for the fact that not all assets and liabilities are recorded (as discussed in 3.1). For instance, the ratio of stock market income to assets in the form of stocks is more than 20% in the top income group. This is likely to reflect the importance of unlisted stocks (i.e. closely held firms) for this particular group. Since unlisted stocks are not recorded on the tax return but the income derived from such stocks is recorded, the ratio of income to recorded assets is high. Similarly, the slightly increasing ratio of interest expenses to debt over the income distribution is likely reflect that unrecorded loans, e.g. from foreign banks, are more important at higher income levels.

[17]Note that the category "deposits" also includes bonds.

including the United States (Kuhn et al., 2015).[18]

The composition of the balance sheet is suggestive of how monetary policy may affect households differentially through asset prices: if softer monetary policy increases house prices at the same rate across income groups, the gains will be only slightly increasing through the income distribution; however, if it increases stock prices at the same rate, the gains will be highly concentrated within the top income group. Again, these considerations are instructive, but do not account for heterogeneous price effects: if real estate prices are more responsive to monetary policy in some price segments than in others, it will contribute to the heterogeneity in gains and losses across households belonging to different income groups. Our regressions results account for this source of heterogeneity.

For completeness, Panel C describes six extensive margins: the fraction of individuals within each group that is net creditors, has no debt at all, holds any securities, owns any real estate, has any income (positive or negative) from self-employment, and buys a new car. All six statistics are monotonically related to income: as we move up through the income distribution, there are more net creditors, more stock market participants, more home owners, more self-employed and more households buying new cars, but less households with no debt at all.

4 Empirical design

The aim of the empirical analysis is to measure how monetary policy differentially affects the income, wealth and consumption dynamics of households at different income levels. The endogeneity of monetary policy is a key challenge and we first describe how we address it with an instrumental variable strategy in section 4.1. Next, we develop a specification that uses the instrument to estimate heterogeneous effects of monetary policy over different time horizons in section 4.2.

4.1 Instrumenting monetary policy

Our measure of monetary policy is Δi_t, the change in the Danish policy rate from period $t-1$ to period t. Since monetary policy could potentially be endogenous to the business cycle, and thus to the income, wealth and consumption dynamics we are studying, we instrument Δi_t with the change in the German/Euro policy rate Δi_t^*. The exchange rate peg ensures that

[18] In 2013, the ratio of debt to income in the United States was around 80% for households in the bottom 20% of the income distribution and peaked at around 140% for households just below the top 10% (Kuhn et al., 2015 based on data from the U.S. Survey of Consumer Finances).

the instrument is relevant: policy rate changes decided in Frankfurt are typically mimicked in Denmark with implications for the Danish business cycle.[19]. However, to the extent that the Danish and the German/Euro business cycles are correlated, an endogeneity problem remains: if Frankfurt lowers the policy rate in years where German/Euro output is below the potential, this may coincide with years where Danish output is also below the potential. To address this concern, we further control for the macroeconomic environment in Germany/Euro area. The residual variation in the instrument when conditioning on macro variables is effectively the German/Euro monetary policy shock in the sense of Christiano et al. (1999).[20] Therefore, the validity of the instrument requires that the German/Euro monetary policy shock is orthogonal to other shocks to the income, wealth and consumption processes in Denmark.

Figure 3 illustrates the variation that identifies the effects of monetary policy in our baseline model. It plots the annual change in the German/Euro policy rate (red line) and in the Danish policy rate (green line) together with the German/Euro monetary policy shock (blue bars), in this case the residual from a regression of the change in the policy rate in period t on GDP growth and inflation in periods $t-1$ and t. The macro controls absorb a lot of the variation in the policy rate. For instance, the largest negative change in the German/Euro policy rate occurred in 2009 just after the financial crisis; however, after purging for the effect of GDP growth and inflation, the monetary policy shock is slightly positive.

In the regression analysis, we conduct a number of robustness tests where we broaden the set of macro controls and thus further restrict the identifying variation in the German/Euro policy rate. Specifically, we control for *ex ante* forecasts of GDP growth and inflation, as expectations about the future macroeconomic environment may well influence monetary policy decisions, and for the macroeconomic environment in Denmark. We also control for *ex post* Danish exports to address the concern that monetary policy choices in Frankfurt may affect household outcomes through its impact on Germany/Euro area demand for Danish products.

The key advantage of the Danish institutional setting is that the currency peg introduces a highly transparent source of exogenous variation in monetary policy: Denmark to a large extent adopts the monetary policy that is decided in Frankfurt with no regard to the economic conditions in Denmark. A recent paper applies the same argument in a macro-setting exploiting

[19]Regressing the change in the Danish policy rate in period t on the change in the German/Euro policy rate in period t while controlling for German/Euro GDP growth and inflation in periods $t-1$ and t, the same macro controls as in the household-level regression below, produces a coefficient on the change in the German/Euro policy rate of 1.36 [s.e. 0.24]

[20]We show that our main results barely change when using the German/Euro monetary policy shock as an instrument for changes in the Danish policy rate.

the currency pegs of 17 advanced economies over more than a century to estimate the effect of monetary policy on real GDP growth (Jordà et al., 2020). It also resembles the argument used by recent papers estimating the effect of monetary policy on the risk-taking of Spanish banks by exploiting that the monetary policy decisions made jointly by the members of the Euro Area are partly exogenous to the economic conditions in Spain (Jiménez et al., 2012, 2014).

4.2 Specification

In the first part of the analysis, we are interested in the heterogeneous effect of monetary policy on *income* dynamics. The explanatory variable of interest is the change in the policy rate from period $t-1$ to t interacted, to allow for heterogeneity, with income group indicators. The dependent variable is the change in an income variable Y from the ex ante level (before t) to the level in either period $t+1$ or $t+2$. The model thus captures the short-term and medium-term effects of monetary policy: the change in income taken over the year where the policy rate changes and the following one or two years. Our approach resembles the local projection method, which has become popular in empirical macroeconomics (Jordà, 2005). It yields reduced-form estimates of how today's changes in the policy rate (the impulse) shapes income in the future (the response). We thus obtain the following model:

$$\frac{Y_{j;t+n} - \overline{Y}_{j;<t}}{\overline{D}_{j;<t}} = \sum_{k=1}^{K} \mathbb{1}[j \in k]\Big[\alpha^k + \beta^k(-\Delta i_t) + \delta^k Z_t\Big] + \varepsilon_{j,t} \tag{1}$$

where j, t and k denote the household, the year and the income group and $n = 1, 2$ indicates the time horizon. On the left-hand side, $\overline{Y}$ expresses the ex ante levels of the outcome and $\overline{D}$ the ex ante level of disposable income; both are averages taken over the three periods before period t to reduce the effect of transitory shocks on these baseline income levels. On the right-hand side, $\mathbb{1}[j \in k]$ indicates if household j belongs to income group k and Z denotes the vector of macro controls. We estimate the model using micro-data for the period 1987-2014 and report standard errors that are clustered at the level of households and municipality-years.[21]

The main coefficients of interest are β^k. Given that the change in the policy rate enters the model with a negative sign, these coefficients measure the effect of lowering the monetary policy rate by one percentage point, a *softening* of monetary policy, on the outcome for the

[21] Clustering at the level of households crucially corrects standard errors for auto-correlation in the error term (Bertrand et al., 2004). The monetary policy stance varies by local economic conditions and, moreover, the variation in the main explanatory variable is at the level of income groups and time, suggesting that we should cluster at the level of income group-municipality-years (Moulton, 1986, 1990; Abadie et al., 2017). We generally present standard errors with clustering at the level of municipality-years, which is more general; however, we also show how standard errors change under alternative clustering schemes in Section 5.3.

average household in income group k. We note that the model allows for heterogeneous effects of the macro environment on household outcomes (i.e. separate coefficients on Z by income group). In robustness tests, we also include time fixed effects that absorb the (average) effect of *any* aggregate shock.[22] The model includes no household-level controls as our goal is not to compare otherwise similar households at different income levels, but to measure how systematic differences across income groups creates differential exposure to monetary policy. In robustness tests, however, we include household fixed effects, which effectively adds a household-specific linear trend to the set of controls.

Disposable income is our main income variable and, given the scaling, estimating the model with this outcome expresses the percentage change in disposable income relative to the ex ante level. When we investigate the *channels* through which disposable income is affected by monetary policy, we also apply the model separately to each of the positive and negative income components: salary income, business income, stock market income, interest income, private pension income, net government transfers and interest expenses. In these regressions, we retain the scaling with *ex ante* disposable income to obtain an (approximate) decomposition of the total effect on disposable income through the different channels.

In addition to the effects on disposable income, monetary policy may also create important gains and losses through its effect on asset prices. While monetary policy may, in principle, affect the prices of all types of assets and liabilities, our analysis focuses on two major asset classes: stocks and housing assets.[23] We use a slightly modified framework to estimate the price effects of monetary policy on these assets. Letting P and Q denote prices and quantities respectively, we define the capital gain on a given asset over time horizon n as $P_{j,t+n}Q_{j,t-1} - P_{j,t-1}Q_{j,t-1}$ and use this as the outcome in the model (scaled by disposable income as usual). By holding quantities constant (i.e. fixing the portfolio), this concept of capital gains is unaffected by potentially endogenous portfolio adjustments, but may differ from actual capital gains in the presence of such adjustments.

For stocks, we observe the market value of each household's portfolio at the end of each year but have no information on the underlying securities. In practice, we therefore approximate capital gains and losses with the percentage change in the national stock market index multiplied

[22]This specification only identifies the *gradient* in the effects of monetary policy, i.e. the average effect for a given income group relative to the average effect for a reference group.

[23]Due to specific institutional arrangements, mortgage loans with a fixed rate are also an important source of capital gains and losses: since borrowers have an option to repay mortgage loans at the market price of the underlying bonds, the market value of existing loans vary inversely with the market interest rate. The strength of this mechanism, however, depends strongly on the maturity of the loan, which we do not observe in our data; hence, we are unable to estimate the capital gains and losses on mortgage loans with any precision.

by the ex ante value of the portfolio.[24] This approach yields capital gains estimates that are roughly correct in the aggregate (assuming that most Danish households invest in Danish stocks or foreign stocks with similar returns). The income gradient in the capital gains estimates thus captures systematic differences in portfolio sizes across income groups, but not systematic differences in portfolio risk (Calvet et al., 2007) and heterogeneous returns conditional on risk (Fagereng et al., 2018).

For housing assets, we know the location of each property and construct local price indices based on real estate transaction data. We compute the capital gain on each property as the ex ante market value of the property multiplied by the percentage change in the local housing price index (see details in the Online Appendix). The fact that we observe heterogeneous price developments across local areas is a major advantage compared to the analysis of stocks. The income gradient in the capital gains estimates captures both systematic differences in the value of housing assets across income groups as well as systematic cross-locality differences in the responsiveness of house prices to monetary policy.

Finally, we study the effects of monetary policy on wealth accumulation and consumption across the income distribution. Importantly, monetary policy may affect wealth and consumption through a range of channels. First, directly related to the analysis above, it creates gains and losses, which must pass through to either wealth or consumption due to the intertemporal budget constraint. These gains and losses are created through direct channels (e.g. household interest expenses go down when the policy rate is reduced because of pass-through to market interest rates) as well as more indirect channels (e.g. salary income goes up when the policy rate is reduced because of job creation and upward pressure on wage rates). Second, by changing market interest rates, monetary policy affects the trade-off between consumption and savings as captured by the intertemporal elasticity of substitution. Third, monetary policy may induce households to restructure their balance sheets with possible implications for wealth accumulation.

Concretely, we study the effect of monetary policy on wealth accumulation by using as an outcome the change in net wealth (scaled by disposable income). Compared to the analysis of capital gains and losses, we no longer hold quantities constant (do not fix the portfolio); we make no assumptions about stock price returns (we observe the market values of stock portfolios); and we include all observable balance sheet components in the net wealth measure (including

[24] Formally, the approximation is $(P_{j,t-1}Q_{j,t-1})(\Pi_{t+n} - \Pi_{t-1})/\Pi_{t-1}$ where Π is the national stock price index. It is easy to see that the approximation assumes that the price of the individual portfolio ($P_{j,t}$) exhibits the same growth rate as the price of the national portfolio (Π_t).

deposits and loans). We study the effect of monetary policy on consumption by using as an outcome the change in the number of new cars registered by the household (i.e. the change relative to the average taken over the three periods before period t).

5 Main results

5.1 Disposable income

Figure 4 shows the estimated effects on disposable income of a one percentage point reduction in the policy rate at different positions in the income distribution and at different time horizons. Generally, both the one-year effects (red line) and the two-year effects (blue line) are positive at all income levels. At the middle of the income distribution, the magnitude is around 1% after one year and 1.5% after two years. The income gradient in the estimated effects is strikingly positive and becomes more pronounced over time. The effect is virtually zero for the lowest incomes and as high as 5% for the top 1% after two years. This key result is robust to extending the set of macro controls and to different sets of fixed effects, as shown in Section 5.3.

Figure 5 investigates the channels underlying this striking income gradient by showing estimates for each component of disposable income.[25] In order to illustrate the income gradient as clearly as possible for all components, the panels have different scales on the y-axis, but we illustrate the results in an alternative way that facilitates quantitative comparisons below (in Figure 6).

As shown in Figure 5A, a decrease in policy rate increases disposable income by lowering interest payments, which is consistent with pass-through to market interest rates.[26] There is a pronounced income gradient in these gains as households with higher incomes tend to experience a larger drop in interest expenses when the policy rate is reduced. The income gradient partly reflects that the ratio of debt to disposable income increases up through the income distribution (see Table 1) so that households with higher income benefit more from a given decrease in market interest rates. However, differences in the debt-income ratios cannot fully account for the estimated income gradient suggesting that the pass-through from policy rates to market interest rates is higher at the top of the income distribution. Presumably, pass-through is muted at lower income levels due to a lower propensity to use mortgage products with variable interest rates and more frequent failures to take advantage of opportunities to refinance mortgage loans

[25] The analogous results over a one-year horizon are shown in Figure A1 in the Online Appendix.

[26] For instance, the ratio of debt to disposable income is roughly 200% at the median income level, suggesting a pass-through rate of around one half given the estimated effect of around -1%.

with a fixed rate (Andersen et al., 2015).[27]

As shown in Figure 5B, a decrease in policy rate reduces disposable income by lowering interest income, which is again consistent with pass-through to market interest rates.[28] Again, a clear income gradient emerges with households at higher income levels suffering more larger losses of interest income when the policy rate is reduced. Analogous to the case of interest expenses, the estimated income gradient partly owes itself to balance sheet differences: households at higher income levels tend to hold more interest bearing assets relative to their disposable income.

In sum, Figures 5A-5B are concerned with the *direct channels* of monetary policy, i.e. the effect on income components that are mechanically linked to interest rates: interest payments on loans and interest income from deposits and bonds. The remaining panels of Figure 5 are concerned with the *indirect channels* of monetary policy, i.e. the effect on income components such as salary income, business income and stock income.

As shown in Figure 5C, softer monetary policy tends to increase disposable income by raising salary income. The gain is largest for households at the 25th percentile of the income distribution, significantly smaller at the top and slightly negative at the bottom. The estimates may reflect quantity effects as well as price effects: salary income go up because workers are employed more hours or because the hourly wage rate goes up. In Figure A2 in the Online Appendix, we show that a similar hump-shaped relation between the effects of monetary policy and the income level emerges when we use weeks of employment as the outcome suggesting that quantity effects are at least partly driving the income gradient in salary income. This is consistent with the commonly held view that the gains created by monetary policy through the labor channel are concentrated among relatively low-income workers (Draghi, 2016). However, the results also highlight that the most disadvantaged groups who have very low employment rates through the business cycle do not appear to reap any gains through the labor channel.

As shown in the next panels, reducing the policy rate increases disposable income across all income groups by raising business income (Figure 5D) and stock market income (Figure 5E), but the effect is much stronger at the highest income levels. This pattern, at least partly, reflects that the propensity to be self-employed is increasing in income and that stock ownership is heavily concentrated at the very top of the income distribution (see Table 1). However, the strong non-

[27] One may ask to what extent lower market interest rates increase disposable income of the household sector as a whole or merely redistributes disposable income within the sector from lenders to borrowers. As shown in Table A1 in the Appendix, only a small fraction of outstanding mortgage bonds, the majority of total lending, are held by households with the majority being held by banks, insurance companies, pension funds and foreign investors.

[28] For instance, the ratio of deposits to disposable income is roughly 50% around the middle of the income distribution, suggesting a pass-through rate of around 80% given the estimated effect of around -0.4%.

linearity in the effects on business income also suggests that self-employed at different positions in the income distribution are differentially exposed to monetary policy shocks. Similarly, the steep gradient in the effect on stock market income may reflect that stockholders at different income levels systematically prefer stocks with different dividend policies and risk characteristics (Fagereng et al., 2018) and exhibit different propensities to realize latent capital gains over the business cycle (Hoopes et al., 2016).

As shown in Figure 5F, softer monetary policy tends to decrease net transfers from the government: by raising taxable income (e.g. salary income) and at the same time lowering deductible expenses (interest expenses), a lower policy rate indirectly reduces transfers from the government (such as unemployment benefits) and increases transfers to the government (such as tax payments). This mechanism is particularly strong in two parts of the income distribution: around the 25th percentile where most of the employment gains occur and within the top income group where the overall effect on non-salary income and interest expenses is largest. The results highlight that taxes and transfers moderate the effect of monetary policy on disposable income, notably at the top of the income distribution. In economies with less fiscal redistribution than in Denmark, we should therefore expect an even steeper income gradient in the effects of monetary policy.

Finally, as shown in Figure 5G, softer monetary policy tends to lower payouts from private pension plans. This is consistent with employees postponing retirement in response to improved labor market opportunities and retires suspending annual pay-outs in response to income gains from other sources. The effect is uniformly negative at all income levels except at the very top of the income distribution where it is marginally positive (but statistically insignificant).

Figure 6 visualizes the same results in a way that facilitates a quantitative comparison of the various channels: at different positions of the income distribution, we show the overall effect of a one percentage point decrease in the policy rate on disposable income (horizontal lines) as well as the contribution to this effect from each of the components (colored bars).[29]

In the lower end of the income distribution (25th percentile) and around the middle (50th percentile), lower policy rates primarily expand disposable income by raising salary income (green bars) and, to a lesser extent, by lowering interest expenses (red bars). These positive effects are muted by lower net government transfers (orange bars) and, less importantly, by a reduction in interest income (light brown) and private pensions (dark brown). At the upper end of the income distribution (75th percentile), the picture is much the same except that interest

[29]The contributions by individual components approximately sum to the total as illustrated in Figure A3 in the Online Appendix.

expenses are a more important channel than salary income and that net government transfers do not mitigate these positive effects. Finally, at the very top of the income distribution (top 1%), the decrease in interest expenses is the most important channel and far larger than the analogous decrease in interest income (light-brown bars). The increase in salary income is dwarfed by the relatively large increases in business income (blue bars) and stock market income (black bars).

In sum, while softer monetary policy tends to increase disposable income more at higher income levels, as shown in Figure 4, this is the product of many channels working in different directions. A lower policy rate produces gains in the form of higher business incomes, higher stock market incomes and lower interest expenses, which are concentrated at the highest incomes, whereas gains in the form of higher salaries are larger near the bottom. Losses in the form of lower interest income are borne primarily by households at the top whereas losses in the form of lower government transfers and higher taxes are largest at the lower end of the distribution and at the very top.

5.2 Asset values

Figure 7 shows the estimated effect of a lower policy rate on asset values at different positions in the income distribution and at different time horizons. The estimates capture the "price effect" of monetary policy, the effect on asset values working through changes in house prices and stock prices holding *ex ante* portfolios constant, but not the effect working through changes in the portfolios. The estimated effects are positive at all income levels: softer monetary policy boosts asset prices and, thus, drives up asset values. There is a clear income gradient in the effects, which becomes more pronounced over time: a one percentage point decrease in the policy rate increases asset values by around 20% of disposable income at the bottom of the income distribution and by around 75% of disposable income at the top over the two-year horizon. While the gradient is qualitatively similar to the one we found for disposable income (Figure 4), the effects are generally larger by more than an order of magnitude. Measured relative to total asset values (see Table 1), the estimated effects range from around 6% at the bottom to around 8% at the top.

Figure 8 investigates the channels underlying these estimates by showing the contribution from housing assets and stocks to the overall two-year effect.[30] As shown in Figure 8A, softer monetary policy increases the value of housing assets at all income levels and the magnitude of

[30]The analogous contributions to the one-year effect are shown in Figure A4 in the Online Appendix. The contributions by individual components approximately sum to the total as illustrated in Figure A5 in the Online Appendix.

the effect is monotonically increasing in income: the estimated gain is around 20% of disposable income at the bottom of the income distribution and by around 50% of disposable income at the top. The positive income gradient is largely explained by *ex ante* differences in the ratio of housing assets to disposable income (see Table 1). Combining the estimates in the figure with statistics on the value of housing assets suggests that a one percentage point reduction in the policy rate increases the value of housing assets by 6-9%, slightly more at the top than at the bottom.[31] Thus, the results suggest that the gradient in the estimated effects is mainly due to high-income households owning more real estate (relative to their disposable income) and to a lesser extent due to a differential sensitivity of real estate prices to interest rates.[32]

As shown in Figure 8B, softer monetary policy generally increases the value of household portfolios of stocks, but the gains are highly concentrated at the top of the income distribution: the estimated gain is around 15% of disposable income in the top income group and entirely negligible below the median income level. The striking income gradient in the estimates largely reflects the concentration of stock ownership in the highest income groups (see Table 1). Combining the estimates with statistics on the value of stock portfolios suggests that a one percentage point reduction in the policy rate increases the value of stocks by around 6% for the top income group.[33]

In sum, the results suggest that a softer monetary policy creates gains in the form of higher asset values that are many times larger than the gains in the form of higher disposable income. Moreover, both housing and stock portfolios contribute importantly to the heterogeneous effects on asset values. The gains created through increases in housing prices contribute most to the overall gains, but the gains created through increases in stock prices are most unequally distributed and thus contribute most to the income gradient.

5.3 Robustness

We first test whether our main finding, a significant income gradient in the gains from a lower policy rate (higher disposable income and asset values), is robust to including more controls. Our baseline model controls for *ex ante* GDP growth and inflation in Germany/Euro area (interacted

[31]These estimates are very close to estimates of the effect of monetary policy rates on house prices of around 8-9% often cited in the literature (e.g. Taylor, 2007).

[32]As shown in Figure A6 in the Online Appendix, using the raw changes in the appraisal values of property rather than the imputed changes in market values gives similar results, although with a considerably steeper slope at the very top of the income distribution. These estimates capture both changes in the appraisal values as well as sales and purchases of property.

[33]This is close to the widely cited estimate of the effect of monetary policy rates on stock prices of 6.8% (Rigobon and Sack, 2004).

with income group indicators). We now sequentially add time fixed effects (not interacted with income), more macro controls (interacted with income) and household fixed effects. Figure A7 in the Online Appendix illustrates the two-year effects on disposable income (Panel A) and asset values (Panel B) estimated in the different models.

We start by introducing time fixed effects that absorb unobserved factors affecting all income groups in the same way (black line). This comes at the cost that only the income gradient in the estimates is identified. We choose the median income group (p45-p50) to be the omitted category and thus measure the effects of monetary policy in other income groups relative to the effects in that income group. The results suggest that lowering the policy rate by one percentage point raises disposable income by almost 4 percentage points more in the top income group than at the median and by almost 2 percentage points less in the bottom group than at the median. Similarly, the asset price effects (measured as a fraction of disposable income) are almost 50 percentage points larger in the top income group and almost 10 percentage points smaller in the bottom group than at the median.

Next, we sequentially add more parametric controls to assess whether the income gradient emerging in the baseline model may be driven by omitted variables. Specifically, we add: (i) *ex ante* forecasts of GDP growth and inflation in the Euro area (red line) to account for the fact that expected business cycle developments affect monetary policy choices and may potentially correlate with the income gradient in economic outcomes in Denmark; (ii) *ex ante* GDP growth and inflation in Denmark (green line) to account for the possibility that the residual part of monetary policy in the Euro area correlates with the Danish business cycle; (iii) *ex post* exports from Denmark (light brown line) to control for the effect of monetary policy decisions in Frankfurt through changes in demand for Danish products; (iv) policy rate changes in period $t+1$ (blue line) to account for any serial correlation in the monetary policy interventions. All the new controls are interacted with a full vector of income group indicators.

The results are qualitatively unchanged but adding more controls attenuates the income gradient modestly. For instance, in a saturated model that controls for *ex ante* GDP growth, inflation and macro forecasts in the Euro area, *ex ante* GDP growth and inflation in Denmark and *ex post* exports from Denmark, decreasing the policy rate by one percentage point crates a differential gain of disposable income for the top income group of around 3 percentage points (compared to 4 percentage points in the more parsimonious baseline model) and a differential increase in asset values of around 35% of disposable income (compared to just below 50% in the baseline model).

Further, we re-estimate the baseline model with an alternative estimation procedure, an alternative measurement of the policy rate and an alternative sample period. Specifically, (i) we estimate the Euro area monetary policy shock in a "stage zero" and use it to instrument for the change in the Danish policy rate (red line); (ii) measure the policy rate with the shadow rate that accounts for the zero lower bound (green line); and (iii) add observations for the period 2015-2017 in the asset value model (light brown line), which is possible because we impute the outcome from ex ante asset values observed on the tax return (available until 2014) and subsequent changes in real estate and stock prices (available until 2017). For each of these variations of the baseline model, Figure A8 in the Online Appendix illustrates the two-year effects on disposable income (Panel A) and asset values (Panel B). The results are qualitatively robust to all of these perturbations and quantitatively very similar to the baseline model. First, adding an extra stage to the estimation procedure makes the gradient somewhat steeper for disposable income and somewhat flatter for asset values. Second, using the shadow rate makes virtually no difference for any of the estimates. Third, extending the sample period makes the gradient slightly steeper for asset values.

Moreover, we augment the baseline model with household fixed effects. Given that our outcomes are *changes* in income or asset values, household fixed effects effectively add a household-specific linear trend to the set of controls. As shown in Figure A9 in the Online Appendix, the results are qualitatively robust to this demanding extension of the model. The gradient in the income gains created by a decrease in the policy rate is substantially steeper, with estimated gains to the top 1% exceeding gains to the median income group by more than 5 percentage points (Panel A). By contrast, the gradient in the gains of asset values is moderately flatter than in the baseline model (Panel B).

Finally, we probe the sensitivity of the standard errors to assumptions about the correlation structure in the error term. Specifically, Figure A10 in the Online Appendix shows the two-year effects from the baseline model for disposable income (Panel A) and asset values (Panel B) with four different confidence intervals based on clustering at the level of: (i) households; (ii) households and municipality-years; (iii) households and income-municipality-years; (iv) households and income-municipality. While clustering at the level of households corrects standard errors for auto-correlation in the error term (Bertrand et al., 2004), we add a second dimension of clustering to reflect that the monetary policy stance varies by local economic conditions and that the variation in the main explanatory variable is at the level of income groups and time (Moulton, 1986, 1990; Abadie et al., 2017). Clustering at the level of households alone produces tiny standard errors. Adding a second dimension of clustering generally widens the confidence intervals considerably; however, the income gradient continues to be statistically significant in all cases.

6 Further results

6.1 Leverage

To investigate the role of household leverage in monetary policy transmission, we re-estimate the baseline model while allowing the effect of the policy rate to vary with household leverage. While the baseline model includes interactions between the policy rate variable and indicators of *ex ante* income, we now interact each of these terms with indicators of *ex ante* leverage.[34] Specifically, we define leverage as the ratio of debt to gross income and consider four groups defined with reference to the sample distribution of this ratio: households with no debt, low debt ($<$ 20th percentile), medium debt (20th - 80th percentile) and high debt ($>$ 80th percentile).

We start by considering how leverage mediates the effect of monetary policy on interest expenses. Specifically, Figure 9A shows the estimated gain in the form of lower interest expenses associated with a decrease in the policy rate at different positions in the income distribution and for each leverage group separately. The results highlight that the magnitude of the direct effect of monetary policy is tightly linked to the size of the debt. Comparing within income groups, the gain is increasing monotonically in leverage. Comparing within leverage groups, the gain is roughly the same size across households with different income. The main exception is that gains are larger for the top income group than for households with comparable leverage in other income groups, which may reflect differential pass-through of policy rates, unrecorded debt or differential new borrowing.[35]

[34] The augmented model also includes a full set of three-way interactions between macro controls, income group indicators and leverage indicators.

[35] Pass-through may be stronger for high-income households to the extent that they more frequently have mortgage loans with a variable rate or have a higher propensity to refinance mortgage loans with a fixed rate. Unrecorded debt is a potential issue because tax returns, our main data source, only include information on debt from domestic financial institutions (including domestic branches / subsidiaries of foreign banks), as discussed in Section 3. Note that interest expenses do not suffer from the same measurement problem as households must self-report interest payments to foreign banks on the tax return (and have an incentive to do so in order to obtain a tax deduction). Finally, it may be the case that households in the top income group have higher average than the other groups within each leverage group and that leverage itself responds more strongly to the interest rate in the top income group (e.g. paying back more debt in periods with falling interest rates). Relatedly, the gains estimated for households with no debt may reflect interest payments on unrecorded debt or the extensive margin of borrowing: some households with no *ex ante* leverage take on debt and thus start incurring interest expenses when the policy rate is raised (for instance, because they buy a house in response to falling house prices induced by the tighter monetary policy).

As shown in Figure 9B, the striking monotonicity in leverage remains when we consider the effect of softer monetary policy on overall disposable income: at each position in the income distribution, the increase in disposable income following a decrease in the policy rate is larger for households with more leverage. Only in the top income group do households with no debt gain appear to gain more from a softer monetary policy than households with moderate leverage implying that they have large gains through indirect channels. As before, comparing households with roughly the same leverage, the increase in disposable income is roughly similar across income levels suggesting that differences in leverage account for a significant part of the income gradient in the effect of monetary policy on disposable income. The only exception to this pattern is the top-1% where gains are considerably larger than elsewhere in the income distribution at all levels of leverage suggesting larger gains through indirect channels.

Next, we consider how leverage mediates the effect of monetary policy on asset values. Figure 10 shows the "price effect" on the value of housing assets (Panel A) and stock portfolios (Panel B) for households with different income and leverage. The patterns for the two asset classes are strikingly different. On the one hand, leverage explains most of the income gradient in the effect on housing assets: when comparing households with the same leverage, the gain is similar across income levels. Presumably, this reflects that most real estate is partly financed with debt; hence, highly leveraged households tend to have more housing assets and therefore benefit more from increases in housing prices. This mechanism applies to a lesser extent to the top income group where owning significant housing assets without debt is more prevalent. On the other hand, leverage explains almost none of the income gradient in the effect on stock values: when comparing households with the same leverage, the income gradient remains highly pronounced. Moreover, when comparing households in the same income group, the effect tends to be stronger for households with less debt. These patterns reflect that leverage and stock holdings tend to be negatively correlated.[36]

6.2 Wealth and consumption

We now turn to the effect of monetary policy on wealth accumulation and consumption. This relates directly to our main analysis because the gains created by a softening of monetary policy, whether in the form of disposable income or increased asset values, are necessarily either consumed or added to the wealth stock due to the intertemporal budget constraint.

[36]Whether we compare households in the full sample or within income groups, households with more leverage have smaller stock portfolios. This negative correlation is, for instance, consistent with a simple behavioral rule model where households first use savings to pay off mortgage debt and then use them to invest in financial assets.

However, monetary policy also affects wealth accumulation and consumption through other channels. Most importantly, by changing market interest rates, it affects the overall fraction of income saved for future consumption as captured by the intertemporal elasticity of substitution. Moreover, it may induce households to restructure their balance sheets with possible implications for wealth accumulation, for instance by increasing leverage or changing the share of risky assets.

Figure 11A shows the estimated effect on net wealth at different positions in the income distribution over a two-year horizon. The results are suggestive that monetary policy has a strong effect on net wealth across the entire income distribution, but more so at higher income levels: the estimate is in the range of 20-30% of disposable income below the median income level and then rises monotonically to almost 80% of disposable income in the top income group. Measured relative to net wealth (see Table 1), the estimated effects are generally in excess of 10%.[37]

Strikingly, the estimated effects of monetary policy on net wealth are very similar to the estimated "price effects" on asset values (Figure 7). This is consistent with existing evidence that only a small fraction of the gains and losses created by asset price changes are channelled into consumption in the short term (Aladangady, 2017; Di Maggio et al., 2020). The results are also consistent with an important role for "saving by holding" (Fagereng et al., 2019) whereby capital gains on, for instance, homes are only to a limited extent transformed into consumption through a reduction in liquid assets or new mortgage loans (Andersen and Leth-Petersen, 2019).

Figure 11B shows the estimated two-year effect on the propensity to purchase a new car. Cars are arguably the most important durable consumption good and many empirical papers use changes in car consumption to approximate changes in total durable consumption (e.g. Di Maggio et al., 2017). The results indicate that the effects of softening monetary policy on car consumption are highly heterogeneous across income groups: a one percentage point reduction in the policy rate increases the annual purchases of new cars by around 0.001 cars at the median income level; four times more at the top of the income distribution and virtually not at all at the bottom.[38] This suggests that the differential income gains and capital gains created by a softening of monetary policy are also associated with differential consumptions gains for high-income households.

[37]The implied percentage return on net wealth is somewhat higher than implied percentage return on the underlying assets reflecting that households are levered.

[38]As we do not observe car values, we are not able to estimate the effect of monetary policy on car consumption in fractions of disposable income. It is not obvious how the gradient would change as both disposable income and the average value of newly purchased cars would increase though the income distribution.

6.3 Age

While our analysis until now has focused on the heterogeneous effects of monetary policy across income groups, this section investigates the heterogeneity in an entirely different dimension: age. Exposure to the various channels of monetary policy varies across age groups due to life cycle patterns in labor market participation, borrowing and wealth accumulation, as summarized in Table A2 in the Online Appendix. Our model remains the same as the baseline before except that the change in the policy rate is now interacted with indicators of age rather than with indicators of income.

Figure 12 illustrates the estimated effects on disposable income (Panel A) and asset values (Panel B) for different age groups over a two-year time horizon. There is a hump-shaped relation between the effects on disposable income and age: the effects are close to zero for the young (below age 35) and the old (above age 75) and roughly at the same positive level for the age groups in between. By contrast, the effect on asset values is almost monotonically increasing in age. These relationships reflect that exposure to the various direct and indirect channels of monetary policy change markedly over the life cycle (Table A2). Importantly, the middle-aged (age 35-65) have most debt relative to disposable income and therefore benefit most from lower interest expenses when the policy rate is lowered and the elderly (above age 65) have most assets and therefore benefit most from higher prices on stocks and houses.

In sum, the results suggest that the income channel of softer monetary policy is stronger for middle-aged households whereas the asset price channel is stronger for old households. Both channels create the smallest gains for younger households.

7 Income inequality

The strong income gradient in the effects of monetary policy suggests that there may be important implications for inequality. In this section, we use our estimates of the heterogeneous effects of monetary policy to conduct a simple simulation exercise that quantifies the effect of a one percentage point decrease in the policy rate on one of the most commonly used distributional measures: income shares (e.g. Piketty, 2014).

We first determine the *actual* shares of aggregate disposable income for each of the 21 income groups. We then compute the gain in disposable income for each household over a two-year horizon in a *counterfactual* scenario where the policy rate is lowered by one percentage point. To establish the counterfactual, we assume that the effects of a decrease in the policy rate vary across income groups in the way we estimated in our baseline model (Figure 4) and hold everything else constant. We finally plot the percentage difference between the counterfactual shares and the actual shares of aggregate disposable income in Figure 13.

The results show that the effect of monetary policy on income shares is strongly monotonic: a lower policy rate increases the income share for high-income households and decreases it for low-income households. Specifically, lowering the policy rate by one percentage point increases the share of aggregate disposable income by around 3% for the top-1% and decreases it by around 1.5% for the bottom income group. Hence, our results suggest that monetary policy, through a range of direct and indirect channels, has an economically significant impact on the distribution of disposable income. While disposable income increases for *all* income groups when monetary policy is softened, the gains are larger for high-income than for low-income households so the distribution of disposable income becomes more unequal.

To put these estimates in perspective, we note that the income share of the top-1% has increased by around 50% over our sample period, from around 7.5% in 1990 to around 11% in 2013 (World Inequality Database, 2020). Importantly, however, the income concepts are different: our estimates concern the distribution of disposable income whereas most of the literature, including the one cited here, concerns the distribution of market income before government transfers and taxes. As government transfers and taxes generally mute the income gradient in the effects of monetary policy on income, as shown in Figure 5, our simulation results most likely understates the effect of monetary policy on inequality in market income. Finally, the simulation does not account for the distribution of the gains created by the wealth channel, as shown in Figure 7.

8 Conclusion

In this paper, we have studied the *distributional effects* of monetary policy across income groups. Our results document a strong income gradient in the gains from expansionary monetary policy: while households at all income levels benefit from a lower policy rate in terms of disposable income, asset values, net wealth and durable consumption, households at higher income levels generally benefit more. The distributional effects reflect systematic differences across income groups in the exposure to the direct and indirect channels of monetary policy. The results suggest that monetary policy has a sizeable effect on inequality: lowering the policy rate by one percentage point increases the share of aggregate disposable income by around 3% for the top-1% and decreases it by around 1.5% at the bottom of the income distribution.

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Table 1: Descriptive statistics The table shows describes the composition of disposable income (Panel A) and net wealth (Panel B) and describes some important behavioral margins (Panel C) by income groups. To define the income groups, we rank households within each age-cohort. The seven income groups are: individuals up to the 20th percentile (*p0-20*); between the 20th and 40th percentile (*p20-40*); between the 40th and 60th percentile (*p40-60*); between the 60th and 80th percentile (*p60-80*); between the 80th and 90th percentile (*p80-90*); between the 90th and 99th percentile (*p90-p99*) and above the 99th percentile (*p99-100*). All income and wealth elements are expressed as a fraction of disposable income.

	p0-p20	p20-p40	p40-p60	p60-p80	p80-p90	p90-p99	p99-p100
Panel A: income components (% of disposable income)							
salary income	40%	96%	118%	128%	135%	130%	73%
business income	4%	5%	6%	8%	12%	27%	62%
stock market income	0%	0%	1%	1%	2%	6%	41%
interest income	1%	2%	2%	2%	3%	5%	10%
net government transfers	58%	5%	-18%	-35%	-51%	-67%	-80%
interest expenses	8%	13%	15%	16%	18%	21%	23%
private pension	4%	5%	6%	10%	15%	17%	11%
other income	1%	1%	1%	1%	2%	3%	7%
Panel B: net wealth components (% of disposable income)							
deposits	64%	67%	66%	82%	96%	129%	234%
stocks	8%	10%	11%	16%	23%	42%	180%
housing	283%	348%	366%	435%	506%	604%	578%
debt	145%	210%	235%	263%	294%	337%	321%
net wealth	210%	214%	208%	270%	331%	438%	671%
Panel C: descriptive indicators							
is net creditor	64%	71%	74%	77%	81%	84%	87%
has no debt	30%	25%	23%	20%	18%	16%	15%
holds stocks	19%	27%	31%	40%	48%	58%	70%
owns real estate	37%	54%	59%	68%	74%	82%	90%
is self-employed	8%	9%	10%	12%	16%	26%	49%
buys new car	1%	3%	3%	4%	5%	6%	7%

Figure 2 – Exchange rates. Notes: The figure shows the following daily exchange rates: Danish Kroner per German Mark (1/1-1960-31/12-1998); Danish Kroner per Euro (1/1-1999-31/12-2018) and Danish Kroner per US Dollar (1/1-1960-31/12-2018). The dashed lines indicate the following events: *UK and DK devaluate* indicates 20 November 1967 where the UK devalued the Pound and Denmark swiftly followed; *End of Bretton Woods* indicates 19 March 1973 where the European currencies started to float against the dollar effectively ending the pegged exchange rate regime known as Bretton Woods; *Fixed rate against the ECU* indicates 10 September 1982 where a new Danish center-right government took office and announced its commitment to a fixed exchange rate; *Fixed rate against Mark* indicates 12 January 1987 where ERM governments decided a major realignment of the ERM exchange rates, the last one where Kroner was devalued against Mark; *ERM crisis* indicates 2 August 1993 where the ERM governments decided to broaden the fluctuation bands to 15% in response to almost 12 months of speculative attacks; *Germany adopts Euro* indicates 1 January 1999 where Germany adoted the Euro; *Lehman collapse* indicates 15 September 2008 where Lehmann Brothers Filed for bankruptcy.

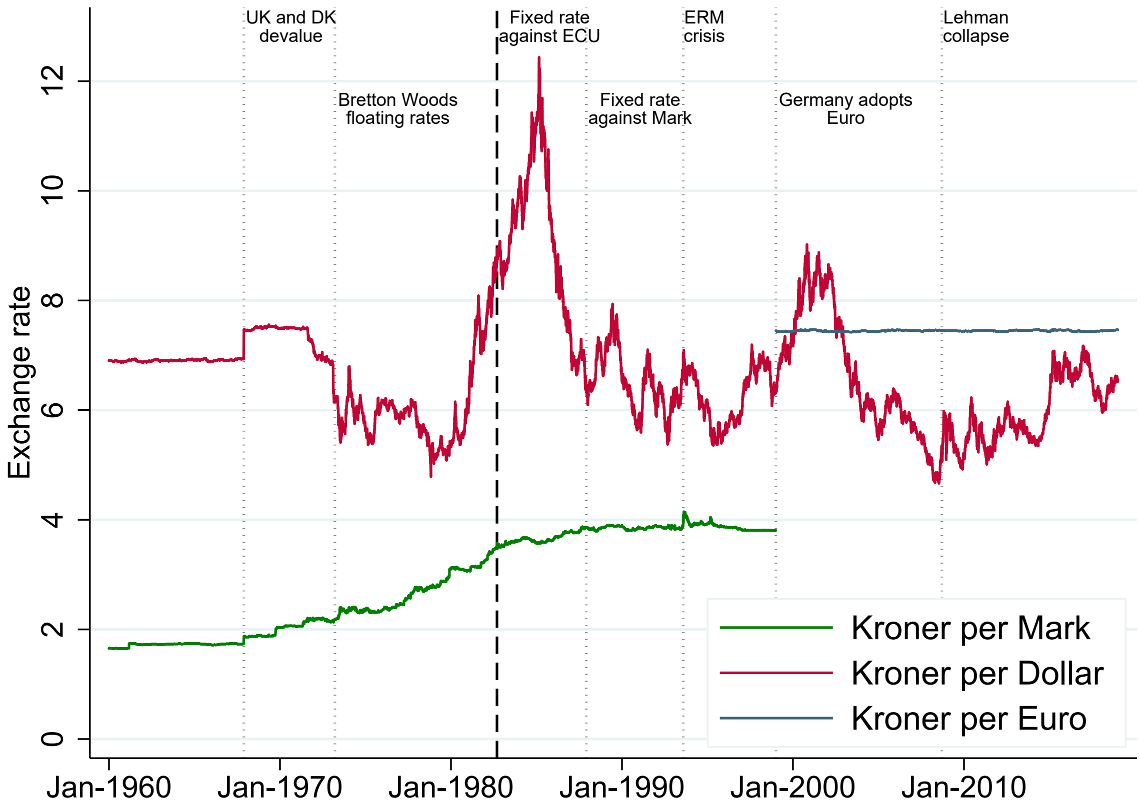

Figure 3 – Policy interest rates. Notes: The figure shows leading policy interest rates for Denmark, Germany (January 1960 - December 1998) and the Euro Area (January 1999 - December 2018). The leading policy rate is: the lending rate until November 2013 and then the deposit certificate rate (Denmark); the Lombard rate until 1987 and then the repo rate (Germany); the major refinancing operations rate until November 2013 and then the deposit rate (Eurozone). The dashed lines indicate the following events: *UK and DK devaluate* indicates 20 November 1967 where the UK devalues and Denmark swiftly follows; *End of Bretton Woods* indicates 19 March 1973 where the European currencies start to float against the dollar effectively ending the pegged exchange rate regime known as Bretton Woods; *Fixed rate against the ECU* indicates 10 September 1982 where a new center-right government takes office and announces its commitment to a fixed exchange rate; *Fixed rate against Mark* indicates 12 January 1987 where ERM governments decide a major realignment of the ERM exchange rates, the last one where Kroner is devalued against Mark; *Norway drops peg* indicates 10 December 1992 where Norway abandons the to the ECU; *ERM crisis* indicates 2 August 1993 where the ERM governments decide to broaden the fluctuation bands to 15% in response to almost 12 months of speculative attacks; *Mexico drops* peg indicates 20 December 1994 where Mexico abandons the peg to the US dollar; *Russia drops peg* indicates 17 August 1998 where Russia abandons the peg to the dollar; *Danish EURO vote* indicates 28 September 2000 where a Danish referendum rejects that Denmark join the Euro; *Lehman collapse* indicates 15 September 2008 where Lehmann Brothers files for bankruptcy; *Debt crisis in Eurozone* indicates 26 October 2011 where 50% of the Greek sovereign debt held by banks is written off; *Switerland drops peg* indicates 15 January 2015 where Switzerland abandons the peg to the Euro.

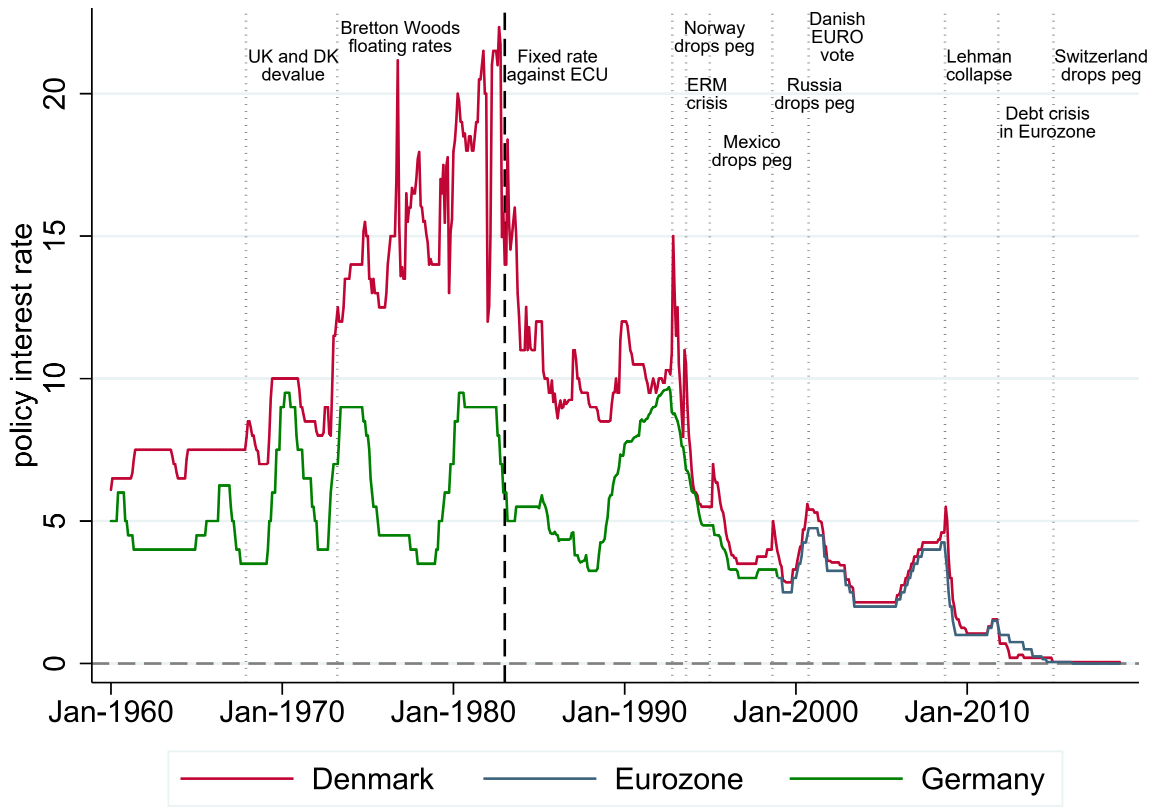

Figure 3: Monetary policy rates The figure shows the annual change in the policy rate in Germany / Euro area (red line); the annual change in the policy rate in Denmark (green line); and the residual variation in the policy rate in Germany / EuroaArea after regressing on current and lagged values of GDP growth and inflation (blue bars).

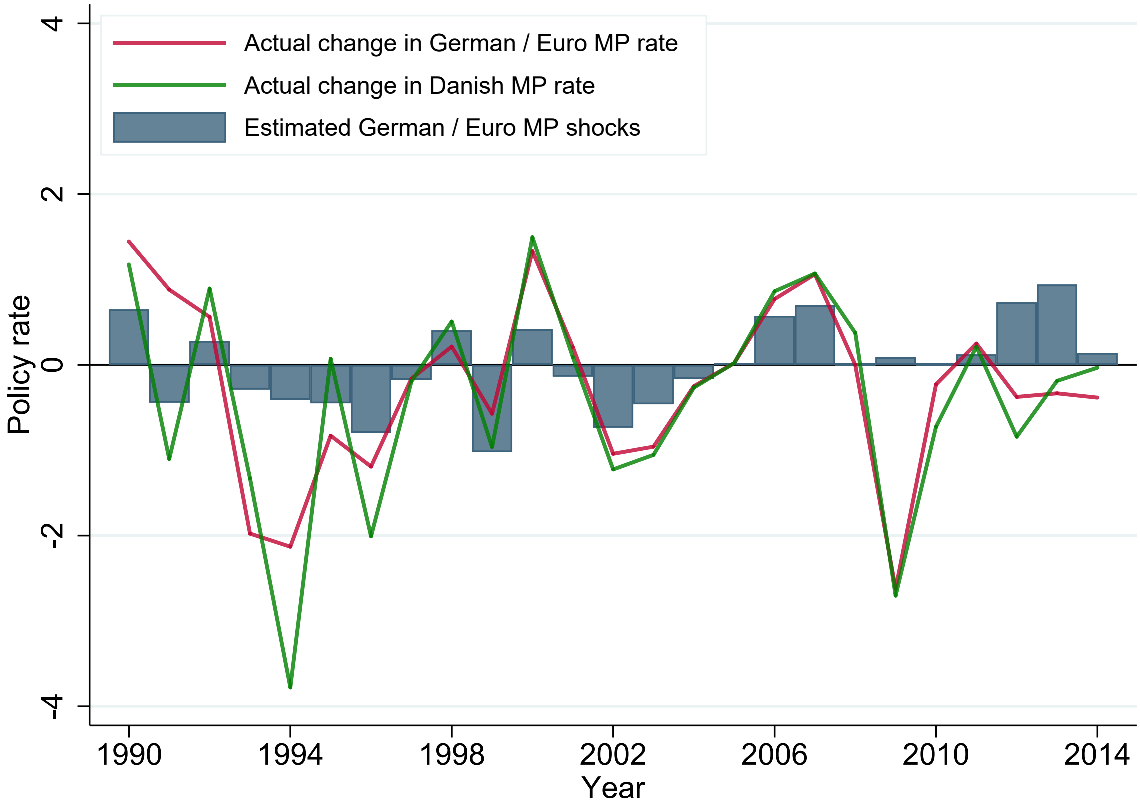

Figure 4: Heterogeneous effects of monetary policy on disposable income. The figure shows the estimated effects of a one percentage point decrease in the monetary policy rate on disposable income at different positions in the income distribution and over different time horizons: one year after the policy rate is changed (red squares) and two years after the policy rate is changed (blue squares).

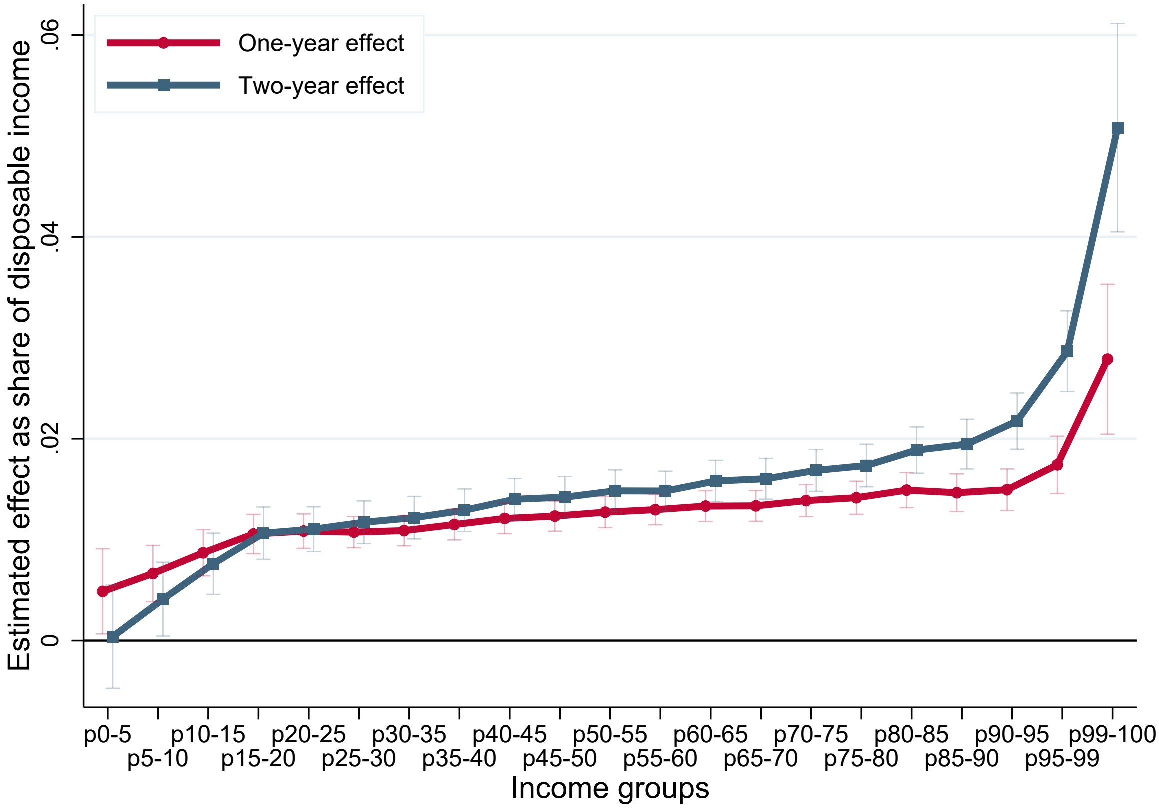

Figure 5: Heterogeneous effects of monetary policy on income by component. The figure shows the estimated two-year effect of a one percentage point decrease in the monetary policy rate on the components of disposable income at different positions in the income distribution. The estimates for shorter time horizons are reported in Figure A1 in the Online Appendix.

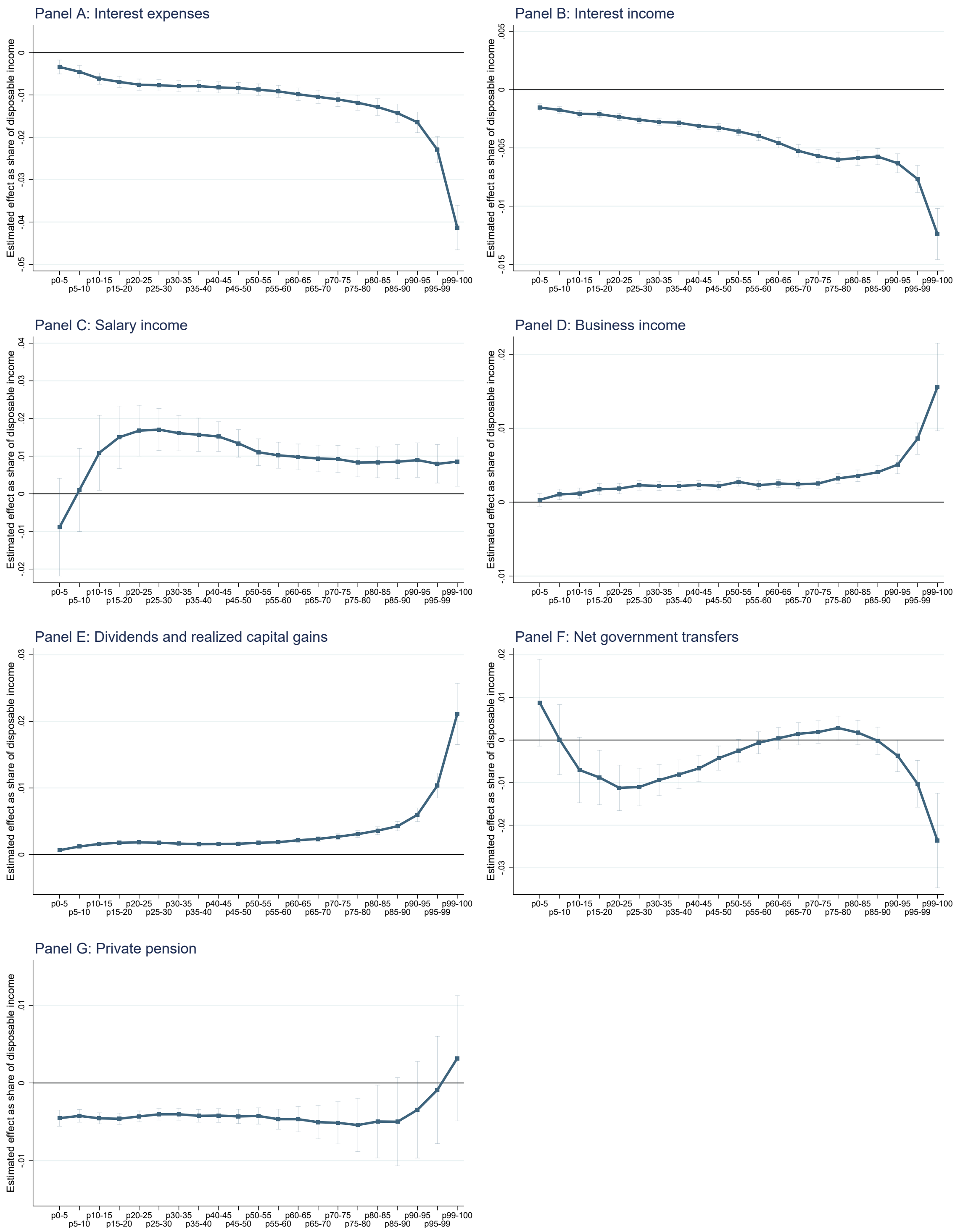

Figure 6: Heterogeneous transmission of monetary policy to disposable income. The figure shows the estimated two-year effect of a one percentage point decrease in the monetary policy rate on disposable income at four distinct positions in the income distribution (black horizontal lines) as well as the contributions to this overall effect from each of the components of disposable income (colored bars).

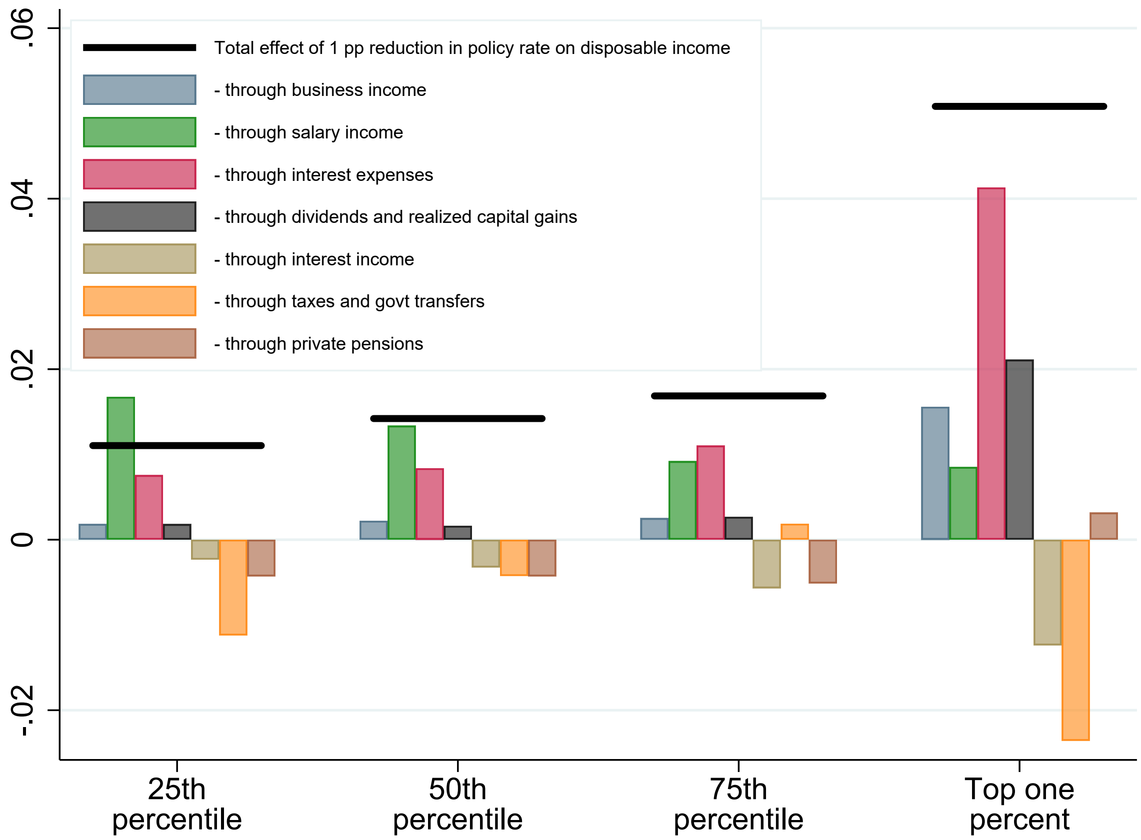

Figure 7: Heterogeneous effects of monetary policy on asset values. The figure shows the estimated ”price effect” of a one percentage point decrease in the policy rate on the combined value of housing and stocks at different positions in the income distribution and over different time horizons: one year after the policy rate is changed (red squares) and two years after the policy rate is changed (blue squares).

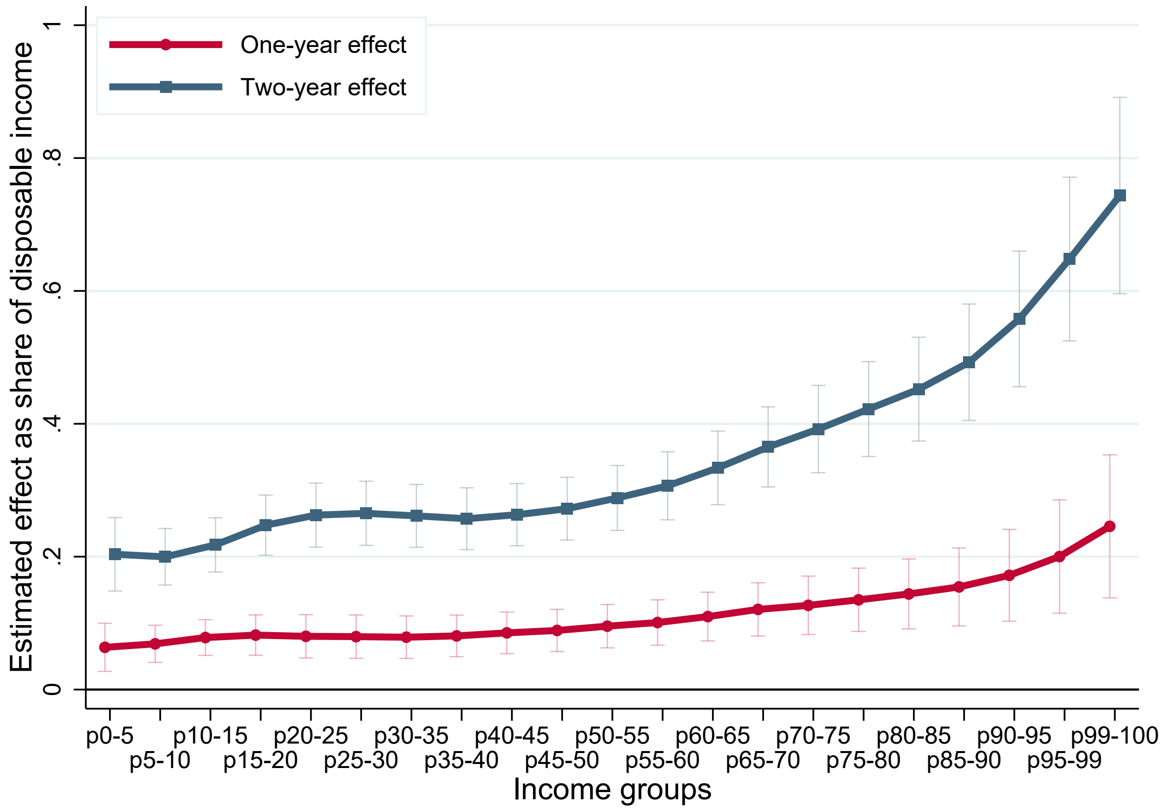

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Figure 8: Heterogeneous effects of monetary policy on asset values by type. The figure shows the estimated two-year "price effect" of a one percentage point decrease in the monetary policy rate on the value of housing assets and stock portfolios at different positions in the income distribution. The estimates for shorter time horizons are reported in Figure A4 in the Online Appendix.

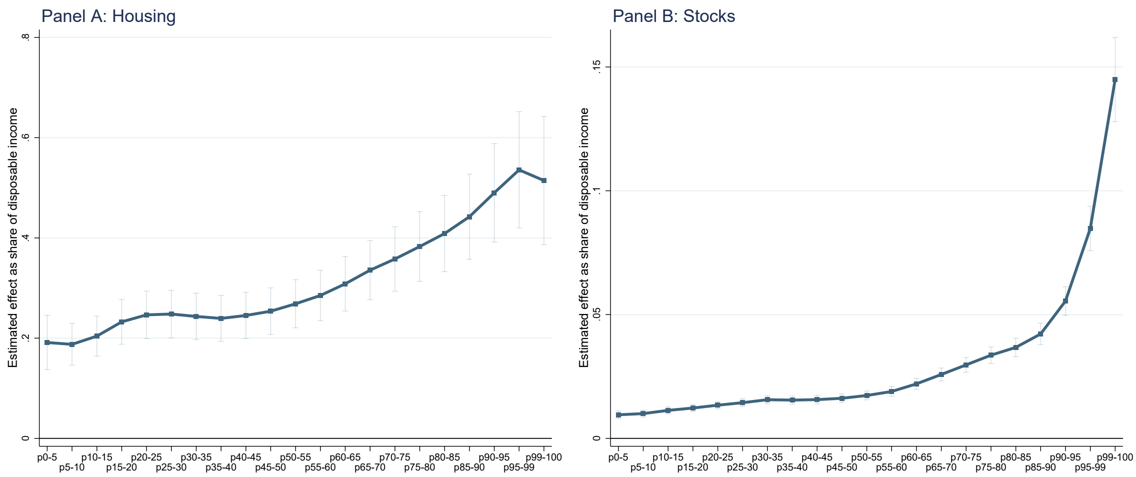


Figure 9: Household leverage and the effect of monetary policy on income. The figure shows the estimated two-year gain created by a one percentage point decrease in the monetary policy in the form of interest expenses (Panel A) and higher disposable income (Panel B) at different positions of the income distribution and for households with different ratios of debt to income. To derive the results, we split the sample into four groups based on their ratio of debt to income (DTI) and interact the explanatory variables in the baseline model with indicators of belonging to the four groups in the *ex ante* period.

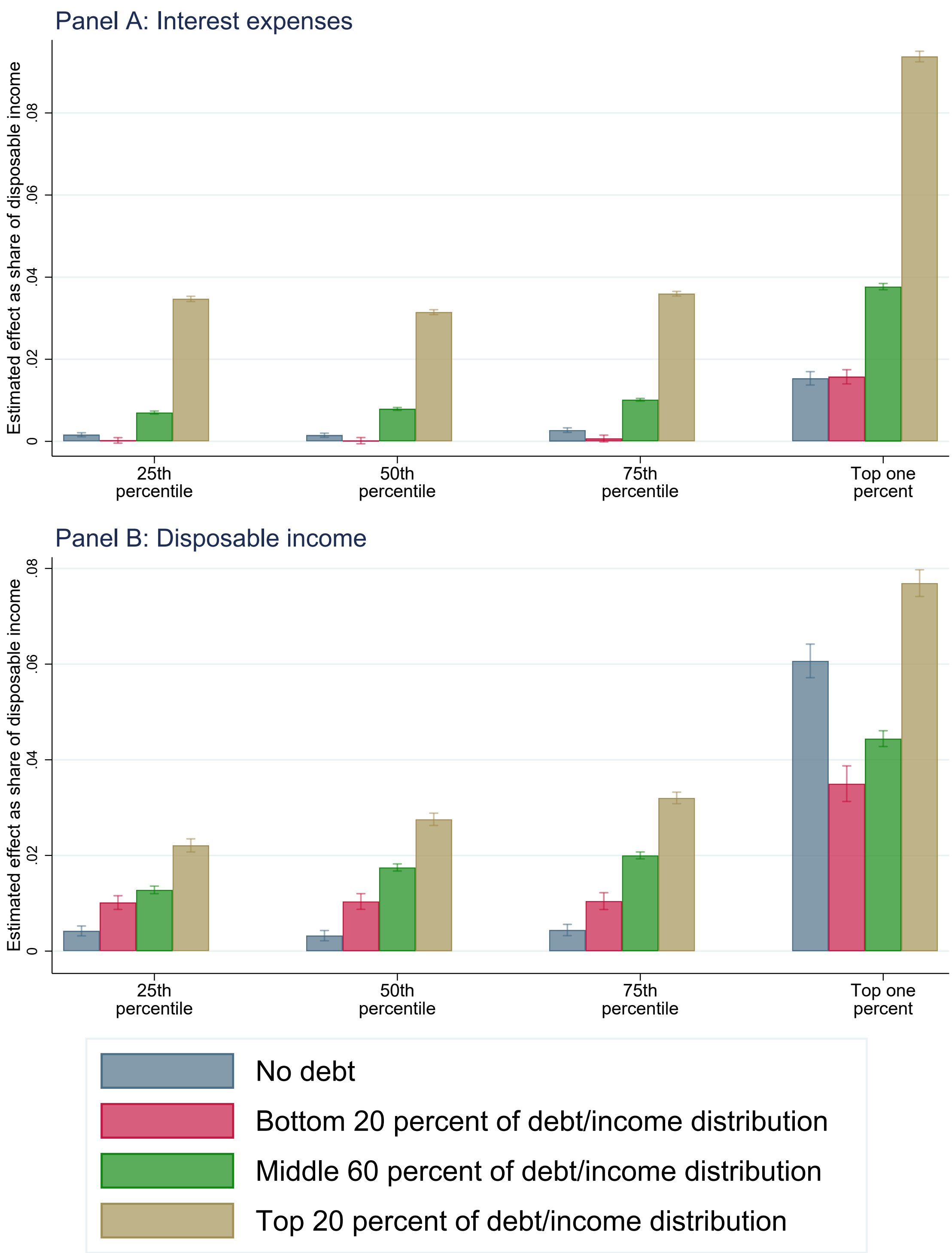


Figure 10: Household leverage and the effect of monetary policy on asset values. The figure shows the estimated two-year price-effect of a one percentage point decrease in the policy rate on the value of housing assets (Panel A) and stock portfolios (Panel B) at different positions of the income distribution and for households with different ratios of debt to income. To derive the results, we split the sample into four groups based on their ratio of debt to income (DTI) and interact the explanatory variables in the baseline model with indicators of belonging to the four groups in the *ex ante* period.

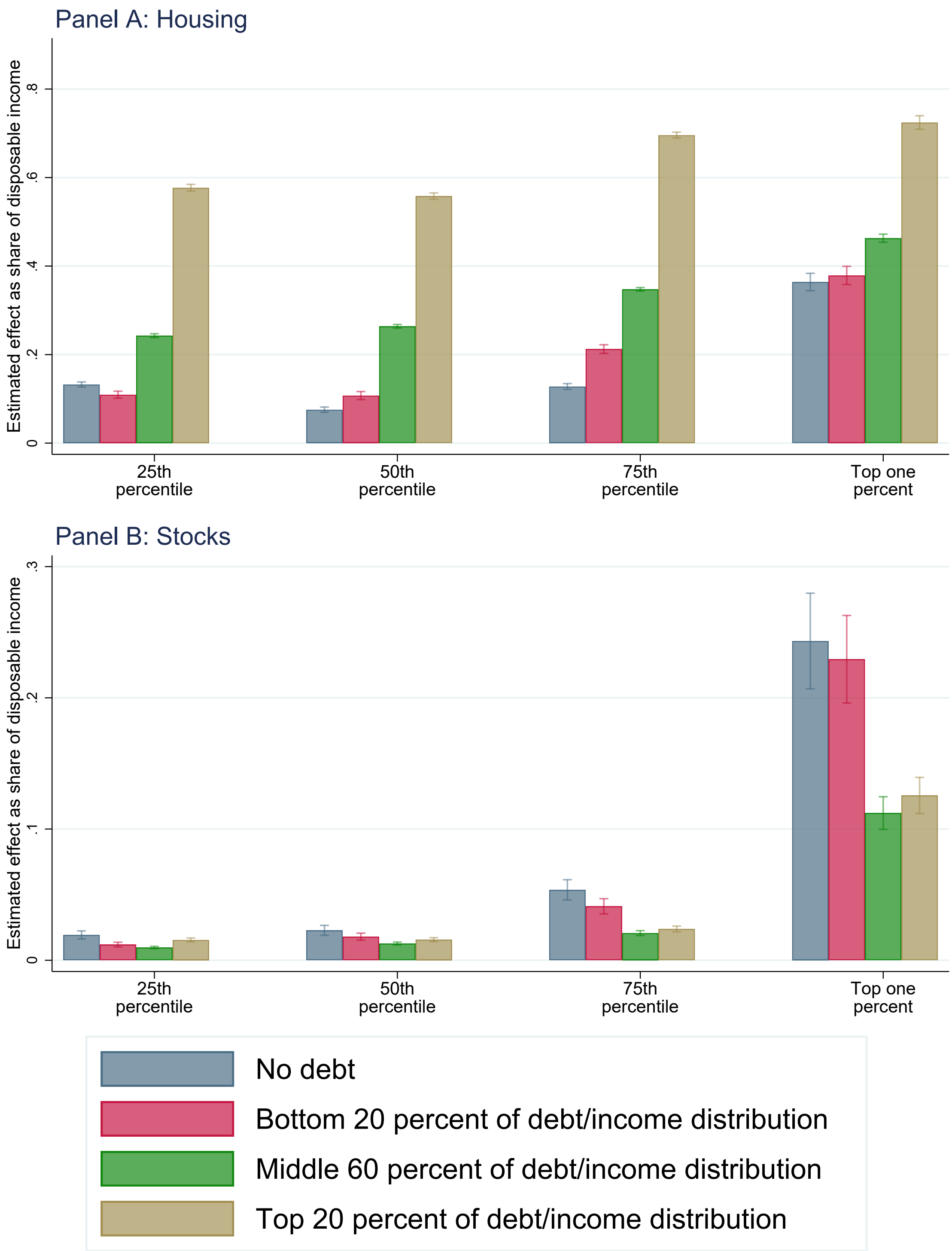


Figure 11: Implications for wealth accumulation and consumption. The figure shows the estimated two-year effect of a one percentage point decrease in the policy rate on the change in net wealth (Panel A) and the number of newly registered cars (Panel B) at different positions in the income distribution.

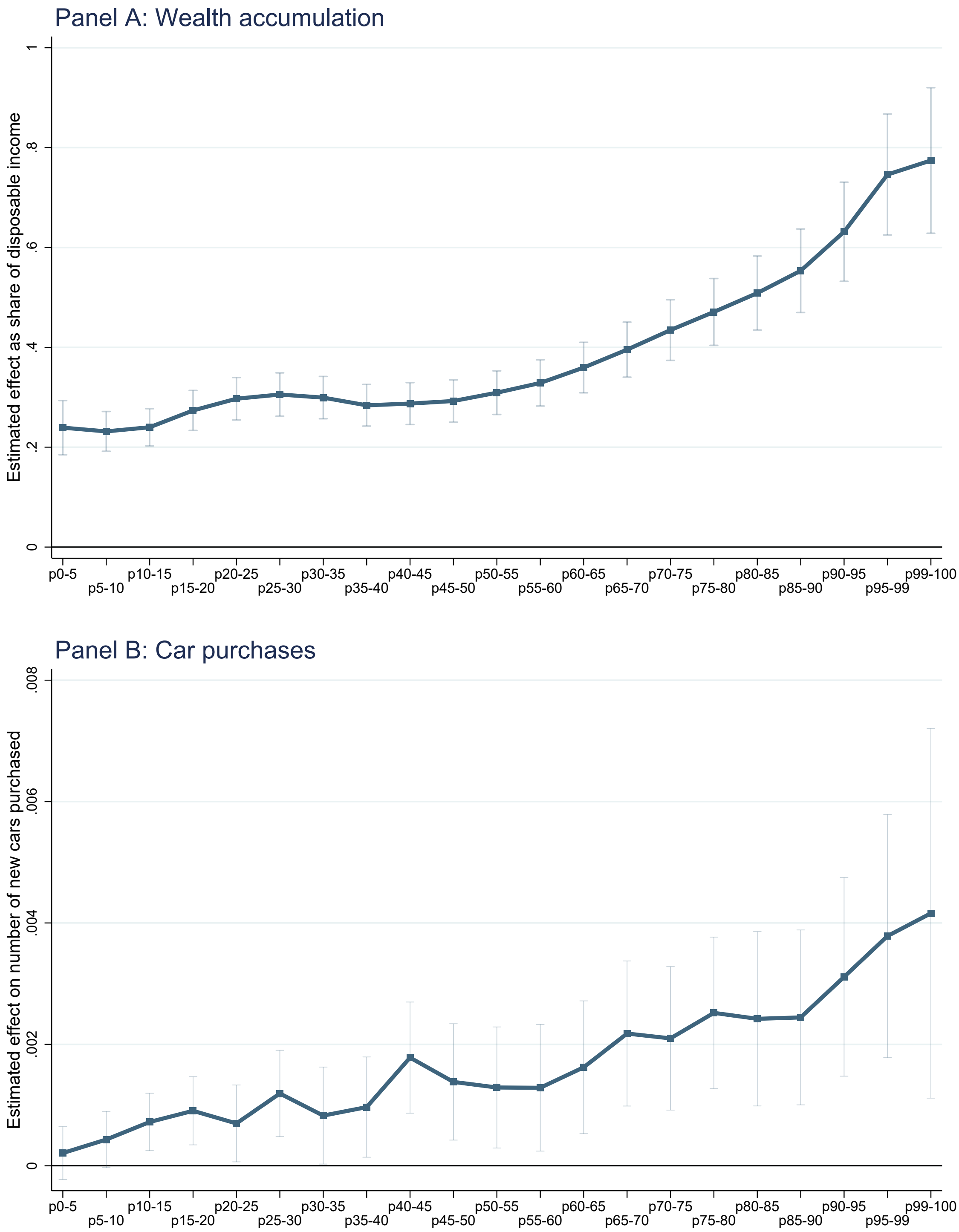


Figure 12: The heterogeneous effect of monetary policy by age groups. The figure shows the estimated two-year effects of a one percentage point decrease in the policy rate on disposable income (Panel A) and asset values (Panel B) for different age groups.

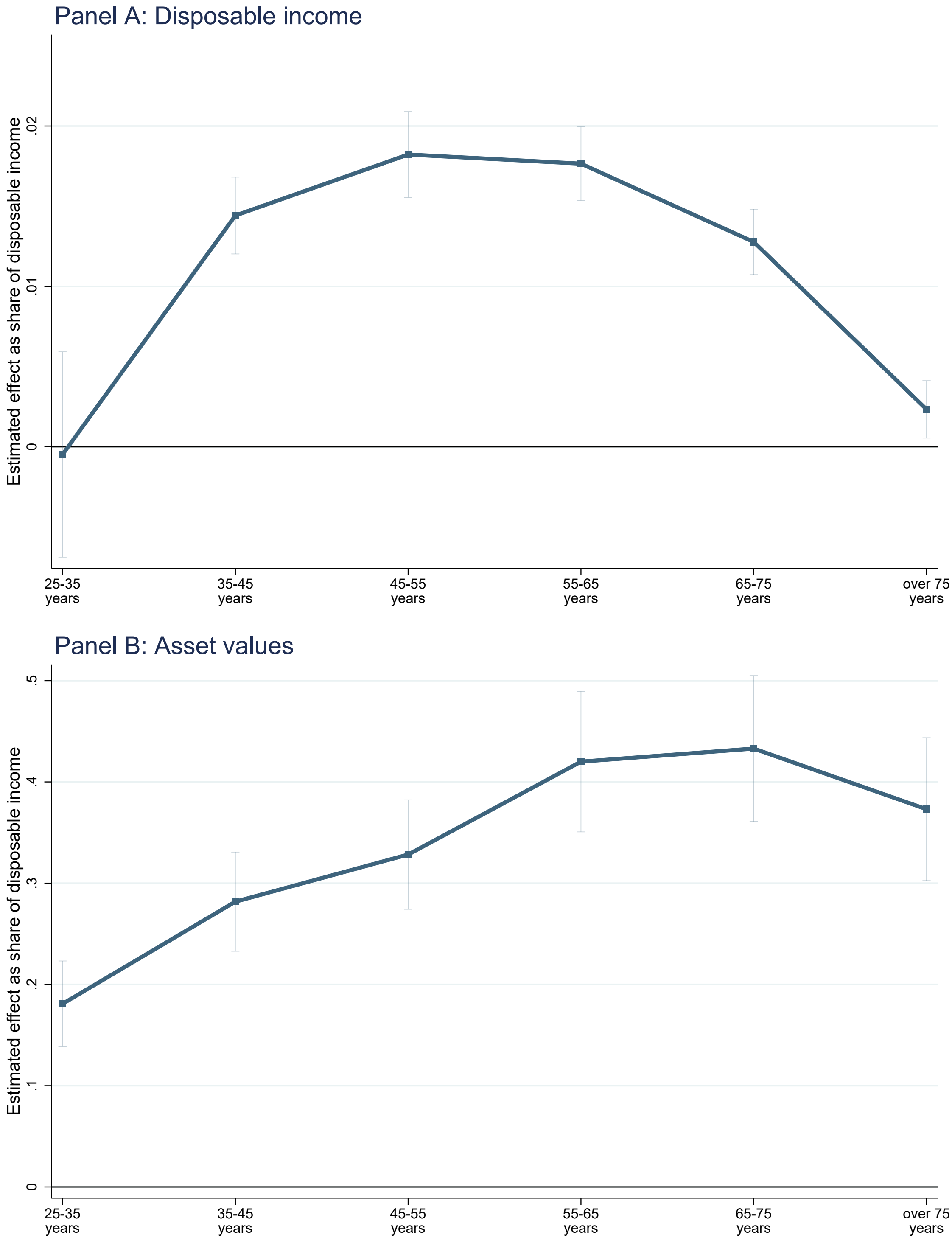
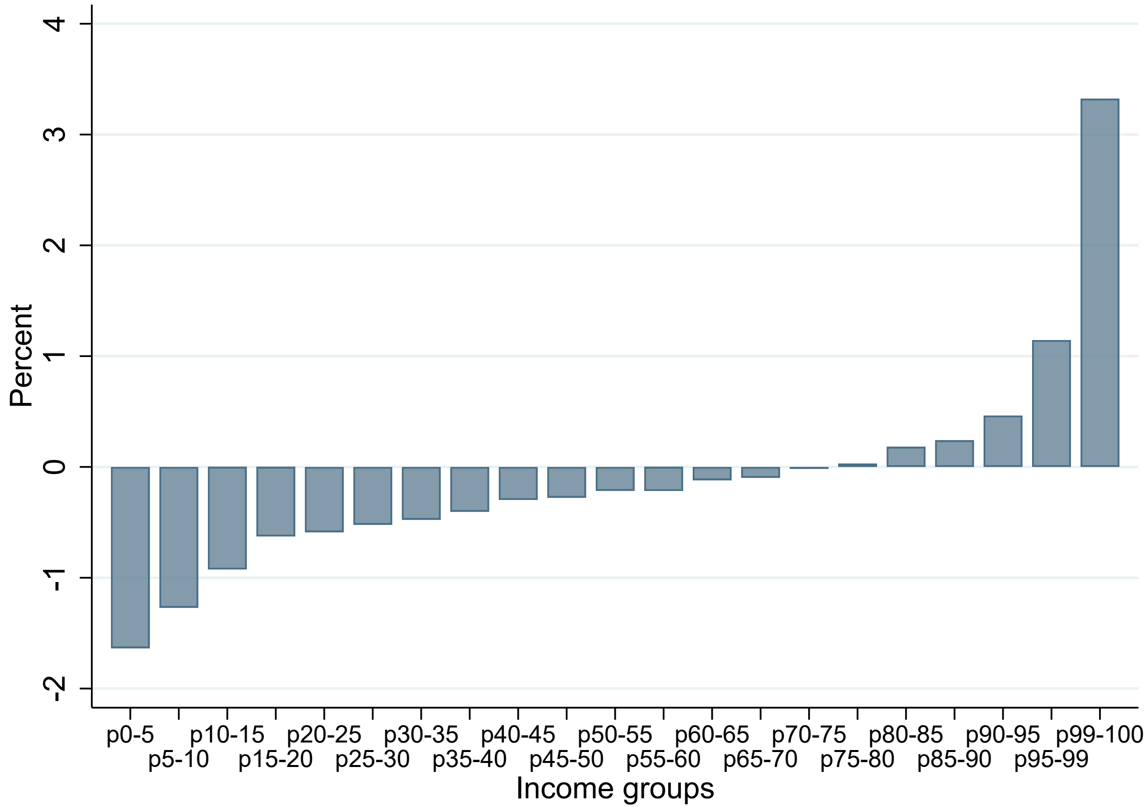


Figure 13: Implications for income inequality. The figure shows the simulated percentage change in each income group's share of total disposable income resulting from a one percentage point decrease in the policy rate. Applying the two-year coefficients from Figure 4, the simulation first computes the counterfactual income gain accruing to each household given its position in the income distribution if the policy rate were lowered by one percentage point and, next, computes the resulting counterfactual shares of total disposable belonging to each income group. The bars indicate the percentage difference between the actual and counterfactual income shares.



ONLINE APPENDIX

Appendix: Property values

From the real estate transaction register ("EJSA"), we have information about each property transaction, including the transaction price, the tax value of the property and a unique property identifier. Combining with the property register ("BOL"), we also obtain information about property characteristics (e.g. type of property, number of square meters).

To construct the house price index, we consider private transactions of one-family houses that are traded no more than two times within the year. Following Finance Denmark (2014), we further restrict the sample of transactions to: (i) Houses where the number of square meters is between 25 and 750; (ii) Transactions with sales price between DKK 100,000 and DKK 25,000,000; (iii) Transactions with sales prices per square meter between DKK 1,000 and DKK 200,000.

For the transactions satisfying these criteria, we calculate the sales price per square meter and the tax value per square meter and winsorize both metrics at the percentiles 2.5 and 97.5 within each year.

Within each municipality and each year, we calculate the average sales price per square meter, which we define to be the *price level* in the municipality-year. We also calculate an *adjustment factor* within each municipality and each year as the sum of sales price per square meter divided by the sum of the tax values per square meter. The adjustment factor serves to approximate the market value of non-traded properties based on tax values.

Finally, based on the series of price levels, we construct a municipality-specific house price index. To compute the change in the asset value of a given property, we apply the index to the estimated market value of the property, which is the tax value stepped up with the adjustment factor.

For example, assume that a property in municipality m has tax value 80 at the end of year t; that the adjustment factor for municipality m in year t is 1.25, and that the price index indicates an increase in the price level of properties in municipality m between year t and year $t+1$ of 10%. The estimated market value of the property at the end of year t is then 100, the tax value of 80 stepped up by the adjustment factor 1.25, and the estimated increase in the value of the property over year $t+1$ is 10, i.e. the 10% price increase applied to the estimated market value of 100.

The available micro-data allows is to calculate price levels and adjustment factors for the period 1992-2013. In the few sample years outside of this period, we use the national house price index from Abildgreen (2018) instead of the municipality-specific price index. Moreover, we apply the adjustment factor for 1992 and 2013 to years before 1992 and after 2013 respectively.

Figure A1: Heterogeneous effects of monetary policy on income components. The figure shows the estimated two-year effect of a one percentage point decrease in the monetary policy rate on the components of disposable income at different positions in the income distribution and over different time horizons: one year after the policy rate is changed (red squares) and two years after the policy rate is changed (blue squares).

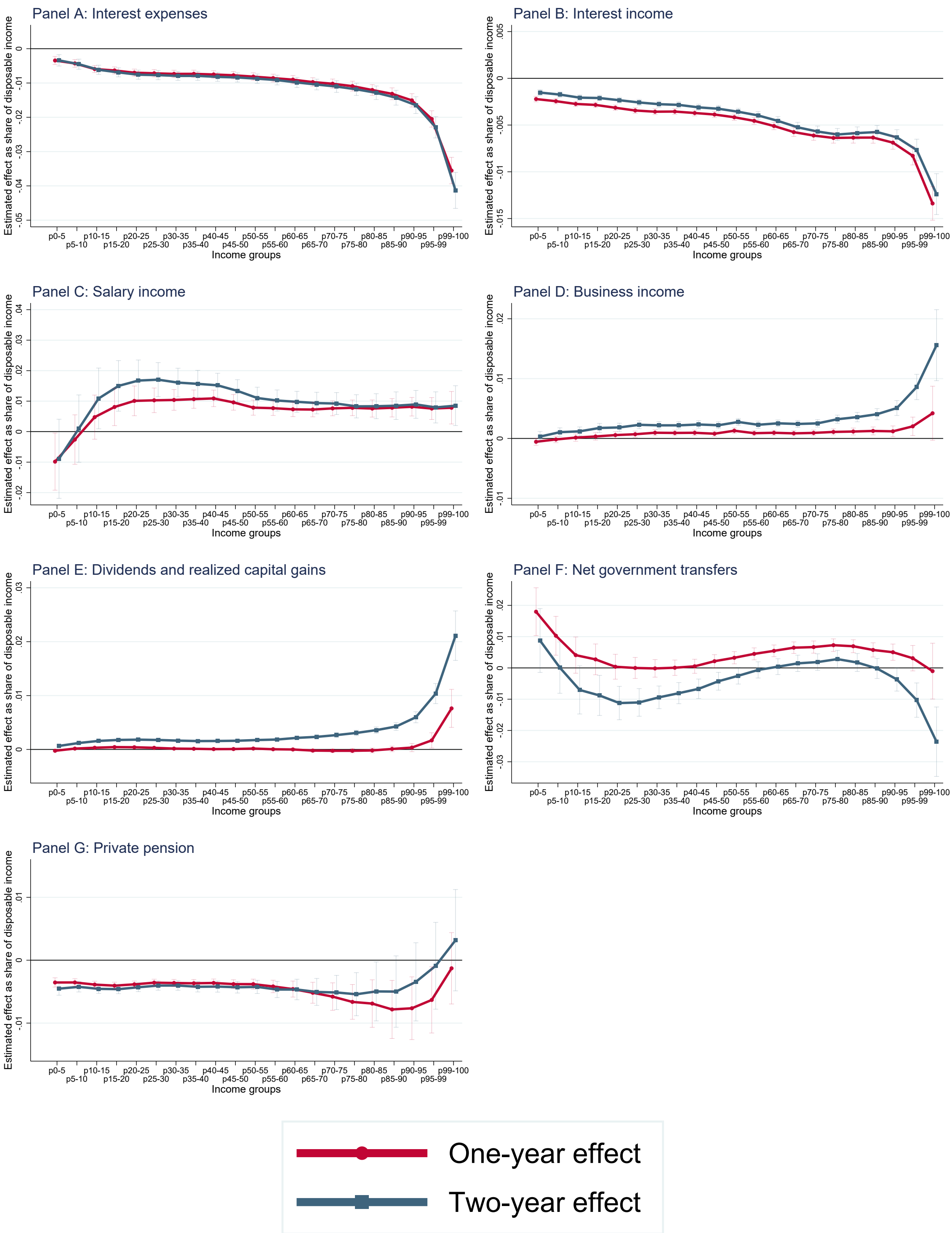


Figure A2: Employment effects. The figure shows the estimated effects of a one percentage point decrease in the monetary policy rate on employment (in weeks) at different positions in the income distribution and over different time horizons: one year after the policy rate is changed (red squares) and two years after the policy rate is changed (blue squares).

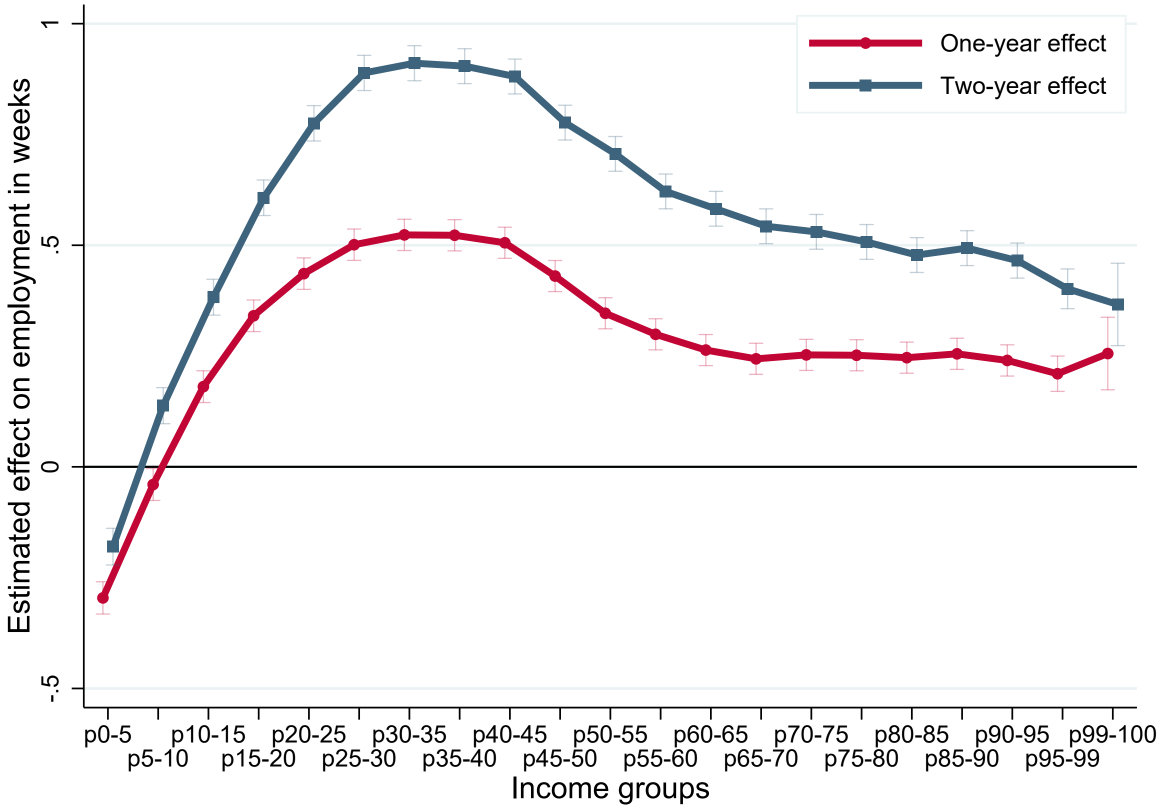


Figure A3: Adding up the estimated effects on components of disposable income. The figure compares, at each position in the income distribution, the estimated two-year effect of a one percentage point decrease in the policy rate on disposable income (Figure 4) to the sum of the estimated effects on the components of disposable income (Figure 5).

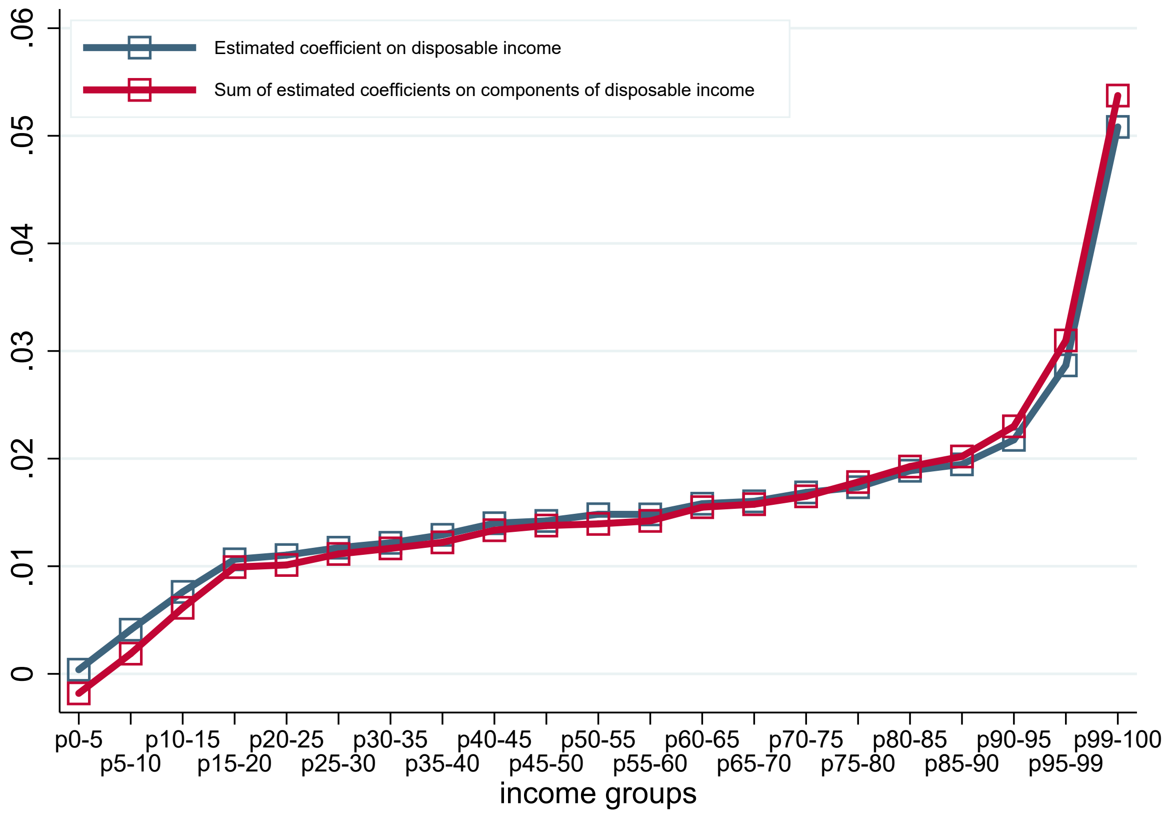


Figure A4: Heterogeneous effects of monetary policy on asset values. The figure shows the estimated "price effect" of a one percentage point decrease in the policy rate on the components of wealth at different positions of the income distribution and over different time horizons: one year after the policy rate is changed (red squares) and two years after the policy rate is changed (blue squares).

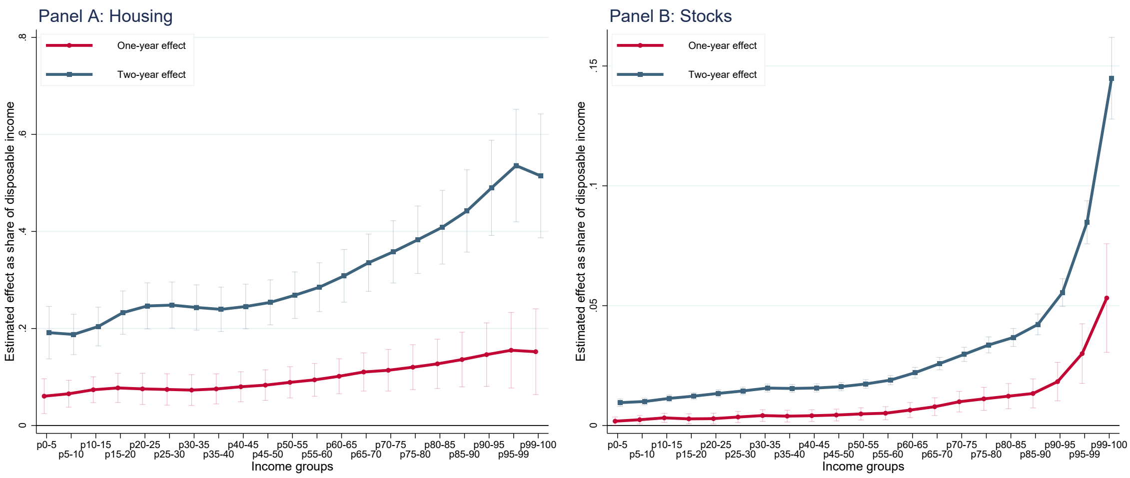


Figure A5: Adding up the estimated effects on asset values. The figure compares, at each position in the income distribution, the estimated two-year price effect of a one percentage point decrease in the policy rate on combined asset values (Figure 7) to the sum of the estimated effects on the individual asset values (Figure 8)

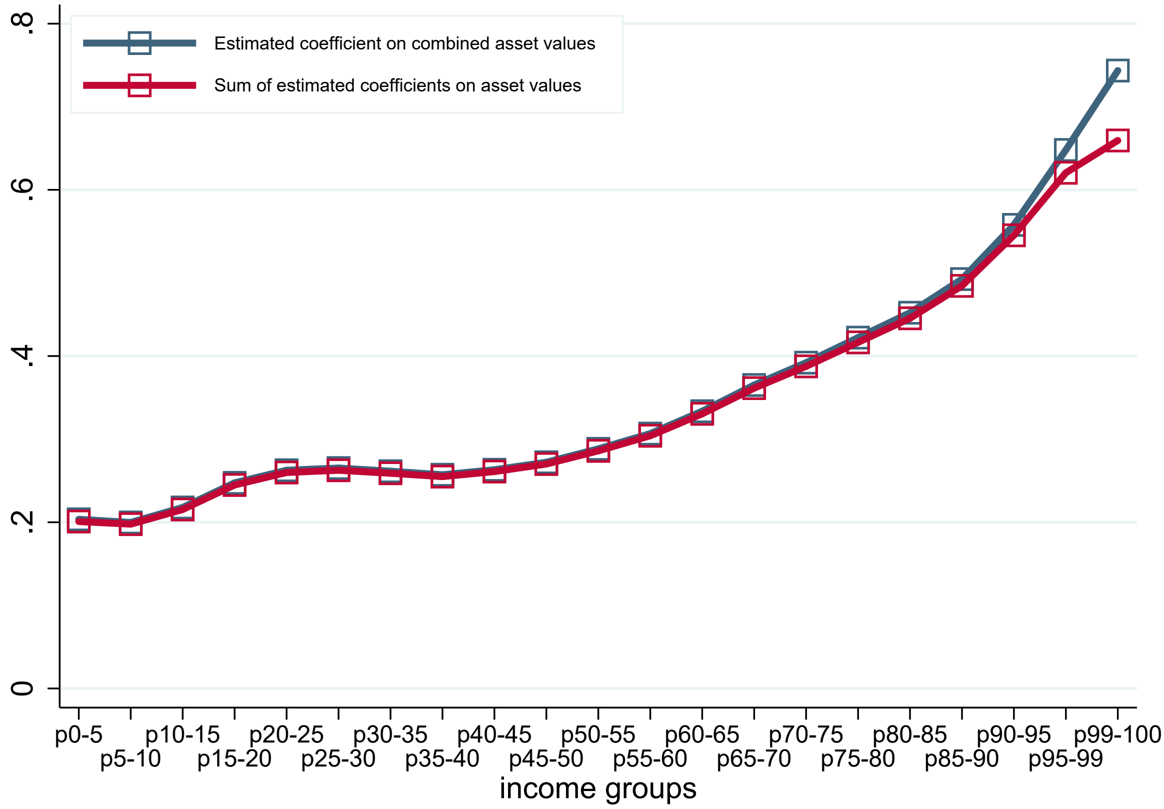


Figure A6: Robustness of heterogenous effects on housing values. The figure compares the estimated "price effect" of a one percentage point decrease in the policy rate on housing wealth (black line - baseline also reported in Figure 8) to the effect on the raw change in the appraisal values of housing (red line).

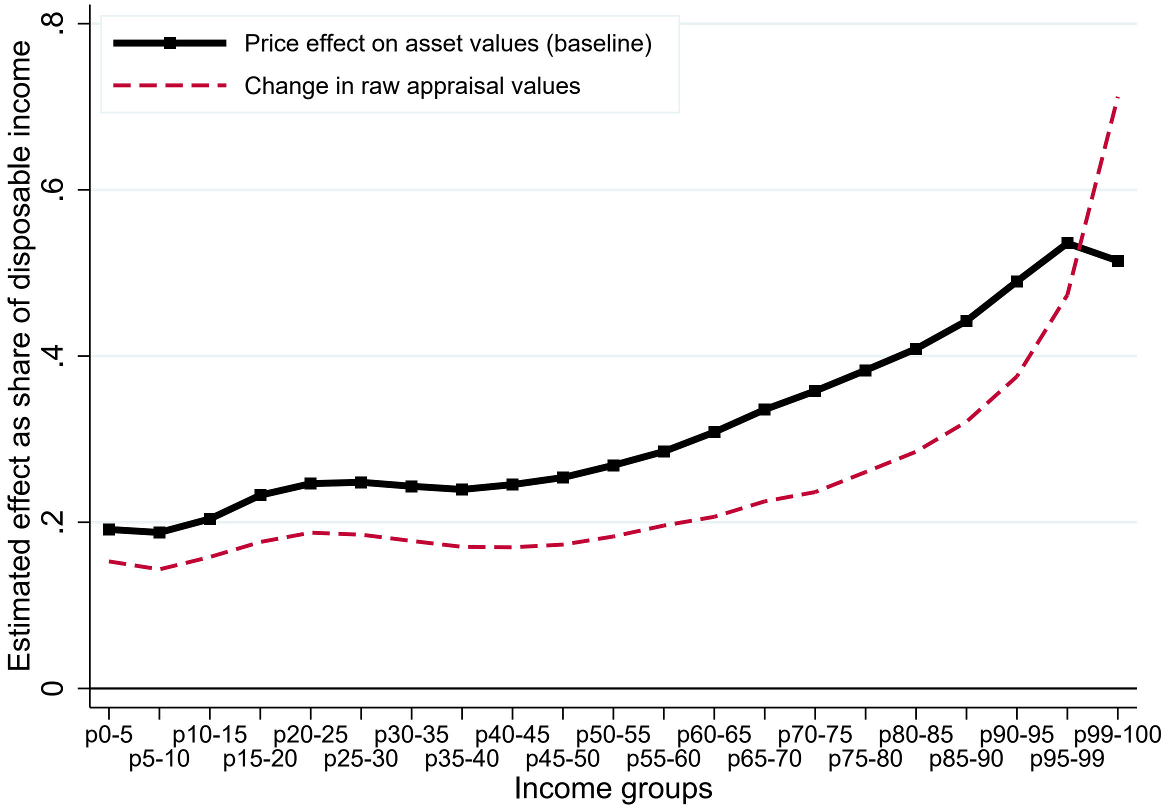
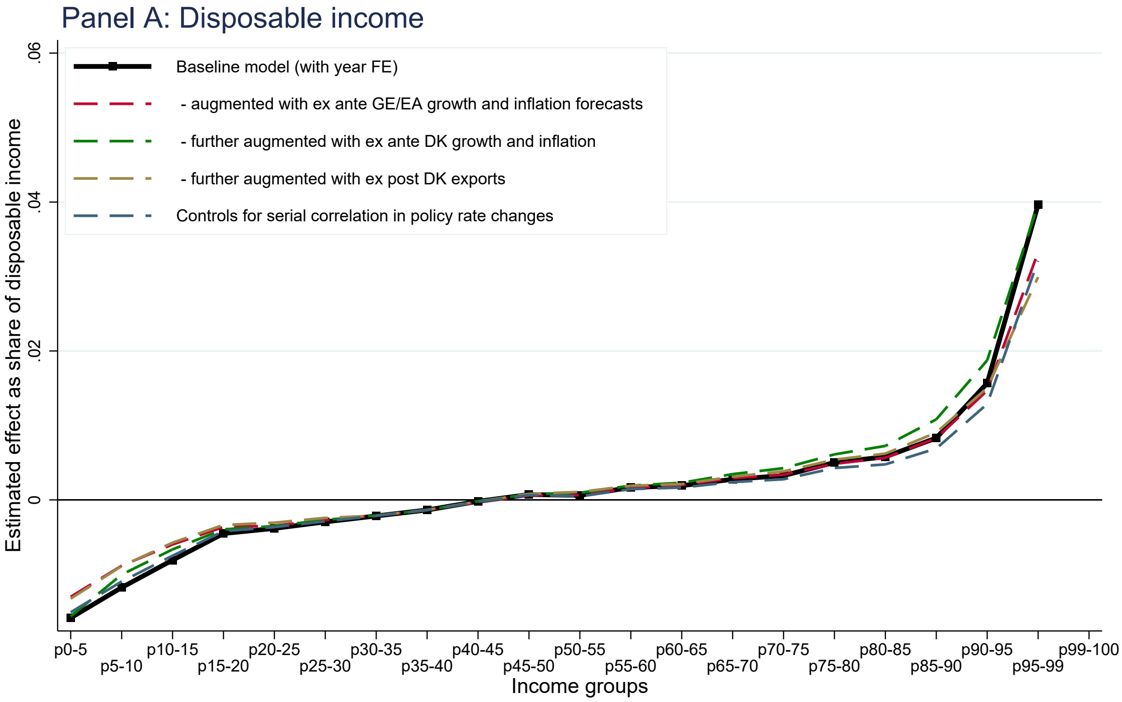


Figure A7: Sensitivity to macro controls. The figure shows the income gradient in the effect of monetary policy on disposable income (Panel A) and asset values (Panel B) estimated with different sets of macro controls. The lines indicate the two-year effect of a one percentage point decrease in the policy rate relative to the effect at the median income level (p45-50). Departing from the baseline model with year fixed effects (black line), the models sequentially add: *ex ante* forecasts of GDP growth inflation in GE/EA (red line); *ex ante* realizations of GDP growth and inflation in Denmark (green line); *ex post* realizations of exports from Denmark (light brown line). The final model includes controls for the leaded monetary policy shock (blue line). All the new controls are interacted with a full vector of income group indicators.



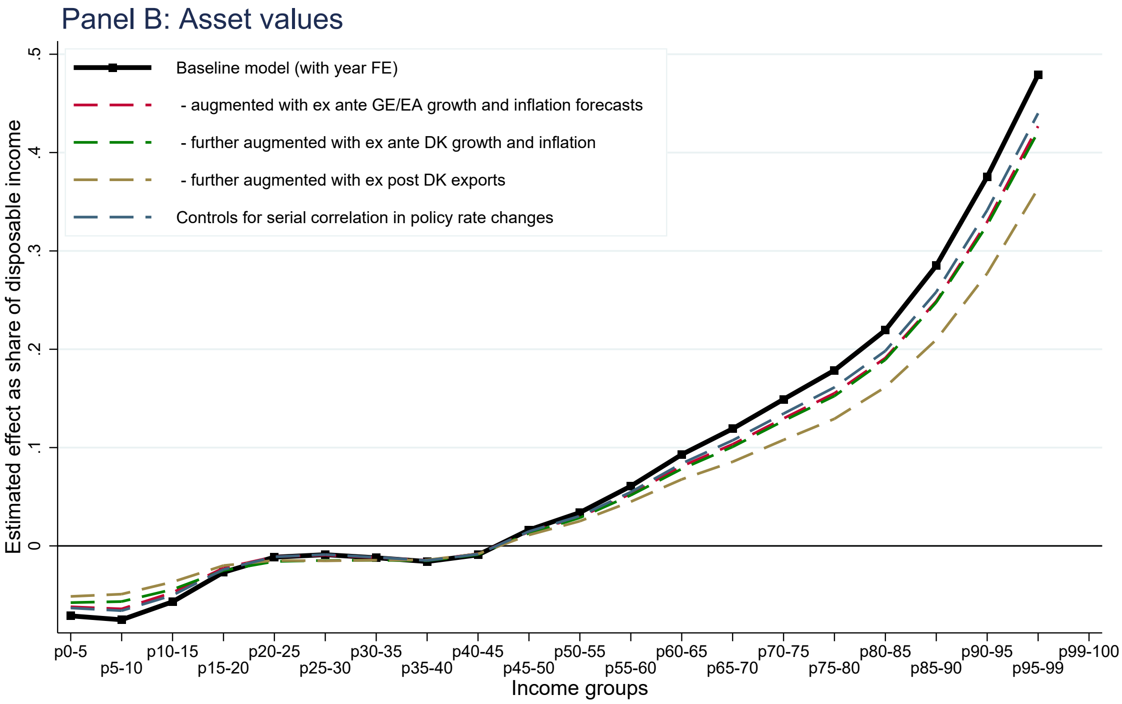


Figure A8: Sensitivity to estimation procedure, measurement and sample. The figure shows the income gradient in the effect of monetary policy on disposable income (Panel A) and asset values (Panel B) estimated with an alternative model, measurement and sample period. The lines indicate the two-year effect of a one percentage point decrease in the policy rate relative to the effect at the median income level (p45-50). Departing from the baseline model with year fixed effects (black line), the model is modified so that changes in the Danish policy rate are instrumented with residual variation in the GE/EA policy rates (red line); measurement is modified so that the German/Euro monetary policy stance is captured by the shadow rate rather than the policy rate (green line); and the sample is extended to include years 2015-2017 in the asset price model (blue line).

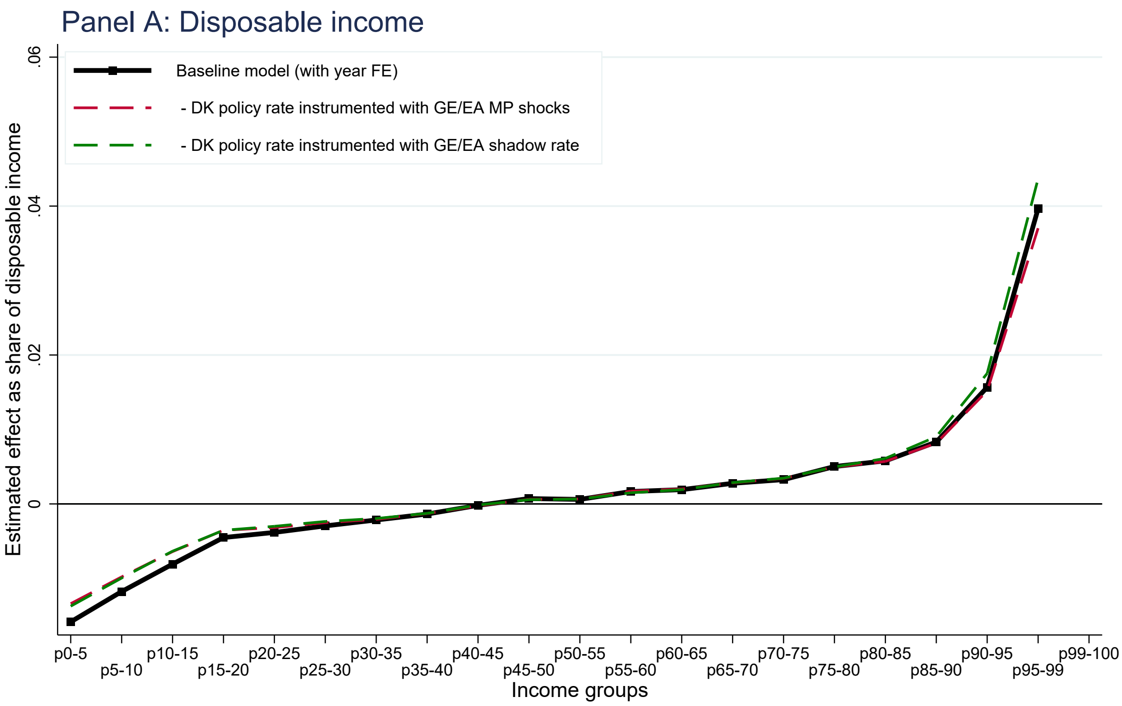


Figure A9: Sensitivity to household fixed effects. The figure shows the income gradient in the effect of monetary policy on disposable income (Panel A) and asset values (Panel B) estimated with the baseline model augmented with household fixed effects. The lines indicate the two-year effect of a one percentage point decrease in the policy rate relative to the effect at the median income level (p45-50).

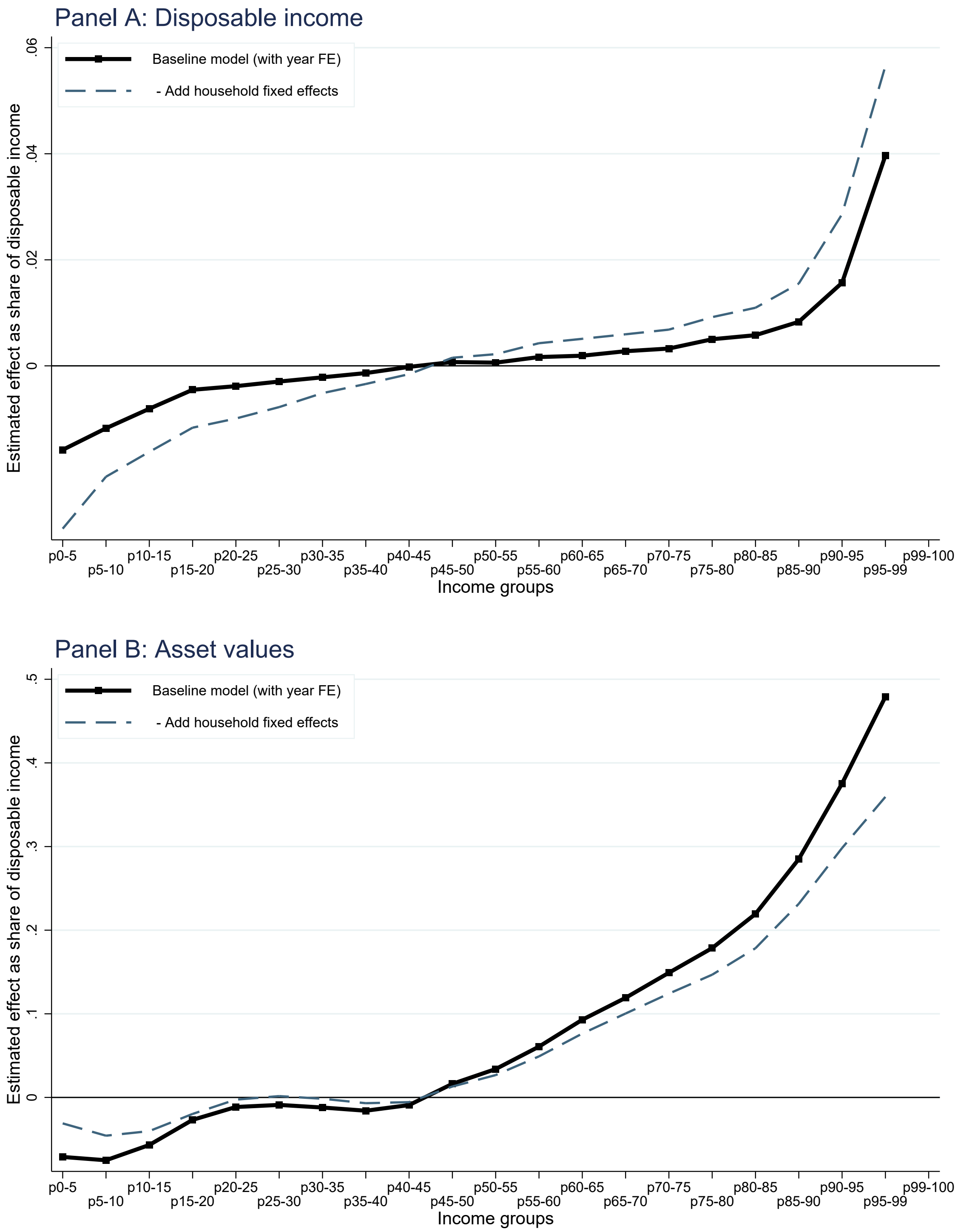


Figure A10: Sensitivity to clustering. The figure shows the 95% confidence levels around the two-year effects of a one percentage point decrease in the monetary policy rate on disposable income (Panel A) and asset values (Panel B) under different clustering schemes: one-dimensional clustering at the level of household (red line); two-dimensional clustering at the level of households and municipality-years (brown line); two-dimensional clustering at the level of households and income-municipality-years (green line); two-dimensional clustering at the level of households and income-municipalities (blue line). The model includes year fixed effects so the effects are measured relative to the effect at the median income level (p45-p50).

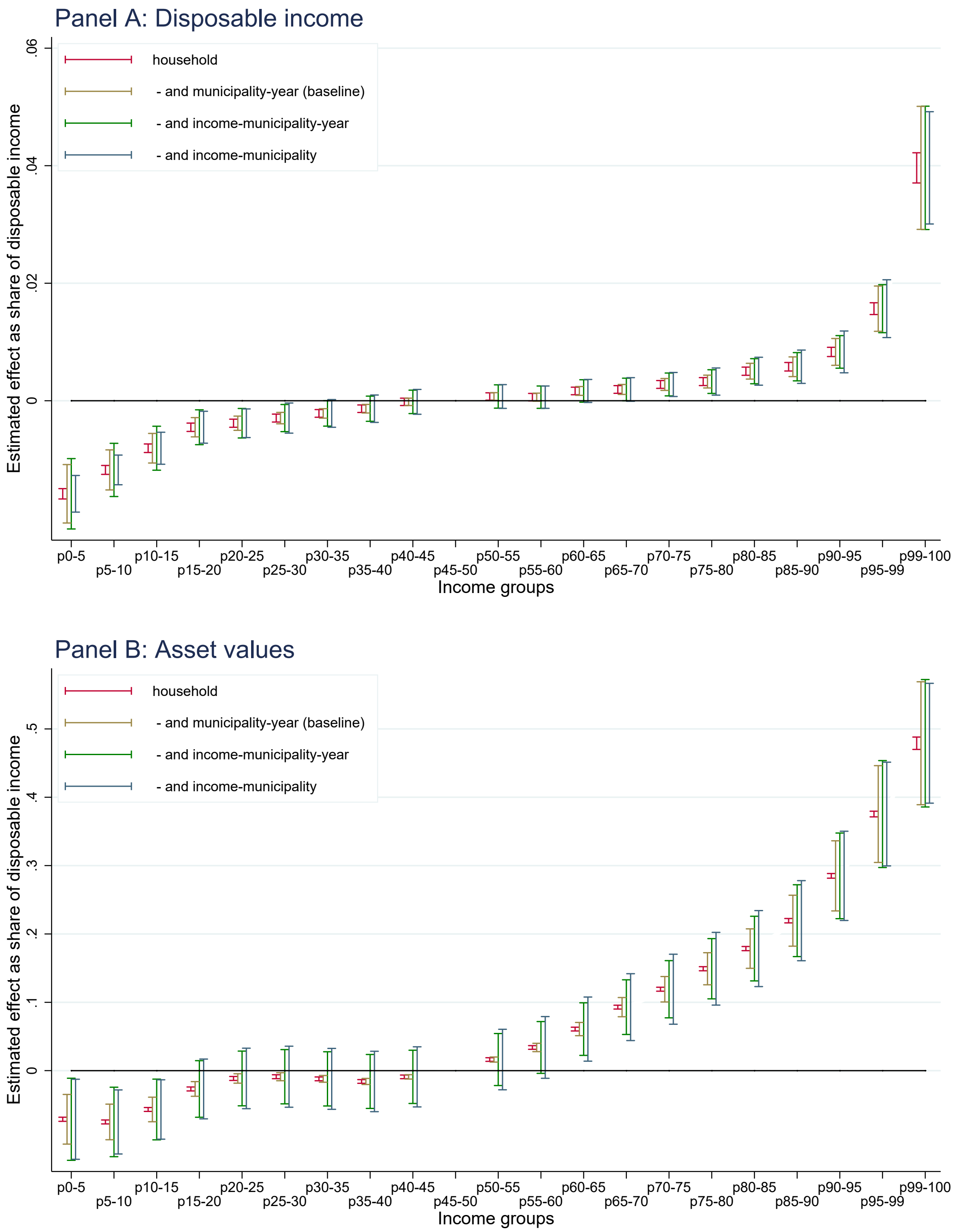


Table A1: Owners of Danish mortgage bonds. The table allocates outstanding Danish mortgage bonds to the sector of the owners over the period 1999-2014.

Non-financial firms	5.5%
Financial firms	42.4%
Insurance companies and pension funds	26.4%
Public Sector	4.6%
Households	6.3%
Foreign investors	12.1%
Unallocated	1.4%

Table A2: Descriptive statistics by age The table shows describes the composition of disposable income and net wealth by age groups. Panel A describes the components of disposable income: salary income, business income, stock income (i.e. dividends and realized capital gains), interest income, transfer income (i.e. government transfers net of taxes), other income and interest expenses. All the income components are expressed as a fraction of disposable income and thus sum to 100%. Panel B describes the components of net wealth: deposits (including bonds), stocks, housing wealth, debt and net wealth (i.e. assets net of debt). All the net wealth components are expressed as a fraction of disposable income. Panel C describes 6 binary indicators: whether the individual is a net creditor (i.e. has positive net wealth); has any debt; owns any stocks; owns any real estate; is at least partly self-employed; and buys a new car.

	25-35 years	35-45 years	45-55 years	55-65 years	65-75 years	>75 years
Panel A: income components (% of disposable income)						
salary income	140%	144%	141%	111%	27%	2%
business income	6%	12%	16%	16%	9%	3%
stock market income	1%	2%	3%	5%	5%	4%
interest income	1%	1%	2%	3%	6%	9%
net government transfers	-32%	-41%	-46%	-28%	23%	49%
interest expenses	18%	20%	19%	14%	9%	4%
private pension	0%	0%	1%	6%	36%	34%
other income	1%	1%	2%	2%	3%	3%
Panel B: net wealth components (% of disposable income)						
deposits	31%	39%	58%	95%	175%	241%
stocks	6%	8%	14%	26%	53%	81%
housing	256%	373%	426%	496%	570%	448%
debt	264%	316%	292%	247%	187%	79%
net wealth	29%	105%	206%	371%	611%	690%
Panel C: descriptive indicators						
is net creditor	47%	58%	69%	80%	90%	96%
has no debt	12%	9%	10%	16%	35%	65%
holds stocks	22%	27%	33%	39%	43%	39%
owns real estate	44%	61%	67%	69%	62%	44%
is self-employed	8%	13%	17%	16%	10%	5%
buys new car	3%	4%	5%	4%	3%	1%