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The Lifecycle of Inequality: Childhood Neighborhoods, Redistribution, and Lifetime Pensions in Finland and Europe

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DOCTORAL DISSERTATION

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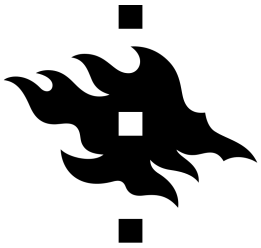

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Abstract

This doctoral dissertation investigates the mechanics of inequality throughout the human lifecycle. It starts from the formation of opportunity in childhood, continues through the mitigation of inequality during working age, and ends with the realization of different life expectancies and pension wealth in retirement. Consisting of an introduction and three essays, the thesis examines these dynamics within the institutional contexts of the Nordic welfare state and Europe.

In the first essay, *The Impact of Childhood Neighborhoods on Intergenerational Mobility in Finland*, I exploit a movers design to isolate the role of location in determining long-term outcomes. My results remain inconclusive. On the other hand, I find significant exposure effects: every additional year spent in a neighborhood where children have a higher income in adulthood improves a child's future income. Surprisingly, for the probability of high school matriculation, this seems to be the opposite. Every additional year in a neighborhood with high proportion of children matriculating from high school lowers a child's own probability of matriculating. These effect sizes are comparable to recent findings from the United States, suggesting that even within the Finnish welfare state characterized by uniform schooling and social security, residential location might still remain a determinant of opportunity. However, the effects disappear with the inclusion of family fixed effects, combined with reduced statistical precision due to sample limitations. The wide confidence intervals include the baseline results and the results of previous literature. Hence, the estimates remain consistent both with the hypothesis that place exerts a causal influence on intergenerational mobility, and with the hypothesis that places do not matter for mobility.

In the second essay co-authored with Markus Jäntti and Jukka Pirttilä, *Is Redistribution Fair? The Effect of Taxes and Transfers on Equality of Opportunity*, we shift focus to adulthood, analyzing how tax-benefit systems across 31 European countries interact with "inequitable" inequality, that is, inequality arising from circumstances beyond an individual's control. We find that while redistribution reduces inequitable inequality in all observed countries, the extent of redistribution often exceeds what is required to strictly neutralize the effect of circumstances. This suggests that European fiscal systems prioritize a broad range of objectives that go beyond correcting for unequal opportunities.

In the third essay, *Asymmetric Information and Lifetime Pensions in the Finnish Pension System*, I examine the final stage of the lifecycle: the decumulation of wealth and insurance against longevity risk. Using administrative data on retirement timing and mortality, I test for adverse selection in the decision to postpone retirement. Although I find evidence that individuals who retire later have longer life expectancies, the fiscal implications of this selection are negligible. The findings indicate that the flexibility of the Finnish pension system does not compromise its fiscal stability by giving extra remuneration to wealthy individuals with longer life expectancies.

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I would like to thank my supervisors Jukka Pirttilä and Roope Uusitalo for their patience over the years and providing guidance and support in my work. I am deeply grateful to my coauthors, Markus Jäntti and Jukka Pirttilä, for their guidance and research insights, and for the collaborative spirit that defined our work in chapter 3. I am grateful to my pre-examiners, Eva Österbacka and Gaute Torsvik, for their valuable comments, and I am honored to have Gaute as my opponent.

I also thank the University of Helsinki for providing the facilities and the Helsinki Graduate School of Economics for fostering a supportive academic community around me. The VATT institute for Economic Research has provided access to Statistics Finland data used in two of my essays. I am especially grateful to Senior Researcher Marja Riihelä at VATT for introducing me to research work and guiding me in my early steps.

During my PhD, I had the opportunity to visit Harvard University and the Opportunity Insights research lab. I sincerely thank professor Raj Chetty for making this visit possible. I am also grateful to Research Principal Benjamin Goldman at Opportunity Insights for his invaluable help with the empirical challenges that I faced at the time.

A PhD has been much more than I initially expected. While it has been more challenging than expected, it has also offered many rewarding opportunities. I have been fortunate to present my work and attend conferences and intensive courses in Finland and abroad. I appreciate the valuable feedback received in seminars at the University of Helsinki, the VATT seminar at the VATT Institute for Economic Research, the Finnish Center for Pensions, the Annual Meeting of the Finnish Economic Association, the Finnish Center of Excellence in Tax Systems Research, the Summer Seminar of Finnish Economists, the OI Morning talk at Opportunity Insights, the Canazei Winter School on Inequality and Social Welfare Theory, and the Annual Congress of the IIPF in Nairobi. These experiences not only improved my research but also allowed me to connect with others working on similar topics.

Pursuing a PhD involves a tradeoff between entering the workforce and investing in the development of independent research skills with uncertain but potentially rewarding future prospects. I am deeply grateful to my funders – Yrjö Jahnsson Foundation, OP Group Research Foundation, Finnish Cultural Foundation, Emil Aaltonen Foundation and University of Helsinki – for making this choice feasible.

I would also like to thank all the people I have met during my PhD. Special thanks to Oliver, Jussipekka, and Salla, with whom I shared an office and many laughs during the first year. Going through the coursework together made the experience both enjoyable and much easier. I am also grateful to Ramin, Teemu, Ulviyya, Satu, Petteri, Nea, Milla, Jetro, Saara, Ella, Ossi, Marc, Kobby, Antti and Peter for their companionship and for making me feel part of a community.

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Chapter 1

Introduction

Economic and social inequality has moved to the forefront of academic and political debate. While traditional economic analysis has focused heavily on inequality of outcomes measured by income or wealth, more recently attention has shifted toward a broader understanding of the determinants of life chances, or equality of opportunity.

This dissertation contributes to this literature by analyzing inequality as a dynamic process that evolves over the course of life. It asks: How do early-life environments shape future prospects? How effectively do state institutions correct for unfair advantages during adulthood? And finally, does private information about health distort the fairness of social insurance in old age?

Early research on economic inequality focused primarily on measurement and description. Seminal contributions include Lorenz (1905), who introduced the Lorenz curve as a graphical representation of income concentration, and Gini (1912), who proposed the Gini coefficient, which remains one of the most widely used summary measures of inequality.

Subsequent work shifted attention toward the determinants of income differences. In particular, Becker (1964) explained earnings inequality through differences in education and skill formation, while Mincer (1974) formalized this relationship in the Mincer earnings equation, which has become a cornerstone of labor economics.

Another important strand of the pre–inequality-of-opportunity literature examined intergenerational mobility. Using data from the Panel Study of Income Dynamics, Solon (1992) provided new estimates of the intergenerational income elasticity (IGE), showing substantially stronger persistence of income across generations than previously thought. This work demonstrated that economic advantage and disadvantage are transmitted across generations, even in relatively advanced economies.

Normative concerns entered the inequality literature through welfare economics. Atkinson et al. (1970) established a formal link between inequality measurement and social welfare functions, introducing normative inequality indices grounded in explicit social preferences. From a philosophical perspective, Rawls (1971) articulated the

principle of fair equality of opportunity, arguing that inequalities are acceptable only if they benefit the least advantaged members of society.

The equality of opportunity and redistribution literature is closely related to the literature on optimal taxation, beginning with Mirrlees (1971), who establishes the foundational framework for designing optimal income tax schedules. Saez (2001) shows how elasticities determine the optimal level of tax progressivity, while Diamond and Saez (2011) argue that realistic assumptions about inequality and social preferences justify progressive taxation in practice.

Although optimal tax theory provides normative limits on redistribution, it does not directly evaluate fairness in terms of circumstances versus effort. The equality of opportunity framework complements this analysis by assessing how taxes and transfers affect inequitable inequality.

Building on earlier contributions, Roemer (1998) introduced a formal framework distinguishing circumstances from effort in the generation of economic outcomes, thereby establishing the modern concept of inequality of opportunity. Building on that, Fleurbaey (2008) formalizes how fairness should account for individual responsibility and develops the normative foundations of responsibility-sensitive justice. He argued that inequalities due to circumstances beyond individual control should be compensated, while inequalities arising from individual responsibility need not be.

Developing this perspective, Fleurbaey and Peragine (2013) distinguish between ex ante approaches, which focus on equality of prospects across circumstance types, and ex post approaches, which compare outcomes conditional on effort. These alternative concepts reflect different normative priorities and imply different empirical strategies. This framework has become the benchmark in the field and is further discussed in Roemer and Trannoy (2015).

For the empirical implementation of inequality of opportunity, a large literature operationalizes this framework by partitioning the population into types defined by circumstances and measuring inequality between these types as inequality of opportunity (Checchi and Peragine, 2010; Ferreira and Gignoux, 2011). This approach has been widely applied in cross-country and intertemporal analyses (Brunori et al., 2013) and encompasses a range of methodological choices regarding the definition of types and the measurement of between-type inequality (Ramos and Van de Gaer, 2016).

A related strand of the literature approaches inequality of opportunity through the lens of intergenerational mobility, emphasizing realized outcomes rather than normative decompositions. Influential contributions in this tradition include Chetty et al. (2014) and Chetty and Hendren (2018a), which use large administrative datasets to document substantial spatial variation in mobility and identify childhood environment as a key determinant of economic opportunity.

While early life conditions shape the accumulation of human capital, the final stage

of the lifecycle is defined by the decumulation of wealth and the risks of longevity. Marmot et al. (1991) found a strong correlation between socioeconomic status and mortality among British civil servants. Chetty et al. (2016) show a strong association between income and life expectancy in the US and Tarkiainen et al. (2012) in Finland. Brown (2003) and Bommier et al. (2011) point out that mandatory pension insurance redistributes across individuals with different life expectancies from short- to long lived individuals often from lower- to higher socioeconomic groups.

Here, inequality also manifests not just in life expectancy, but in the private information individuals possess about their own health. Coile and Gruber (2007) analyze individuals' responses to incentives and selection into different retirement ages in flexible pension systems.

While the theoretical foundations of inequality are well-established, empirical evidence often struggles to connect these lifecycle stages due to data limitations. The Nordic context, with its comprehensive administrative registers and strong welfare institutions, offers a unique laboratory to test these theories. By examining Finland and Europe, this dissertation assesses whether the Nordic model successfully breaks the cycle of inequality, from the neighborhood effects of childhood to the redistribution of income in adulthood, and finally to the actuarial fairness of pensions in retirement. The following sections summarize the three essays that constitute this investigation.

1.1 Summary of the Essays

1.1.1 Essay I: The Impact of Childhood Neighborhoods on Intergenerational Mobility in Finland

In the first essay, I examine the early-life determinants of inequality. If neighborhoods shape the outcomes of children growing up in the area, they also affect the equality of opportunity. Building on the methodology of Chetty and Hendren (2018a), I exploit a movers design to estimate the causal effect of childhood neighborhoods on intergenerational income mobility and education in Finland.

My findings are in line with evidence from the United States and other countries that the place where children grow up have causal effect on their outcomes. However, I also find insignificant exposure effects for income and high school matriculation once family fixed effects are included. Due to the smaller sample size and the low geographical variability of outcomes I cannot rule out meaningful causal effects. Also, the baseline results are well within the confidence intervals. Despite of the crucial distinction between the US and Nordic contexts, like the uniformity of Finnish institutions such as schooling and social security, places still seem to matter for equality of opportunity.

I use Finnish administrative data from 1988 to 2018. The data are high-quality data

from the Finnish Tax Administration, the Social Security Institution Kela, and the Digital and Population Data Services Agency. The data I have are more reliable and cover a longer time span than in Chetty and Hendren (2018a), but have fewer observations. One major difference in the data quality is that the Finnish data covers all resident people in Finland, while Chetty and Hendren (2018a) only include those whose parents file a tax return. If parents never file a tax return, they cannot be linked to the child and are excluded from the analysis. They can link approximately 94% of children to their parents in their study. On the other hand, fewer observations and smaller geographical variation in the Finnish data make my study empirically challenging compared to their study.

I use the Movers Design to distinguish between sorting of different families to move and the causal effect of growing up in a certain area. It is based on the identification assumption that the potential outcomes of the children moving with their families are orthogonal to their age at the time of the move. This assumption seems plausible enough in the article of Chetty and Hendren (2018a), but might not hold in the Nordic context, as pointed out by Maarseveen (2020), and also cannot be ruled out by my findings.

This can be due to multiple reasons. The institutions differ substantially between the US and the Nordics, starting from the extent of social security networks and public services. Hence, it is likely that in the Nordics the socio-economic background matter less for getting important services such as healthcare, schooling, housing, and safe environment. In the Nordics, it is customary for people from all socioeconomic backgrounds to go to the nearest public school instead of paying extra for private education. The trust in the quality of the school institution is very high in all Nordic countries (Christensen and Zhang, 2021). In the Nordics, the school day is shorter, there are more breaks during the day, and there is less homework. The school starts at age 5-6 in the US, and at age 7 in Finland during the period of analysis [1].

My first essay adds to the growing evidence on childhood place effects in different countries. My results are in line with the previous evidence from other studies that childhood neighborhoods matter for long term outcomes. However, it is also in line with the cautious remarks that the identification assumption of the mover's design might not necessarily hold in the Nordic context. I cannot exclude the possibility that families moving with children of different ages might be differentially selected depending on the child's age at move.

1.1.2 Essay II: Is Redistribution Fair? The Effect of Taxes and Transfers on Equality of Opportunity

Co-authored with Markus Jäntti and Jukka Pirttilä, in the second essay I shift focus to the working-age population and the concept of Equality of Opportunity (EOp). Standard

[1] Preschool (at age 6) became compulsory in Finland in August 2015.

inequality measures like the Gini coefficient do not distinguish between inequality arising from effort, which arguably should be rewarded, and inequality arising from circumstances, which is considered inequitable.

Using EU-SILC data across 31 European countries, we decompose inequality into equitable and inequitable components. We find that while redistribution reduces overall inequality, the extent of redistribution exceeds what would be required to strictly neutralize the effect of circumstances in many European countries. However, it does not imply that redistribution would be undesirable, but merely reflects broader objectives beyond correcting for differences stemming from circumstances.

Economic analysis of redistribution has traditionally emphasized equality of outcomes, typically measured by differences between market and disposable incomes. Although this perspective is informative, it neglects an important normative dimension: the origin of inequality. The EOp framework offers an alternative lens, distinguishing between income differences arising from circumstances beyond individual control, such as parental background, and those resulting from individual effort, such as labor supply decisions. From an ethical standpoint, inequalities due to circumstances are considered inequitable, whereas those due to effort are regarded as equitable. This distinction implies that redistribution aimed at correcting inequitable differences is more defensible than redistribution targeting effort-based disparities.

Currently, there is expanding empirical research on inequality of opportunity (IEOp) for various countries. The approach typically used in the literature is based on decomposing overall inequality into between-type and within-type components, where types are defined by circumstances. Between-type inequality is interpreted as inequitable inequality. However, this literature has largely overlooked the role of taxes and transfers in shaping IEOp. Understanding how redistribution interacts with IEOp is essential to evaluate the fairness of fiscal systems and to design policies that align with responsibility-sensitive principles of justice.

We address this gap by systematically analyzing the relationship between redistribution and IEOp in Europe. Using harmonized EU-SILC data for 2010 and 2018, which include detailed information on parental background, we compare IEOp based on market incomes with IEOp based on disposable incomes. Our analysis covers 31 European countries and provides both country-level and aggregate estimates for Europe as a whole.

Our research questions are the following. We first ask to what extent actual redistribution reduces inequality of opportunity across European countries? This is achieved by quantifying the change in IEOp when moving from market to disposable incomes. Second, we investigate what share of observed redistribution can be considered equitable, in the sense of targeting inequitable inequality. In doing so, we assess whether fiscal systems correct income differences due to circumstances or income differences due to effort. Third, we evaluate how much redistribution would be required if policy

focused exclusively on eliminating inequitable inequality? This counterfactual exercise provides insights into the potential scope of fairness-oriented redistribution. In addition, we also explore how redistribution patterns and their impact on IEOp have evolved between 2010 and 2018. By comparing two periods, we explore whether trends in fiscal policy have strengthened or weakened fairness objectives. And finally, we discuss which characteristics of tax- and benefit policies are associated with contributing to greater equitable redistribution in a country comparison.

We find that inequality between groups interpreted as inequality of opportunity is generally modest across European countries. Redistribution reduces both total inequality and inequitable inequality, but the majority of redistribution operates within groups and therefore targets income differences related to effort rather than circumstances. From an EOp perspective, many European tax-benefit systems thus redistribute substantially beyond what would be required to compensate for circumstance-based disadvantages. This does not imply that such redistribution is socially undesirable. Rather, it reflects broader policy objectives, including social insurance, poverty reduction, and solidarity. At the same time, it implies that EOp considerations alone cannot fully rationalize the observed extent of redistribution and highlights a tension between the principles of compensation and reward.

By linking inequality-of-opportunity outcomes to aggregate policy variables, we find that the progressivity of income taxation is more strongly associated with reductions in inequitable inequality than the progressivity of transfer payments. From a policy perspective, this suggests that policymakers should clearly distinguish between the desired extent of redistribution and its design. Moreover, policies aimed at improving equality of opportunity may require less ex post redistribution if effective predistribution policies, such as investments in education and early-life conditions, are in place.

Methodologically, our analysis follows the established literature that decomposes inequality into components attributable to circumstances and effort, and it therefore shares its limitations. Circumstances are imperfectly observed and effort is not directly measured, implying that our estimates of inequality of opportunity should be interpreted as lower bounds. In addition, the results depend on the choice of inequality indices and decomposition methods, reflecting the fact that different measures place different weight on various parts of the income distribution.

1.1.3 Essay III: Asymmetric Information and Lifetime Pensions in the Finnish Pension System

In the third essay, I investigate the role of asymmetric information in the Finnish pension system. With the introduction of flexible retirement ages, a key policy question is whether individuals with longer private life expectancies delay retirement to maximize lifetime

payouts, entailing adverse selection in the system. With considerable wealth in the pension system, adverse selection is potentially very costly, compromising the fairness of the system.

Using mortality data and a Cox Proportional Hazard model, I find that while there is some correlation between retirement age and mortality, the variation in life expectancy across retirement ages is negligible. Consequently, the fiscal cost of adverse selection is minimal, approximately 0.07% of lifetime pensions. This suggests that the current incentives for later retirement are not being excessively exploited by those with longer life expectancies and/or informational advantage.

Throughout the paper, I conduct three distinct assessments to demonstrate that selection is not a significant concern within the Finnish pension system. Firstly, I estimate a Cox Proportional Hazard Model to show that retirement age is correlated with mortality rates and life expectancy. This test only tells about the mortalities during the time window observable in my sample. I can observe retirees up 76-82 years old depending on the retirement age, because the earliest possible retirement year was in 2005, and my data end in 2018.

In order to translate mortalities into life expectancies, I extrapolate the mortalities for ages over 76 years old using cross sectional mortality rates by demographic composition. I estimate that people who retire at age 68 live on average 0.9 years longer than people who retire at age 63. However, since these are conditional lifetime expectations conditional on living until retirement age, the conditioning is different at each retirement age. People who retire at the age of 68 cannot have deceased at the age of 65, while people who retire at the age of 63 could. Therefore, I compare these life expectancies to counterfactual case, where the mortalities have been harmonized across retirement ages, but the conditioning is at the same age in the counterfactual case. When I do this, the differences between observed and counterfactual life expectancies are negligible.

Secondly, I want to check if the fiscal effects of selection are negligible. In order to do that, I estimate the implied cost effects based on differencies in life expectancies. I compare the estimated lifetime costs with different life expectancies to the one, where life expectancy is uncorrelated with the retirement age.

Since people who retire at age 63 have a longer life expectancy than in the counterfactual, the expected cost of their pensions increases by 1.0% compared to equal mortalities case. The expected cost of pensions to people who retire at other ages does not follow any systematic pattern and varies between -1.0 - 1.8% depending on the retirement age. The aggregated cost increases by 233 euros per person or 0.07% of the lifetime pensions due to the selection. The aggregated cost of pensions of all people retiring between 2005 and 2016 over their expected lifetime is 2.2 million euros. That translates to 0.0009% of the total funds in the pension system, or 0.007% of the earnings related pension yearly expenditure, which were 242.4 and 31.4 billion euros in 2022 respectively

(Finnish Center for Pensions, 2023b).

Thirdly, to gain a more nuanced understanding, I delve into estimating lifetime pensions based on counterfactual scenarios representing alternative choices pensioners could have made. This approach enables a detailed analysis of how delaying retirement impacts the pension system's costs and how variations in life expectancies influence these costs.

In this analysis, I account for differences in expected lifetimes corresponding to individuals' actual retirement age choices. By applying the accrual rules of the pension system and considering the expected lifetime at the actual retirement age, I calculate hypothetical expected lifetime pensions across all potential retirement ages. Unlike the previous section where life expectancies were varied while fixing the retirement age, here I fix the life expectancies for each individual and vary the retirement age choice [2].

This method allows me to determine the expected change in lifetime pensions for those opting to postpone retirement. Additionally, I assess how much the expected lifetime pensions would have changed for those who initially decided to retire had they postponed their retirement. By comparing these changes with the hypothetical changes for those who choose to retire earlier, I can assess the relative cost effects of postponing retirement.

Furthermore, I conduct these analyses using counterfactual life expectancies, assuming retirement age is independent of mortality. Surprisingly, in both scenarios, the gains from postponing retirement in terms of expected lifetime pensions are outweighed by the additional pension payments made during the postponement period, except for the cases where individuals delay retirement from the early retirement age of 62 to the first old-age pension retirement age of 63. There are also marginal gains observed when delaying retirement from age 63 to 64 for those who actually postpone, but not for those opting to retire at age 63.

Finally, I calculate the difference between these two sets of calculations to quantify how differences in life expectancies affect the cost implications of postponing retirement. I find that the differences in life expectancies have little effect on the costs of postponing.

[2]That implicitly assumes that changing the retirement age would not change the life expectancy, i.e., no causal effect of retirement on life expectancy and no moral hazard.

Author's Contribution

The dissertation consists of an introduction and three original research articles.

Essay I: ***The Impact of Childhood Neighborhoods on Intergenerational Mobility in Finland***

Single authored.

Essay II: ***Is Redistribution Fair? The Effect of Taxes and Transfers on Equality of Opportunity***

Jäntti and Pirttilä developed the idea, which we then developed further together. Kauhanen conducted the main analysis. Pirttilä and Jäntti correlated the results with macroeconomic variables. Jäntti calculated the Lorenz curves. Jäntti, Kauhanen, and Pirttilä wrote the paper together.

Essay III: ***Asymmetric Information and Lifetime Pensions in the Finnish Pension System***

Single authored.

Report on co-authored essay "Is Redistribution Fair? The Effect of Taxes and Transfers on Equality of Opportunity"

As a doctoral candidate, I played a significant role in the research and development of this co-authored paper. The idea of the paper originated from senior co-authors Markus Jäntti and Jukka Pirttilä, which we then developed further together. My main contribution encompassed creating the dataset, conducting the main analysis, and co-writing the essay with the other contributing authors.

In the collaborative research effort, each team member took on distinct roles and responsibilities. I conducted the main analysis with EU-SILC data, except Jäntti calculated the Lorenz curves. Jäntti and Pirttilä conducted analysis with the results and macroeconomic variables from other datasets. I wrote the Results section of my own analysis, the Data and Methods section and the Conclusions. Jäntti and Pirttilä wrote the Introduction and Literature Review.

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Date: 9/2/2026

Chapter 2

The Impact of Childhood Neighborhoods on Intergenerational Mobility in Finland

Arttu Kauhanen[1]

Abstract

One possible factor affecting inequality in adulthood is the place of living during childhood. My results remain inconclusive. On the other hand I find significant exposure effects of childhood neighborhoods on intergenerational income mobility and on the probability of matriculating from high school. However, these effects disappear when controlling for family fixed effects because of the low power of the test due to the small sample size and small variation of upward mobility between regions. The baseline results and the findings in the previous literature in different countries remain within the confidence intervals.

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2.1 Introduction

If neighborhoods shape the outcomes of children growing up in the area, they also affect the equality of opportunity. In the literature, the concept of equality of opportunity has become increasingly important in recent years, for it pays attention to the process of how inequality in outcomes is formed. Organizations like Opportunity Insights [2] have become prominent in the United States, yet despite extensive efforts in the area of equality of opportunity, only a little research similar to what they do has been conducted in other countries. Nordic welfare state model differs substantially from laissez-faire type of social security in the United States. Hence, it is unclear how these findings are generalizable to different societies. This compromises the external validity of these findings, since we have limited evidence on how the results apply to the countries different than in these studies. Furthermore, these questions have been increasingly of interest in political debates.

In this article, I study the importance of childhood neighborhoods on intergenerational mobility in Finland. My findings confirm the previous findings from other countries in the nordic context. I find significant exposure effects of childhood neighborhoods on intergenerational income mobility and on the probability of matriculating from high school. However, these effects become statistically insignificant when controlling for family fixed effects. The estimates also become imprecise, so that the baseline results are contained within the confidence intervals and I cannot exclude the possibility of economically meaningful effects.

I exploit the movers design of Chetty and Hendren (2018a) using Finnish administrative data from 1988 to 2018. The data I use are high-quality register-based data from the Finnish Tax Administration, the social security institution Kela, and the Digital and Population Data Services Agency. The data I have are more reliable and cover a longer time span than in Chetty and Hendren (2018a), but have fewer observations. One major difference in the data quality is that the Finnish data covers all resident people in Finland, while Chetty and Hendren (2018a) only include those who' s parents file a tax return. If parents never file a tax return, they cannot be linked to the child and are excluded from the analysis. Approximately 94 % children are linked to their parents in their study. On the other hand, fewer observations and smaller geographical variation in the Finnish data make my study empirically challenging compared to their study.

The Movers Design is used to distinguish between sorting of different families to move and the causal effect of growing up in a certain area. It is based on the identification assumption that the potential outcomes of the children moving with their families are orthogonal to their age at the time of the move. This assumption seems plausible enough in the article of Chetty and Hendren (2018a), but might not hold in the Nordic context, as pointed out by Maarseveen (2020) and cannot be ruled out by my findings. This

[2] https://opportunityinsights.org

can be due to multiple reasons. The institutions differ substantially between the US and the Nordics, starting from the extent of social security networks and public services. Hence, it is likely that in the Nordics the socio-economic background matter less for getting important services such as healthcare, schooling, housing and safe environment. In the Nordics, it is customary for people from all socioeconomic backgrounds to go to the nearest public school instead of paying extra for private education. The trust in the quality of the school institution is very high across all Nordic countries. In the Nordics, the school day is shorter, there are more breaks during the day and there is less homework. The school starts at age 5-6 in the US, and at age 7 in Finland during the period of analysis [3]. (Christensen and Zhang, 2021)

This work is organized as follows. Section 2 reviews previous literature. Section 3 presents the institutional context of the study. Section 4 presents research methods, the data and reports the results of the empirical work. Section 5 concludes and section 6 is for appendices. Through my text, I use the concept *better area* for brevity to refer to areas producing higher outcomes and it should have no normative meaning in a broader sense. It takes no stance on how those areas relate in other dimensions which could be more important after all.

2.2 Literature Review

Jencks and Mayer (1990) list possible mechanisms why childhood environment could have an effect on children's outcomes. These mechanisms are peer effects, institutional influences and indigenous adult influences. Peer effects mean a social pressure to, for example, finish high school because "everyone else" does. Institutional influences mean influences of adults outside of the community, such as teachers or police. Even if the neighbourhoods per se are irrelevant, better teachers and different treatment of delinquents by police in affluent areas compared to poor neighbourhoods can affect children's life chances. Indigenous adult influences are neighbourhood role models which prove that success is possible if you work hard. Jencks and Mayer (1990) also point out that the definition of neighbourhoods is always geographical rather than social. However, nongeographic communities such as friendship networks can be even more important. If neighbourhoods are heterogenous enough, children are likely to find friends similar to themselves, as most people prefer to do.

Jencks and Mayer (1990) also note that affluent neighbourhoods can also have a negative effect on children's outcomes. People compare themselves to others around them and may judge their success or failure relative to neighbors and friends. The same income would make people feel poorer in a richer area and richer in a poor neighbourhood,

[3]Preschool (at age of 6) became compulsory in Finland in August, 2015.

and a college dropout could feel less competent in a highly educated area than in an area of high school dropouts. Because of these effects on the opinion of one's own abilities, the most affluent area might not be the best option for children of low income families with less education. Competition for resources such as good grades and jobs can also make an affluent neighbourhood a liability. This is compatible with the estimate of Chetty and Hendren (2018b) that the most affluent areas do not produce the best outcomes in the United States.

In the United States, equality of opportunity measured as income rank correlations between parents and children varies across areas (Chetty et al., 2014). According to Chetty and Hendren (2018a) these differences stem from causal effects of places instead of different families sorting to different areas. Bergman et al. (2019) find that low income families do not move to high-opportunity areas because of barriers that prevent them from making such moves. Also students who had randomly assigned to a more experienced teacher in kindergarten have higher earnings in adulthood (Chetty et al., 2010). Moreover, low- and middle-income students attend selective schools at much lower rates than their peers from richer families with the same test scores (Chetty et al., 2020). Chetty et al. (2020) also report that the degree of parental income segregation across colleges is very high, similar to that across neighborhoods. Using simulation methods they find that equalizing application, admission, and matriculation rates across parental income groups conditional on test scores would reduce segregation substantially. They conclude that changing how students are allocated to colleges could substantially reduce segregation and increase intergenerational mobility in the United States.

The findings of Chetty and Hendren (2018a) have been replicated in other countries in the literature. Faurschou (2018) find that living in economically deprived neighborhoods in Denmark significantly increases psychiatric medication use among vulnerable populations, particularly men and non-Western immigrants. Deutscher (2020) replicate the findings in Australia, which is a country with lower inequality than the US. The place where a child grows up has a causal effect on their adult income, education, marriage, and fertility. Alesina et al. (2021) find that despite substantial spatial sorting in Africa, areas also have causal effects on uneducated parents children's probability to complete primary school. Laliberté (2021) estimate that 50%-70% of the benefits of moving to a better area on educational attainment in Montreal are due to access to better schools. Britto et al. (2022) find that the causal effects of the places explain 57% of the spatial variation in mobility in Brazil. According to their estimates, having 10 percentile points higher parental income rank is associated with a 5.5 percentile points higher child income rank, and persistence is even higher in the top 5%.

Since geographic differences are smaller in Northern Europe compared to the US, movers might be differently selected (Maarseveen, 2020). Using administrative data from the Netherlands and Sweden, he finds that cognitive abilities of children decline with

their age at move, which is a violation of the identification assumption of the design in Chetty and Hendren (2018a). Furthermore, this decline cannot be explained by the observed household characteristics such as parental education. He suggests that researchers apply caution when using similar designs in Northern-European contexts, especially when sibling comparison is infeasible.

2.3 Institutional Context

Income rank correlation and the differences between geographical areas are smaller in Finland than in the United States. This can be due to many reasons, for example, different culture, differences in the role of government in the economy, or different schooling system. However, years of education, completion of university degrees and personal income are related to parents' income, especially at the top of the income distribution. Suoniemi (2017) reports regional differences in upward mobility, although these differences are relatively small. For example, between the most mobile and the most immobile areas, children of families in the lowest income decile in the Tavastia Proper (Kanta-Häme) region have outcomes at 43–47 percentile point in national income distribution on average, whereas children with similar parental income from Helsinki region or Lapland area have outcomes at around 36–38 percentile points on average.

The smaller geographic variation in intergenerational income mobility in Finland raises the question of whether the results of Chetty and Hendren (2018a) hold in a different institutional context. It is possible that the uniform quality of Finnish comprehensive schools, free higher education, and homogeneous culture result in places having small or no causal effect on outcomes. However, it is also possible that the areas have causal effects on children's outcomes in Finland, even though the economic significance would be limited compared to the United States. This would mean that moving a given child to a different area would change his or her outcome, at least slightly.

Since income mobility and the fraction of children matriculating from high school vary between areas, the question arises of whether this variation is causal. If this is the case, it would be important to know. It would also be of interest to obtain information about how large those effects are. Firstly, it would provide information on how much geographical aspects affect the differences in outcomes. If these effects are significant, they should matter for policy decisions. Secondly, it would provide further research topics on what characteristics of the areas cause or are related to differences in the outcomes. This in turn could give a political tool to intervene in the heritability of income, health- and educational outcomes, which is often discussed in politics.

2.4 Research Methods and Data

For estimation purposes, it is useful to classify two possible reasons why children's outcomes can vary between regions. The first is that places have causal effects on economic mobility. In this case, moving children to a different environment would change their outcomes later in their lives. The other possibility is that the differences between the areas are due to the self-selection of different people to live in those areas, which implies that the decision of a family to move from one area to another would have no effect on the outcomes of the children of the family. Then unobservables common to households in the same neighborhoods may be mistakenly attributed to neighborhood effects (Aaronson, 1998). The purpose of this study is to test using Finnish data whether areas have causal effects on children's outcomes, and if so, how large those effects are.

2.4.1 Important Concepts

I follow Chetty and Hendren (2018a) to define an important concept for estimation, *potential outcomes*. In reality, it is not possible to observe all the potential outcomes and they have to be estimated from the data. The potential outcome in the area of origin is the outcome the moving children would have obtained had they lived their entire childhood in the area they moved from. Similarly, the potential outcome in the destination area is the outcome the moving children would have obtained had they lived from birth until adulthood in the area where they move to. We never get to know either of them for children who actually move because neither of the potential outcomes is actually realized. Similarly, it is possible to observe only one of the potential outcomes for children who live in the same area through their entire childhood, the actually realized outcome, but we never observe the outcome they would have obtained had they lived in another area.

As in Chetty and Hendren (2018a) I define the *causal effect of the move* as the difference between potential outcomes and estimate it as a difference between the estimated potential outcomes. They estimate the potential outcomes using the data of children who live in the same area for their entire childhood, i.e. what the average outcome of children in each area is, given the income of the parents. It is necessary to condition on parental income, because the income of the parents is an important predictor of the outcomes of the children. Controlling has to be done separately for each area, because place effects can vary across areas at each income level. They also control for the birth cohort for each commuting zone, because it is possible that place effects vary over time, and this variation can be different at each income level. I use income *ranks* for the same statistical advantages as Chetty and Hendren (2018a). The relationship for income ranks between the child and the parent is more linear than the relationship for

income or log-income. It is also less time-dependent while income itself is not directly comparable if measured in different years or decades.

To elaborate more on this matter, I define the concept of *exposure effect*. The objective of the estimation is to determine how much children's potential outcomes improve on average when they move. Estimating childhood exposure effects answers this question. I follow Chetty and Hendren (2018a) and define the exposure effect at age m as the impact of spending the year m in an area where the permanent resident's outcomes are one percentile point higher.

In an idealized experiment, children born in year s are randomly assigned to a new area d starting at age m for the rest of their childhood. The best linear predictor of the outcomes y_i based on the same year s born permanent residents' outcomes $\bar{y}_{pos} = \frac{1}{N_{pos}} \sum_{j \in (p,o,s)} y_j$ in parents' income percentile p in the area d would be

$$y_{\mathrm{i}} = \alpha_m + \beta_m \bar{y}_{pds} + \theta_i \tag{2.1}$$

where θ_i is the error term that is orthogonal to $\bar{y}_{pds}$[4] due to random assignment and captures the effects of family inputs and other uncontrollable determinants on children's outcomes. Hence β_m gives the *average* effect of spending year m onward of one's childhood in an area, where permanent residents have one percentile point higher outcomes. It should be noted that the estimate does *not* give a causal effect of any given area because these effects are likely to differ between areas. (Chetty and Hendren, 2018a)

Given the estimation in the idealized experiment, the exposure effect at age m is given as $\gamma_m = \beta_m - \beta_{m+1}$. A positive exposure effect γ_m of any age allows one to reject the null hypothesis that areas do not matter. The magnitude of $\beta_0 = \sum_{m=0}^{T} \gamma_m$ – the impact of assigning children to a better area from birth until the outcome is measured at age T – is an estimate of the degree to which the differences in outcomes are due to causal effects vs. selection. If place effects are homogeneous across children within birth cohorts and parent income groups, the Chetty and Hendren (2018a) framework implies that $\beta_0 = \sum_{m=0}^{T} \gamma_m = 0$ would imply that all variation is due to selection effects and $\beta_0 = \sum_{m=0}^{T} \gamma_m = 1$ would imply that all variation is due to causal effects. However, it is important to note that this interpretation specifically rules out exposure-based effects; if neighborhoods exert age-invariant 'level' effects—impacts that are constant regardless of the age at move—these would be differenced out in the calculation of γ_m and would not be captured by this summation. (Chetty and Hendren, 2018a, p. 14)

[4]Note: $\bar{y}_{pos} = \frac{1}{N_{pos}} \sum_{j \in (p,o,s)} y_j$ depends on the individual index i only through the parental income percentile p, origin o, and birth cohort s, and is constant within each (p, o, s) cell. I follow the notation of Chetty and Hendren (2018a).

2.4.2 Observational Data

I follow the same identification strategy as Chetty and Hendren (2018a). They exploit variation in children's ages at the time when their parents move between different areas. Clearly, parents who move are not representative of the population as a whole, and the decision about whether and where to move is also endogenous. Hence, the error term θ_i in Equation (2.1) is correlated with $\bar{y}_{pds}$. For example, parents who move to a richer area may have a latent ability or wealth that affects the outcomes of their children. Therefore, estimating (2.1) using observational data gives the regression coefficient

$$b_m = \beta_m + \delta_m \tag{2.2}$$

where $\delta_m = \frac{cov(\theta_i, \bar{y}_{pds})}{var(\bar{y}_{pds})}$ is a selection bias. It measures the extent to which movers' unobservable inputs covary with permanent residents' outcomes in the destination area. However, if the *magnitude* of this selection bias does not vary with children's age at the time of the move, i.e. $\delta_m = \delta$ for all m, the average causal effect of moving for those who move — the treatment on the treated — at age m can be obtained as the difference of the effect of moving at age m and $m+1$, because the selection effect δ cancels out at estimation: $\gamma_m = \beta_m - \beta_{m+1} = b_m - b_{m+1}$. Therefore, to isolate the causal exposure effects $\{\gamma_m\}$, we must impose the strict identifying assumption that the unobserved family selection effects $\{\delta_m\}$ are orthogonal to the child's age at the time of the move. In other words, families moving to better areas can be different from those moving to worse areas or from those who do not move, as long as the extent of this selection does not vary with the age of the child at the time of the move. (Chetty and Hendren, 2018a)

Expanding this estimation on all children who move exactly once when they are m years old yields to the following linear regression:

$$y_i = \alpha_{qos} + b_m \Delta_{odps} + \varepsilon_{1i}, \tag{2.3}$$

where y_i denotes the child's i income rank (or probability of completing high-school matriculation examination), α_{qos} is a fixed effect for the origin area o by parent income decile q and birth cohort s and $\Delta_{odps} = \bar{y}_{pds} - \bar{y}_{pos}$[5] is the difference in predicted outcomes of permanent residents in the destination versus origin areas given parent income rank p and birth cohort s. Equation (2.3) can be interpreted as an observational analog of Equation (2.1).

Let us examine $\bar{y}_{pcs}$ more closely, which denotes the average rank of children with parents in the percentile p of the region c in the birth cohort s, that is,

$$\bar{y}_{pcs} = E[y_i | p(i) = p, c(i) = c, s(i) = s].$$

[5]This difference in the fitted values is again constant within each (p,d,o,s) cell.

Then, the explanatory variable Δ_{odps} can be written,

$$\begin{aligned}\Delta_{odps} &= \bar{y}_{pds} - \bar{y}_{pos} \\ &= E[y_i|p(i) = p, c(i) = d, s(i) = s] - E[y_i|p(i) = p, c(i) = o, s(i) = s].\end{aligned}$$

In practice, I proxy neighborhood quality in the origin and destination areas using average outcomes of children growing up in the areas. If neighborhood quality affects children's outcomes, differences in these averages should predict the outcomes of children who move between areas.

Because some cells defined by parental income percentile, birth cohort, and region contain few or no observations, I estimate the corresponding mean outcomes $\bar{y}_{pcs}$ using a linear regression model, thus smoothing across cells:

$$y_i = \alpha_{cs} + \phi_{cs} p_i + \varepsilon_i \tag{2.4}$$

Hence, I obtain estimated potential outcomes for the movers as fitted values from regression (2.4):

$$\Delta_{odps} = \bar{y}_{pds} - \bar{y}_{pos} = \underbrace{\underbrace{\hat{\alpha}_{ds} + \hat{\phi}_{ds} p_i}_{\text{in the destination}} - \underbrace{\hat{\alpha}_{os} - \hat{\phi}_{os} p_i}_{\text{in the origin}}}_{\text{fitted values from the regression (2.4)}} \tag{2.5}$$

Expanding regression equation (2.3) for all ages from 1 to 17 gives:

$$y_i = \alpha_{qosm} + \sum_{m=1}^{17} b_m I(m_i = m) \Delta_{odps} + \sum_{s=1980}^{1988} \kappa_s I(s_i = s) \Delta_{odps} + \varepsilon_{2i}, \tag{2.6}$$

where α_{qosm} is an origin area o fixed effect by parent income decile q, birth cohort s, and the age at move m. $I(x_i = x)$ is an indicator function that equals 1 when $x_i = x$ and 0 otherwise. This generalizes (2.3) by interacting the age at move m with the explanatory variable Δ_{odps}. It also allows the effects of Δ_{odps} to vary across birth cohorts, because the locations of the children are observed from the age of 8 for the 1980 cohort, but already from birth for the 1988 cohort. This implies a greater measurement error in Δ_{odps} for earlier birth cohorts, as their area of origin is undetermined for a longer time in their childhood. Even if it is random, a measurement error in an explanatory variable creates a bias in the estimates of b_m that is greater for earlier birth cohorts. This bias is a problem if the age at move is not balanced across cohorts. However, it can be accounted for by the coefficients κ_m. Inclusion of cohort interactions means identification from within-cohort variation in ages at move, i.e., comparing children with other children born in the same year (Chetty and Hendren, 2018a).

The problem with (2.6) is that with 10 income deciles, 18 regions, 9 birth cohorts, and

children moving at 17 different ages it requires estimating $10 * 18 * 9 * 17 = 27540$ fixed effects (α_{qosm}) and with 19 birth cohorts $10 * 18 * 19 * 17 = 58140$ fixed effects (α_{qosm}), which makes it difficult to estimate in small samples and additional controls. To reduce the number of parameters in the estimation, I follow Chetty and Hendren (2018a) to parametrically control for two main factors of (α_{qosm}) fixed effects: (1) the quality of the origin location and (2) the disruption costs of moving. This leads to:

$$\begin{aligned} y_i = & \underbrace{\sum_{s=1980}^{1988} I(s_i = s)(\alpha_s^1 - \alpha_s^2 \bar{y}_{pos})}_{\text{origin quality}} + \underbrace{\sum_{m=1}^{17} I(m_i = m)(\zeta_m^1 + \zeta_m^2 p_i)}_{\text{disruption costs of moving}} \\ & + \underbrace{\sum_{m=1}^{17} b_m I(m_i = m)\Delta_{odps}}_{\text{exposure effects of interest}} + \underbrace{\sum_{s=1980}^{1988} \kappa_s^d I(s_i = s)\Delta_{odps}}_{\text{different measurement errors across birth cohorts}} + \varepsilon_{3i}, \end{aligned} \tag{2.7}$$

where the terms in the first sum control for origin quality by interacting the predicted outcomes of permanent residents in the origin area with birth cohort fixed effects by parent income percentile p_i. The terms in the second sum control for the disruption costs of moving. The disruption costs can vary with the age at move and the parental income level. The terms $\{\zeta_m^1\}$ control for different ages of children at the time of the move m. The terms $\{\zeta_m^2 p_i\}$ allow the disruption effects to vary with family income p_i. The third sum consists of the exposure effects of interest, and the in the fourth control for the different measurement errors across birth cohorts, as in equation (2.3). In the article of Chetty and Hendren (2018a), specifications (2.6) and (2.7) lead to very similar results, and therefore it is adequate to control parametrically for the quality of the origin by using permanent residents' outcomes. It implies that the outcomes of the movers can be modeled as a weighted average of the permanent residents' outcomes in the origin and destination using weights that correspond to the proportion of childhood spent in two places.

2.4.3 Critical Age Effects versus Exposure Effects

There are two possible mechanisms that could explain how areas affect the outcomes of the children: exposure effects that affect in proportion to the exposure time and critical age effects which imply different effects at different ages. In the critical age model, moving to a better area changes children's outcomes with declining probability, but once moved, the exposure time to a more affluent area would not matter for longer term outcomes. These two effects cannot be distinguished in a sample of onetime movers, because of perfect multicollinearity of child's age at move and his exposure time. One possibility to distinguish the two effects is to consider a sample of children who move to a better area,

and then move back to where they started. The exposure model predicts that the effect is proportional to the time spent in a new area, whereas according to the critical age model, the gains depend only on the age at which the child moves to the more affluent area. In their article, Chetty and Hendren (2018a) conclude that their results support the exposure effect model.

2.4.4 Available Data and Sample Definitions

I use Finnish Total Statistics on Income Distribution and Folk data from Statistic Finland. The data consist of a register of information from different sources. The outcome variables that I use are the income ranks at ages $28 - 33$ and the high school matriculation status at the age of 20 years. The data include every Finnish person, their municipality of residence, child-parent links, and personal identification codes for each person at each year between 1988 and 2018. The postal codes are available from 1993 and income data from 1995 onward.

I follow Chetty and Hendren (2018a) by focusing on variation across wide geographical areas to maximize statistical precision in the estimation. By doing so, I also alleviate the selection issues, because the selection effects are stronger within commuting zones (or regions) compared to between them. Part of this selection problem averages out when using larger geographical units for estimation and makes estimation based on a natural experiment more plausible.

I restrict the data to individuals born between 1980–1998, of whom there are data available for each of the years 1988–2018. I impose this requirement to exclude those children who have lived abroad during the analysis period and to restrict the analysis to those who are observed until the end of the year 2018. I further restrict the data to individuals who either live in one region the whole period of 1988–2018 (residents), or move between regions exactly once (one-time movers). Individuals who move between regions more than once are excluded from the analysis. In addition, I restrict the sample of movers to those children who move with their parents between regions. If a child has the same postal code as the parent, I deduce that they live with their parent. So, I define children moving with their parents as those children who have the same postal code as their parents before and after the move. For children who stay in the same region, I do not check if their postal codes match the postal codes of their parents.

The data I have are more reliable and cover a longer time span than in Chetty and Hendren (2018a), but have fewer observations. One major difference in the data quality is that the Finnish data covers all resident people in Finland, while Chetty and Hendren (2018a) only include those who' s parents file a tax return. If parents never file a tax return, they cannot be linked to the child and are excluded from the analysis. Approximately 94% of children can be linked to their parents. On the other hand,

fewer observations and smaller geographical variation in the Finnish data make my study empirically challenging compared to their study.

2.4.5 Variable Definitions and Summary Statistics

In this section, I define the variables that I use in the analysis. I convert all monetary variables into 2018 euros to adjust for inflation using the consumer price index (CPI). My key variables that I use for the estimation are:

Parental Income: For the measure of parental income, I calculate the total taxable income of the household during the years 1995-1999. Because I measure family income for a fixed set of years, the age of the child when family income is measured varies across birth cohorts. As in Chetty and Hendren (2018a) article, I account for this variation by controlling for the child's birth cohort in the analysis.

Parent Income Deciles: Based on the parental income measure, I rank all households at a nationwide level and classify them into ten equal-sized income deciles by the equivalised household income[6].

Income Ranks: I calculate the income ranks of the children based on the ordering in the national distribution in each year. Alternatively, I also rank children based on their income over the ages of 28–33 relative to other children in the same age range. The average income ranks by region are shown in Figure 2.1 (a).

High School Completion/Matriculation: By high school completion, I mean that a given individual has completed the Finnish high school matriculation examination by the age of 20 years. People who would matriculate at older ages are considered to have no high school degree in the analysis. The average matriculation rates by region are shown in Figure 2.1 (b).

As we can see from Table 2.1 in the Appendix, the average high school matriculation rates vary significantly between family income deciles. Only 25% of the children of the resident families in the lowest income decile have matriculated from high school, compared to 72% of the children of the families in the highest income decile. Matriculation rate of all children in resident families is 51%. Matriculation rates of the one-time movers are slightly higher, and rates of the children who move several times between regions are lower except in the highest quintile.

Age: The age of an individual is defined as the age measured in full years on the last day of the calendar year.

Location: In each year, all individuals are assigned municipality codes according to

[6]Household equivalent income is measured by OECD-modified scale. When calculating the size of the household, this scale assigns a value of 1 to the household head, of 0.5 to each additional adult member (over 13 years old) and of 0.3 to each child. The size of the household is measured at the last day of the year. The equivalence income is defined as the total net income after transfers and taxes of the household divided by the modified size of the household.

the municipality in which they live on the last day of the year. Based on this information, I classify all the individuals into 18 regions (excluding Ahvenanmaa region [7]) using Statistics Finland 2015 division.

As we can see in Figure 2.1 or Table 2.2 in the Appendix, the lowest high school matriculation rate (excluding Ahvenanmaa) is in Kainuu region, 42%. The highest rate, 59% is in Uusimaa region. I conclude that there is significant variation in high school matriculation rates across regions. Significant regional variation further supports the use of the high school matriculation rate as an outcome variable in the analysis. The average income ranks vary less between regions. For example, the average rank percentile at age 30 is 47 in Pohjois-Karjala and 53 in Uusimaa and Pohjanmaa.

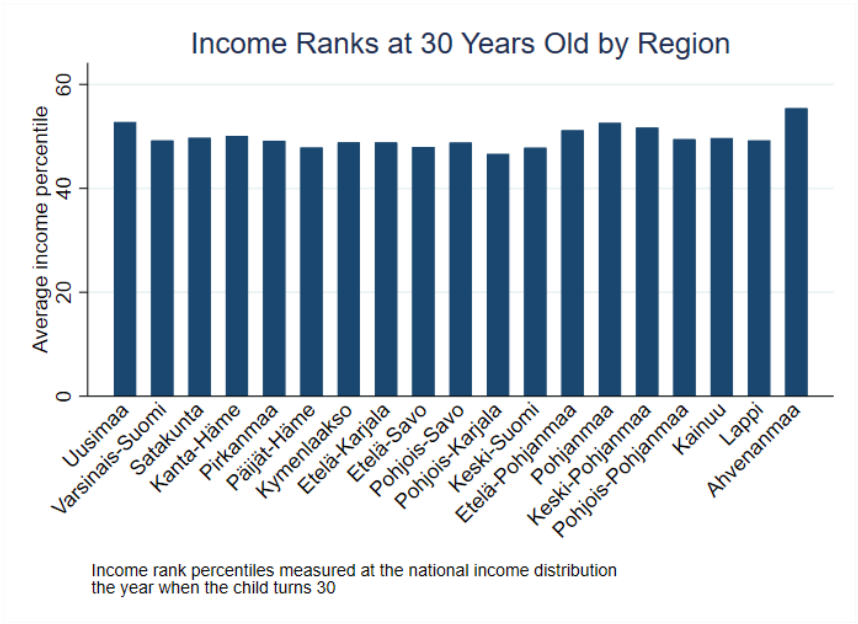
Figure 2.2 presents the rank-rank slope or how I would estimate $\bar{y}_p$ for all the children in my data. This figure plots the mean child rank at the age of 28-33 within each percentile bin of the parent income distribution, $E[y_i \mid p(i) = p]$, while parents' incomes are defined as total household taxable income from 1995 to 1999. The conditional expectation of a child's rank given his parents' rank is almost perfectly linear, except at the top 10 income percentiles, as also noted by Suoniemi (2017). Figure 2.3 illustrates the same for high-school matriculation rates. Panel (a) shows the results for the whole Finland, and panel (b) for the Uusimaa region. In the analysis, I run a similar regression in each region c controlling for the birth cohort s to obtain the fitted values $\bar{y}_{pcs} = \hat{\alpha}_{cs} + \hat{\psi}_{cs}p$.

2.4.6 Estimates of Exposure Effects

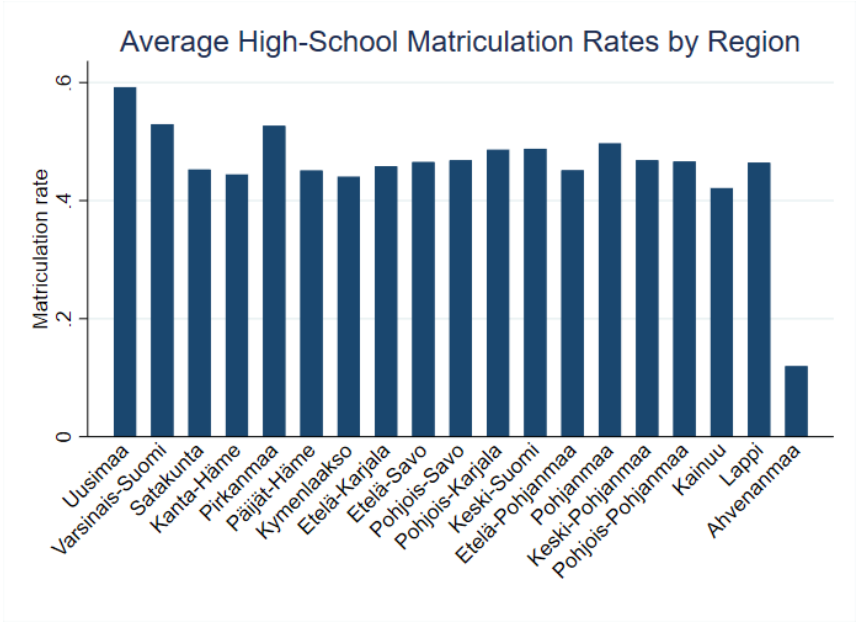
In this section, I present the parametric estimates of the exposure effects, $\{\gamma_m\}$, based on Equation (2.3) and Equation (2.7). Figure 2.4 displays a non-parametric binned scatter plot corresponding to the regression (2.3) for children who move at age $m = 13$ and Figure 2.5 does the same for high-school matriculation. As in Chetty and Hendren (2018a), I first demean both y_i and Δ_{odps} within the parent decile (q) by origin (o) by birth cohort (s) cells in the sample of movers at age $m = 13$ to construct residuals: $y_i^r = y_i - E[y_i \mid q, o, s]$ and $\Delta_{odps}^r = \Delta_{odps} - E[\Delta_{odps}|q, o, s]$. We then divide the residuals Δ_{odps}^r into twenty equal-size groups (ventiles) and plot the mean value of y_i^r vs. the mean value of Δ_{odps}^r in each bin.

Figure 2.4 shows that children who move to areas where children of permanent residents earn more at the age 28-33 earn more when they are 28-33. The relationship between y_i and Δ_{odps} is linear. The regression coefficient of $b_{13} = 0.564$, estimated in the microdata using (2.3), implies that a 1 percentile increase in $\bar{y}_{pds}$ is associated with a 0.564 percentile increase in y_i for children who move at age 13. The result is similar to the coefficient of 0.615 obtained by Chetty and Hendren (2018a), but much more

[7] Ahvenanmaa region looks very different from every other region for several reasons: it is mostly Swedish speaking autonomous area located in the sea between Finland and Sweden. Due to its different nature and small population (around 30,000), it is common to exclude in the analysis.



(a) Average income ranks in national income distribution at the age of 30



(b) Average high-school matriculation rates by region

Figure 2.1: Average outcomes of residents by region.

Notes: Author's calculations using Finnish Total Statistics on Income Distribution for years 1995-2018 and Folk data for years 1988-2018 from Statistic Finland. Residents are the children who live their entire childhood in the same region without moving between regions. High school matriculation is defined by having completed high school matriculation examination by the age of 20. Income ranks are measured at the age of 30 by ordering all individuals within the same birth cohort in the national income distribution.

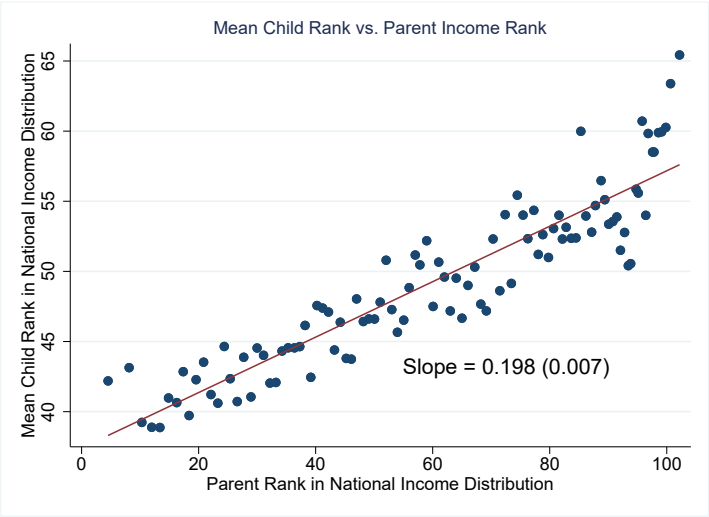
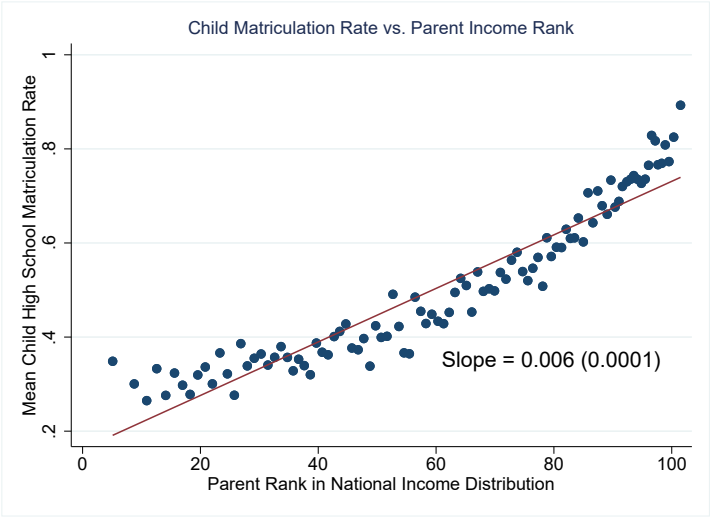


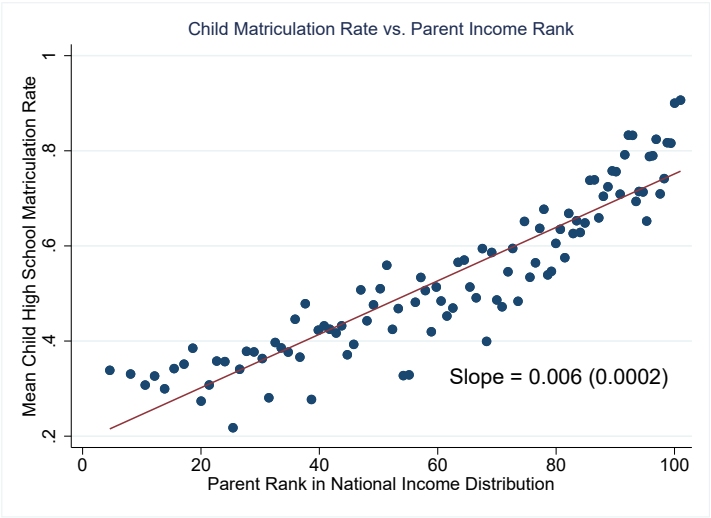
Figure 2.2: Mean Child Income Rank versus Parent Income Rank

This figure presents a binned scatter plot of the relationship between children's income ranks and parent income ranks for children raised in Finland. The points on the figure plot the mean rank of children within each parental income percentile bin. The best-fit line is estimated using an OLS regression on the underlying micro data. The figure also reports the slope of the best-fit line (the rank-rank slope), along with the standard error of the estimate (in parentheses). The sample includes all children in the 1980-1998 birth cohorts in the analysis sample whose parents were permanent residents in Finland during the sample period (1988–2018). Children's incomes are measured at the household (i.e., family) level at age 28-33; parents' incomes are defined as total household taxable income from 1995 to 1999. Children are assigned ranks based on their incomes relative to all other children in their birth cohort. Parents' are assigned ranks based on their incomes relative to other parents of children in the same birth cohort. (Chetty and Hendren, 2018a, p. 1124).

Compared to the corresponding Figure I by Chetty and Hendren (2018a) for Chicago, the slope is about a half of their figure (0.198 vs. 0.384) with greater standard errors (0.007 vs. 0.004). Even though I find a strong association between childrens' outcomes and parents' outcomes, the children's outcomes are significantly more correlated with parents' outcomes for children raised in Chicago compared to children raised in Finland.



(a) Children Born in Finland



(b) Children Born in the Uusimaa Region

Figure 2.3: Average High School Matriculation Rates by Parental Income Decile.
This figure presents a binned scatter plot depicting the relationship between the average high school matriculation rates of children by each parental income decile. The Figure (a) presents the relationship for all children born in Finland between 1988 and 1998. The Figure (b) presents the relationship for the same cohorts born in the Uusimaa region. The figure also reports the slope of the best-fit line, along with the standard error of the estimate (in parentheses).

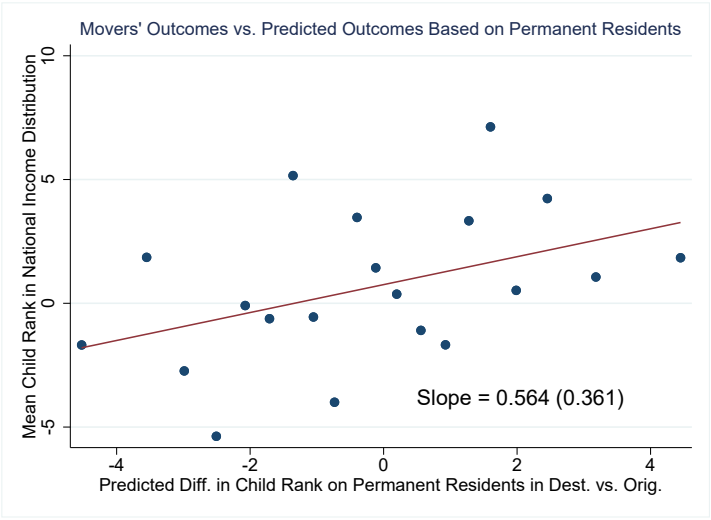


Figure 2.4: Movers' Outcomes vs. Predicted Outcomes Based on Permanent Residents in Destination: Income Ranks.

> This figure presents a binned scatter plot depicting the relationship between the income ranks of children who moved to a different region at age 13 and the differences in the outcomes of permanent residents in the destination versus origin region. The sample includes all children in the 1980–1998 birth cohorts whose parents moved when the child was 13 years old and moved only once between 1988 and 2018. Children's family income ranks y_i are measured at age 28-33. Permanent residents' predicted ranks for each parent income percentile p, region c, and birth cohort s ($\bar{y}_{pcs}$) are constructed using the methodology described in the section 4.2. To construct the figure, I demean both y_i and $\Delta_{odps} = \bar{y}_{pds} - \bar{y}_{ops}$ within the parent decile (q) by origin (o) by birth cohort (s) cells in the sample of movers at age $m = 13$ to construct residuals: $y_r^i = y_i - E[y_i|q, o, s]$ and $\Delta_{odps}^r = E[\Delta_{odps}|q, o, s]$. I then divide the Δ_{odps}^r residuals into 20 equal-size groups (ventiles) and plot the mean value of y_i^r versus the mean value of Δ_{odps}^r in each bin. The slope of the best-fit line, which corresponds to b_{13} in equation (2.6), is estimated using an OLS regression on the underlying microdata, with standard errors in parentheses. (Chetty and Hendren, 2018a, p. 1131).

Compared to the corresponding Figure III by Chetty and Hendren (2018a), this figure has significantly more variation than their figure. They obtain a slightly steeper slope (0.615 vs. 0.564) with significantly more precision (standard error 0.025 vs. 0.361). This already suggests that moving to a different area in Finland might be relatively less significant factor for the childrens' outcomes than in the US.

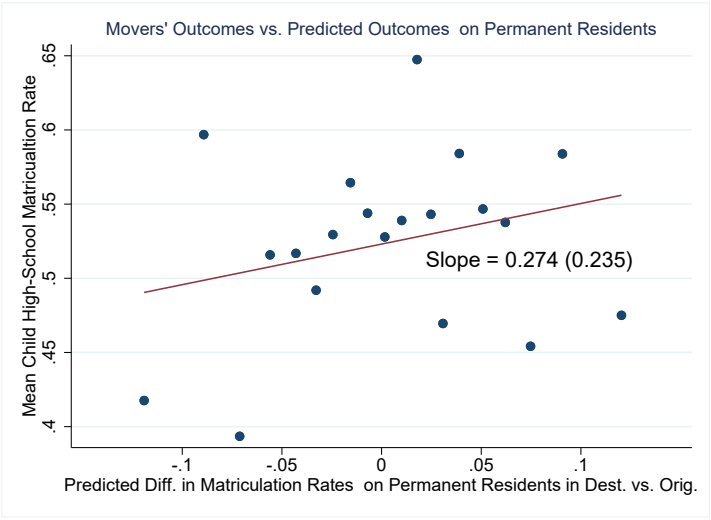


Figure 2.5: Movers' Outcomes vs. Predicted Outcomes Based on Permanent Residents in Destination: High School Matriculation.

> This figure presents a binned scatter plot depicting the relationship between the average high-school matriculation rates of children who moved to a different region at age 13 and the differences in the outcomes of permanent residents in the destination versus origin region. The sample includes all children in the 1980–1998 birth cohorts whose parents moved when the child was 13 years old and moved only once between 1988 and 2018. Children's family income ranks y_i are measured at age 28-33. Permanent residents' predicted matriculation rates for each parent income percentile p, region c, and birth cohort s ($\bar{y}_{pcs}$) are constructed using the methodology described in the section 4.2. To construct the figure, I demean $\Delta_{odps} = \bar{y}_{pds} - \bar{y}_{ops}$ within the parent decile (q) by origin (o) by birth cohort (s) cells in the sample of movers at age $m = 13$ to construct residuals: $\Delta^r_{odps} = E[\Delta_{odps}|q, o, s]$. I then divide the Δ^r_{odps} residuals into 20 equal-size groups (ventiles) and plot the mean value of y_i versus the mean value of Δ^r_{odps} in each bin. The slope of the best-fit line, which corresponds to b_{13} in equation (2.6), is estimated using an OLS regression on the underlying microdata, with standard errors in parentheses. (Chetty and Hendren, 2018a, p. 1131).

Compared to the corresponding Figure III of income ranks by Chetty and Hendren (2018a), this figure has significantly more variation than their figure. They obtain a much steeper slope (0.615vs. 0.274) with significantly more precision (standard error 0.025 vs. 0.235). This already suggests that moving to a different area in Finland might be relatively less significant factor for the childrens' outcomes than in the US.

imprecise due to the smaller sample size and the lower variation in outcomes between areas. The corresponding coefficient on high-school matriculation at the age of 20 is 0.274 in Figure 2.5.

Figure 2.6 illustrates the relationship between age at move and adult income rank by plotting the estimated coefficients $\{b_m\}$ from Equation (2.7) against the age at which the move occurred. I estimate a slope of -0.066 with a standard error of 0.022. Compared to the corresponding figure in Chetty and Hendren (2018a), the estimated slope is steeper (-0.066 versus -0.038) but substantially less precise (0.022 versus 0.003). Despite the statistical significance of the overall slope, approximately half of the individual coefficients $\{b_m\}$ estimated using Equation (2.7) are not statistically distinguishable from zero. The downward slope indicates that moves occurring at older ages are associated with smaller gains in adult income rank, consistent with the presence of childhood exposure effects.

Taken together, the figures provide consistent evidence that neighborhood exposure effects on adult income rank are strongest earlier in childhood and decline with age. Despite substantial uncertainty surrounding individual age-specific estimates, the overall pattern supports the interpretation that childhood environment plays a causal role in shaping long-term economic outcomes.

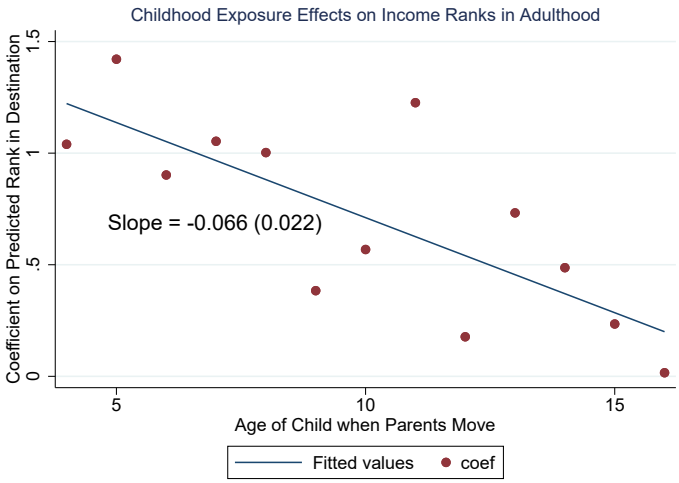


Figure 2.6: Effect of moving on the income rank of the child at age 28-33.

This figure plots estimates of the coefficients $\{b_m\}$ versus the child's age when the parents move (m) using the semiparametric specification in equation (2.7), measuring children's incomes at age 28-33. The sample includes all children in the primary analysis sample whose parents moved exactly once between 1988 and 2018. The $\{b_m\}$ coefficients can be interpreted as the effect of moving to an area where permanent resident outcomes are 1 percentile higher at age m. They are estimated by regressing the child's income rank in adulthood y_i on $\Delta_{odps} = \bar{y}_{pds} - \bar{y}_{pos}$, the difference between permanent residents' predicted ranks in the destination versus the origin, interacted with each age of the child at the time of the move m. I include indicators for the child's age at the time of the move interacted with parent income rank and predicted outcomes for permanent residents in the origin interacted with birth cohort fixed effects. Best-fit lines are estimated using unweighted OLS regressions of the $\{b_m\}$ coefficients on m. The slope of the regression line is reported along with standard error (in parentheses). The magnitude of the slope represent the estimate of annual childhood exposure effects. The identification assumption underlying the analysis is that the selection effect δ does not vary with the child's age at move m. (Chetty and Hendren, 2018a, p. 1133).

Compared to the figure obtained by Chetty and Hendren (2018a), I obtain a steeper slope (-0.066 vs. -0.038) with less precision (0.022 vs. 0.003). Even though my slope is statistically significant, about a half of the individual coefficients $\{b_m\}$ estimated using Equation (2.7) are statistically insignificant.

2.4.7 High-School Matriculation Rates

In their article, Chetty and Hendren (2018a) focus on outcomes measured by income, but also consider other outcomes beyond income: college attendance, marriage, teenage birth, and teenage employment. They find that the effect on college attendance is similar to the effect on income (3.7% per year of exposure), and the effect on marriage is slightly smaller (2.5% per year of exposure). The effect on adolescent births is particularly large between the ages of 13 and 18. However, the effect on teenage employment is discontinuous just before the age when employment is measured. Moving to an area with higher teenage employment earlier increases the probability of being employed, but the exposure effect is relatively small compared to the jump. This may suggest that exposure effects consist of different experiences at different points in childhood — such as available summer jobs at certain areas — that may aggregate to produce the linear exposure effects. (Chetty and Hendren, 2018a)

I find opposite effects on high school matriculation than Chetty and Hendren (2018a) on college attendance (or any outcome). The coefficient is 0.104 with a standard error of 0.017 and statistically significant at 1% significance level. Individual coefficients are significant only from the age of 13 onward. The later a child moves to an area with a higher average high school matriculation rate, the more likely the child is to matriculate from high school themselves (Figure 2.7). This can reflect discouragement effects or confounding factors in the regression. It is possible that families move because they want to give the child the opportunity to attend high school. The effect of high school matriculation on income could be different in Finland compared to the US. According to Silliman and Virtanen (2022), completing the vocational track instead of the general track in high school increases earnings around the admission threshold.

2.4.8 Sibling comparisons

To assess the validity of the key identification assumption that the potential outcomes of children who move between regions do not vary with the age at which they move, I do the same analysis as above comparing siblings within the same families with different age at the time of the move. Following Chetty and Hendren (2018a), I partition the unobserved determinant of θ_i into a part that is fixed within families $\bar{\theta}_i$ and into a residual component $\tilde{\theta}_i = \theta_i - \bar{\theta}_i$ by including family fixed effects in the regression:

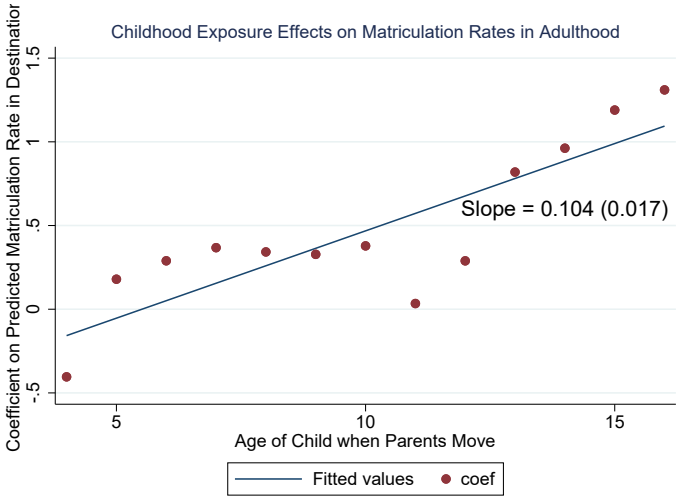


Figure 2.7: Effect of moving on the probability of matriculating from high-school.

This figure plots estimates of the coefficients $\{b_m\}$ versus the child's age when the parents move (m) using the semiparametric specification in equation (2.7), measuring children's high-school matriculation status at the age of 20. The sample includes all children in the primary analysis sample whose parents moved exactly once between 1988 and 2018. The $\{b_m\}$ coefficients can be interpreted as the effect of moving to an area where permanent resident outcomes are 1 percentile higher at age m. They are estimated by regressing the child's income rank in adulthood y_i on $\Delta_{odps} = \bar{y}_{pds} - \bar{y}_{pos}$, the difference between permanent residents' predicted ranks in the destination versus the origin, interacted with each age of the child at the time of the move m. I include indicators for the child's age at the time of the move interacted with parent income rank and predicted outcomes for permanent residents in the origin interacted with birth cohort fixed effects. Best-fit lines are estimated using unweighted OLS regressions of the $\{b_m\}$ coefficients on m. The slope of the regression line is reported along with standard error (in parentheses). The magnitude of the slope represent the estimate of annual childhood exposure effects. The identification assumption underlying the analysis is that the selection effect δ does not vary with the child's age at move m. (Chetty and Hendren, 2018a, p. 1133).

Compared to the results of Chetty and Hendren (2018a), I obtain an upward-sloping instead of a downward-sloping line. However, the coefficients $\{b_m\}$ are statistically significant only for $m \geq 13$ and they are likely to represent selection effects. One major difference between the outcomes is that I use the Finnish high-school matriculation examination, which would be similar to graduate from an academic track in high school in the US. Their outcome would be similar to complete any secondary degree in Finland.

$$
\begin{aligned}
y_i = & \sum_{s=1980}^{1985} I(s_i = s)(\alpha_s^1 - \alpha_s^2 \bar{y}_{pos}) + \sum_{m=2}^{17} I(m_i = m)(\zeta_m^1 + \zeta_m^2 p_i) \\
& + \sum_{m=2}^{17} b_m I(m_i = m)\Delta_{odps} + \sum_{s=1980}^{1985} \kappa_s^d I(s_i = s)\Delta_{odps} \\
& + \sum_{f=1}^{N_f} \bar{\theta}_f \, \mathbb{1}\{i \in \text{family } f\} + \varepsilon_{4i},
\end{aligned} \tag{2.8}
$$

where N_f is the number of families in the data. Then, "the exposure effect γ is identified by the extent to which the difference in siblings' outcomes, $y_1 - y_2$, covaries with the difference in their ages interacted with the change in permanent residents' outcomes, $(m_1 - m_2)\Delta_{odps}$" (Chetty and Hendren, 2018a, p. 24).

In Figure 2.8, we see a slightly upward sloping line with a statistically insignificant slope of 0.042 regressed on the coefficients $\{b_m\}$ when outcome rank is measured at ages 28-33. In addition, all individual coefficients are statistically insignificant. However, the estimation becomes so imprecise that I cannot exclude economically significant effects and also the baseline estimates are well within any confidence intervals constructed around the estimates.

For high-school matriculation, we still see an upward sloping line with a statistically significant slope of 0.104 in Figure 2.9. Although the estimated slope of the regression of the coefficients $\{b_m\}$ on the age at move m is statistically significant, all individual coefficients $\{b_m\}$ coming from regression (2.8) are statistically insignificant. Any confidence intervals would again be large and include the baseline estimates. Figure 2.10 shows the relationship between parental background and the age of the child at the time of the move (without controls). The figure shows that the family background is correlated with the age at move, which could imply a violation of the identification assumption.

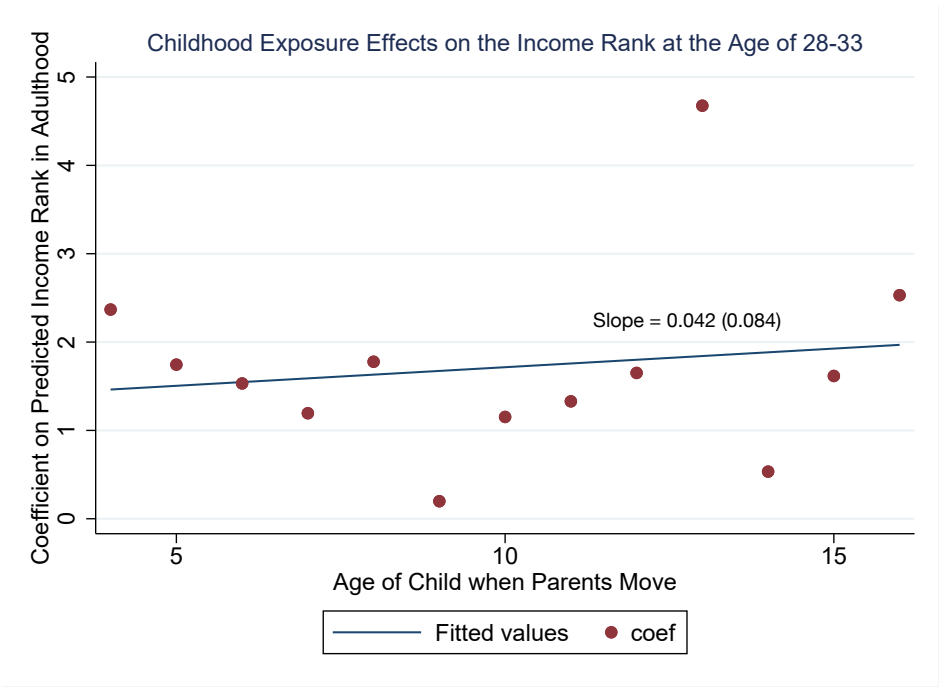


Figure 2.8: Effect of moving on the child income rank at age 28-33, sibling comparison (family fixed effects).

This figure replicates Figure 2.6, using specification of equation (2.8) that includes family fixed effects. The slope and all of the individual coefficients are statistically insignificant, suggesting that the previous result was driven by the family unobservable characteristics varying with the age of the child at the time of the move. However, the baseline results remain within the confidence intervals due to the reduced power of the test.

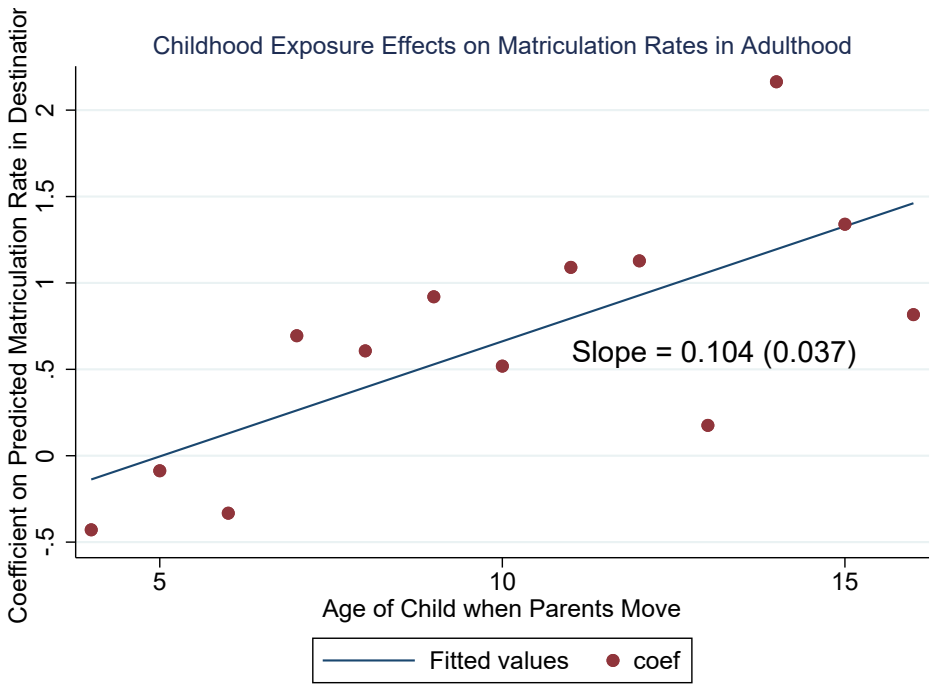


Figure 2.9: Effect of moving on the probability of matriculating from high-school, sibling comparison (family fixed effects).

This figure replicates Figure 2.7, using specification of Equation (2.8) that includes family fixed effects. Even though the slope estimated on the coefficients $\{b_m\}$ on m is statistically significant, all of the individual coefficients $\{b_m\}$ coming from regression (2.8) are statistically insignificant.

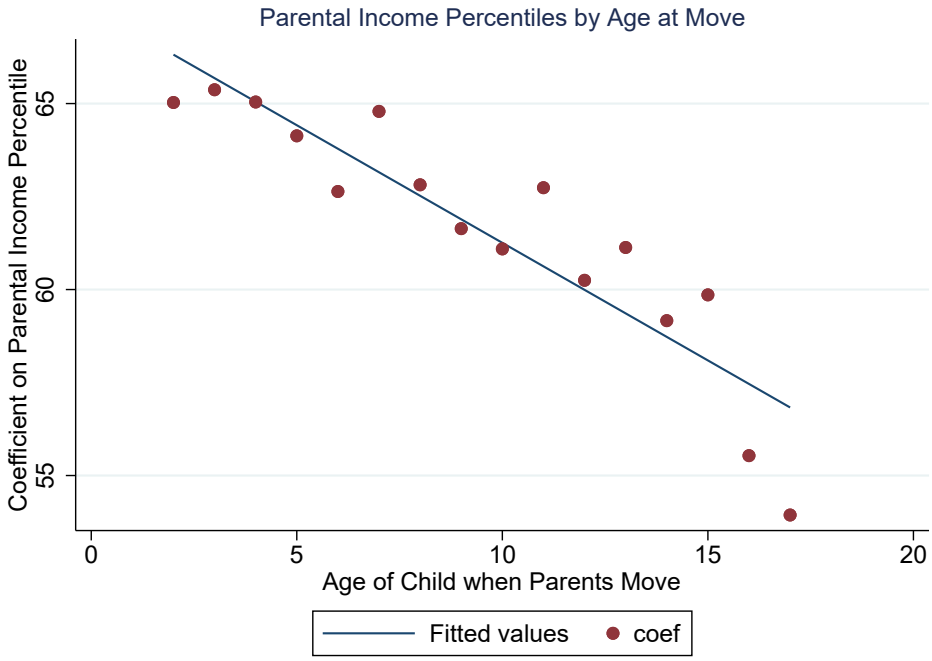


Figure 2.10: The average parental income percentile by the age at move.

This figure shows the relationship of the parental background and age of the child at the time of the move. Even though the regression controls for parental income percentile, this figure shows that the family background is correlated with the age at move, being in accordance with a possible violation of the identification assumption that the selection bias is constant with all ages of moving.

2.5 Conclusions

My results remain inconclusive. On the other hand, I find significant childhood exposure effects on income ranks later in life and significant effects on the probability of matriculating from high-school. The sign of the effects on high-school matriculation is opposite compared to any outcome in the study of Chetty and Hendren (2018a). Children moving to a higher (lower) outcomes producing area at older ages have higher (lower) outcomes than children moving at younger ages. This can reflect either confounding factors that are not fixed within families or discouragement effects. However, the results become statistically insignificant with family fixed effects, but also imprecise and underpowered because of the small sample size and small geographical variation in outcomes. My results replicate the findings of Chetty and Hendren (2018a) in the context of a different society from a Nordic Welfare state. My findings are also in accordance with the results of Maarseveen (2020) that the children's cognitive ability declines with age of move, which is also noted by Van Maarseveen (2021).

2.6 APPENDIX

Table 2.1: High School Matriculation Rates of Permanent Residents by Region and Income Decile

	1	2	3	4	5	6	7	8	9	10
Etelä-Karjala	.1919	.1896	.2296	.2422	.2575	.3092	.3584	.3794	.4316	.5063
Etelä-Pohjanmaa	.1982	.2098	.2396	.2785	.3150	.3586	.4175	.4253	.4388	.55823
Etelä-Savo	.2145	.1997	.2465	.2998	.3023	.3486	.3521	.3930	.45	.4556
Kainuu	.1560	.1850	.2113	.2668	.2952	.3041	.3719	.3784	.4450	.5079
Kanta-Häme	.1892	.1896	.2062	.2523	.2752	.3022	.3441	.3871	.4629	.5110
Keski-Pohjanmaa	.2176	.2375	.2602	.2947	.3306	.3733	.4036	.4036	.4533	.4068
Keski-Suomi	.2356	.2307	.2656	.2953	.3496	.3796	.4236	.4763	.5241	.5722
Kymenlaakso	.1656	.1697	.2142	.2167	.2610	.2836	.2953	.3137	.3675	.4459
Lappi	.2247	.2185	.2454	.2823	.3356	.3623	.3938	.4239	.4363	.5838
Pirkanmaa	.2774	.2695	.3036	.3425	.3642	.4124	.4574	.5218	.6024	.6755
Pohjanmaa	.2512	.2350	.2710	.3418	.3630	.4008	.4340	.4621	.5106	.6094
Pohjois-Karjala	.2344	.2576	.2991	.3345	.3819	.4259	.4939	.5190	.5477	.5108
Pohjois-Pohjanmaa	.2236	.2454	.2820	.3304	.3872	.4379	.4934	.5622	.6196	.6842
Pohjois-Savo	.2146	.2089	.2602	.3026	.3361	.3550	.4136	.45	.4851	.5312
Päijät-Häme	.1926	.1911	.2358	.2667	.3038	.3282	.3791	.4223	.4474	.5217
Satakunta	.2026	.2291	.2545	.2854	.3169	.3366	.3979	.4701	.4992	.5273
Uusimaa	.3678	.3453	.3574	.3972	.4494	.4886	.5411	.6057	.6896	.7872
Varsinais-Suomi	.2965	.2936	.3159	.3511	.3884	.4254	.4851	.5367	.6175	.7071
Total	.2516	.2541	.2880	.3289	.3713	.4116	.4664	.5275	.6121	.7223
Observations	51591	68783	76001	75469	71891	67453	60133	51020	41319	36724

Table 2.2: Average outcome percentiles of residents by region

Region	high-school	rank at 28	rank at 29	rank at 30	rank at 31	rank at 32	rank at 33	rank at 28-33
1 Uusimaa	59	48	51	53	55	56	58	60
2 Varsinais-Suomi	53	45	47	49	51	52	54	57
4 Satakunta	45	46	48	50	51	52	54	58
5 Kanta-Häme	44	47	49	50	52	53	54	58
6 Pirkanmaa	53	45	47	49	51	52	54	58
7 Päijät-Häme	45	44	46	48	50	51	53	56
8 Kymenlaakso	39	46	47	49	50	52	53	56
9 Etelä-Karjala	44	45	47	49	51	52	54	56
10 Etelä-Savo	46	45	46	48	49	51	52	56
11 Pohjois-Savo	47	45	47	49	50	52	53	56
12 Pohjois-Karjala	49	43	45	47	48	50	51	55
13 Keski-Suomi	49	44	46	48	49	51	53	56
14 Etelä-Pohjanmaa	45	48	50	51	53	54	55	60
15 Pohjanmaa	50	50	51	53	54	55	57	60
16 Keski-Pohjanmaa	47	49	50	52	53	54	56	60
17 Pohjois-Pohjanmaa	47	46	48	49	51	53	54	58
18 Kainuu	42	46	48	50	51	53	54	57
19 Lappi	46	46	47	49	51	52	54	57
21 Ahvenanmaa	11	54	57	55	58	59	66	45

Chapter 3

Is Redistribution Fair? The Effect of Taxes and Transfers on Equality of Opportunity[1]

Markus Jäntti[2], **Arttu Kauhanen**[3], **Jukka Pirttilä**[4]

Abstract

A common criticism against redistribution is that it focuses on the inequality of outcomes and that the sources of income differences are neglected. Indeed, income differences may also reflect differences in effort, and theories of Equality of Opportunity postulate that such differences should not be equalized by society. This paper examines how redistribution, via tax-benefit policies, influences equality of opportunity in European countries. The results indicate that redistribution of outcomes also improves opportunities by reducing inequitable income differences. But, from the point of view of equality of opportunity, redistribution may go too far in many European countries. This is corroborated by the finding that the extent of redistribution required to neutralize the effect of circumstances on income is smaller than the actual redistribution in most, but not all, countries.

Keywords: Redistribution, Equality of Opportunity, Inequality, Taxation, EU-SILC.

[1]Financing by the Research Council of Finland (Grant nr:s 340197 and 364190) is gratefully acknowledged. This paper is based on data from Eurostat, EU Statistics on Income and Living Conditions [2010, 2018]. The responsibility for all conclusions drawn from the data lies entirely with the authors.We thank participants at the VATT Seminar for helpful comments.

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3.1 Introduction

Economic analysis of redistribution has traditionally emphasized equality of outcomes, typically measured by differences between market and disposable incomes. Although this perspective is informative, it neglects an important normative dimension: the origin of inequality. The The framework of Equality of Opportunity (EOp) offers an alternative lens, distinguishing between income differences arising from circumstances beyond individual control, such as parental background, and those resulting from individual effort, such as labor supply decisions. From an ethical standpoint, inequalities due to circumstances are considered inequitable, whereas those due to effort are regarded as equitable. This distinction implies that redistribution aimed at correcting inequitable differences is more defensible than redistribution targeting effort-based disparities.

Currently, there is an expanding empirical research on inequality of opportunity (IEOp) for various countries. There are several ways in which inequality of opportunity can be operationalized and studied, but in much of the empirical literature, the approach is roughly the following. The researcher picks a set of observable *circumstances* that can be argued to be beyond an individual's control (typically including but not necessarily limited to parental socio-economic background); a set of specific circumstances, such as low parental education and parental working-class background, define an individual's *type*. The assumption is that income differences *within* a type reflect different levels of individual effort[5], whereas differences *between* types reflect inequality due to circumstances. The approach typically used in the literature is then based on comparing overall inequality – which is due to both within-type variation in effort and between-type differences in income levels – with the level of inequality that would pertain if all persons of a given type had equal incomes, i.e., if differences due to variations in effort were eliminated within each type. Since individuals are responsible for their own effort, but not their type, between-type inequality is typically interpreted as inequitable inequality.

However, this literature has largely overlooked the role of taxes and transfers in shaping IEOp. Understanding how redistribution interacts with inequality of opportunity is essential to evaluate the fairness of fiscal systems and to design policies that align with responsibility-sensitive principles of justice. This paper addresses this gap by systematically analyzing the relationship between redistribution and IEOp in Europe. Using harmonized data from the European Union's Survey of Income and Living Conditions (EU-SILC) for 2010 and 2018, which include detailed information on parental background, we compare IEOp based on market incomes with IEOp based on disposable incomes. Our analysis covers more than 30 European countries and provides both country-level and aggregate estimates for Europe as a whole.

[5]In practice, it may also reflect unobserved circumstances, measurement error, luck, and other factors that are not attributable to individual responsibility.

Our research questions are the following. We first ask to what extent actual redistribution reduces inequality of opportunity across European countries. This is achieved by quantifying the change in IEOp when moving from market to disposable incomes. Second, we investigate what share of observed redistribution can be considered equitable, in the sense of targeting inequitable inequality. In doing so, we assess whether fiscal systems disproportionately correct income differences due to circumstances. Third, we evaluate how much redistribution would be required if policy focused exclusively on eliminating inequitable inequality. This counterfactual exercise provides insights into the potential scope of fairness-oriented redistribution. In addition, we also explore how redistribution patterns and their impact on IEOp have evolved between 2010 and 2018? By comparing two periods, we explore whether trends in fiscal policy have strengthened or weakened fairness objectives. And finally, we discuss which characteristics of tax- and benefit policies are associated with contributing to greater equitable redistribution in a country comparison.

Our work builds on a rich philosophical, theoretical, and empirical literature on EOp ((Roemer and Trannoy, 2016; Ferreira and Peragine, 2016) and complements earlier studies that have explored the relationship between redistribution and fairness ((Roemer et al., 2003; Almås et al., 2011; Dunnzlaff et al., 2011; Checchi et al., 2016). This literature is discussed in more detail in the following section. We contribute in several ways to this literature. First, we provide a systematic and up-to-date analysis covering multiple countries and periods. Second, we assess how large the actual redistribution in different countries is in comparison to the extent of redistribution which would only rectify the role of circumstances for incomes. Third, we also examine equality of opportunity in Europe as a whole, treating the country of residence as one of the circumstances. Finally, we discuss how redistributive policies have changed over time.

The results of the analysis indicate that the between-group component, capturing IEOp, is moderate in comparison with total inequality in European countries. Between-group inequality is greater when the Gini index is used to measure inequality, whereas it is smaller when using measures which give a higher weight to income differences in the tails of the distribution, such as the mean log deviation or (one half of) the squared coefficient of variation (i.e., the generalised entropy index GE (2)). Redistribution reduces not only total inequality, but also inequitable inequality. Hence, redistribution of outcomes also leads to greater equality of opportunity. However, in most countries, most of redistribution targets differences arising from efforts. This result is compatible with another finding of ours, namely that often – but not in all countries – the extent of actual redistribution in the countries in our sample is greater than a volume of redistribution which would be needed to rectify inequitable inequality. This is the case especially if the initial share of inequitable inequality is low (perhaps due to predistribution policies) or the overall extent of redistribution is substantial. We also find that both overall inequality

and inequitable inequality declined between 2010 and 2018.

The paper is organized as follows. Section 3.2 discusses the earlier work and how our analysis is linked with it. Section 3.3 introduces the data used in the analysis and it also presents our analytical methods. Results are presented in Section 3.4. Section 3.5 concludes.

3.2 Literature Review

The philosophical ideas underpinning Equality of Opportunity (EOp) were developed independently by Roemer (1998) and Fleurbaey (2008), among others. In the EOp framework, an ethically relevant outcome y (such as income) depends on the circumstances of individuals (c), for which individuals are not held responsible, and effort (e), for which they are. A given combination of particular circumstances – assumed for simplicity here to be discrete – defines a type t. The outcome is also affected by policies φ, such as education provision or progressive taxation. According to (Roemer and Trannoy, 2016, p. 7), one could summarize the goal of Equality of Opportunity policies as "to find that policy which nullifies, to the greatest extent possible, the effect of circumstances on outcomes, but still allows outcomes to be sensitive to effort."

More precisely, the outcome can be written as a type-specific function of effort and policy, $y = v^t(e^t(\pi), \varphi)$, where $e^t(\pi)$ is the effort of the π:th quantile / person of type t. A policy that targeted equality of opportunity would, according to Roemer and Trannoy (2015), focus on maximizing the outcome for the most disadvantaged type (or more generally, the lower envelope of outcomes across types).

An important building block in the EOp analysis is the Principle of Compensation, according to which society should compensate people for differences in outcomes that are due to factors that are beyond their control. According to this principle, individuals with the same level of effort should receive the same income. A distinction can be made between the *ex ante* and *ex post* compensation. The former refers to compensation carried out prior to the determination of effort, whereas the latter is related to compensation aimed at equalizing outcomes for individuals exerting the same amount of effort.

Another principle, developed especially in Fleurbaey and Maniquet (2011), is the Reward Principle, which requires societies to respect rewards to individual effort, and not to compensate outcome differences that are due to different effort levels. The Reward Principle is called Utilitarian if no compensation is carried out in a society where all members have the same circumstances. Liberal (or natural) Reward seeks, in turn, to minimize redistribution related to differential effort levels, suggesting that transfers to individuals who have the same circumstances should be the equal ((Fleurbaey and Maniquet, 2011)). The authors also note that, in general, the compensation principle and the natural reward principle may be incompatible. That is why they suggest weakening

one or both principles. For instance, in the case of egalitarian equivalence, circumstances are fully compensated for such that transfers are set in line with the reward principle for a certain reference type, such as the most disadvantaged type. This forms the theoretical foundation for one part of our analysis below, which compares actual redistribution to (one of the) theoretical ideal(s) in which transfers are equal across types and fully compensate differences between types (making type-specific mean equal overall mean income).[6]

The earlier empirical findings related to EOp are collected and discussed by Roemer and Trannoy (2016) and Ferreira and Peragine (2016). In empirical analysis, most of the work has proceeded using the decomposition into within- and between-type inequality, where the types are constructed based on circumstances. This approach, as noted by Ferreira and Peragine (2016), is consistent with the ex ante view of compensation, because inequality between types (evaluated as the inequality between the means of each type) is preserved. An alternative, utilized by, for example, Almås et al. (2011)[7], is to focus on inequality among those who exert the same amount of effort. Often, the availability of data dictates the feasibility of approaches. The EU-SILC data, for instance, provide reasonably good proxies of circumstances, but effort is not really observable in the data, as is arguably the case for most data sets.

Only a few earlier papers explicitly deal with redistribution in the context of an Equality of Opportunity analysis. Roemer et al. (2003) develop a theoretical optimal tax model, which seeks to maximize the well-being of the group with the worst circumstances. They then fit actual tax rates for several economies, regressing disposable income on market incomes, and compare these tax rates with the theoretical ideal. Their results suggest that certain countries redistribute too much. In particular, in Nordic countries, there is very little need for redistribution given the low extent of IEOp in the pre-fiscal situation. Dunnzlaff et al. (2011) use EU-SILC data for 2005 to calculate how much redistribution (also distinguishing between taxes and benefits) affects one particular measure of EOp, the Gini Opportunity index. They find that the opportunity-equalizing effects are the largest in the Nordic and Continental European countries, whereas redistribution only reduces IEOp little in the Anglo-Saxon countries. Almås et al. (2011) study the implications of responsibility-sensitive theories of justice for the evaluation of the pre- and post-tax income distribution in Norway in the period 1986-2005. They find that the Norwegian tax system has become weaker in reducing inequality of opportunity. Overall inequality and inequality of opportunity have therefore not moved in the same direction. Finally, using a similar approach (where population groups are constructed using proxies of responsibility tranches), Figueiredo and da Fonseca Silva (2014) provide

[6] As noted by Kanbur (2019), such transfers may lead to large monetary flows across groups, which may exceed the technical capacity to redistribute or become politically problematic in divided societies.

[7] They assume in their main analysis that people are held responsible for the hours worked, years of education, sector (public versus private) and county of residence.

estimates about the role of redistribution in several Latin American countries.[8]

The EOp approach is not without its critics, of course. Kanbur and Wagstaff (2016) note that one issue is that the empirical analysis using the within-types (formed by sets of circumstances) tend to lead to quite low levels of inequality of opportunity, which could be simply because circumstances are not comprehensively captured with the data at hand. For instance, Björklund et al. (2012) show that IQ is a very important characteristic, likely to belong to circumstances, which is often missing in data. Hence, estimates often represent lower bounds of the actual extent of IEOp.

One criticism is related to the situation of those below the poverty line. Should these individuals be left on their own, even if a case can be made that their situation partly reflects their own choices? Hufe et al. (2022) therefore suggest a modified, or hybrid, approach, where unfair inequality is defined as the sum of the extent of poverty and the variation of inequality due to circumstances for those above the poverty line.

3.3 Data and Methods

3.3.1 Conceptual Framework

In this study, we gauge the extent of inequality of opportunity (IEOp) by comparing overall inequality to the level of inequality that would pertain if every person within a type had the same level of income, the within-type mean. For additively decomposable inequality indices (the Generalised Entropy [GE] class), which are often used in related research, this is akin to decomposing the inequality index into between- ("inequitable") and within-group ("equitable") components that sum up to total inequality (Juárez and Soloaga, 2014). The groups are the types that represent specific sets of circumstances beyond individual control, such as family background and biological sex. Hence, the between-group inequality component can be seen as the inequitable part of the inequality. We use the term inequitable inequality and between-group inequality interchangeably going forward, and this is how Inequality of Opportunity is measured in the study, as in much of the previous work.

3.3.2 Empirical Estimation

First, we regress the outcome variable y on the circumstance variables C to estimate the conditional expectation $\hat{y} = \widehat{E(y \mid C)}$. This estimate for a given circumstance group (type) is equal to the mean income within the group. For example, if the only

[8]In addition, Decoster et al. (1997) provide an early analysis focused on the role of indirect taxes. They divide households into groups based on their needs (such as consumption related to dealing with illnesses) versus tastes to investigate the horizontal fairness of commodity taxation. This can be seen to be closely related to EOp, since households are not responsible for their needs in their analysis.

Table 3.1: The analytical framework

	Total ineq.	Between ineq.	Between share
Market income	$I_{M,T} = I(y_M)$	$I_{M,B} = I(\hat{y}_M)$	$\frac{I(\hat{y}_M)}{I(y_M)}$
Disposable income	$I_{D,T} = I(y_D)$	$I_{D,B} = I(\hat{y}_D)$	$\frac{I(\hat{y}_D)}{I(y_D)}$
Redistribution	R_T	R_B	$\frac{R_B}{R_T}$
Redist, in %	$\frac{R_T}{I_{M,T}}$	$\frac{R_B}{I_{M,B}}$	

circumstance variable were biological sex, the groups would consist of men and women, and the conditional expectations would correspond to the average income of men and women, respectively.

Second, we calculate inequality of opportunity by using an inequality measure I on $\hat{y}$ to obtain $I_B = I(\hat{y})$. In the example of using biological sex as the only circumstance variable, we would assign each individual the average income of men or women, depending on their sex, and use that in the calculations of $I(\hat{y})$. Since the variation in $\hat{y}$ is exclusively due to (observed) circumstances (the biological sex of the person), I_B is interpreted as (the lower bound of) inequality of opportunity. We apply the same inequality index I to the actual outcome y to obtain a measure of total inequality $I(y)$. That means calculating the inequality index using each individual's actual income in the calculations.

We repeat this exercise for market income y_M, taxable income after transfers y_T, and disposable income after taxes and transfers y_D to obtain the respective inequality measures $I(y_M)$ and $I(\hat{y}_M)$ for market income, $I(y_T)$ and $I(\hat{y}_T)$ for taxable income, and $I(y_D)$ and $I(\hat{y}_D)$ for disposable income. The extent of redistribution R_T can be calculated as the difference $R_T = I(y_M) - I(y_D)$. The redistribution affecting between-type inequality is similarly the difference calculated on the inequality measures applied to the conditional expectations $\hat{y}$: $R_B = I(\hat{y}_M) - I(\hat{y}_D)$. Finally, in order to obtain the share of redistribution that affects between-types inequality, we can simply divide these two terms: $\frac{R_B}{R_T}$. These different concepts and their relations are illustrated in Table 3.1.

We measure inequality using a variety of indices, including the Gini index, the mean log deviation (MLD), and the generalized entropy GE(2) index. Among these, GE(2) emphasizes differences at the top of the income distribution, whereas MLD is more sensitive to changes at the bottom (Jenkins and van Kerm, 2011). While a conventional decomposition of the Gini index into within- and between-group components is not available, one can still compute the Gini index based on group-average incomes. This approach produces an "inequitable inequality" measure that depends solely on differences in group means.

3.3.3 Data

We use data from the EU Statistics on Income and Living Conditions (EU-SILC), which covers 31 EU countries and provides detailed income variables for 2004–2021. Using these data, we calculate market income and disposable income (market income less taxes plus public transfers) for each individual. For the years 2010 and 2018[9], we use the detailed circumstance variables that SILC collected in those years to compute the share of inequality attributable to factors beyond individual control. These circumstances include

1. country of birth
2. respondent's parents' highest education level (father: 2010 & 2018; mother: 2010)
3. parental occupation
4. number of siblings
5. household financial situation at age 14 (coded as moderately bad, bad, very bad, or unknown),
6. household tenancy status at age 14
7. parental presence in the household at age 14.

In preparing the analysis data, we follow quite closely the steps taken in Hufe et al. (2022). One example is that for those with missing labour income but positive working hours, we impute labour income using a country-specific regression of log labour income on squared working hours, squared age, education, migration status, biological sex, and year dummies. This procedure replaces 16,396 missing observations out of 402,034 households with observed positive labour income.

The analysis is conducted at the individual level using household-level income, with incomes made comparable across different kinds of households using the OECD equivalence scale. We restrict the sample to individuals aged 30–59: for 2010, this corresponds to birth years 1952–1981, and for 2018, 1960–1989. After applying these restrictions and retaining observations with complete background variables, the sample includes 249,000 individuals in 2010 and 236,960 in 2018.

In addition to the EU-SILC, we use macro-level data from OECD and EUROSTAT to calculate real PPP price levels and other country-level variables used in Section 3.4.5 to examine the determinants of redistribution.

[9]We refer to the years when income was earned; the EU-SILC of year $t+1$ contains incomes in year t (so our data are from the SILCs of 2011 and 2019).

3.4 Results

3.4.1 Country by Country Results

We first present country-level results for 2018, with Europe-wide results and changes over time discussed in subsequent subsections[10].

Figure 3.1 shows the overall Gini index for market income as well as the "between" Gini, the Gini that would apply if all within a type had the same income, the group mean; the countries are ordered by the level of overall inequality. In most countries, the level of inequality taking into account only type-specific means (between-group inequality) is moderate, typically around 0.1, while the overall Gini index lies mostly between 0.3 and 0.4. However, there are exceptions, such as Bulgaria, where the Gini using group means reaches approximately 0.27.

The corresponding results for the GE(0) and GE(2) indices are presented in the Appendix (Figures A.22 and A.23). For these measures, an even smaller share of total inequality is due to differences between-groups. This pattern is expected, because group mean incomes are generally closer to the overall mean than individual incomes. Indices sensitive to the tails of the distribution capture most variation within groups rather than between them.

For these reasons, we focus primarily on the Gini index results in the main text, both because of its interpretability and because its levels are more familiar to most readers.

The reduction in inequality due to taxes and transfers is illustrated in Figure 3.2 (countries are here ordered by the reduction in between-inequality). In countries with the most redistributive tax-benefit systems, the Gini index for overall inequality (R_T in Table 3.1) is reduced by more than 0.1. In contrast, the absolute reduction in between-group inequality (R_B in Table 3.1) is typically smaller, around 0.02 to 0.03. Although overall redistribution is substantial in the Nordic countries, the largest reductions in between-group inequality occur in Belgium, Ireland, Portugal, and Austria. Total redistribution is high in Belgium and Ireland, but lower in Portugal and Austria, indicating that country rankings differ when based on reductions in between-group inequality.

The ratio of the reduction in between-inequality relative to reduction in overall-inequality (R_B/R_T in Table 3.1) is shown in Figure 3.3. Except for three countries – Romania, Bulgaria and Luxembourg – this share is below 20%, with a typical country around 10%. As with absolute reductions, the countries with the largest relative share of redistribution affecting inequitable inequality differ from those with the highest total redistribution.

Figure 3.4 decomposes the absolute reduction in between-group inequality into

[10] Analysis using the Gini decomposition by Heikkuri and Schief (2025) can be found in the Appendix.

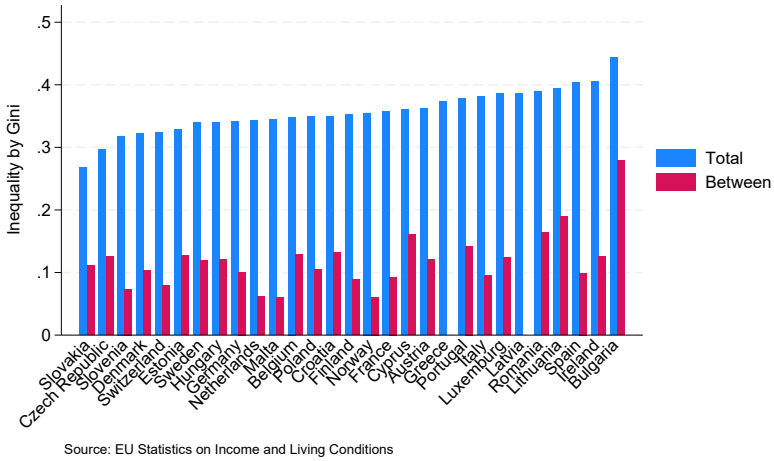


Figure 3.1: Overall and between-group Gini index of market incomes, 2018

Notes: Authors' calculations using the EU Statistics on Income and Living Conditions (EU-SILC) for the year 2018. The circumstance variables used to form the groups are the country of birth, farther's highest education level, parental occupation, number of siblings, household financial situation at age 14, household tenancy status at age 14 and parental presence in the household at age 14. The total Gini coefficient is calculated using individual disposable income. The between component is the Gini coefficient calculated using imputed group-specific average incomes.

components due to taxes and transfers. In most countries, transfers account for the larger share of the reduction, although taxes are equally important in a few cases. In particular, in Hungary and Bulgaria, the tax system slightly increases between-group inequality.

Another relative measure, the share of between-type redistribution relative to inequitable inequality in market incomes ($R_B/I_{M,B}$ in Table 3.1), captures how large a part of inequitable inequality is redistributed (see Figure 3.5). In the graph, this is compared with how large a part of total inequality is redistributed. The results suggest that typically a slightly larger share of between-types inequality is redistributed than of total inequality.

The corresponding results when using the MLD as an inequality measure are presented in the Appendix, Figures A.24 - A.27[11]. They indicate that the absolute reduction in inequitable inequality is small and that most of it stems from the operation of the benefit system. Also, the share of between-group redistribution out of total redistribution is smaller than when using the Gini index, in most cases below 20%. As discussed earlier, these observations are likely due to the fact that between-groups share of before-taxes-and-transfers inequality is small when the MLD is used.

[11]The results for GE(2) are available on request.

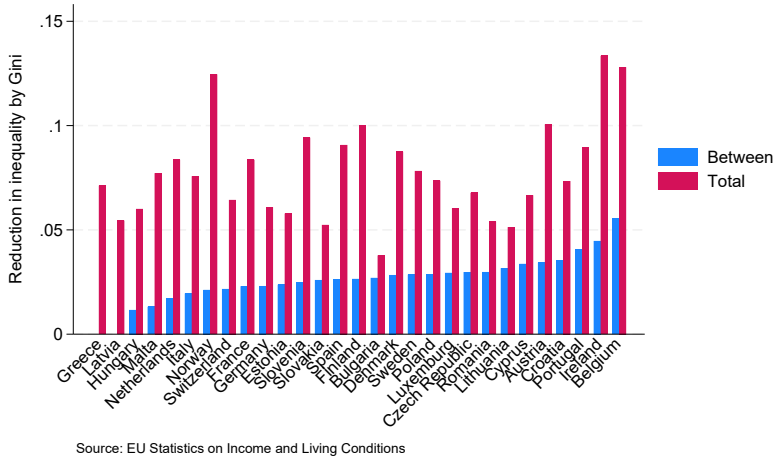


Figure 3.2: Reduction in overall and between-groups Gini because of redistribution, 2018

Notes: Authors' calculations using the EU Statistics on Income and Living Conditions (EU-SILC) for the year 2018. The circumstance variables used to form the groups are the country of birth, farther's highest education level, parental occupation, number of siblings, household financial situation at age 14, household tenancy status at age 14 and parental presence in the household at age 14. The total Gini coefficient is calculated using individual market and disposable income. The between component is the Gini coefficient calculated using imputed group-specific average incomes. The reduction is obtained as the difference between market and disposable income inequalities.

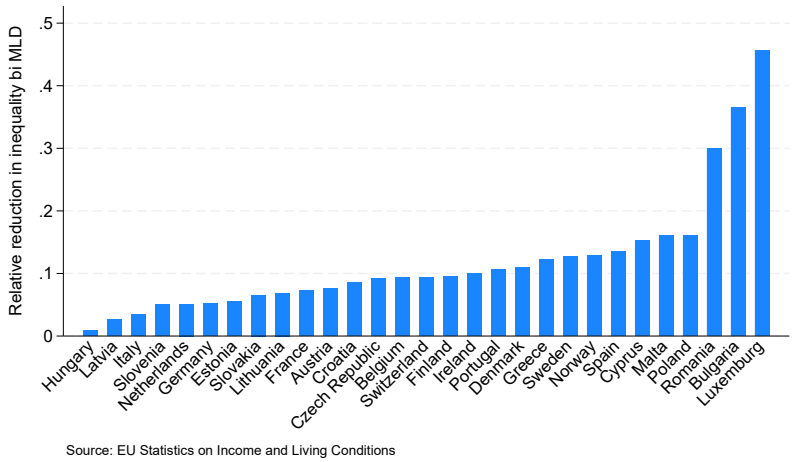


Figure 3.3: Reduction in between-groups Gini relative to the overall reduction in Gini because of redistribution, 2018

Notes: Authors' calculations using the EU Statistics on Income and Living Conditions (EU-SILC) for the year 2018. The circumstance variables used to form the groups are the country of birth, farther's highest education level, parental occupation, number of siblings, household financial situation at age 14, household tenancy status at age 14 and parental presence in the household at age 14. The total Gini coefficient is calculated using individual market and disposable income. The between component is the Gini coefficient calculated using imputed group-specific average incomes. The reduction is obtained as the difference between market and disposable income inequalities. Finally, the reduction in between groups Gini is divided by the reduction in market income Gini.

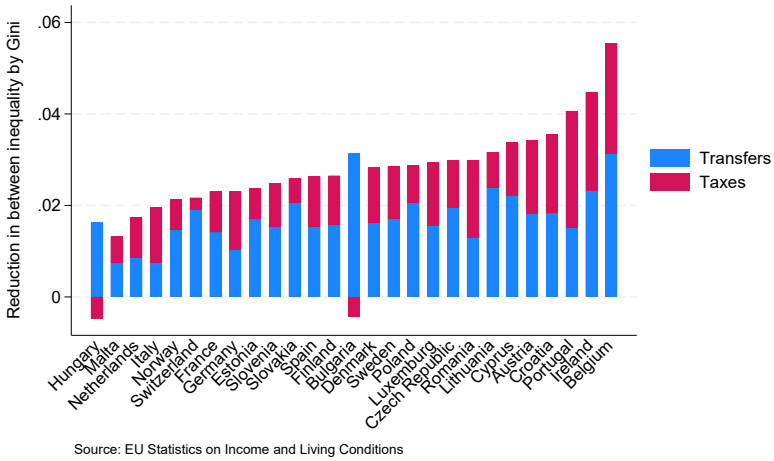


Figure 3.4: Reduction in between-type Gini: the role of taxes vs transfers, 2018

Notes: Authors' calculations using the EU Statistics on Income and Living Conditions (EU-SILC) for the year 2018. The circumstance variables used to form the groups are the country of birth, farther's highest education level, parental occupation, number of siblings, household financial situation at age 14, household tenancy status at age 14 and parental presence in the household at age 14. The total Gini coefficient is calculated using individual market and disposable income. The between component is the Gini coefficient calculated using imputed group-specific average incomes. The reduction is obtained as the difference between market and disposable income inequalities divided by the market income inequality.

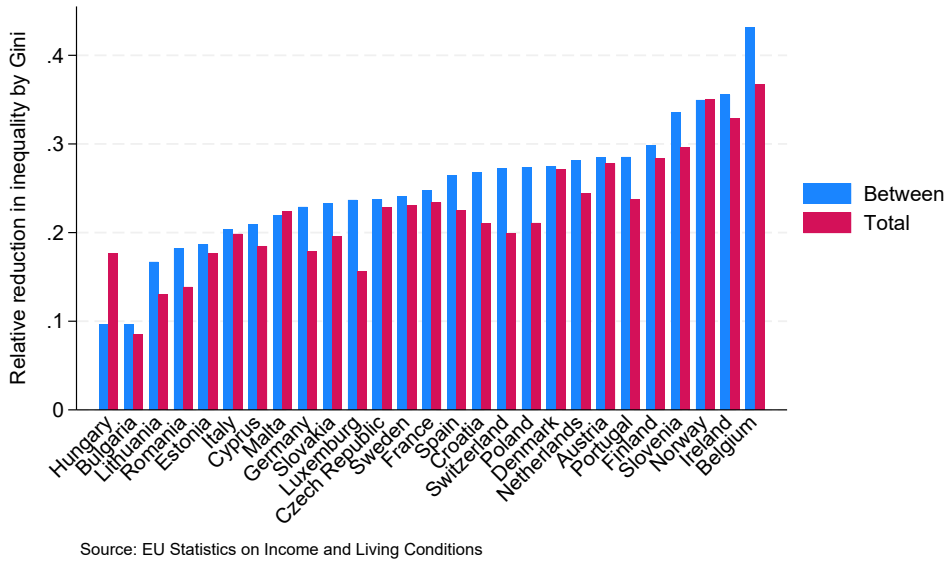
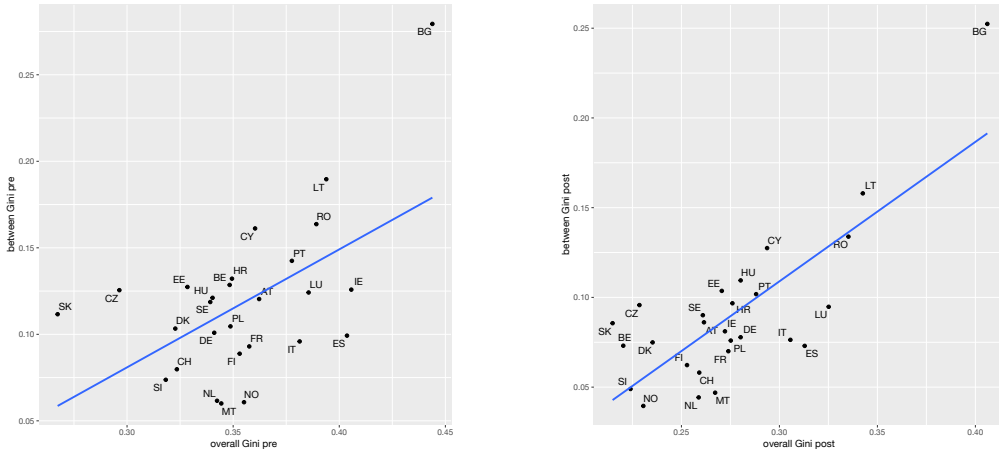


Figure 3.5: Reduction in between-group and total inequality due to taxes and transfers, relative to their respective market-income inequalities.

Notes: Authors' calculations using the EU Statistics on Income and Living Conditions (EU-SILC) for the year 2018. The circumstance variables used to form the groups are the country of birth, farther's highest education level, parental occupation, number of siblings, household financial situation at age 14, household tenancy status at age 14 and parental presence in the household at age 14. The total Gini coefficient is calculated using individual market and disposable income. The between component is the Gini coefficient calculated using imputed group-specific average incomes. The reduction is obtained as the difference between market and disposable income inequalities divided by the market income inequality.

To better understand how the distribution of disposable income is affected by redistribution and what the linkages between redistribution and inequality are, we present some further analyzes. Figure 3.6 first documents that when the overall inequality is at a high level, the inequitable inequality is also large and that the pattern remains even after the operation of the tax and transfer systems. This is in line with the finding illustrated in Figure 3.7, that between-group disposable income inequality is reduced by almost a constant amount relative to the between-group component in market income inequality. In overall inequality, there is more dispersion in the mapping between the market and disposable income distributions. Furthermore, these results imply that the disposable income distribution exhibits more between-group inequality when the overall inequality is at a high level (Figure 3.8).



(a) Market income inequality

(b) Disposable income inequality

Figure 3.6: Relation between overall and between-group inequality in market and disposable income

Notes: Authors' calculations using the EU Statistics on Income and Living Conditions (EU-SILC) for the year 2018. The circumstance variables used to form the groups are the country of birth, farther's highest education level, parental occupation, number of siblings, household financial situation at age 14, household tenancy status at age 14 and parental presence in the household at age 14. The total Gini coefficient is calculated using individual market and disposable income. The between component is the Gini coefficient calculated using imputed group-specific average incomes.

Figure 3.9, in turn, highlights how redistribution and market-income inequality are linked. While there appears to be no association between overall redistribution and market inequality, a positive relation exists between inequitable market-income inequality and how redistribution alleviates it. Thus, if the between-group component in inequality is large to begin with, redistribution has more scope to lower it. We will come back to the determinants of how features of the tax-benefit system and country characteristics are related to the reduction in inequitable inequality in Section 3.4.5.

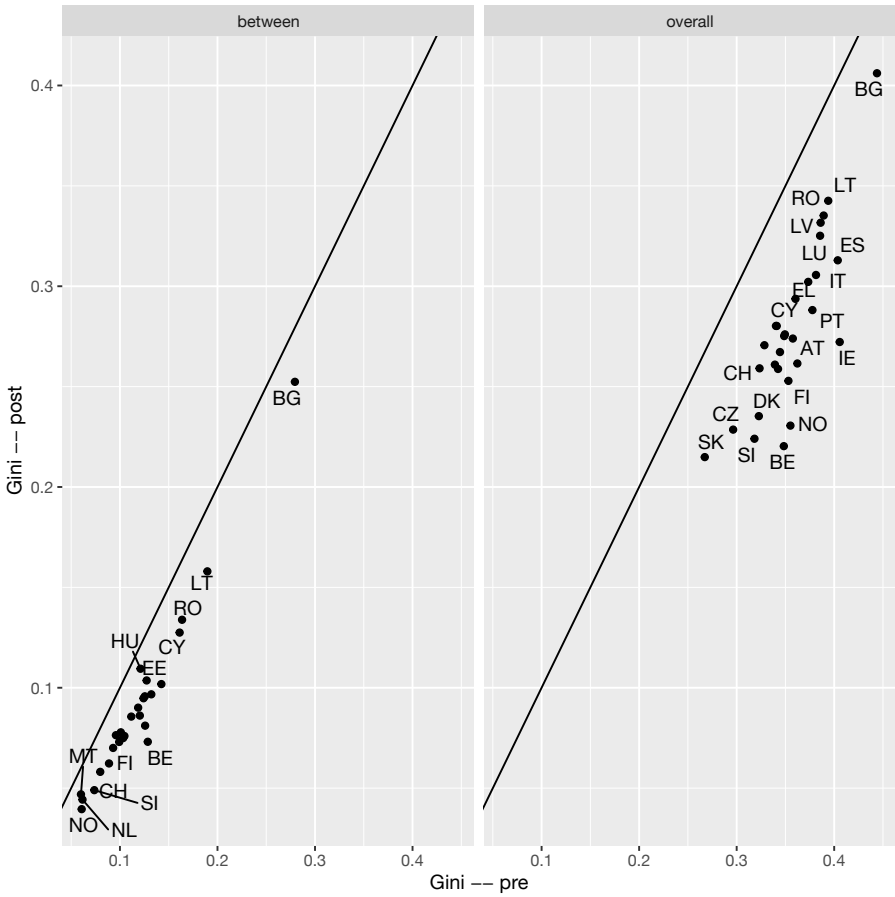


Figure 3.7: Inequality before and after redistribution. Between groups (left) and overall (right).

Notes: Authors' calculations using the EU Statistics on Income and Living Conditions (EU-SILC) for the year 2018. The circumstance variables used to form the groups are the country of birth, farther's highest education level, parental occupation, number of siblings, household financial situation at age 14, household tenancy status at age 14 and parental presence in the household at age 14. The total Gini coefficient is calculated using individual market and disposable income. The between component is the Gini coefficient calculated using imputed group-specific average incomes.

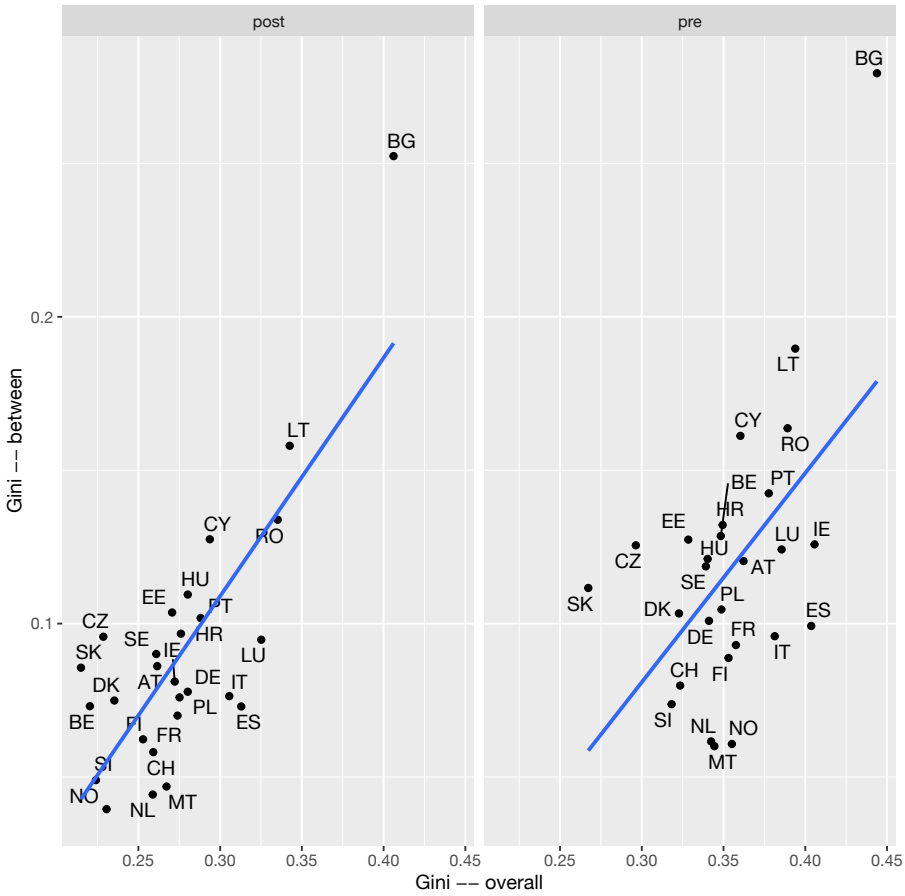


Figure 3.8: Contrasting overall and between-group inequality. Disposable income (left) and market income (right).

Notes: Authors' calculations using the EU Statistics on Income and Living Conditions (EU-SILC) for the year 2018. The circumstance variables used to form the groups are the country of birth, farther's highest education level, parental occupation, number of siblings, household financial situation at age 14, household tenancy status at age 14 and parental presence in the household at age 14. The total Gini coefficient is calculated using individual market and disposable income. The between component is the Gini coefficient calculated using imputed group-specific average incomes.

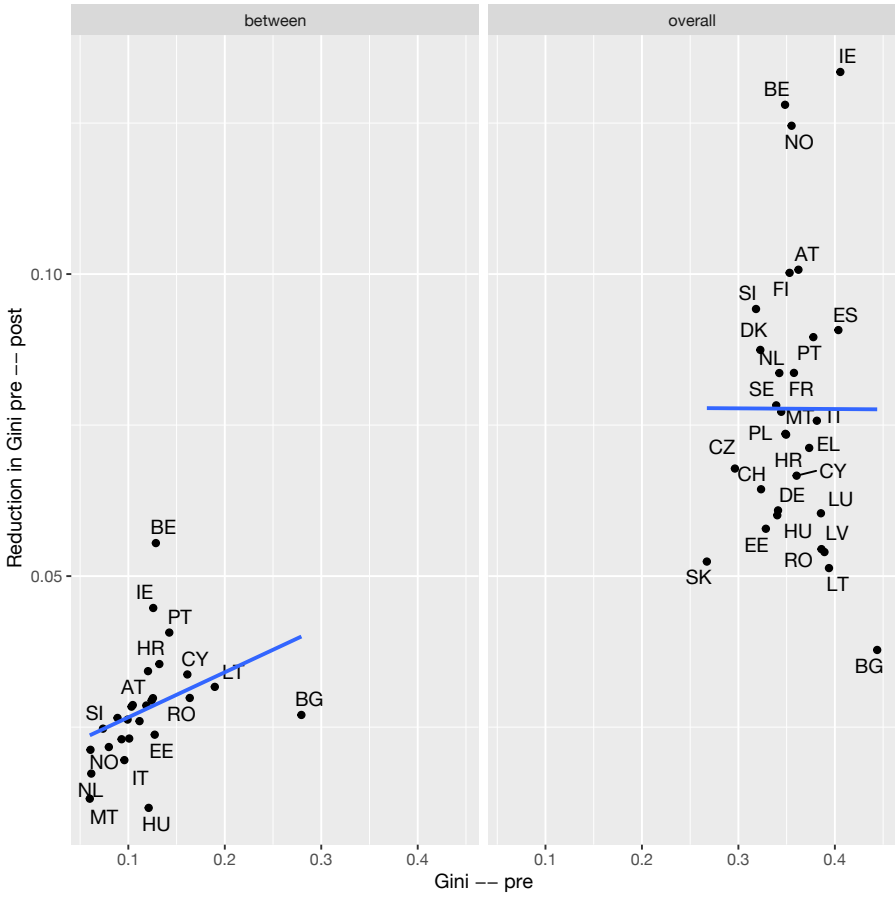


Figure 3.9: Contrasting redistribution and market-income inequality. Between groups (left) and overall (right).

Notes: Authors' calculations using the EU Statistics on Income and Living Conditions (EU-SILC) for the year 2018. The circumstance variables used to form the groups are the country of birth, farther's highest education level, parental occupation, number of siblings, household financial situation at age 14, household tenancy status at age 14 and parental presence in the household at age 14. The total Gini coefficient is calculated using individual market and disposable income. The between component is the Gini coefficient calculated using imputed group-specific average incomes. The reduction is calculated as a difference between market and disposable income inequalities for the individual incomes and group-specific averages.

3.4.2 Changes between 2010 and 2018

We have also conducted our baseline analysis for the year 2010, and in what follows, we describe how the situation has changed between the two time points. Figure 3.10 shows the change in overall and between-group inequality between 2010 and 2018. In most countries, both types of inequality have declined, but there are countries with quite substantial increases, including Bulgaria, Cyprus and also Sweden. The redistributive impact of tax-benefit policies has also declined in most countries (Figure 3.11). The change in the relative share redistribution targeting at between-group inequality ($\Delta\frac{R_B}{R_T}$, with Δ denoting the change over time) suggests that this ratio has typically declined over time, although there are also ten countries with a positive change.

The qualitative picture is similar when MLD is used as an inequality measure (Figures A.28 - A.30), although the relative change in the share of redistribution affecting between-groups inequality is more positive.

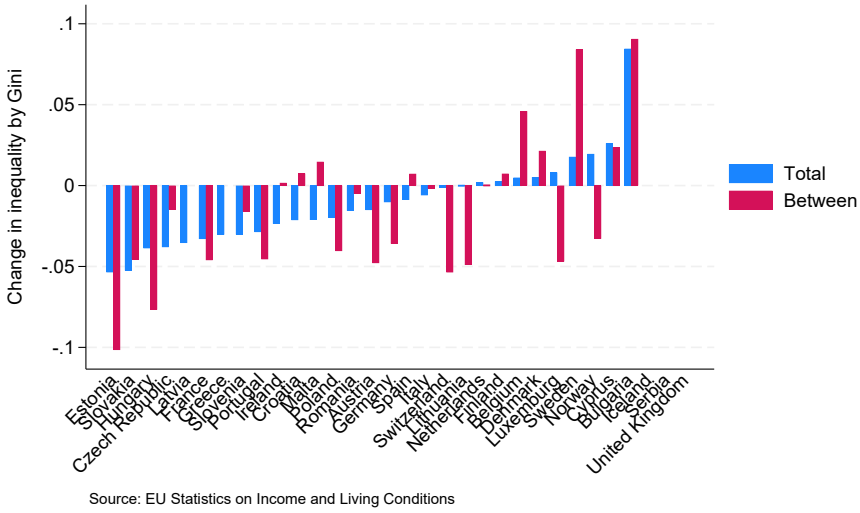


Figure 3.10: Change in inequality of market income by country

Notes: Authors' calculations using the EU Statistics on Income and Living Conditions (EU-SILC) for the years 2010 and 2018. The circumstance variables used to form the groups are the country of birth, farther's highest education level, parental occupation, number of siblings, household financial situation at age 14, household tenancy status at age 14 and parental presence in the household at age 14. The total Gini coefficient is calculated using individual market income. The between component is the Gini coefficient calculated using imputed group-specific average incomes. The change is calculated as a difference between the inequalities in 2018 and 2010.

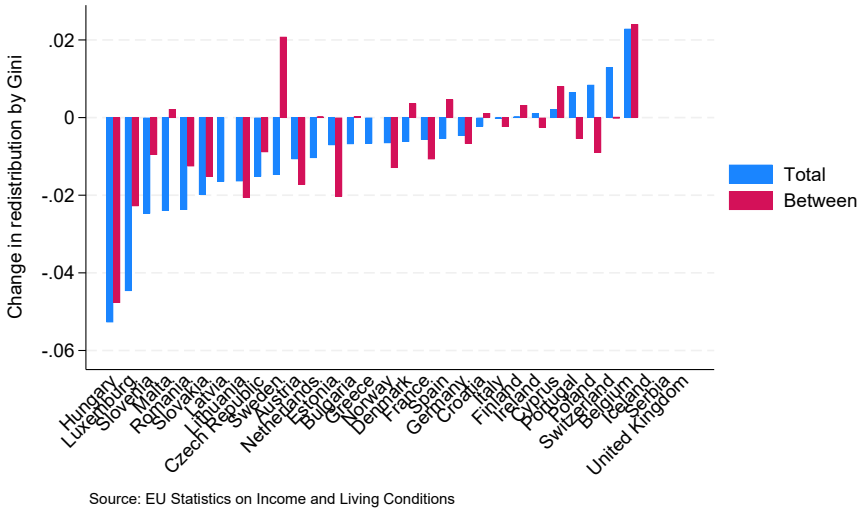


Figure 3.11: Change in redistribution by country

Notes: Authors' calculations using the EU Statistics on Income and Living Conditions (EU-SILC) for the years 2010 and 2018. The circumstance variables used to form the groups are the country of birth, farther's highest education level, parental occupation, number of siblings, household financial situation at age 14, household tenancy status at age 14 and parental presence in the household at age 14. The total Gini coefficient is calculated using individual market income. The between component is the Gini coefficient calculated using imputed group-specific average incomes. The redistribution is calculated as a difference between market and disposable income inequalities for the individual incomes and group-specific averages. The change is calculated as a difference between the redistribution in 2018 and 2010.

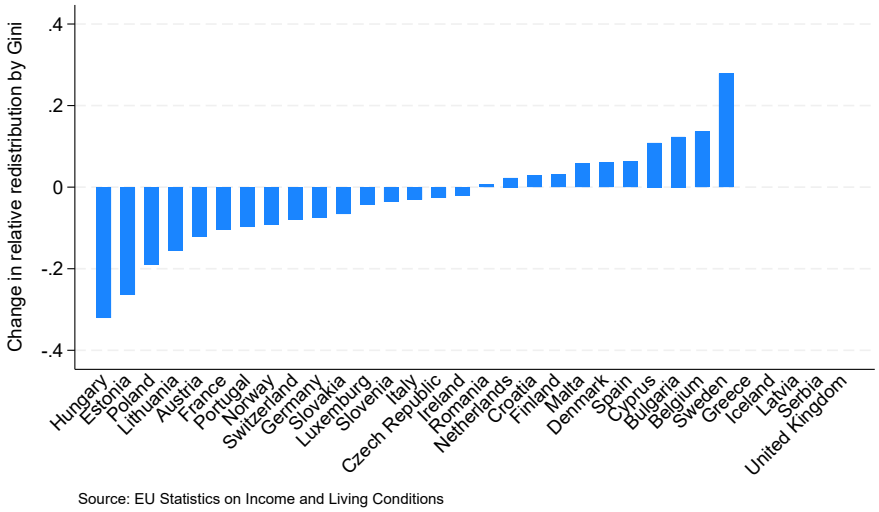


Figure 3.12: Change in between-group redistribution relative to the change in total redistribution by country

Notes: Authors' calculations based on the EU Statistics on Income and Living Conditions (EU-SILC), 2010 and 2018. The figure displays the change in redistribution of the between component relative to the change in redistribution of the total component. The change of redistribution is defined as in the previous figure.

3.4.3 Removing Between-group Inequality

One key idea in Equality of Opportunity theories is that the income differences that do not arise from differences in effort should be compensated for. In fact, one of the features of the approach suggested by Fleurbaey and Maniquet (2011) is that transfers should be uniform within a type (or a group) when types are formed based on circumstances.

We follow this idea and calculate type-specific lump-sum transfers, which bring each type's mean income to the level of the mean income of the entire society. We then compute how large redistributive flows such a transfer would represent and compare them with actual redistribution. In other words, we examine how much market income inequality is reduced when transfers are set to equalize mean-incomes of groups. This represents the desirable degree of redistribution, viewed from the EOp angle.

These results are presented in Figure 3.13. It shows that actual redistribution exceeds the extent of redistribution that would be needed to remove differences across groups, but there are cases (including many Eastern European countries but also Luxembourg and Ireland) where the required redistribution would be greater than the current redistribution achieved by the actual tax-benefit policies.

One could also arrive at a distribution where the mean-incomes of the groups would be equalized by finding a coefficient which is used to multiply the incomes of all group

members. In such a case, the relative income differences in a group are retained, but the transfers to different group members are not the same within the group. The reduction in the Gini coefficient that would follow from such a redistribution is illustrated in Figure 3.14. The overall picture that emerges is very similar to the transfers arranged in a lump-sum fashion.

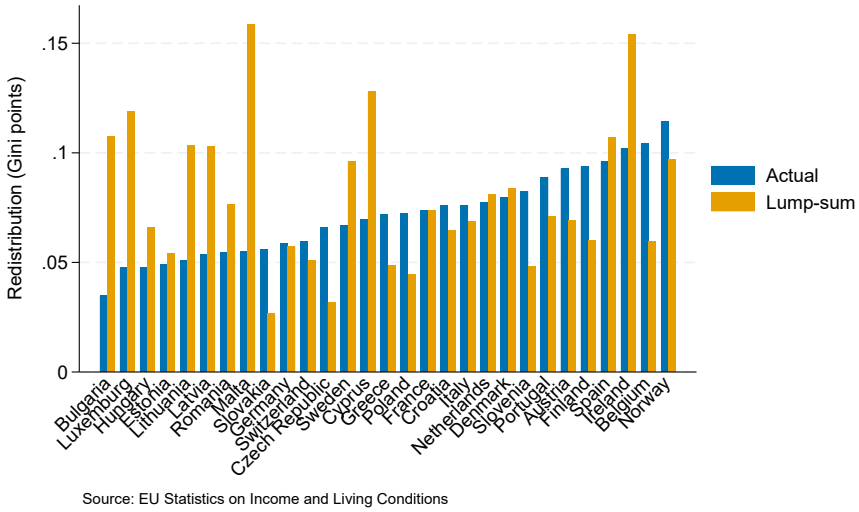


Figure 3.13: Actual redistribution versus redistribution that removes only between-group differences via lump-sum transfers.

Notes: Authors' calculations based on EU-SILC (2018). The figure shows the change in inequality due to redistribution (Gini) and the hypothetical reduction in the Gini if redistribution removed only between-group differences via group-specific lump-sum transfers.

3.4.4 Results at the European Level

In addition to comparing individual countries within Europe, an interesting point of view is to look at Europe as a whole. This analysis is similar in spirit to an emerging literature which has examined the role of within- and between-country inequality in the world, such as Lakner and Milanovic (2015). The difference in our approach is that the between-groups component can account for not only the country of residence but also other circumstances. This analysis is further motivated by European integration, where Europe as a whole provides another region of concern apart from individual countries.[12]

Figure 3.15 depicts the levels of total and between-type inequality before and after redistribution in Europe using different sets of circumstance variables. We examine three sets of circumstance variables: the original variables used above; only country

[12]The analysis could, of course, be done separately for EU-member countries only.

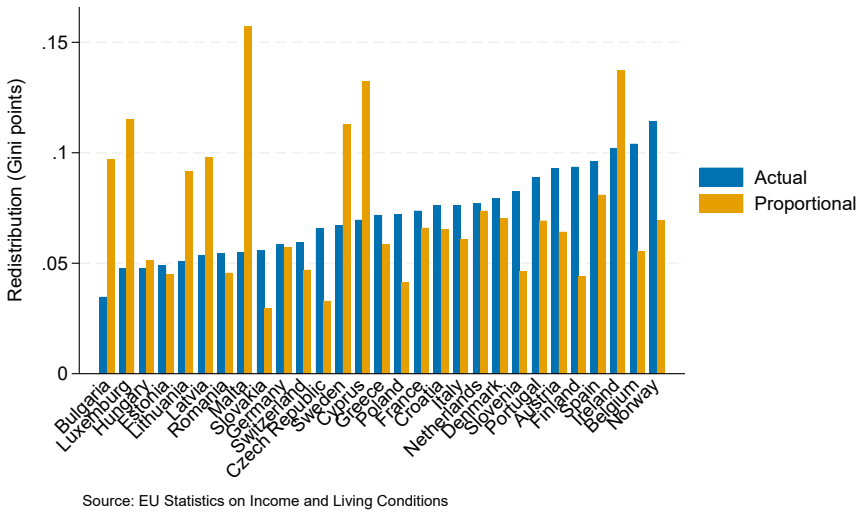


Figure 3.14: Actual redistribution versus redistribution that removes only between-group differences, preserving within-group Gini coefficients.

Notes: Authors' calculations based on EU-SILC (2018). The figure displays the change in inequality due to redistribution, measured by the Gini coefficient, and the hypothetical reduction in the Gini if redistribution removed only between group differences via group-specific proportional transfers that preserve within-group Gini.

of residence; and those used above, as well as the country of residence. The total inequality is, of course, always higher than between-type inequality, regardless of the set of circumstance variables. However, when both country of residence and all other circumstances are accounted for, the between-type inequality in Europe comes very close to overall inequality.

The reason for this strong result is likely due to the fact that the country of residence alone is a strong determinant of income. Although the country of residence explains a smaller share of overall inequality than other circumstances, its influence is still quite substantial. This remains true also when examining the inequality of disposable income. The redistribution which affects disposable income inequality is conducted in the presence of potential between-country transfers in Europe, also demonstrating that such transfers have not neutralized income differences related to the country of residence.

The corresponding results for MLD are available in the Appendix, see Figure A.41, which is perhaps easier to compare to Figure A.40 that has the results for the Gini in the same format. Again, the between-groups share of MLD is lower than for the case of the Gini index, but the qualitative picture that emerges from the analysis remains similar.

We also calculated the corresponding results for Europe as a whole in 2010, and the changes over time are shown in Figure 3.16. It illustrates how the overall inequality and the inequality between-types, using the full set of circumstances, have declined over time,

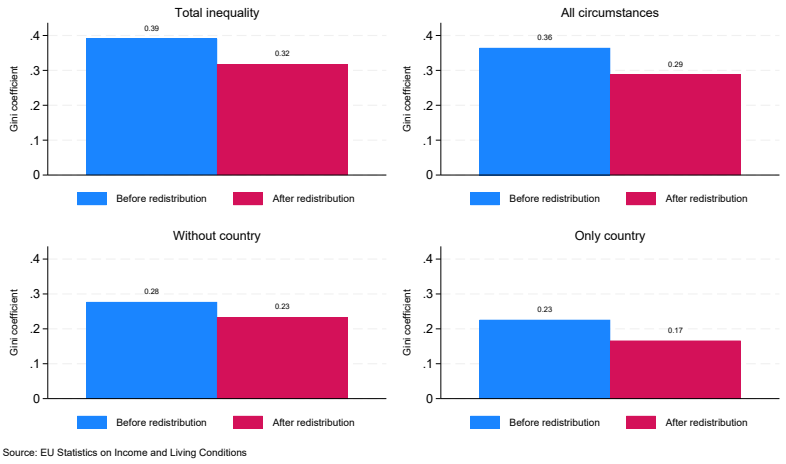


Figure 3.15: Levels of total and between-group inequality before and after redistribution in Europe using different sets of circumstance variables.

Notes: Authors' calculations based on EU-SILC (2018). The figure shows levels of total and between-group inequality before and after redistribution using different sets of circumstance variables. The circumstance variables are: (i) baseline variables (country of birth, father's highest education level, parental occupation, number of siblings, household financial situation at age 14, household tenancy status at age 14, and parental presence at age 14); (ii) baseline variables plus country of residence; and (iii) country of residence only.

consistent with the country-level analysis. Interestingly, the disposable income inequality for the between-group component when country alone is used as a group has declined quite substantially, in spite of a small increase for market inequality for the same concept.

This can be traced to a higher redistribution of the between-group component in 2018 compared to 2010, see Figure 3.17. The figure also splits the reduction of the Gini index of the between-group inequality into tax- and benefit-related components, illustrating that their relative role in this context is reasonably similar.

3.4.5 Determinants of between-groups inequality reduction

In this subsection, we explore what country-level factors are associated with the reduction of inequitable inequality by the redistributive system and compare the results to the determinants of overall inequality reduction. To be more specific, we attempt to explain in macro-level regressions the reduction of the between-type component in inequality or overall inequality, measured using either the Gini index or MLD, with the reduction of between-types inequality presented in Figures 3.2 and A.24.[13]

We approach the analysis using two sets of explanatory variables: (1) macro indicators

[13]In addition to using the absolute reduction as a dependent variable, we also explain the share of between-type inequality out of total inequality reduction, which is reported in the Appendix, Figures A.36-A.39.

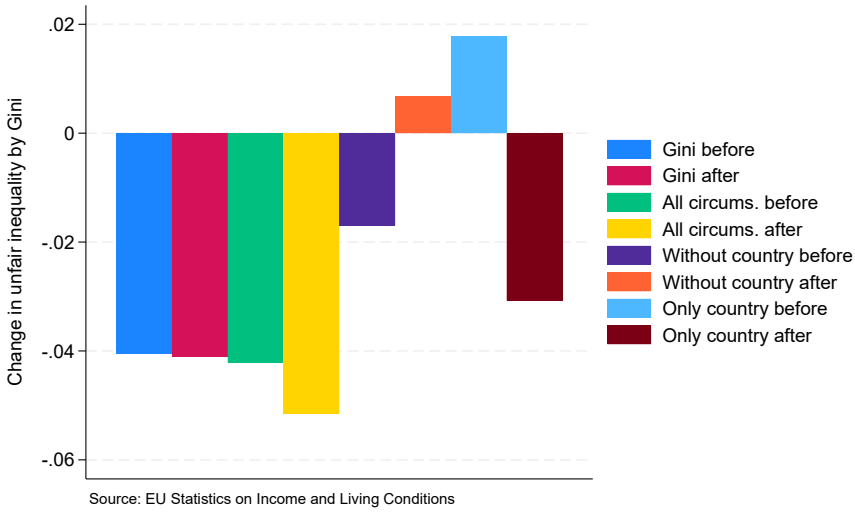


Figure 3.16: Changes in the Gini index calculated at the European level

Notes: Authors' calculations based on EU-SILC (2010 and 2018). The figure shows changes between 2010 and 2018 in total and between-group inequality before and after redistribution using different sets of circumstance variables. The circumstance variables are: (i) baseline variables (country of birth, father's highest education level, parental occupation, number of siblings, household financial situation at age 14, household tenancy status at age 14, and parental presence at age 14); (ii) baseline variables plus country of residence; and (iii) country of residence only.

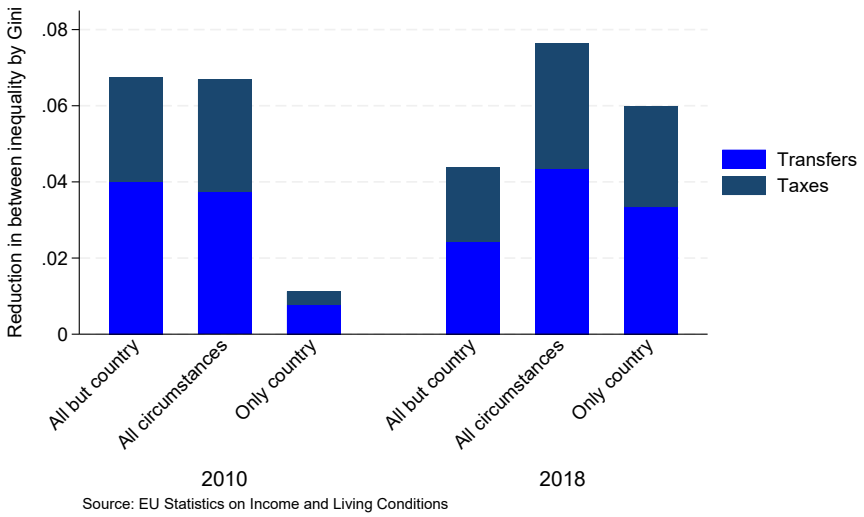


Figure 3.17: The redistributive impact of tax-benefit policies for Europe, measured using the Gini index

Notes: Authors' calculations based on EU-SILC (2010 and 2018). The figure shows change in between-group inequality before and after redistribution using different sets of circumstance variables. The circumstance variables are: (i) baseline variables (country of birth, father's highest education level, parental occupation, number of siblings, household financial situation at age 14, household tenancy status at age 14, and parental presence at age 14); (ii) baseline variables plus country of residence; and (iii) country of residence only.

obtained from external sources and (2) indicators calculated from the SILC data ourselves. As per the first, the explanatory variables capture country characteristics (GDP per capita, demographics), tax revenues (overall and the shares of direct vs. indirect taxes), expenditure on different social programs, and levels and progressivity of the income tax (which draws on OECD Taxing Wages database). The exact definitions and data sources are listed in Table A.2.

The results pertaining to the determinants obtained from external sources are reported in Figures 3.18 and 3.19 for inequality using the Gini index and the mean log deviation, respectively. These figures report OLS coefficients and confidence bands from models where the log of the between-type inequality measure is explained, one variable at a time, with potential determinants, also in the log format, and hence the coefficients can be interpreted as elasticities. Due to the low number of observations, the confidence bands typically include zeros, and statistical significance is limited. A high dependency ratio, a high share of child-related social spending, as well as the progressivity of income taxation (for MLD) are positively associated with the reduction in between-types inequality. A high dependency ratio implies that pension payments are at the high level, which is recorded as government redistribution in the analysis. Somewhat surprisingly, the overall tax-to-GDP ratio is negatively whereas the indirect tax share is positively linked with inequitable, between-types, inequality reduction. Perhaps indirect tax revenues enable governments to allocate more funding for certain social programs in the countries in our data. The factors associated with the reduction in overall inequality are not always the same: The overall tax take and the income tax share are positively correlated with overall inequality reduction, whereas there is a negative correlation with the indirect tax share.

For the second approach, we have calculated several indices from the underpinning SILC data, with the benefit that these are available for all the countries in both years. The corresponding results are presented in Figures 3.20 and 3.21. Indicators measure the progressivity of the tax and transfer system (by the Gini, concentration, and Kakwani indices), as well as the level of different kinds of transfer payments, relative to the mean income in the country.

The progressivity of the tax system is quite clearly associated with a reduction of inequitable inequality. Although the level of transfer payments as a whole is also positively associated with lower inequitable inequality, the progressivity of transfer payments is not. In contrast, the progressivity of the transfer system has a clear positive correlation with the reduction of the overall inequality. Another result that emerges is that the appears to be linked with overall inequality reduction.

Taken as a whole, the results indicate that the progressivity of the tax system and the overall level of social transfers, but not their progressivity, appear to be those indicators that are most closely linked with the reduction of inequitable inequality.

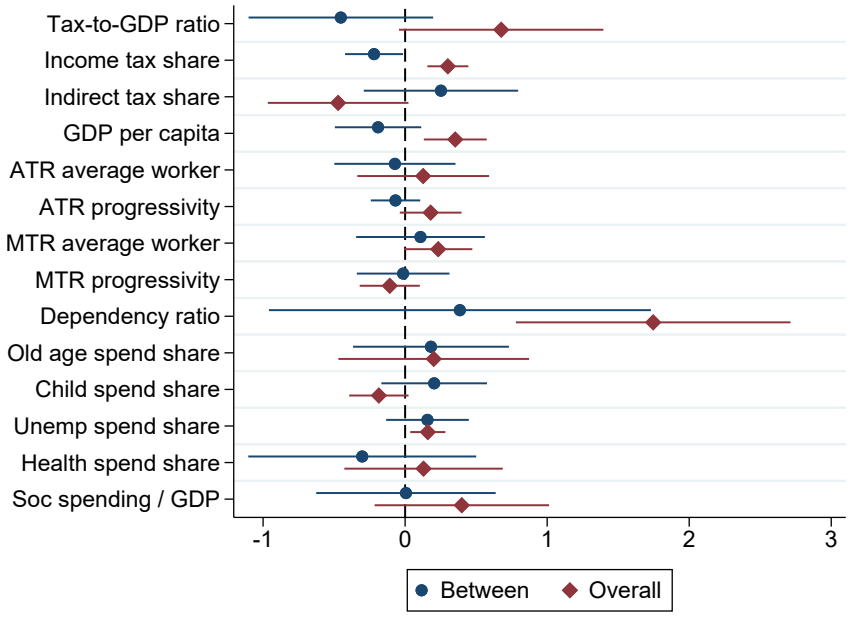


Figure 3.18: Explaining the reduction in between-types and overall inequality, measured using the Gini coefficient. Indicators obtained from external sources.

Notes: Coefficients of regression coefficients and their 95% confidence intervals from models where the log of the reduction of between-group or overall inequality, measured by the Gini index, is regressed one-by-one on the log of the explanatory variables listed in the chart. In regressions using the independent variables Tax-to-GDP ratio – GDP per capita, two waves of data (2010 and 2018) are used. In these models, the regression equation also includes a time dummy. In the remaining regressions, only the latter wave is used for data availability reasons. In models using OECD Taxing Wages data (regressions ATR average worker – MTR progressivity), non-OECD countries are excluded.

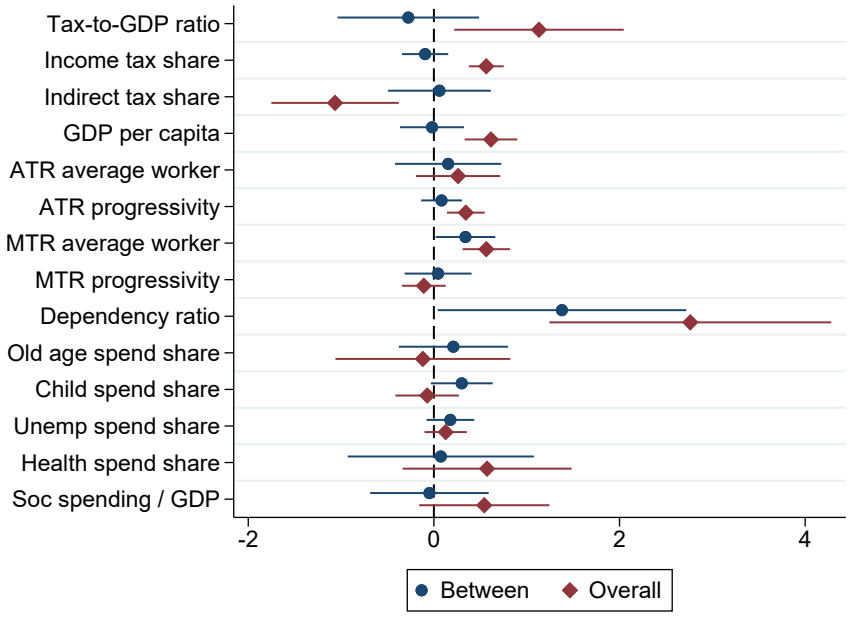


Figure 3.19: Explaining the reduction in between-group and overall inequality, measured using MLD. Indicators obtained from external sources.

Notes: Coefficients of regression coefficients and their 95% confidence intervals from models where the log of the reduction of between-group or overall inequality, measured by the MLD, is regressed one-by-one on the log of the explanatory variables listed in the chart. In regressions using the independent variables Tax-to-GDP ratio – GDP per capita, two waves of data (2010 and 2018) are used. In these models, the regression equation also includes a time dummy. In the remaining regressions, only the latter wave is used for data availability reasons. In models using OECD Taxing Wages data (regressions ATR average worker – MTR progressivity), non-OECD countries are excluded.

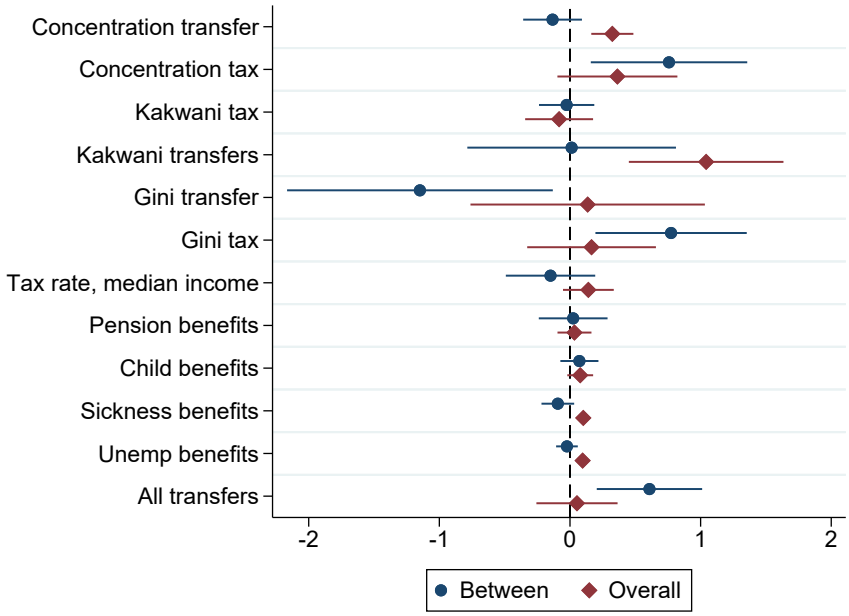


Figure 3.20: Explaining the reduction in between-group and overall inequality, measured using the Gini coefficient. Indicators based on own calculations from SILC data.

Notes: Coefficients of regression coefficients and their 95% confidence intervals from models where the log of the reduction of between-group or overall inequality, measured by the Gini index, is regressed one-by-one on the log of the explanatory variables listed in the chart. In all regressions, two waves of data (2010 and 2018) are used. The regression equation also includes a time dummy.

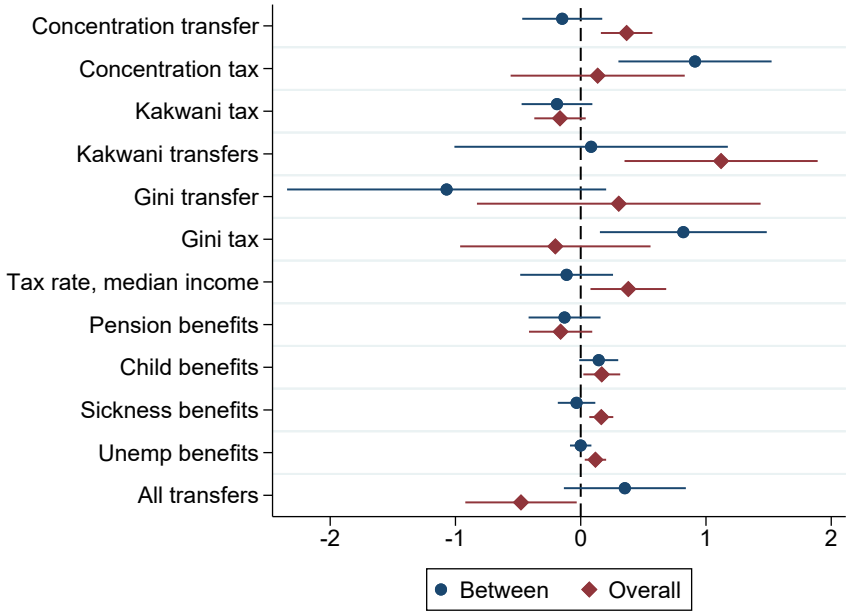


Figure 3.21: Explaining the reduction in between-group inequality, measured using mean log deviation. Indicators based on own calculations from SILC data.

Notes: Coefficients of regression coefficients and their 95% confidence intervals from models where the log of the reduction of between-group or overall inequality, measured by the MLD, is regressed one-by-one on the log of the explanatory variables listed in the chart. In all regressions, two waves of data (2010 and 2018) are used. The regression equation also includes a time dummy.

3.5 Conclusions

The extent of redistribution is typically evaluated in the research literature in terms of its impact on the inequality of outcomes before and after taxes and transfers, rather than in terms of inequality of opportunity. In this paper, we contrast actual redistribution with redistribution that would be consistent with the principles of Equality of Opportunity (EOp). Using EU-SILC microdata and standard EOp methods, we assess whether European tax-benefit systems can be considered redistributive from an equality-of-opportunity perspective, rather than merely redistributive. Our analysis covers a broad set of European countries and allows for comparisons over time.

We find that inequality between groups, interpreted as inequality of opportunity, is generally modest across European countries. Redistribution reduces both total inequality and inequitable inequality, but the majority of redistribution operates within groups, and therefore targets income differences related to effort rather than circumstances. From an EOp perspective, many European tax-benefit systems thus redistribute substantially beyond what would be required to compensate for circumstance-based disadvantages. This does not imply that such redistribution is socially undesirable. Rather, it reflects broader policy objectives, including social insurance, poverty reduction, and solidarity. At the same time, it implies that EOp considerations alone cannot fully rationalize the observed extent of redistribution and highlights a tension between the principles of compensation and reward.

By linking inequality-of-opportunity outcomes to aggregate policy variables, we find that the progressivity of income taxation is more strongly associated with reductions in inequitable inequality than the progressivity of transfer payments. From a policy perspective, this suggests that policymakers should clearly distinguish between the desired degree of redistribution and its design. Moreover, policies aimed at improving equality of opportunity may require less ex post redistribution if effective predistribution policies such as investments in education and early-life conditions are in place.

Methodologically, our analysis follows the established literature that decomposes inequality into components attributable to circumstances and effort, and therefore shares its limitations. Circumstances are imperfectly observed and effort is not directly measured, implying that our estimates of inequality of opportunity should be interpreted as lower bounds. In addition, the results depend on the choice of inequality indices and decomposition methods, reflecting the fact that different measures place different weights on various parts of the income distribution.

3.6 APPENDIX

3.6.1 Additional results by countries

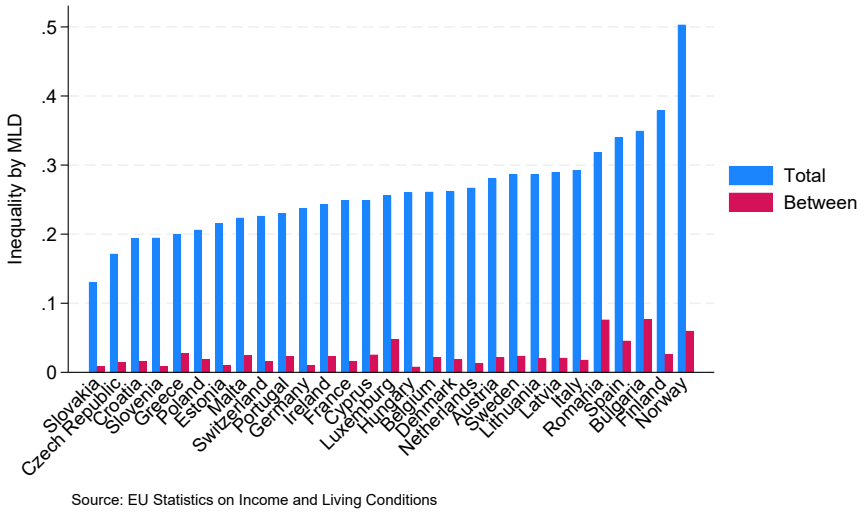


Figure A.22: Overall and between-group inequality, measured using MLD, 2018.

Notes: Authors' calculations based on EU-SILC (2018). The figure shows total and between-group inequality of market income measured by the mean log deviation (MLD). The circumstance variables are: country of birth, father's highest education level, parental occupation, number of siblings, household financial situation at age 14, household tenancy status at age 14, and parental presence at age 14.

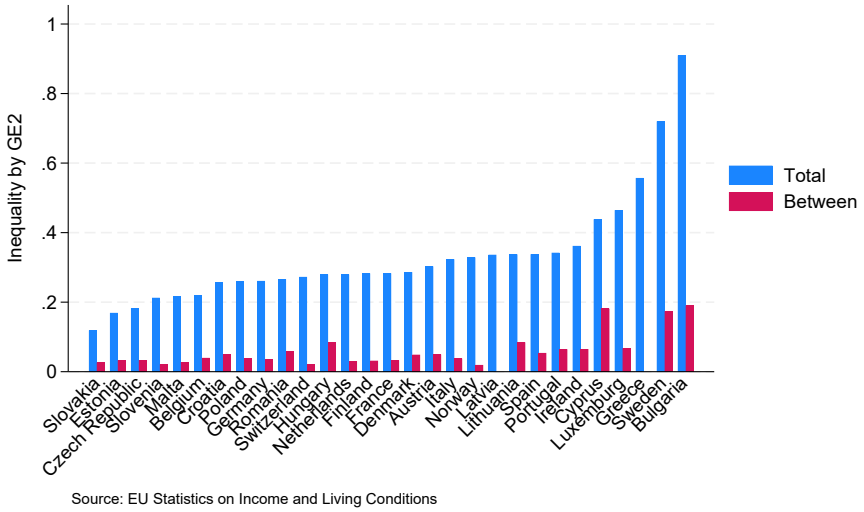


Figure A.23: Overall and between-group inequality, measured using GE(2), 2018.

Notes: Authors' calculations based on EU-SILC (2018). The figure shows total and between-group inequality of market income measured using GE(2). The circumstance variables are: country of birth, father's highest education level, parental occupation, number of siblings, household financial situation at age 14, household tenancy status at age 14, and parental presence at age 14.

3.6.2 The Gini Decomposition Following Heikkuri and Schief (2025)

In the main analysis, we have computed the Gini index based on group-average incomes. However, this measure does not fully capture within-group inequality, because the Gini index is not directly decomposable. To complement our analysis, we use the novel decomposition proposed by Heikkuri and Schief (2025), which allows the total Gini to be expressed as the sum of between- and within-group components:

$$G = G_B + G_W,$$

where G is the total Gini, G_B the between-group component, and G_W the within-group component.

Unlike the simple Gini computed on group means, the Heikkuri et al. decomposition preserves the Gini's pairwise-distance perspective. It accounts not only for differences in group means but also for the dispersion of all individuals across groups.

The within-group component is given by:

$$G_W = \left(\sum_{k=1}^{K} \sqrt{\pi_k \, \theta_k \, G_k} \right)^2,$$

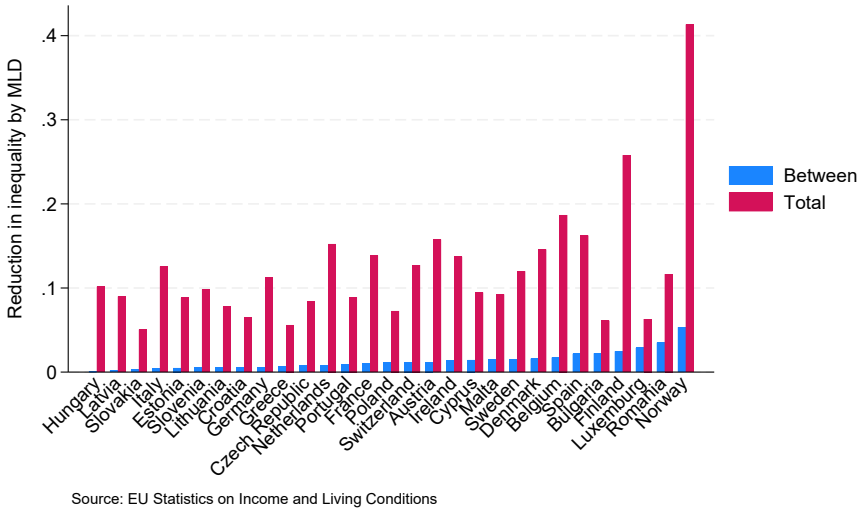


Figure A.24: Reduction in overall and between-group inequality, measured using MLD, in 2018

Notes: Authors' calculations based on EU-SILC (2018). The figure shows the reduction in total and between-group inequality of market income due to redistribution, measured by the mean log deviation (MLD). The circumstance variables are: country of birth, father's highest education level, parental occupation, number of siblings, household financial situation at age 14, household tenancy status at age 14, and parental presence at age 14.

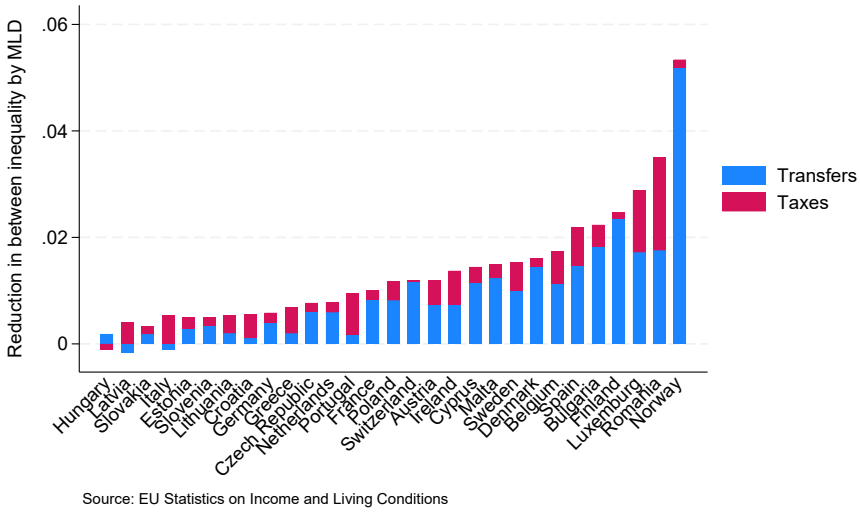


Figure A.25: Reduction in between-group inequality: the role of taxes and transfers, MLD, in 2018

Notes: Authors' calculations based on EU-SILC (2018). The figure shows the reduction in between-group inequality due to redistribution, decomposed into taxes and transfers, measured by the mean log deviation (MLD). The circumstance variables are: country of birth, father's highest education level, parental occupation, number of siblings, household financial situation at age 14, household tenancy status at age 14, and parental presence at age 14.

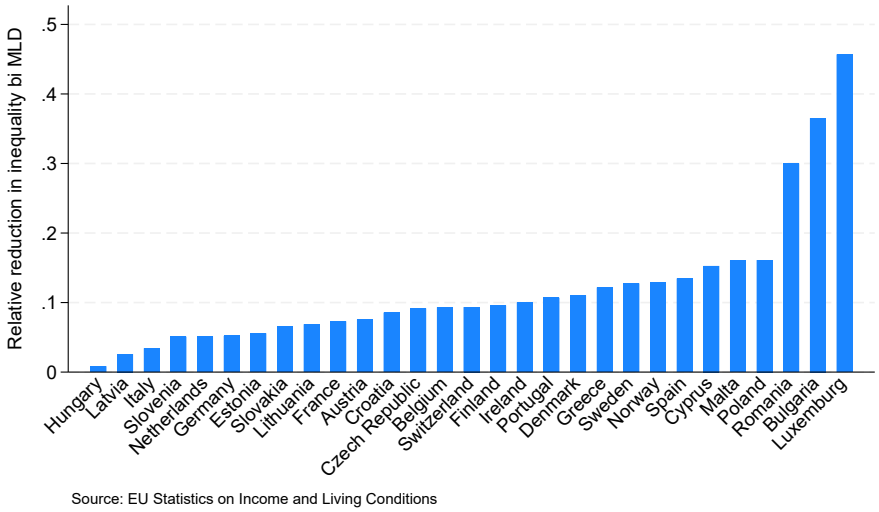


Figure A.26: Reduction in between-group inequality relative to reduction in overall inequality, MLD, in 2018

Notes: Authors' calculations based on EU-SILC (2018). The figure shows the reduction in between-group inequality due to redistribution relative to the reduction in total inequality, measured by the mean log deviation (MLD). The circumstance variables are: country of birth, father's highest education level, parental occupation, number of siblings, household financial situation at age 14, household tenancy status at age 14, and parental presence at age 14.

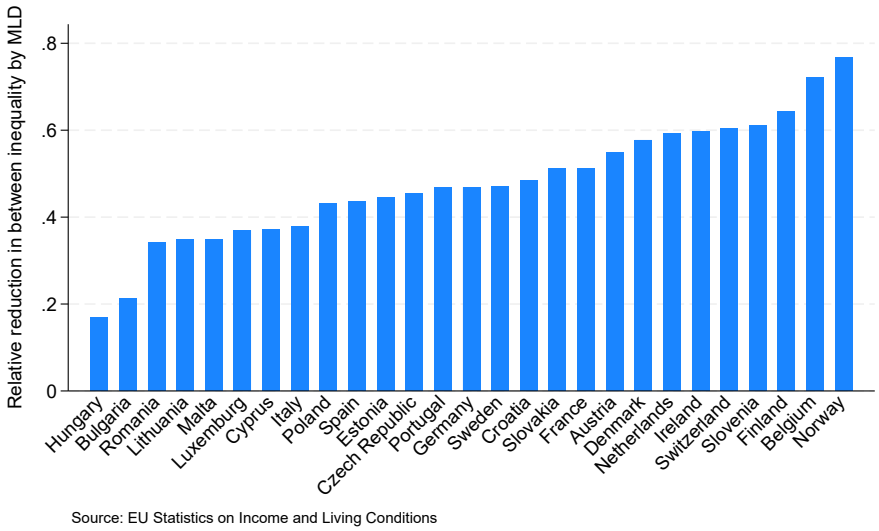


Figure A.27: Reduction in between-group inequality relative to the level of between-group inequality, MLD, in 2018

Notes: Authors' calculations based on EU-SILC (2018). The figure shows the reduction in between-group inequality due to redistribution relative to the level of market income between-group inequality, measured by the mean log deviation (MLD). The circumstance variables are: country of birth, father's highest education level, parental occupation, number of siblings, household financial situation at age 14, household tenancy status at age 14, and parental presence at age 14.

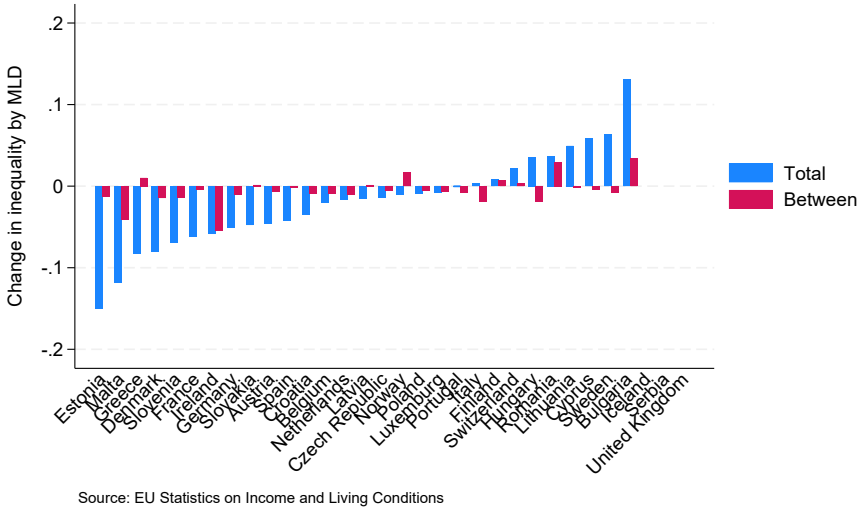


Figure A.28: Changes in inequality of market incomes, MLD

Notes: Authors' calculations based on EU-SILC (2010 and 2018). The figure shows the change in between-group inequality of market income between 2010 and 2018, measured by the mean log deviation (MLD). The circumstance variables are: country of birth, father's highest education level, parental occupation, number of siblings, household financial situation at age 14, household tenancy status at age 14, and parental presence at age 14.

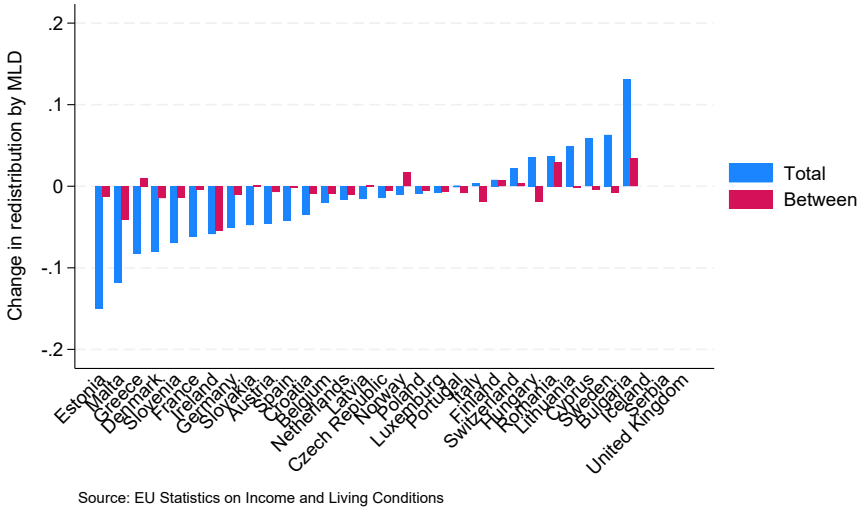


Figure A.29: Changes in redistribution between 2010 and 2018, MLD

Notes: Authors' calculations based on EU-SILC (2010 and 2018). The figure shows the change in redistribution of total and between-group inequality between 2010 and 2018, measured by the mean log deviation (MLD). The circumstance variables are: country of birth, father's highest education level, parental occupation, number of siblings, household financial situation at age 14, household tenancy status at age 14, and parental presence at age 14.

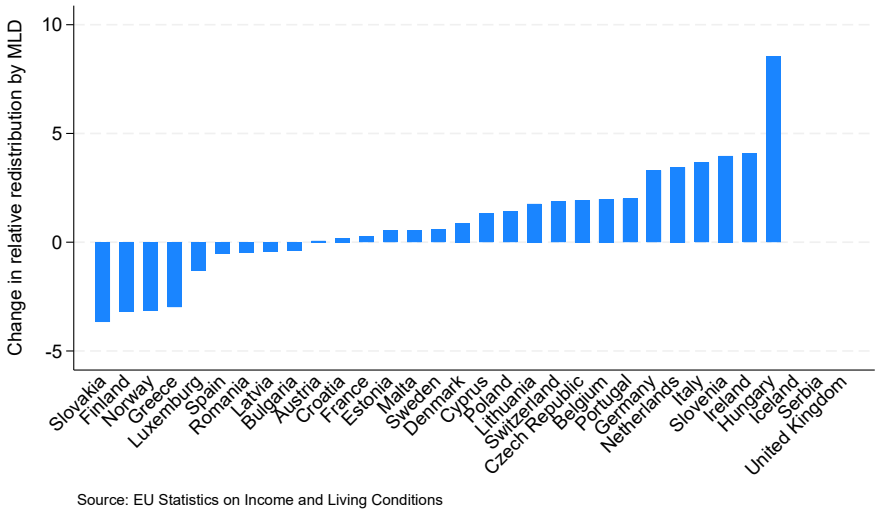


Figure A.30: Change between 2010 and 2018 in the share of between-group redistribution relative to overall redistribution, measured by mean log deviation (MLD).

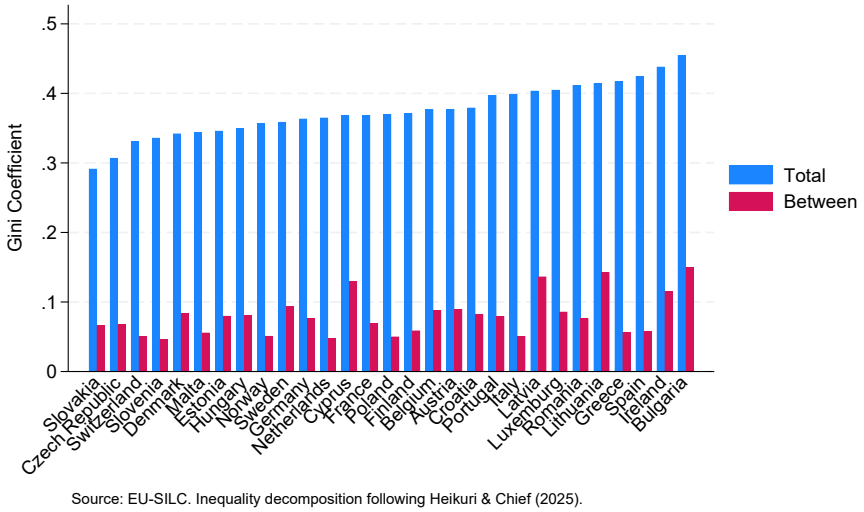
Notes: Authors' calculations based on EU-SILC (2010 and 2018). The figure shows the change between 2010 and 2018 in the relative redistribution of between-group inequality compared to total inequality, measured by the mean log deviation (MLD). We first compute total and between-group inequality before and after redistribution. The change in each component is then used to construct the relative redistribution of between-group inequality (defined as the change in between-group inequality divided by the change in total inequality). Finally, we take the difference in this relative measure between 2010 and 2018. The circumstance variables used to construct between-group inequality are: country of birth, father's highest education level, parental occupation, number of siblings, household financial situation at age 14, household tenancy status at age 14, and parental presence at age 14.

and the between-group component is obtained as the residual:

$$G_B = G - G_W,$$

where π_k, θ_k, and G_k denote the population share, income share, and Gini coefficient of subgroup k, respectively.

This decomposition shows that if inequality is high within groups with large population or income shares, the between-group inequality will be relatively small. Indeed, Figure A.31 shows that between-group inequality is smaller under this decomposition than when using the Gini on group-mean incomes (Figure 3.1). Figure A.32 highlights that the ranking of countries by between-group inequality differs depending on the method used. Similar re-ranking of countries for the redistribution of between-group inequality can be observed in Figures A.33, A.34 and A.35.



Source: EU-SILC. Inequality decomposition following Heikuri & Chief (2025).

Figure A.31: Levels of total and between-group Gini following Heikkuri and Schief (2025).

Notes: Authors' calculations based on EU-SILC (2018). The figure shows the total and between-group inequality based on the Gini decomposition of Heikkuri and Schief (2025). The circumstance variables are: country of birth, father's highest education level, parental occupation, number of siblings, household financial situation at age 14, household tenancy status at age 14, and parental presence at age 14.

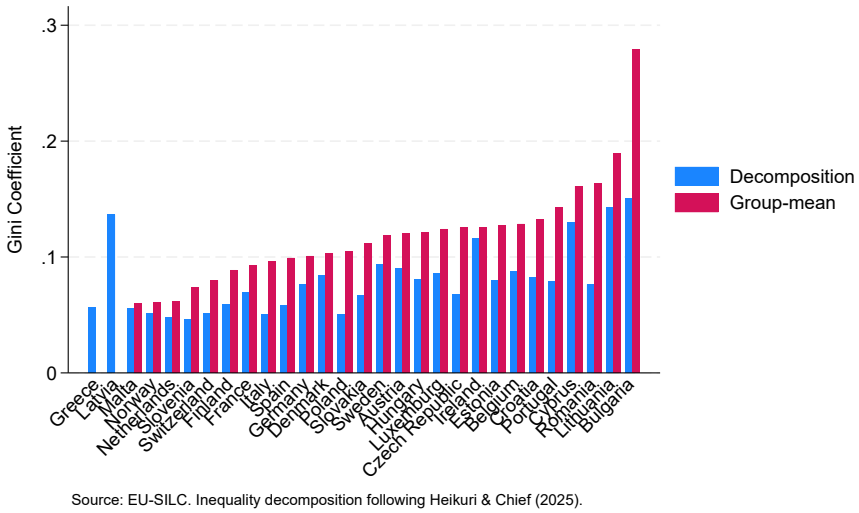


Figure A.32: Between-group Gini following Heikkuri and Schief (2025) versus Gini based on group-mean incomes.

Notes: Authors' calculations based on EU-SILC (2018). The figure compares between-group inequality based on the Gini decomposition of Heikkuri and Schief (2025) with the Gini coefficient computed using group-specific mean incomes, as in the baseline analysis. The circumstance variables are: country of birth, father's highest education level, parental occupation, number of siblings, household financial situation at age 14, household tenancy status at age 14, and parental presence at age 14. The between-group inequality in the decomposition is smaller than the inequality of group-mean incomes, since the between component is reduced if inequality is high within groups with large population or income shares.

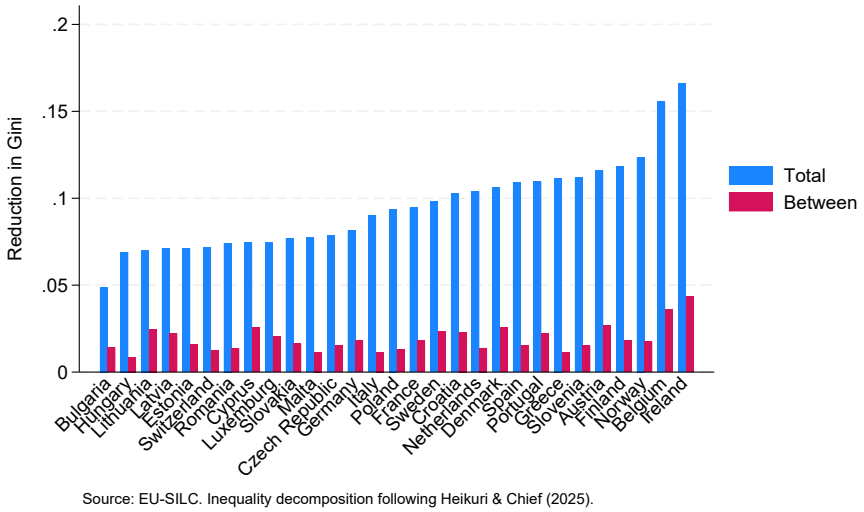


Figure A.33: Redistribution of total and between-group inequality by Gini following Heikkuri and Schief (2025).

Notes: Authors' calculations based on EU-SILC (2018). The figure shows the total and between-group redistribution based on the Gini decomposition of Heikkuri and Schief (2025). The circumstance variables are: country of birth, father's highest education level, parental occupation, number of siblings, household financial situation at age 14, household tenancy status at age 14, and parental presence at age 14.

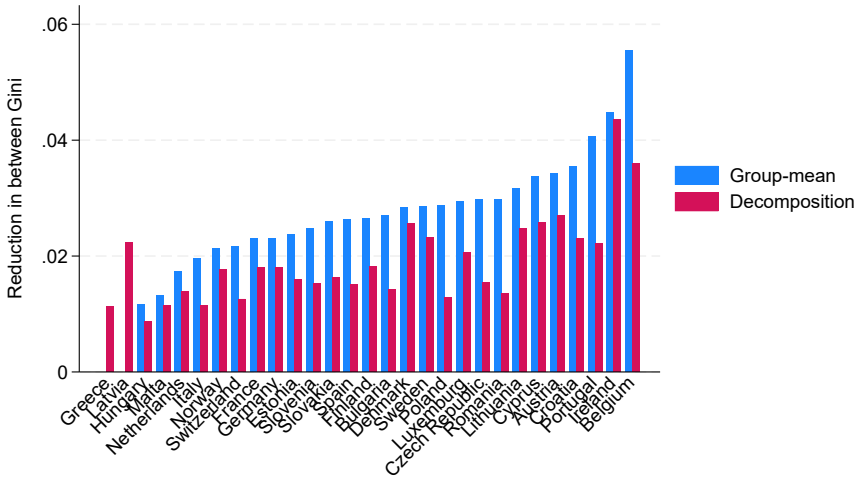
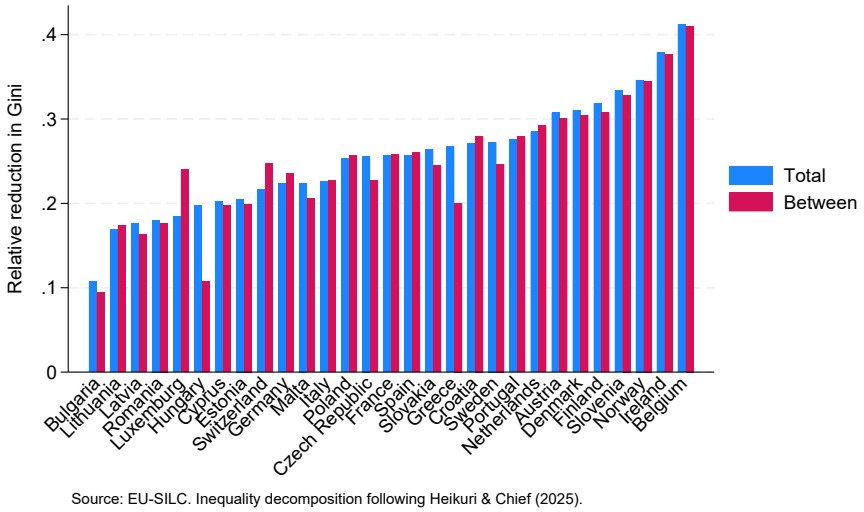


Figure A.34: Redistribution of between-group inequality by Gini following Heikkuri and Schief (2025) vs. redistribution calculated on group-mean incomes.

Notes: Authors' calculations based on EU-SILC (2018). The figure compares between-group inequality reduction due to redistribution based on the Gini decomposition of Heikkuri and Schief (2025) with the Gini coefficient computed using group-specific mean incomes, as in the baseline analysis. The circumstance variables are: country of birth, father's highest education level, parental occupation, number of siblings, household financial situation at age 14, household tenancy status at age 14, and parental presence at age 14.



Source: EU-SILC. Inequality decomposition following Heikuri & Chief (2025).

Figure A.35: Relative redistribution of total and between-group inequality by Gini following Heikkuri and Schief (2025).

Notes: Authors' calculations based on EU-SILC (2018). The figure shows the total and between-group redistribution relative to the level of inequality based on the Gini decomposition of Heikkuri and Schief (2025). The circumstance variables are: country of birth, father's highest education level, parental occupation, number of siblings, household financial situation at age 14, household tenancy status at age 14, and parental presence at age 14.

3.6.3 Macro analysis

Table A.2: Definitions and sources of macro data

Variable	Definition	Source
Tax-to-GDP ratio	Ratio of all tax revenues including social security contributions to GDP	UNU-WIDER GRD
Income tax share	Share of personal income tax out of all tax revenues	UNU-WIDER GRD
Indirect tax share	Share of indirect tax out of all tax revenues	UNU-WIDER GRD
GDP per capita	GDP per capita, in PPP €	Eurostat
ATR average worker	Average tax rate (ATR) at average earnings level	OECD Taxing Wages
ATR progressivity	Difference in ATR between workers at 167 and 67% of average earnings	OECD Taxing Wages
MTR average worker	Marginal tax rate (ATR) at average earnings level	OECD Taxing Wages
MTR progressivity	Difference in MTR between workers at 167 and 67% of average earnings	OECD Taxing Wages
Dependency ratio	Population 0 to 24 years and 65 years or over to population 25 to 64 years	Eurostat
Old age spending share	Share of old age spending out of all social spending	Eurostat
Child spending share	Share of child-related spending out of all social spending	Eurostat
Unemployment spending share	Share of unemployment spending out of all social spending	Eurostat
Health spending share	Share of health spending out of all social spending	Eurostat
Social spending / GDP	Social spending relative to GDP	Eurostat

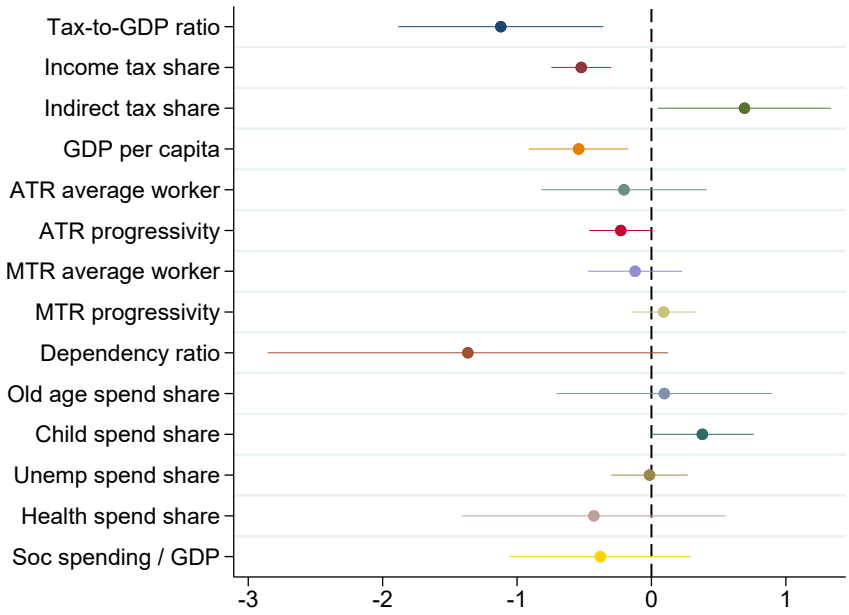


Figure A.36: Explaining the reduction in between-group inequality, measured using the Gini coefficient. Indicators obtained from external sources.

Notes: Coefficients of regression coefficients and their 95% confidence intervals from models where the log of the share of the between-group component in inequality out of all inequality reduction, measured by the Gini index, is regressed one-by-one on the log of the explanatory variables listed in the chart. In regressions using the independent variables Tax-to-GDP ratio – GDP per capita, two waves of data (2010 and 2018) are used. In these models, the regression equation also includes a time dummy. In the remaining regressions, only the latter wave is used for data availability reasons. In models using OECD Taxing Wages data (regressions ATR average worker – MTR progressivity), non-OECD countries are excluded.

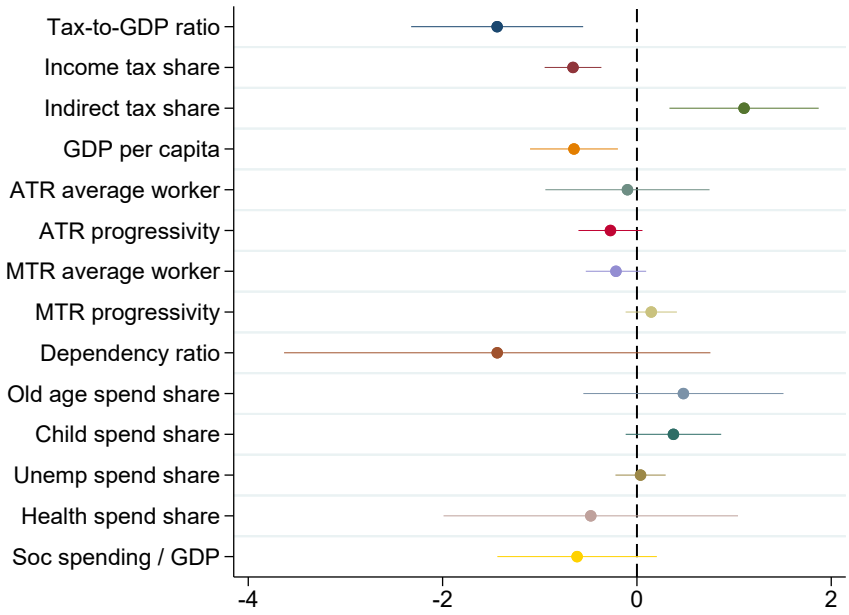


Figure A.37: Explaining the reduction in between-group inequality, measured using MLD. Indicators obtained from external sources.

Notes: Coefficients of regression coefficients and their 95% confidence intervals from models where the log of the share of the between-group component in inequality out of all inequality reduction, measured by the MLD, is regressed one-by-one on the log of the explanatory variables listed in the chart. In regressions using the independent variables Tax-to-GDP ratio – GDP per capita, two waves of data (2010 and 2018) are used. In these models, the regression equation also includes a time dummy. In the remaining regressions, only the latter wave is used for data availability reasons. In models using OECD Taxing Wages data (regressions ATR average worker – MTR progressivity), non-OECD countries are excluded.

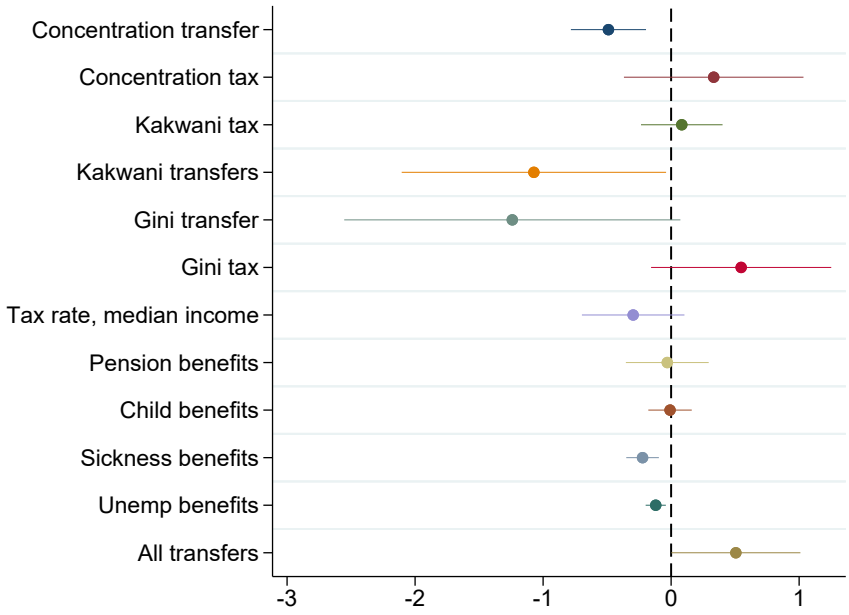


Figure A.38: Explaining the reduction in between-group inequality, measured using the Gini coefficient. Indicators based on own calculations from SILC data.

Notes: Coefficients of regression coefficients and their 95% confidence intervals from models where the log of the share of the between-group component in inequality out of all inequality reduction, measured by the Gini index, is regressed one-by-one on the log of the explanatory variables listed in the chart. In all regressions, two waves of data (2010 and 2018) are used. The regression equation also includes a time dummy.

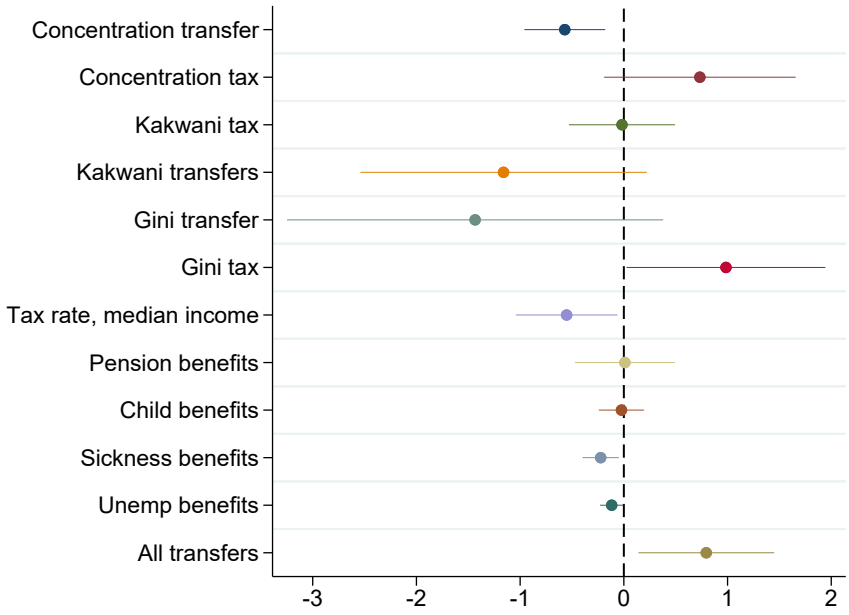


Figure A.39: Explaining the reduction in between-group inequality, measured using mean log deviation. Indicators based on own calculations from SILC data.

Notes: Coefficients of regression coefficients and their 95% confidence intervals from models where the log of the share of the between-group component in inequality out of all inequality reduction, measured by the MLD, is regressed one-by-one on the log of the explanatory variables listed in the chart. In all regressions, two waves of data (2010 and 2018) are used. The regression equation also includes a time dummy.

3.6.4 Further results at the European level

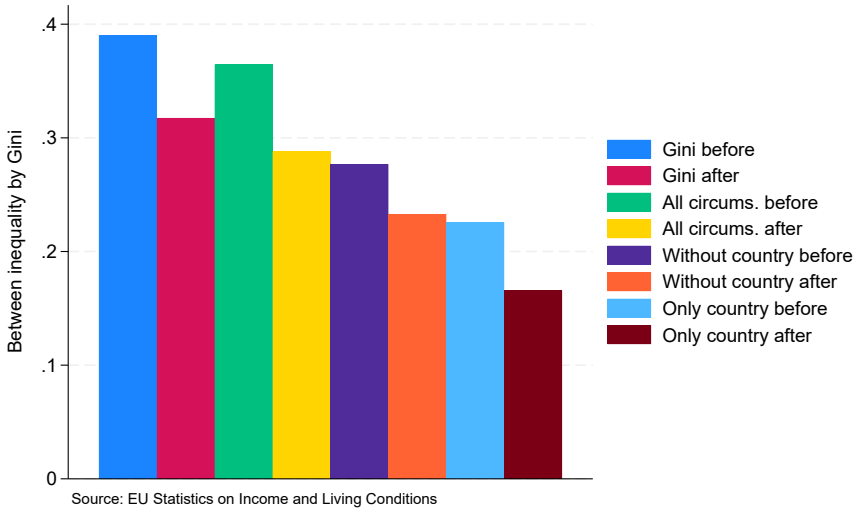


Figure A.40: Results for the Gini index at the European level

Notes: Authors' calculations based on EU-SILC (2018). The figure shows the levels of total and between-group inequality measured by the Gini coefficient of before and after redistribution using different sets of circumstance variables. The circumstance variables are: (i) baseline variables (country of birth, father's highest education level, parental occupation, number of siblings, household financial situation at age 14, household tenancy status at age 14, and parental presence at age 14); (ii) baseline variables plus country of residence; and (iii) country of residence only.

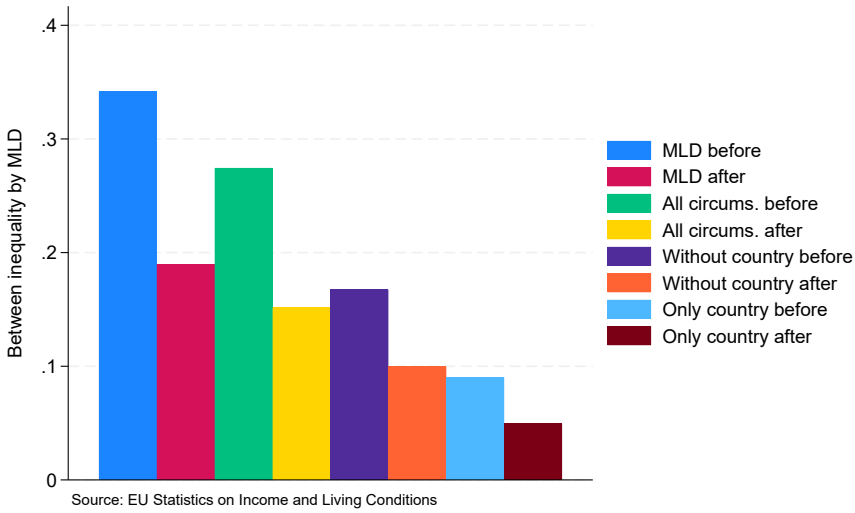


Figure A.41: Results for MLD at the European level

Notes: Authors' calculations based on EU-SILC (2018). The figure shows the levels of total and between-group inequality measured by the mean log deviation (MLD) before and after redistribution using different sets of circumstance variables. The circumstance variables are: (i) baseline variables (country of birth, father's highest education level, parental occupation, number of siblings, household financial situation at age 14, household tenancy status at age 14, and parental presence at age 14); (ii) baseline variables plus country of residence; and (iii) country of residence only.

Chapter 4

Asymmetric Information and Lifetime Pensions in the Finnish Pension System

Arttu Kauhanen[1]

Abstract

The presence and implications of asymmetric information within the Finnish pension system is largerly an open question. Based on the observed and estimated mortalities, I find that the variation across life expectancies in different retirement ages is negligible, yielding to an average increase of 233 euros or 0.07% in lifetime pensions for people who retired between 2005 and 2016. Furthermore, the supplementary pension contributions associated with postponing retirement are lower than the expected increase in lifetime pension benefits. If ensuring actuarial fairness is a priority for the system, stronger incentives should be implemented to encourage later retirement.

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4.1 Introduction

The potential importance of asymmetric information has been known since the seminal paper of Akerlof (1970). Despite of being an ubiquitous phenomenon in the insurance markets, empirical work devoted to quantifying the inefficiencies caused by private information is relatively new, mostly after 2010. While finding evidence for adverse selection, the literature often finds small monetary and welfare effects (see for example Bloom et al. (2006); Einav et al. (2010b); Beauchamp and Wagner (2012); Fitzpatrick (2015); Brinch et al. (2018)). However, this literature is mostly concentrated on the UK and US context. The only evidence in the context of Finnish pensions by Nivalainen (2022) examines part-time old-age pensions and the findings are in accordance with the presence of adverse selection. Since the pension system involves a considerable amount of funds, the presence of asymmetric information in the system can result in significant monetary and welfare losses. For example, the statutory pension expenditure in Finland made up 13.2 per cent of the GDP in 2021 and the proportion is expected to rise in the future (Finnish Center for Pensions, 2024).

The objective of this study is to examine the adverse selection or moral hazard concerning life expectancies across various retirement ages and its monetary implications in the context of the Finnish pension system. By doing so, this research contributes to the literature trying to establish the relatively small importance of selection in the context of pensions. Furthermore, it provides valuable insights into the anticipated pension costs in Finland in the future.

I exploit a flexible retirement age in the Finnish pension system to examine if higher retirement age is associated with higher life expectancy, a sign of adverse selection or moral hazard (Chiappori and Salanie, 2000). A pension can be seen as a form of a longevity insurance, where pensioners with higher expected longevity are more costly to insure. In the Finnish system, between 2005 and 2016 pensioners were able to choose their retirement age between 63 and 68 years old. Also an early old age pension retirement age of 62 was possible. The system incentivized the decision to retire later with higher pensions, i.e., better insurance coverage. Postponing retirement effectively bought an additional annuity. The price of the annuity was the extra pension payments paid while working longer and the current pension benefits that were given up when postponing.

A positive correlation between retirement age and life expectancy may reflect either adverse selection or moral hazard. Adverse selection arises if individuals with higher expected longevity are more likely to retire later, while moral hazard arises if retiring later causally increases life expectancy through behavioral changes. Although both mechanisms generate similar empirical patterns, the analysis focuses on detecting selection effects and evaluating their policy implications, acknowledging that the observed correlation may also capture behavioral responses. While both mechanisms may increase

pension liabilities, their policy implications differ, as adverse selection reflects a sorting problem, whereas moral hazard reflects a causal effect of retirement behavior on longevity.

As seen in Figure 4.1, the life expectancy varies considerably by sex and education level. If the retirement age distribution between these groups differs considerably, that would already imply different life expectancies in different retirement ages. As we can see in Figure 4.2, this is not the case. The retirement age distribution follows a similar pattern in all the demographic groups. The age of 63 seems to be the norm for all the groups while only few people postpone retirement past the age of 65. However, it is possible that the retirement age is correlated with life expectancy for other reason or through other channels. To verify this is not the case, a more straightforward examination of life expectancies by retirement age is required.

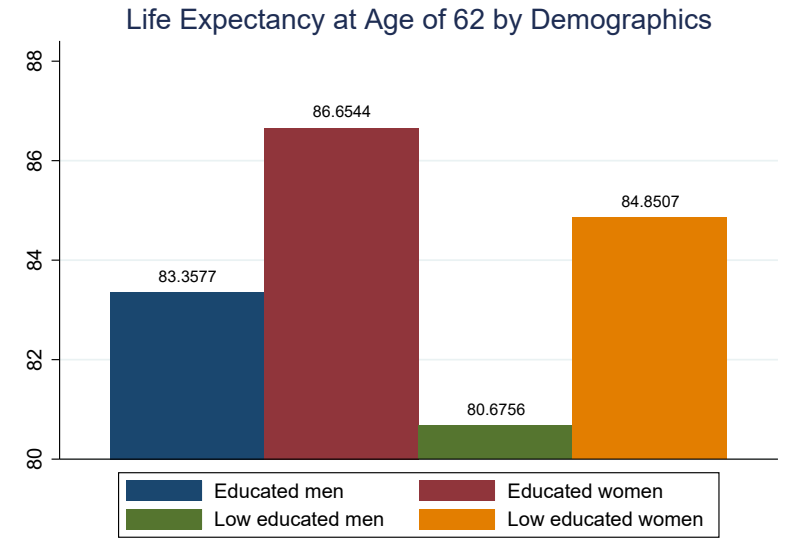
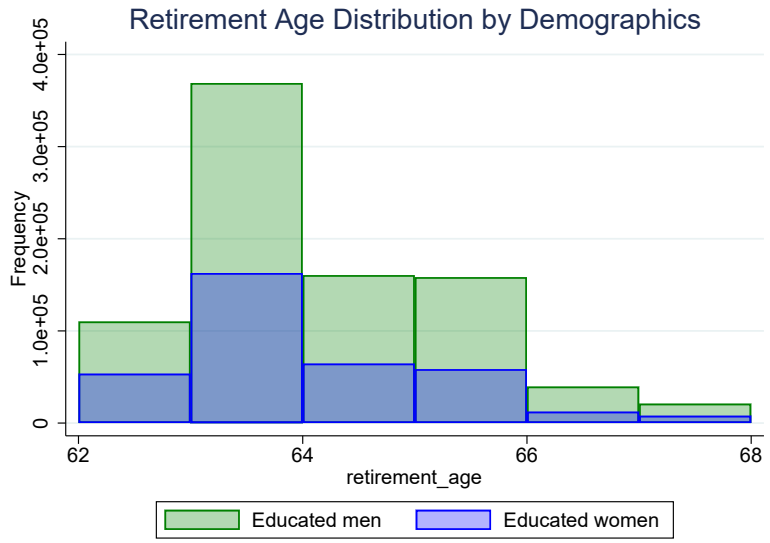


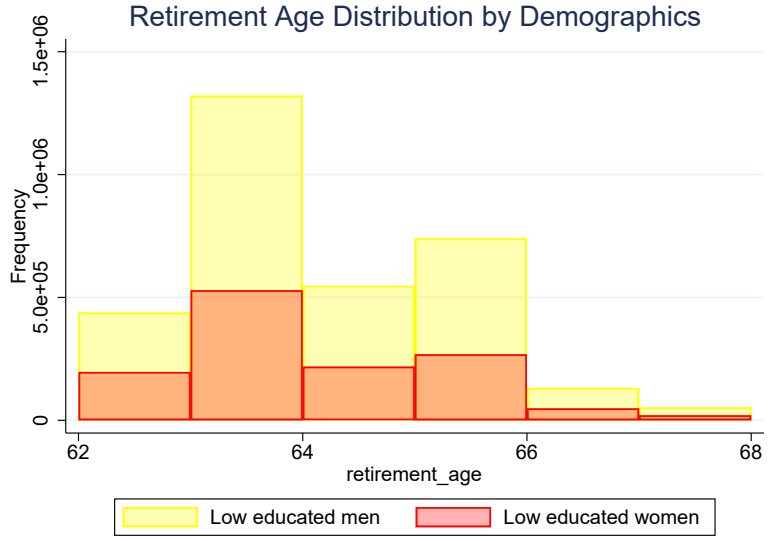
Figure 4.1: Life expectancy at the age of 62 by sex and education. The life expectancy varies significantly between the groups in the figure. If the demographic composition varies considerably by retirement age, that would already result in different life expectancies in different retirement ages.

Notes: Author's calculations using the Finnish Total Statistics on Income Distribution for the years 1995-2020 and Folk data for the years 1988-2020 from Statistics Finland. The sample includes all Finnish residents who retired between 2005 and 2017. Being educated is defined having at least a lower tertiary degree.

Since higher earnings also accrue higher pensions and more insurance coverage, it is also possible to have selection or moral hazard in terms of accrued pensions given the retirement age. The selection can a priori be positively correlated, uncorrelated or negatively correlated with the chosen retirement age and life expectancy. Negative correlation of accrued pensions and life expectancy would imply advantageous selection,



(a) Retirement age distribution of high educated



(b) Retirement age distribution of low educated

Figure 4.2: Observed retirement age distributions by sex and education. The retirement age distribution follows a similar pattern for all of the demographic groups. The age of 63 seems to be the norm for all the groups while only few people postpone retirement past the age of 65.

Notes: Author's calculations using the Finnish Total Statistics on Income Distribution for the years 1995-2020 and Folk data for the years 1988-2020 from Statistics Finland. The sample includes all Finnish residents who retired between 2005 and 2017. Being educated is defined having at least a lower tertiary degree.

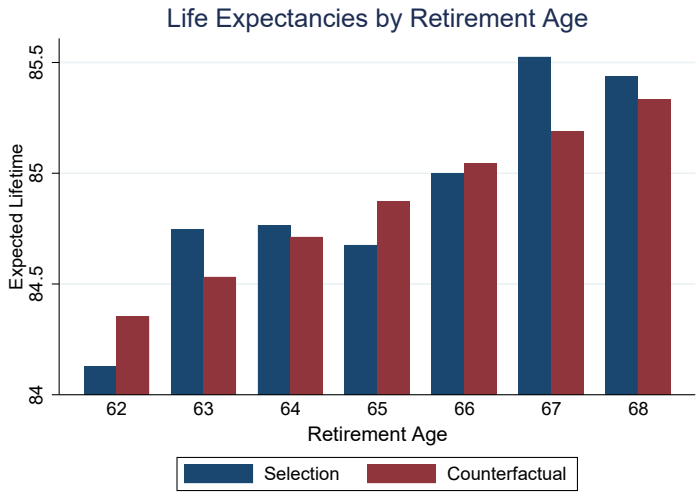
which would reduce the costs to the insurer. The total cost effect to the insurer due to differences in life expectancies would then be a combination of advantageous and adverse selection.

Throughout the remainder of the paper, I conduct three distinct assessments to demonstrate that selection or moral hazard is not a significant concern within the Finnish pension system. Firstly, I estimate a Cox Proportional Hazard Model to show that retirement age is correlated with mortality rates and life expectancy. This test only tells about the mortalities during the time window I observe the people in my sample. It means up to 76-82 years old depending on the retirement age, because the earlies possible retirement was in 2005 and my data ends in 2018.

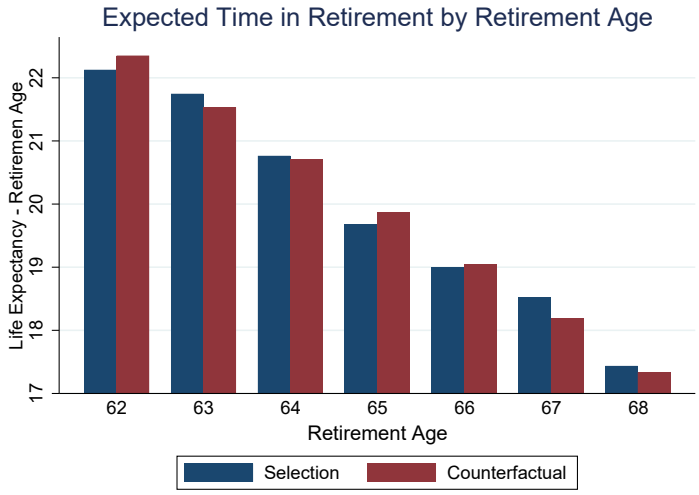
In order to translate mortalities into life expectancies, I extrapolate the mortalities for older ages (past 76 years old) using cross sectional mortality rates by demographic composition. I estimate that people who retire at age 68 live on average 0.9 years longer than people who retire at age 63. The estimated life expectancies are shown in Figure 4.3. However, since these are lifetime expectations conditional on living until retirement age, the condition is different at each retirement age. People who retire at the age of 68 cannot have deceased at the age of 65, while people who retire at the age of 63 could have. Therefore, I compare these life expectancies to counterfactual case, where the mortalities has been harmonised across retirement ages, but the conditioning is at the same age in the counterfactual case. When I do this, the differences between observed and counterfactual life expectancies are negligible.

Secondly, I want to check that also the fiscal effects of the observed correlation are negligible. In order to do that, I estimate the implied cost effects based on differencies in life expectancies. I compare the estimated lifetime costs with different life expectancies to the one where life expectancy is uncorrelated with the retirement age. Since people who retire at age 63 have a longer life expectancy than in the counterfactual, their cost is increased by 1.0 % compared to equal mortalities case. The cost of people who retire at other ages follows no systematic pattern and varies between -1.0 - 1.8 % depending on the retirement age. The aggregated cost is increased by 233 euros per person or 0.07 % of the lifetime pensions due to the differences in mortalities. The aggregated cost for all people retiring between 2005 and 2016 over their expected lifetime is 2.2 million euros. That is 0.0009 % of the total funds in the pension system, or 0.007% of the earnings related pension yearly expenditure, which were 242.4 and 31.4 billion euros at the end of 2022 respectively (Finnish Center for Pensions, 2023b).

Thirdly, to gain a more nuanced understanding, I delve into estimating lifetime pensions based on counterfactual scenarios representing alternative choices pensioners could have made. This approach enables a detailed analysis of how delaying retirement impacts the pension system's costs and how variations in life expectancies influence these costs.



(a) Life Expectancies



(b) Expected Time in Retirement

Figure 4.3: Life expectancies and expected time in retirement by retirement age.

Notes: Author's calculations using the Finnish Total Statistics on Income Distribution for the years 1995-2020 and Folk data for the years 1988-2020 from Statistics Finland. The sample includes all Finnish residents who retired from the private sector between 2005 and 2017 and were working the previous year before retirement. The mortalities are observed from the retirement until the age of 76, after which cross-sectional mortalities from the full population during the years 2005-2018 are used to estimate the life expectancy. Cross-sectional mortalities are calculated in different socioeconomic groups based on sex and education level, which are then used to match the socioeconomic composition of each retirement age. The counterfactual life expectancy is constructed using the weighted average of the hazard rates in different retirement ages, with weights given by the number of retirees at each retirement age relative to the total number of retirees.

In this analysis, I account for differences in expected lifetimes corresponding to individuals' actual retirement age choices. By applying the accrual rules of the pension system and considering the expected lifetime at the actual retirement age, I calculate hypothetical expected lifetime pensions across all potential retirement ages. Unlike the previous section where life expectancies were varied while fixing the retirement age, here I fix the life expectancies for each individual and vary the retirement age choice [2].

This method allows me to estimate the expected change in lifetime pensions for individuals who choose to postpone retirement, under the assumption of no moral hazard or causal effect of retirement timing on life expectancy. In addition, I estimate the counterfactual lifetime pensions for those who initially choose to retire early, had they instead postponed retirement, and derive the corresponding change in expected lifetime pensions. By comparing these changes across individuals retiring at different ages, I assess the relative cost effects of postponing retirement.

Furthermore, I conduct these analyses using counterfactual life expectancies, assuming retirement age is independent of mortality. In both scenarios, the gains from postponing retirement in terms of expected lifetime pensions are outweighed by the additional pension payments made during the postponement period. An exception arises when individuals delay retirement from the early retirement age of 62 to the first old-age pension retirement age of 63. There are also marginal gains observed when delaying retirement from age 63 to 64 for those who actually postpone, but not for those opting to retire at age 63.

Finally, I calculate the difference between these two sets of estimates to quantify how differences in life expectancies affect the cost implications of postponing retirement. The results indicate that variation in life expectancy has only a limited effect on these cost implications.

4.2 Literature Review

The literature on adverse selection starts with the seminal paper of Akerlof (1970), which presents the idea that when sellers have private information about quality, prices influence the composition of goods traded, which may lead to market breakdown. Since then the literature has developed to cover many other areas, especially the market of insurances. Insurance market is particularly prone to adverse selection, since the riskiness of an agent purchasing the insurance has a direct effect on the firm's costs. In the context of pensions, agents with higher expected longevity are more costly to insure. If the agents have private information about the factors affecting their longevity, such as their health status, riskier agents might be more inclined to buy better insurance coverage, which is adverse selection by definition.

[2]That implicitly assumes that changing the retirement age would not change the life expectancy, i.e., no causal effect of retirement on life expectancy and no moral hazard.

According to Wolfe (1983), in a standard economic model the economic incentives lead to a presence of adverse selection in the context of pensions and heterogeneous life expectancies. The actuarial reduction associated with early claiming makes expected present values of different pension streams equal only for those with an average life expectancy. He uses an actuarial model to show that retirees with shorter life expectancy can maximize the expected present value of their benefits by choosing early retirement, while those with high probability of survival can receive greater lifetime benefits by retiring later. He also uses an economic model to argue that working life should be longer for those who expect to live longer. That way adverse selection can increase the average expected present value of benefits for the population as a whole, i.e., the costs of the pension system. He supports his argument with empirical findings. The empirical part of the study finds a relationship between mortality and retirement age, however, leaving open the possibility that retirement itself increases mortality.

Chiappori and Salanie (2000) provide a simple and general test of the presence of asymmetric information, the positive correlation test. The theoretical notion of adverse selection or moral hazard implies a positive correlation between two (conditional) distributions of the choice of a contract and the occurrence of an accident. They use data on contracts and accidents to investigate the extent of asymmetric information in the French market for automobile insurance. They find no evidence for the presence of asymmetric information in this market. However, their positive correlation test has been widely used in the later literature including this work.

Finkelstein and Poterba (2004) test for adverse selection in the UK annuity market. They find systematic relationships between ex post mortality and annuity characteristics, a finding consistent with the presence of asymmetric information. The mortality difference between otherwise identical individuals who purchase back-loaded and non-back-loaded annuities is larger than the mortality difference between male and female annuitants. However, they find no evidence of substantive mortality differences by annuity size. Their results suggest that the absence of selection on one contract dimension does not preclude its presence on others. Their research highlights the importance of considering detailed features of insurance contracts when testing theoretical models of asymmetric information.

Bloom et al. (2006) study the effect of subjective longevity on retirement decisions in the US context. They use survey data from the Health and Retirement Study with subjective survival probability to live beyond age 75 as a proxy for subjective longevity. They instrument the subjective survival probabilities using information on the mortality of their parents. That also addresses the endogeneity problem of the subjective survival probabilities possibly being affected by wealth and retirement decisions. They find that higher subjective survival probabilities lead to greater household wealth (exclusive of social security and pension wealth) among couples, but not among singles, and

no significant differences in the probability of retirement. This finding is consistent with the view that a longer expected lifespan leads to increased wealth accumulation because institutional incentives effectively constrain retirement decisions. Their results also suggest that eliminating institutional barriers on retirement behavior could lead to welfare-enhancing changes in labor supply and savings. That leaves room to study pension systems with more flexible retirement age.

Einav et al. (2010a) use the idea from Chetty (2009) to estimate the welfare loss using sufficient statistic method with less restrictive assumptions. They employ conventional consumer and producer theory to measure the welfare detriment linked to inefficient pricing in insurance markets affected by selection. They demonstrate how this welfare decline can be empirically assessed by leveraging identifying variation in the price. By combining this variation with quantity data, they estimate demand, and alongside cost data, they estimate supply. The gradient of this estimated cost curve serves as a direct assessment for both the presence and characteristics of selection, while combining the demand and cost curves aids in estimating welfare. They exemplify their methodology by applying it to employer-provided health insurance data from one company in the US. Their analysis detect adverse selection, yet they estimate that the quantitative welfare impact is less than 2% of annualized wealth.

Einav et al. (2010b) develop a model of annuity contract choice and estimate it using data from the U.K. annuity market. They find that asymmetric information along the guarantee margin reduces welfare relative to a first-best symmetric information benchmark by about £127 million per year or about 2 percent of annuitized wealth. They also find quantitatively small welfare implications despite of detecting adverse selection.

Fitzpatrick (2015) applies the framework introduced by Einav et al. (2010a) in the context of pensions in the US, yielding similarly minimal welfare effects. She investigates the willingness-to-pay for pension benefits among Illinois public school employees relative to the associated costs. By examining a 1998 policy that permitted these employees to purchase additional retirement benefits, she evaluates the demand for such benefits and compares it to the expected present discounted value. Her analysis reveals that employees are willing to pay 20 cents for every dollar increase in the present value of expected retirement benefits. These findings highlight significant inefficiencies in compensation practices and raise doubts about the effectiveness of deferred compensation schemes in attracting employees.

Beauchamp and Wagner (2012) find in the US context that individuals who live longer opt for larger annuities by delaying benefit claims, indicating adverse selection. Their model shows that adverse selection raises system costs and lowers social welfare by 1-3 percent. Increasing the pension accrual rate, through reducing early retirement adjustments, could save costs and slightly boost social welfare. However, raising the retirement age saves costs but significantly reduces social welfare. Eliminating early

claiming would cut costs but result in substantial welfare losses and a 50 percent rise in disability insurance claims.

Brinch et al. (2018) study the claiming behavior of old age pensions in the Norwegian system, where claiming and retirement decisions are decoupled. In their case, a delay in claiming is equivalent to buying an annuity: current pension benefits are given up in return for a higher future income stream from old-age pensions. They study to what extent early claiming is related to short expected longevity. The idea of their study is that in the case of flexible claiming without earnings testing, potential claimants might take into account that their expected longevities might differ from the average in their birth cohort, and they might exploit this difference to increase expected pension payouts. Such behavior would be an example of adverse selection. They describe these incentives by the concept of Relative Money Worth (RMW), which expresses the relative value of different annuity options relative to the option of claiming early. RMW is a function of expected longevity, which they estimate individually for each claimant. They find that early claiming is related to financial incentives, but the additional costs to the system are modest. They also use parental longevity as an instrument to show that part of the selection is active, people claim early because they gain from doing so. Their study is a good example of the importance of quantifying the effects of adverse selection. They find evidence of adverse selection, but the monetary implications are small. Their study is also in a context of pure insurance decisions, while in many systems including Finland the retirement and claiming decisions are more linked, so it is unclear how their results are applicable to these different contexts.

Beauchamp and Wagner (2020) employ the unused observables version of the positive correlation test in the US context. They establish evidence that individuals anticipating shorter lifespans tend to opt for smaller annuities by claiming benefits early, while also being less costly to insure. This suggests the presence of adverse selection within the system. By utilizing the unused observables approach, they instrument the risk measure to ascertain whether the observed correlation can be deemed causal, thereby distinguishing adverse selection from moral hazard, where causation runs in the opposite direction. They employ parental death ages as proxies for longevity risk, enabling them to identify a causal relationship between longevity risk and claiming age while addressing measurement error issues. To ensure the identification does not stem from early paternal death, which is less likely to have biological causes, they use a spline in the father's death age as an instrument for longevity.

Dushi et al. (2021) compute expected lifetime benefits for individuals claiming between ages 62 and 70 under different mortality assumptions. They use population-average mortality by cohort or average mortality by observed claiming age allowing claiming-age specific mortality. They repeat the analysis distinguishing also lifetime earnings quartile, so they can disentangle the relationships between claiming age, mortality, and lifetime

earnings. They find for men in the US context that late claimants have lower mortality than those who claim at age 62, so late claimants are adversely selected. They find heterogenous changes in the incentives of claiming: the return on delayed claiming has become systematically positive for those who actually delay, but not for those who claim early. They find that aggregate cost effect of selection was 0.5% for the most recent retiring cohorts. Yet, the difference between the highest and lowest quartiles is 2.8% of the expected lifetime benefits.

Nivalainen (2022) studies partial old-age pension claiming in Finland. As of February 2017, Finns have been able to claim a partial old-age pension early at age 61 independent of their working hours or earnings. Hence, the setting is similar to the context of Brinch et al. (2018). She finds a negative connection between the same-sex parent being alive and claiming. The connection disappears after controlling for gender, but the negative connection between the age (at death) of the same-sex parent and the claiming remains significant in a fully adjusted model. Known risk factors for shorter longevity, such as being male, having a lower income and wealth, also increase the likelihood of claiming. The results indicate that people take their expected longevity into account when making pension claiming decisions. This may hold critical implications for actuarial neutrality, the key assumption of the pension system design, and can lead to an unexpected increase in public pension expenditure. A limitation of this study is to concentrate on partial old-age pensions leaving the full retirement decisions still to be investigated.

This study contributes to the literature by being the first to examine asymmetric information in Finland's full-time old-age pension system. It is also politically relevant, as it informs debates on the system's sustainability and actuarial fairness.

4.3 Institutional Context

Pension plans can be classified into two different categories: defined benefit or defined contribution plans. They differ in how they determine the pension benefits provided to employees.

Defined benefit plan specifies a monthly benefit for the rest of the life. The retirement benefit is determined based on a predetermined formula that takes into account factors such as salary history, years of service, and a predetermined accrual rate. Many European pension systems are defined benefit plans.

Defined contribution plan allows the agent to use what they have saved. The eventual benefit depends on the amount saved and investment performance, and the individual assumes the risk of market fluctuations. An example of a defined contribution plan is 401k in the US.

The Finnish pension system is a defined benefit pension plan. Between 2005 and 2016, pensioner were able to choose their retirement age between 62-68 years. The benefit level

was determined over the work history according to the formula

$$\frac{\text{yearly earnings } * \text{accrual\%}}{\text{12 months}} = \text{pension/month}$$

where accrual% is equal to

- 1.5 % for 18-52 years old
- 1.9 % for 53-62 years old
- 4.5 % for 63-67 years old

In case the agent wanted to retire between 62 and 63 years old, the pension was reduced by 0.6% of accrued pensions for each month retiring before the age of 63. Retiring early at the age of 62 instead of 63 cut 7.2 % of the accrued pensions.

In the 2005 reform, an employment-specific calculation model that takes into account the last ten years was renounced. That method excluded the years – perhaps of low earnings – preceding the last ten years from the calculation. This form of calculation model favors long employment relationships, in which the best earnings level occurs during the last ten years. The calculation model was unfavorable for such persons whose career consisted of shorter employments and who had gained their highest earnings during a younger age (Finnish Center for Pensions, 2023a).

Average pension contribution in 2005 was 21.6 % of the salaries where the employees contribution was 4.6 per cent for less than 53 years old and 5.8 per cent for older employees. The average employer contribution was 16.8 per cent and varied between employers.

In order to illustrate the incentives to postpone retirement from the age of 63 years as a function of longevity, I draw an example of a retiree with 18000 euros/year pension and 30000 euros/year salary. The gains of postponing as a function of longevity are shown in Figure 4.4. This figure would look different for every individual depending on the accrued pension level and current salary. In this example, if the individual happens to die before the age of 78 years, the lifetime gain is negative. After the age of 82 years, gains of postponing to 64-68 years become all positive and after 83 years postponing retirement until 68 years old gives the highest returns. As a result, those who think they have a good chance of living a long life have higher incentives to postpone retirement. On the contrary, expecting to die relatively young gives a financial incentive to retire as early as possible.

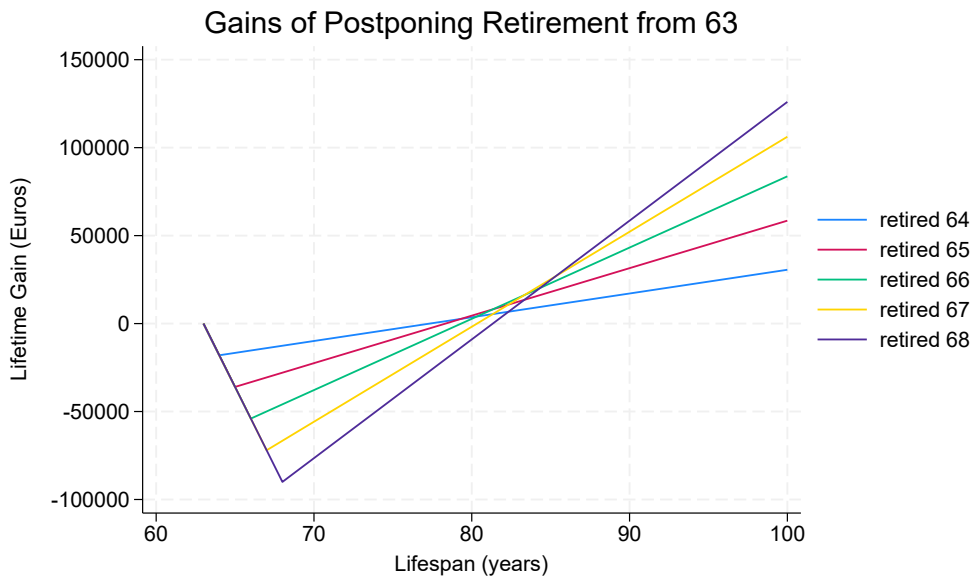


Figure 4.4: Example of Financial Incentives of Postponing Retirement as a function of longevity.

Notes: Author's calculations using retirement accrual rules in a hypothetical example. Accrued pension at the age of 63 is assumed to be 18 000 euros per year, salary during the postponement is assumed to be 30 000 euros per year and accrued pension is 4.5% of the salary for ages 63-67. During the postponement period the individual does not withdraw any pension benefits, but earns a salary which is excluded from the calculations. In this example, if the individual happens to die before the age of 78 years, the lifetime gain is negative. After the age of 82 years, gains of postponing to 64-68 years become all positive and after 83 years postponing retirement until 68 years old gives the highest returns.

4.4 Data and Methodology

4.4.1 Conceptual Framework

Adverse selection is an economic concept that refers to a situation where one party in a transaction has more information than the other, leading to an imbalance of information and potentially undesirable outcomes. This concept is often discussed in the context of insurance markets, financial transactions, and other scenarios involving asymmetric information.

In adverse selection, individuals or entities that possess private information about their own characteristics, risks, or intentions are more likely to engage in transactions that favor them, while the uninformed party faces increased risk or disadvantage. This can lead to a breakdown in the efficiency of markets and suboptimal outcomes.

In the context of pensions risk means longer life expectancy, since people with higher life expectancy are more costly to insure. Instead of contemplating the decision whether to buy the insurance or not, I observe the decision of how much insurance to purchase, i.e., the intensive margin of the insurance decision. People can acquire higher annuities and hence buy more insurance coverage by postponing their retirement or earning higher salaries. Adverse selection in the context of pensions can arise due to differences in individuals' characteristics and expectations regarding their lifespan and financial needs in retirement. Some individuals may have a better understanding or accurate information about how their own health and family history can impact their life expectancy. Also people with higher life expectancy might have a preference to work longer even without such information, since the retirement decision is not a pure insurance decision. Hence the decision to postpone retirement is based only partly on insurance motives and possibly dominated by other motives. Also Chiappori and Salanie (2000) point out that this kind explanation is in fact very close to the initial story; the difference is more a matter of interpretation than of structure. Also, from the point of view of the insurer the motives behind the selection have little importance. If people with higher life expectancies tend to retire later, we have adverse selection in the pension system. If the pension system doesn't account for these differences, those with longer life expectancies may be more likely to purchase more insurance and benefit from the longer stream of retirement income.

The problem of moral hazard rises when the people change their behavior because of having a higher pension level in a way that increases their longevity and hence results in higher claims. Generally that may be considered as desirable behavior and it may feel weird to call such behavior as moral hazard. However, since pension is an insurance against longevity where the costly agents are those who live longer, and moral hazard is defined as behavior that increases the risk the agents are insured for, in this case moral hazard is by definition all behavior that reduce mortality and increase expected lifespan.

Such behavior could be taking better care of themselves, eating healthier and exercising, or having more frequent health checks in order to cure possible diseases early. That kind of behavior could arise for example because with higher income it may be easier to take care of own health. Observationally moral hazard and adverse selection are equivalent without identifying variation in the price of the insurance. Such variation would require exploiting a reform in the pension system and is not an aim of this study. In this case, correlation of retirement age and mortality would still be evidence of asymmetric information, but the interpretation of the causality of the relationship between contract choice and subsequent mortality would be different. Distinguishing between adverse selection and moral hazard is a task of its own merit. However, as the point made by Finkelstein and Poterba (2004), interpreting findings due to moral hazard is arguably weaker for annuity markets than for most other insurance markets. Even recognizing this potential effect, they suspect that its quantitative importance is small, especially in developed countries such as UK or Finland. Also, for the point of view of the cost to the system the reason behind the differences in life expectancies has no importance.

4.4.2 Data

I use Finnish Total statistics on income distribution 1995-2020 and FOLK data 1988-2020 from Statistic Finland. The data include every Finnish person, personal identification codes, personal and household level income data for each person at each year between 1995 and 2018. I restrict my main sample to the people who have been working the year before retirement and who retired between 2005 and 2017. I convert all the income variables into 2018 prices using consumer price index.

For the mortality rates, I use the main sample until the last observed age, which is 76-81 years old depending on the retirement age. From that on, I use the full Folk data during years 2005-2018. I estimate hazards for four demographic groups based on sex and education, high- and low educated males and females. I define high education in terms of having at least tertiary degree. Then I extrapolate these hazards for older ages based on demographic composition of the groups of different retirement ages. The composition of the groups is shown in Figure 4.5.

4.4.3 Hazard Model

To test for adverse selection and moral hazard, I estimate the Cox proportional hazard regression of the form:

$$\lambda(t|X) = \lambda_0(t) \exp(\sum_{j=1}^{5} \beta_j 1(ret_age = 63 + j) \tag{4.1}$$

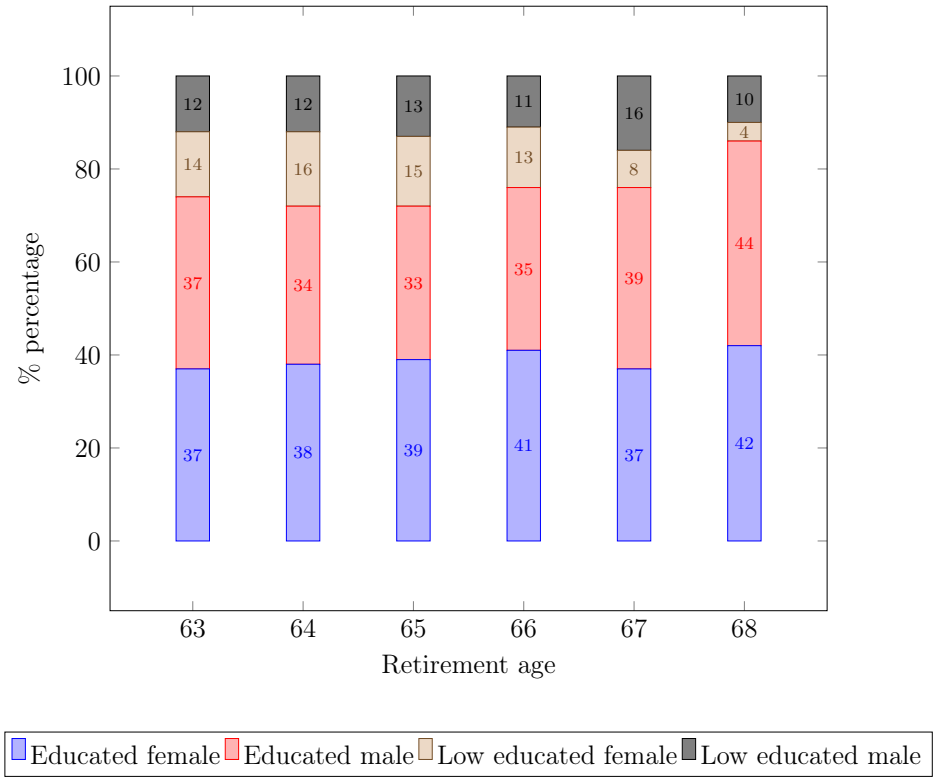


Figure 4.5: Demographic composition by retirement age

Notes: Author's calculations using the Finnish Total Statistics on Income Distribution for the years 1995-2020 and Folk data for the years 1988-2020 from Statistics Finland. The sample includes all Finnish residents who retired from the private sector between 2005 and 2017 and were working the previous year before retirement.

where $\lambda_0(t)$ is the baseline hazard at time t determined by the duration variable (time). The hazard ratios of the previous regression are the quantities $\exp(\beta_j)$:

$$HR = \frac{\text{hazard of retiring at age j}}{\text{hazard of retiring at age 63}} = \frac{\lambda(t|X_j)}{\lambda(t|X_{63})} = \frac{\lambda_0(t)\exp(\beta_j)}{\lambda_0(t)\exp(0)} = exp(\beta_j) \tag{4.2}$$

4.4.4 Conditional Life Expectancies

To calculate life expectancies conditional on retirement age, I use group specific hazard rates. Since I observe people only up to 76-81 years old (depending on the retirement age), I need some kind of extrapolation of hazards for older ages. In order to do that, I use demographic variables (sex and education) to estimate hazard rates at older ages for each group using cross sectional data.Then, I create a survival function for each group based on these estimated hazard rates and use the obtained conditional survival functions to calculate the present value of expected pensions within each retirement age.

I derive life expectancy from the obtained mortality rates. Let q_x be the mortality rate at age x. Hence, the fraction $1 - q_x$ of the people alive at age x will live at least to the age $x + 1$ and the fraction $(1 - q_x)(1 - q_{x+1})$ will make it at least to the age of $x + 2$. Therefore, the fraction of people alive at age x who live at least to the age $x + T$ is given by

$$\prod_{t=0}^{T-1}(1 - q_{x+t})$$

and the fraction of people alive at age x who die at age $T \geq x$ is

$$\underbrace{q_T}_{\text{mortality rate at age } T} * \underbrace{\prod_{t=0}^{T-1}(1 - q_{x+t})}_{\text{fraction of people alive at age } x \text{ who live at least until age } T}$$

Hence, the conditional life expectancy at age x, $62 \leq x < 100$, is given by

$$\sum_{T=x}^{100} T q_T \prod_{t=0}^{T-1}(1 - q_{x+t}) \tag{4.3}$$

The summation ends at age 100, because the estimates for older ages are so noisy that I assume 100 % mortality at the age of 100. Since very few people live that old it has little effect on the results. The results are shown in Figure 4.3.

4.4.5 Expected Lifetime Pensions

The present value of future pensions can be calculated using the formula:

$$\begin{aligned} PV &= \sum_{i=x}^{100} \text{pension at age i} * \left(\frac{1}{1+r}\right)^{i-x} * \text{probability of survival until age i} \\ &= \sum_{i=x}^{100} \text{yearly starting pension} * \left(\frac{1+g}{1+r}\right)^{i-x} * \text{probability of survival until age i} \end{aligned}$$

where PV is the expected present value of pensions, r is the discount rate and g is the growth rate of pensions.

Once the conditional expected lifetime is known and for simplicity assuming that the growth rate of pensions equals the discount rate, i.e., $\left(\frac{1+g}{1+r}\right) = 1$, it is straightforward to calculate the expected present value of pensions over lifetime. In that case it is calculated using formula

$$PV = \text{yearly starting pension} * \text{expected remaining lifetime.}$$

4.5 Results

4.5.1 Cox Proportional Hazard Model

In order to test for the existence of adverse selection and moral hazard, I estimate the Cox Proportional Hazard model in Equation (4.1). The results are shown in Figure 4.6. The baseline is estimated using all the data. However, since these are conditional lifetime expectations conditional on living until retirement age, the condition is different at each retirement age. Since it is impossible for people who retired at age 68 to have died before age 68, I also estimate a sanity check using people who only died older than 68 years old [3]. If I condition on living until 69 years old, these differences vanish and I have no evidence of adverse selection.

In the baseline, all the coefficients are negative and statistically significantly different from zero (hazard ratios are statistically significantly less than a unity). It means that the mortality rates of those who retire later than at age 63 are lower than those who retire at age 63. However, estimating the model on those who died between 69-76 years old gives higher and statistically same hazards for those who retire later.

Since restricting the model on those who died between 69-76 years old ignores what happens between the ages 63-68, I also estimate the Cox Proportional Hazard model of

[3] If I observe someone retiring at age 68, they are alive at the end of that year and first possible year for them to die is the year they turn 69.

binomial choice: retire at age 63 or continue working. For that I take a sample of people who have been working at the age of 62 and either retire or continue working at the age of 63. I also require the possible retirement to happen between 2005-2016. If I estimate the model without further restrictions, I get the hazard ratio of 1.37 (p=0.000) on those who retire, i.e., those who continue working have lower mortalities. However, when I restrict the data between the ages 63-79, which is observable to all, I get the hazard ratio of 1.005 (p=0.227). It means that there are no differences in the relevant mortalities.

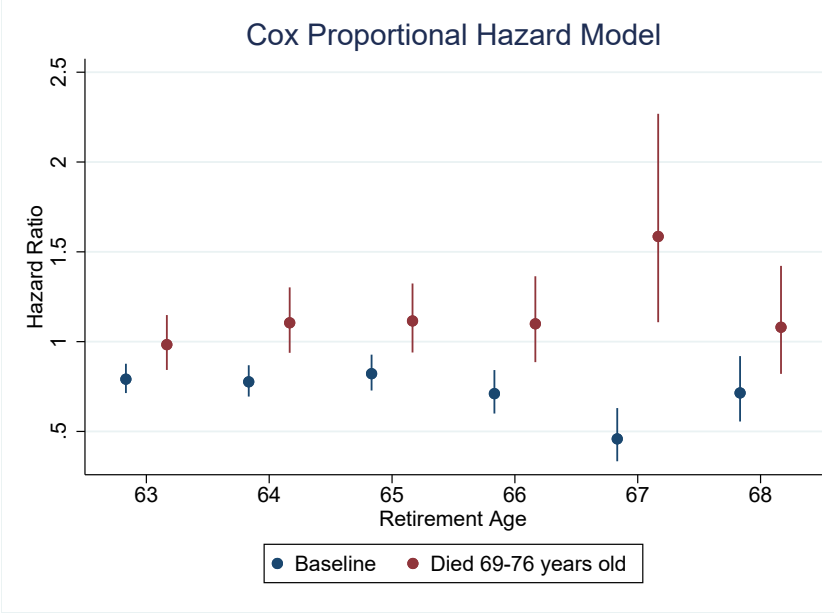
It is worth noting that this kind of test would not tell if the differences in mortalities are due to selection or moral hazard. It can be the case that people who retire later have longer life expectancies irrespective of their retirement age (adverse selection), or that people who retire later start to behave in a way that reduces their risk of dying (moral hazard). Also retiring might have a causal effect on mortality itself, which could be either increasing or decreasing mortality. A hazard ratio of 0.8 means that the mortality risk is 80 per cent of the risk of the reference group, i.e., those who retired at the age of 63. The relationship between hazard ratios and beta coefficients is shown in Equation (4.2).

4.5.2 Survival Functions and Expected Lifetime Pensions

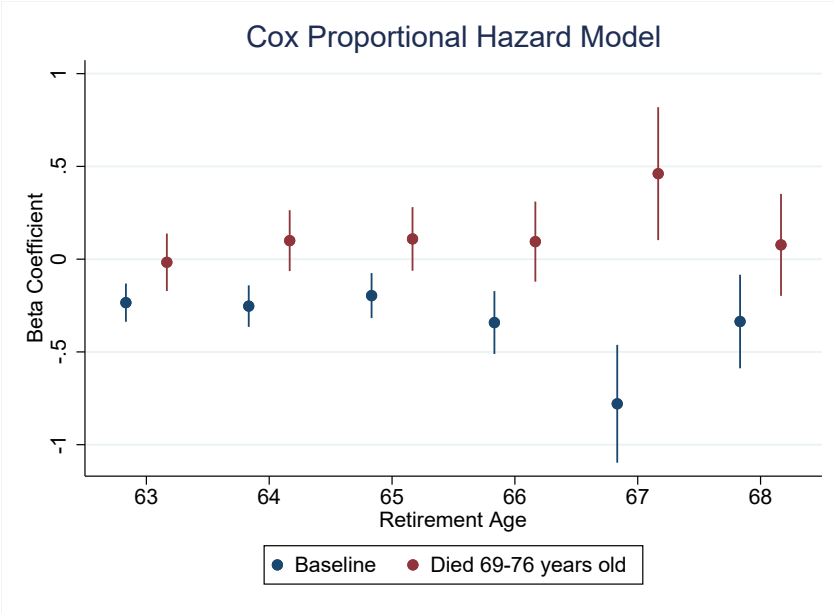
The observed and predicted hazards and corresponding survival functions are shown in Figure 4.7. Until the age of 76, I use observed hazards, and for older ages the cross-sectional predicted hazards. I aggregate these hazards to a counterfactual survival function using population weights to preserve expected lifetime over all retirement ages. The counterfactual survival function is shown in Figure 4.8.

In order to calculate the monetary effect of selection based on differencies in life expectancy, I calculate the expected lifetime pensions in each retirement age using survival functions shown in Figure 4.7 and compare that to expected pensions based on survival function in Figure 4.8. I take number of retirees at different ages and their observed pensions in the period 2005-2016 and assume 3 % discount rate and 3 % growth rate for the pensions. The result is shown in Table A.1. The results are presented in Figure 4.9. Part (a) is expected lifetime pensions assuming either differences (adverse selection) or no differences (counterfactual) in life expectancies and part (b) is the cost of adverse selection (difference in cost due to differences in life expectancies). Adverse selection allows different life expectancies by retirement age and counterfactual assumes the aggregated survival function of Figure 4.8 for everyone irrespective of the retirement age. The total aggregated cost per person due to adverse selection is 233 euros, i.e., the cost is increased by 233 euros per person. This is 0.07% of average expected lifetime pensions.

Since people who retire at age 63 have a longer life expectancy than in the counterfactual, their cost is increased by 1.0 % compared to equal mortalities case. The



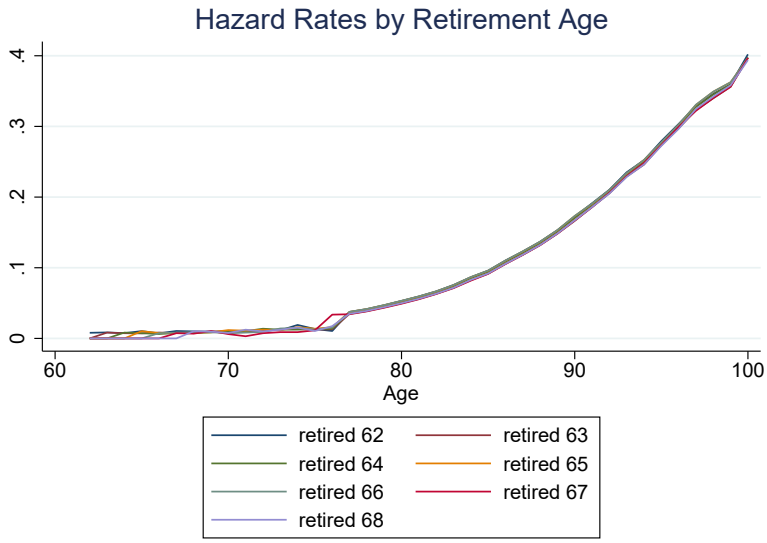
(a) Hazard Ratios



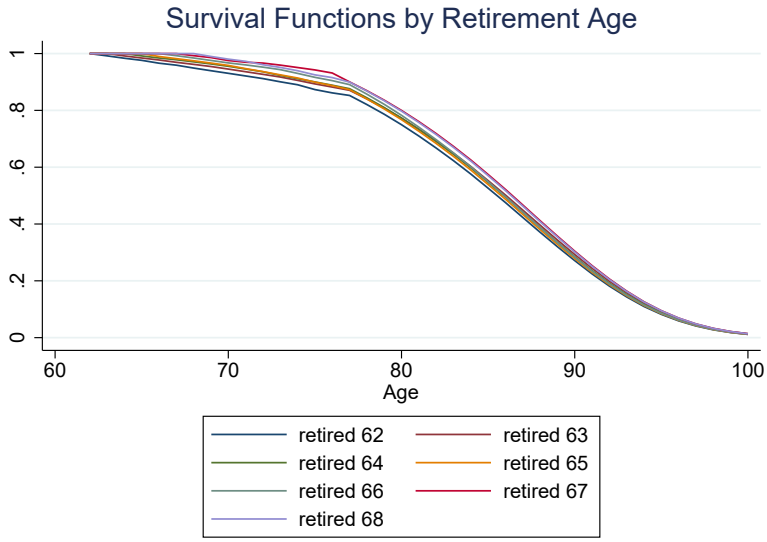
(b) Beta Coefficients

Figure 4.6: Cox Proportional Hazard Model estimation results.

Notes: Author's calculations using the Finnish Total Statistics on Income Distribution for the years 1995-2020 and Folk data for the years 1988-2020 from Statistics Finland. The sample includes all Finnish residents who retired from the private sector between 2005 and 2017 and were working the previous year before retirement. A hazard ratio of 1 or a beta coefficient 0 implies no differences in mortalities between a given group and the reference group.



(a) Hazard Rates



(b) Survival Functions

Figure 4.7: Observed and predicted hazard rates and corresponding survival functions.

Notes: Author's calculations using the Finnish Total Statistics on Income Distribution for the years 1995-2020 and Folk data for the years 1988-2020 from Statistics Finland. The sample includes all Finnish residents who retired from the private sector between 2005 and 2017 and were working the previous year before retirement. The hazard rates being close to each other reflect the small differences in mortalities. The (small) differences in survival functions are mostly due to the conditioning being alive at different retirement ages.

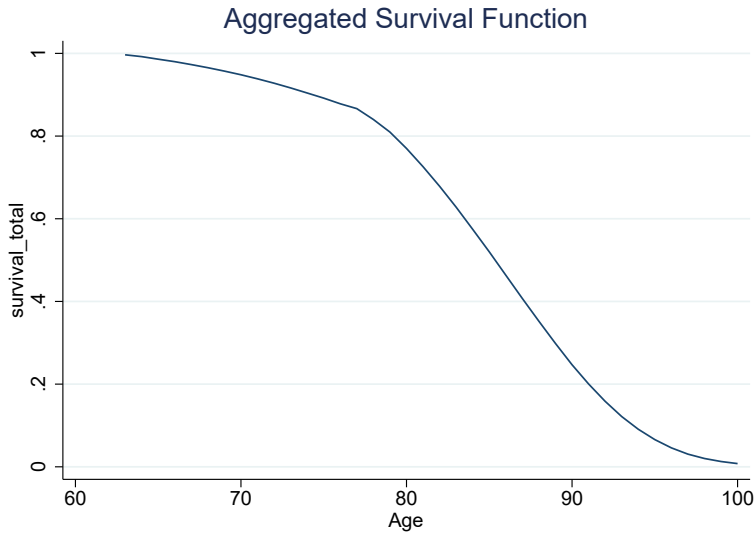


Figure 4.8: Counterfactual survival function.

Notes: Author's calculations using the Finnish Total Statistics on Income Distribution for the years 1995-2020 and Folk data for the years 1988-2020 from Statistics Finland. The sample includes all Finnish residents who retired from the private sector between 2005 and 2017 and were working the previous year before retirement. The counterfactual survival function is constructed by averaging the relevant hazard rates shown in the previous figure, with weights given by the number of retirees at each retirement age relative to the total number of retirees.

Table A.1: Summary of the Results

Retirement age	62	63	64	65	66	67	68
# retirees	21791	58878	25450	32133	5999	1090	1275
Life expectancy	84.1	84.7	84.8	84.7	85.0	85.5	85.4
Counterfactual LE	84.4	84.5	84.7	84.9	85.0	85.2	85.3
Difference (years)	0.2	0.2	0.1	-0.2	-0.04	0.3	0.1
Average pension	24004	24752	24943	23981	26667	32256	33872
Lifetime pensions	332719	367808	276098	266036	303506	353513	342807
Counterfactual	336099	364173	275407	268691	304213	347077	340807
Difference (%)	-1.0	1.0	0.3	-1.0	-0.2	1.8	0.6

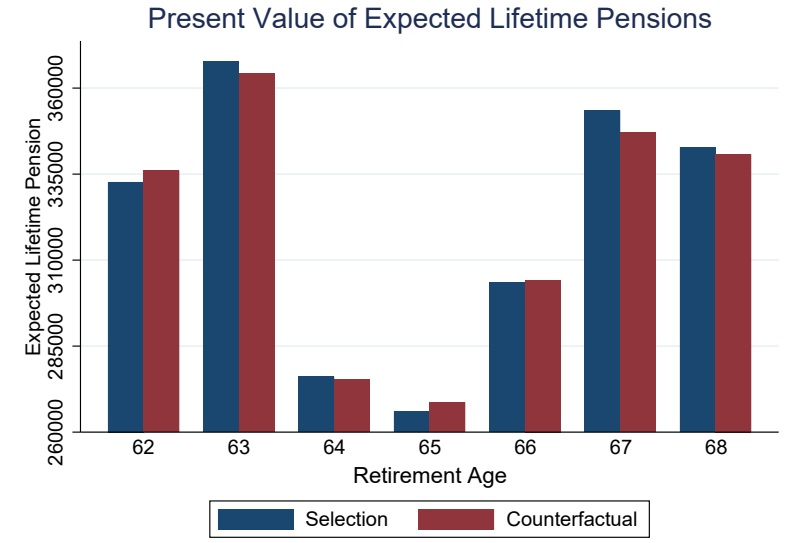
Notes: The table includes my sample of all Finnish people retiring (from private sector) between 2005 and 2016. Life expectancy is based on the observed and predicted hazards, which have been calculated separately for each retirement age. Counterfactual life expectancy harmonizes the hazards across retirement ages, so that the difference in counterfactual life expectancy across different retirement ages comes solely from conditioning being alive at the age of retirement.

cost of people who retire at other ages follows no systematic pattern and varies between -1.0 - 1.8 % depending on the retirement age. The aggregated cost is increased by 233 euros per person or 0.07 % of the lifetime pensions due to the selection. The aggregated cost for all people retiring between 2005 and 2016 over their expected lifetime is 2.2 million euros. That is 0.0009 % of the total funds in the pension system, which was 242.4 billion euros at the end of 2022 (Finnish Center for Pensions, 2023b).

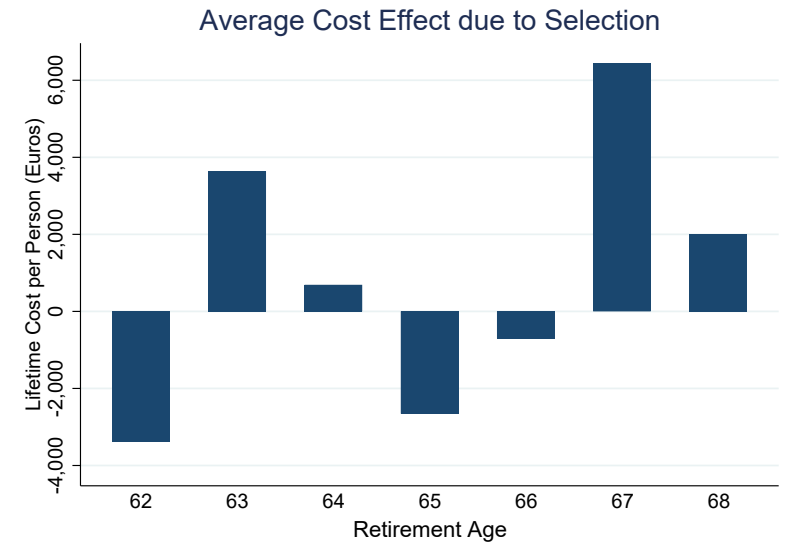
As the third part of my study, I undertake the following steps to analyze the benefit levels and gains associated with different retirement ages: First, for each individual, I calculate the benefit levels at all possible retirement ages. I employ two approaches: forward and backward calculations. In the forward approach, I use the pre-retirement salary and apply an annual increase of 4.5% for each year after the actual retirement age [4]. In the backward approach, I utilize observed salaries to calculate the actual accrual before retirement. Next, I calculate the gains associated with different retirement ages by taking the differences between the present values of the expected pensions at various counterfactual retirement ages. I do these analysis separately for those who postponed and for those who decided to retire at each retirement age. To evaluate the cost to the insurer, I subtract paid pension contributions from the gains. The result is shown in Figure 4.10. In part (a) is allowing adverse selection and part (b) is counterfactual.

We can see that the average gain is positive only for those who retire at the age of 63 instead of 62, or at the age of 64 instead of 63. All the other gains are negative, which means that postponing retirement on average saves the costs to the pension system. This is an important remark, since the system is monetarily discouraging later retirement which is against the intention of the reform in 2005. Also, if the system aims to be

[4]For 62-63 transition, I use 7.2 % as this is the early retirement correction on the pension level.



(a) Lifetime Pensions by Retirement Age



(b) Average Costs due to Adverse Selection

Figure 4.9: Implications of Adverse Selection

Notes: Author's calculations using the Finnish Total Statistics on Income Distribution for the years 1995-2020 and Folk data for the years 1988-2020 from Statistics Finland. The sample includes all Finnish residents who retired from the private sector between 2005 and 2017 and were working the previous year before retirement. I take number of retirees at different ages and their observed starting pensions in the period 2005-2016 and assume 3% discount rate and 3% growth rate for the pensions during retirement for the expected lifetime estimated as described earlier.

actuarially neutral, the net gains should be around zero instead of being negative.

The analysis presented here abstracts from the effects of taxation and income from prolonged employment. If these factors were included, the financial incentives to postpone retirement would be smaller than the estimated gains from the retiree's perspective. From a policymaker's perspective, however, longer careers and higher pensions generate additional tax revenues and therefore support the implementation of stronger incentives for later retirement.

4.6 Discussion and Conclusions

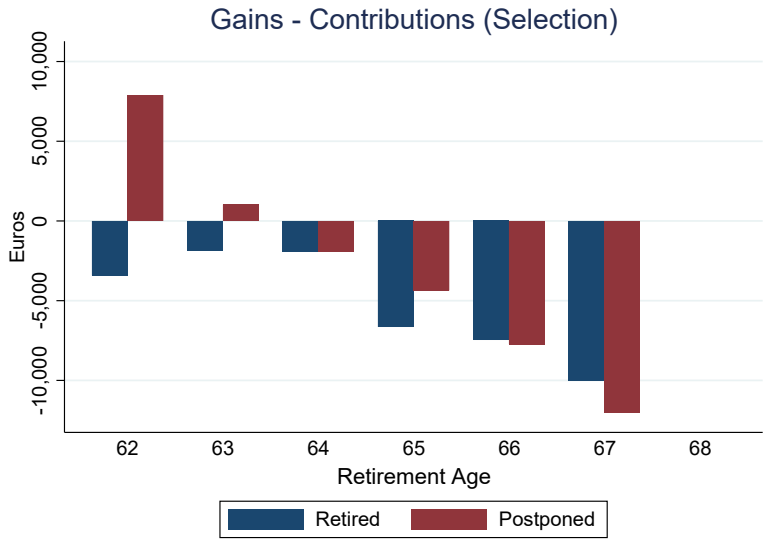
In the course of this study, I find a significant disparities in life expectancies across different retirement ages. However, since these are conditional lifetime expectations conditional on living until retirement age, the condition is different at each retirement age. If I condition on living until 69 years old, these differences are small and statistically insignificant.

By extrapolating hazard rates, I derive estimations revealing that those who opt to retire at the age of 68 tend to enjoy an extended life expectancy of about a year longer in comparison to their counterparts retiring at 63 (conditional on being alive at retirement age). I perform a comparative analysis of different life expectancies against a scenario where life expectancy is assumed to be unrelated to retirement age. The results manifest that individuals retiring at 63, with a comparatively longer life expectancy, witness a corresponding increase in costs by 1.0% in contrast to the equal life expectancy scenario. Conversely, individuals retiring at other ages experience a change in costs ranging from -1.0% to 1.8%, contingent on their retirement age.

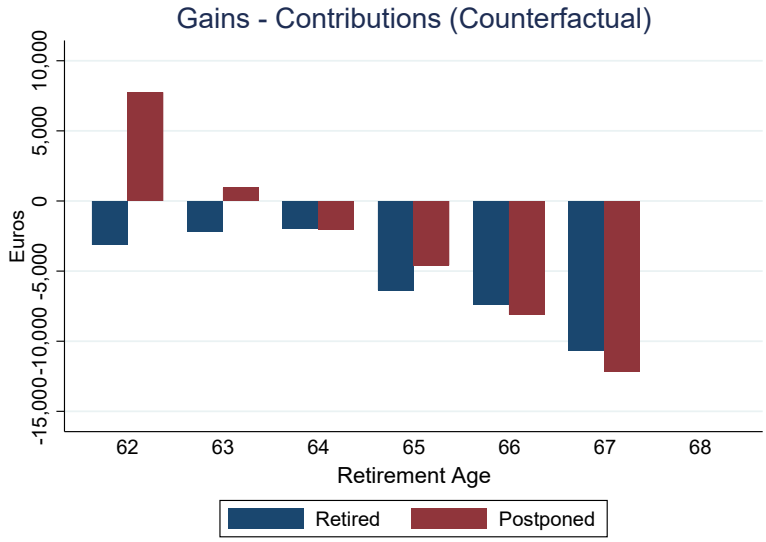
However, on aggregate I find that adverse selection increases costs to the pension system by 233 euros per person who retired between 2005 and 2016 over their expected lifetime (conditional on living until retirement age). That is 0.07 per cent of the average lifetime pensions or 2.2 million euros on the aggregate level, which is 0.0009 per cent of the total amount of pension assets. In other words, the effects are relatively small compared to the average lifetime pensions or to the total funds in the pension system.

I also show that postponing retirement reduces the costs of the pension system: people gain less than they contribute over their expected lifetime when they postpone. It means that the pension system is monetarily discouraging later retirement which is against the intention of the reform in 2005 and the principle of actuarial fairness. I compare the gains in the adverse selection case to the equal life expectancy case. I do this both assuming differences in life expectancies as well as with the equal life expectancy assumption. The differences in life expectancies have only a minor effect on the costs associated with postponing retirement.

In conclusion, I provide strong evidence that adverse selection was not a significant



(a) Adverse Selection



(b) Equal Life Expectancy

Figure 4.10: Net Costs to the Pension System

Notes: Author's calculations using the Finnish Total Statistics on Income Distribution for the years 1995-2020 and Folk data for the years 1988-2020 from Statistics Finland. The sample includes all Finnish residents who retired from the private sector between 2005 and 2017 and were working the previous year before retirement. The bars show the change in lifetime pensions net of pension payments for a one year postponement at a given retirement age, comparing individuals who retired at that age with those who postponed retirement. Panel (a) assumes observed mortality differences by retirement age, and panel (b) assumes equal mortality across retirement ages. Expected lifetimes are estimated as described earlier.

concern in the Finnish pension system during the period 2005–2016. Nevertheless, it is important to acknowledge that the estimation of life expectancies and their monetary implications relies on assumptions regarding mortality at advanced ages. These assumptions introduce uncertainty that may affect the results. Access to more detailed health data would allow for more precise extrapolation of mortality rates. Moreover, the pension system underwent substantial reform in 2017, and the effects of asymmetric information may have changed under the new rules. Once sufficient post-reform data become available, reassessing these effects will be an important avenue for future research.

4.7 APPENDIX

Suunnitteluosasto
Ville Merilä

22.10.2020

LIITE 1. Tilastokeskuksen toimittamat, viideltä vuodelta tasoitetut ikäkohtaiset kuolemanvaaraluvut (q_x) sekä niistä elinaikakertoimen laskentaa varten johdetut l_x- ja L_x-luvut vuosille 2009 ja 2021.

Liite 1.

Vuosi	2009				2021			
	Kuolemanvaaraluvut 2003−2007 (q_x) sekä niistä johdetut l_x- ja L_x -luvut				Kuolemanvaaraluvut 2015−2019 (q_x) sekä niistä johdetut l_x- ja L_x -luvut			
Ikä, x	**q_x , o/oo**	**l_x**	**L_x**	**$1{,}02^{-(x+0.5-62)} * L_x / l_{62}$**	**q_x , o/oo**	**l_x**	**L_x**	**$1{,}02^{-(x+0.5-62)} * L_x / l_{62}$**
62	9.59	1.00000	0.99521	0.98540	7.68	1.00000	0.99616	0.98635
63	10.29	0.99041	0.98531	0.95648	8.57	0.99232	0.98807	0.95915
64	11.05	0.98022	0.97480	0.92772	9.46	0.98382	0.97916	0.93187
65	12.31	0.96939	0.96342	0.89891	10.51	0.97451	0.96939	0.90448
66	13.02	0.95745	0.95122	0.87012	11.10	0.96427	0.95892	0.87716
67	13.69	0.94499	0.93852	0.84167	11.59	0.95356	0.94804	0.85021
68	15.26	0.93205	0.92494	0.81323	12.90	0.94251	0.93643	0.82333
69	16.54	0.91783	0.91024	0.78461	14.17	0.93035	0.92376	0.79627
70	18.40	0.90265	0.89434	0.75579	15.47	0.91717	0.91008	0.76909
71	20.32	0.88604	0.87704	0.72663	16.87	0.90298	0.89536	0.74182
72	21.98	0.86803	0.85849	0.69733	18.29	0.88775	0.87963	0.71449
73	25.28	0.84895	0.83822	0.66751	20.22	0.87151	0.86270	0.68700
74	26.72	0.82749	0.81644	0.63741	21.36	0.85389	0.84477	0.65953
75	30.65	0.80538	0.79304	0.60701	24.51	0.83565	0.82541	0.63178
76	33.10	0.78070	0.76778	0.57615	27.10	0.81517	0.80412	0.60342
77	38.32	0.75486	0.74039	0.54470	30.12	0.79308	0.78113	0.57468
78	43.19	0.72593	0.71025	0.51228	33.51	0.76919	0.75630	0.54550
79	47.67	0.69458	0.67802	0.47945	39.01	0.74341	0.72891	0.51543
80	53.66	0.66147	0.64372	0.44627	43.30	0.71441	0.69895	0.48455
81	60.03	0.62597	0.60718	0.41268	47.18	0.68348	0.66736	0.45358
82	66.50	0.58840	0.56883	0.37904	54.35	0.65123	0.63354	0.42215
83	76.00	0.54927	0.52840	0.34519	61.11	0.61584	0.59702	0.39002
84	83.82	0.50752	0.48625	0.31143	70.86	0.57820	0.55772	0.35720
85	94.07	0.46498	0.44311	0.27823	79.67	0.53723	0.51583	0.32390
86	106.44	0.42124	0.39882	0.24551	93.10	0.49443	0.47142	0.29020
87	117.49	0.37640	0.35429	0.21382	103.84	0.44840	0.42512	0.25657
88	130.85	0.33218	0.31045	0.18369	116.40	0.40184	0.37845	0.22393
89	149.96	0.28871	0.26707	0.15492	131.79	0.35506	0.33167	0.19240
90	162.49	0.24542	0.22548	0.12823	149.50	0.30827	0.28523	0.16221
91	188.32	0.20554	0.18619	0.10381	165.24	0.26218	0.24052	0.13411
92	202.56	0.16683	0.14994	0.08196	184.72	0.21886	0.19865	0.10859
93	225.06	0.13304	0.11807	0.06327	206.59	0.17843	0.16000	0.08575
94	238.53	0.10310	0.09080	0.04771	227.41	0.14157	0.12547	0.06592
95	257.39	0.07851	0.06840	0.03523	255.25	0.10938	0.09542	0.04915
96	283.97	0.05830	0.05002	0.02526	272.77	0.08146	0.07035	0.03553
97	309.15	0.04174	0.03529	0.01747	301.22	0.05924	0.05032	0.02491
98	332.80	0.02884	0.02404	0.01167	326.78	0.04139	0.03463	0.01681
99	349.08	0.01924	0.01588	0.00756	339.67	0.02787	0.02313	0.01101
100	1000.00	0.01252	0.00626	0.00292	1000.00	0.01840	0.00920	0.00429
101		0.00000				0.00000		
$\sum 1{,}02^{-(x+0{,}5-62)} * L_x / l_{62}$				**16.778288**				**17.664322**

Elinaikakerroin **0.94984**

* Tilastokeskus julkaisee kuolemanvaaraluvut ikään 99 saakka, mistä johtuen elinaikakertoimen laskenta päätetään ikään 100. Laskentateknisesti tämä tehdään asettamalla q_x = 1000 iässä 100.

Table A.2: Life expectancy by age calculated from mortality rates

Age	Conditional life expectancy	Mortality
62	82.69618	.00959
63	82.89658	.01029
64	83.10345	.01105
65	83.3169	.01231
66	83.5452	.01302
67	83.77664	.01369
68	84.00951	.01526
69	84.25759	.01654
70	84.5142	.0184
71	84.78627	.02032
72	85.07221	.02198
73	85.366	.02528
74	85.68671	.02672
75	86.00756	.03065
76	86.35561	.0331
77	86.71011	.03832
78	87.09703	.04319
79	87.50767	.04767
80	87.93353	.05366
81	88.38338	.06003
82	88.85491	.0665
83	89.34324	.076
84	89.86497	.08382
85	90.40155	.09407
86	90.96244	.10644
87	91.55356	.11749
88	92.15978	.13085
89	92.78604	.14996
90	93.45396	.16249
91	94.12408	.18832
92	94.8489	.20256
93	95.57256	.22506
94	96.31968	.23853
95	97.04631	.25739
96	97.75558	.28397
97	98.45182	.30915
98	99.10149	.3328
99	99.65092	.34908
100	100	1

Notes: Mortality rates are from the Finnish Center for Pensions, as shown in the previous table. Conditional life expectancy is calculated using Equation (4.3).

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