London School of Economics and Political Science

# Essays on Inequality of Opportunity: Measurement, Drivers and Consequences

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# Declaration of Authorship

I certify that the thesis I have presented for examination for the PhD degree of the London School of Economics and Political Science is solely my own work.



I declare that my thesis consists of 55,275 words (excluding references).

# Declaration of Editorial Help

I confirm that a first version of this thesis was proofread for conventions of language, spelling and grammar by Clare Sandford.

# Declaration of Funding and Support

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# Abstract

I study Inequality of Opportunity (IOp), its measurement, effects, and relationship to intergenerational persistence using empirical analysis based on data for high-income countries. Inequality of opportunity (IOp) is the part of inequality of outcomes attributable to differences in inherited circumstances. I provide evidence that IOp is higher than previously estimated, that it reduces economic growth, and that it accounts for an important part of intergenerational persistence.

Chapter 2 provides lower and upper bound estimates of IOp for 24 European countries, between 2005 and 2011, using EU-Statistics on Income and Living Conditions (EU-SILC) data. Most estimates of IOp are lower bounds of its true level and provide a partial view of the importance of involuntarily inherited factors. My upper bound estimates account for up to 90.5%, almost as high as total inequality of outcomes. Inequality of outcomes is strongly correlated with the upper bound estimates of IOp, suggesting a close relationship between the two.

Chapter 3 studies the effect of my upper bound estimates of IOp on short-term economic growth using System GMM regressions applied to data for 27 European countries covering the period 2005-2011. A one-standard-deviation increase in IOp results in a decrease in growth rates ranging from 1.2 to 3.1 percentage points. Inequality of outcomes also has a statistically significant effect on growth, albeit much less robust. These estimates suggest that while all income inequalities might hinder growth, IOp is particularly harmful.

Chapter 4 studies the relationship between IOp and the intergenerational elasticity for individual earnings and family income. Using the Panel Study of Income Dynamics (PSID) I find that circumstances account for around 55% of the IGE. Parental education accounts for a quarter of the total contribution of circumstances, reflecting the importance of educational inequalities in the U.S.A. I also find that childhood circumstances have an important influence on the income of the offspring that is not accounted for by the IGE.

# Contents

# List of Tables

# List of Figures

# Chapter 1

# Introduction

## 1.1 Background

The concept of equality of opportunity is frequently used in policy discourse. National governments, policy think tanks, NGOs and international organizations use phrases such as a 'level playing field' or an 'equal starting point', both concerning equal opportunity. But because its use is so widespread – with people across the political spectrum referring to it as their goal – it can have different and even contradictory definitions. Depending on how the term 'inequality of opportunity' is interpreted, it can represent anything from no discrimination on the grounds of sex, race or religion, to a meritocracy, to a radical position that calls for a large amount of redistribution (Swift, 2013).

In this introductory chapter I explain the concept of equality of opportunity. I also discuss the intrinsic and instrumental arguments in favour of equal opportunity and provide a summary of current approaches and estimates. I finish by describing the main contributions of my thesis and by providing a brief outline of the chapters that follow.

### 1.1.1 What is Equality of Opportunity?

Are all inequalities equally objectionable? Intuitively, the answer depends on what drives said inequality. Experimental evidence finds that, when asked to distribute certain rewards such as income or other resources, people are averse to inequalities when they arise from bad luck and that they tolerate inequalities due to choice (Alesina and Angeletos, 2005; Almås et al., 2010; Cappelen et al., 2007, 2010; Mollerstrom et al., 2015). We treat inequalities due to inherited circumstances, over which we have no control, differently from inequalities due to effort or choice. Inequality of opportunity is concerned with those inequalities over which we have no control.

The literature on inequality of opportunity follows up on that intuition. This research stems from the work of luck-egalitarians in political philosophy, who propose that society should equalise opportunities to achieve an outcome rather than equalise the outcome itself (see, e.g., Dworkin (1981a,b); Arneson (1989); Cohen (1989)). Luck egalitarians recognise the role of autonomous choices and focus not only on the distribution of a given outcome but also on the factors that determine that outcome, particularly those that we consider morally objectionable.

The idea that choices and luck have different normative implications permeated early research on economic inequality. As a result, several theoretical and empirical efforts were made to determine the existence of 'unfair' inequalities. Early research from Roemer (1993), Van de gaer (1993) and Fleurbaey (1994) proposed a non-welfarist approach to defining inequality of opportunity, in that they focused not on the outcome (or the welfare given by that outcome) but on how that outcome – and its distribution – came to be. This first wave of economic research was followed by Roemer (1998), who proposed a simple algorithm to determine the extent of inequality of opportunity by splitting the sources of inequality into two factors: efforts and circumstances. Effort, on one hand, represents tolerable sources of inequality, typically choices. On the other hand, circumstances are involuntarily inherited sources of inequality and represent the unfair component of inequality.

Roemer (1998) frames his algorithm in terms of policies that reduces inequality of opportunity. Suppose we can identify all relevant circumstances and that they can be correctly measured as a vector of $n$ components. The population is then partitioned into types: groups that share the same values for each of the $n$ circumstances. For example, all women born in the same region, whose parents had higher education, etc. An equal-opportunity policy will distribute the same amount of resources to each individual in a specific type, assigning additional resources to the worse off types.

Given their type and available resources, individuals are free to choose their level of effort (defined here as a one-dimensional factor). While the level of effort is a choice, the within-type distribution of effort is itself a circumstances. To measure inequality of opportunity, Roemer (1998) proposes to compare individuals from different types that are at the same position in their own type's effort distribution, say, at the median. Assuming that effort is monotonically associated with the outcome, being in the median of their type's effort distribution implies being in the median of their outcome's distribution. The higher the differences in outcomes among people in the same position of their type's distribution, the higher the level of inequality of opportunity.

Roemer proposes that the equal-opportunity policy should equalise the outcome for those at the same quantile of the distribution of effort, irrespective of their type. In practice, such a policy result in complex solutions. Roemer argues that there will not be a single distribution of resources that achieves this goal across all quantiles, in fact, there will be as many distributions as there are quantiles. As a comprise, he then proposes to focus on a single and feasible point, such as equalising the average of each type distribution.

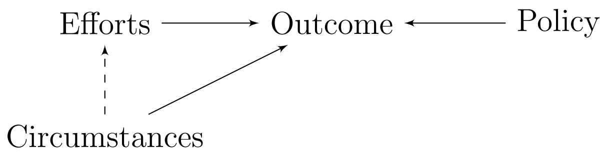
The Roemer (1998) algorithm exemplifies the empirical challenges of estimating inequality of opportunity. First, it requires a large number of observations to properly account for each type's distribution. Because each type represents a unique combination of circumstances, the number of types grows exponentially with each additional circumstance. Even if circumstances have only two categories (say, high or low parental education), the number of types equals $2^n$ where $n$ is the number of circumstances. With a large number of circumstances is highly likely that some types will have no observations in a given survey. For that reason, several authors such as Bourguignon et al. (2007) have proposed regression-based approaches as an alternative. Second, in theory, the Roemer (1998) algorithm (as well as most of the following approaches) requires we identify and measure all relevant circumstances. Any less than that and we will underestimate the true extent of inequality of opportunity. For example, if we exclude gender as a circumstance, types will include both women and men, and their differences in outcomes will be attributed to effort. This issue results in 'lower bound' estimates of the true level of inequality of opportunity (Ferreira and Gignoux, 2011). I explicitly address this issue in Chapter 2.

The initial contributions into inequality of opportunity have since expanded into a large body of research in economics. Many new approaches to measuring inequality of opportunity and multiple applications to several countries have been developed, as shown in the many surveys and handbook chapters written in the last few years (Bourguignon, 2018; Ferreira and Peragine, 2016; Ramos and Van de gaer, 2016; Roemer and Trannoy, 2015), as well as the appearance of equalchances.org, a global database with measures of inequality of opportunity for almost 50 countries.

In this thesis, inequality of opportunity is best defined by the four concepts detailed in Roemer (1998): the outcome of interest, circumstances, efforts and the policies to address it. Circumstances, effort and policies shape the outcome, while circumstances partly determine efforts. The interaction between these concepts is displayed in Figure 1.1.[1]

[1] Bourguignon (2018) proposes a more complex model for the relationship between individual circumstances, opportunities, and outcomes, in which he accounts for the role of preferences and luck, as well as the recursive nature of the model (see Figure 5.1 in that paper).

Figure 1.1: Inequality of opportunity framework




Note: Continuous lines represent a direct influence. The dashed line from circumstances represents the indirect influence of circumstances (Roemer, 1998).

The outcome is our variable of interest, through which we measure inequality of outcomes and inequality of opportunity. Typically studied outcomes in the literature include income, health, education and – more recently – wealth (Salas-Rojo and Rodríguez, 2021) or multidimensional measures (Kobus et al., 2020). My focus is on frequently individual earnings and family income, as using well-known outcomes allows me to compare my work to previous studies and to place my research in a broader context.

Swift (2013) propose three definitions of inequality of opportunity that depend on what we consider a circumstance to be. 'Minimal' inequality of opportunity is concerned with discrimination on the grounds of race, gender, religion, or other personal characteristics. 'Conventional' inequality of opportunity adds to that our socioeconomic background, where our parents' characteristics are also circumstances. 'Radical' inequality of opportunity also includes innate "talent" or "abilities" as a circumstance. While the conventional definition is frequently chosen to define inequality of opportunity – due to philosophical criteria as well as for data availability –, Swift (2013) argues that only a radical definition is consistent with the idea of circumstances as 'morally arbitrary' factors. Torche (2015) holds a similar point of view, arguing that it is hard to disentangle socioeconomic background from innate talent. I discuss the implications of the different definitions of inequality of opportunity throughout the thesis.

Effort is the counterpart to circumstances. It represents the choices and decisions we take that shape our outcomes. Effort reflects the contribution of autonomy to inequality of outcomes, and therefore the part of inequality for which we are responsible. As Figure 1.1 shows, circumstances affect effort, sometimes called the indirect effect of circumstances. For example, differences in parental investment in their children (either financial or in time) are strongly related to differences in school grades, graduate education or occupational success, commonly used measures of effort (see, e.g., Calarco (2014); Schneider et al. (2018)). Throughout my thesis, I consider the influence of circumstances on effort as a circumstance and therefore as part of the share of inequality of outcomes that is accounted for by inequality of opportunity. However, whether the correlation between effort and circumstances is a source of inequality of opportunity or not is a matter of debate (see e.g., Jusot et al. (2013)).

Another matter of debate is whether individual preferences should be considered circumstances or effort. These debate can be summarised by the 'control' and 'preference' views. The control view, represented by Cohen (1989) among others, considers preferences to be circumstances, as they are influenced to a great extent by our upbringing. In Cohen's conception of inequality of opportunity, preferences fall within the scope of personal responsibilities only when we have control over them. The preference view, on the other hand, is held by Fleurbaey (2008) and others, and considers preferences as effort to the extent that you can identify with them, thus excluding addictive or self-harming behaviours but including most individual decisions. While Roemer subscribes with the control view (see, e.g., Roemer (2004); Roemer and Trannoy (2016)), he ultimately argues that the final decision is a matter of democratic deliberation.

The last concept is policies, the interventions that modify the allocation of the outcome, such as taxation or social spending, and reduce inequality of opportunity. Equal opportunity policies aim to reduce the influence of circumstances on outcomes and promote the influence of effort. These policies typically follow the compensation and reward principles, which I discuss later in this section. My thesis does not address specific policies, but I discuss policy implications throughout the thesis.

### Other approaches to study the parent-offspring relation

Inequality of opportunity is one of many different approaches to study how parental and offspring outcomes are associated. Together with inequality of opportunity, Björklund and Jäntti (2020) identify three other approaches: intergenerational immobility, intergenerational effects, and sibling correlations. These approaches reflect different aspects of the intergenerational transmission process and the importance of family background. Among the four approaches, inequality of opportunity is the only approach that makes explicit normative assumptions about the legitimacy of the parent-offspring relationship by splitting the determinants of offspring outcomes into circumstances and effort. Björklund and Jäntti (2020) highlight the lack of comparative papers that study these approaches together, an important motivation for Chapter 4.

Intergenerational immobility summarises the relationship between parents and offspring for a given outcome such as income or education, usually using an elasticity (the estimated coefficient of a log-log regression). Other measures of intergenerational association are the rank-rank correlation or the absolute measure of mobility, i.e., the share of offspring that have an income higher than their parents (see Jäntti and Jenkins (2015)). These measures are commonly used by researchers to quantify the importance of the parent's socioeconomic position in determining the position of the offspring and are sometimes interpreted as measures of inequality of opportunity. I discuss this interpretation of intergenerational immobility as measures of inequality of opportunity in Chapter 4.

Summary measures of the relationship between parents and offspring can also be estimated non-parametrically. Usually through transition matrixes where the rows represent the parent's position in the income distribution and the columns are the offspring's position. These positions can be either relative (e.g., quintiles or percentiles) or absolute (e.g., inflation-corrected income brackets). By construction, relative measures are a zero-sum game as someone experiencing upwards mobility must result in someone else experiencing downwards mobility. Absolute measures, on the other hand, potentially allow for everyone to experience upwards mobility.

In recent years, absolute mobility estimates have been used by Raj Chetty and his coauthors to capture intergenerational mobility trends in the U.S.A. using administrative tax records at the commuting zone level (Chetty et al., 2014). Since, they have used these estimates to study the effect of the neighbourhood, county, and college allocation on intergenerational mobility (Chetty and Hendren, 2018a,b; Chetty et al., 2020). This line of work has highlighted the high heterogeneity in regional mobility patterns within a country and has given rise to an extensive literature in other countries (see, e.g., Corak (2017) for Canada, Heidrich (2017) for Sweden, and Eriksen and Munk (2020) for Denmark).

Other approaches have looked at the drivers behind the intergenerational association between parents and offspring. This 'intergenerational effects' literature studies the causal effect of specific factors such as parental income or parental education through three mechanisms: by comparing monozygotic twins, by studying adopted children, and through the use of instrumental variables. Björklund and Jäntti (2020) find that intergenerational effects are usually smaller than an equivalent intergenerational mobility estimate. However, intergenerational effect estimates are not directly comparable with intergenerational immobility estimates because they account for additional covariates, similarly to inequality of opportunity estimates.

Sibling correlations are another approach to summarise intergenerational persistence. Roughly speaking, this literature measures the contribution of family background by accounting for the shared upbringing among siblings. The correlation in, say, income among siblings captures all factors that are common between them, but that still excludes some circumstances. Björklund and Jäntti (2020) highlight how genetic differences, differences in how siblings are treated, or changes in family composition or neighbourhoods will not be captured by this correlation. For that reason, sibling correlations are a lower bound of the importance of family background, although not necessarily lower bound estimates of inequality of opportunity. Standard inequality of opportunity estimates might not account for the whole of family background, but they might account for circumstances that go beyond family background, such as neighbourhood or place of birth. In sum, sibling correlations are a good complement of inequality of opportunity estimates, as they focus solely on the importance of family background.

These estimates do not convey the same information as inequality of opportunity estimates. Intergenerational mobility estimates might capture factors not considered among circumstances, something I explore in Chapter 4. Intergenerational effects identify the effect of one specific channel or factor, while sibling correlations only capture the importance of the family background to the extent that it is shared among siblings. Despite their relationship, there has been limited interactions between these approaches. Together, these estimates provide a broad view of the interplay between parents and offspring and they all have important complementaries with inequality of opportunity estimates.

## 1.2 Why does Equality of Opportunity matter?

Most people would agree that pursuing equality of opportunity is a laudable goal, but not for the same reasons. There are two types of arguments about why equality of opportunity matters. First, equality of opportunity matters intrinsically or normatively: it promotes a fairer distribution, one where circumstances play no major role. Second, equality of opportunity matters instrumentally, because of its consequences. Equality of opportunity creates an efficient allocation of rewards to those that deserve them (be it income, wealth, a job position, access to higher education, etc.). Conversely, inequality of opportunity constrains people's access to rewards on the grounds of arbitrary circumstances. I discuss these two lines of argument in this section.

Inequality of opportunity is a captivating concept because it highlights the importance of a fairer distribution of outcomes, where fairness is defined as circumstances playing no role in determining outcomes. If inequality of opportunity is high, then effort is not enough to achieve certain rewards in society because other factors such as parental social networks and inherited financial or cultural capital become just as important in allocating these rewards. Ahrens (2020) finds in the European Social Survey (Wave 9) that the support for 'equity' (i.e., a society where hard-working people can earn more than others) is higher than for 'equality' (i.e., a society where income and wealth are distributed equally), and as high as the support for a society that cares for those in need. Because of its relation to fairness, reducing inequality of opportunity is a common policy goal for many countries and institutions. For example, the European Commission has included equal opportunities as one of its three pillars of social rights.[2] Chapters 2 and 4 are concerned with this line of argument, the first by measuring the upper bound of inequality of opportunity and the third by determining how much of the intergenerational elasticity is accounted for by circumstances.

The second argument in favour of equal opportunity is that inequality of opportunity is inefficient. Early research focused on how greater inequality of outcomes reduces growth by modelling this relationship through the distortionary role of taxation, sometimes called a 'political mechanism' (see, e.g., Alesina and Rodrik (1994); Benabou (1996); Persson and Tabellini (1994)). In this line of research, high inequality countries have a higher demand for redistribution, higher tax rates and, as a result, lower growth. These models share two common characteristics: they all use inequality of outcomes as their measure of inequality and it is the redistribution of income that has a damaging effect in the economy. These two aspects together denote a model where inequalities are to be tolerated to promote growth. As Persson and Tabellini (1994) put it, higher taxation results in more private appropriation of the fruits of individual effort.

In contrast to the 'political mechanism', another line of research has focused on the role of liquidity constraints and poverty traps in investment. People without access to financial resources – due to lack of savings or access to debt – cannot invest in their offspring, restricting their access to education and reinforcing income inequality across generations. As a result, parental investments on education depend on their wealth, creating a polarized labour market with skilled and unskilled

[2] ec.europa.eu/commission/priorities/deeper-and-fairer-economic-and-monetary-union/european-pillar-social-rights.

dynasties. This literature began with Galor and Zeira (1993), Banerjee and Newman (1993), and Galor and Tsiddon (1997) and has since expanded enormously (see, e.g., Owen and Weil (1998); Bourguignon and Verdier (2000); Checchi (2004) and Berg and Hebous (2021) for an empirical estimate of the influence of parents wealth on offspring's earnings).

The literature focused on liquidity constraints questions the assumed trade-off between economic growth and equality, not because of the damage that a redistributive agenda could have, but because lower inequality could unlock the potential of those without opportunities. For example, the Galor and Zeira (1993) model discusses how higher inequality can increase growth in very low income countries, as the wealth concentration allows for a few families to invest in education. Chapter 3 pursues this argument by studying the effect of inequality of opportunity on growth rates and contrasting it to the effect of inequality of outcomes.

## 1.3 Measurement of Inequality of Opportunity

### 1.3.1 Compensation and reward principles

A summary measure of inequality of opportunity has to embody two principles. First, it must account for differences due to circumstances and second, it must exclude differences due to effort. While there are several ways of measuring inequality of opportunity, almost all are consistent with these two principles. The theoretical literature has called these two principles the compensation and reward principles (Fleurbaey and Peragine, 2013; Fleurbaey et al., 2017). An equal-opportunity policy has to compensate for differences due to circumstances while rewarding differences due to effort.

The compensation principle seeks to eliminate differences due to circumstances and can be defined as either ex-ante or ex-post (Ramos and Van de gaer, 2020). The difference between the two is whether individuals have made their choices yet or not (i.e., whether they have exerted effort). The ex-ante principle is concerned with potential outcomes: are the possible outcomes a person can achieve constrained by their circumstances? The ex-post principle, on the other hand, is concerned with their actual outcomes: are there differences in the outcome among people with the same level of effort due to having different circumstances?[3] Fleurbaey and Peragine (2013) find the two principles are incompatible, and whenever possible, the ex-post principle should be preferred as it embodies the general equality of opportunity concept of 'equal effort, equal reward'. Nonetheless, Roemer and Trannoy (2015) downplays the problem from an empirical point of view: if one does not have (or does not trust) their available effort data, the ex-ante approach remains available.

The reward principle also has two main interpretations: the liberal or natural principle and the utilitarian reward principle. The liberal reward states that resource allocation should be independent of individual effort. That is, if everyone had the same circumstances there would be no redistribution. The utilitarian principle states that there should be zero aversion to inequality of effort. If everyone had the same circumstances we would opt for the distribution with the highest average outcome. The main difference between the two reward principles is that the utilitarian principle allows for redistribution between people with the same circumstances if that allows for higher social welfare (as would be the case with different marginal utilities for effort), making the two principles incompatible.

In addition to the two reward principles, Ramos and Van de gaer (2016, 2020) propose a third reward principle that is averse to high levels of inequality due to effort. This third reward principle can be justified on the grounds of luck or unobserved circumstances playing an important role in determining your position in life, a topic discussed in detail in Frank (2016). If small differences in efforts or luck can make important differences in outcomes, as is the case of winner-take-all markets, one can argue in favour of being averse to inequality of effort (albeit less so than to inequality of opportunity).

[3] Checchi and Peragine (2010) use the name 'tranche' to refer to a group of individuals that exert the same level of effort, and to define the ex-post compensation principle.

### 1.3.2 Inequality of opportunity estimates

The literature on inequality of opportunity has grouped estimation techniques into parametric and non-parametric approaches (see page 1313 in Roemer and Trannoy (2016)). The former imposes a functional form to the relationship between circumstances and outcomes. For example, a linear model assumes that each circumstance influences the outcome separately with no interactions and at a constant rate. On the other hand, the non-parametric approach groups respondents into types: each individual is assigned to a group that shares their same circumstances. The non-parametric approach is equivalent to a parametric model where all possible interactions between circumstances are included as covariates. While the non-parametric approach is the most flexible way to account for the influence of circumstances, it is extremely data demanding. For a fixed sample size, a large number of circumstances results in types with no observations. Excluding these types from the analysis would our inequality of opportunity estimates.

Conversely, having only a few circumstances is a problem as well. If we cannot account for the full set of circumstances we will end up with a lower bound estimate of the real level of inequality of opportunity (Ferreira and Gignoux, 2011). This is an issue that commonly arises when estimating inequality of opportunity using survey data and it is not unique of the ex-ante approach. Luongo (2011) explains why this problem – what she calls the 'partial observability' problem – also holds for ex-post measures of inequality of opportunity. Empirical studies like Hufe et al. (2017) have shown the extent of the 'partial observability' problem. They find that expanding the set of available circumstances beyond commonly used variables such as parental education or place of birth to include time investments and other circumstances can substantially increase the share of income inequality accounted for by circumstances (from 27% to 43% for the U.S.A. and from 18% to 27% for the UK).

My thesis is mostly concerned with, and addresses the issues arising from, the estimation stage. I follow an ex-ante compensation principle as I focus on circumstances rather than on effort. I do not address the reward principle explicitly throughout the thesis, although I discuss in Chapter 2 the problems that arise from zero aversion for inequality of effort, given the issue with lower bound estimators in light of how high my upper bound estimates of inequality of opportunity are. Chapter 3 also explores the issues related to the lower bound estimates of inequality of opportunity in the context of the effect of inequality and growth.

The literature on inequality of opportunity has explored other areas as part of the measurement and estimation of inequality that I do not address in the thesis and that reflect new approaches and techniques. One line of research has proposed an approach to summarise inequality of opportunity when there are multiple outcomes (Kobus et al., 2020; Yalonetzky, 2010). Some authors have proposed machine learning methods to address overfitting issues in parametric approaches (Brunori et al., 2018; Brunori and Neidhoefer, 2020). Other researchers have looked at the causal effect of policies on inequality of opportunity, bridging the gap with the intergenerational effects literature (Van de gaer et al., 2014; Camarero Garcia, 2018). I do not study these recent approaches as I aim to provide estimates that are comparable to previous papers, for example, for the effect of inequality of opportunity on growth or for intergenerational elasticities of earnings and income.

### 1.3.3 Existing estimates of inequality of opportunity

In this section I provide existing estimates on inequality of opportunity and answer two questions. First, what are the current levels of inequality of opportunity around the world? Chapters 2 and 3 study Europe and the estimates in this section provide some context to my results. Second, what is the relationship between inequality of opportunity and the intergenerational elasticity of income? I'm interested in this relationship as Chapter 4 decomposes the intergenerational elasticity for income and earnings using insights from the inequality of opportunity literature.

The equalchances.org database holds inequality of opportunity estimates for 47 countries based on 124 surveys. These estimates use household equivalised income

Figure 1.2: Inequality of opportunity and inequality of outcomes


Note: IOp is measured as a share of total inequality, both measured using the Gini. Thenestimation includes three circumstances: parental education, parental occupation, origin. Data from most recent available year. Source: equalchances.org.

as their outcome and follow the same methodology: the parametric approach of Ferreira and Gignoux (2011) followed with the cross-validation process described by Brunori et al. (2018). The purpose of this dataset is to increase comparability across countries, which is why all of the estimates use the same three circumstances: parental education, parental occupation, and origin, which can represent ethnic origin or area or birth depending on available data.

Figure 1.2 presents the latest available estimates (2010–2012) of inequality of opportunity across the world, measured as the share of inequality of outcomes accounted for by circumstances. Within Europe – my reference for Chapters 2 and 3 – Italy, Luxembourg and Bulgaria experience the highest share of inequality of opportunity (around 45%), followed by Spain and Romania (40%). On the other hand, Iceland and Denmark experience the lowest share of inequality of opportunity (around 13%), followed by Finland, Norway and the Netherlands (around 19%). Among the countries in the equalchances.org database, Guatemala, Brazil and South Africa report the highest shares of inequality of opportunity (58%). Overall, African and Latin American countries have higher levels of inequality of opportunity as a share of their inequality of outcomes.

Are the inequality of opportunity estimates correlated with inequality of outcomes? Figure 1.2 shows that they are. A similar result by Corak (2013) has been coined the 'Great Gatsby curve': an increase in inequality of outcomes (measured through the Gini) is associated with an increase in intergenerational persistence (measured through the intergenerational elasticity). As journalist Timothy Noah frames it, if intergenerational persistence reflects the difficulty of moving up the social ladder, higher inequality moves the rungs of the ladder further apart.[4] The relationship between inequality of opportunity and inequality of outcomes is a topic I explore in chapter 2. I explore the relationship between inequality of opportunity and the intergenerational elasticity – the Great Gatsby curve's counterpart to Figure 1.2 – in chapter 4.

## 1.4 Aims and contributions of the thesis

My thesis consists of three separate but complimentary research papers, set out in Chapters 2 to 4. In each I study a different aspect of the empirical literature on inequality of opportunity. Chapter 2 provides upper bound measures of inequality of opportunity. Chapter 3 studies the relationship between inequality and growth using the estimates from the previous chapter. Chapter 4 studies the relationship between inequality of opportunity and the intergenerational elasticity by decomposing the latter and measuring the importance of childhood circumstances. Chapters 2 and 3 focus on Europe while chapter 4 looks at the U.S.A.

Each of the chapters is concerned with the relationship between inequality of op-

[4] newrepublic.com/article/99651/white-house-heres-why-you-have-care-about-inequality.

portunity and a well-established topic in the economic inequality literature: inequality of outcomes and its decomposition, economic growth, and intergenerational mobility. Overall, the thesis provides a better understanding of inequality of opportunity and can contribute to the study of economic inequality more generally.

In addition to my methodological contributions, I discuss how inequality of opportunity can inform policy. Kanbur and Wagstaff (2016) argue that empirical applications of the inequality of opportunity concept can be problematic, to the extent that they might not be a useful concept at all. I believe the opposite is true: equality of opportunity will still be a policy goal for governments and other institutions as it resonates with the public. Efforts should be aimed at improving our understanding of what inequality of opportunity entails and its consequences. The present thesis contributes to a better understanding of the empirical issues associated with the measurement of inequality of opportunity, how to address them, and how can they contribute to the study of unfair inequalities.

The goal of this thesis is to contribute to the understanding of how parents' characteristics shape the outcomes of their offspring, focusing on the importance of factors that are deemed as unfair sources of advantage. These factors can play a larger role than previously estimated; they can affect economic growth and can account for a substantial part of intergenerational persistence. This thesis shows the importance of circumstances in shaping outcomes in high-income countries.

## 1.5 Thesis outline

Chapter 2 provides upper bound estimates of inequality of opportunity, in contrast to the more well established lower bound estimates. I apply the approach of Niehues and Peichl (2014) to obtain estimates of inequality of opportunity for 24 European countries for the period 2005–2011. As part of this exercise, I test the extent to which their approach can be applied to short panels. In addition, having estimates of inequality over time allows me to test the assumptions behind the upper bound approach, namely that circumstances (and their influence) do not change over time.

The problem with upper bound estimates of inequality of opportunity is that they are a black box. Under the upper bound approach, all time-invariant determinants of the outcome are treated as circumstances and are grouped into a single individual fixed effect. In other words, it is not possible to know what factors are being accounted for – whether circumstances or time invariant efforts – nor their relative importance. To partially address this issue, I study the gap between the upper and lower bound estimates of inequality of opportunity as a measure of the importance of time invariant factors not observed in the data. The gap between bounds vary substantially between countries, suggesting that observed circumstances are sufficient in some countries while not so in others. I also explore how additional circumstances reduce the size of the gap by increasing the lower bound estimate of inequality of opportunity.

Chapter 3 uses the upper bound estimates of inequality of opportunity to study the relationship between short-term growth and inequality in the context of the Great Recession and the European Debt crisis. Previous papers have studied this relationship using lower bound estimates of inequalities of opportunity. However, lower bound estimates omit the influence of certain circumstances. A lower bound estimate might show a low level of inequality of opportunity because the main circumstance for that country was excluded. For example, Singh (2011) finds that caste is an extremely relevant circumstance for the case of India, while Golley and Kong (2018) do the same for the household registration status (*hukou*) in China. The exclusion of relevant country-specific circumstances is problematic for cross-country comparisons, particularly when the gap between the upper and lower bound estimates is large. I also compare the influence of inequality of outcomes and the influence of inequality of opportunity to test the idea that some inequalities might be harmful to growth, while others might not.

Chapter 4 studies the relationship between IOp and another common measure of persistence, the intergenerational elasticity (IGE). Intergenerational immobility is commonly interpreted as a measure of inequality of opportunity, but this interpretation depends on what determines the intergenerational elasticity. As Elliot Major and Machin (2018) state, "no one knows what the optimal level [of income mobility] for any country is". Behind this statement lies the idea that parental income is a summary measure which accounts for multiple factors, some of which might be justified sources of persistence. This interpretation differs from the IOp one, where parental income is a circumstance and its complete influence contributes towards IOp. In this chapter, I discuss whether parental income is an appropriate summary of all other circumstances.

# Chapter 2

# Upper and lower bound estimates of Inequality of Opportunity: A cross-national comparison for Europe

## 2.1 Introduction

Promoting equal opportunities lies at the core of several national and cross-national policy agendas. Many governments and international institutions have incorporated the challenge of achieving equal opportunities in their long-term strategies. Indeed, the first of the three European Pillars of Social Rights of 2017 is to promote equal opportunities.[1] The same holds for other institutions as well as many national governments. However, to be able to pursue the goal of equal opportunity, we first require an appropriate measurement of unequal opportunities. In this chapter I provide upper bounds estimates of inequality of opportunity for 24 European countries between 2005 and 2011, complemented with the more standard

[1]See ec.europa.eu/social/pillar.

lower bound estimates of IOp.

The literature on inequality of opportunity (IOp) states that sources of inequality matter from an ethical point of view. In particular, what matters is the distinction between morally legitimate sources, commonly called ‘effort’, ‘preferences’ or ‘responsibility’, and morally illegitimate sources, called ‘circumstances’ (Fleurbaey, 1994; Roemer, 1993, 1998), with IOp quantifying the importance of the latter. The growing interest in measuring IOp can be seen in the multiple applications for several countries, as well as the many approaches to measuring IOp (Ferreira and Peragine, 2016; Roemer and Trannoy, 2016; Bourguignon, 2018; Ramos and Van de gaer, 2020).

Most approaches to measuring IOp account only for circumstances that are observed in the data, thus resulting in lower bound estimates of IOp (Ferreira and Gignoux, 2011; Luongo, 2011). On their own, lower bound estimates can be problematic for three reasons. First, from a measurement point of view, we do not know how far these estimates are from the ‘real’ level of IOp (Ferreira and Peragine, 2016). Second, from a policy point of view, lower bound estimates can be misinterpreted as the real level of IOp, reducing redistributive efforts from policy makers (Kanbur and Wagstaff, 2016). Third, from a normative point of view, misinterpreting lower bound estimates of IOp can diminish the perceived importance of structural causes of inequality, which increased concerns about inequality (Mijs, 2019). Lower bound estimates can misrepresent the extent of IOp, and by only showing the lowest possible level of IOp, lower bound estimates can have a detrimental effect on overall demands for lower inequality.

I show that upper bound estimates account for over 90% of total inequality, while lower bound estimates account for at most 30%, showing that the potential extent of IOp could be well above the levels shown by the latter. I also find that upper bound estimates provide new information, as both country rankings and trends over time show different patterns when looking at the two different estimates. Lastly, explore the gap between upper and lower bound estimates of IOp and find that it differs greatly across countries and years, hinting at the importance of time-invariant factors, whether omitted circumstances (excluded from the lower bound) or time-invariant efforts (mistakenly included in upper bound estimates). Overall, these estimates show that upper bound estimates of inequality of opportunity and inequality of outcomes are closely related, as the correlation between the two – both cross-sectionally and over time – is much stronger than when looking at lower bound estimates.

The rest of the chapter is organized as follows. Section 2 proposes a small model to explain what is being captured by the lower and upper bound estimates of IOp, as well as the estimation approaches in both cases. Section 3 describes the data. Section 4 reports the estimates, the differences between the two estimates, and the gap between them. Section 5 explores robustness checks. Section 6 concludes.

## 2.2 Estimating lower and upper bounds of IOp

### 2.2.1 Decomposing total inequality: The role of circumstances

My outcome of interest is an individual's yearly household equivalized disposable income; that is, the total income of a household that is available to spend or save in a year, divided by the number of 'equivalized' adults, using the modified OECD equivalence scale. Equivalized income provides a measure of disposable income, and therefore of overall welfare. It is also the most common outcome when measuring IOp[2] and, contrary to earnings, it avoids issues with cross-country differences in labour market participation, particularly among women.

I use two approaches to estimate IOp, one to estimate a lower bound of IOp and another to estimate an upper bound. Both measure 'ex-ante' IOp, that is, they account for differences in circumstances before effort has been exerted (see, e.g.,

[2]See, e.g., Ferreira and Gignoux (2011); Ramos and Van de gaer (2016); Brunori (2017).

Fleurbaey and Peragine (2013); Fleurbaey et al. (2017)). These two approaches differ in how the set of circumstances is constructed: the lower bound approach uses variables available in the data, such as parental education or place of birth, while the upper bound approach uses a fixed effect regression to capture time invariant factors, capturing both circumstances and time-invariant efforts. Whether the real level of IOp is closer to one or the other estimate will depend on the relative importance of omitted circumstances and of the time-invariant efforts captured in the upper bound estimate.

To explain how IOp is measured and how the lower and upper bounds differ, I expand the 'canonical model' of equal opportunity (Ferreira and Peragine, 2016) by including a time dimension, and by making a distinction between factors that change over time, and those that do not. I use this model as a benchmark to represent what is being captured by each of the two approaches.

I assume that circumstances are constant over time. Under this assumption circumstances are predetermined factors, such as the place of birth or the investment made by parents. This is consistent with the idea of initial brute luck in Dworkin (1981a,b), where circumstances account for social background or inherited traits, in contrast with later brute luck, that represent events that take place later in life, such as economic crises or accidents. Efforts can be vary or be fixed over time. For example, they can represent the choice of working part time or full time, that might change. They can also account for a personality trait, namely having a 'hard working attitude'. Both circumstances and effort shape the outcome.

Concretely, I model the logarithm of income $Y$ for individual $i$ in year $t$ as a function of circumstances $C_i$ and efforts $E_{it}$ and $E_i$, as shown in equation 2.1:

$$log(Y_{it}) = \alpha_0 + \beta_0 C_i + \gamma_0 E_{it} + \eta_0 E_i + \mu_t + \varepsilon_{it}. \tag{2.1}$$

I use the logarithm of income to address common issues of heteroscedasticity in the residual. $C_i$ is a vector of (time-invariant) circumstance variables, $E_{it}$ and $E_i$ are effort variables (time varying and time-invariant, respectively), $\mu_t$ a year fixed-effect, and $\varepsilon_{it}$ the error term. In this model, and following the idea or initial brute luck, time fixed effects are not part of my analysis. One could argue that they reflect 'later' brute luck, and thus a circumstances, or that they are efforts, to the extent that people take choices based on the current context in a similar line to option luck. The answer is probably in between the two cases – with effects being heterogeneous across individuals. Following the fact that this approach is based on capturing initial brute luck (thus omitting potential time-varying circumstances), I exclude time fixed effects from the model.

Circumstances affect income directly, but they also have an indirect effect through efforts. Efforts are determined by circumstances and purely autonomous choices, with only the latter being a source of legitimate inequality.[3] Efforts are modelled as a linear combination of circumstances and an error term that represents the part of efforts not influenced by circumstances:

$$E_{it} = \alpha_1 + \beta_1 C_i + v_{it}. \tag{2.2}$$

$$E_i = \alpha_2 + \beta_2 C_i + u_i. \tag{2.3}$$

These equations capture the influence of circumstances on effort. Choices like the number of hours someone works or the type of contract may qualify as 'efforts', but they are partly determined by the socioeconomic background of those that take them. Similarly, circumstances like the place of birth can affect income directly through labour market discrimination, but also indirectly through labour market choices. The error term provides a measure of 'relative' effort, effort that is not determined by circumstances.

By substituting these two equations into equation 2.1, we get:

$$\begin{aligned} log(Y_{it}) = \quad & (\overbrace{\alpha_0 + \gamma_0\alpha_1 + \eta_0\alpha_2}^{\tilde{\alpha}}) + \mu_t + \overbrace{\eta_0 u_i}^{\tilde{u}_i} \\ & + (\underbrace{\beta_0 + \gamma_0\beta_1 + \eta_0\beta_2}_{\tilde{\beta}})C_i + (\underbrace{\varepsilon_{it} + \zeta_0 v_{it}}_{\tilde{\varepsilon}_{it}}). \end{aligned} \tag{2.4}$$

[3] This is a common assumption in the IOp literature. However, Barry (2005) considers the effect of circumstances on efforts to be within the space of personal responsibility. He states that differences due to individual choice should result in legitimate inequality, irrespective of what drives theses choices (see Jusot et al. (2013) for an empirical application on this issue).

This equation includes all effects of circumstances on income, both direct and indirect. Reorganising it, we get equation 2.5.

$$log(Y_{it}) = \tilde{\alpha} + \tilde{\beta}C_i + \mu_t + \tilde{u}_i + \tilde{\varepsilon}_{it}. \tag{2.5}$$

Income is determined by circumstances ($C_i$), a time effect ($\mu_t$), an individual fixed effect that stems from efforts ($\tilde{u}_i$), and an error ($\tilde{\varepsilon}_{it}$).

While I aim to estimate a reduced form model (equation 2.5), an alternative would be to estimate the complete structural model (equations 2.1 to 2.3). Such an estimation provides information on the indirect paths through which circumstances shape efforts. It also allows for the inclusion of market conditions, or for influence of circumstances to vary according to demographics that, depending on the view regarding IOp, can be interpreted as 'preference shifters' or circumstances (Roemer and Trannoy, 2016). While I explore the empirical role of the indirect effect in section 2.4.4, my focus is on determining the complete influence of circumstances, whether direct or indirect, and I therefore concentrate on the estimation of the reduced form equation.

### 2.2.2 Measuring IOp: Estimation and prediction of a counterfactual distribution

Both the lower and upper bound estiamtes use a parametric or 'regression-based' approach. That is, I estimate the association between circumstances and the outcome using a regression model rather than by estimating the average outcome across groups with the same circumstances. This is a fairly standard approach in the literature that allows for a linear specification as well as potential interactions between circumstances (Bourguignon et al., 2007; Ferreira and Gignoux, 2011; Brunori et al., 2013).

The upper bound and lower bound estimates are estimated in a similar way. They differ, however, on how circumstances are measured. The lower bound vector of circumstances $C_i^{LB}$ includes circumstance variables that are available in the dataset, where $C_i^{LB} \subset C_i$. On the other hand, the circumstance vector in the upper bound approach $C_i^{UB}$ is the predicted fixed effect from a longitudinal regression. As it accounts for all time invariant factors, $C_i^{UB}$ includes both $C_i$ and $\tilde{u}_i$ in equation 2.5.

We are interested in predicting the conditional mean $E(Y_i|C)$, but equation 2.5 estimates $E(log(Y_i))$. If we were to predict $E(Y_i|C)$ using this equation, it would lead to biased estimates as $log(E(Y_i)) \neq E(log(Y_i))$. In order to address this issue, all models are estimated using Poisson regressions on income instead of OLS on the log of income (Santos Silva and Tenreyro, 2006). The Poisson estimator specifies the conditional mean as $E(Y_i|C) = exp(\alpha + \beta C_i)$ instead of $E(log(Y_i)|C)$, eliminating the need of an additional term to get $E(Y_i|C)$ (i.e, a smearing retransformation).[4]

Lower bound and upper bound IOp are estimated using a Poisson regression following equations 2.6 and 2.7:

$$log(Y_i) = \alpha + \beta C_i^{LB} + u_i \tag{2.6}$$

$$log(Y_i) = \alpha + \beta C_i^{UB} + u_i \tag{2.7}$$

From these equations I recover the predicted values $\hat{Y}_i^{LB}$ and $\hat{Y}_i^{UB}$, respectively, which are then used to measure IOp.

I use the Mean Logarithmic Deviation (MLD) to measure inequality. This index can be additively decomposed into within and between group inequalities, making it very helpful when displaying the decomposition of total inequality as in this case. If $I(\cdot)$ represents the MLD, then the level of IOp is defined as:

$$IOL^{LB} = I(\{\hat{Y}^{LB}\}). \tag{2.8}$$

$$IOL^{UB} = I(\{\hat{Y}^{UB}\}). \tag{2.9}$$

[4]The smearing retransformation is used when $E(log(Y_i)|C)$ is estimated. The predicted outcome is proposed in Duan (1983) and is equal to $E(\hat{Y}_i|C) = exp(\hat{\alpha} + \hat{\beta} C_i) \cdot exp(\frac{1}{2}\hat{\sigma}^2)$.

When using the MLD, we know that the difference between total inequality and IOp is the level of inequality attributed to differences in efforts that vary over time. This property is particularly useful when reporting relative IOp, equal to the ratio between IOp and inequality of outcomes.

$$IOR^{LB} = \frac{I(\{\hat{Y}^{LB}\}}{I(\{Y\})}. \tag{2.10}$$

$$IOR^{UB} = \frac{I(\{\hat{Y}^{UB}\}}{I(\{Y\})}. \tag{2.11}$$

While the MLD is useful for its decomposition properties, the fact that it is not bounded from above makes the interpretation of the IOL difficult, unlike, say the Gini index. It can also be sensible to extreme values, growing exponentially when income volatility grows beyond certain threshold (Brunori et al., 2019), potentially downward biasing IOp estimates. However, Ramos and Van de gaer (2020) show their findings to be robust to the choice of index, with similar country rankings under the Gini and the MLD. Similarly, Cowell and Flachaire (2007) find that the MLD and the Gini behave similarly under finite samples.[5] Being the first study to explore upper bound estimates of IOp over time, I have prioritised the decomposability property of the MLD over other inequality indexes.

I present lower and upper bound IOp estimates using the MLD for both the IOL and IOR in 2005 and 2011. I also report the complete series from 2005 to 2011 for the upper bound estimate. While my interest is in the IOp ratio (i.e., the share of total inequality attributed to differences in inequality) I present the IOp level and total inequality separately to better explain the changes in the ratio. Estimates for the ratio itself are reported in the Appendix. The following sections provide additional details on how I estimate lower and upper bound levels of IOp.

[5] My estimates are consistent with this finding. Across all years and for the 24 countries, 98% of the changes in country rankings are of 3 positions or less. 52% of all cases do not change position. Overall, country rankings using the MLD and the Gini show similar orderings.

### 2.2.3 The lower bound approach

The lower bound approach is probably the most commonly used method to measure IOp (Ferreira and Peragine, 2015; Roemer and Trannoy, 2015). This approach uses circumstance variables that are available in the survey or data source. Common examples of such variables are the education of the parents, gender or place of birth, which are commonly available in cross sectional surveys. Just like with any IOp estimate, the stronger the influence of the observed circumstances, the higher IOp is. However, the fact that some circumstances will inevitably be omitted explains why this approach results in 'lower bounds' of the real level of IOp.

When estimating IOp, all omitted circumstances will be included in the error term, together with efforts. Based on equation 2.5, we can represent the error term as:

$$\hat{u}_i = log(Y_i) - log(\hat{Y}_i) = log(Y_i) - (\hat{\alpha} + \hat{\beta}C_i). \tag{2.12}$$

The use of a lower bound estimate can be problematic if interpreted incorrectly as the 'real' value of IOp. A policy maker interested in equal opportunities - when faced with these estimates- may mistakenly assume that IOp is not as large, underestimating the role of circumstances and limiting policy responses (Kanbur and Wagstaff, 2016). At best, a lower bound estimates shows an incomplete picture of the extent of unequal opportunities.

I estimate the lower bounds of IOp using the supplementary EU-SILC questionnaire on the intergenerational transmission of disadvantages, available for 2005 and 2011. I include as circumstances the gender of the respondent, both parents' education and main activity, the father's occupation, and household composition, all at age 15 and measured retrospectively. This set of circumstances is fairly standard in the literature as it is commonly asked in surveys. It paints a picture of how a person was raised: the resources available to the parents in terms of income, culture, and even time. It is, however, an incomplete picture as it will inevitably omit important circumstances – such as parental investments in cultural capital (books, extracurricular activities) or time (e.g., playing or reading time), as well as school and neighbourhood characteristics – that the upper bound can pick up.

An important caveat when estimating lower bound IOp is the one discussed in Brunori, Peragine, and Serlenga (2018). If the number of circumstances is large (relative to the sample size) these estimates might suffer from overfitting, resulting in an upwardly biased estimate of IOp. Framing the estimation of IOp as a prediction problem, Brunori, Hufe, and Mahler (2018) and Brunori and Neidhoefer (2020) propose using machine learning methods that find the functional form that minimises an objective criteria such as the mean square error. These approaches consider both sources of bias – a downward bias due to omitted information and an upwards bias due to 'too much' information – and aim to maximise predictive power, irrespective of the final choice of circumstances.

The presence of an upward bias for 'lower bound' estimates of IOp has two related implications for my analysis. First, these estimates might not be the lowest possible level of IOp for a given set of circumstances (i.e., increasing the sample size could reduce it even further). Second, a better estimation approach (in the sense of its goodness of fit) show allow for different circumstances in each country. Both issues affect the lower bound estimation and therefore the interpretation of the gap between bounds that I discuss in section 2.4.3. As my interest lies in the impact of the upper bound of IOp when discussing the gap I 'hold constant' the lower bound and opt for common set of circumstances across countries. As this might result in overfitting issues for countries with a small sample size I only study the gap for countries where the lower bound is estimated with 4,000 observations or more, the point at which Brunori, Peragine, and Serlenga (2018) show the 95% confidence interval of the IOp estimate overlaps the IOp level in the population (see their Figure 3).

### 2.2.4 The upper bound approach

The upper bound approach to measuring IOp is a two-step process. First we estimate the circumstance set, second (and along the same line of the lower bound approach), we use the estimated measure of circumstance to measure IOp. The first step uses long term panel data to capture all time invariant characteristics for each respondent, which are then treated as the measure of circumstances in the second step. This set of time invariant factors captures standard circumstances such as parental schooling or place of birth, but it also captures circumstances that are hard to observe in the data such as innate non-cognitive skills, health status, test scores during childhood, or inherited financial and cultural capital. On the other hand, this measure of circumstance also includes time invariant efforts, thus resulting in an 'upper bound' of the real value of IOp.

The first step involves a fixed effect regression across all available years except for the year in which IOp is measured. In the case of Niehues and Peichl (2014), this means at least 5 consecutive years (7 years on average) to measure IOp in 2009 for Germany and 2010 for the US. In my case, this means 3 years to estimate the fixed effect plus the year to measure IOp. For example, IOp in 2008 is estimated using the estimated fixed effect for the years 2009, 2010, and 2011.[6]

The fixed effect regression can be interpreted in terms of the structural model in equation 2.5, where the log of income is determined by individual and time fixed effects. For respondent $i$ in year $t$, the fixed effect equation is given by:

$$log(Y_{it}) = \alpha + \eta_i + u_t + \varepsilon_{it}. \tag{2.13}$$

If properly estimated, the predicted fixed effect $\hat{\eta}_i$ will capture all time invariant factors. Following equation 2.5 the fixed effect will capture all time invariant circumstances $\tilde{\beta}C_i$ as well as the residual time invariant effort $\tilde{u}_i$:

$$\eta_i = \tilde{\beta}C_i + \tilde{u}_i \tag{2.14}$$

Time invariant efforts can include determinants of labour market outcomes, such as '*long-term motivation and work effort*' (Niehues and Peichl, 2014). The extent

[6]Three years is a substantially smaller window to measure a fixed effect than the one in Niehues and Peichl (2014). I discuss the implications of using a short $T$ and a small simulation in section 2.5.1.

to which the upper bound estimate of IOp differs from the 'real' level of IOp will depend on the relative importance of this term.

I use the predicted fixed effect ($\hat{\eta}_i$) to measure the upper bound of IOp. I measure IOp over the year which was not used to estimate the upper bound. In this case, I estimate it for the first year of the 4-year panel.

$$log(Y_{is}) = \psi\hat{\eta}_i + \omega_{is}. \tag{2.15}$$

The upper bound estimate of IOp picks up all time invariant circumstances as a single variable. As a result it does provide information on what these circumstances are, nor their relative importance. The benefit of this approach is that it can adapt to each specific context, picking up whatever circumstances are important for that country.

There are two assumptions that allow us to interpret the fixed effect as a measure of all time invariant circumstances. These assumptions have to do with estimated coefficients and predicted fixed effects being constant over time. As this is the first piece of research to obtain upper bound estimates of IOp over time, I also show that these assumptions hold empirically. These estimates are discussed in detail in the appendix (section 2.A.1).

Being a data demanding approach, Niehues and Peichl (2014) limit their application to one year of the German SOEP and the PSID for the US. However, this does not mean that the approach requires long panels. As the goal of this chapter is to estimate cross-country comparisons of IOp over time, I apply this approach to the EU SILC. In order to do so, I depart from Niehues and Peichl (2014) in two ways, which I describe in the following section.

**Using the upper bound approach on a short rotating panel**

To adapt the upper bound approach to the longitudinal component of the EU-SILC I make two departures from the approach proposed by Niehues and Peichl

(2014). The first is a requirement imposed by the data, while the second one is a choice to estimate upper bound IOp for the same year as for the lower bound estimates. First. I use a shorter panel to estimate the fixed effects. While they use an average of 7 years, I use 3. The second departure involves the set of years used to estimate the fixed effect. To estimate IOp for Germany in 2009, they use the previous period (2002 to 2008) to estimate the fixed effects. Instead, I use later years to predict the fixed effect, choosing the years 2010, 2011, and 2012 to estimate IOp in 2009. In other words, the second departure is to use the following years (rather than the previous years) to estimate the fixed effect regression.

The use of a shorter panel might be econometrically troublesome. The fixed effect estimation might be noisy if the time dimension is short (a "large $N$, small $T$" problem). Following equation 2.13, the fixed effect is estimated as:

$$\hat{\eta}_i = \overline{log(Y_i)} - \hat{\alpha} - \overline{u}. \tag{2.16}$$

Where the bar represents the sample average: $\overline{log(Y_i)} = \frac{\sum_t^T log(Y_{it})}{T}$ and $\overline{u} = \frac{\sum_t^T u_t}{T}$.

The fixed effect $\eta_i$ might not be consistent when $N$ grows (for a given $T$), as each new observation requires a new $\eta_i$ parameter, and therefore information does not accumulate on $\eta_i$ as $N$ grows, only when $T$ grows. In other words, with a small $T$, the fixed effect parameters may contain substantial noise (Wooldridge, 2010, pp. 272-4).

The implications of the second departure depend on the first one. If the fixed effect is properly estimated, it should not matter what is the set of years we use to estimate it. If fixed effect estimates suffer from estimation issues, then whether I use the first or the last set of years will matter for the estimation of IOp.

The implications of both departures can be examined empirically through the 2010 longitudinal sample for Luxembourg, which follows the same respondents for up to 7 years. For the first departure I estimate the fixed effects for different lengths of $T$. For the second departure, I estimate the fixed effect using previous and later years separately. In addition, I provide a simulation of the distribution of the fixed effect estimator under different sample sizes and time horizons. I show in section 2.5.1 that these departures make little difference to the upper bound estimates of IOp.

## 2.3 European data: The cross sectional and longitudinal EU-SILC

This chapter uses data from the European Union Statistics on Income and Living Conditions (EU-SILC). The EU-SILC collects cross-sectional and longitudinal data on poverty and income dynamics for Europe, with some countries conducting surveys and others using a combination of surveys and administrative registries.[7] The cross-sectional sample gathers information for respondents each year, while the longitudinal sample follows each respondent for four consecutive years, before renewing the sample in a rotating panel structure.

I use the cross-sectional sample to obtain the lower bound estimates of IOp, and the longitudinal sample for the upper bound estimates. Unfortunately, it is not possible to use a common sample for both approaches, or to merge them in order to use the same group of respondents. The longitudinal sample does not include retrospective information on the respondents, and the cross-sectional sample does not allow for the estimation of fixed effect regressions. I re-estimate the lower bound estimates for the first rotation group in the cross-sectional sample, the same group I use to estimate the upper bound IOp in section 2.5.2.

I use the cross-sectional sample for the years 2005 and 2011 to estimate lower bounds of inequality of opportunity as these are the only two years with secondary questionnaire that includes retrospective information. The choice of circumstance variables is detailed in table 2.1. To derive the upper bound, I use the longitudinal

[7] From Törmälehto et al. (2013): '*Nine countries currently collect income data mostly from registers: Denmark, Ireland, the Netherlands, Slovenia, Finland, Sweden, Iceland, Switzerland and Norway. Ten other countries use both interviews and registers for income measurement: Bulgaria, Belgium, Cyprus, Spain, France, Italy, Lithuania, Latvia, Malta and Austria*'.

samples from 2008 to 2014 and estimate IOp from 2005 to 2011.

Table 2.1: Circumstance variables

| 1. Occupation (father) | 2. Activity (both parents) | 4. Gender of respondent |
|---|---|---|
| a. Armed forces | a. Employee | a. Male |
| b. Managers | b. Self-employed | b. Female |
| c. Professionals | c. Unemployed | 5. Household composition |
| d. Technicians | d. Retired | at age 15 |
| e. Clerical support | e. Housework | a. Both parents |
| f. Service and sales | f. Other (inactive) | b. Only father |
| g. Skilled agricultural | 3. Education (both parents) | c. Only mother |
| h. Craft and trades workers | a. Low | d. No parents |
| i. Plant operators | b. Medium | e. Collective institution |
| j. Elementary occupations | c. High | |

I constrain the set of circumstances based on two criteria. First, I only include circumstances that are available in both 2005 and 2011. Excluding circumstances such as the migration status of the parents, which only appear in 2011, as well as having experienced financial difficulties at a young age, as the question and response categories differ across years.[8] Second, I also exclude circumstances with low response rates, specifically the occupation of the mother. The final set of circumstances described in table 2.1 is similar to previous studies that used the same dataset, although not directly comparable. For example Ramos and Van de gaer (2020) include place of birth and mother's occupation, while Hufe et al. (2018) include immigration status but exclude household composition.

The goal of this chapter is to provide cross country comparisons of IOp, using both a lower bound and an upper bound estimate. In order to provide upper bounds we require panel data while providing lower bounds requires a set of comparable circumstance variables. Previous research that provides both bounds has had to choose between using few countries with long running panel data as in Niehues and Peichl (2014), or to include more countries using datasets that are not necessarily

[8]In 2005 the question is 'Financial problems in household when young teenager' with five categories, from 'Most of the time' to 'Never'. In 2011 the question is 'Financial situation of the household' when the respondent was around 14 years of age, with 6 categories going from 'Very bad' to 'Very good'.

comparable, as in Hufe et al. (2019). The EU-SILC provides a common set of circumstances as well as a rotating panel for several countries over time, in order to estimate comparable bounds for the 'real' level of IOp.

The outcome variable is yearly household income, net of transfers and taxes, and divided by the number of 'equivalized' adults (using the modified OECD equivalence scale).[9] I focus on individuals with positive income, aged 25 to 55, and only on countries with data for the complete 2005 to 2011 period. The resulting sample includes 24 countries for which it is possible to estimate upper bounds of IOp between 2005 and 2011. The data are weighted by the year-four longitudinal weight or by each year's personal cross-sectional weight for the upper and lower bound approach, respectively.

Countries in the EU-SILC use surveys or registers (or a combination of both) to provide information on income. As a result, 'register countries' will provide income measures that are less prone to measurement error and also less likely to suffer from top income bias (see, e.g., Jenkins (2016)). For my period of study, register countries include Austria, Belgium, Cyprus, Denmark, Spain, Finland, France, Iceland, Italy, Lithuania, Latvia, the Netherlands, Norway, Sweden and Slovenia. Ultimately, the use of registers make inequality estimates less volatile and more precise (i.e., closer to the 'real' distribution of household income) and it is something to consider when making cross-country comparisons. For a detailed overview of the use of registers, see Törmälehto et al. (2013).

Table 2.2 shows the unweighted number of observations for the longitudinal sample. The first column for each year is the total number of respondents that appear in all four years (i.e., those respondents that can be included in the fixed effect estimation and the IOp estimation). The second column limits the sample by age range (25 to 55), and the third includes all respondents with positive income. The sample

[9] According to Eurostat, the income reference period is a fixed 12-month period (such as the previous calendar or tax year) for all countries except the United Kingdom, for which the income reference period depends on the date of the interview, and Ireland, for which they ask for income in the last twelve months. All countries are then converted to annual equivalents, which are unlikely to be a source of non-comparability (Iacovou et al., 2012).

Table 2.2: Unweighted observations in the longitudinal sample

| | 2005 | | | 2011 | | |
|---|---|---|---|---|---|---|
| Country | Total obs. | Age range | Income | Total obs. | Age range | Income |
| Austria | 1,992 | 1,105 | 1,104 | 2,034 | 1,108 | 1,107 |
| Belgium | 2,232 | 1,197 | 1,195 | 2,019 | 1,023 | 1,018 |
| Cyprus | 1,780 | 929 | 928 | 2,032 | 996 | 995 |
| Czech Republic | 6,621 | 3,371 | 3,371 | 3,952 | 1,904 | 1,902 |
| Denmark | 1,643 | 1,010 | 1,002 | 1,343 | 663 | 660 |
| Estonia | 983 | 488 | 484 | 2,336 | 1,126 | 1,112 |
| Greece | 2,240 | 1,156 | 1,138 | 2,193 | 1,043 | 1,000 |
| Spain | 5,382 | 2,971 | 2,911 | 4,550 | 2,413 | 2,369 |
| Finland | 2,754 | 1,572 | 1,570 | 4,340 | 2,335 | 2,332 |
| France | 8,592 | 4,775 | 4,764 | 9,511 | 4,914 | 4,903 |
| Hungary | 3,199 | 1,700 | 1,692 | 6,098 | 3,269 | 3,267 |
| Iceland | 990 | 577 | 575 | 992 | 567 | 567 |
| Italy | 8,451 | 4,476 | 4,390 | 7,094 | 3,579 | 3,508 |
| Lithuania | 1,528 | 793 | 784 | 2,254 | 1,009 | 995 |
| Luxembourg | 4,721 | 2,816 | 2,801 | 1,339 | 797 | 787 |
| Latvia | 1,540 | 762 | 751 | 2,484 | 1,204 | 1,185 |
| Netherlands | 4,164 | 2,809 | 2,786 | 3,833 | 2,157 | 2,145 |
| Norway | 4,522 | 2,748 | 2,734 | 2,531 | 1,425 | 1,425 |
| Poland | 6,756 | 3,727 | 3,709 | 6,002 | 3,130 | 3,128 |
| Portugal | 1,880 | 334 | 334 | 3,013 | 1,470 | 1,470 |
| Sweden | 2,258 | 1,233 | 1,230 | 1,753 | 805 | 801 |
| Slovenia | 3,878 | 2,117 | 2,117 | 3,951 | 2,104 | 2,104 |
| Slovakia | 2,532 | 1,355 | 1,352 | 2,993 | 1,548 | 1,546 |
| United Kingdom | 2,938 | 1,560 | 1,552 | 2,357 | 1,126 | 1,105 |

sizes vary greatly across countries, going from 567 to almost 5,000 observations in 2011. Table 2.A.1 in the Appendix shows the number of observations for the longitudinal sample for all countries in the EU-SILC.

Both the cross-sectional and the longitudinal sample lose around 50% of respondents, mainly because of the age range. However, it is the cross-sectional sample that loses the larger share of respondents when including the circumstance variables, because of their low response rate. The longitudinal sample keeps 44% to 67% of all respondents, with the exception of Portugal in 2005, which keeps only 18% of the sample, due to its high share of over 55-year olds (41.7% versus an cross-country average of 30.6%). On the other hand, the cross-sectional sample keeps from 3% to 43% of the original sample. Particularly troubling is Sweden, with only 400 observations in 2011. This is somewhat alleviated by the use of sampling weights in all cases, together with the confidence intervals estimated by bootstrapping the complete estimation process described in section 2.2 over 1,000 repetitions, using random samples with replacement.[10]

## 2.4 Upper and lower bound IOp estimates

### 2.4.1 Inequality of opportunity by country: 2005 to 2011

My estimates are reported in two sets of figures. First, I report all time trends for each particular country (figures 2.1a and 2.1b). Second, I show all estimates – lower bounds, upper bounds, and total inequality – for all countries, separately for the years 2005 and 2011 (figures 2.2 and 2.3). All estimates are for the IOL using the MLD index. The estimates for the IOR are shown in the Appendix (figures 2.A.3 and 2.A.4).

[10]Because of the variability in sample size, all analyses comparing the lower bound to the upper bound estimate (e.g., the study of the gap between the two) focus only on countries where the former was estimated with at least 4,000 observations. Brunori et al. (2018) show that at this point the IOp estimate does not differ in a statistically significant way with the IOp level in the population.

Figures 2.1a and 2.1b suggest a positive correlation between the upper bound estimate of IOp and total inequality, with a few exceptions for particular years such as Norway or Hungary in 2006. This is not the case for the lower bound, with several countries showing lower bounds and total inequality going in different directions. Not only do the upper bound estimate of IOp and total inequality move together, they are also close in level, suggesting that time invariant factors play an important role in explaining income inequality.[11]

Countries show a relatively stable level of IOp over time. Between 2005 and 2011, 10 out of 23 countries showed a decrease in their upper bound estimate, but most of the changes were small.[12] Only one country shows an increase larger than 0.003 points (Denmark), while three countries show a decrease of the same extent (Portugal, Estonia, and Greece).

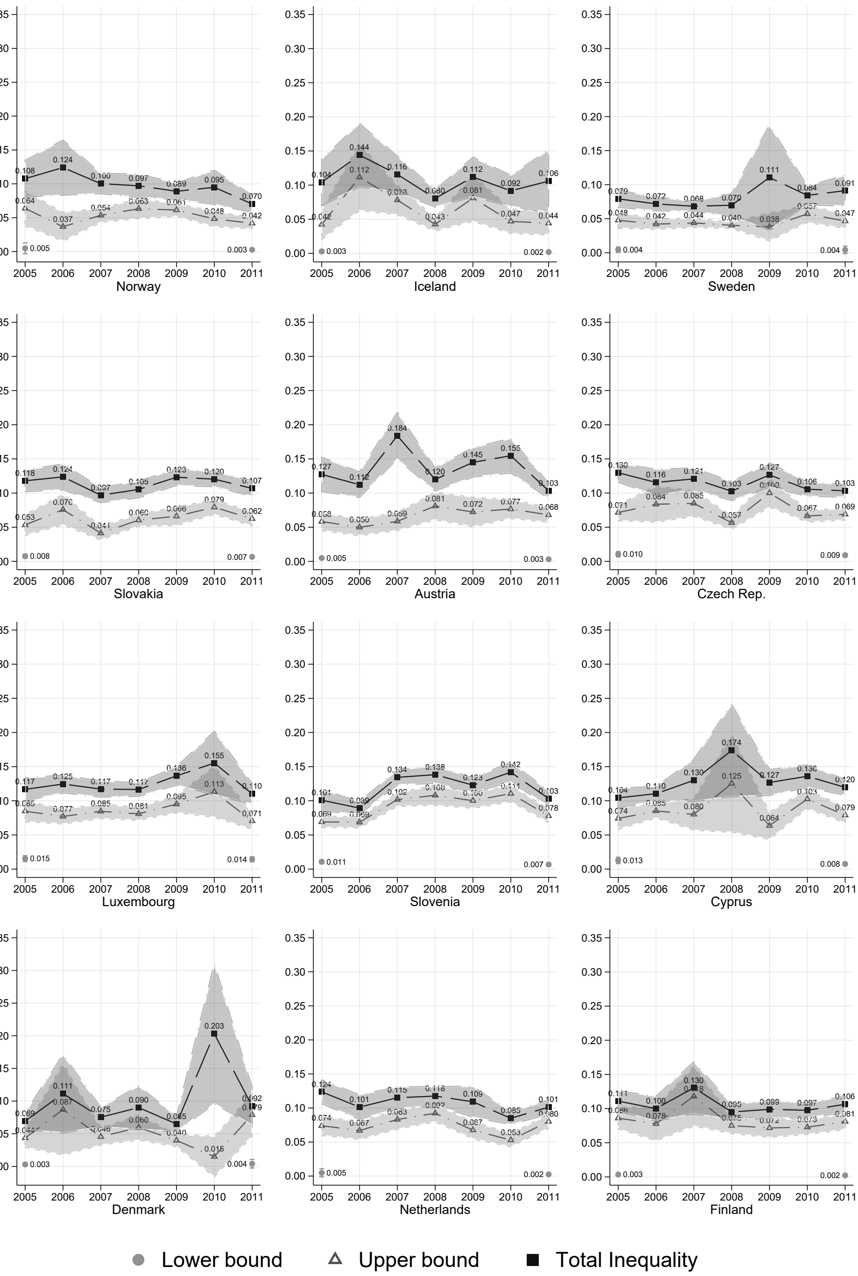
There are stark differences in the upper bound estimates of IOp. Figure 2.3 shows that, for 2011, upper bound IOp ranges from 0.04 to 0.15. Countries with low IOp include Sweden, Iceland, Norway, Slovakia, Austria, and the Czech Republic to a lesser extent. High IOp countries include Latvia, Estonia, and Lithuania, followed by Portugal, Poland, Greece, and Spain. Interestingly, this ranking differs from the lower bound IOp ranking, with countries such as the Netherlands and Finland having a low level of lower bound IOp and an intermediate level of upper bound IOp. The different rankings from the lower and upper bound IOp are discussed in section 2.4.2.

The estimates can also be discussed in relative terms, as the share of total income inequality explained by circumstances, by using the IOR. The IOR is the ratio between the level of IOp and the level of income inequality (see equations 2.10 and 2.11 in page 27), and the corresponding estimates are shown in the Appendix (figures 2.A.3 and 2.A.4 on pages 65-66). Looking at 2011, the lower bound IOp estimates range from 1.4% of total income inequality for Iceland to 11.6% for Luxembourg, while the upper bound estimates go from 41.7% for Iceland to 85.6% of

[11]The fact that upper bounds tend to move in tandem can also be seen as a stable ratio between IOp and total inequality (the IOp ratio, or IOR) in Figures 2.A.3 and 2.A.4 in the Appendix..

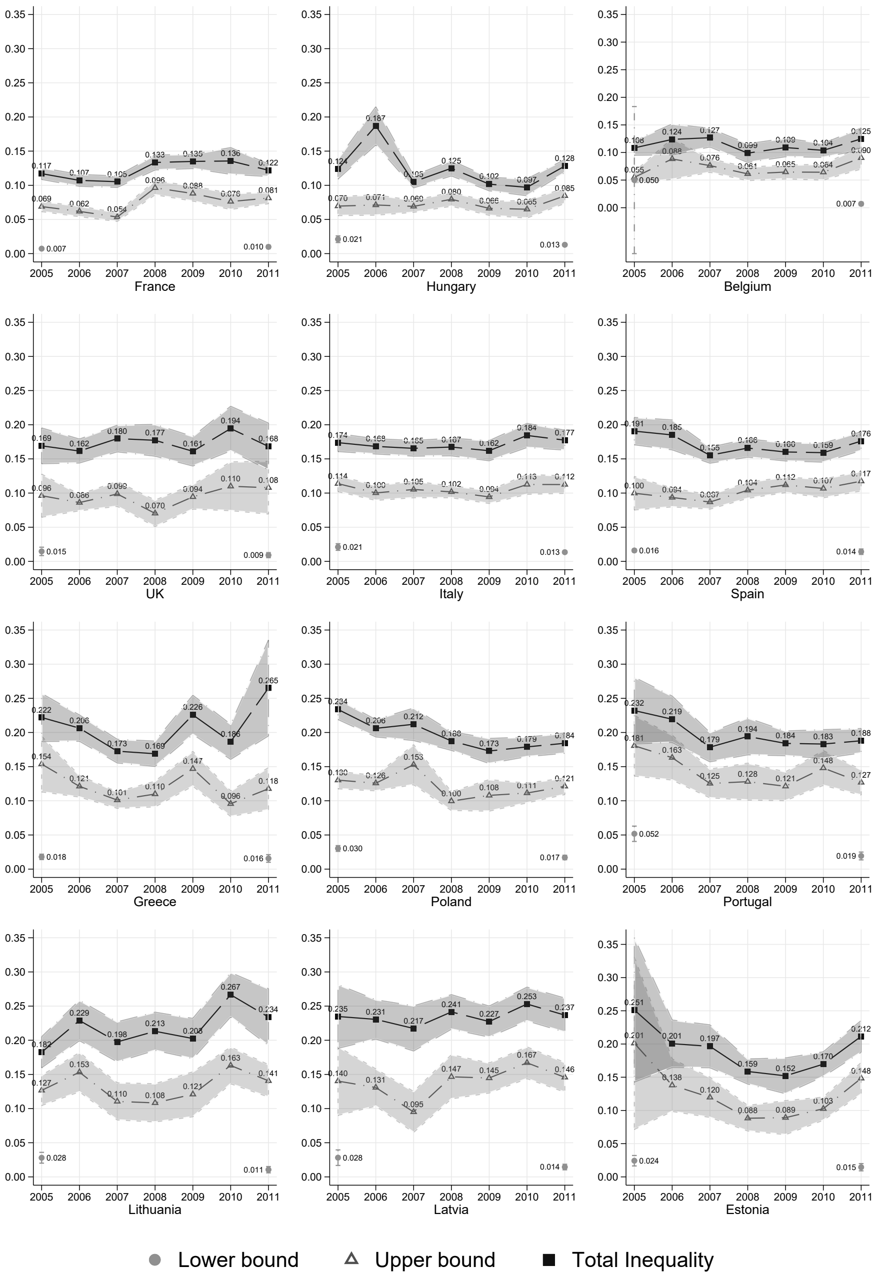
[12]I exclude Belgium as it has an extremely volatile lower bound estimate in 2005.

Figure 2.1a: Inequality of Opportunity level (IOL) by country (MLD)




Note: The figure includes the inequality of outcomes and inequality of opportunity trends between 2005 and 2011 for the countries in the sample. Total inequality is represented by squares, and the upper bound estimate of IOp is represented by triangles. Lower bound estimates (the circle) of inequality of opportunity are only available in 2005 and 2011. All inequality estimates use the Mean Logarithmic Deviation index. 95% confidence interval from a 1,000 iteration boostrap of the whole estimation process, with replacement.

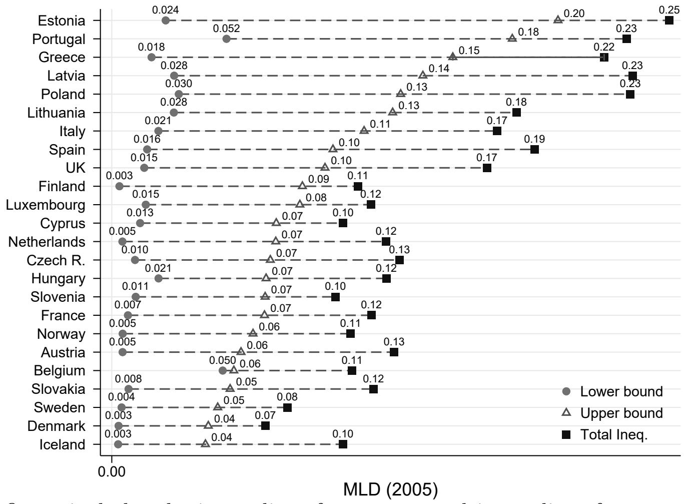
Figure 2.1b: Inequality of Opportunity level (IOL) by country (MLD)




Note: The figure includes the inequality of outcomes and inequality of opportunity trends between 2005 and 2011 for the countries in the sample. Total inequality is represented by squares, and the upper bound estimate of IOp is represented by triangles. Lower bound estimates (the circle) of inequality of opportunity are only available in 2005 and 2011. All inequality estimates use the Mean Logarithmic Deviation index. 95% confidence interval from a 1,000 iteration boostrap of the whole estimation process, with replacement.

income inequality for Denmark. Other high IOR countries include the Netherlands (79.2%), Finland (76.2%), Slovenia (75.5%), and Belgium (72.5%). The upper bound estimates of the IOR suggest that time invariant factors play a crucial role in determining inequalities.

Figure 2.2: Inequality of Opportunity by country (2005)

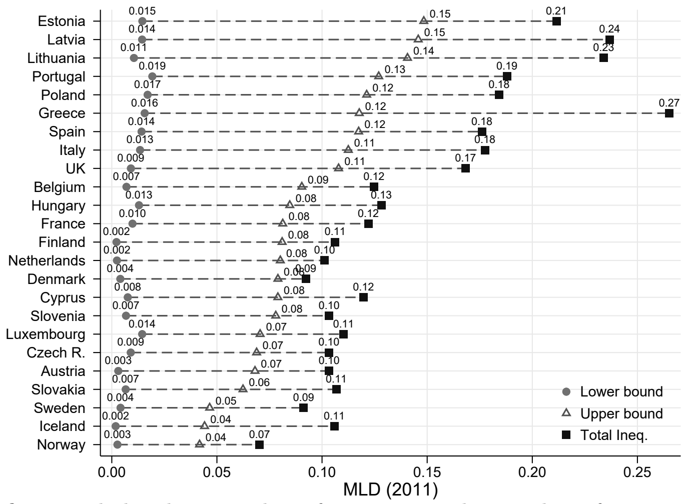



Note: The figure includes the inequality of outcomes and inequality of opportunity level in 2005 for all the countries in the sample. Countries are sorted by their upper bound estimate of inequality of opportunity. Total inequality is represented by squares, the upper bound estimate of IOp is represented by triangles, and the lower bound estimates is the circle. All inequality estimates use the Mean Logarithmic Deviation index.

While the upper bound and lower bound trends do not move together over time, they appear to be correlated for a given year. In 2011, the two bounds are correlated both in absolute terms and in relative terms (using the IOL and IOR, respectively). However, this is not the case for 2005, where the two bounds show no correlation. The changing relationship between the lower and upper bound estimates of IOp over time and between countries shows that the upper bound is not simply capturing the same information, but additional and – more importantly – new information.

The upper and lower bound estimates of IOp complement each other in the sense that the upper bound can be estimated in cases where there are no circumstance variables available, but contrary to the lower bound, provides no information on

Figure 2.3: Inequality of Opportunity by country (2011)




Note: The figure includes the inequality of outcomes and inequality of opportunity level in 2011 for all the countries in the sample. Countries are sorted by their upper bound estimate of inequality of opportunity. Total inequality is represented by squares, the upper bound estimate of IOp is represented by triangles, and the lower bound estimates is the circle. All inequality estimates use the Mean Logarithmic Deviation index.

the relative importance of each circumstance. However, despite the two bounds being somewhat related, they do not result in the same country rankings nor show the same trends over time. The following sections explore these issues.

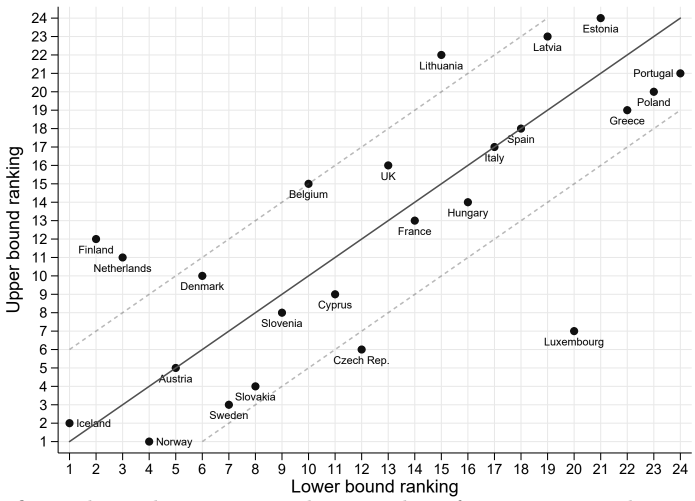
### 2.4.2 Differences between the lower bound and upper bound estimates: Rankings and changes over time

Upper bound estimates of IOp differ from lower bound estimates in two ways. First, I show how country rankings differ for a given year. Second, I show that time series show different trends. In this section I discuss how upper bound estimates can show an alternative picture of the importance of circumstances.

Figure 2.4 ranks all countries using the lower bound estimate (x-axis) and the upper bound estimate (y-axis) in 2011. The countries are ranked from lower IOp to higher, 1 being the country with lowest IOp. The diagonal line is the 45º degree line, where countries would be if their rank remained constant. The two dashed lines show changes in rank of at most 5 positions. Countries below the 45° line rank worse under the lower bound estimate while countries above the 45° line rank worse under the upper bound estimate.

Only four countries change their position by more than 5 positions. Finland, the Netherlands, and Lithuania have better positions in the lower bound ranking while The Czech Republic and Luxembourg have better positions in the upper bound rankings. On the other hand, only three countries (Austria, Italy and Spain) remain in the same position on the two rankings. The change in positions suggests that new time invariant factors (i.e., those not captured by the lower bound estimate) play different roles for different countries; for some countries they help their relative position, while for others they worsen it.

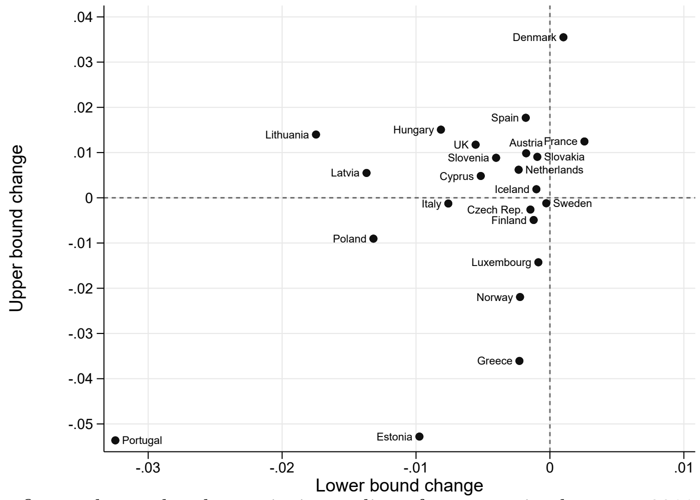
Figure 2.4: IOp ranking positions for 2011




Note: The figure shows the position in the inequality of opportunity ranking, going from the lowest level (1) to the highest (24). The x-axis shows the ranking for the lower bound estimate of inequality of opportunity, while the y-axis shows the ranking for the upper bound estimate. The solid diagonal line is the 45 degree line, while the two dashed lines represent a difference between the lower and upper bound rankings of at most 5 positions.

Figure 2.5 shows the absolute changes between 2005 and 2011 for both estimates. That is, it computes the difference between the 2011 and 2005 levels of IOp, both for the lower bound (x-axis) and the upper bound (y-axis) estimates. The dashed lines are set at zero and indicate no difference between 2005 and 2011. For the lower bound, only 2 countries (Denmark and France) have an increase in the lower bound estimate. On the other hand, around half of all countries (12 out of 23) move in the same direction when looking at the lower bound and upper bound estimates. While the upper bound estimate shows an heterogeneous result, the lower bound shows that most countries decreased their level of IOp in this period.

Figure 2.5: Changes in IOp between 2011 and 2005




Note: The figure shows the change in inequality of opportunity between 2011 and 2005 using the Mean Logarithmic Deviation index (i.e., $IO^{2011} - IO^{2005}$). The x-axis shows the change for the lower bound estimate of inequality of opportunity, while the y-axis shows the change for the upper bound estimate. The dashed lines represent no change between 2011 and 2005.

These comparisons suggest that the upper bound estimate of IOp is not just a 'larger' version of the upper bound. We see that both rankings and trends change when going from the lower to the upper bound estimate. These differences are driven by the relative importance of all additional time invariant factors – whether circumstance or effort – and how that differs across countries.

### 2.4.3 The gap between the upper and lower bound

The previous section shows how upper bound estimates of IOp provide new information, changing both rankings for a given year and trends over time. This new information captures time invariant factors that the lower bound estimate does not include, and can be country specific. In this section I explore these 'new' factors by exploring the gap between the upper and lower bound estimates of IOp. Concretely, I report the gap between IOp ratios, interpreted as the share of total inequality attributed to these new time invariant factors.

Let $\{Y_i^{LB}\}$ be the predicted counterfactual distribution for the lower bound approach and $\{Y_i^{UB}\}$ be the predicted counterfactual distribution for the upper bound approach. Given the IOR index defined in equations 2.10 and 2.11, the gap is computed as the difference between the level of IOp for each bound:

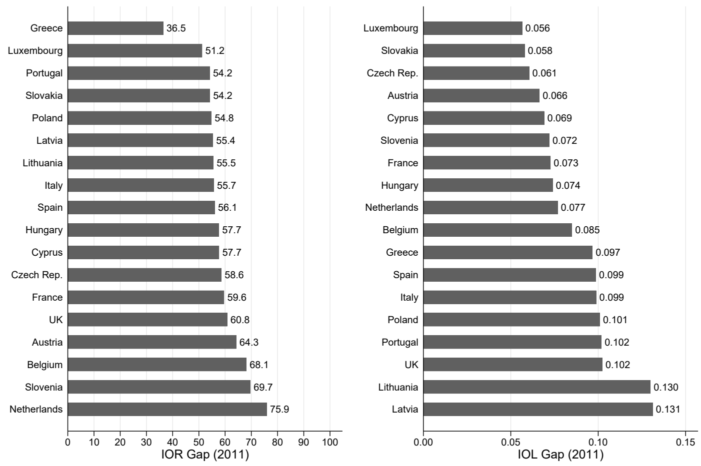
$$Gap = \frac{I(\{\hat{Y}_i^{UB}\}) - I(\{\hat{Y}_i^{LB}\})}{I(\{Y_i\})}. \tag{2.17}$$

I have discussed before the issue of overfitting of lower bound estimates when the sample of size is small. For that reason, I only report estimates of the gap for countries that estimated the 2011 lower bound using at least 4,000 observations, a cutoff based on the simulation in Brunori et al. (2018). For completeness, I also report estimates for the IOp level.

Figure 2.6 shows the size of the gap for each country 2011. On the left I show the IOR gap and the IOL gap on the right. The IOR gap goes from 36 percentage points for Greece to 77 percentage points for the Netherlands, with the gap for most countries ranging from 50 to 60 percentage points. The figure to the right shows that the ranking in levels differs from the rank in ratios. Luxembourg and Slovakia show the smallest absolute gap (0.06 points of the MLD) and Lithuana and Latvia have the largest gap (0.13 points). The size of the gap – whether absolute or relative – is substantive, representing from 4 to 31 times the size of the lower bound IOR estimate.

To put the size of the gaps in perspective we can compare these estimates with the only ones available, from Niehues and Peichl (2014) and Hufe et al. (2019). The former provides upper and lower bound estimates for Germany (2009) and the US (2007), while the latter includes estimates for 12 developing countries (10 of which use household income). Using the MLD index, they find that the gaps between the upper and lower bounds for gross annual income are 0.05 points for Germany and

Figure 2.6: Gap between lower and upper bounds (IOL)

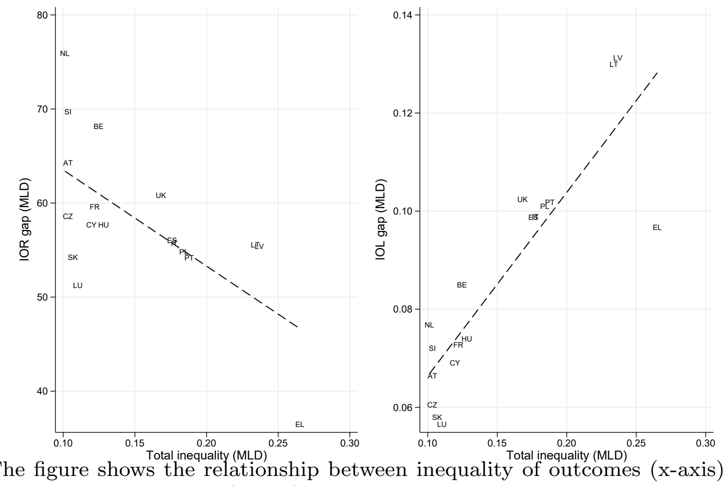



Note: The figure shows the gap between the lower and upper bound (i.e., the difference between the upper bound estimate of inequality of opportunity and the lower bound estimate, using the Mean Logarithmic Deviation index). All estimates for 2011. The figure on the left shows the gap in terms of the IOp ratio while the figure on the right shows the gap in terms of the IOp level. Excludes all countries where lower bound IOp was estimated with fewer than 4,000 observations.

0.06 points for the US. The gaps for developing countries are more heterogeneous, ranging from 0.01 to 0.34 points, with an average of 0.13. The gaps in this chapter are closer to the lower half of the Hufe et al. (2019) range and several countries are close the Germany and US levels. Interestingly, the difference in the size of the gaps between Niehues and Peichl (2014) and this chapter is explained by the lower bound estimates. While the upper bounds are similar in this chapter and their paper, the fact that they account for more circumstances results in higher lower bound estimate.

Figure 2.7 shows the association between the gap and inequality of outcomes, in a similar way to that of the 'Great Gatsby' curve Corak (2013). The figure on the left focuses on the relative gap (IOR) while the one in the right shows the absolute gap (IOL). We see a negative correlation between the IOR gap and total inequality, and a positive correlation between the IOL gap and total inequality. Between the two we conclude that the gap grows when inequality of outcomes is higher, but it grows to a slower rate than inequality of outcomes (the regression coefficient shows a rate of 0.4 points of the gap for each point of inequality of outcomes).[13]

Figure 2.7: Gap between bounds vs. total inequality



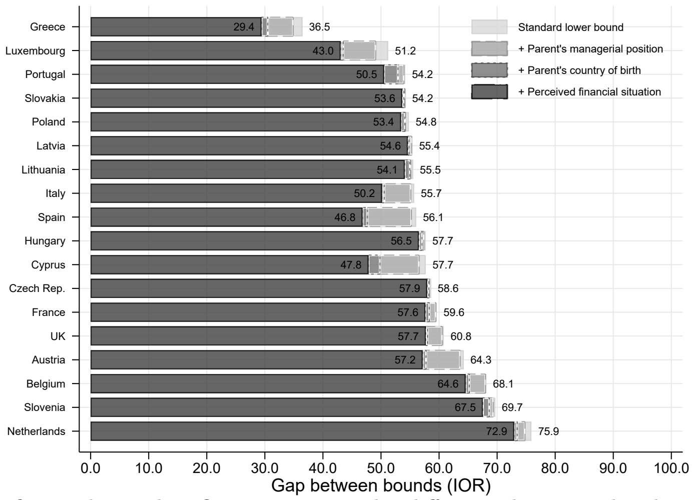

Note: The figure shows the relationship between inequality of outcomes (x-axis) and the inequality of opportunity gap (y-axis), all using the Mean Logarithmic Deviation index. The figure on the left shows the gap in terms of the IOp ratio while the figure on the right shows the gap in terms of the IOp level. The dashed line is the linear fit for each regression. Excludes all countries where lower bound IOp was estimated with fewer than 4,000 observations.

Because of how upper bound IOp is estimated I cannot infer what are the circumstances or time invariant efforts driving this findings. In order to explore this issue, I use the 2011 cross-sectional EU-SILC to study how the IOR gap decreases when I include additional circumstances that are not included in the analysis, as shown in figure 2.8.

I include three additional set of circumstances to get new lower bound estimates of IOp, which I use to re-calculate the gap between the upper and lower bound estimates. The new circumstances are included in the 2011 cross-sectional survey, but were not included in the analysis, as they did not appear in 2005, or the sample size decreased too much when including them. I first include whether the father or the mother had managerial positions when the respondent was growing up. The second set of circumstances accounts for whether the parents were born in the country, in the EU, or in the rest of the world. Lastly, the third set of circumstances includes the perceived financial situation and how easily the respondent's family

[13]A similar result is shown using a meta analysis of several different estimates of IOp for different countries (Brunori et al., 2013).

Figure 2.8: Reduction in the gap when including new circumstances




Note: The figure shows the IOp ratio gap – the difference between the share of total inequality accounted for by the upper and lower bound estimates of IOp – when new circumstances are included in the lower bound estimate. The rightmost level (darker in colour) is the standard gap. Countries are sorted using this metric. Each bar to the left represents the inclusion of a particular circumstance. The leftmost bar (lighter in colour) represents the inclusion of all three circumstances shown in the legend, and the level of the gap once they have been included. Excludes all countries where standard lower bound IOp was estimated with fewer than 4,000 observations.

could make ends meet.

Figure 2.8 shows the role played by these new circumstances in reducing the gap. The reduction in the gap goes from less than a percentage point for countries like Slovakia and the Czech Republic to almost 10 points for Spain and Cyprus. Luxembourg, Austria and Greece decrease the gap in 7 to 8 percentage points. Among these countries changes, it is the parent's country of birth that accounts for the largest part, particularly in Austria, Spain and Cyprus. Parents having a managerial occupation plays an important role in decreasing the gap in Greece and Luxembourg, while the perceived financial situation matters in Portugal and Cyprus. Two-thirds of the countries see a decrease below 5 percentage points, suggesting that these additional circumstances are strongly correlated with the already included circumstances, thus providing little additional information.

The decrease in the gap is limited, as the additional circumstances do not explain a substantial part of the omitted circumstances, but it is a first attempt to explain the gap between bounds. Future research should exploit large panel datasets in order to measure more exhaustive lower bound estimates while at the same estimating upper bound estimates with the same sample. Understanding what is behind the gap between upper and lower bound estimates will provide relevant insights for the literature on inequality of opportunity.

### 2.4.4 Direct and indirect paths: IOp when including effort variables

So far I have focused on the complete reduced form of my model, considering the complete influence of circumstances, both directly but also through efforts. I opted for this approach as my interest is on measuring the impact of circumstances. However, studying channels might provide important information on how circumstances influence inequality, but they also matter as the indirect influence of circumstances is not considered a circumstance by all authors (Jusot et al., 2013). In this section I expand the model discussed in section 2.2.4 by removing the influence of time-invariant effort from my measure of circumstances.

I consider four measures of effort. First, I include the activity status of each individual (at work, unemployed, retired or inactive). Second, I include the marriage status (never married, married or other). The third effort variable is the number of worked hours per week (zero, 1–21, 22–39, 40, or more than 40). The final effort variable is the years of education. With the exception of education, all variables are categorical. These are relatively standard effort variables and represent autonomous choices taken by the individual.[14]

To account for efforts I follow the original approach of Niehues and Peichl (2014). Their approach seeks to control for efforts (particularly, the time invariant component of effort) and to remove it from our measure of circumstances. Considering that the direct indirect influence of circumstances can be treated as either an effort or a circumstance, I provide to models. The first model removes the complete influence of efforts and the second model only removes its direct influence. This is done by expanding equation estimating a fixed effect (i.e., my measure of circumstance) that is uncorrelated to efforts:

$$log(Y_{it}) = \alpha + \eta_i^{E_1} + u_t + \gamma E_i + \varepsilon_{it}. \tag{2.18}$$

Where $E_i$ is a measure of 'absolute' efforts and includes all previously mentioned variables. From equation 2.18 we recover the predicted fixed effect $\hat{\eta}_i^{E_1}$ and use it to estimate IOp, as in equation 2.15.

$$log(Y_{is}) = \psi^{E_1}\hat{\eta}_i^{E_1} + \omega_{is}. \tag{2.19}$$

The upper bound estimate of IOp is then estimated over the counterfactual distribution of $\hat{\psi}^{E_1}\hat{\eta}_i^{E_1}$.

While this approach controls for the total influence of the effort variables, one could

[14] While similar, the difference in data sources does not allow me to use the same circumstances as in Niehues and Peichl (2014). They include weekly working hours, age-standardized experience, individual's education in years, and industry dummies.

argue that the indirect influence of circumstance on efforts is itself a circumstance. To correct for this, we need a 'clean' or 'relative' measure, uncorrelated with circumstances. This is done by including an intermediate step before measuring IOp.

We start by estimating equation 2.13 and recovering a predicted fixed effect $\hat{\eta}_i$ and use that fixed effect to 'remove' its influence on each effort variable $E_i$:

$$E_{it} = \alpha + \hat{\eta}_i + \mu_t + u_{it}. \tag{2.20}$$

We then re-estimate the fixed effect, now controlling for our measure of 'relative' effort, the predicted residual from equation 2.20, $\hat{u}_{it}$.

$$log(Y_{it}) = \alpha + \eta_i^{E_2} + u_t + \gamma\hat{u}_{it} + \varepsilon_{it}. \tag{2.21}$$

And we use the predicted fixed effect from this equation to estimate IOp.

$$log(Y_{is}) = \psi^{E_2}\hat{\eta}_i^{E_2} + \omega_{is}. \tag{2.22}$$

The upper bound estimate of IOp is then estimated over the counterfactual distribution of $\hat{\psi}^{E_2}\hat{\eta}_i^{E_2}$.

As a result, I have three upper bound estimates of IOp for each country-year pair. The benchmark estimate, that accounts for both the direct and indirect influence of circumstances, a second estimate that completely removes the influence of efforts from my measure of circumstances (using an 'absolute' measure of effort), and a third and intermediate estimate that considers the indirect influence of circumstances as a circumstance (thus using a 'relative' measure of effort). Figure 2.A.5 and 2.A.6 in the Appendix show these three measures of IOp.

From these figures we see that the overall impact of efforts is limited. The IOR does not show substantial changes once we remove the influence of efforts from our measure of circumstances. As expected, the measure of IOp including 'relative' efforts lies somewhere in the middle of the two. We also see that the biggest differences lie between the benchmark and the other two estimates, suggesting that the indirect influence of circumstances on efforts makes little difference on IOp.

Among the complete panel, only 7 data points show a change in the IOR of more than 10 percentage points: Cyprus (2007 and 2011), Luxembourg (2008 and 2011), Denmark (2007), Estonia (2008) and the UK (2008). These are all countries that suffered, to different extents, from both the Great Recession and the European Debt Crisis. It might be the case that these countries where particularly hit in terms of the effort variables (particularly on job status, with many workers becoming inactive or unemployed), making the 'time invariant' aspect of these efforts much more important. However, the general change is small, with almost 80% of the cases seeing changes below 5 percentage points.[15]

A last point is the role played what Roemer and Trannoy (2016) call 'preference shifters' and market conditions. Preference shifters are demographic factors that, while not necessarily considered circumstances, might shift how circumstances influence efforts. For example, how older workers might react differently from younger workers while having the same circumstances. Similarly, market conditions are time-contingent and external factors that might shape individual efforts, such as looking for a job in the middle of a recession. Depending on the definition of circumstances, these factors might or might not be considered to be sources of unfair inequality.

My benchmark estimates treat preference shifters as circumstances, as they are accounted for in the reduced form equation. This is consistent with the 'control' view of IOp (see Roemer and Trannoy (2016)). Market conditions, on the other hand, are not included in my benchmark estimate of IOp, as the predicted fixed effect removes the influence of year fixed effects. However, the predicted fixed effect (and thus, the upper bound IOp estimate) does not account for sub-national differ-

[15]Niehues and Peichl (2014), who study Germany in 2009 and the US in 2017, only find large differences among men in Germany, with negligible differences for women in Germany for the US. Perhaps consistent with my estimates, these differences could capture the exceptional context of the Great Recession in Europe.

ences in a given country, for example regional unemployment rates or price levels, and are therefore excluded from my estimates. While both preference shifters and market conditions (to the extent that they can be captured) could be included, the small differences shown in Figures 2.A.5 and 2.A.6 suggest that their influence is negligible.

## 2.5 Robustness checks

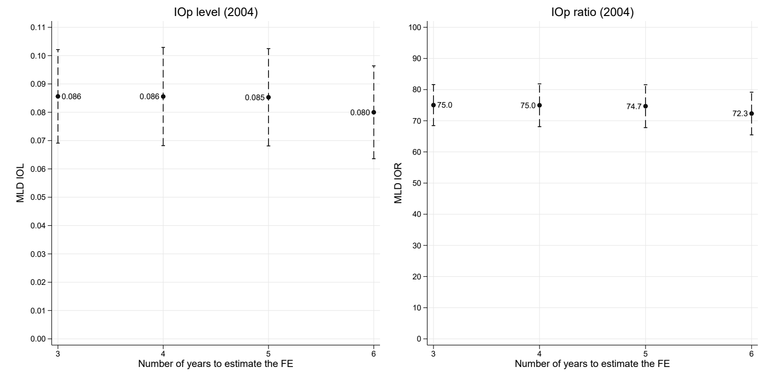
In this section I explore two departures from the methodological assumptions described in section 2.2. The first subsection explores how upper bound estimates change when we use a larger period of time to estimate fixed effects. It also discusses the more general implications of predicting fixed effect with few years of data. The second subsection explores the lower bound estimation. Particularly, how it changes when we improve comparability with the longitudinal dataset. These two departures bridge the space between the original methodology and this chapter's application of it.

### 2.5.1 Upper bound estimates and the choice of time window

In this section I explore the implications of the departures from the approach as proposed in Niehues and Peichl (2014). The first one is to estimate the fixed effect for year $t$ with the three following years ($t + 1$, $t + 2$ and $t + 3$) instead of the previous years. This is a choice and I do it to I can provide upper bound estimates for the same years as the lower bound estimates. The second departure is to use a shorter time frame, as the EU-SILC only includes a rotating panel of 4 years.

To explore these departures, I exploit the fact that the 2010 survey for Luxembourg includes 7 waves of data (2004–2010). Using this sample, I estimate IOp for 2004 using predicted fixed effects estimated under different numbers of years (from 3

Figure 2.9: IOp with FE including additional years




Note: The figure shows the upper bound estimates of inequality of opportunity for Luxembourg in 2004, together with its bootstrapped 95% confidence interval. Estimates use following years (2005 onwards) to estimate the predicted fixed effect and from 3 (my benchmark) to to 6 years of data to estimate the fixed effects. The left panel reports the IOp level (IOL) and the right panel reports the IOp ratio (IOR).

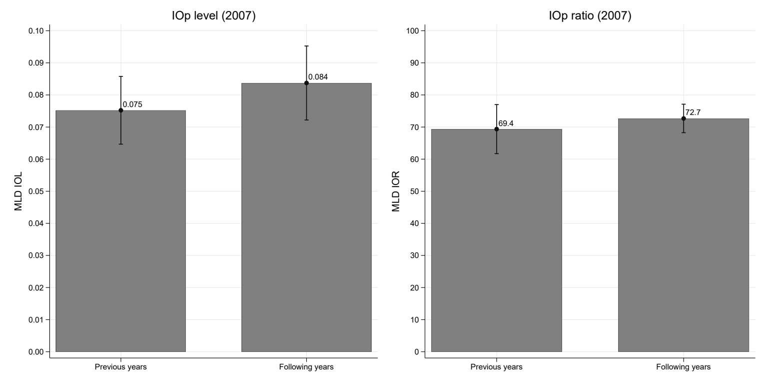
to 6). Similarly, I estimate IOp for 2007 using the previous years (2004 to 2006) as well as the following years as in this chapter (2008 to 2010). Together, these exercises show that these methodological departures make little difference in the IOp estimates.

Figure 2.9 shows the 2004 upper bound IOp estimates together with their bootstrapped confidence intervals. The horizontal axis shows the number of periods for each case, from 3 (2005 to 2007) to 6 (2005 to 2010). The figure to the left shows the estimates for the IOL, while the figure to the right shows the IOR estimates.

The figure shows that IOp estimates are robust to the number of periods to estimate the fixed effect. There is a slight decrease in the level of inequality when more periods are included, going from 0.086 to 0.08 (something I discuss further in the simulation), but all confidence intervals overlap. We see something similar for the IOR, with the share of inequality explained by circumstances going from 75% to 72.3% as the number of years increases. While IOp estimates decrease as the number of years used to estimate the fixed effect grows, this increments fall within the confidence interval of my benchmark estimates.

Figure 2.10 shows the 2007 upper bound IOp estimate when estimated with the

Figure 2.10: IOp with FE from previous or following years




Note: The figure shows the upper bound estimates of inequality of opportunity for Luxembourg in 2007, together with its bootstrapped 95% confidence interval. The first bar in each panel uses previous years (2004–2006) to estimate IOp and the second bar uses following years (2008–2010). The left panel reports the IOp level (IOL) and the right panel reports the IOp ratio (IOR).

previous and following years, and their confidence intervals. The first bar in each panel uses previous years (2004–2006) to estimate IOp and the second bar uses following years (2008–2010). The figure to the left shows the estimates for the IOL, while the figure to the right shows the IOR estimates.

Figure 2.10 shows very small differences when using previous and following years to estimate the fixed effect. The 2007 IOp level is 0.075 in the former case and 0.084 in the latter. The ratio, on the other hand equals 69.4% and 72.7%, respectively. Both for the level and ratio we see that the point estimates fall within the confidence intervals of the other estimate.

A relevant question that arises from this exercise is whether Luxembourg is representative of the countries in the EU-SILC. Luxembourg has a median income that is more than twice the EU average, as well as a higher GDP growth in recent years. On the other hand, Luxembourg has levels of inequality and poverty close to the EU average.[16] In this sense, we can say that Luxembourg is a representative country for the purposes of our analysis. Indeed, previous studies have also used Luxembourg as a case study (Törmälehto et al., 2013, pp.189-202).

[16]Source: Eurostat - ec.europa.eu/eurostat/data/database. Inequality is measured using the Gini index and the Quintile share ratio. Poverty is the AROPE rate.

As a final focus, I discuss how fixed effect estimates change as the number of years used in their estimation increases. The problem with the estimation of fixed effect is that, for a fixed $T$, increasing the sample size increases the number of parameters to estimate and might not result in a consistent estimation of these parameters. This problem is typically referred to as the 'incidental parameter' problem (Neyman and Scott, 1948; Lancaster, 2000). For the purpose of this chapter, my interest lies in determining how this problem impacts the upper bound estimate of IOp.

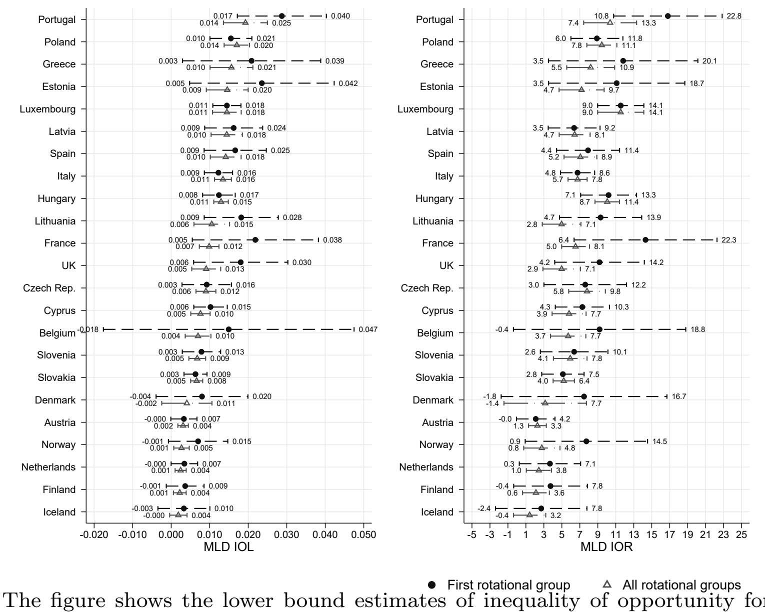
Figure 2.9 already hints the answer. Increasing the number of periods (for a fixed sample size) slightly reduces the upper bound estimate of IOp. This is consistent with the simulation in Buddelmeyer et al. (2008), who show for the least squares dummy variable estimator that the root mean squared error more than halves when going from $T = 5$ to $T = 10$, irrespective of the sample size (halving again for $T = 20$). The source of bias is intuitive; the shorter the time frame, the more things appear to be time invariant, resulting in a larger fixed effect estimator and higher upper bound estimate of IOp.

I provide a simulation to focus on the IOp measurement rather than the bias in the fixed effect estimator. The model includes time invariant circumstances as well as a time invariant component of efforts. I discuss the details of the simulation in section 2.B in the Appendix. My simulation shows that, for a 'real' IOp ratio of around 16%, the upper bound estimate should be around 35% for $T = 3$ (my case), around 30% when $T = 5$ and around 26% when $T = 15$. Upper bound estimate of IOp over estimates the true IOp ratio (of roughly 15%) by 17 to 21 percentage points when the time dimension is short ($T = 3$), depending on the sample size. This over estimation falls to 13-14 percentage points for $T = 5$ and for 10-15 for $T = 15$. While simple, the simulation exemplifies why longer time horizons result in smaller estimates (i.e., closer to the real level) of IOp. Nonetheless, relative to the 'real' level of IOp, the differences between the different upper bound estimates are minor. It appears that longer time horizons result in better (i.e., closer to the 'real' level of IOp) upper bound estimates, but they still overestimate the true influence of circumstances by a substantial margin.

### 2.5.2 Lower bound estimates for a differently defined cross-sectional sample

The longitudinal component of the EU-SILC includes four rotational groups, each being interviewed four times in a staggered design. The upper bound approach requires respondents with at least four waves of data, which constrains – for any given year – my analysis to the first rotational group. On the other hand, the lower bound estimate of IOp uses all respondents. These longitudinal and cross-sectional SILC cannot be merged to use a common sample but I can re-estimate the lower bound the first rotational group alone.[17]

Figure 2.11: Lower bound IOp (2011): Complete and first rotational samples




Note: The figure shows the lower bound estimates of inequality of opportunity for each country, comparing for each case the use of the whole longitudinal sample (triangle) with the use of only the first rotational group of the longitudinal sample (circle) – the same sample used to obtain the upper bound estimates of inequality of opportunity. All measures use the Mean Logarithmic Deviation index. The figure to the left represents the inequality of opportunity level, while the figure to the right is the inequality of opportunity ratio. Each estimate includes a bootstraped 95% confidence interval.

[17] In practice, the cross-sectional and longitudinal files are based on the same sample of households (Iacovou et al., 2012), but this may not always be the case, as countries are allowed to use different survey instruments if desired.

To derive estimates based on a more consistently defined sample, I re-estimate all lower bound estimates of IOp for 2011 using only the first rotational group of that year, i.e., the respondents that were surveyed the first year, and that will be interviewed at least three more times. Given that I use the 2014 sample to estimate the upper bound of IOp for 2011, I can compare the same group of respondents in both cases, as they use the same survey instrument.

Figure 2.11 shows lower bound estimates of IOp for 2011 using only the first rotational group of that year as well as the complete sample. The first rotational group excludes Sweden as there are no available observations in that group in that year. We see small differences between using the first rotational group and the complete cross-sectional sample, with all confidence intervals overlapping for each country. The median absolute difference is 0.002 points of the MLD, while the average is 0.003. France and Portugal show the largest differences, 0.12 and 0.009, respectively. These estimates suggest that focusing on the first rotational group does not make a large difference, so using the complete cross-sectional and the longitudinal sample should allow for comparability between the two estimates.

## 2.6 Discussion

Inequality of opportunity has gained national and global recognition as an issue that needs to be addressed. However, most methods estimate its lowest possible level, providing no information on its upper bound. I address this problem by estimating upper bounds of inequality of opportunity for household income. These estimates use panel data to capture time invariant factors, which are then interpreted as a measure of all circumstances plus time invariant efforts. Using the EU-SILC, I provide estimates for 24 European countries between 2005 and 2011, showing that it is possible to provide comparisons of IOp that can, at the same time, (1) include a large number of countries, (2) go beyond the lower bounds of the true level of IOp, and (3) be comparable over time and over countries. I apply the upper bound approach proposed by Niehues and Peichl (2014), which together with Hufe et al. (2019), are the first papers to apply this approach. By using EU-SILC data, this is the first piece of research to provide the upper bounds for countries over time, as well as the first to discuss the estimation issues that arise when using short running panel surveys.

My estimates show that IOp could potentially determine a substantial part of inequality of outcomes. For 2011, total inequality of income ranges from a MLD index of 0.07 for Norway to 0.265 for Greece. On the other hand. the lower bound estimates range from 0.002 for Iceland and Finland, to 0.036 for Romania. In relative terms, the lower bounds explain between 1.4% (Iceland) and 17.4% (Romania) of inequality of outcomes while the upper bounds explain between 34.5% (Switzerland) and 85.7% (Romania). Over time, IOp trends remain relatively stable, with very few countries showing large changes between 2005 and 2011.

I find that the upper bound estimates of IOp are not just a larger version of the lower bound estimates, they provide new insights about IOp trends. Country rankings differ when using lower bound or upper bound estimates of IOp, with some countries even changing 10 positions between rankings. The same holds true for comparisons over time. Between 2005 and 2011, only two countries show increases when looking at the lower bound estimate, but almost two thirds of countries show an increase when considering the upper bound estimate of IOp. The fact that, over time, the upper bound and the lower bound move together for some countries and in opposite directions for others suggests that omitted circumstances and time invariant efforts – the factors captured in the upper bound estimate but not in the lower bound– differ in their relative importance across countries.

To provide a better understanding of what is behind the upper bound estimate of IOp, I explore the gap between it and the lower bound estimate. I show that the gap varies greatly across countries, ranging from 0.06 points of the MLD index for Luxembourg and Slovakia to 0.13 for Lithuania and Latvia. In relative terms, the gap accounts from 37% to 76% of inequality of outcomes. Using the few circumstances that are available in the EU-SILC, I provide preliminary estimates in this direction. However, future research should explore in more detail how omitted circumstances can explain the gap between bounds.

Providing upper bound and lower bound estimates together gives us a better idea of the true level of IOp than just showing the lower bound, as most papers do. If our goal is to use these measures as a way of understanding intergenerational links and as a way of monitoring progress towards an equal opportunity goal, providing a bounded range of estimates rather than just a single estimate allows for a more nuanced way of moving forward. Upper bound estimates also show a closer relationship with inequality of outcomes than when we look at lower bound estimates, not only by being closer in absolute terms, but by showing a stronger correlation both over time and cross-sectionally. This correlation points to a point raised by Atkinson (2015), among others. Inequality of outcomes today affects IOp for the next generation, and if we care about IOp, we need to address inequality of outcomes as well.

## 2.A Appendix

### 2.A.1 On the assumptions behind the upper bound approach

The main assumption behind the upper bound approach is that circumstances, and their effect on the outcome, do not change over time. Specifically, this means that the predicted fixed effect $\hat{\eta}$ in equation 2.15 and the $\tilde{\beta}$ coefficient in equation 2.14 should hold constant for every year. I explore whether these assumptions hold empirically, by estimating both parameters for every country, over time.

Estimates are shown in figure 2.A.1 for the mean fixed effect and on figure 2.A.2 for the effect of circumstances on the outcome (i.e, the $\tilde{\beta}$ coefficient). We see that the parameters are relatively constant over time, particularly in figure 2.A.2. We do see some exceptions, however. For example, the mean fixed for Latvia increases over time, as does Italy to a lesser extent. We also see a slight decreasing trend for Greece in figure 2.A.1 and for Estonia in figure 2.A.2. Overall, both assumptions appear to hold in a reasonable manner.

Figure 2.A.1: Mean Fixed Effect by country

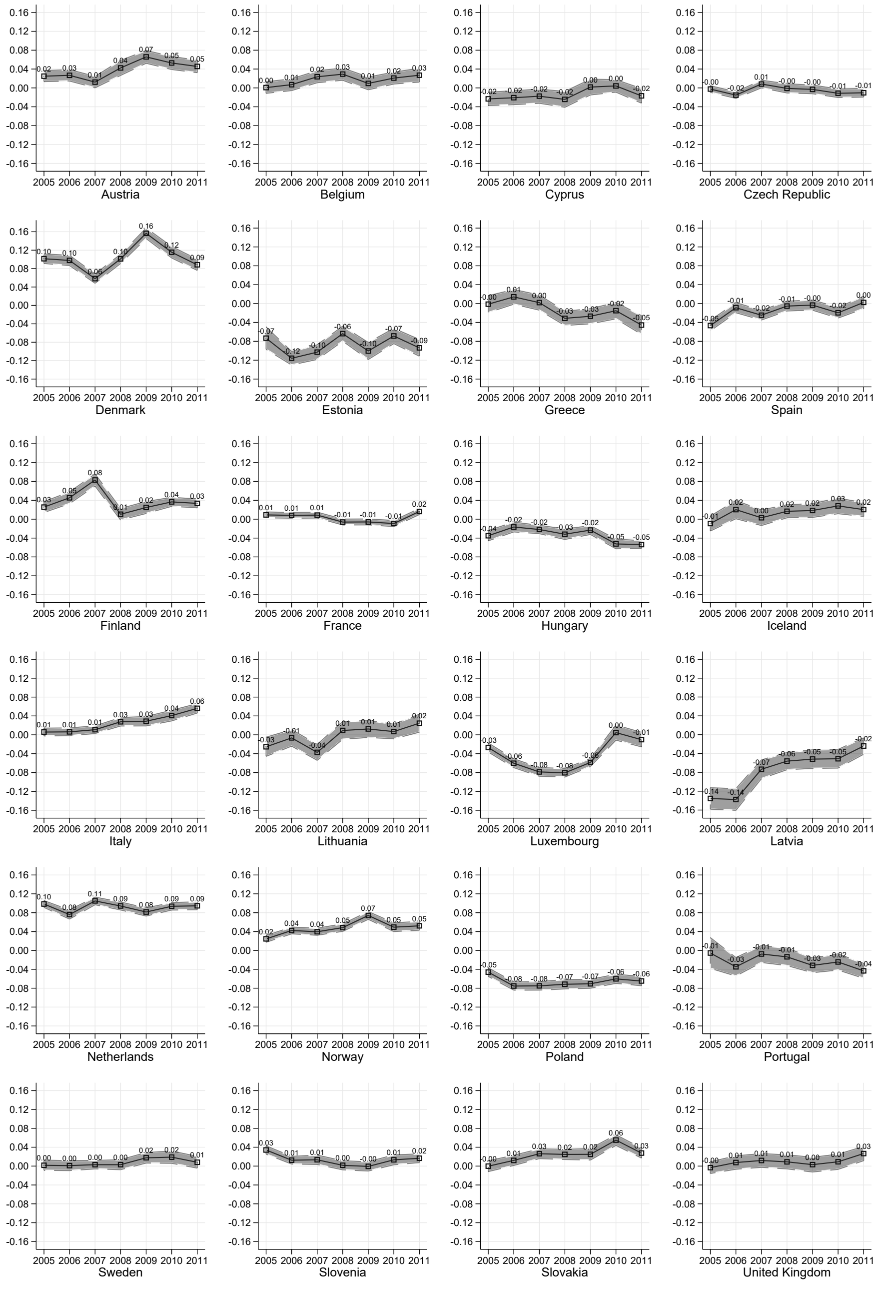
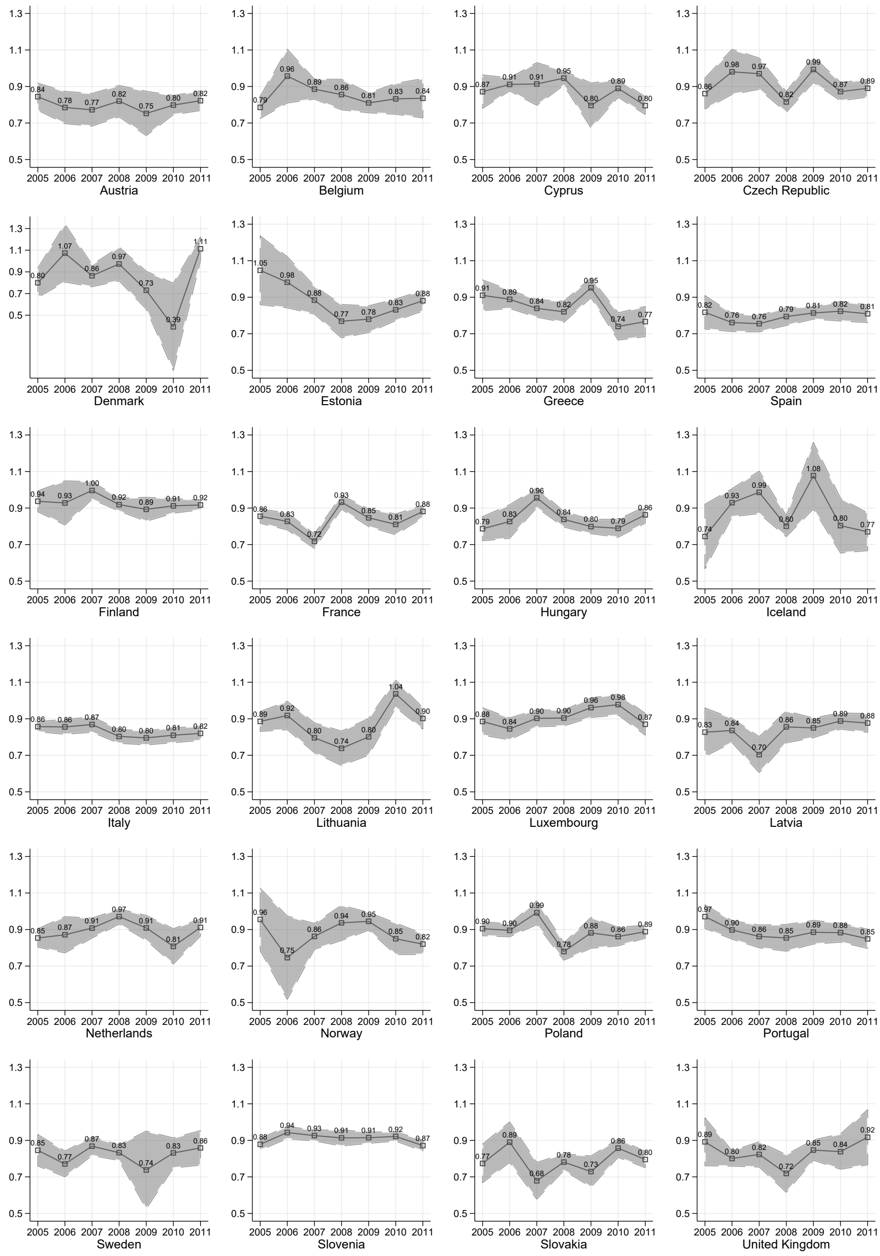


Figure 2.A.2: Coefficient of the circumstance variable by country



Note: The y-axis for Denmark is in a different scale.

### 2.A.2 Number of observations by country

Table 2.A.1: Observations in the longitudinal sample (Final sample only)

| | 2005 | 2006 | 2007 | 2008 | 2009 | 2010 | 2011 |
|---|---|---|---|---|---|---|---|
| Austria | 1,104 | 1,070 | 1,156 | 1,123 | 1,292 | 1,298 | 1,107 |
| Belgium | 1,195 | 1,128 | 1,159 | 938 | 1,023 | 1,071 | 1,018 |
| Cyprus | 928 | 889 | 848 | 723 | 716 | 1,432 | 995 |
| Czech Republic | 3,371 | 2,921 | 2,281 | 1,578 | 2,056 | 2,090 | 1,902 |
| Denmark | 1,002 | 913 | 952 | 919 | 856 | 756 | 660 |
| Estonia | 484 | 1,303 | 1,166 | 1,147 | 938 | 1,120 | 1,112 |
| Greece | 1,138 | 1,318 | 1,158 | 1,402 | 1,281 | 1,121 | 1,000 |
| Spain | 2,911 | 2,967 | 3,145 | 3,175 | 3,005 | 2,726 | 2,369 |
| Finland | 1,570 | 1,461 | 1,364 | 1,285 | 1,190 | 2,176 | 2,332 |
| France | 4,764 | 4,762 | 5,021 | 5,195 | 5,526 | 5,081 | 4,903 |
| Croatia | - | - | - | - | - | 1,058 | 1,007 |
| Hungary | 1,692 | 1,875 | 1,999 | 1,773 | 2,377 | 1,885 | 3,267 |
| Iceland | 575 | 519 | 561 | 609 | 585 | 543 | 567 |
| Italy | 4,390 | 4,140 | 4,294 | 3,857 | 3,201 | 2,815 | 3,508 |
| Lithuania | 784 | 1,125 | 1,184 | 1,087 | 1,104 | 1,265 | 995 |
| Luxembourg | 2,801 | 2,872 | 2,933 | 2,946 | 3,311 | 855 | 787 |
| Latvia | 751 | 817 | 1,022 | 1,229 | 1,140 | 1,237 | 1,185 |
| Netherlands | 2,786 | 1,418 | 2,245 | 2,055 | 1,826 | 2,032 | 2,145 |
| Norway | 2,734 | 2,478 | 2,400 | 2,168 | 2,020 | 1,486 | 1,425 |
| Poland | 3,709 | 3,922 | 3,717 | 3,269 | 3,243 | 3,300 | 3,128 |
| Portugal | 334 | 939 | 1,030 | 972 | 1,259 | 1,245 | 1,470 |
| Sweden | 1,230 | 1,192 | 1,446 | 1,064 | 1,092 | 979 | 801 |
| Slovenia | 2,117 | 2,141 | 2,207 | 2,538 | 2,346 | 2,186 | 2,104 |
| Slovakia | 1,352 | 1,382 | 1,582 | 1,569 | 1,575 | 1,507 | 1,546 |
| United Kingdom | 1,552 | 1,266 | 1,227 | 1,141 | 988 | 1,013 | 1,105 |

## 2.A.3 IOp estimates with 95% confidence intervals

Figure 2.A.3: Confidence interval for IOR (MLD - part 1)

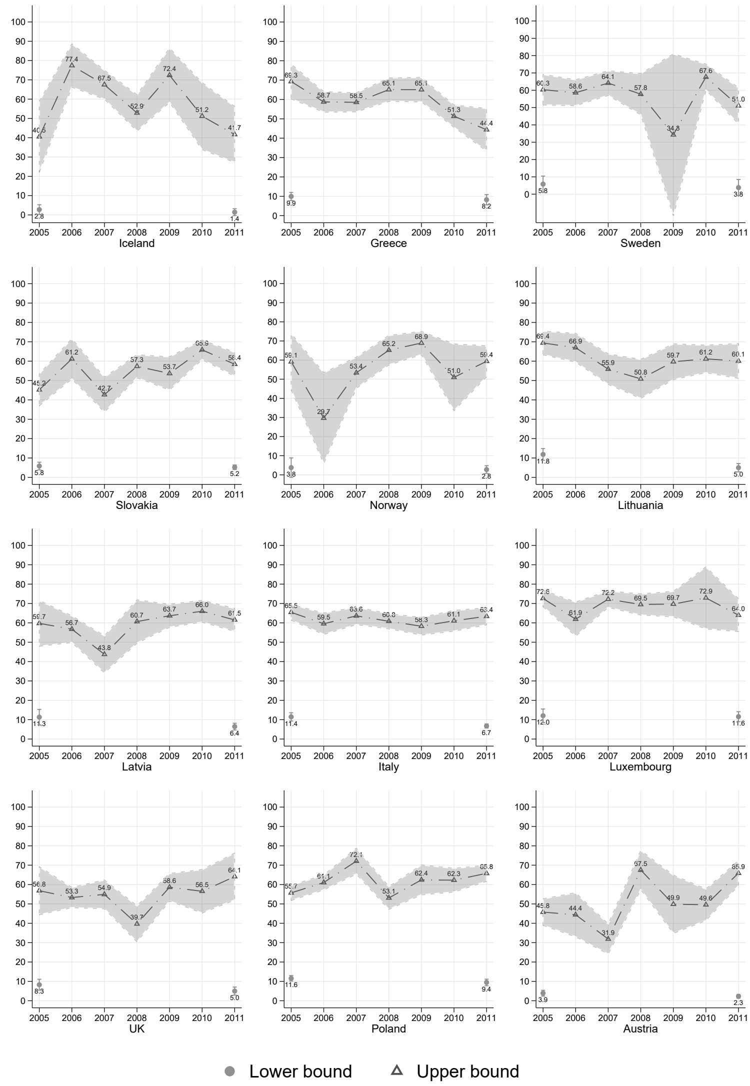
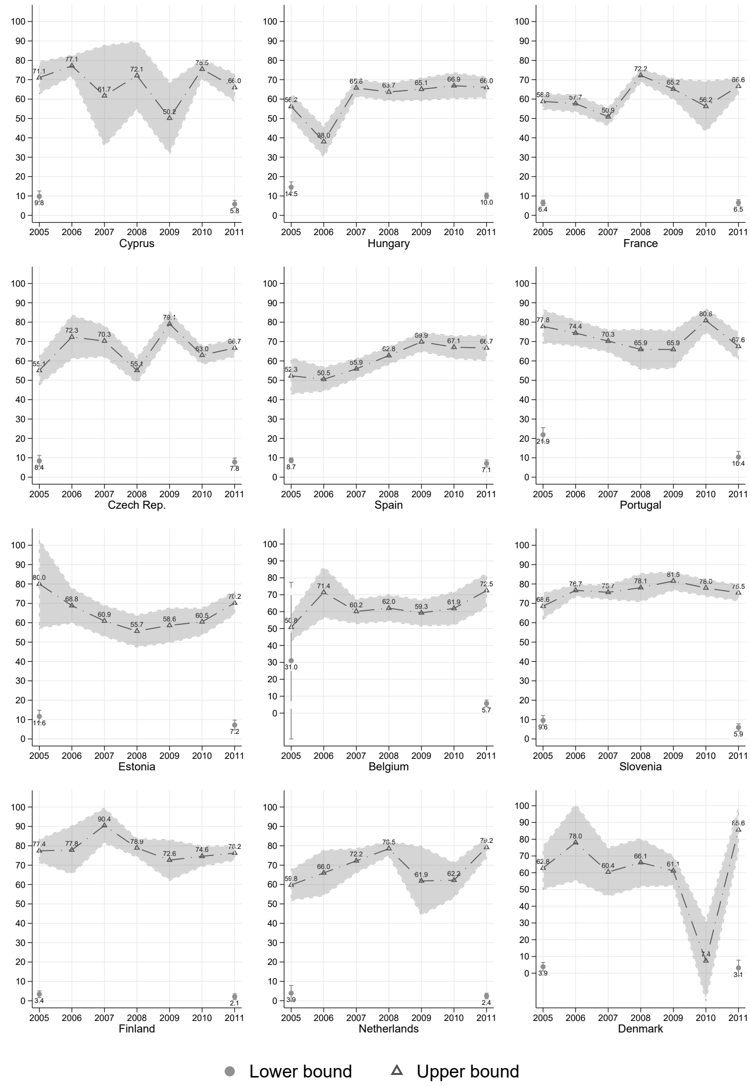
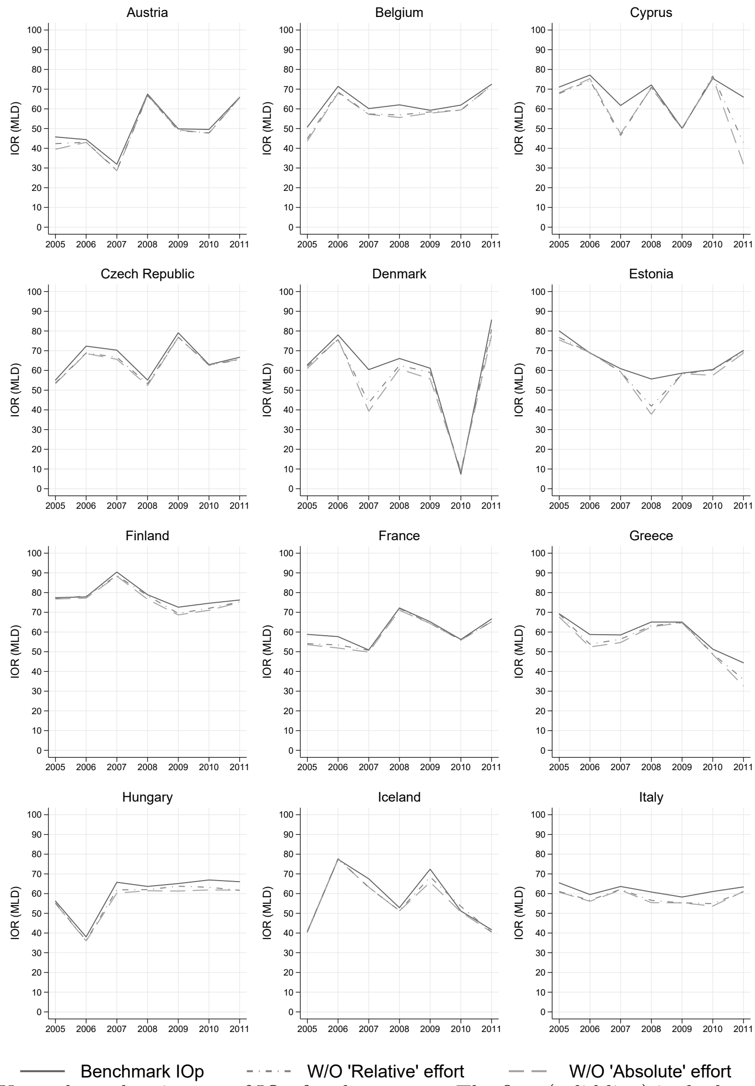


Figure 2.A.4: Confidence interval for IOR (MLD - part 2)



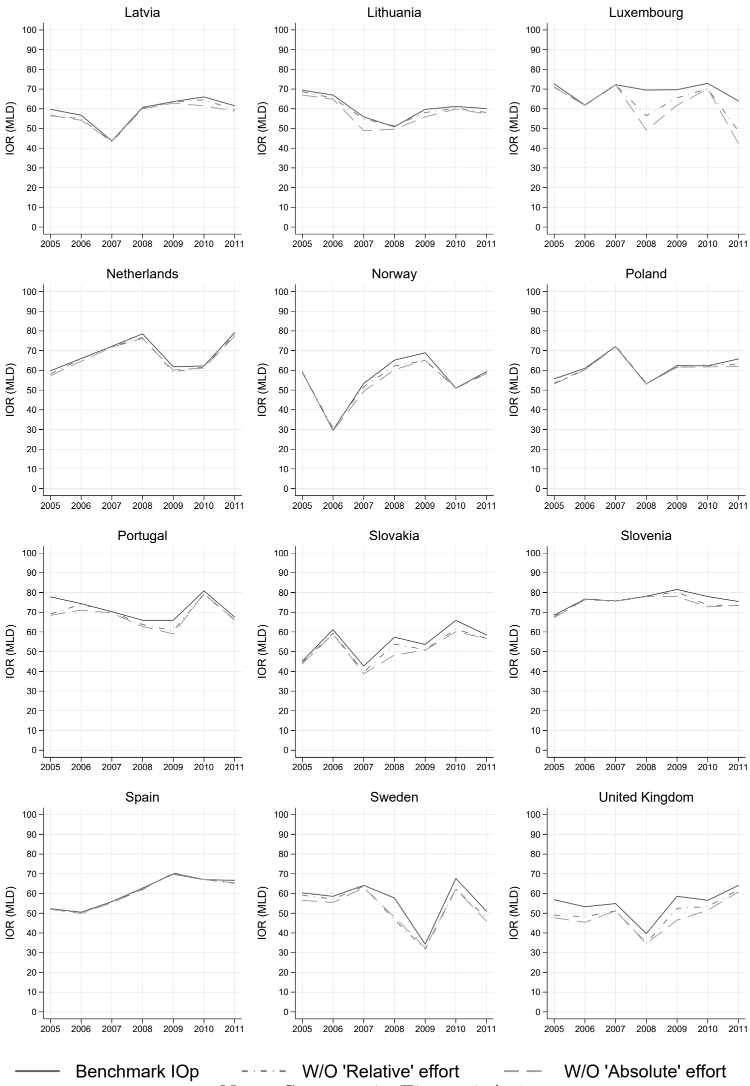
## 2.A.4 IOp estimates when including effort variables

Figure 2.A.5: Upper bound IOp ratio with efforts (MLD - part 1)




Note: Upper bound estimates of IOp for three cases. The first (solid line) is the benchmark estimation from this chapter. The second (dashed line) accounts for 'absolute' efforts when estimating the predicted fixed effect. The third (short-dashed and dotted line) accounts for 'relative' efforts (i.e., uncorrelated with circumstances) when estimating the predicted fixed effect.

Figure 2.A.6: Upper bound IOp ratio with efforts (MLD - part 2)




Note: See note in Figure 2.A.5.

## 2.B Dealing with a 'short T': Simulating the fixed effect distribution

I propose a simulation to explore the importance of the length of the time period to estimate a fixed regression, and more importantly, to recover its predicted value. The sample size $N$ can be 350, 1000, 2000 or 5000, and represents the heterogeneity of sample sizes by country in the EU-SILC. I also study three time frames: 3 years, as I use in this paper, 7 years, used (on average) in Niehues and Peichl (2014) and 15 years, a longer period where the time dimension becomes less of an issue.

I simulate $N$ observations for 100 time periods using equation 2.23.

$$y_{it} = \alpha + \sum_{j=1}^{4} \beta_j x_{it}^{j} + \mu_i^C + \mu_i^E + \varepsilon_{it}. \tag{2.23}$$

Where the parameters and variables are defined as follows:

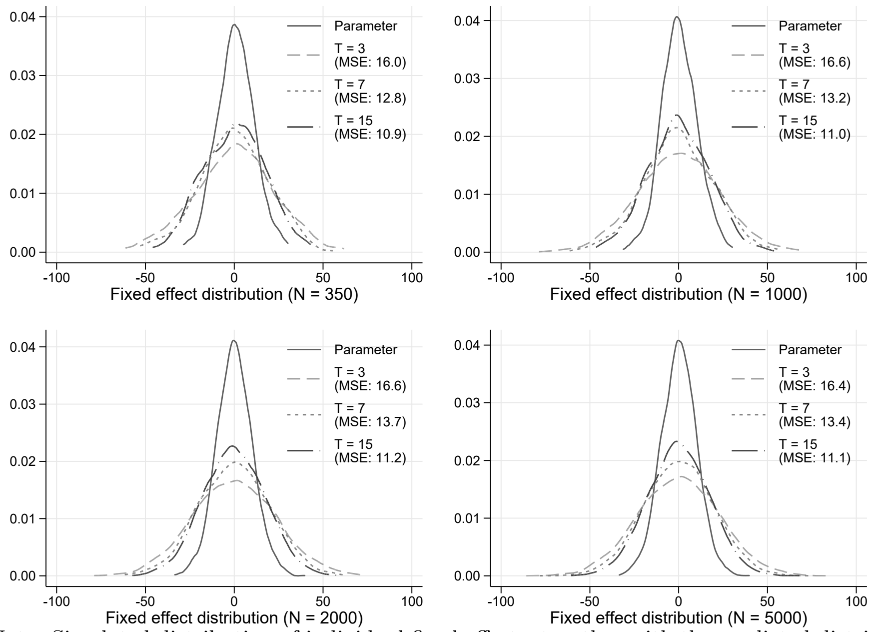
$$\begin{aligned} \alpha &= 5,\ \beta_j = 10 \\ x_t^j &= 5 + 0.5 \cdot x_{t-1}^j + \eta_t \\ x_0^j &\sim \mathcal{N}(0,1) \end{aligned}$$

For all $j = 1, \ldots, 4$ and with $\varepsilon_t \sim \mathcal{N}(0,4)$, and $\eta_t \sim \mathcal{N}(0,1)$. $\mu^C, \mu^E \sim \mathcal{N}(0,100)$ represent the circumstance component and the time invariant component of effort, respectively.

For each simulation of $N$ observations I use the periods $99-T$ to 99[18] and estimate a fixed effect regression only including year fixed effects. In total, this results in 12 scenarios: A sample size of 350, 1000, 2000, and 5000 respondents, where the fixed effect is estimated using 3, 7, and 15 years of data. Figure 2.B.1 plots these distributions, together with the 'original' distribution and the corresponding Mean Square Error (MSE).

[18] For example, if $T = 3$, I estimate the fixed effect for $t = 97, 98, 99$.

Figure 2.B.1: Simulated distribution of the predicted fixed effects




Note: Simulated distribution of individual fixed effects, together with the predicted distribution estimated including $N$ individuals across $T$ years.

Following the approach described in the paper I estimate IOp using year 100. I use the predicted fixed effect (or the actual fixed effect) as the only covariate and use the predicted value of the regression as my counterfactual distribution. I measure inequality using the MLD index for both the counterfactual distribution as well as the observed distribution of $y$. I present the ratio between the two in Table 2.B.1.

Table 2.B.1: IOp ratio when using each predicted fixed effect

| | Sample size | | | |
|---|---|---|---|---|
| | 350 | 1,000 | 2,000 | 5,000 |
| Estimation (T= 3) | 36.7% | 35.0% | 31.9% | 34.0% |
| Estimation (T= 5) | 29.6% | 27.7% | 28.4% | 28.6% |
| Estimation (T= 15) | 30.8% | 25.1% | 26.6% | 27.1% |
| Parameter | 17.7% | 15.9% | 14.7% | 15.4% |

Note: IOp ratio using the MLD index. Estimates use the predicted fixed effect as the circumstance vector. The last row presents the estimates when using the ‘real’ value for the fixed effect.

# Chapter 3

# Inequality of Outcomes, Inequality of Opportunity, and Economic Growth

## 3.1 Introduction

The question of the effect of inequality on growth can be traced to the seminal work of Galor and Zeira (1993) and Galor and Tsiddon (1997). In this line of work, this relationship is mediated by human capital accumulation which is hampered by imperfections in the credit market. In the presence of credit market imperfections, only those with access to wealth can invest in human capital. Higher human capital investments are accompanied by higher intergenerational mobility, expansion of high-skilled sectors, and ultimately, economic growth. Underlying these models is the idea that inequality constraints the access to opportunities, particularly access to education, restricting access to productive positions in which people can contribute to economic growth.

The notion that unequal opportunities constrain growth has been picked up in recent years, specially following the work on inequality of opportunity measurement Roemer (1998); Fleurbaey (2008). When looking at its effects on growth, inequality can be thought of as cholesterol: some inequalities might harm growth, while others might promote it.[1] Following Marrero and Rodríguez (2013), harmful inequality is embodied by inequality of opportunity, which reduces growth by limiting opportunities due to involuntarily inherited factors. On the other hand, beneficial inequality captures the role of autonomous choices and effort. In light of this distinction, the ambiguity of the effects of inequality on growth can be explained by the different roles that inequality of opportunity and inequality of efforts might play.

Inequality of opportunity captures the differences in life outcomes in relation to factors we cannot control, for example the place we were born, the time our parents spent with us when we were children and our gender, among others. Roemer (1998) coins the term 'circumstances' to refer to these involuntarily inherited factors. Inequality of opportunity differs from inequality of effort, which represents differences in outcomes related to autonomous choices that are not influenced by circumstances. Inequality of opportunity has a negative effect on growth because inequality in life outcomes is driven by circumstances rather than effort.

When studying the relationship between inequality and growth, most studies focus on medium to long term economic growth, sometimes referred to as 'secular trends'. Among the papers reviewed in Voitchovsky (2009), all focus on long growth spells of 5, 10 or more years. Indeed, researchers looking at the relationship between inequality and opportunity and growth (Marrero and Rodríguez (2013); Ferreira et al. (2018); Marrero and Rodríguez (2019); Aiyar and Ebeke (2019), to name a few) have also looked at medium to long growth spells. While these papers are concerned with 'structural' determinants of growth, such as human capital or aggregate productivity, in this paper I focus on short-term growth and the role that inequality of opportunity plays in attenuating or accentuating these fluctuations.

[1] See the feature story "*Inequality of Opportunity: New Measurements Reveal the Consequences of Unequal Life Chances*" on the World Bank website (March 28th, 2019).

I focus on a set of European countries in particular period of time. I study the 2005 to 2011 period, that includes both the Great Recession (2007-2009) and the European debt crisis (starting in late 2009). Jenkins et al. (2012) argue that the Great Recession is more of a 'structural break' rather than a standard business cycle shock, thus making it hard to extrapolate conclusions beyond this specific context. However, that does not mean that we should not pay attention to this period. This period of time – for this set of countries – provides a insightful look into how inequality of opportunity interacts with growth in the context of financial crises in high income economies.

In this chapter I estimate the effect of inequality of opportunity on the annual growth rate of GNI per capita and contrast these estimates with the equivalent effect of inequality of outcomes. The estimates of IOp and inequality of outcomes are based on household equivalised data for 27 European countries between 2005 and 2011 derived in the previous chapter. The present inequality of opportunity estimates address the 'lower bound' problem of most estimates, where inequality of opportunity only accounts the influence of observed circumstances. My estimates follow the method discussed in the previous chapter, where I use panel data to capture a summary measure of all time-invariant circumstances, thus providing 'upper bound' estimates of inequality of opportunity.

Using System GMM regression I find that an increase in inequality of outcomes reduces economic growth, albeit with a small and non-robust effect. An increase in inequality of opportunity also reduces growth, but this effect is robust to multiple functional forms, estimation approaches, and control variables. A decrease of one standard deviation in inequality of opportunity increases growth between 1.2 and 3.1 percentage points. Compared to previous studies, these effects are substantially larger, which could be explained by the use of 'upper bound' estimates in contrast to 'lower bound' estimates or by the focus on short-term economic growth. Future research will require to better disentangle the impact of each of these departures.

This chapter contributes to the literature of inequality of opportunity and growth in two ways. First, by using upper bound estimates of IOp, which provide a more exhaustive measure of the influence of circumstances. Second, by focusing on short-term dynamics in the context of financial crises. In this context, increases in inequality of opportunity result in decreases in economic growth. However, increases in inequality of 'efforts' also report a negative relationship with economic growth, albeit not statistically significant in most cases.

This chapter is consistent with previous research in that inequality of opportunity drives most of the overall effect of inequality of outcomes. However, my estimates differ from previous studies where increases in inequality of efforts promote (or do not influence) long growth spells (Marrero and Rodríguez, 2013, 2019). In the short term, both inequality of opportunities and inequality of effort are negatively correlated with economic growth, although the bulk of the overall effect is driven by the former. I draw insights from the literature on inequality and macroeconomic instability to discuss these results (van Treeck (2013), among others).

## 3.2 Measuring IOp

Suppose the outcome of an individual $i$ is represented by $Y_i$. Inequality of outcomes is the inequality of $Y$, summarised by an inequality index, in this case the Mean Log Deviation (MLD ). In contrast, IOp refers to the inequality of $Y_i$ related to factors over which we have no control, called circumstances. The standard model of IOp focuses on the role played by circumstances $C_i$ and efforts $E_i$, plus an unobserved random term $u_i$, in determining $Y_i$. In this context, efforts are partly determined by circumstances.

$$Y_i = f(C_i, E_i(C_i), u_i). \tag{3.1}$$

Typically, we use a reduced form of equation 3.1, represented as $Y_i = \phi(C_i, u_i)$, which accounts for both the direct effect of $C_i$, and the indirect effect through $E_i(C_i)$. This equation is traditionally estimated as a linear function of the log of $Y_i$, which is known as the parametric approach to estimating IOp, shown in

Bourguignon et al. (2007) and Ferreira and Gignoux (2011).

$$\log(Y_i) = \beta C_i + u_i. \tag{3.2}$$

I follow standard practice and use the estimates of 3.2 to construct a counterfactual distribution where only differences in $C$ explain differences in the outcome.[2]

$$\hat{\mu}_i = \exp(\hat{\beta} C_i). \tag{3.3}$$

The counterfactual distribution of $\hat{\mu}_i$ captures inequalities that are explained by differences in the circumstance vector $C_i$. The estimate of IOp for a given inequality index $I$ is the inequality of the counterfactual distribution, $I^O = I(\{\hat{\mu}_i\})$.

In order to estimate the effect of IOp on economic growth I follow Ferreira et al. (2018) by decomposing total inequality into inequality of opportunity, and a residual term usually referred to as inequality of 'efforts'. If $I_{jt}$ represents total inequality, then inequality of effort is defined as the residual $I^R_{j,t} = I_{jt} - I^O_{j,t}$. $I^O_{j,t}$ represents IOp, the between group component, while the interpretation of the residual $I^R_{j,t}$ depends on whether the inequality index is additively decomposable.

I use the Gini index to measure inequality, which is not additively decomposable. However, it less susceptible to extreme values at the top than indexes such as the Theil or the MLD (Cowell and Victoria-Feser, 1996; Cowell and Flachaire, 2007), having a simpler and well-established interpretation. As a result, the residual $I^R_{j,t}$ cannot be interpreted as solely within-type inequality because it also includes a term quantifying the overlap between types (Lambert and Aronson, 1993).

### 3.2.1 Upper bound estimates of IOp

Upper bound estimates of IOp capture the influence of time invariant determinants of income. They capture circumstances typically included in surveys such as

[2] I estimate equation 3.2 using Poisson regressions to avoid the need for 'smearing' or adjusting for the $\hat{\mu}_i$ when going from the predicted log of income, to predicted income (Duan, 1983).

parental education or occupation, place of birth and gender, but also other important circumstances that are not typically included such as parental interactions or innate abilities. However, they also capture time invariant efforts such as personal 'attitudes' (e.g., being hard-working or punctual). The formal derivation of these estimates is detailed in the Appendix.[3]

Just like with any IOp estimate, inequality of effort is constructed the difference between inequality of outcomes and IOp and it is therefore a residual term. In this case, given that the upper bound estimate includes all time invariant efforts, inequality of effort is a measure of the influence of factors that vary over time. Given that some efforts might be time invariant, this measure of inequality of efforts can be be interpreted as 'lower bound' estimates of effort.

Given that EU-SILC dataset includes circumstance variables only for 2005 and 2011 (and soon, for its 2019 version), lower bound estimates have only been estimated for those two years (see, e.g., Yalonetzky (2010)). Because upper bound estimates are based on panel data rather than on the availability of circumstance variables, this approach allows the estimation of IO for many more years. The use several years of data allows for the use of modern estimation techniques on comparable cross country data, such as System GMM.

## 3.3 The effect of inequality and IOp on growth

### 3.3.1 Understanding the relationship between IOp and growth

The idea behind decomposing inequality of outcomes and looking into the effect of IOp and inequality of efforts is that the former is not morally illegitimate but also inefficient. Higher IOp means that 'privilege' – for example, having highly

[3]It could be argued that there are time-invariant circumstances, for example having a new roommate or colleague, as they still lie outside of individual control. However, here – as in most IOp exercises – I focus on childhood circumstances, what Dworkin (1981a,b) calls 'initial brute luck', in contrast to 'later brute luck'.

educated parents – shape the distribution of rewards (concretely, household income) while providing no contribution to overall economic growth. This argument provides an instrumental rather than an intrinsic motivation to reduce IOp. The following section discusses the empirical approaches to study this relationship, and the expected effects.

This is the first paper to study this relationship using upper bound estimates of IOp. Contrary to lower bound estimates of IOp that suffer from omitted variables issues, upper bound estimates err on the side of including too many determinants of income, some of which can be construed as efforts. The overall effect on economic growth thus depends on the what is captured by the upper bound estimate of IOp. Based on the 'cholesterol hypothesis', if the upper bound estimates captures new circumstances then we would expect a larger effect than with a lower bound estimate, on the other hand, if the upper bound captures mostly efforts, then we would expect a smaller effect.

Beyond the measure of IOp, there are two important departures from previous studies. First, I focus on annual growth rates rather than medium-term (i.e., 5 or more years) growth spells, studying the impact of inequality on short term responses rather than in more structural or 'secular' trends. Short term variations in inequality (both of outcomes and opportunity) mostly reflect changes in labour income rather than in capital income or wealth.[4] As such, Royuela et al. (2018) suggest that higher inequality can have a substantial short term effects on household spending, particularly among those with liquidity constrains, reducing aggregate consumption and thus economic growth.

While the previous discussion is about inequality of outcomes, these effects might be stronger when driven by IOp. For example, if liquidity-constraint households are also those suffering from poor circumstances. Galor and Tsiddon (1997) propose a model of economic growth in periods of technological inventions. In the short term, a new technology will benefit (the few) who can use it, likely to be those

[4]There might be changes in capital income, but the probability of receiving capital income is unlikely to change. Moreover, labour income comprises around 80% of total income, having a bigger income on household inequality (Jenkins et al., 2012).

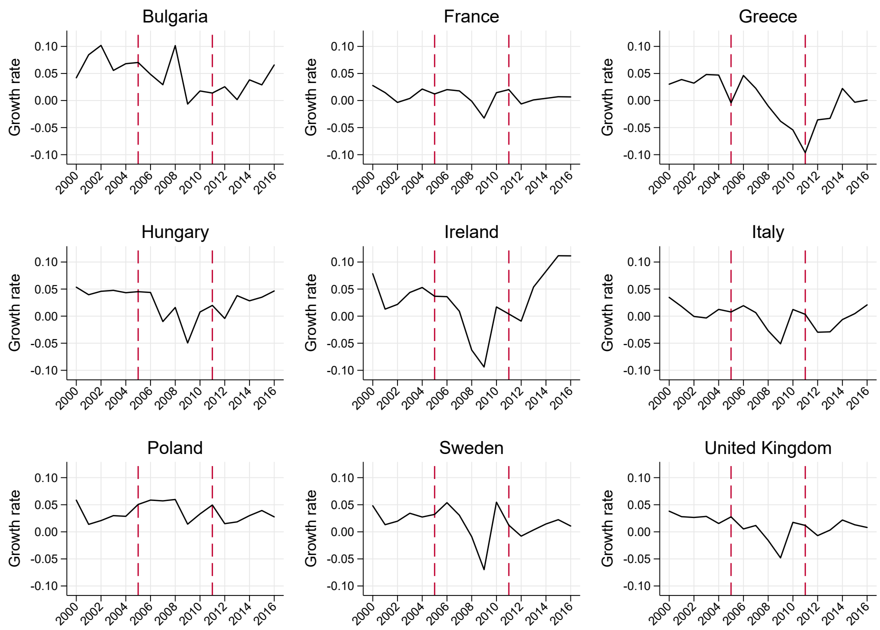
that grew up with better circumstances. This increases the skill-premium gap and wage inequality, while reducing growth as the rest of the work forces 'catches-up' in the form of on-the-job-training. That association might dissipate (or even revert) in the medium to long term, depending on how the use of the new technology evolves. To the extent that IOp represents these mechanisms such as differences in skills or credit constraints, it should have a stronger effect on economic growth than inequality of outcomes.

The second departure relates to the specific time period I study. I study the 2005 to 2011 period, that includes both the Great Recession (2007-2009) and the European debt crisis (2009 onwards). This is a very specific context, as shown for nine countries in Figure 3.1. There are large drops in GNI per capita growth around 2009 and 2011, with very different trends to both before and after that period. Indeed, when refering to this period, Jenkins et al. (2012) talk about "structural changes" rather than short term volatility or business-cycle fluctuations. This particular context might have changed the (short-term) relationship between inequality and growth.

Recent studies have shown a negative association between inequality of outcomes and growth in the context of the Great Recession. Royuela et al. (2018) finds a negative association between inequalities and growth for OECD countries in the 2003-2013 period, particularly in urban regions. Similarly, Lewin et al. (2017) finds that US counties with higher income inequality entered the recession earlier than those with lower inequality. Cynamon and Fazzari (2015) argue that this negative association in the US is due to increased borrowing constraints among the bottom 95% of the income distribution, reducing aggregate demand. The Great Recession appears to have created a particular context where higher economic inequality (i.e., inequality of outcomes) reduces economic growth.

Income inequality has also been suggested as a cause of the Great Recession. In his review, van Treeck (2013) discusses the effect of inequality on economic growth in the context of the Great Recession. He argues that this relationship can be explained by the 'relative' income hypothesis (in contrast to the 'permanent' income

Figure 3.1: Annual GNI per capita growth rates




Note: GNI per capita growth rates for nine countries in the sample, between the years 2000 and 2016. The red vertical dashed lines cover the period of study of this chapter (2005-2011). GNI per capita data from World Bank.

hypothesis). Under this hypothesis, households react to their relative position in the income distribution. Higher inequality increases the consumption gap between the top income earners and the rest of the distribution, who attempt to 'catch up' to the former through higher debt. Along the same line, Wisman (2013) discusses three ways in which inequality made the economy vulnerable to systemic shocks, two of which relate to constrains in consumption that triggered higher debt (the third being wealth concentration and its impact on politics). These arguments highlight how inequality, independent whether it arises from differences in efforts or in circumstances, has created a context of low consumption, high debt, and ultimately, lower economic growth.

### 3.3.2 Estimating the effect of inequality on growth

Empirical techniques to study the effect of inequality on growth have developed tremendously over recent years. The first papers to study the relationship between inequality and growth used OLS (or 2SLS) applied to cross-sectional data. For example, Alesina and Rodrik (1994) study several countries and explain how an increase in inequality reduces growth with reference to tax: higher inequality increases demands for redistribution, which in turn reduces growth. The estimates of Deininger and Squire (1998) show that an increase in land inequality results in a decrease in the growth rate, highlighting the importance of productive investments to promote both less inequality and higher growth. Other papers have used panel data and fixed effect regressions to control for time-invariant unobserved factors. Both Li and Zou (1998) and Forbes (2000) find that an increase in inequality results in an increase in growth rates. Overall, this line of research is far from resolved.

One of the explanations for the diversity of estimates is the presence of other sources of bias in the estimation, even when using a country-year. Particularly relevant in the context of growth regressions using panel data is dynamic panel bias, otherwise known as 'Nickell bias' (Nickell, 1981). Nickell bias arises because the lagged dependent variable is correlated with the error term, as the lagged regressor includes observations for all previous periods, which include past errors. Nickell bias is not eliminated by increasing $N$ (in this case, the number of countries), which is why it becomes a large problem under 'small $T$, large $N$' settings. When $T$ is small, as is the case in this chapter, Nickell bias can be an important source of distortion (Cameron and Trivedi, 2005, pp.763-5). I address this problem by estimating growth regressions using System GMM (Arellano and Bover, 1995; Blundell and Bond, 1998).

System GMM uses both equations in levels and in first differences, using lagged first-differences as instrument variables in the former case and lags of the dependent variables in levels in the latter. System GMM estimates the dynamic panel mode by creating a system of equations – levels and first differences – with relevant instrumental variables for each case. These instruments satisfy the exclusion restriction, that is, they are not correlated with the error term (as they precede the error), while being correlated with the endogenous variable (in this case, the lagged dependent variable). System GMM has become the most commonly used method for estimating regressions under panel data, particularly when looking at the effect of inequality.[5]

System GMM is prone to instrument proliferation, a problem described in detail in Roodman (2009a). Because these methods use lags of each variable as instrumental variables for each endogenous variable, the number of instruments potentially grows quadratically with each additional year of data. A large number of instruments may result in overfitting problems, as well as a weaker test of overidentifying restrictions. This problem is particularly acute when the number of observations (e.g., countries) is small. Using 2SLS as an analogy, if the first stage regression is overfitted due to a large number of instruments, then its $R^2$ is close to 1 and the predicted value of the endogenous variable is close to its original value (i.e., $\hat{X}_i = X_i$). If that is the case, then the second stage estimates are equal to the

[5]The need for lagged observations as instruments can result in a trade-off between sample length and lag length, particularly when one needs lagged differences. However, this is not a problem for 'GMM-style' instruments, as missing observations (i.e., the unavaiable lags) are substituted for zeros in the final instrument matrix. Roodman (2009b, pp.107–108) discusses the process in detail.

biased OLS estimates. If all possible instruments are included, our estimations using System GMM will provide no additional more information compared to a standard OLS estimate.

As a rule of thumb, Roodman (2009a) suggests using at most as many instruments as there are countries in the data. He proposes using several techniques to satisfy this rule. One is to simply cap the number of lags. Another approach is to 'collapse' the instrument matrix, in other words, to go from having one first stage regression for the instrumental variable to having fewer regressions that include several instruments at the same time (see equation 11 in Roodman (2009a)). Another option is to only use the first differences of each variable as instruments, or to only use the variables in levels. However, these approaches limit the instrument count in arbitrary ways because they do not take into account the information that each instrument can provide, potentially leaving out relevant information. A fourth alternative is to use Principal Component Analysis to group instruments while aiming to minimise the loss of information conveyed in them (Bontempi and Mammi, 2015). All of these approaches limit the number of instruments but, in this chapter, I focus on the PCA method for the previously described reasons. By reducing the number of instruments using PCA I can estimate the model with 27 less instruments, while preserving a larger part of the informational content of the original instrument matrix.[6]

#### Growth regressions and dynamic panel data models

My growth regression specification follows from previous estimates for the effect of IOp and growth, such as Ferreira et al. (2018); Marrero and Rodríguez (2019). Particularly, it follows the latter in not including additional control variables beyond the GNI per capita level. The intuition being that the coefficient for IOp (and equivalently, the coefficient for inequality of effort) is capturing its direct and indirect effect. Where, as Barro (2000) puts it, the direct effect represents the

[6]Despite choosing one specific approach to reduce the number of instruments, I provide robustness checks for alternative approaches in section 3.5.2.

effect of inequality beyond its effects on potential covariates such as education or investment. The specifications are shown in equation 3.4 and 3.5.

$$g_{j,\{t-1,t\}} = \beta_0 \log(y_{j,t}) + \beta_1 I_{j,t} + \alpha_j + \eta_t + u_{jt}. \quad (3.4)$$

$$g_{j,\{t-1,t\}} = \gamma_0 \log(y_{j,t}) + \gamma_1 I^O_{j,t} + \gamma_2 I^R_{j,t} + \alpha_j + \eta_t + u_{jt}. \quad (3.5)$$

I define growth ($g_{j,\{t-1,t\}}$) as the annual growth rates of GNI per capita from year $t-1$ to year $t$, where $t = 2005, \ldots, 2011$. GNI per capita (from World Bank Open Data) is measured in 2010 USD. I focus on GNI per capita rather than GDP per capita because it focuses on people living in the territory by including includes factor income earned by foreign residents, which also feature on my inequality estimates (For a detailed discussion on the relationship between household income in surveys, and GDP/GNI see Nolan (2020)).

Equation 3.4 studies the effect of inequality of outcomes ($I_{j,t}$), whereas equation 3.5 decomposes it into an upper bound estimate of IOp ($I^O_{j,t}$) and the residual, interpreted as inequality of efforts ($I^R_{j,t}$). I also include the log of GNI per capita for country $j$ in year $t$ (in constant 2010 USD), a country-level fixed effect ($\alpha_j$), a time fixed-effect ($\eta_t$), and the residual ($u_{jt}$).

This specification follows Marrero and Rodríguez (2019) in that I do not include other covariates. This is done to capture the direct and indirect effect of inequality. That is, the coefficient for inequality of opportunity $\gamma_1$ will account for all 'channels' or 'paths' through which it can influence economic growth, either directly or through its influence on other covariates. In the robustness section I use two specifications that allow for covariates, one based on the model proposed by Forbes (2000) and later used in Ferreira et al. (2018), and another to account for determinants of growth in the context of the Great Recession.

## 3.4 Data

### 3.4.1 Upper bound estimates of IOp

All data for the inequality estimates comes from the EU-SILC. I take IOp and inequality of outcomes estimates for 27 countries in the period 2005-2011 from the previous chapter and expand that sample to include an estimate for 2012. I do not include this additional year of data in the previous chapter as I focus on the period bounded by the two lower bound estimates (2005 and 2011). Given that all countries except for Ireland and the United Kingdom report income from the previous year, I lag their estimates to represent the 2004-2011 period.

IOp is measured over household equivalised income (using the OECD equivalence scale) for all individuals aged 25 to 55. Inequality is measured using the Gini index. When estimating IOp, sample sizes vary significantly between countries. On average, I use around 1,800 observations per country to estimate IOp, ranging from countries with 350 to 400 observations to countries with over 4,000 observations.

I do not use the lower bound estimates of inequality of opportunity describe in the previous section. These estimates include two data points per country (2005 and 2011) and System GMM needs at least three time periods in order to work, as it uses both levels and first differences. The fact that only two years of data are available when looking at lower bound estimates of IOp is the main reason why previous research on Europe has not used estimates from the EU-SILC.

My analysis includes an unbalanced panel of 27 countries. 20 countries have complete data for the period 2004-2011 (2005-2012 for Ireland and the UK) and 7 have missing data for particular years. Bulgaria, the Czech Republic, Lithuania, Malta and Slovenia enter the SILC survey in 2006 (reporting 2005 income), Romania enters in 2007, and Ireland does not have data for the 2007 and 2008 waves. I do not include Iceland as it does not have GNI per capita data in constant USD on the World Bank Open Dataset. For each country, I have between 6 and 8 years of data, with an average of 7.7 years per country.

My income of interest, the annual growth rate of GNI per capita (measured in constant 2010 US dollars), was downloaded from the World Bank Open Database. Table 3.A.1 reports descriptive statistics for this variable as well as for inequality of outcomes and IOp. The average growth rate was 1.6%, with high heterogeneity, mostly accounted for by within country differences, as one would expect from a period of two economic crises. Average inequality of outcomes (measured through the Gini) is 28.4 points and average IOp is 23.1 points. In both cases the largest share of the variance is explained by between country differences.

In addition to GNI and inequality, I include additional covariates in some robustness exercises. These variables come from different sources. The share of the population over 25 with at least completed upper secondary (ISCED 0 to 4), separate for men and women, comes from Eurostat. The price level of capital formation (in PPP) relative to the United States exchange rate, as well as the average annual hours worked by persons engaged (converted into weekly hours) were downloaded from the Penn World Table (version 9.1). The annual growth rate for per capita consumption (for households and NPISHs) and the domestic credit to private sector by banks (as a share of GDP) come from the World Bank Open Database. Table 3.A.2 report descriptive statistics for these variables.

## 3.5 Results

I start by showing the effect of both total inequality and IOp on growth. In the second part I look at some robustness checks related to the estimation approach, among which I include a few covariates that might shine a light on potential drivers of my main estimates. Together, my analysis contribute to a comprehensive examination of the effect of both inequality and IOp on short-term economic growth.

### 3.5.1 Main estimates: The effects on economic growth of inequality and IOp

The main estimates are shown in table 3.1. All columns use the same specification, with columns 1 to 4 using for inequality of outcomes as their dependant variable, and columns 5 to 8 using IOp. All estimates use System GMM with Windmeijer-corrected cluster–robust errors (i.e., two-step corrected standard errors) clustered at the country level.

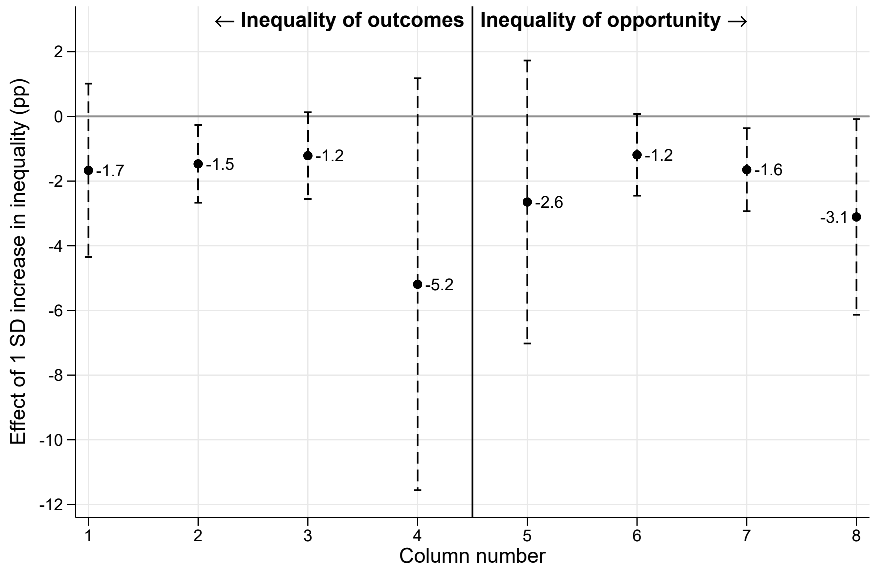
I use the same specifications for inequality of outcomes and for IOp. That is, columns 1 and 5 use the same estimation approach, and the same holds for columns 2 and 6, 3 and 7, and 4 and 8. The first column provides a System GMM estimation without any major restrictions. It includes all available lags as well as the complete 'GMM-style' instrument matrix. As result, the number of instruments is quite large (82 for inequality of outcomes and 117 for IOp, as I also instrument the residual inequality term). The second column caps the number of lags at 1, thus using only the first available lag for each of the instrumented variables. The third column collapses the instrument matrix, in contrast with the 'uncollapsed' matrix that uses one column for each instrument (see page 108 in Roodman (2009b)). Finally, the fourth column uses PCA to reduce the instrument matrix based to a few principal components, based on their correlation. Figure 3.2 summarises the coefficient for inequality of outcomes or IOp in terms of a one-standard deviation change in the particular inequality measure.

Table 3.1: Effect of inequality on GNI per capita growth rate (System GMM)

| VARIABLES | (1) Ineq | (2) Ineq | (3) Ineq | (4) Ineq | (5) IOp | (6) IOp | (7) IOp | (8) IOp |
|---|---|---|---|---|---|---|---|---|
| Inequality | -0.370 | -0.326** | -0.269* | -1.152 | | | | |
| | (0.304) | (0.136) | (0.152) | (0.722) | | | | |
| IOp | | | | | -0.613 | -0.275* | -0.382** | -0.720** |
| | | | | | (0.517) | (0.149) | (0.152) | (0.357) |
| IR | | | | | -0.883 | -0.144 | -0.280** | -0.406 |
| | | | | | (0.727) | (0.231) | (0.124) | (0.406) |
| Log GNI | -0.040*** | -0.038*** | -0.031* | -0.057* | -0.035** | -0.024 | -0.034* | -0.041** |
| | (0.013) | (0.012) | (0.018) | (0.030) | (0.016) | (0.017) | (0.019) | (0.019) |
| Constant | 0.521** | 0.490*** | 0.407** | 0.946* | 0.545* | 0.318* | 0.463** | 0.631** |
| | (0.214) | (0.149) | (0.187) | (0.511) | (0.306) | (0.178) | (0.180) | (0.284) |
| Observations | 207 | 207 | 207 | 207 | 207 | 207 | 207 | 207 |
| Number of countries | 27 | 27 | 27 | 27 | 27 | 27 | 27 | 27 |
| Instruments | 82 | 40 | 25 | 25 | 117 | 54 | 33 | 28 |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| All lags | Yes | No | No | Yes | Yes | No | Yes | Yes |
| PCA | No | No | No | Yes | No | No | No | Yes |
| Collapsed instrument | No | No | Yes | No | No | No | Yes | No |
| Sargan Test | 0.000 | 0.047 | 0.722 | 0.145 | 0.008 | 0.035 | 0.926 | 0.341 |
| Hansen Test | 1.000 | 0.967 | 0.577 | 0.162 | 1.000 | 1.000 | 0.959 | 0.343 |
| AR(1) Test | 0.172 | 0.176 | 0.167 | 0.110 | 0.125 | 0.170 | 0.176 | 0.166 |
| AR(2) Test | 0.236 | 0.245 | 0.240 | 0.321 | 0.279 | 0.237 | 0.246 | 0.284 |
| KMO measure | | | | 0.900 | | | | 0.891 |

Note: *** p<0.01, ** p<0.05, * p<0.1. Windmeijer-corrected standard errors, clustered at the country level. The dependent variable is the annual growth rate of GNI per capita (in constant 2010 US dollars). All estimations include 27 European countries for the years 2004 to 2011 (2005-2012 for the UK and Ireland). The main independent variable for columns 1 to 4 is the level of income inequality, and the upper bound estimate of inequality of opportunity (IOp) for columns 5 to 8. Both using the Gini index. System GMM use the inequality estimates and log GNI per capita as 'GMM style' instruments (making use of multiple lags). The years fixed effects are included as regular 'IV style' instruments. Columns differ in the number of lags. For both inequality of outcomes and IOp I include, respectively: all lags, only the first lag, a collapsed instrument matrix, a reduced instrument matrix based on PCA. The Sargan and Hansen statistics are tests of overidentifying restrictions, the null being the joint validity of all instruments. The AR(1) and AR(2) statistics are tests of autocorrelation of order 1 and 2, the null being no autocorrelation of the residuals. The KMO measure is the Kaiser-Meyer-Olkin test for sampling adequacy when using PCA. As a rule of thumb, a KMO measure below 0.5 is unacceptable and above 0.8 is desirable.

Figure 3.2: Effect of a 1sd increase in inequality on growth (pp)




Note: Graph includes all coefficients for IOp obtained from table 3.1, multiplied by the standard deviation of the corresponding inequality index. Coefficients are in percentage points of the annual growth rate of GNI per capita. 95% confidence intervals.

### Inequality and economic growth

The coefficient for inequality of outcomes on growth rates ranges from -0.37 to 0.03 percentage points. The collapsed instrument matrix (column 3) and the PCA approach (column 4) reduces the number of instruments below the rule of thumb – fewer instruments than countries, 27 in this case. However, in the PCA case the coefficient is not statistically significant. The Arellano-Bond tests for autocorrelation and the Hansen and Sargan tests of overidentifying restrictions suggest no issues in any of the specifications. In sum, column 3 provides a reasonable estimate for the effect of inequality of outcomes on economic growth, that satisfy the required number of instruments as well as not rejecting the statistical tests.

The coefficient for inequality of outcomes on economic growth in column 3 is -0.27, and it is statistically significant at the 90% confidence level. We can also interpret this coefficient in terms of a change in one standard deviation. The standard deviation for inequality of outcomes is 4.5 points of the Gini (as shown in Table 3.A.1). A one standard deviation is equivalent to moving from a country with low inequality such as Norway or Denmark, to a country in the middle of the ranking such as France or Austria. From Figure 3.2 we see than an increase of a one standard deviation in inequality of outcomes is associated with a decrease in 1.2 percentage points of the growth rate.

To compare these estimates with previous papers, I look at the effect in terms of changes of one standard deviation of inequality of outcomes. Using a set of income and expenditure surveys and the MLD index to measure inequality, Ferreira et al. (2018) report an effect of -1.8 percentage points.[7] Using the same set of surveys as Ferreira et al. (2018) and measuring inequality using the Gini index, Marrero and Rodríguez (2019) report a coefficient of -0.7 percentage points.[8] My estimate lies between the two, however, both estimates are statistically non-significant. My results hint at a potential short-term effect of inequality of outcomes on economic growth that does not hold when studying medium to long term dynamics.

#### IOp and economic growth

Columns 5 to 8 in table 3.1 show the estimates of the effect of IOp on growth. The coefficients range from -0.72 to -0.28. All point estimates are negative, consistent with an increase in IOp resulting in a decrease in the growth rate. The Arellano-Bond tests for autocorrelation and the Hansen and Sargan tests of overidentifying restrictions suggest no issues in any of the specifications. Going from column 5 to column 8 we see an important drop in the number of instruments, highlighted by the Hansen test of 1 at the first two columns. Despite the last column reducing the number of instruments to 28, it is still one above the rule of thumb proposed

[7] I compute the standard deviation for total inequality in Ferreira et al. (2018) using Table A.1. in the online Appendix. The relevant coefficient is in Table 1, column 5.

[8] Marrero and Rodríguez (2019) report their standard deviation in Table 1. The relevant coefficient is found in Table 3, column 9.

by Roodman (2009a).[9]

The coefficient for IOp on economic growth in column 8 (the PCA adjusted instrument matrix) is -0.72, and it is statistically significant at the 95% confidence level. The standard deviation for IOp is 0.043 (see Table 3.A.1). Similar to inequality of outcomes, this is equivalent to going from the bottom of the ranking (Norway or Denmark) to the middle (the Netherlands or Austria). As a result, a one standard deviation increase in IOp is equivalent to a decrease of 3.1 percentage points in the annual growth rate for GNI per capita. Taking only the statistically significant coefficients, from Figure 3.2 we see that the effect of a one standard deviation in IOp ranges between -1.2 and -3.1 percentage points.

My estimates have some overlap with previous studies. Given a one standard deviation increase in IOp, Ferreira et al. (2018), using the MLD index and system GMM, find a non-significant effect of -1.3 percentage points of the growth rate. Using the same dataset and the Gini index, Marrero and Rodríguez (2019) report a statistically significant effect of -2.5 percentage points. Both their estimates fall within the range of estimates provided in Table 3.1 but are below my preferred estimate for the effect of IOp, in column 8. While there appears to be some overlap between short and medium term effects (both in size and statistical significance), the former are somewhat larger than the latter.

Contrary to previous estimates, the coefficient for residual inequality ('inequality of efforts') is also negative. Based on columns 6 to 8 in Table 3.1, an increase in one standard deviation of the residual term (equal to 0.02 points of the Gini) results in a decrease of growth rates ranging from 0.3 to 0.9. This is not consistent with the 'cholesterol hypothesis', that suggests that there are two components underlying inequality of outcomes. These components have significant but opposite effects on economic growth, cancelling out each other and resulting in inequality of outcomes having an ambiguous effect. In the context of short-term growth and the Great Recession, higher inequality of outcomes, opportunity, or 'effort' results in lower

[9]For that reason I include among the robustness check a set of regressions using PCA and capping the number of instruments. Results do not vary substantially from those in column 8.

growth rates.[10]

Despite the coefficients for IOp and for residual inequality being negative, the former is larger in absolute value in columns 6 to 8. This is consistent with what Ramos and Van de gaer (2020) call the 'weak hypothesis' about the effects of opportunity and efforts on economic growth. That is, that the effect of IOp is more negative than the effect of effort inequality. My estimates show that, everything else constant, higher inequality of outcomes reduces growth, and that an increase in IOp is more detrimental to economic growth that an increase in inequality of effort.

### 3.5.2 Robustness Checks

In this section I report several robustness checks. The first two deal with the issue of instrument proliferation. First, I modify the number of instruments by capping the number of lags to be considered by the System GMM estimation. Second, I cap the number of instruments by forcing the PCA algorithm to select fewer principal components. The third check involves an instrumental variable approach that addresses potential issues of reverse causality. Fourth, I use alternative dynamic panel models estimators that also address Nickell bias. Finally, I study the effect of including additional covariates. Overall, these checks are consistent with the main results.

#### Different choice of instruments

Bazzi and Clemens (2013) discuss several issues frequent in growth regression. Regarding System GMM, the discuss the need to 'unpack' the black box that is the instrument matrix of internal instruments. For that reason, in my first

[10]It could be argued that this not because of the context but rather because of the use of an 'upper bound' estimate of IOp. However, the fact that inequality of outcomes as a whole has a negative effect suggests that this is not the case.

check U unbundle the system GMM instruments to allow for different number of instruments for the different covariates, as well as differences in the the level and difference equations.

I provide estimates for the effect of inequality of outcomes and for IOp. For the latter I also include the same regressions without instrumenting for the residual component of inequality. In each case, the first model includes in differences – for the level equation – the first lag of log GNI per capita and the first three lags of inequality (i.e., if inequality in $t$ is $I_t$, I include $I_{t-1} - I_{t-2}, \dots, I_{t-3} - I_{t-4}$ as instruments). In levels – for the difference equation – I include the second and third lags, both for log GNI per capita and inequality (i.e., if inequality in $t$ is $I_t$, I include $I_{t-2}$ and $I_{t-3}$ as instruments). These choices stem from the idea that these are the most important instruments to include: changes in inequality tend to be slower than changes in GNI per capita levels – for example – so additional lags need to be included. The first lag of inequality, on the other hand, is already included as it is already a part of the difference equation (which looks at $I_t - I_{t-1}$ as the outcome). The second set of estimates caps the number of instruments to two by only using the first two lags of inequality in the level equation. The third caps the number of instruments at one by only using the first lag of inequality in the level equation and the second lag for log GNI per capita and inequality in the difference equation. Cingano (2014) and Kraay (2015) follow similar approaches in their main estimations when studying the effect of total inequality, unbundling instruments and then capping the number of lags in each case.

Table 3.A.3 in the appendix presents the estimates when unpacking the GMM instrument matrix. The main conclusions do not change from those in Table 3.1. Inequality of outcomes has a negative coefficient and it is statistically significant in two of the three specifications. Both IOp and the residual inequality term have negative coefficients, albeit only the former is statistically significant (in 4 of the 6 specifications). The coefficient for IOp tends to be larger than the one for residual inequality. Excluding the residual inequality term as an instrument makes little difference in the coefficient for IOp. The point estimate for the effect of inequality of outcomes ranges from -0.75 to -.46, while the coefficient for IOp

ranges from -0.84 to -0.51. However, all specifications fail to satisfy the Sargan test of overidentifying restrictions which, together with the very large value for the Hansen test suggests the presence of too many instruments.

None of the specifications in Table 3.A.3 manage to reduce the number of instruments to be equal or below the number of countries (the rule of thumb in Roodman (2009b)). Which is why I repeat the estimation in column 8 of Table 3.1 by reducing the number of instruments using PCA, but also capping to the number of instruments to satisfy this rule of thumb. The estimates are reported in Table 3.A.4 and show that the main results do not change substantially when including 27, 26, or 25 instruments, if anything, the coefficients show a small decrease in absolute value when including fewer instruments.

### An IV approach to address reverse causality

System GMM, through its use of internal instruments, is not the only way to address the issue of endogeneity. An alternative approach is to use external instruments for inequality, i.e., factors that affect inequality but are independent of growth. Brueckner and Lederman (2018) propose constructing a synthetic measure of inequality, one that captures changes that are not due to changes in GNI per capita or its growth rate. This approach is designed to explicitly address reverse causality, as, by construction, growth will have no impact on the synthetic measure of inequality, thus 'shutting' the causal channel going from income to inequality. This approach has also been used by Marrero and Rodríguez (2019), who use it to look at IOp and its effect on growth.

The first step in this IV approach is to estimate the effect of income on inequality.

$$I_{j,t} = \alpha_j^1 + \delta_t^1 + \beta^1 \log(y_{j,t}) + \varepsilon_{it}^1 \tag{3.6}$$
$$I_{j,t}^O = \alpha_j^2 + \delta_t^2 + \beta^2 \log(y_{j,t}) + \varepsilon_{it}^2 \tag{3.7}$$

where $I_{j,t}$ is total inequality for country $j$ in year $t$, and $I_{j,t}^O$ is the equivalent for

IOp. $\log(y_{j,t})$ is the log of GNI per capita. The coefficients $\beta^1$ and $\beta^2$ are estimated using OLS, which are then used to construct the external instrument $Z$.[11]

$$Z_{j,t} = I_{j,t} - \hat{\beta}^1 \log(y_{j,t}) \tag{3.8}$$

$$Z^O_{j,t} = I^O_{j,t} - \hat{\beta}^2 \log(y_{j,t}) \tag{3.9}$$

The instruments $Z_{j,t}$ and $Z^O_{j,t}$ capture the variation in inequality that is not explained by the variation in log GNI per capita. Table 3.A.5 shows the estimations for this process in columns 2 and 4. Columns 1 and 3, on the other hand, include a 'naive; OLS estimation that does not address the double causality problem in columns 1 and 3.

The IV approach is consistent with my previous estimates. An increase in either inequality or IOp results in a decrease in growth rates – albeit a larger one for IOp. If I do not account for reverse causality (columns 1 and 3), the point estimate is not statistically significant and 3 to 5 times smaller than the corresponding estimate in column 2 or 4. These IV estimates show that not addressing the causal effect of income on inequality underestimates the effect of IOp on growth.

**Different dynamic panel estimation methods**

My third robustness check employs two alternative approaches to estimate dynamic panel models: Quasi–Maximum Likelihood (QML) and Bootstrap-Based Bias Correction with Fixed Effects (BCFE). Just like System GMM, these approaches address dynamic panel bias and are particularly useful when the time dimension is small.

QML (Kripfganz, 2016) is a special case of structural equation modelling. It fits a fixed effect model that accounts for Nickell bias without using instrumental

[11] To get a consistent estimate of $\beta$, equations 3.6 and 3.7 are estimated using 2SLS. Following Marrero and Rodríguez (2019) I use the first two lags ($t-1$ and $t-2$) of gross savings as a percentage of GDP and the second lag of GNI per capita growth rate ($t-2$) as instruments. Data from the World Bank.

variables by specifying the joint distribution of the outcome variable (both in levels and first differences) and the distribution of the error terms. BCFE (De Vos et al., 2015) addresses this bias using a two-step process: it first obtains a biased estimator, and then removes the bias using a bootstrap procedure. Unlike System GMM, that simply omits missing instruments, these approaches drop one year of observations when using first differences as instruments. Therefore, these estimates not directly comparable with those reported earlier. This is particularly true for QML, as countries with interior gaps such as Ireland are dropped altogether.

The estimates from QML and BCFE show that an increase in either total inequality or IOp results in a statistically significant decrease in growth rates. One potential explanation for both estimates being significant could lie in their standard errors. Unlike the previous estimates, the software for these methods does not allow for the calculation of robust standard errors (e.g., with countries as clusters). QML uses the Huber–White estimator for heteroscedasticity-consistent standard errors, while BCFE uses standard errors that follow from the bootstrap distribution of the point estimate under a t-distribution.

What stands out more than both coefficients being significant is that the point estimates for total inequality and IOp are very similar. Both approaches show negative coefficients for all three inequality terms (Inequality of outcomes, IOp and the residual term), with the coefficient for the residual term being higher than the one for IOp. BCFE estimates are not statistically significant (with the exception of the residual term, significant at the 90% level). While less robust than the main estimates, both the QML and BCFE approaches show negative coefficients for the effect of IOp on growth.

### 3.5.3 Including covariates

To better understand the relationship between IOp and growth, I complement the main estimates by including two set of covariates. The first one follows from Forbes (2000) and Ferreira et al. (2018), and considers medium to long-term determinants of growth, namely measures of human capital and market distortions. The second set of covariates includes short-term determinants of growth in the context of the Great Recession (van Treeck, 2013; Wisman, 2013). These covariates include consumption growth rates, worked hours, and the level of private debt. These variables are described in Table 3.A.2 in the Appendix.

Table 3.A.7 reports the estimations for inequality of outcomes (columns 1 and 3) and for IOp and the residual term (columns 2 and 4). All estimates use PCA to reduce the number of instruments. Columns 3 and 4 report fewer observations because the consumption growth rate does not have data for Malta and there is no private debt data for a few years of Lithuania, Latvia, and Slovakia. As a result, the second set of covariates estimates the effect of inequality on growth for 26 countries with 187 observations.[12]

Estimates show very little change when accounting for these covariates. When following the specification in Forbes (2000), the coefficients for inequality remain negative with a small decline in absolute value. The coefficient for inequality of outcomes becomes statistically significant at the 90% and the coefficient for IOp remains statistically significant at the 95% level. The coefficient for the residual inequality remains insignificant. None of the additional covariates is statistically significant. When comparing these estimates with those in Table 3.1, we see that coefficients are smaller (in absolute term). However, the main takeaways remain the same: higher inequality results in lower growth rates, with IOp having a larger effect than the residual term.

Contrary to human capital and market distortions, short-term determinants of growth have a statistical significant effect in the IOp regression (column 4). Both the growth rate of per capita consumption and the number of hours worked in a week increase economic growth. In addition, both the coefficient for IOp and for the residual term are statistically significant. Moreover, while the coefficient for IOp does not change from the one in Table 3.1, the coefficient for the resid-

[12]When using the reduced sample, the estimates in columns 4 and 8 of Table 3.1 remain qualitatively unchanged (i.e., signs and statistical significance does not change).

ual inequality almost doubles once we control for these variables. Once we hold consumption, worked hours and debt levels constant, an increase in the residual inequality term (i.e., inequality of 'effort') reduces economic growth, more so than IOp.

## 3.6 Discussion

I use System GMM to estimate the effect of IOp on growth and show a negative effect of IOp on economic growth, measured as the annual growth rate of GNI per capita. I study 27 European countries between 2004 and 2012. In contrast with previous studies, I use 'upper bound' estimates of IOp, which account for all time-invariant sources of inequality. My estimates show that an increase in IOp results in a decrease in economic growth. This is consistent with the idea that unequal circumstances can hamper growth. However, my estimates also show that inequality of outcomes has a negative coefficient, albeit a less robust one. IOp, on the other hand, is robust to choice and number of instruments, to alternative estimation approaches and the inclusion of covariates. Overall, increases in both inequality of outcomes and IOp result in lower growth rates, with a much stronger effect for the latter.

It is important to highlight the context in which I estimate the effect of inequality on growth. This period of time includes two very large financial crises, the Great Recession and the European Debt crisis. As such, and together with the focus on short-term growth rates, my estimates cannot be directly extrapolated to other context. These estimates shows whether IOp (and inequality of outcomes) hinders growth in the context of generalised private debt, low consumption, and high unemployment, a context in which the distinction between efforts and circumstances appears to become less relevant.

This is not to say that the differences between efforts and circumstances became irrelevant. In almost all specifications the coefficient for IOp is higher in absolute value than the residual term (the difference between inequality of outcomes and IOp, sometimes interpreted as inequality of efforts). This is not consistent with the 'cholesterol hypothesis' (see, e.g., Marrero and Rodríguez (2019)), the idea that IOp is harmful for growth while the inequality of efforts. My estimates are consistent with a weaker version of this hypothesis, discussed in Ramos and Van de gaer (2020), where the effect of IOp is stronger than the effect of the residual term.

To some extent, my findings could be driven by the use of an upper bound estimate of IOp, in contrast with the more widely used lower bound estimates. The stronger effect of IOp could be driven by time-invariant factors that might not be considered to be circumstances (a common example of such a factor is having a 'hard working attitude'). Unfortunately, given current data availability, it is impossible to decompose the role of the upper bound estimate from the context in which I estimate its impact on growth. However, the fact that inequality of outcome also reports a negative coefficient suggests the context matters more than the measurement of IOp. The 2019 wave of the EU-SILC will include circumstance variables, making it possible to get enough years of lower bound estimates to compare their effect on growth using similar estimation techniques to those used in this paper.

To further explore the role of the financial crises in explaining these results I control for measures of private debt, worked hours and consumption growth. These variables have been highlighted as determinants of short-term growth as well as factors that reinforced the relationship between inequality and growth in high income countries (van Treeck, 2013). I find that worked hours and consumption growth are both predictors of short-term growth, and IOp remains a statistically significant predictor. Moreover, once I control for these variables, the coefficient for the residual term of inequality becomes larger (i.e., more negative) than IOp.

How to interpret the effect the residual component of inequality? In light of the upper bound measurement of IOp, this term includes all sources of income inequality that vary over time. In addition, as I use the Gini index to measure inequality, this measure accounts for both within-type inequality and the extent to each the different income types across types overlap. As a result, it cannot be directly interpreted as a measure of inequality of efforts, but rather as a ‘catch-all’ measure of inequality beyond that captured in the IOp.

In the context of two financial crises, I argue that this component accounts for the worsening economic climate as well as the efforts and individual choices taken by households, given this climate. Framed as an optimization problem, this measure captures both changing restrictions (access to liquidity, for example) and changing responses (in terms of consumption and work). If worked hours and levels of debt represent the latter, then they are proxies of ‘efforts’ (indeed, both have a positive coefficient on economic growth). Under that assumption, and once we control for these variables, the residual component of inequality becomes a measure of the worsening economic climate, which has a worse effect on economic growth than inequalities due to differences in childhood circumstances, represented by IOp.[13]

Future search should look into this relationship. Inequality of outcomes appears to be negatively associated with economic growth in the context of financial crises. What role does inequality of effort play in this context? Does the distinction between efforts and circumstances become less relevant as predictors of growth? Or does it depend on how we decompose inequality of outcomes. In a similar vein, Ramos and Van de gaer (2020) discuss how the effect of IOp (and inequality of efforts) on growth might not be robust to the measure of IOp. For example, the distinction between ex-ante or ex-post estimates of IOp, but also between upper and lower bound estimates. A better understanding of the relationship between growth and inequality in the context of financial crises can prove helpful in understanding the welfare implications in the context of high income inequalities, even if they are driven by differences in effort rather than circumstances.

[13]This distinction can also be framed using the work of Dworkin (1981a,b). IOp would represent initial brute luck, both socioeconomic and genetic, whereas the residual component (once we exclude efforts) could represent later brute luck (see Ferreira and Peragine (2015)).

# 3.A Appendix

## 3.A.1 Upper bound estimates of inequality of opportunity

Upper bound estimates use predicted fixed effects instead of a vector of circumstance variables, following a two-step process. The first step is a fixed effect regression for income, including both individual ($\eta_i$) and time fixed effects ($u_t$). This regression uses all years except the first, which in this case means three years. For example, to get the upper bound estimate of IOp of 2008 we need to estimate a fixed effect regression for years 2009, 2010, and 2011.[14]

$$\log(Y_{it}) = \alpha + \eta_i + u_t + \varepsilon_{it} \quad \text{for } t = \{2, 3, 4\}. \tag{3.10}$$

The second step uses the predicted fixed effect from the first step ($\hat{\eta}_i$) as a measure of circumstances. Using the first year for each respondent ($t = 1$).

$$\log(Y_{it}) = \delta + \psi\hat{\eta}_i + \omega_{it} \quad \text{for } t = \{1\}. \tag{3.11}$$

From equation 3.11, we build a counterfactual distribution that is only determined by changes in the circumstance variable:

$$\widehat{\log(Y_i)} = \hat{\delta} + \hat{\psi}\hat{\eta}_i. \tag{3.12}$$

If we measure inequality over the counterfactual distribution of earnings $\hat{Y}$ – in this case using the MLD index – we get the Inequality of Opportunity Level, or IOL.

$$\text{IOL} = I(\{\hat{Y}\}). \tag{3.13}$$

## 3.A.2 Descriptive statistics

[14] The complete methodology, including the departures from the method described in Niehues and Peichl (2014), as well as estimates and robustness checks are described the previous chapter.

Table 3.A.1: Descriptive statistics (growth rates and inequality)

| | | Mean | Std. Dev. | Min | Max | Observations |
|---|---|---|---|---|---|---|
| Growth | Overall | 0.016 | 0.050 | -0.278 | 0.184 | Total = 207 |
| | Between | | 0.017 | -0.011 | 0.051 | Countries = 27 |
| | Within | | 0.048 | -0.258 | 0.205 | Avg = 7.66 |
| Ineq. | Overall | 0.284 | 0.045 | 0.188 | 0.379 | Total = 207 |
| | Between | | 0.042 | 0.208 | 0.355 | Countries = 27 |
| | Within | | 0.017 | 0.239 | 0.338 | Avg = 7.66 |
| IOp | Overall | 0.231 | 0.043 | 0.087 | 0.345 | Total = 207 |
| | Between | | 0.038 | 0.164 | 0.314 | Countries = 27 |
| | Within | | 0.022 | 0.150 | 0.309 | Avg = 7.66 |

Note: Growth is the annual growth rate for GNI per capita. Inequality of outcomes (Ineq.) and inequality of opportunity (IOp) are measured using the Gini index.

Table 3.A.2: Descriptive statistics (covariates)

| | | Mean | Std. Dev. | Min | Max | Observations |
|---|---|---|---|---|---|---|
| Education (W) | Overall | 72.8 | 9.4 | 54.2 | 90.0 | Total = 207 |
| | Between | | 9.3 | 58.1 | 87.2 | Countries = 27 |
| | Within | | 2.6 | 65.3 | 80.4 | Avg = 7.66 |
| Education (M) | Overall | 76.4 | 7.6 | 60.5 | 89.9 | Total = 207 |
| | Between | | 7.5 | 66.6 | 88.4 | Countries = 27 |
| | Within | | 1.8 | 68.8 | 82.6 | Avg = 7.66 |
| Investment | Overall | 0.84 | 0.19 | 0.45 | 1.41 | Total = 207 |
| | Between | | 0.18 | 0.53 | 1.25 | Countries = 27 |
| | Within | | 0.08 | 0.66 | 1.05 | Avg = 7.66 |
| Consumption | Overall | 1.73 | 4.65 | -16.5 | 19.8 | Total = 200 |
| | Between | | 1.84 | -0.35 | 5.26 | Countries = 26 |
| | Within | | 4.30 | -19.3 | 16.3 | Avg = 7.69 |
| Weekly hours | Overall | 33.7 | 3.8 | 27.2 | 41.7 | Total = 207 |
| | Between | | 3.8 | 27.5 | 41.1 | Countries = 27 |
| | Within | | 0.5 | 31.5 | 35.4 | Avg = 7.66 |
| Bank debt | Overall | 97.4 | 46.4 | 26.0 | 243.1 | Total = 194 |
| | Between | | 44.5 | 35.2 | 195.4 | Countries = 27 |
| | Within | | 13.7 | 49.3 | 145.0 | Avg = 7.19 |

Education (W and M): The share of the population over 25 with at least completed upper secondary (ISCED 0 to 4), separate for women and men (Eurostat). Investment: The price level of capital formation (in PPP) relative to the United States exchange rate (Penn World Table). Weekly hours: Average weekly hours worked by persons engaged (Penn World Table). Consumption: The annual growth rate for per capita consumption for households and NPISHs (World Bank). Bank debt: The domestic credit to private sector by banks as a share of GDP (World Bank).

Table 3.A.3: Robustness check 1 - Different instrument choice

| VARIABLES | (1) Ineq | (2) Ineq | (3) Ineq | (4) IOp | (5) IOp | (6) IOp | (7) IOp | (8) IOp | (9) IOp |
|---|---|---|---|---|---|---|---|---|---|
| Inequality | -0.463 | -0.673** | -0.749** | | | | | | |
| | (0.321) | (0.330) | (0.358) | | | | | | |
| IOp | | | | -0.513 | -0.579** | -0.773* | -0.656 | -0.702* | -0.842** |
| | | | | (0.353) | (0.273) | (0.452) | (0.433) | (0.385) | (0.418) |
| IR | | | | -0.660* | -0.302 | -0.151 | -0.660 | -0.438 | -0.334 |
| | | | | (0.361) | (0.319) | (0.619) | (0.512) | (0.363) | (0.547) |
| Log GNI | -0.048** | -0.045*** | -0.046*** | -0.049** | -0.041*** | -0.045** | -0.055*** | -0.051** | -0.047*** |
| | (0.022) | (0.016) | (0.015) | (0.021) | (0.012) | (0.023) | (0.020) | (0.022) | (0.016) |
| Constant | 0.635** | 0.668*** | 0.706*** | 0.661** | 0.587*** | 0.685* | 0.765*** | 0.732** | 0.726*** |
| | (0.277) | (0.249) | (0.262) | (0.292) | (0.175) | (0.356) | (0.295) | (0.329) | (0.266) |
| Observations | 207 | 207 | 207 | 207 | 207 | 207 | 207 | 207 | 207 |
| Number of countries | 27 | 27 | 27 | 27 | 27 | 27 | 27 | 27 | 27 |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Instruments | 53 | 45 | 36 | 68 | 60 | 42 | 53 | 45 | 36 |
| Instrument IR | – | – | – | Yes | Yes | Yes | No | No | No |
| Lags growth in level | 1st | 1st | 1st | 1st | 1st | 1st | 1st | 1st | 1st |
| Lags growth in diff | 2 to 3 | 2nd | 2nd | 2 to 3 | 2nd | 2nd | 2 to 3 | 2nd | 2nd |
| Lags ineq. in level | 1 to 3 | 1 to 3 | 1st | 1 to 3 | 1 to 3 | 1 to 3 | 1 to 3 | 1 to 3 | 1 to 3 |
| Sargan Test | 0.001 | 0.003 | 0.001 | 0.001 | 0.002 | 0.002 | 0.000 | 0.001 | 0.001 |
| Hansen Test | 0.998 | 0.995 | 0.987 | 1.000 | 1.000 | 0.971 | 1.000 | 0.990 | 0.945 |
| AR(1) Test | 0.179 | 0.168 | 0.164 | 0.161 | 0.167 | 0.153 | 0.167 | 0.179 | 0.154 |
| AR(2) Test | 0.248 | 0.274 | 0.294 | 0.265 | 0.273 | 0.297 | 0.273 | 0.277 | 0.316 |

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Windmeijer-corrected standard errors, clustered at the country level. The dependent variable is the annual growth rate of GNI per capita (in constant 2010 US dollars). All estimations include 27 European countries for the years 2004 to 2011 (2005-2012 for the UK and Ireland). The main independent variable for columns 1 to 3 is the level of income inequality, and the upper bound estimate of inequality of opportunity (IOp) for columns 4 to 9. Both using the Gini index. Columns 1, 4, and 7: In differences (i.e., for the level equation), I include the first lag of log GNI per capita and the first three lags of inequality. In levels (i.e., for the difference equation), I include the second and third lags, both for log GNI per capita and inequality. Columns 2, 5, and 8 drop the third lag of inequality (level equation), and columns 3, 6, and 9 also drop the third lag of both instruments (difference equation). The Sargan and Hansen statistics are tests of overidentifying restrictions, the null being the joint validity of all instruments. The AR(1) and AR(2) statistics are tests of autocorrelation of order 1 and 2, the null being no autocorrelation of the residuals. Hansen tests for each subset of instruments were estimated (not included), the null hypothesis is not rejected in any of the cases.

Table 3.A.4: Robustness check 2 - Fewer instruments (PCA only)

| VARIABLES | (1)<br>IOp | (2)<br>IOp | (3)<br>IOp |
|---|---|---|---|
| IOp | -0.727* | -0.720* | -0.691* |
| | (0.411) | (0.385) | (0.385) |
| IR | -0.403 | -0.403 | -0.404 |
| | (0.503) | (0.454) | (0.447) |
| Log GNI | -0.043** | -0.043** | -0.042** |
| | (0.019) | (0.020) | (0.019) |
| Constant | 0.651** | 0.647** | 0.630** |
| | (0.309) | (0.306) | (0.294) |
| Observations | 207 | 207 | 207 |
| Number of countries | 27 | 27 | 27 |
| Instruments | 27 | 26 | 25 |
| Year FE | Yes | Yes | Yes |
| All lags | Yes | Yes | Yes |
| PCA | Yes | Yes | Yes |
| Collapsed instrument | No | No | No |
| Sargan Test | 0.317 | 0.254 | 0.214 |
| Hansen Test | 0.340 | 0.280 | 0.221 |
| AR(1) Test | 0.164 | 0.164 | 0.165 |
| AR(2) Test | 0.288 | 0.289 | 0.284 |
| KMO measure | 0.891 | 0.891 | 0.891 |

Note: *** p<0.01, ** p<0.05, * p<0.1. Windmeijer-corrected standard errors, clustered at the country level. The dependent variable is the annual growth rate of GNI per capita (in constant 2010 US dollars). All estimations include 27 European countries for the years 2004 to 2011 (2005-2012 for the UK and Ireland). The main independent variable for columns 1 to 4 is the level of income inequality, and the upper bound estimate of inequality of opportunity (IOp) for columns 5 to 8. Both using the Gini index. System GMM use the inequality estimates and log GNI per capita as 'GMM style' instruments (making use of multiple lags). The years fixed effects are included as regular 'IV style' instruments. Columns differ in the number of lags. For both inequality of outcomes and IOp I include, respectively: all lags, only the first lag, a collapsed instrument matrix, a reduced instrument matrix based on PCA. The Sargan and Hansen statistics are tests of overidentifying restrictions, the null being the joint validity of all instruments. The AR(1) and AR(2) statistics are tests of autocorrelation of order 1 and 2, the null being no autocorrelation of the residuals. The KMO measure is the Kaiser-Meyer-Olkin test for sampling adequacy when using PCA. As a rule of thumb, a KMO measure below 0.5 is unacceptable and above 0.8 is desirable.

Table 3.A.5: Robustness check 3 - IV approach

| VARIABLES | (1)<br>Ineq | (2)<br>Ineq | (3)<br>IOp | (4)<br>IOp |
|---|---|---|---|---|
| Inequality | -0.193 | -0.644* | | |
| | (0.194) | (0.354) | | |
| IOp | | | -0.179 | -1.011** |
| | | | (0.201) | (0.511) |
| IR | | | -0.258 | -0.928** |
| | | | (0.210) | (0.442) |
| Log GNI | -0.254*** | -0.259*** | -0.258*** | -0.259*** |
| | (0.061) | (0.056) | (0.058) | (0.060) |
| Constant | 2.800*** | 2.976*** | 2.840*** | 3.064*** |
| | (0.653) | (0.615) | (0.626) | (0.664) |
| Observations | 204 | 207 | 204 | 207 |
| R-squared | 0.597 | 0.581 | 0.597 | 0.529 |
| Year FE | Yes | Yes | Yes | Yes |
| Country FE | Yes | Yes | Yes | Yes |
| Estimation | OLS | 2SLS | OLS | 2SLS |

Note: *** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered at the country level. All estimations include 27 European countries for the years 2005 to 2011. The main independent variable for columns 1 and 2 is the level of income inequality, and the upper bound estimate of inequality of opportunity (IOp) for columns 3 and 4. Both using the Gini index. The 2SLS estimation (columns 2 and 4) requires a two steps process, done separately for total inequality and for IOp. The first step is an 2SLS estimation of inequality on time and year fixed effects, as well as the log of GNI (with lagged savings and growth rates as instruments). That estimation is then used to build the instrument $Z_{j,t} = I_{j,t} - \hat{\beta}\log(y_{j,t})$. The second step is a 2SLS estimation that uses said instrument to estimate the effect of inequality or IOp on growth.

Table 3.A.6: Robustness check 4 - Alternative estimation approaches

| VARIABLES | (1)<br>Ineq | (2)<br>IOp | (3)<br>Ineq | (4)<br>IOp |
|---|---|---|---|---|
| Inequality | -0.422** | | -0.277 | |
| | (0.199) | | (0.179) | |
| IOp | | -0.460* | | -0.266 |
| | | (0.238) | | (0.165) |
| IR | | -0.571** | | -0.313* |
| | | (0.291) | | (0.186) |
| Lagged growth | 0.183* | 0.188* | 0.280* | 0.282* |
| | (0.111) | (0.109) | (0.144) | (0.144) |
| Log GNI | -0.321*** | -0.323*** | -0.333*** | -0.335*** |
| | (0.064) | (0.065) | (0.062) | (0.063) |
| Constant | 3.376*** | 3.410*** | | |
| | (0.635) | (0.639) | | |
| Observations | 175 | 175 | 182 | 182 |
| Number of countries | 26 | 26 | 27 | 27 |
| Estimation | QML | QML | BCFE | BCFE |
| Repetitions | - | - | 250 | 250 |

Note: *** p<0.01, ** p<0.05, * p<0.1. QML: Quasi–maximum likelihood estimation of linear dynamic models. BCFE: Bootstrap-based bias correction for dynamic Panels with fixed effects. As they all use first differences and control for the first lag of growth, these estimates include a lower number of observations. QML excludes Ireland, as countries with interior gaps are dropped. BCFE includes all countries. BCFE uses boostrapped standard errors, each with 250 repetitions. All estimations include 27 European countries for the years 2005 to 2011. The main independent variable for columns 1 and 3 is the level of income inequality, and the upper bound estimate of inequality of opportunity (IOp) for columns 2 and 4. All using the Gini index.

Table 3.A.7: Effect of IOp on Growth: Including covariates

| VARIABLES | (1) Ineq | (2) IOp | (3) Ineq | (4) IOp |
|---|---|---|---|---|
| Inequality | -0.912* | | -0.264 | |
| | (0.495) | | (0.233) | |
| IOp | | -0.583** | | -0.707*** |
| | | (0.247) | | (0.214) |
| IR | | -0.207 | | -0.914** |
| | | (0.344) | | (0.362) |
| Fem. second. educ. | 0.002 | -0.001 | | |
| | (0.007) | (0.004) | | |
| Male second. educ. | -0.002 | -0.001 | | |
| | (0.007) | (0.003) | | |
| Price level of inv. | -0.326 | 0.057 | | |
| | (0.202) | (0.221) | | |
| Consumption p/c (growth) | | | 0.002 | 0.009*** |
| | | | (0.005) | (0.003) |
| Weekly worked hours | | | -0.012 | 0.007** |
| | | | (0.008) | (0.003) |
| Domestic credit from banks | | | 0.000 | 0.000 |
| | | | (0.000) | (0.000) |
| Log GNI | 0.009 | -0.062 | -0.048** | 0.002 |
| | (0.036) | (0.068) | (0.021) | (0.016) |
| Constant | 0.467 | 0.916 | 0.948** | -0.045 |
| | (0.441) | (0.739) | (0.445) | (0.200) |
| Observations | 207 | 207 | 187 | 187 |
| Number of countries | 27 | 27 | 26 | 26 |
| Instruments | 25 | 28 | 24 | 29 |
| Year FE | Yes | Yes | Yes | Yes |
| All lags | Yes | Yes | Yes | Yes |
| PCA | Yes | Yes | Yes | Yes |
| Collapsed instrument | No | No | No | No |
| Sargan Test | 0.133 | 0.245 | 0.168 | 0.236 |
| Hansen Test | 0.352 | 0.303 | 0.263 | 0.789 |
| AR(1) Test | 0.095 | 0.185 | 0.223 | 0.160 |
| AR(2) Test | 0.393 | 0.255 | 0.213 | 0.313 |
| KMO measure | 0.900 | 0.891 | 0.894 | 0.871 |

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Windmeijer-corrected standard errors, clustered at the country level. All estimations include 27 European countries for the years 2005 to 2011. The main independent variable is the upper bound of inequality of opportunity. System GMM uses log GNI per capita and inequality variables as 'GMM style' instruments (making use of multiple lags), as well as the years fixed effects, which are included as regular 'IV style' instruments. The Sargan and Hansen statistics are tests of overidentifying restrictions, the null being the joint validity of all instruments. The AR(1) and AR(2) statistics are tests of autocorrelation of order 1 and 2, the null being no autocorrelation of the residuals. The KMO measure is the Kaiser-Meyer-Olkin test for sampling adequacy for the use of Factor Analysis. As a rule of thumb, a KMO measure below 0.5 is unacceptable and above 0.8 is desirable. The sample size is constrained in columns 3 and 4 due to the availability of covariates.

# Chapter 4

# How much of intergenerational immobility can be attributed to differences in childhood circumstances?

## 4.1 Introduction

What is the relationship between inequality of opportunity (IOp) and a measure of intergenerational immobility, such as the intergenerational elasticity (IGE)? The IGE is the slope coefficient ('Beta') from a least-squares linear regression of the log of the offspring income (or earnings) and the log the same outcome for the parent (Jäntti and Jenkins, 2015). IOp estimates quantify the explanatory power – for example, through the R-squared of a linear regression – of a set of factors over which we have no control, typically referred to as circumstances (Roemer and Trannoy, 2015). If parental income is the only circumstance, then the IGE and the IOp estimate share the same functional form and Bourguignon (2018, pp. 114–115) shows how the IGE and IOp are directly associated. In this chapter, however, I focus on the case where parental income is not the only circumstance.

Both estimates of IOp and of the IGE summarise the influence of parental background on the offspring's outcome, albeit in different ways. The IGE considers the relationship between the income of the parent and their offspring. IOp estimates, on the other hand, represent parental background through multiple variables. While the IGE makes no assumptions on the legitimacy of intergenerational persistence, IOp explicitly states that all circumstances are sources of illegitimate inequality.

The IGE literature does not delve on the sources of persistence and thus avoids discussions on the 'optimal' level of mobility. On the other hand, achieving equality of opportunity means an IOp index of zero. This has explicit implications for how the influence of parental income is treated in each case. In the IGE case only part of the influence of parental income is treated as an illegitimate source of persistence (a 'circumstance'), whereas all of its influence – and indeed more than that – is considered as circumstance.

The influence of circumstances interacts with parental and offspring income in multiple ways. First, they can act as mediators between parents and their children. For example, high-income parents can invest in housing or or other assets, providing a financial buffer for their offspring. Second, certain circumstances can precede parental income. Parental occupation and education are strong predictors of their income, which then influences their offspring's income. Third, circumstances can directly influence the income of the offspring. The first two ways described here are part of the IGE, whereas the third one is not. I propose an empirical way of decomposing the influence of circumstances into each of these different ways.

I base my framework on the recursive models of Bowles and Nelson (1974), Conlisk (1974, 1977), Leibowitz (1974), Atkinson (1983), Jenkins (1985), among others (see Haveman and Wolfe (1995) for a review of this literature). These models use diagrams to describe how different factors account for the relationship between parental and offspring income. They include factors that account for background characteristics, parental investment choices, as well as choices taken by the offspring. I follow this approach to describe the three ways in which circumstances and income interact.

I start with parental income being the only circumstance. As mentioned before, in this case the IGE and IOp estimates are equivalent, as shown in Figure 4.1.

Figure 4.1: Parental income as the only circumstance

$$Y^P \longrightarrow Y^C$$

Note: $Y^P$: parental income. $Y^C$: offspring income.

Mediating circumstances ($C_2$) intervene in this relationship splitting the association between parental and offspring income into two: a direct and and an indirect path, as shown in Figure 4.2. Previous papers have used such a model to decompose the IGE (Blanden et al., 2007; Palomino et al., 2018) or the relationship between family income and children's outcomes (Washbrook et al., 2014).

Figure 4.2: Parental income and mediating circumstances

$$\begin{tikzcd} Y^P \arrow[r] \arrow[rr, bend right] & C_2 \arrow[r] & Y^C \end{tikzcd}$$

Note: $Y^P$: parental income. $Y^C$: offspring income. $C_2$: mediating circumstances.

Preceding circumstances ($C_1$) pre-date parental income. If we focus solely on the IGE, that is, the relationship between parental and offspring income, then preceding circumstances can only have an influence to the extent that they are correlated to parental income, as shown in Figure 4.3.

Figure 4.3: Parental income, preceding, and mediating circumstances

$$\begin{tikzcd} C_1 \arrow[r] & Y^P \arrow[r] \arrow[rr, bend right] & C_2 \arrow[r] & Y^C \end{tikzcd}$$

Note: $Y^P$: parental income. $Y^C$: offspring income. $C_1$: preceding circumstances. $C_2$: mediating circumstances.

Figures 4.2 and 4.3 tell us how much of intergenerational immobility (in income) can attributed to differences in childhood circumstances but there are be other factors at play. In an empirical exercise, for example, unobserved circumstances will not be considered. There might be other factors that also influence parental income, such as factors not deemed as circumstances. Jencks and Tach (2006) argue that innate talent is one such factor, in which case innate talent might contribute to the IGE but would not be considered a source of IOp. To account for these factors, Figure 4.4 includes the term $\Phi$ into the model, which, by construction, has a residual nature: it accounts for all determinants of parental income that are not included in $C_1$.

Figure 4.4: Parental income, preceding, and mediating circumstances

$$\begin{tikzcd} C_1 \arrow[r] & Y^P \arrow[r] \arrow[rr, bend right] & C_2 \arrow[r] & Y^C \\ \Phi \arrow[ur, bend right] & & & \end{tikzcd}$$

Note: $Y^P$: parental income. $Y^C$: offspring income. $C_1$: preceding circumstances. $C_2$: mediating circumstances. $\Phi$: all determinants of parental income not included in $C_1$.

The model in Figure 4.4 includes two departures from previous studies that decompose the IGE (see, e.g., Blanden et al. (2007)). First, I focus exclusively on factors that are conventionally defined as circumstances in the IOp literature. Hence I exclude individual characteristics that are determined later in life and that might be construed as choices, such as going into higher education or labour market outcomes. Second, I allow some circumstances to precede the relationship between parental and offspring's income, as well as for parental income to influence the offspring's income directly, not only through its influence on mediators.

The idea of an 'optimal' level of intergenerational immobility relates to whether we can interpret these estimates as a measure of inequality of opportunity or not. Black and Devereux (2011) state that while people tend to favour equality of opportunity as a goal, zero intergenerational correlation is not necessarily the optimum. Major and Machin (2018) argue that few people would advocate for a world of zero intergenerational immobility. However, these arguments do not account for the fact that circumstances – the driving force of IOp – can also have an influence beyond that of parental income. While the influence of circumstances might not account for the complete IGE, their influence might go beyond that of parental income.

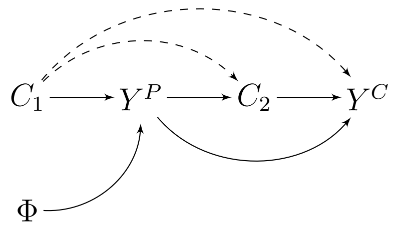
It is important to note that most (if not all) inequality of opportunity studies treat parental income as a circumstance. This is because most IOp estimates (whether by choice or due to lack of additional data on circumstances) follow a 'conventional' definition of IOp where outcomes depend on people's ability and effort, but not on their socioeconomic background (Cohen, 2009; Swift, 2013). This interpretation is inconsistent with the idea that the 'optimal' level of intergenerational immobility is anything but zero, as equality of opportunity is achieved when the influence of all circumstances is eliminated.[1] While there are nuances to this argument, for example, that we might tolerate some aspects of family influence (see Swift (2013, pp. 181–188) for a discussion) I treat parental income as a circumstance. In light of that, the residual term $U$ in Figure 4.5 can be interpreted as a measure of unobserved circumstances.

The final step of this model is to address the direct influence of circumstances. Concretely, preceding circumstances can influence mediating circumstances (for example, if the parent's occupations requires them to move to a different area) or the offspring income (if their children opt for the same occupation), as shown in Figure 4.5. By including this component, I go beyond the decomposition of the IGE to fully account for the influence of preceding circumstances on the income of the offspring.

Such a decomposition is highly demanding in terms of data. It requires information on the income of the offspring and their parents, ideally at a similar age. It also requires information on circumstances, preferably measured or reported by the parents themselves when they happened rather than retrospectively by their offspring. For that reason, I use Panel Study of Income Dynamics (PSID). The

[1] One way for the treatment of parental income as a circumstances to be consistent with

Figure 4.5: Direct and indirect influence of preceding circumstances




Note: $Y^P$: parental income. $Y^C$: offspring income. $C_1$: preceding circumstances. $C_2$: mediating circumstances. $\Phi$: all determinants of parental income not included in $C_1$. The dashed paths represent the influence of preceding circumstances outside that of parental income.

PSID is a longitudinal panel survey in the US, starting on 1968 with 4,800 families and following them, their offspring, and all future generations, with the last survey carried out on 2017. Because of its long-running and exhaustive nature, the PSID has been extensively used to estimate intergenerational mobility patterns in the US (Mazumder, 2018).

I focus on two outcomes: individual earnings (where I study father-sons couples) and family income (where I include both women and men). Both outcomes are averaged over 6 to 9 survey waves: 1981–1989 for the parent's generation, and 2001–2017 for the offspring, as the survey became biennial in 1997. I observe the offspring generation in 2017 and the parent generation almost 30 years before that, in 1989, when the offspring were 0 to 20 years old (median age: 9). Studying earnings captures intergenerational persistence in the labour market. On the other hand, persistence in family income allows for a broader measure of economic welfare that, unlike earnings, does not suffer from selection issues and can account for other dynamics such as the earnings of their partners and the working status of their offspring, which might reinforce or weaken existing inequalities in earnings.

The IGE for individual earnings is 0.35 (95% CI: [0.23;0.47]) and the IGE for family income is 0.53 (95% CI: [0.47;0.58]). I report the decomposition in steps, following Figures 4.2, 4.4 and 4.5. First, circumstances mediate around a third of the relationship between parental and offspring income (32% for earnings, 36% for income). Among the mediating circumstances, families having above-median savings accounts for almost all of the total contribution (19% and 25% of the IGE, respectively). Preceding circumstances make a big difference, accounting for over half of the IGE (55% and 53%), with parental education (in years) explaining over a third of that contribution. Both high savings and parental education make substantial contributions to the IGE, but the influence of parental education on savings accounts for a negligible share of the their total contribution. Overall, few circumstances account for most of the IGE, with very little interaction among them.

The direct influence of circumstances (specifically, of preceding circumstances) accounts for a large part of their total influence the income of the offspring. Over half of the contribution of parental education – the circumstance that accounts for most of the IGE – does not 'pass' through parental income (55% in the case of earnings, 64% in the case of income). While childhood circumstances explain most of the IGE, they also have an influence beyond the correlation for the income of the parents and their children. If we care about equality of opportunity, we need to consider that influence when discussing intergenerational immobility patterns.

This chapter contributes to the literature on intergenerational persistence in two ways. First, I expand on the literature of IGE decomposition to include factors that precede parental income as well as treating parental income (and thus, the IGE) as a mediator of the larger relationship between childhood circumstances and offspring income, as presented in IOp studies. Second, I bridge the gap between the work on IGE and IOp estimates. Previous paper have noted their isomorphism and similarities (Brunori et al., 2013; Ferreira and Gignoux, 2014; Bourguignon, 2018), but no study to date has provided a systematic way to study the relationship between parental income and other circumstances, and their influence of the income of the offspring. This chapter puts in perspective the role played by a measure of immobility such as the IGE in the context of the IOp literature: parental income is a circumstance that cannot be fully accounted for by other more 'traditional' circumstances and, at the same time, these other circumstances do play a role that goes beyond that of parental income. As such, equality of opportunity would imply an IGE of zero, while the converse might not be true.

## 4.2 An 'IOp' decomposition of the IGE elasticity and beyond

### 4.2.1 Decomposition framework

In this section I model the interaction between the income of the parent, the income of the offspring and childhood circumstances. I present the decomposition framework in three steps, following the description in the introduction. First, I account for mediating circumstances that lie between parental and offspring income and account for part of the IGE. Second, I include preceding circumstances, that is, circumstances that influence parental income. Keeping the focus on the IGE, I study the role of these circumstances to the extent that they correlate with parental income. Lastly, I account for factors that lie outside of the IGE by allowing for preceding circumstances to have a direct influence on the income of the offspring.

By following this order, I first determine the extent to which childhood circumstances account for intergenerational immobility and then move to their influence beyond parental income. As Roemer (2004) puts it, the first two decompositions are an appropriate measure of IOp if the influence of parental income on the income of the offspring summarises all transmission mechanisms between parents and their children. However, in the IOp literature parental income is one of many potential circumstances that influence children's income. The last step of my decomposition follows this approach and accounts for the share of the total influence of preceding circumstances that is uncorrelated with parental income.

The first part of my framework, the decomposition of the IGE, is based on the literature of determinants of intergenerational persistence (see Blanden et al. (2007); Washbrook et al. (2014); Gregg et al. (2017), among others). This literature uses a system of equation to describe a 'quasi-structural' model of the different paths through which parental income can influence the children's outcomes such as income, education, or early childhood tests. These 'paths' account for a share of the total association between parent's and their children, usually measured through the IGE or an equivalent metric.

I also draw from previous work on recursive models (see, e.g., Haveman and Wolfe (1995)). This line of research also studies the determinants of children's attaintment, albeit in a broader way, allowing for other 'paths' outside of parental income. For example, in the model of Leibowitz (1974) parental abilities and education influences family income (as preceding circumstances do, in my model) but they also influence heredity (i.e., biological inheritance), that influences the ability of the offspring, their education, choices, and income. These models allow for a more comprehensive economic perspective that specifies different ways in which circumstances influence the income of the offspring.

The decomposition approach, as described in the introduction, starts with an estimate of intergenerational persistence. I use an estimate of the IGE, $\beta$, measured as the slope coefficient from an OLS regression of the log of offspring income (or earnings) on the log of parental income (or earnings), described in equation 4.1.

$$\ln Y^C = \alpha + \beta \ln Y^P + \phi. \tag{4.1}$$

In my model, $\ln Y_i$ is either the log of individual earnings or the log of total family income. The superscript $C$ or $P$ represents the offspring or the parent, respectively. $\alpha$ is a constant and $\phi$ is an error term. For simplicity's sake I refer to $Y^C$ and $Y^P$ as income in this section.

### 4.2.2 Accounting for mediating circumstances

Mediating circumstances fall between parental income and offspring's income, being influenced by the former and influencing the latter. I include as mediating circumstances the region of birth of the offspring, measures of assets of the parents (owning a house, stocks, businesses, or savings) and whether the family used food stamps, all measured in 1989 when the offspring were between 0 and 20 years old.[2] The inclusion of mediating circumstances results in two possible components of transmission, a mediated and an unmediated component.

The $C_2$ term in Figure 4.2 represents a vector of circumstances, a fact better represented in the following equations rather than in the Figure. Each circumstance within $C_2$ accounts for a separate part of the IGE and there are no interactions between them (i.e., if were to I expand $C_2$ into its components, there would be no arrows between them, see Figure 4.A.1 in the Appendix).

Equation 4.2 represents the influence of mediating circumstances and of parental income on the offspring's income. Equation 4.3 represents the association between parental income and each of the circumstances in $C_2$. Note that Equation 4.2 is the standard reduced form equation that researchers use to derive a version of the lower bound estimates of IOp if parental income and $C_2$ are the only circumstances (see, e.g., Ferreira and Gignoux (2014)). If we have $K_2$ circumstances in $C_2$, indexed by $k$, there are 2 equations:

$$\begin{aligned} \mathrm{ln}Y^C &= \omega_1 + \sum_{k=1}^{K_2} \pi_{1k} C_{2k} + \theta_1 \mathrm{ln}Y^P + u_1 && (4.2) \\ C_{2k} &= \alpha_{2k} + \lambda_{1k} \mathrm{ln}Y^P + \varepsilon_{2k}, \ \forall \ k = 1, \ldots, K_2. && (4.3) \end{aligned}$$

By including equation 4.3 into equation 4.2, I get:

$$\mathrm{ln}Y^C = \omega_1 + \sum_{k=1}^{K_2} \pi_k \alpha_{2k} + \left( \theta_1 + \sum_{k=1}^{K_2} \pi_{1k} \lambda_{1k} \right) \mathrm{ln}Y^P + u_1 + \sum_{k=1}^{K_2} \pi_{1k} \varepsilon_{2k}. \quad (4.4)$$

Equation 4.4 shows the two components through which parental income influences offspring income. To decompose $\beta$ from equation 4.1 into these two components, I use the definition for the regression coefficient under a linear model:

$$\beta = \frac{\mathrm{Cov}(\mathrm{ln}Y^C, \mathrm{ln}Y^P)}{\mathrm{Var}(\mathrm{ln}Y^P)}. \quad (4.5)$$

[2] I also include robustness checks, capping the offspring's age in 1989 at 18 and 22 years of age with minor differences in the decomposition.

By substituting equation 4.4 into equation 4.5 and given that the correlation between $\ln Y^P$ and the predicted error term is zero, I get the following decomposition of the IGE coefficient $\beta$:

$$\beta = \overbrace{\theta_1}^{Y^C \to Y^P} + \overbrace{\sum_{k=1}^{K_2} \pi_{1k}\lambda_{1k}}^{Y^C \to C_2 \to Y^P}. \tag{4.6}$$

Equation 4.6 shows how $\beta$ is decomposed into two components, each represented as a combination of regression coefficients. The first term $\theta_1$ accounts for the association between parental and offspring's income, once we control for mediating circumstances. The second term accounts for mediating circumstances $C_2$ and comprises $\pi_k$, the regression coefficient for mediating circumstance $C_{2k}$ on offspring's income and $\lambda_{1k}$, the regression coefficient for parental income on mediating circumstance $C_{2k}$.

### 4.2.3 Accounting for preceding and mediating circumstances

By including preceding circumstances, I describe the model shown in Figure 4.4. $C_1$ denotes the set of preceding circumstances: circumstances that come before parental income chronologically. In this group I include the IQ of the head of the family (measured in 1972)[3], the years of education of the parent with the highest education and the occupation of the parent (measured in 1989 using the 3-digit 1970 Census codes and then grouped into seven categories), the ethnicity of the parent (binary category: white or person of colour) and the size of the place in which they grew up in (farm, town, city, other).

Under preceding circumstances, the framework decomposes the IGE into four components. First, the mediated and unmediated channels discussed before – whether the component passes through $C_2$ or not. Second, influence can stem from preced-

[3]I assign to the parent in 1989 the IQ score of whoever is the head of family in 1972, and therefore should be interpreted as a rough measure of 'family abilities'.

ing circumstances ($C_1$) or through the residual term $\Phi$. Similarly to the definition of 'effort' for most IOp estimates, $\Phi$ has a residual nature: whatever is not considered a preceding circumstance falls within $\Phi$, including unobserved circumstances or factors that might not be considered circumstances.

As with $C_2$, $C_1$ is also a vector of circumstances with no interaction among them. However, every circumstance in $C_2$ is associated with every circumstance in $C_1$. Figure 4.A.1 in the Appendix is an extended version of Figure 4.4, including all existing interactions in the following equations.

By including $C_1$ in the decomposition of $\beta$ I add three new equations (technically, I add one new equation and extend equations 4.2 and 4.3 to account for $C_1$). If we have $K_2$ circumstances in $C_2$, indexed by $k$, we get a set of $K_2 + 2$ equations:

$$\ln Y^P = \alpha_1 + \sum_{j=1}^{K_1} \kappa_j C_{1j} + \phi_2. \tag{4.7}$$

$$C_{2k} = \alpha_{2k} + \lambda_{2k} \ln Y^P + \sum_{j=1}^{K_1} \delta_{kj} C_{1j} + \varepsilon_{2k}, \ \forall \ k = 1, \ldots, K_2. \tag{4.8}$$

$$\ln Y^C = \omega_2 + \sum_{j=1}^{K_1} \rho_{2j} C_{1j} + \sum_{k=1}^{K_2} \pi_{2k} C_{2k} + \theta_2 \ln Y^P + u_2. \tag{4.9}$$

The final set of equations represented in Figure 4.4 includes equation 4.7, 4.8, and 4.9. Equation 4.7 represents the influence of preceding circumstances on parental income (i.e, $C_1 \rightarrow Y^P$) and that of the residual term ($\Phi \rightarrow Y^P$). Equation 4.8 represents the mediated components and includes the influence of parental income ($Y^P \rightarrow C_2$) and that of of preceding circumstances ($C_1 \rightarrow C_2$). Lastly, equation 4.9 represents the influence of all factors on offspring's income: the unmediated influence of preceding circumstances ($C_1 \rightarrow Y^C$), the influence of mediating circumstances ($C_2 \rightarrow Y^C$) and the influence of parental income ($Y^P \rightarrow Y^C$). Just like equation 4.2, equation 4.9 is the standard way to measure IOp when parental income, $C_1$ and $C_2$ are circumstances.

By substituting equations 4.7 and 4.8 into equation 4.9 and using the same approach as in the previous section, I decompose $\beta$ into the four components of Figure 4.4.

$$\beta = \theta_2 + \sum_{k=1}^{K_2} \pi_{2k}\lambda_{2k} + \sum_{j=1}^{K_1} \left[ \left( \rho_{2j} + \sum_{k=1}^{K_2} \pi_{2k}\delta_{jk} \right) \frac{\text{Cov}(C_{1j}, \ln Y^P)}{\text{Var}(\ln Y^P)} \right]. \tag{4.10}$$

The first component $\theta_2$, the influence of parental income conditional on all circumstances, is the only component not associated to circumstances. This component can be interpreted as the influence of $\Phi$ in Figure 4.4: the residual influence of parental income, once I control for preceding and mediating circumstances. All other components are associated with either preceding circumstances, mediating circumstances, or both.

The other three components account for the contribution of circumstances to the IGE. The term $\sum_{k=1}^{K_2} \pi_{2k}\lambda_{2k}$ represents influence of $\Phi$ on mediating circumstances. $\lambda_{2k}$ is the regression coefficient for parental income on mediating circumstances and $\pi_{2k}$ is the regression coefficient for mediating circumstances on offspring income, for each of the $K_2$ circumstances. The term $\rho_{2j}$ represents the unmediated influence of each of the $K_1$ preceding circumstances. Lastly, the term $\sum_{k=1}^{K_2} \pi_{2k}\delta_{jk}$ represents the mediated influence of the same preceding circumstances. It combines $\delta_{jk}$, the regression coefficient for preceding circumstances on mediating circumstances and $\pi_{2k}$, the regression coefficient for mediating circumstances on offspring's income. The latter two terms are weighted by the correlation between preceding circumstances and parental income.

We can get a clearer idea of the contribution of preceding circumstances by comparing the first decomposition – that only includes mediating circumstances – to that of the second decomposition, with both mediating and preceding circumstances. It shows how both the mediated and the unmediated components are divided into two terms each: one stemming from $C_1$ and one stemming from $\Phi$.

$$\theta_1 = \theta_2 + \sum_{j=1}^{K_1} \rho_{2j} \frac{\text{Cov}(C_{1j}, \ln Y^P)}{\text{Var}(\ln Y^P)} \tag{4.11}$$

$$\sum_{k=1}^{K_2} \pi_{1k}\lambda_{1k} = \sum_{k=1}^{K_2} \pi_{2k}\lambda_{2k} + \sum_{j=1}^{K_1}\sum_{k=1}^{K_2} \pi_{2k}\delta_{jk} \frac{\text{Cov}(C_{1j}, \ln Y^P)}{\text{Var}(\ln Y^P)} \tag{4.12}$$

I can also rearrange the final decomposition in equation 4.10 to reflect each of the components into which the IGE is decomposed.

$$\begin{aligned}\beta = {} & \overbrace{\theta_2}^{\Phi \to Y^P \to Y^C} + \overbrace{\sum_{k=1}^{K_2} \pi_{2k}\lambda_{2k}}^{\Phi \to Y^P \to C_2 \to Y^C} + \overbrace{\sum_{j=1}^{K_1} \rho_{2j} \frac{\text{Cov}(C_{1j}, \ln Y^P)}{\text{Var}(\ln Y^P)}}^{C_1 \to Y^P \to Y^C} \\ & + \underbrace{\sum_{j=1}^{K_1}\sum_{k=1}^{K_2} \pi_{2k}\delta_{jk} \frac{\text{Cov}(C_{1j}, \ln Y^P)}{\text{Var}(\ln Y^P)}}_{C_1 \to Y^P \to C_2 \to Y^C} .\end{aligned} \tag{4.13}$$

Note that up to now the association between preceding circumstances and offspring's income is exclusively mediated by parental income (i.e., $C_1 \to Y^P \to Y^C$). Given that I have focused on the IGE, preceding circumstances matter to the extent that they correlate with the income of the father. Even preceding circumstances have an important influence of the income of the offspring (captured by the $\rho$ coefficient), their contribution to the IGE will be zero if they do not correlate with parental income $\big(\text{Cov}(C_{1j}, \ln Y^P) = 0\big)$. I remove this restriction in the following section to study the total contribution of $C_1$ on $Y^C$.

### 4.2.4 Accounting for the direct influence of preceding circumstances

To account for the complete influence of preceding circumstances on the offspring of the income, I need to move beyond the relationship between parent and offspring income. That means partitioning the contribution of preceding circumstances into the ones influencing the IGE (represented by equation 4.7) and their direct influence (as determined by the regression coefficient for $C_1$ in equations 4.8 and 4.9).

I start by including equations 4.7 and 4.8 into equation 4.9. Grouping all terms associated to $C_1$, I get all the potential ways in which preceding circumstances influence the income of the offspring.

$$\ln Y^C = \Xi + \sum_{j=1}^{K_1} \left( \rho_{2j} + \theta_2 \kappa_j + (1+\kappa_j) \sum_{k=1}^{K_2} \pi_k \delta_{kj} \right) C_{1j} + \Sigma. \tag{4.14}$$

Where the constant term and the error term include:

$$\Xi = \omega_2 + \sum_{k=1}^{K_2} \pi_{2k} \alpha_{2k} + \left( \theta_2 + \sum_{k=1}^{K_2} \pi_{2k} \lambda_{2k} \right) \alpha_1, \tag{4.15}$$

$$\Sigma = u_2 + \sum_{k=1}^{K_2} \pi_{2k} \varepsilon_{2k} + \left( \theta_2 + \sum_{k=1}^{K_2} \pi_{2k} \lambda_{2k} \right) \phi_2. \tag{4.16}$$

Using the same decomposition approach as in the previous section, but now focusing on the regression coefficient for preceding circumstance $C_{1j}$ on offspring income $lnY^C$ , we get:

$$\frac{\mathrm{Cov}(\ln Y^C, C_{1j})}{\mathrm{Var}(C_{1j})} = \underbrace{\overbrace{\rho_{2j}}^{C_1 \to Y^C} + \overbrace{\theta_2 \kappa_j}^{C_1 \to C_2 \to Y^C}}_{\text{Direct}} + \underbrace{\overbrace{\sum_{k=1}^{K_2} \pi_k \delta_{kj}}^{C_1 \to Y^P \to Y^C} + \overbrace{\kappa_j \sum_{k=1}^{K_2} \pi_k \delta_{kj}}^{C_1 \to Y^P \to C_2 \to Y^C}}_{\text{Indirect (through the IGE)}}. \tag{4.17}$$

The first two capture the 'direct' influence of preceding circumstances. That is, the influence that does not pass through parental income, thus being excluded in the IGE. The last two terms, on the other hand, capture their influence passing through parental income, that is, their contribution to the IGE.

This decomposition only accounts for the influence of one preceding circumstances at at time. That is, it is equivalent to decompose the regression coefficient of one particular circumstance on the income of the offspring:

$$\ln Y^C = \omega_3 + \psi_j C_{1j} + u_3. \tag{4.18}$$

Where the $\psi_j$ coefficient is equal to the definition in equation 4.17. As a result, I do not provide a summary of the 'total' contribution of circumstances, nor of their relative importance. To provide some measure of the relative importance each circumstance plays, I include the R-squared of the OLS regression of equation 4.18.[4]

## 4.3 Data

I use the Panel Study of Income Dynamics (PSID), a household panel survey for the USA that has followed the same individuals and their descendants since 1968. The PSID has been used extensively to study the intergenerational mobility of many different outcomes (Mazumder, 2018). Being a long-running panel, it also includes extensive information on multiple generations, particularly childhood circumstances as reported by the parents themselves when they happened, in contrast with most cross sectional surveys where circumstances are repored retrospectively by the offspring. Because of its detailed characterization of the socioeconomic background while growing up, the PSID is among the best surveys to study inequality of opportunity and intergenerational transmission in the context of high-income countries.

To maximise comparability, I use similar definitions and samples as previous research on IGE estimations (see Mazumder (2018) for a survey). For individual earnings I study only fathers and sons. For family income I include both men and women. I restrict the sample to the head of the family unit, as most circumstances are only measured for them. My outcome variables are individual earnings and

[4]Bourguignon (2018) shows that the R-squared can be interpreted as a measure of relative IOp if our inequality index is the variance of the logarithm of the predicted outcome, $\texttt{Var}\left(log(\hat{\beta}C_i)\right)$.

family income, averaged over 6 to 9 years of data. Long-term averages reduce the attenuation bias from measurement error or transitory fluctuations (Solon, 1992). Overall, my IGE estimates – 0.35 for earnings and 0.53 for income – fall within the range of previous estimates. For example, Gouskova et al. (2010) reports IGE estimates ranging from 0.3 and 0.4 for individual earnings and Lee and Solon (2009) reports estimates ranging from 0.35 to 0.55 for family income.

I match parents and their offspring using the PSID's Family Identification Mapping System (FIMS). The FIMS assigns the ID of every parent to each offspring. I merge each offspring to their biological or adoptive parents. The 2017 sample includes individuals from the 2nd PSID generation (with an median age of 50 years) up to the 7th generation (with an median age of 6 years). Of the 2017 offspring sample, 85% have a FIMS map (i.e., is the offspring of a previous PSID respondent). Within that group, 77% have at least one parent in the 1989 sample. The remaining sample (equivalent to 45% of the 2017 sample) includes 2017 respondents with no observed parents in the 1989 wave of the PSID, either because they do not have a FIMS map (as their parents were not interviewed, for example in the case the 1997 or 2017 immigrant refresher sample), or because their parents had died or attrited by 1989, in which case they have a FIMS map but no parent data.[5]

### 4.3.1 Outcome variables

I look at two outcomes: individual labour earnings and total family income. Individual labour earnings reflect the intergenerational persistence of skills and characteristics that are valued in the labour market. Unfortunately, to avoid dealing with the low labour market participation among women most IGE estimates are derived from samples that exclude mothers and daughters (see Chadwick and Solon (2002) for a case in which they do address it). Family income includes other sources besides earnings as well as income from other people in the family,

[5] Among the matched sample, 0.04% of respondents have three or more parents in the data (e.g., two biological parents and one adoptive parent). There are seven cases with three parents in the same household, with at least one parent with no information on its relation to the 1989 head of the family unit (`ER30608` = 0). I exclude these cases from the final sample.

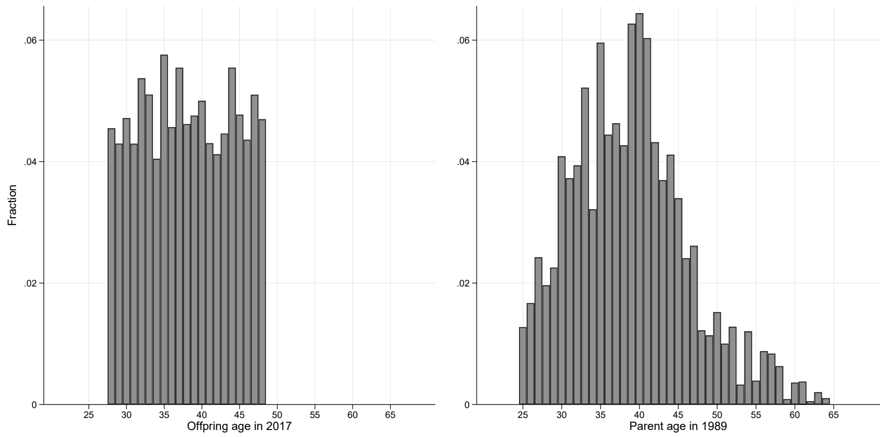
if present. The IGE for family income reflects the intergenerational persistence of other non-labour market attributes, such as capital income, social transfers, or income from the spouse. Whereas earnings focus on labour market advantages, Mazumder (2018) arguers that family income is much closer to consumption and therefore to the concepts of 'utility' or 'welfare'.

To reduce transitory fluctuations and measurement error I average both outcomes over multiple years. Mazumder (2005, 2016) shows that these fluctuations can result in a downward bias of up to 30%. I include 9 years of data for both the parents and offspring generations. In the parents' case, the period covers 1981 to 1989. For the offspring's generation, it covers the period 2001 to 2017, as the PSID changed from annual to biannual interviewing in 1997. I include all respondents with at least six observations over this period. On average, each respondent in the offspring's generation has 8.6 observations for earnings and 8.8 for income.

The outcomes were measured in 1989 for the parent's generation and in 2017 for that of the offspring. Circumstances were measured in 1989, with the exception of the parent's IQ score that was measured in 1972. For circumstances to be considered as such, I only include offspring that were 20 years of age or younger in 1989, as older offspring might be able to influence their own circumstances (for example, if they buy a house for their parents). For that reason, my sample consists of offspring aged 28 to 48 in 2017. I limit the sample of parents to those older than 25 years of age in 1989 to exclude younger respondents whose incomes could be substantially below their 'permanent' or long-term income (Jenkins, 1987; Haider and Solon, 2006) and I cap their age at 64, as the share of parents with positive earnings decreases rapidly after that. Figure 4.6 plots the age distribution for the offspring generation in 2017 (left plot) and the parental generation in 1989 (right plot). The Figure shows that offspring age is more constrained uniformly distributed than the parents. Both the average and the median age for both generations is around 39 years of age.

My earnings variable is the total labour income of the head of household. This includes farm and business income, wages, bonuses and overtime, and income

Figure 4.6: Age distribution for parents and offspring




Note: Family income sample ($N = 2,021$).

from independent professional practice. It also includes the labour part of market gardening (farm or gardening businesses) and of roomers and boarders (hospitality businesses). The PSID assigns 75% of the gardening business income to labour income (the rest being asset income) and 50% of the roomers and boarders income to labour income if they own the house (100% if the owners rent the house). If the respondent's business reports a loss, there is no labour income (i.e., there is no negative labour part of business income). I focus only on the earnings of the fathers and sons, to replicate previous estimates of IGE.

Family income includes total taxable income and transfers for all family members.[6] This includes taxable income, that is, wages and salaries, bonuses, overtime, and/or commissions, wife's labour income, farm and business income, income from rent, dividends, interest, trust funds, and royalties, alimony and other income from assets. It also includes transfer income, which comprises Aid to Dependent Children (ADC) or Aid to Families with Dependent Children (AFDC) – and after 1997 the Temporary Assistance for Needy Families (TANF) –, supplemental security income, other welfare, social security payments, veterans' administration pensions, other retirement, pensions and annuities, unemployment pay, workers' compensa-

[6]Following Mazumder (2016, 2018) and with the goal of comparability in mind, I focus on total rather than equivalised income.

tion, child support, help received from relatives and other transfers. I assign to each respondent the family income of their family unit in the corresponding year (1989 or 2017). For respondents with parents living in different households in 1989 and with both households in the survey, I opt for the household with the highest income.

I measure all outcomes in 2017 US dollars using the CPI provided by the U.S. Bureau of Labor Statistics. The reference period is the calendar year prior to the survey year (e.g., the 1989 survey includes all earnings from 1988). I drop all missing values for any of the variables (outcomes and circumstances). I keep siblings in the sample, and assign to each the outcome of the same parent, thus clustering the bootstrap at the parental family level. I use the 2017 cross-sectional sample weight to account for differential attrition in the SRC sample (the SEO sample is excluded).

My final sample includes 2,021 parent-offspring pairs for family income and 721 for individual earnings. The complete PSID sample includes 41,901 respondents for the 1989 sample and 26,445 for 2017. After using the FIMS to map parents and their offspring, the sample includes 16,453 parent-offspring pairs. By restricting the age range for both parents and offspring the sample decreases to 3,224 observations. Excluding the SEO sample results in a sample size of 2,056. Finally, constraining the sample to those offspring with circumstance data and, in the case of earnings, to only sons and fathers, leaves us with the final sample.

### 4.3.2 Circumstance variables

In the IOp literature, circumstances are involuntarily inherited factors that influence offspring's income and earnings. All of the circumstances used for my decomposition analysis are listed in Table 4.1. Except for the IQ score and the years of education of the parent with the highest education, all other variables are categorical. Except for the IQ score, which was measured in 1972, and the state where the offspring was born, all other circumstances were measured in 1989. The

IQ score is only available for 1972 (and earlier dates) and it is assigned to the head of the family of the test-taker in 1989. That means that this test was not necessarily taken by the 1989 head of the family, for example if a 1989 head of family lived with their parents in 1972. In such a case, that 1989 head of family will have had the test taken by one of their parents.[7]

Table 4.1: Description of all circumstance variables

| **Name** | **Description** |
|---|---|
| | **Preceding circumstances** |
| IQ score | Score on sentence completion test taken in 1972 (13 multiple choice questions – score goes from 0 to 13). |
| Education (years) | Years of education of the parent with the highest education (0 to 17 years). |
| Ethnicity | 1 if Black, American Indian, Aleut, Eskimo, Asian, Pacific Islander, other. 0 if White. |
| Occupation (Main occupation/Most important activity using 3-digit code 1970 Census) | Grouped into 7 categories: Professional, Manager, Clerical, Craftman, Operative, Farmer and Services. |
| Parent grew up in (Four categories) | |
| Farm | Farm, rural area, country. |
| Small town | Small town, any size town, suburb. |
| Large city | Large city, any size city. |
| Other | Other, several different places, combination of places, doesn't answer. |
| | **Mediating circumstances** |
| Homeowner | Family owns or is buying home, fully or jointly (includes mobile home owners who rent lots). |
| Over median: Business | Family owns above-median market value of farm or business. |
| Over median: Stocks | Family owns above-median market value of shares of stock, mutual funds, or investment trusts (incl. stocks in IRAs). |
| Over median: Savings | Family owns above-median money in checking or savings accounts, money market bonds, or Treasury bills (incl. IRAs). |
| Over median: Food stamps | Family received above-median food stamp benefits (now SNAP). |
| State where born | State where the offspring was born (50 states plus D.C., U.S. territory/outside U.S., and no response) |

Note: All circumstances are measured in 1989 (when the offspring were 0 to 20 years of age) with the 'parent grew up in' measured retrospectively. The two exceptions are the state where the offspring was born (measured at the year of birth) and the IQ score (taken by the head of the family unit in 1972).

[7] Among all heads in 1989, around half were not the head of family in 1972. This is the group that reports the IQ score of their parent.

Preceding circumstances ($C_1$) are allowed to influence mediating circumstances ($C_2$). However, the circumstances within each group do not influence each other (as shown in Figure 4.A.1 in the Appendix). This is because the temporal order is not as clear as it is between preceding and mediating circumstances. Also, given the large number of circumstances, adding these interactions would add an unnecessary amount of complexity to the model. Each new interaction would require an additional equation, rapidly increasing the number of individual components to be described. For example, if a circumstance $C_{1a}$ (say, parental education) were allowed to influence another circumstance $C_{1b}$ (parental occupation), both being part of $C_1$, the component $C_1 \rightarrow Y^C$ would need to be decomposed into $C_{1a} \rightarrow Y^C$ and $C_{1a} \rightarrow C_{1b} \rightarrow Y^C$, as would any other component in $C_1$. Such a detailed model is beyond the scope of this chapter.

A more complex model of intergenerational transmission would also need to include factors that might not be considered circumstances, e.g., post-school investments (as in Figure 1 in Haveman and Wolfe (1995)). I intentionally exclude these factors from my analysis. For example, the education of the offspring is an important factor when accounting for the intergenerational transmission of income, but I do not control for, nor for measured cognitive skills or the formation of preferences, as not everyone would consider them to be circumstances. As my focus is on the relationship between IOp and the IGE, I focus on circumstances that can be unequivocally interpreted as circumstances.[8]

## 4.4 IGE estimates and decomposition analysis

This section is organised into four subsections. I first report the IGE estimates and contrast them with previous studies. Then I move to the first decomposition of the IGE, by accounting for the influence of mediating circumstances. In the third part I also include preceding circumstances. The last subsection moves beyond the IGE

[8]For a detailed discussion on what constitutes a circumstance, see e.g., Cohen (1999); Bowles and Gintis (2002); Roemer (2004); Swift (2004); Jencks and Tach (2006); Torche (2015).

decomposition to account for the complete influence of preceding circumstances.

### 4.4.1 IGE estimates

Table 4.2 reports the IGE estimates for individual earnings and family income. The IGE is 0.35 for earnings and 0.53 for income. These estimates are within the range of previous estimates that have used the same database. Two good references for that comparison are Mazumder (2016, 2018). Mazumder (2016) estimates the IGE for both earnings and income by averaging these outcomes over a different number of waves. He restricts the PSID sample to all father-son pairs with available individual earnings or family income between the ages of 25 and 55 from 1967 to 2010. Mazumder (2018) provides an extensive review of IGE estimates using the PSID and other data sources.

Table 4.2: IGE estimate for individual earnings and family income

| | Earnings | Income |
|---|---|---|
| IGE | 0.351 | 0.526 |

Note: Individual earnings for fathers and sons only ($N = 721$) and family income for all offspring and the head of household in 1989 ($N = 2,021$).

The IGE estimates for earnings in Mazumder (2016) range from 0.3 for a one-year measure, to over 0.65 for 15-year averages for fathers and 10-years averages for sons. If we look at the equivalent of my estimate, 9-year averages for fathers and sons, the estimate is 0.39, while the arithmetic average for estimates with 6 to 9-year averages is 0.40. Mazumder (2018) reports the estimates from several papers. Among these estimates, most account for life-cycle bias resulting in IGE estimates of around 0.65.[9] For example, Gouskova et al. (2010) restrict the sample to the

[9]Life cycle adjustments can make an important difference when estimating the IGE, particularly for earnings. For example, Lee and Solon (2009) control for the interaction between parental income and a quartic polynomial of parental and offspring's age. Using their approach and centring the estimates around age 35, my IGE estimates increase to 0.58 for earnings and 0.61 for income. Accounting for this adjustment in my decomposition would require the inclusion of an additional term to reflect the inclusion of the age variables and their interactions.

male head of the household and their fathers and report an IGE for earnings of 0.41, which increases to 0.63 once they correct for age-varying attenuation bias. My estimates account for transitory variation by averaging the outcomes over a large number of years but do not account for life-cycle bias as that would require accounting for the adjusted estimation process (e.g., the inclusion of a polynomial of age) when decomposing the IGE.

For income, Mazumder (2016) reports IGE estimates ranging from 0.38 to 0.66. The 9-year averages for fathers and sons result in an IGE of 0.49, while the simple average for estimates with 6 to 9-year averages is 0.44. These estimates are particularly sensitive to the different samples. For example, the estimate using 8-year averages is 0.37. For that reason, Mazumder (2016) repeats his analysis for income using a fixed sample, keeping only individuals with 10 years of data. Using one-year measures for sons and fathers with 10-years of data, the IGE estimates are around 0.58. Among the selected papers in Mazumder (2018), the IGE for income ranges from 0.53 to 0.62. For example, Hertz (2005) restricts the PSID sample to all children born between 1942 and 1972 and observes their income when they were between 25 and 55 years of age. He reports an IGE estimate for the age-adjusted family income of around 0.5.

These estimates – and indeed, most IGE estimates – might suffer from different sources of bias due to the data quality. Following Jäntti and Jenkins (2015), there are two main issues when looking for 'long-term' measures of economic status to estimate $\beta$. The first one is the presence of transitory variations in income measures, resulting in attenuation bias. This issue results in a downward bias for IGE estimates and based on Solon (1992), it is typically solved by averaging multiple years of income data for both parents and their offspring, as I do in this paper. The second is life cycle bias, which states that observed income is below permanent or long-term income earlier in life, while being above it later in life. One solution is to observe incomes at similar ages for both parents and children (Grawe, 2006), which is why I cap the age of both parents and their offspring. To this two issues I add an additional source of bias, due to co-residency of parents and their children (Francesconi and Nicoletti, 2006). While this is an issue in short panels, the long running nature of the PSID helps in addressing it. Inevitably, these sources of bias might still persist despite my efforts to attenuate. However, the purpose of this chapter is not amend each source of bias but to obtain estimates as close as possible to those that have already been estimated, as to focus on the following decomposition.

### 4.4.2 Decomposing the IGE: Mediating circumstances

The inclusion of mediating circumstances splits the IGE into two components. A mediated component, where parental income influences these circumstances, and they in turn influence offspring income, and a second one where parental income influences offspring income directly. By construction, the latter component is a residual: it accounts for all other factors that are not included among mediating circumstances.

Table 4.3 presents the decomposition, including the contribution of each mediating circumstance. I also include the 95% confidence interval obtained from a bootstrap with replacement that iterated the whole decomposition process 1,000 times, clustered at the parental family level. In total, the mediating component accounts for 32% of the IGE for individual earnings and 36% for family income. The relative size is similar for both outcomes, but the IGE is much higher for family income. This shares as a part of each IGE are shown in Figure 4.7.

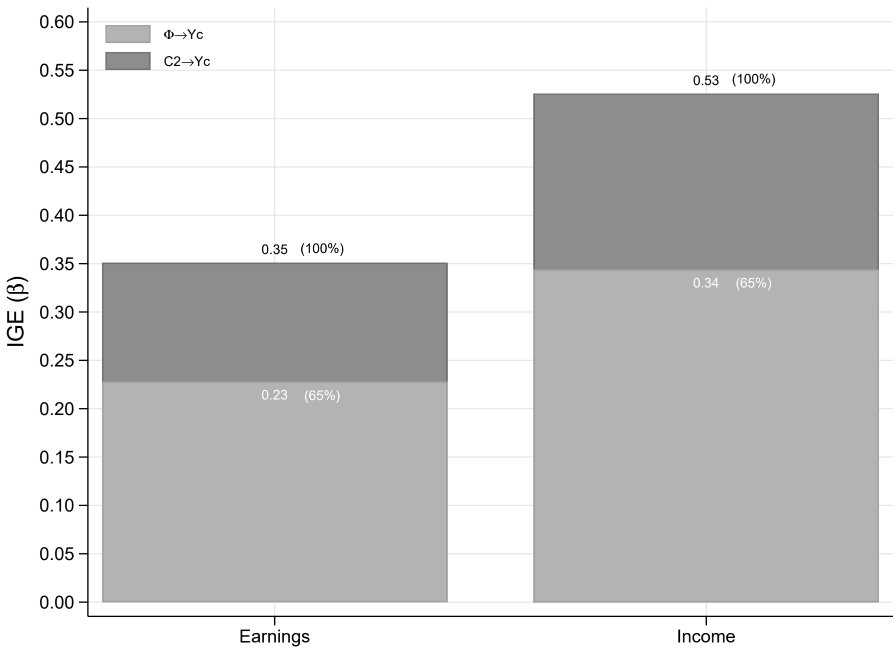
Table 4.3: IGE decomposition (Mediating circumstances)

| | Earnings | | | | | | Income | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Coef. | 95% CI | | % of IGE | 95% CI | | Coef. | 95% CI | | % of IGE | 95% CI | |
| Mediating Circumstances | | | | | | | | | | | | |
| Homeowner | 0.009 | -0.02 | 0.04 | 1.64 | -3.95 | 7.23 | 0.006 | -0.03 | 0.04 | 1.74 | -8.32 | 11.81 |
| Region: Mideast | -0.000 | -0.01 | 0.01 | -0.01 | -1.03 | 1.02 | 0.006 | -0.02 | 0.03 | 1.81 | -4.72 | 8.34 |
| Region: Great Lakes | 0.001 | -0.00 | 0.01 | 0.14 | -0.90 | 1.19 | 0.001 | -0.01 | 0.01 | 0.17 | -3.25 | 3.60 |
| Region: Plains | 0.001 | -0.00 | 0.01 | 0.14 | -0.72 | 0.99 | 0.008 | -0.02 | 0.04 | 2.19 | -6.86 | 11.24 |
| Region: Southeast | 0.014 | -0.00 | 0.03 | 2.64 | -0.54 | 5.83 | 0.005 | -0.01 | 0.03 | 1.51 | -4.52 | 7.53 |
| Region: Southwest | -0.002 | -0.01 | 0.00 | -0.42 | -1.77 | 0.94 | 0.000 | -0.02 | 0.02 | 0.02 | -6.06 | 6.11 |
| Region: Rocky Mount. | 0.000 | -0.00 | 0.00 | 0.01 | -0.31 | 0.33 | 0.001 | -0.01 | 0.01 | 0.15 | -1.92 | 2.22 |
| Region: Far West | -0.003 | -0.01 | 0.00 | -0.61 | -1.96 | 0.75 | -0.006 | -0.03 | 0.01 | -1.81 | -9.95 | 6.33 |
| Region: Outside U.S.A. | 0.000 | -0.00 | 0.00 | 0.05 | -0.32 | 0.42 | -0.005 | -0.02 | 0.01 | -1.35 | -7.32 | 4.62 |
| Region: No Answer | -0.000 | -0.00 | 0.00 | -0.00 | -0.27 | 0.27 | -0.000 | -0.00 | 0.00 | -0.00 | -0.49 | 0.49 |
| Over median: Business | 0.001 | -0.01 | 0.01 | 0.12 | -2.52 | 2.75 | 0.000 | -0.01 | 0.01 | 0.05 | -2.47 | 2.57 |
| Over median: Stocks | 0.026 | 0.00 | 0.05 | 5.01 | 0.63 | 9.39 | 0.002 | -0.04 | 0.05 | 0.64 | -13.76 | 15.03 |
| Over median: Savings | 0.100 | 0.07 | 0.13 | 19.00 | 12.68 | 25.33 | 0.088 | 0.04 | 0.14 | 25.25 | 8.84 | 41.67 |
| Used food stamps | 0.023 | -0.01 | 0.05 | 4.39 | -1.34 | 10.12 | 0.020 | -0.01 | 0.05 | 5.78 | -3.37 | 14.92 |
| **Summary** | | | | | | | | | | | | |
| Yp → C2 → Yc | 0.169 | 0.12 | 0.22 | 32.11 | 21.84 | 42.37 | 0.125 | 0.05 | 0.20 | 36.15 | 14.57 | 57.73 |
| Yp → Yc | 0.357 | 0.28 | 0.43 | 67.89 | 57.63 | 78.16 | 0.222 | 0.11 | 0.33 | 63.85 | 42.27 | 85.43 |
| Total | 0.526 | 0.47 | 0.58 | 100.00 | 100.00 | 100.00 | 0.347 | 0.23 | 0.47 | 100.00 | 100.00 | 100.00 |

Note: Individual earnings for fathers and sons only ($N = 721$) and family income for all offspring and the head of household in 1989 ($N = 2,021$). All circumstances measured for the head of family in 1989. Homeowner: parent owning a house in 1989. Region where born has 'New England' as the reference category. 'Outside U.S.' category includes U.S. territories. The asset variables (including the use of the Food Stamp programme, renamed SNAP in 2008) takes the value 1 for those parents above the median in 1989 (e.g., by being above the median value of the food stamp benefit or by having above median savings). Confidence interval based on a 1,000 iteration bootstrap, clustered at the parental family level, using random sampling with replacement over the whole estimation and decomposition process.

A pattern that arises from this decomposition (and the next) is that a few circumstances account for most of the share attributed to circumstances. The most relevant circumstance is whether the family had above-median savings in 1989. It accounts for 19% of the IGE for earnings and 25% for income. Family savings – and more generally, wealth and assets – act both as a stock for human capital or other investments as well as a buffer for external shocks such as medical risks (De Nardi and Fella, 2017). Savings also have a direct intergenerational transfer, through bequests and inheritances (Killewald et al., 2017), reinforcing wealth inequalities across generations.

Figure 4.7: IGE decomposition: Mediating circumstances




Note: Individual earnings for fathers and sons only ($N = 721$) and family income for all offspring and the head of household in 1989 ($N = 2,021$).

The only other circumstance with a statistically significant contribution at the 95% level is having above-median investment in stocks, albeit only for earnings immobility. This circumstance accounts for 5% of the IGE for earnings, but less than a percentage point for income. Financial investments can act as a similar buffer as savings, but are more highly concentrated at the top of the distribution.

Another important circumstance is whether families used food stamps (now called Supplemental Nutrition Assistance Program, SNAP) in 1989. It accounts for 4.4% of the IGE for earnings and 5.8% for income, although neither is statistically significant. The high share accounted for by this circumstances reflects that intergenerational persistence happes not only at the top of the distribution (as suggested by the importance of savings and investment) but also at the bottom.

### 4.4.3 Decomposing the IGE: Preceding and mediating circumstances

I expand the previous decomposition by adding circumstances that precede the relationship between parental and offspring's outcomes. As a result, each of the two components discussed in the previous section are divided in two: One component that follows from preceding circumstances, and another component stemming for all other sources of immobility.

As both the sets of preceding and mediating circumstances include a large number of components, I report a summary of the complete decomposition. For each of the $K_1$ circumstance $C_1$, there are two components, one unmediated and another one mediated from $C_2$ that includes $K_2$ different components. The same is true for the component starting from $\Phi$. That means that there are $(1 + K_1) \cdot (1 + K_2)$ specific decomposition components to report (182 components in my case). In Tables 4.4 and 4.5 I report the decomposition estimates only for $C_1$ by adding up the influence of each mediating circumstance. For example, I report the total influence of education over parents being homeowners, over being born in the Mideast, over having used food stamps, and all other mediating circumstances. I present the opposite table – where the influence of each preceding circumstance has been added up – in Table 4.A.1 in the Appendix.

Tables 4.4 and 4.5 report the decomposition for individual earnings and family income, respectively. I also include the 95% confidence interval obtained from a bootstrap with replacement that iterated the whole decomposition process 1,000

Table 4.4: IGE decomposition for individual earnings (All circumstances)

| | Earnings | | | | | |
|---|---|---|---|---|---|---|
| | Coef. | 95% CI | | % of IGE | 95% CI | |
| **Unmediated influence of Φ:** | | | | | | |
| $\Phi \rightarrow$ Yp $\rightarrow$ Yc | 0.157 | 0.04 | 0.28 | 45.27 | 16.21 | 74.34 |
| **Mediated influence of Φ:** | | | | | | |
| $\Phi \rightarrow$ Yp $\rightarrow$ C2 $\rightarrow$ Yc | 0.068 | -0.00 | 0.14 | 19.52 | -0.74 | 39.78 |
| **Unmediated influence of C1: C1 $\rightarrow$ Yp $\rightarrow$ Yc** | | | | | | |
| IQ score | 0.011 | -0.01 | 0.03 | 3.27 | -2.38 | 8.92 |
| Education (years) | 0.063 | 0.01 | 0.12 | 18.03 | 0.59 | 35.47 |
| Ethnicity: Non-white | -0.003 | -0.01 | 0.00 | -0.99 | -3.28 | 1.30 |
| Occup: Manager | 0.009 | -0.01 | 0.03 | 2.64 | -4.03 | 9.31 |
| Occup: Clerical | 0.006 | -0.01 | 0.02 | 1.80 | -2.27 | 5.88 |
| Occup: Craftsman | -0.009 | -0.03 | 0.01 | -2.66 | -9.36 | 4.05 |
| Occup: Operative | 0.004 | -0.03 | 0.03 | 1.26 | -8.28 | 10.80 |
| Occup: Farmer | 0.008 | -0.01 | 0.03 | 2.28 | -3.04 | 7.59 |
| Occup: Services | -0.001 | -0.01 | 0.01 | -0.30 | -2.49 | 1.90 |
| Occup: Other | 0.001 | -0.02 | 0.02 | 0.34 | -4.87 | 5.56 |
| P grew in Small town | 0.001 | -0.01 | 0.01 | 0.33 | -2.57 | 3.22 |
| P grew in Large city | 0.005 | -0.01 | 0.02 | 1.57 | -3.57 | 6.71 |
| P grew in Other | 0.001 | -0.00 | 0.01 | 0.36 | -1.22 | 1.95 |
| **Mediated influence of C1: C1 $\rightarrow$ Yp $\rightarrow$ C2 $\rightarrow$ Yc** | | | | | | |
| IQ score | 0.001 | -0.01 | 0.01 | 0.39 | -1.80 | 2.58 |
| Education (years) | 0.010 | -0.02 | 0.04 | 2.93 | -6.52 | 12.38 |
| Ethnicity: Non-white | 0.001 | -0.00 | 0.01 | 0.43 | -0.78 | 1.64 |
| Occup: Manager | 0.001 | -0.00 | 0.01 | 0.24 | -1.12 | 1.61 |
| Occup: Clerical | 0.001 | -0.00 | 0.00 | 0.31 | -0.58 | 1.20 |
| Occup: Craftsman | 0.002 | -0.01 | 0.01 | 0.67 | -1.66 | 3.00 |
| Occup: Operative | 0.002 | -0.01 | 0.02 | 0.47 | -3.87 | 4.80 |
| Occup: Farmer | 0.001 | -0.00 | 0.01 | 0.30 | -0.88 | 1.47 |
| Occup: Services | 0.000 | -0.00 | 0.00 | 0.10 | -1.19 | 1.38 |
| Occup: Other | 0.003 | -0.00 | 0.01 | 1.00 | -1.52 | 3.53 |
| P grew in Small town | 0.000 | -0.00 | 0.00 | 0.05 | -1.21 | 1.31 |
| P grew in Large city | 0.000 | -0.01 | 0.01 | 0.10 | -1.55 | 1.76 |
| P grew in Other | 0.001 | -0.00 | 0.00 | 0.28 | -0.69 | 1.26 |
| **Summary** | | | | | | |
| $\Phi \rightarrow$ Yp $\rightarrow$ Yc | 0.157 | 0.04 | 0.28 | 45.27 | 16.21 | 74.34 |
| $\Phi \rightarrow$ Yp $\rightarrow$ C2 $\rightarrow$ Yc | 0.068 | -0.00 | 0.14 | 19.52 | -0.74 | 39.78 |
| C1 $\rightarrow$ Yp $\rightarrow$ Yc | 0.097 | 0.03 | 0.17 | 27.94 | 6.93 | 48.94 |
| C1 $\rightarrow$ Yp $\rightarrow$ C2 $\rightarrow$ Yc | 0.025 | -0.01 | 0.06 | 7.27 | -3.09 | 17.63 |
| Sum circumstances | 0.190 | 0.09 | 0.29 | 54.73 | 25.66 | 83.79 |
| Total | 0.347 | 0.23 | 0.47 | 100.00 | 100.00 | 100.00 |

Note: Individual earnings for fathers and sons only ($N = 721$) and family income for all offspring and the head of household in 1989 ($N = 2,021$). The parent's IQ test (0 to 13) was taken by the head of family in 1974. Education is a continuous variable going from 1 to 17 for the parent with the highest education in 1989. All other parental characteristics are for the head of the family in 1989. Parent's ethnicity is a binary variable that takes the value 1 for a person of color (POC) and where the reference category is "White". Occupation of the head of household has "Professional" as reference category. The reference category for where the parent grew up in is "Farm". Confidence interval based on a 1,000 iteration bootstrap, clustered at the parental family level, using random sampling with replacement over the whole estimation and decomposition process.

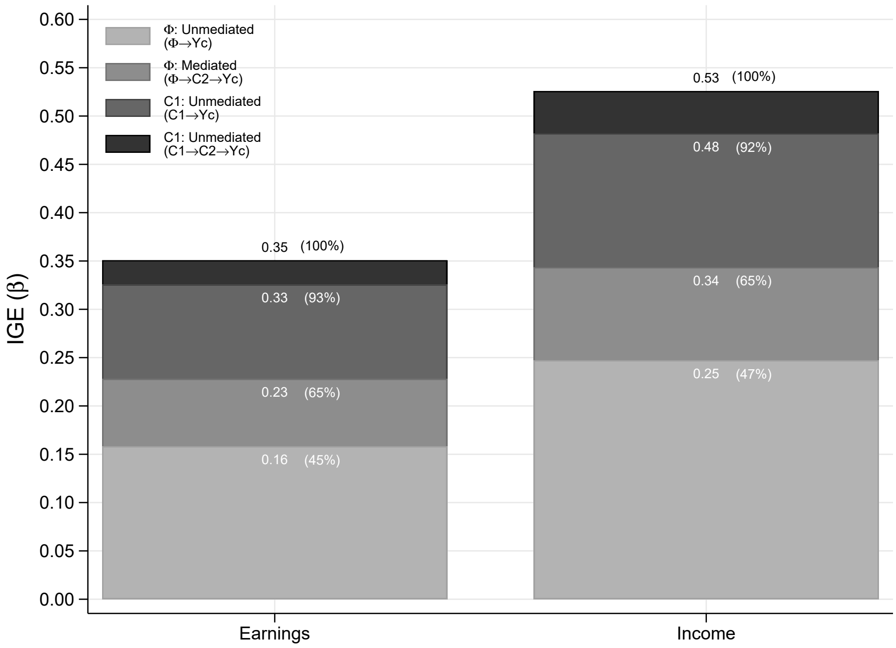
Table 4.5: IGE decomposition for family income (All circumstances)

| | Income | | | | | |
|---|---|---|---|---|---|---|
| | Coef. | 95% CI | | % of IGE | 95% CI | |
| Unmediated influence of $\Phi$: | | | | | | |
| $\Phi \rightarrow$ Yp $\rightarrow$ Yc | 0.247 | 0.17 | 0.32 | 47.03 | 35.14 | 58.92 |
| Mediated influence of $\Phi$: | | | | | | |
| $\Phi \rightarrow$ Yp $\rightarrow$ C2 $\rightarrow$ Yc | 0.096 | 0.05 | 0.14 | 18.31 | 9.96 | 26.67 |
| Unmediated influence of C1: C1 $\rightarrow$ Yp $\rightarrow$ Yc | | | | | | |
| IQ score | 0.019 | -0.00 | 0.04 | 3.62 | -0.42 | 7.65 |
| Education (years) | 0.094 | 0.06 | 0.13 | 17.83 | 11.33 | 24.33 |
| Ethnicity: Non-white | -0.004 | -0.03 | 0.02 | -0.71 | -4.95 | 3.52 |
| Occup: Manager | 0.012 | -0.00 | 0.03 | 2.32 | -0.54 | 5.18 |
| Occup: Clerical | -0.000 | -0.00 | 0.00 | -0.06 | -0.55 | 0.44 |
| Occup: Craftsman | 0.000 | -0.00 | 0.00 | 0.05 | -0.33 | 0.43 |
| Occup: Operative | 0.008 | -0.01 | 0.02 | 1.48 | -1.09 | 4.05 |
| Occup: Farmer | 0.000 | -0.00 | 0.01 | 0.07 | -0.84 | 0.97 |
| Occup: Services | 0.005 | -0.01 | 0.02 | 0.91 | -1.40 | 3.22 |
| Occup: Other | 0.000 | -0.02 | 0.02 | 0.03 | -3.99 | 4.06 |
| P grew in Small town | 0.002 | -0.00 | 0.01 | 0.32 | -0.50 | 1.14 |
| P grew in Large city | 0.002 | -0.00 | 0.01 | 0.39 | -0.62 | 1.39 |
| P grew in Other | 0.000 | -0.00 | 0.00 | 0.09 | -0.40 | 0.58 |
| Mediated influence of C1: C1 $\rightarrow$ Yp $\rightarrow$ C2 $\rightarrow$ Yc | | | | | | |
| IQ score | 0.004 | -0.00 | 0.01 | 0.81 | -0.38 | 2.00 |
| Education (years) | 0.014 | 0.00 | 0.03 | 2.68 | 0.01 | 5.36 |
| Ethnicity: Non-white | 0.007 | 0.00 | 0.01 | 1.38 | 0.04 | 2.73 |
| Occup: Manager | 0.001 | -0.00 | 0.01 | 0.28 | -0.60 | 1.15 |
| Occup: Clerical | 0.000 | -0.00 | 0.00 | 0.04 | -0.12 | 0.19 |
| Occup: Craftsman | -0.000 | -0.00 | 0.00 | -0.03 | -0.21 | 0.14 |
| Occup: Operative | 0.003 | -0.00 | 0.01 | 0.58 | -0.34 | 1.51 |
| Occup: Farmer | 0.001 | -0.00 | 0.00 | 0.23 | -0.11 | 0.58 |
| Occup: Services | 0.002 | -0.00 | 0.01 | 0.43 | -0.38 | 1.24 |
| Occup: Other | 0.011 | 0.00 | 0.02 | 2.03 | -0.01 | 4.06 |
| P grew in Small town | -0.000 | -0.00 | 0.00 | -0.01 | -0.31 | 0.30 |
| P grew in Large city | -0.000 | -0.00 | 0.00 | -0.09 | -0.45 | 0.27 |
| P grew in Other | -0.000 | -0.00 | 0.00 | -0.02 | -0.20 | 0.15 |
| **Summary** | | | | | | |
| $\Phi \rightarrow$ Yp $\rightarrow$ Yc | 0.247 | 0.17 | 0.32 | 47.03 | 35.14 | 58.92 |
| $\Phi \rightarrow$ Yp $\rightarrow$ C2 $\rightarrow$ Yc | 0.096 | 0.05 | 0.14 | 18.31 | 9.96 | 26.67 |
| C1 $\rightarrow$ Yp $\rightarrow$ Yc | 0.139 | 0.10 | 0.18 | 26.34 | 18.22 | 34.47 |
| C1 $\rightarrow$ Yp $\rightarrow$ C2 $\rightarrow$ Yc | 0.044 | 0.02 | 0.06 | 8.31 | 4.24 | 12.38 |
| Sum circumstances | 0.279 | 0.22 | 0.34 | 52.97 | 41.08 | 64.86 |
| Total | 0.526 | 0.47 | 0.58 | 100.00 | 100.00 | 100.00 |

See note in Table 4.4.

times, clustered at the parental family level. After controlling for preceding and mediating circumstances, the coefficient of the logarithm of individual earnings of the parent ($lnY^P$) goes from 0.35 to 0.16, while the coefficient for the logarithm of parental family income goes from 0.53 to 0.25 (see columns 3 and 6 of Table 4.A.5 in the appendix). Overall, circumstances account for 55% of the IGE of earnings and 53% for the IGE of income. Figure 4.8 summarises the decomposition.

Figure 4.8: IGE decomposition: All circumstances




Note: Individual earnings for fathers and sons only ($N = 721$) and family income for all offspring and the head of household in 1989 ($N = 2,021$).

After including preceding circumstances, the share accounted for by circumstances increases from 32% to 55% for earnings and from 36% to 53% for income. By looking at equation 4.11, we know that this increment is accounted for by the 'direct' (or unmediated) influence of preceding circumstances ($C_1 \to Y^P \to Y^C$). A part of the unmediated influence of parental income is now determined by preceding circumstances. This decomposition shows that this influence is substantial, and account for a part of the IGE that goes above and beyond the influence of mediating circumstances.

Among the three components that comprise the influence of circumstances, the largest one is the unmediated influence of preceding circumstances ($C_1 \rightarrow Y^P \rightarrow Y^C$), accounting for around 27% of the IGE in both cases. The second largest component is the mediated influence of non-circumstance factors ($NC \rightarrow Y^P \rightarrow C_2 \rightarrow Y^C$), accounting for almost 20% of the IGE. The third component, the mediated influence of preceding circumstances ($C_1 \rightarrow Y^P \rightarrow C_2 \rightarrow Y^C$ or) accounts for around 8% of the IGE. This decomposition indicates that both preceding and mediating circumstances account for an important share of the IGE, but there is little interaction between the two. Preceding circumstances have a direct influence on offspring's outcomes, and mediating circumstances play an important role in the relationship between non-circumstance factors and offspring's income, but preceding circumstances have a very weak association with mediating circumstances.

Among preceding circumstances, parental education accounts for the largest share of the IGE, accounting for around 21% of the IGE in total by adding up its mediated and unmediated influence. Most of this influence is unmediated: parental education does influences the income of the offspring through factors outside of mediating circumstances. For example, parental education influences choices of the offspring later in life, such as their occupation or type of job, which are strong predictors of their income.

Other preceding circumstances with an unmediated influence include the IQ score of the head of household in 1972 ( around 3.5% of the IGE) and whether the father worked as a manager in 1989 (around 2.5% of the IGE). On the other hand, the ethnicity of the parent reports a mediated influence, particularly for family income (1.4%). Unfortunately none of these circumstances are statistically significant at the 95%, so that the sample size does not allow me to draw robust conclusions from these circumstances.

### Contribution of mediating circumstances

Table 4.A.1 in the Appendix reports the same decomposition – including preceding and mediating circumstances – but for detailing the latter. I have already mentioned that preceding and mediating circumstances report very little interaction in accounting for the IGE. Mediating circumstances matter when explaining the influence of factors other than preceding circumstances ($\Phi$).

Consistent with the previous section, the most important circumstances relate to the holding and lack of wealth and assets. Families holding above-median savings in 1989 account for 13% of the IGE for individual earnings and 10% for family income. Families receiving food assistance in 1989 account for 6% of the IGE for individual earnings and 4% for family income. Similarly, parents being homeowners account for around 3% of the IGE for both outcomes. Overall, the relative contribution of mediating circumstances is fairly similar for both outcomes.

Table 4.A.1 highlights how factors that might not be considered circumstances – and thus included in $\Phi$ – can contribute to IOp. One common example of such a factor are the instillation of preferences on children (Roemer, 2004; Dardanoni et al., 2006). However, the same factors that drive our intention to instill preferences can have an influence on other aspects of the intergenerational transmission process. Say we want to instil the importance of saving to our children, that same interest could drive our own intentions and capacity to accumulate savings. Even though we consider a driver of intergenerational persistence as legitimate, that driver can also have an influence of other factors we consider illegitimate.

## 4.4.4 The direct and indirect influence of preceding circumstances

In this section I go beyond the decomposition of the IGE to account the full contribution of preceding circumstances. From Figure 4.5 we see that $C_1$ can influence the income of the offspring 'directly', that is, outside of its contribution to parental income. This contribution does not contribute to the IGE, which focuses solely on the relationship between parent and offspring income.

From an IOp of view, we are interested in the full influence of circumstances. In most cases, that includes their influence on efforts, which is why most papers estimate a reduced-form equation similar to equation 4.9 (see e.g., Ferreira and Gignoux (2011)). Therefore, a measure of IOp does not only account for the influence of parental income, but also for the influence of all other circumstances. The extent to which these other circumstances influence the income of the offspring can help understand the relationship between the IGE and IOp.

Table 4.6 reports the decomposition into a direct and an indirect component, as shown in equation 4.17. The indirect component comprises the influence of each preceding circumstance on parental income, which in turn influences offspring income, and thus on the IGE. The direct component is the influence of each preceding circumstance on offspring income, not accounted for in the IGE. I report the decomposition for both earnings and for income (Table 4.A.2 in the Appendix includes the bootstrapped confidence intervals of these estimates).

To provide a measure of the 'relevance' of each circumstance, I include the R-squared of an OLS regression of that circumstance on the income of the offspring. Consistent with the IGE decomposition, parental education is the most relevant circumstance under this metric. Other relevant circumstances (although much less so than education) are the IQ score of the parent, the ethnicity of the parent (only for income), and some parent's occupations, namely being a professional, a manager, or an operative. Almost all of these circumstances report statistically significant estimates at the 95% level (see Table 4.A.2 in the Appendix).

The influence of parental education – the circumstance with the highest R-squared – is mostly direct. For earnings, 64% of the contribution of education is associated with its direct contribution (55% for income). Even though parental education accounts for a large share of the IGE, most of its influence on the income of the

Table 4.6: Influence of preceding circumstances not in the IGE (% share)

| | Earnings | | | Income | | |
|---|---|---|---|---|---|---|
| | Direct (non-IGE) | Indirect (IGE) | $R^2$ | Direct (non-IGE) | Indirect (IGE) | $R^2$ |
| IQ score | 52.2 | 47.8 | 3.8 | 41.5 | 58.5 | 7.6 |
| Education (years) | 64.2 | 35.8 | 12.3 | 55.1 | 44.9 | 19.4 |
| Ethnicity: Non-white | 30.4 | 69.6 | 1.7 | 20.6 | 79.4 | 4.7 |
| Occup: Professional | 48.1 | 51.9 | 2.3 | 42.5 | 57.5 | 3.8 |
| Occup: Manager | 63.3 | 36.7 | 2.8 | 44.6 | 55.4 | 4.1 |
| Occup: Clerical | . | . | 0.0 | 28.6 | 71.4 | 0.0 |
| Occup: Craftsman | -30.5 | 130.5 | 0.0 | 143.8 | -43.8 | 0.0 |
| Occup: Operative | 68.4 | 31.6 | 3.2 | 55.0 | 45.0 | 3.9 |
| Occup: Farmer | 51.3 | 48.7 | 0.7 | 26.6 | 73.4 | 0.6 |
| Occup: Services | 54.2 | 45.8 | 1.6 | 36.1 | 63.9 | 1.6 |
| Occup: Other | 25.0 | 75.0 | 1.7 | 18.3 | 81.7 | 3.5 |
| P grew: in Farm | 43.1 | 56.9 | 1.5 | 47.6 | 52.4 | 1.2 |
| P grew in Small town | -17.0 | 117.0 | 0.1 | 48.4 | 51.6 | 0.2 |
| P grew in Large city | 67.6 | 32.4 | 0.3 | 43.6 | 56.4 | 0.2 |
| P grew in Other | 84.0 | 16.0 | 0.1 | 35.7 | 64.3 | 0.0 |

Note: Individual earnings for fathers and sons only ($N = 721$) and family income for all offspring and the head of household in 1989 ($N = 2,021$). The parent's IQ test (0 to 13) was taken by the head of family in 1974. Education is a continuous variable going from 1 to 17 for the parent with the highest education in 1989. All other parental characteristics are for the head of the family in 1989. Parent's ethnicity is a binary variable that takes the value 1 for a person of color (POC) and where the reference category is "White". Missing values reflect shares below -1000% or above 1000%. $R^2$ is the R-squared of the OLS regression of that circumstance ($C_{1j}$) on the income of the offspring ($Y^C$).

offspring is not part of the IGE. The education of the parent has is a strong determinant of inequality of opportunity, both due to its influence on the income of the parent and of the offspring.

Contrary to parental education, the ethnicity of the parent acts mostly as an indirect phenomenon, albeit with a much smaller R-squared. 70% to 80% of its influence on the income of the offspring is accounted for in the IGE. This means that the ethnicity of the parent influences intergenerational persistence in income mostly through its influence on the income of the parent.

The rest of the circumstances show a mixed picture, depending on the outcome. The IQ of the head of household in 1972 is split halfway for earnings (52% vs. 48%) but the indirect influence is stronger for income, accounting for 59% of its total influence. Having parents with a professional occupation has a similar decomposition than that of parental IQ. Having parents with an operative occupation, on the other hand, report a mostly indirect influence (68% for earnings and 55% for income).

The two most important circumstances in term of the R-squared, parental education and IQ score, report an important indirect effect. Only in the case of the IQ score for income we see a higher direct influence. Despite these circumstances accounting for a large share of the IGE, most of their influence lies outside of the relationship between parental and offspring income.

### 4.4.5 Robustness checks and extensions

#### The IGE decomposition

The main assumption in this decomposition approach is that parental characteristics can be interpreted as circumstances as they are measured when the offspring was at most 20 years of age. That restriction imposes a trade-off between sample size and the cut-off age. For that reason, I re-estimate the decomposition for two additional samples based on two different cut-offs: 18 and 22 years of age – roughly speaking, at the end of secondary education and the end of post secondary education, respectively. The 18 years of age cut-off reinforces the idea that circumstances should be measured when the offspring was young, while the 22 years of age cut-off allows for a larger sample while still falling within a reasonable 'responsibility threshold'.

I also explore the minimum number of years used to average earnings and income. My decomposition restricts the sample to individuals with at least 6 years of data (and with a maximum of 9 years). As a robustness check, I re-estimate the decomposition by including individuals with 5, 4, and 3 years of outcome data. Given that most respondents have 9 years of data, the increment in the sample size of including additional individuals is limited. Nonetheless, I present the results of both robustness checks in Table 4.A.3 in the Appendix.

I first compare the different age cut-offs for the sample of individuals with 6 to 9 years of data, shown in the last rows of Table 4.A.3. Columns 3 and 4 ("20 or younger in 1989") report the benchmark findings for the sample of offspring who were at most 20 years of age in 1989. There is a slight increase in the IGE for earnings the older the sample, going from 0.33 to those 18 or younger in 1989 to 0.37 for those 22 or younger 1989. However, the share accounted for by circumstances remains relatively unchanged and around 54%. For income, the IGE almost does not change for the sub 18, sub 20, or sub 22 samples. There is a slight decrease in the share accounted for by circumstances in the first sample, falling from around 53% to 50%. Overall, the change in the age cut-off when the offspring was young makes a small difference in the IGE decomposition for earnings and almost no difference for income.

Including individuals with less than 6 years of data makes almost no difference for the IGE estimates. As expected, the IGE decreases slightly (1 percentage point) when including individuals with 3 years of data, as outcome measures are less precise hence reducing the association between parents and offspring. For income, the inclusion of individuals with fewer years of data does not change the share accounted for by circumstances, but it does increase for earnings. Circumstances account for up to 8 more percentage points (from 55% to 63% for the sub-20 sample) when including individuals with 3 years of data. One explanation could be the smaller size of the earnings sample. However, these changes fall well within the confidence intervals of the earnings decomposition (see Table 4.4 in the Appendix).

### The total influence of preceding circumstances

In this section I re-estimate Table 4.6 with the sub-18 and sub-22 years of age samples. Results are shown in Table 4.A.4 in the appendix. The different age cutoffs make little to no difference in the direct/indirect decomposition. The direct influence of parental education lies between 62% to 64% for earnings and 53 to 57% for income. The direct influence of the IQ score lies between 52% to 56% for earnings and 39% to 46% for income. Overall, and similarly to the previous subsection, these changes fall well within the confidence intervals of the benchmark decomposition (see Table 4.A.2 in the Appendix).

### Non-linear decomposition: A quantile regression approach

As a final extension, I explore the existence of non-linear effects. In a recent paper, Palomino et al. (2018) studies the how the IGE changes across the income distribution, finding that the IGE is highest at the bottom of the distribution. Following their approach, I re-estimate my decomposition using quantile regressions for different percentiles of the income distribution. I focus on family income as an outcome, because the small sample size for earnings does not allow for a proper quantile analysis.

I present two results. First, I report the share of the IGE accounted by circumstances (i.e., the components associated to circumstances in equation 4.14). That is, the total contribution of circumstances to the IGE. Second, I focus solely on the most relevant circumstance – parental education – and study its direct influence (i.e., the influence not passing through parental income in equation 4.17). For each I also report the 95% confidence interval.

Concretely, Figure 4.A.2 in the Appendix reports the share of the IGE not attributed to parental income. Given equations 4.7 to 4.9, this share is represented by $1 - (\hat{\theta}_2/\hat{\beta})$, where the 'hat' represents the OLS estimate.

Similarly, Figure 4.A.3 in the Appendix reports the share of the total contribution of parental education not accounted for by the IGE. If we call $\omega$ the regression coefficient of parental income on the income of the offspring, then this share is represented by $\left(\hat{\rho}_{2j} + \hat{\theta}_2\hat{\kappa}_j\right)/\hat{\omega}$.

The share of the IGE accounted for by all circumstances is be higher around the third decile and at the top of the distribution. However, the overall distribution appears to be homogeneous around the average. As the confidence intervals for these estimations are quite large – due to to the small sample size – no point departure from the average is statistically significant (Palomino et al. (2018) uses a sample of over 25 thousand observations for this exercise).

The direct contribution of parental education is smaller at the bottom of the distribution. This finding is consistent with Palomino et al. (2018), who find that the mediating share of education (i.e., its indirect influence) is higher at the bottom of the distribution. Nonetheless, the confidence intervals are too large to say anything substantial about the distribution.

## 4.5 Discussion

In this chapter I study the relationship between estimates of the IGE and of IOp. I model the interaction between the income of the parent, the income of their offspring, and other childhood circumstances to understand to what extent can we consider the IGE to be a measure of IOp. My model proposes two main decompositions: One to determine how much much of the IGE can be explained by differences in these circumstances, and a second one to determine how much of the total contribution of circumstances is not included in the IGE.

My IGE estimates are constructed to be as consistent as possible with previous estimates. Using 2017 PSID data, the IGE for individual earnings – estimated only for fathers and sons – is 0.35, whereas the IGE for total family income is 0.53. Circumstances account for just over half of the IGE in both cases, with parental education and families having above-median savings being the most important circumstances. Conversely, around 45% of the IGE is not accounted for by circumstances. As a first approximation, this decomposition suggests that an 'optimal' IGE should be at least half of its current level in the USA.

This first decomposition is further split into two, to account for circumstances that mediate the relationship between parents and offspring, and circumstances that precede it. The decomposition reports that both mediating and preceding circumstances matter for the IGE, but that there is little interaction between the two. Mediating circumstances account for around 20% of the IGE while preceding circumstances account for over 25%. The combination of the two – the mediated influence of preceding circumstances – explains only around 8% of the IGE.

Contrary to the IGE, an IOp estimate not only accounts for the full influence of parental income but also also for the influence of other circumstances. For that reason my second decompositions focuses on the extent to which the influence of preceding circumstances is not accounted for in the IGE. I decompose the contribution of each preceding circumstances to the income of the offspring into a 'direct' (i.e., not mediated by parental income) and an 'indirect' (i.e., mediated by parental income and thus accounted for in the IGE) component.

The most relevant preceding circumstance (in terms of its contribution to offspring income and to the IGE) is parental education. Around two-thirds of its influence on offspring earnings is not mediated by parental earnings (58% for income). This decomposition suggests that ff we consider parental education as a circumstance (as is often the case in IOp studies), then the IGE is an insufficient measure of IOp, as the influence of other of other important circumstances is not wholly accounted for by parental income.

One important caveat of this analysis has to do with omitted or unobserved circumstances. My first decomposition accounts for the influence of observed circumstances. Due to the residual nature of this approach, all omitted circumstances contribute to the 'unexplained' part of the IGE (the part stemming from $\Phi$). This problem is common in the IOp literature, and results in 'lower bound' estimates of IOp (Ferreira and Gignoux, 2011). Similarly, the estimates on the first decomposition should be interpreted as lower bounds of the share of the IGE attributable to circumstances. Given the 'correlational' interpretation of these results, the inclusion of additional circumstances will change the decomposition to the extent that they do not correlate with currently observed circumstances. As Hufe et al. (2017) did it for IOp estimates, future research could explore the contribution of additional circumstances to explain this decomposition.

Childhood circumstances account for most – but not all– of the IGE, and they also have an influence outside of the relationship between parent's and offspring's income. Overall, the influence of circumstances on the income of the offspring is substantial, and the IGE captures only a part of it. Conversely, not all the IGE is accounted for by circumstances.

What about the use of the IGE as a measure of IOp? Both decompositions help in clarifying this relationship. As Roemer (2004) puts it, complete intergenerational mobility – more precisely, origin independence – implies equality of opportunity under two conditions. First, if we follow the strongest definition of IOp, where natural or inborn talent is a circumstance (what Swift (2013) calls the 'radical' view of IOp). Second, if the influence of the parental background is summarised in its entirety by parental income. My first decomposition (the share of the IGE attributed to circumstances) relates to the former condition, while my second decomposition (the influence of circumstances excluded from the IGE) relates to the latter.

The first condition presents a normative choice and it shapes how we interpret the part of the IGE not attributed to circumstances. My choice of circumstances is closer to the 'conventional' view of IOp, which accounts for the influence of discrimination and socioeconomic background. As it stands, a radical view of IOp would suffer from several omitted circumstances, namely measures of the offspring's 'ability' at birth. While the discussion on views of IOp is outside the scope of this chapter, I show that even with a 'conventional' view of IOp, more than half of the IGE is attributable to differences in circumstances.

Unlike the first condition, the second condition can be empirically verified for each of the observed circumstances. Under this condition, parental income is a summary measure that accounts for all potential circumstances. My decomposition shows that, but for one case, no circumstance is entirely accounted through the IGE. Parental income does not summarise all potential circumstances, it is one of many relevant circumstances. Following the two condition of Roemer (2004), an IGE of zero does not imply an IOp index of zero, due to there being other factors besides parental income than influence the latter but not the former.[10]

This does not mean than IGE estimates are poor proxies of IOp. I show that circumstances are an important part of the IGE, and that the IGE accounts for part of their influence. The similarities in functional form also highlight the similarities between the two. In a cross-country context, Brunori et al. (2013) show a positive correlation between indices of relative IOp and intergenerational correlations in income and education. In this chapter, however, I focus on unpacking the IGE to explore why this correlation is not perfect.

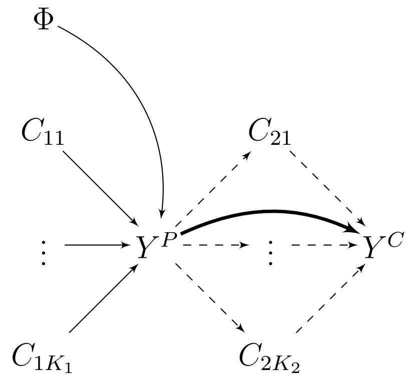
Further research is needed to better understand the determinants and paths behind intergenerational persistence. Other countries, outcomes, and time periods should be explored to understand the role context plays in this decomposition. One such example is wealth transmission where the most important determinants happen later in life, such as receiving inheritances or inter vivos transfers, rather than

[10]Indeed, Roemer (2004) argues that parental education – the most relevant circumstance in my decomposition – is a better proxy for the influence of parents on their offspring.

while growing up (Nolan et al., 2021). Similarly, more complex structural models such as the ones proposed in Haveman and Wolfe (1995) can help understand the role and timing of different factors. These extensions will most likely depart from the notion of 'circumstances' (or perhaps move towards more demanding views of IOp), but will help in understanding what is accounted for in measures like the IGE.

# 4.A Appendix

Figure 4.A.1: Channels of transmission including two factors (Extended)




Note: Extended version of Figure 4.3 with each one of the $K_1$ circumstances in $C_1$ and the $K_2$ circumstances in $C_2$. Circumstances in each vector do not influence other circumstances within the same vector. Circumstance in $C_1$ influence every elements of $C_2$. The dashed lines represent the mediated components (that pass through $C_2$). The bold line between $Y^P$ and $Y^C$ represents the unmediated components.

Table 4.A.1: IGE decomposition (mediating circumstances)

| | Earnings | | | | | | Income | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Coef. | 95% CI | | % of IGE | 95% CI | | Coef. | 95% CI | | % of IGE | 95% CI | |
| $\Phi \rightarrow$ Yp $\rightarrow$ Yc | 0.157 | 0.038 | 0.276 | 45.27 | 16.21 | 74.34 | 0.247 | 0.172 | 0.323 | 47.03 | 35.14 | 58.92 |
| $\Phi \rightarrow$ Yp $\rightarrow$ C2 $\rightarrow$ Yc | | | | | | | | | | | | |
| Homeowner | 0.011 | -0.020 | 0.043 | 3.24 | -6.23 | 12.72 | 0.016 | -0.010 | 0.042 | 3.07 | -1.91 | 8.06 |
| Region: Mideast | 0.007 | -0.013 | 0.027 | 2.02 | -4.01 | 8.05 | 0.000 | -0.005 | 0.006 | 0.04 | -1.00 | 1.09 |
| Region: Great Lakes | 0.000 | -0.013 | 0.014 | 0.07 | -4.18 | 4.31 | -0.001 | -0.007 | 0.005 | -0.20 | -1.33 | 0.93 |
| Region: Plains | 0.002 | -0.026 | 0.030 | 0.57 | -8.23 | 9.37 | 0.001 | -0.010 | 0.012 | 0.18 | -1.87 | 2.23 |
| Region: Southeast | 0.000 | -0.011 | 0.012 | 0.06 | -3.41 | 3.52 | 0.002 | -0.006 | 0.011 | 0.45 | -1.13 | 2.04 |
| Region: Southwest | 0.000 | -0.015 | 0.016 | 0.11 | -4.78 | 5.00 | 0.000 | -0.006 | 0.007 | 0.07 | -1.21 | 1.35 |
| Region: Rocky Mount. | 0.003 | -0.012 | 0.017 | 0.74 | -3.68 | 5.16 | 0.002 | -0.005 | 0.010 | 0.44 | -1.05 | 1.93 |
| Region: Far West | -0.007 | -0.035 | 0.020 | -2.15 | -12.45 | 8.14 | -0.003 | -0.014 | 0.009 | -0.53 | -2.73 | 1.66 |
| Region: Outside U.S.A. | -0.009 | -0.031 | 0.013 | -2.71 | -10.17 | 4.75 | -0.003 | -0.008 | 0.003 | -0.48 | -1.48 | 0.51 |
| Region: No Answer | 0.000 | -0.001 | 0.001 | 0.01 | -0.31 | 0.32 | -0.000 | -0.001 | 0.001 | -0.00 | -0.21 | 0.21 |
| Over median: Business | -0.000 | -0.007 | 0.007 | -0.03 | -2.06 | 2.00 | -0.002 | -0.011 | 0.008 | -0.29 | -2.14 | 1.57 |
| Over median: Stocks | -0.005 | -0.035 | 0.025 | -1.48 | -10.96 | 7.99 | 0.012 | -0.004 | 0.029 | 2.29 | -0.84 | 5.42 |
| Over median: Savings | 0.044 | 0.010 | 0.079 | 12.78 | 2.11 | 23.46 | 0.051 | 0.029 | 0.074 | 9.74 | 5.39 | 14.09 |
| Used food stamps | 0.022 | -0.008 | 0.052 | 6.31 | -3.62 | 16.24 | 0.019 | -0.004 | 0.042 | 3.53 | -0.90 | 7.96 |
| C1 $\rightarrow$ Yp $\rightarrow$ Yc | 0.097 | 0.026 | 0.168 | 27.94 | 6.93 | 48.94 | 0.139 | 0.095 | 0.182 | 26.34 | 18.22 | 34.47 |
| C1 $\rightarrow$ Yp $\rightarrow$ C2 $\rightarrow$ Yc | | | | | | | | | | | | |
| Homeowner | 0.000 | -0.004 | 0.004 | 0.08 | -1.08 | 1.24 | 0.001 | -0.002 | 0.005 | 0.25 | -0.45 | 0.95 |
| Region: Mideast | 0.002 | -0.008 | 0.011 | 0.53 | -2.45 | 3.50 | 0.000 | -0.003 | 0.003 | 0.01 | -0.61 | 0.63 |
| Region: Great Lakes | -0.000 | -0.014 | 0.014 | -0.14 | -4.45 | 4.18 | 0.001 | -0.004 | 0.007 | 0.28 | -0.83 | 1.39 |
| Region: Plains | 0.000 | -0.005 | 0.006 | 0.06 | -1.56 | 1.68 | -0.001 | -0.008 | 0.007 | -0.12 | -1.50 | 1.25 |
| Region: Southeast | 0.000 | -0.014 | 0.015 | 0.14 | -4.33 | 4.61 | 0.007 | -0.005 | 0.018 | 1.30 | -0.97 | 3.56 |
| Region: Southwest | 0.000 | -0.008 | 0.008 | 0.05 | -2.38 | 2.49 | -0.003 | -0.008 | 0.003 | -0.48 | -1.54 | 0.57 |

| | | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Region: Rocky Mount. | -0.002 | -0.013 | 0.009 | -0.58 | -3.80 | 2.64 | -0.002 | -0.009 | 0.005 | -0.41 | -1.76 | 0.94 |
| Region: Far West | 0.003 | -0.009 | 0.014 | 0.73 | -3.07 | 4.52 | 0.001 | -0.004 | 0.007 | 0.23 | -0.81 | 1.26 |
| Region: Outside U.S.A. | 0.005 | -0.004 | 0.013 | 1.36 | -1.39 | 4.11 | 0.003 | -0.002 | 0.007 | 0.53 | -0.35 | 1.41 |
| Region: No Answer | -0.000 | -0.001 | 0.001 | -0.01 | -0.22 | 0.21 | -0.000 | -0.001 | 0.001 | -0.01 | -0.13 | 0.11 |
| Over median: Business | -0.001 | -0.008 | 0.007 | -0.22 | -2.40 | 1.96 | -0.001 | -0.005 | 0.004 | -0.13 | -1.01 | 0.75 |
| Over median: Stocks | -0.002 | -0.016 | 0.011 | -0.67 | -4.95 | 3.61 | 0.004 | -0.002 | 0.010 | 0.81 | -0.38 | 1.99 |
| Over median: Savings | 0.020 | 0.002 | 0.039 | 5.89 | -0.34 | 12.12 | 0.025 | 0.012 | 0.037 | 4.66 | 2.26 | 7.06 |
| Used food stamps | 0.000 | -0.004 | 0.005 | 0.06 | -1.44 | 1.56 | 0.007 | -0.003 | 0.018 | 1.40 | -0.57 | 3.37 |
| **Summary** | | | | | | | | | | | | |
| $\Phi \rightarrow$ Yp $\rightarrow$ Yc | 0.157 | 0.038 | 0.276 | 45.27 | 16.21 | 74.34 | 0.247 | 0.172 | 0.323 | 47.03 | 35.14 | 58.92 |
| $\Phi \rightarrow$ Yp $\rightarrow$ C2 $\rightarrow$ Yc | 0.068 | -0.002 | 0.137 | 19.52 | -0.74 | 39.78 | 0.096 | 0.053 | 0.139 | 18.31 | 9.96 | 26.67 |
| C1 $\rightarrow$ Yp $\rightarrow$ Yc | 0.097 | 0.026 | 0.168 | 27.94 | 6.93 | 48.94 | 0.139 | 0.095 | 0.182 | 26.34 | 18.22 | 34.47 |
| C1 $\rightarrow$ Yp $\rightarrow$ C2 $\rightarrow$ Yc | 0.025 | -0.006 | 0.057 | 7.27 | -3.09 | 17.63 | 0.044 | 0.023 | 0.065 | 8.31 | 4.24 | 12.38 |
| Sum circumstances | 0.190 | 0.086 | 0.294 | 54.73 | 25.66 | 83.79 | 0.279 | 0.218 | 0.339 | 52.97 | 41.08 | 64.86 |
| Total | 0.347 | 0.225 | 0.469 | 100.00 | 100.00 | 100.00 | 0.526 | 0.469 | 0.583 | 100.00 | 100.00 | 100.00 |

Note: Individual earnings for fathers and sons only ($N = 721$) and family income for all offspring and the head of household in 1989 ($N = 2,021$). All circumstances measured for the head of family in 1989. Homeowner: parent owning a house in 1989. Region where born has 'New England' as the reference category. 'Outside U.S.' category includes U.S. territories. The asset variables (including the use of the Food Stamp programme, renamed SNAP in 2008) takes the value 1 for those parents above the median in 1989 (e.g., by being above the median value of the food stamp benefit or by having above median savings). Confidence interval based on a 1,000 iteration bootstrap, clustered at the parental family level, using random sampling with replacement over the whole estimation and decomposition process.

Table 4.A.2: Bootstrap – Contribution of preceding circumstances (% share)

| | Earnings | | | | | | Income | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Direct | | | Indirect | | | Direct | | | Indirect | | |
| | Coef. | 95% CI | | Coef. | 95% CI | | Coef. | 95% CI | | Coef. | 95% CI | |
| IQ score | 52.2 | 30.4 | 74.1 | 47.8 | 25.9 | 69.6 | 41.5 | 28.0 | 55.0 | 58.5 | 45.0 | 72.0 |
| Education (years) | 64.2 | 49.7 | 78.7 | 35.8 | 21.3 | 50.3 | 55.1 | 45.7 | 64.5 | 44.9 | 35.5 | 54.3 |
| Ethnicity: Non-white | 30.4 | -108.8 | 169.7 | 69.6 | -69.7 | 208.8 | 20.6 | -6.4 | 47.7 | 79.4 | 52.3 | 106.4 |
| Occup: Professional | 48.1 | -60.7 | 156.9 | 51.9 | -56.9 | 160.7 | 42.5 | 22.1 | 62.9 | 57.5 | 37.1 | 77.9 |
| Occup: Manager | 63.3 | 39.6 | 87.1 | 36.7 | 12.9 | 60.4 | 44.6 | 29.0 | 60.1 | 55.4 | 39.9 | 71.0 |
| Occup: Clerical | 27343.0 | 25704.4 | 28981.6 | -27243.0 | -28881.6 | -25604.4 | 28.6 | -2378.4 | 2435.5 | 71.4 | -2335.5 | 2478.4 |
| Occup: Craftsman | -30.5 | -3568.9 | 3507.9 | 130.5 | -3407.9 | 3668.9 | 143.8 | -1851.2 | 2138.7 | -43.8 | -2038.7 | 1951.2 |
| Occup: Operative | 68.4 | 50.6 | 86.2 | 31.6 | 13.8 | 49.4 | 55.0 | 38.1 | 72.0 | 45.0 | 28.0 | 61.9 |
| Occup: Farmer | 51.3 | -58.4 | 161.0 | 48.7 | -61.0 | 158.4 | 26.6 | -571.7 | 624.8 | 73.4 | -524.8 | 671.7 |
| Occup: Services | 54.2 | -328.4 | 436.7 | 45.8 | -336.7 | 428.4 | 36.1 | -26.8 | 99.0 | 63.9 | 1.0 | 126.8 |
| Occup: Other | 25.0 | -29.5 | 79.6 | 75.0 | 20.4 | 129.5 | 18.3 | -6.4 | 43.0 | 81.7 | 57.0 | 106.4 |
| P grew: in Farm | 43.1 | -1188.3 | 1274.6 | 56.9 | -1174.6 | 1288.3 | 47.6 | -26.9 | 122.1 | 52.4 | -22.1 | 126.9 |
| P grew in Small town | -17.0 | -63161.5 | 63127.5 | 117.0 | -63027.5 | 63261.5 | 48.4 | -94081.1 | 94178.0 | 51.6 | -94078.0 | 94181.1 |
| P grew in Large city | 67.6 | -1427.7 | 1563.0 | 32.4 | -1463.0 | 1527.7 | 43.6 | -2410.2 | 2497.4 | 56.4 | -2397.4 | 2510.2 |
| P grew in Other | 84.0 | -5071.7 | 5239.6 | 16.0 | -5139.6 | 5171.7 | 35.7 | -2080.8 | 2152.1 | 64.3 | -2052.1 | 2180.8 |

Note: Confidence intervals of Table 4.6. Family income for all offspring and the head of household in 1989 ($N = 2,021$). The parent's IQ test (0 to 13) was taken by the head of family in 1974. Education is a continuous variable going from 1 to 17 for the parent with the highest education in 1989. All other parental characteristics are for the head of the family in 1989. Parent's ethnicity is a binary variable that takes the value 1 for a person of color (POC) and where the reference category is "White". Confidence interval based on a 1,000 iteration bootstrap, clustered at the parental family level, using random sampling with replacement over the whole estimation and decomposition process.

Table 4.A.3: Robustness check – Outcome averages and age cutoffs

| | 18 or younger in 1989 | | | | 20 or younger in 1989 | | | | 22 or younger in 1989 | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Earnings | | Income | | Earnings | | Income | | Earnings | | Income | |
| | Coef. | Share | Coef. | Share | Coef. | Share | Coef. | Share | Coef. | Share | Coef. | Share |
| 3–9 years average | | | | | | | | | | | | |
| $\Phi \rightarrow$ Yc | 0.11 | 35.8 | 0.26 | 49.9 | 0.13 | 37.1 | 0.24 | 46.6 | 0.15 | 41.9 | 0.25 | 47.5 |
| $\Phi \rightarrow$ C2 $\rightarrow$ Yc | 0.08 | 24.3 | 0.08 | 15.7 | 0.08 | 23.3 | 0.09 | 18.1 | 0.07 | 19.4 | 0.09 | 16.9 |
| C1 $\rightarrow$ Yc | 0.11 | 33.2 | 0.14 | 27.2 | 0.10 | 30.8 | 0.14 | 26.7 | 0.11 | 32.1 | 0.15 | 28.1 |
| C1 $\rightarrow$ C2 $\rightarrow$ Yc | 0.02 | 6.7 | 0.04 | 7.2 | 0.03 | 8.7 | 0.04 | 8.5 | 0.02 | 6.7 | 0.04 | 7.4 |
| Circumstances | 0.21 | 64.2 | 0.26 | 50.1 | 0.21 | 62.9 | 0.28 | 53.4 | 0.20 | 58.1 | 0.27 | 52.5 |
| Total | 0.32 | 100.0 | 0.53 | 100.0 | 0.34 | 100.0 | 0.52 | 100.0 | 0.35 | 100.0 | 0.52 | 100.0 |
| 4–9 years average | | | | | | | | | | | | |
| $\Phi \rightarrow$ Yc | 0.13 | 39.3 | 0.27 | 50.4 | 0.14 | 40.0 | 0.25 | 46.9 | 0.16 | 44.0 | 0.25 | 47.7 |
| $\Phi \rightarrow$ C2 $\rightarrow$ Yc | 0.07 | 23.0 | 0.08 | 15.5 | 0.08 | 22.1 | 0.09 | 18.1 | 0.07 | 18.6 | 0.09 | 17.0 |
| C1 $\rightarrow$ Yc | 0.10 | 31.1 | 0.14 | 27.0 | 0.10 | 29.3 | 0.14 | 26.6 | 0.11 | 30.9 | 0.14 | 28.0 |
| C1 $\rightarrow$ C2 $\rightarrow$ Yc | 0.02 | 6.7 | 0.04 | 7.2 | 0.03 | 8.6 | 0.04 | 8.4 | 0.02 | 6.5 | 0.04 | 7.3 |
| Circumstances | 0.20 | 60.7 | 0.26 | 49.6 | 0.20 | 60.0 | 0.28 | 53.1 | 0.20 | 56.0 | 0.27 | 52.3 |
| Total | 0.32 | 100.0 | 0.53 | 100.0 | 0.34 | 100.0 | 0.52 | 100.0 | 0.35 | 100.0 | 0.52 | 100.0 |
| 5–9 years average | | | | | | | | | | | | |
| $\Phi \rightarrow$ Yc | 0.13 | 39.0 | 0.27 | 50.5 | 0.14 | 40.4 | 0.25 | 47.0 | 0.16 | 44.1 | 0.25 | 47.7 |
| $\Phi \rightarrow$ C2 $\rightarrow$ Yc | 0.07 | 23.0 | 0.08 | 15.6 | 0.07 | 21.7 | 0.10 | 18.2 | 0.07 | 18.8 | 0.09 | 17.0 |
| C1 $\rightarrow$ Yc | 0.10 | 31.3 | 0.14 | 26.7 | 0.10 | 30.0 | 0.14 | 26.4 | 0.11 | 31.2 | 0.14 | 27.9 |
| C1 $\rightarrow$ C2 $\rightarrow$ Yc | 0.02 | 6.7 | 0.04 | 7.2 | 0.03 | 7.9 | 0.04 | 8.4 | 0.02 | 5.9 | 0.04 | 7.4 |
| Circumstances | 0.20 | 61.0 | 0.26 | 49.5 | 0.20 | 59.6 | 0.28 | 53.0 | 0.20 | 55.9 | 0.27 | 52.3 |
| Total | 0.32 | 100.0 | 0.53 | 100.0 | 0.34 | 100.0 | 0.52 | 100.0 | 0.36 | 100.0 | 0.52 | 100.0 |
| 6–9 years average | | | | | | | | | | | | |
| $\Phi \rightarrow$ Yc | 0.15 | 46.8 | 0.27 | 50.5 | 0.16 | 45.3 | 0.25 | 47.0 | 0.18 | 48.0 | 0.25 | 47.7 |
| $\Phi \rightarrow$ C2 $\rightarrow$ Yc | 0.06 | 19.3 | 0.08 | 15.7 | 0.07 | 19.5 | 0.10 | 18.3 | 0.06 | 16.9 | 0.09 | 17.1 |
| C1 $\rightarrow$ Yc | 0.09 | 27.9 | 0.14 | 26.7 | 0.10 | 27.9 | 0.14 | 26.3 | 0.11 | 29.3 | 0.14 | 27.8 |
| C1 $\rightarrow$ C2 $\rightarrow$ Yc | 0.02 | 5.9 | 0.04 | 7.0 | 0.03 | 7.3 | 0.04 | 8.3 | 0.02 | 5.8 | 0.04 | 7.3 |
| Circumstances | 0.17 | 53.2 | 0.26 | 49.5 | 0.19 | 54.7 | 0.28 | 53.0 | 0.19 | 52.0 | 0.27 | 52.3 |
| Total | 0.33 | 100.0 | 0.53 | 100.0 | 0.35 | 100.0 | 0.53 | 100.0 | 0.37 | 100.0 | 0.52 | 100.0 |

Note: Sample size differs for each estimation. For the sub-18 sample, for earnings and income, respectively: 720 and 1,911 (3+ years), 760 and 2,036 (4+ years), 812 and 2,159 (5+ years), 708 and 1,909 (6 years). For the sub-20 samples: 747 and 2,034 (3+ years), 799 and 2,157 (4+ years), 697 and 1,902 (5+ years), 734 and 2,027 (6 years). For the sub-22 samples: 783 and 2,148 (3+ years), 683 and 1,896 (4+ years), 720 and 2,021 (5+ years), 769 and 2,142 (6 years).

Table 4.A.4: Robustness check for influence of $C_1$: Age cutoffs

| | 18 or younger in 1989 | | | | 20 or younger in 1989 | | | | 22 or younger in 1989 | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Earnings | | Income | | Earnings | | Income | | Earnings | | Income | |
| | Dir. | Ind. | Dir. | Ind. | Dir. | Ind. | Dir. | Ind. | Dir. | Ind. | Dir. | Ind. |
| IQ score | 53.4 | 46.6 | 39.0 | 61.0 | 52.2 | 47.8 | 41.5 | 58.5 | 56.3 | 43.7 | 45.5 | 54.5 |
| Education (years) | 64.2 | 35.8 | 52.5 | 47.5 | 64.2 | 35.8 | 55.1 | 44.9 | 62.1 | 37.9 | 56.7 | 43.3 |
| Ethnicity: Non-white | 31.2 | 68.8 | 16.7 | 83.3 | 30.4 | 69.6 | 20.6 | 79.4 | 24.8 | 75.2 | 20.0 | 80.0 |
| Occup: Professional | 44.9 | 55.1 | 39.3 | 60.7 | 48.1 | 51.9 | 42.5 | 57.5 | 53.9 | 46.1 | 46.0 | 54.0 |
| Occup: Manager | 65.3 | 34.7 | 44.1 | 55.9 | 63.3 | 36.7 | 44.6 | 55.4 | 59.4 | 40.6 | 47.5 | 52.5 |
| Occup: Clerical | 213.0 | -113.0 | 46.9 | 53.1 | . | . | 28.6 | 71.4 | 551.5 | -451.5 | -437.3 | 537.3 |
| Occup: Craftsman | 35.9 | 64.1 | 128.9 | -28.9 | -30.5 | 130.5 | 143.8 | -43.8 | -38.8 | 138.8 | 110.8 | -10.8 |
| Occup: Operative | 66.9 | 33.1 | 51.5 | 48.5 | 68.4 | 31.6 | 55.0 | 45.0 | 66.3 | 33.7 | 55.5 | 44.5 |
| Occup: Farmer | 15.6 | 84.4 | 30.0 | 70.0 | 51.3 | 48.7 | 26.6 | 73.4 | 48.3 | 51.7 | 24.0 | 76.0 |
| Occup: Services | 56.3 | 43.7 | 37.3 | 62.7 | 54.2 | 45.8 | 36.1 | 63.9 | 50.6 | 49.4 | 41.7 | 58.3 |
| Occup: Other | 26.9 | 73.1 | 15.3 | 84.7 | 25.0 | 75.0 | 18.3 | 81.7 | 27.3 | 72.7 | 20.2 | 79.8 |
| P grew: in Farm | 37.1 | 62.9 | 40.6 | 59.4 | 43.1 | 56.9 | 47.6 | 52.4 | 45.7 | 54.3 | 38.9 | 61.1 |
| P grew in Small town | -186.1 | 286.1 | 44.2 | 55.8 | -17.0 | 117.0 | 48.4 | 51.6 | 15.8 | 84.2 | 62.2 | 37.8 |
| P grew in Large city | 68.9 | 31.1 | 29.8 | 70.2 | 67.6 | 32.4 | 43.6 | 56.4 | 53.6 | 46.4 | -10.2 | 110.2 |
| P grew in Other | 96.2 | 3.8 | 30.7 | 69.3 | 84.0 | 16.0 | 35.7 | 64.3 | 102.2 | -2.2 | 11.3 | 88.7 |

Note: Sample size differs for each estimation. For the sub-18 sample, for earnings and income, respectively: 708 and 1,909. For the sub-20 samples: 734 and 2,021. For the sub-22 samples: 769 and 2,142. The parent's IQ test (0 to 13) was taken by the head of family in 1974. Education is a continuous variable going from 1 to 17 for the parent with the highest education in 1989. All other parental characteristics are for the head of the family in 1989. Parent's ethnicity is a binary variable that takes the value 1 for a person of color (POC) and where the reference category is "White". Missing values reflect shares below -1000% or above 1000%.

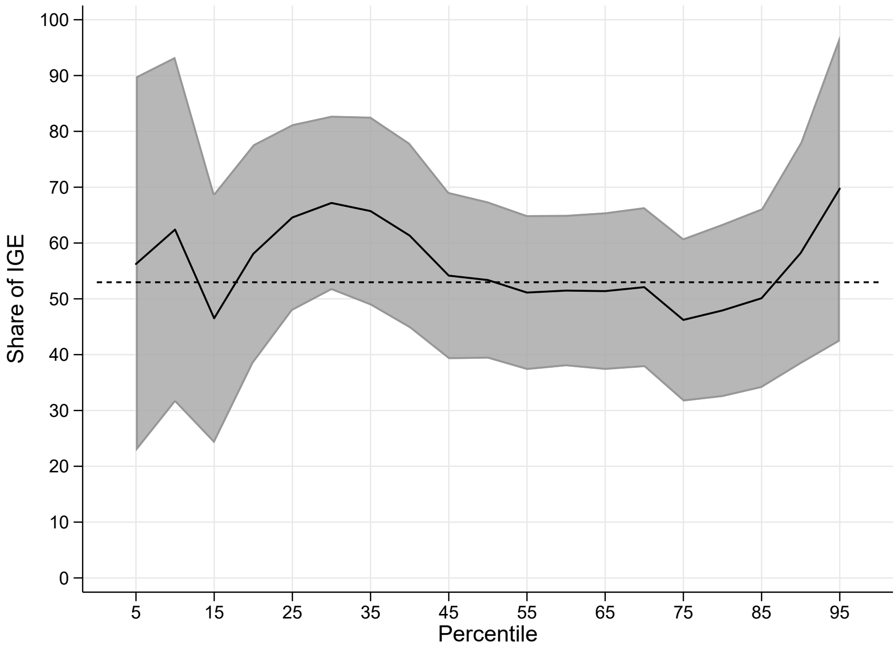
Table 4.A.5: Linear regression for each outcome

| VARIABLES | (1) Earnings | (2) Earnings | (3) Earnings | (4) Income | (5) Income | (6) Income |
|---|---|---|---|---|---|---|
| Parental earnings | 0.351*** | 0.228*** | 0.158*** | | | |
| | (0.058) | (0.057) | (0.057) | | | |
| Parental income | | | | 0.526*** | 0.344*** | 0.247*** |
| | | | | (0.025) | (0.028) | (0.034) |
| IQ score | | 0.025 | 0.022 | | 0.022** | 0.018** |
| | | (0.015) | (0.015) | | (0.009) | (0.009) |
| Education (years) | | 0.044** | 0.038** | | 0.056*** | 0.049*** |
| | | (0.019) | (0.016) | | (0.009) | (0.008) |
| Ethnicity: Non-white | | 0.095 | 0.165 | | -0.021 | 0.022 |
| | | (0.109) | (0.106) | | (0.063) | (0.061) |
| Occup: Professional | | -0.156 | -0.144 | | -0.094** | -0.084* |
| | | (0.100) | (0.095) | | (0.047) | (0.045) |
| Occup: Clerical | | 0.059 | 0.034 | | -0.102* | -0.107* |
| | | (0.127) | (0.125) | | (0.062) | (0.062) |
| Occup: Craftsman | | -0.078 | -0.038 | | -0.082* | -0.047 |
| | | (0.066) | (0.065) | | (0.042) | (0.043) |
| Occup: Operative | | -0.200** | -0.176** | | -0.193*** | -0.155*** |
| | | (0.088) | (0.075) | | (0.052) | (0.050) |
| Occup: Farmer | | -0.590 | -0.532 | | -0.182 | -0.103 |
| | | (0.406) | (0.372) | | (0.115) | (0.106) |
| Occup: Services | | -0.116 | -0.082 | | -0.211*** | -0.163** |
| | | (0.134) | (0.126) | | (0.081) | (0.081) |
| Occup: Other | | -0.317* | -0.205 | | -0.180*** | -0.085 |
| | | (0.180) | (0.204) | | (0.066) | (0.071) |
| P grew: in Farm = o, | | - | - | | | |
| P grew in Small town | | 0.028 | 0.028 | | 0.088 | 0.106 |
| | | (0.073) | (0.071) | | (0.087) | (0.081) |
| P grew in Large city | | 0.087 | 0.085 | | 0.088 | 0.117 |
| | | (0.073) | (0.072) | | (0.089) | (0.083) |
| P grew in Other | | 0.175 | 0.102 | | | |
| | | (0.128) | (0.114) | | | |
| Homeowner | | | 0.062 | | | 0.051 |
| | | | (0.062) | | | (0.037) |
| Region: Mideast | | | 0.145 | | | 0.014 |
| | | | (0.143) | | | (0.077) |
| Region: Great Lakes | | | 0.023 | | | -0.060 |
| | | | (0.137) | | | (0.074) |
| Region: Plains | | | -0.017 | | | -0.016 |
| | | | (0.147) | | | (0.078) |
| Region: Southeast | | | -0.002 | | | -0.105 |
| | | | (0.141) | | | (0.076) |
| Region: Southwest | | | 0.013 | | | -0.195** |
| | | | (0.151) | | | (0.095) |
| Region: Rocky Mount. | | | -0.063 | | | -0.062 |
| | | | (0.166) | | | (0.091) |
| Region: Far West | | | -0.140 | | | -0.047 |
| | | | (0.224) | | | (0.088) |
| Region: Outside U.S.A. | | | 0.553** | | | 0.317 |
| | | | (0.263) | | | (0.203) |
| Region: No Answer | | | -0.064 | | | -0.055 |
| | | | (0.136) | | | (0.287) |
| Over median: Business | | | -0.022 | | | -0.016 |
| | | | (0.078) | | | (0.046) |
| Over median: Stocks | | | -0.021 | | | 0.058 |
| | | | (0.064) | | | (0.036) |

| | | | | | | |
|---|---|---|---|---|---|---|
| Over median: Savings | | | 0.188*** | | | 0.190*** |
| | | | (0.068) | | | (0.038) |
| Used food stamps | | | -0.216* | | | -0.128* |
| | | | (0.117) | | | (0.069) |
| P grew: in Farm | | | | | 0.047 | 0.064 |
| | | | | | (0.088) | (0.084) |
| P grew in Other = o, | | | | | - | - |
| Constant | 7.036*** | 7.542*** | 8.212*** | 5.317*** | 6.381*** | 7.427*** |
| | (0.623) | (0.614) | (0.640) | (0.276) | (0.308) | (0.373) |
| Observations | 725 | 725 | 725 | 2,021 | 2,021 | 2,021 |
| R-squared | 0.093 | 0.152 | 0.200 | 0.257 | 0.315 | 0.346 |

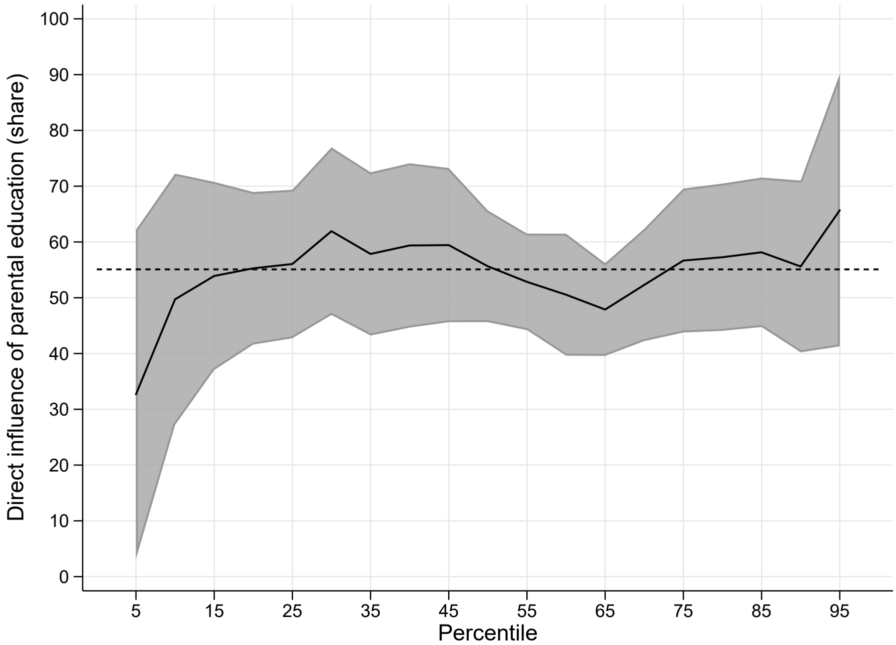
Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Figure 4.A.2: IGE decomposition: Quantile regression




Note: Quantile regression estimation for parental family income on offspring family income, with and without controlling for all other circumstances ($N = 2,021$). Confidence interval based on a 100 iteration bootstrap, clustered at the parental family level, using random sampling with replacement over the whole estimation process.

Figure 4.A.3: Direct influence of parental income: Quantile regression




Note: Quantile regression estimation for parental education on offspring family income, with and without controlling for parental family income ($N = 2,021$). Confidence interval based on a 100 iteration bootstrap, clustered at the parental family level, using random sampling with replacement over the whole estimation process.

# Chapter 5

# Conclusions

## 5.1 Main findings and contributions

The study of inequality of opportunity has grown exponentially in the last 25 years, with research for many countries on its measurement, causes and consequences. In this thesis I examine three topics. I provide upper bound estimates of inequality of opportunity, contrasting them with the more common lower bound estimates. I use these estimates to study their impact on economic growth and what accounts for that relationship. I also study how childhood circumstances – as defined in the inequality of opportunity literature – relate to intergenerational persistence as summarised by the intergenerational elasticity. Using panel data from Europe and the U.S.A., I present three empirical applications at the intersection of inequality of opportunity and other topics within the economic inequality literature.

### 5.1.1 Upper bound estimates of inequality of opportunity

In Chapter 2 I report upper bound estimates of inequality of opportunity for 24 European countries between 2005 and 2011. These estimates capture the contribution of all time-invariant factors, including both circumstances and time invariant efforts. The upper bound estimate of inequality of opportunity captures all circumstances that are typically included in empirical studies (i.e., parental education, place of birth, etc.) as well as others that are not frequently included and that are difficult to measure, such as parental time investment or innate ability. It also captures time-invariant efforts such as having a 'hard-working attitude'. Whether these estimates are closer to the 'real' level of IOp than lower bound estimates depends on the relative importance of these time invariant efforts. In the chapter I argue that providing both lower and upper bound estimates of inequality of opportunity provided a much more nuanced measure of the potential influence of circumstances on inequality of outcomes.

I use EU-Statistics on Income and Living Conditions (EU-SILC) to provide these estimates. I estimate the lower bound estimates for 2005 and 2011 – for which the EU-SILC included a special intergenerational module providing retrospective data on characteristics of the parents. The lower bound estimates accounted for around 2% of inequality of outcomes for Iceland and Finland and Austria in 2011, around 20% for Hungary and Portugal in 2005. The lower bound estimates also include Belgium in 2005, a clear outlier with inequality of opportunity accounting for 30% of inequality of outcomes. For the same two years, the upper bound estimates range from 40% for Iceland (in 2005 and 2011) to over 80% for Estonia in 2005, and Denmark and the Netherlands in 2011. The upper bound estimates are substantially larger than the lower bound estimates of inequality of opportunity.

A question that arises from these results is, how much larger are the upper bound estimates of inequality of opportunity than the lower bound estimates? As the lower bound estimates account for few circumstances while the upper bounds account for all time-invariant factors, the difference between the two reflects the importance of omitted circumstances and time-invariant efforts. For 2011, the gap accounts for between 36% and 78% of inequality of outcomes. The gaps can vary substantially, and that variation is mostly accounted for by the variability in the upper bound across countries. These estimates lead to two main findings. First, the contribution of omitted circumstances and time invariant efforts is quite large and can vary a lot across countries. These large differences in the size of the gap between bounds suggest that in some countries there is still a large margin for the addition of new circumstances to the measurement of lower bound estimates of inequality of opportunity.

### 5.1.2 Economic growth and inequality of opportunity

In Chapter 3 I study the effect of inequality of opportunity on annual GNI per capita growth rates. I contribute to the literature by using upper bound estimates and by focusing on a period that covers to economic crises, the Great Recession and the European Debt crisis. Upper bound estimates of inequality of opportunity capture characteristics that might differ across countries – other than using lower bound estimates as previous studies have done – allowing for each country estimate to capture circumstances that matter for them. Upper bound estimates also show higher variability than lower bound estimates, better capturing between-country heterogeneity. This chapter discusses how inequality of opportunity and growth interact, in the context of low growth rates, credit constraints and high unemployment.

I use System GMM to estimate the effect of inequality of opportunity (measured using the MLD index) on the annual GNI per capita growth rate. I include in my analysis all 24 countries from the previous chapter, plus three other countries with data for 5 of the 7 years of the 2005 to 2011 period. A one-standard-deviation increase in inequality of opportunity – equivalent to moving from the bottom of the country ranking to the middle – results in a decrease in growth rates ranging from 1.2 to 3.1 percentage points. The lower part of this range overlaps with previous estimates that have used a lower bound estimate of inequality of opportunity, where the estimates range between 1.3 to 2.5 percentage points. This relationship is robust to different choices of instruments, estimation approaches, and the inclusion of determinants of long and short-term growth.

Inequality of outcome estimates do have a statistically significant effect on growth, albeit much less robust than for inequality of opportunity. This suggests that, in the context of financial crises, an increase in inequality of outcomes results in lower growth rates, particularly when this increase is a result of higher inequality of opportunity. This finding does not support the 'cholesterol hypothesis', the idea that higher inequality of opportunity decreases growth while higher inequality of effort is increases growth. On the other hand, my findings are consistent with a weaker version of this hypothesis, discussed in Ramos and Van de gaer (2020), where the effect of IOp is stronger than the effect of the residual term.

### 5.1.3 Intergenerational persistence and inequality of opportunity

In Chapter 4 I look at the relationship between intergenerational elasticities and inequality of opportunity. I decompose the former to quantify the contribution of circumstances commonly studied in the context of inequality of opportunity. I also quantify how much of the influence of circumstances is accounted for the intergenerational elasticity. By doing so I take the inequality of opportunity approach – used to decompose the income or earnings distribution for a cross-section – and apply it to understand its relationship with intergenerational income persistence.

I find that circumstances account for 55% of the intergenerational elasticity of individual earnings and 53% of the intergenerational elasticity for family income. The estimates for the intergenerational elasticities (0.35 for earnings and 0.53 for income) are constructed to maximise the comparability with previous estimates. That means that the estimates for earnings only include fathers and sons while the estimates for income include all heads of family and their offspring. The outcomes of both parents and their offspring are averaged over 6 to 9 years to reduce volatility. The sample for earnings is much smaller (721 father-son pairs versus 2,021 parent-offspring pairs for income), making the estimates less precise and hence their confidence interval much wider.

I group circumstances into two categories. Depending on the point in time at which they have an influence, they can be either preceding or mediating circumstances. The former category includes circumstances that influence parental income, such as parental education. The latter group includes circumstances that are influenced by parental income and have an influence over offspring's income. Examples of mediating circumstances include measures of wealth and assets, or the place of birth of the children. I provide a detailed decomposition of the intergenerational elasticity into all three potential paths that stem from the interaction of preceding and mediating circumstances.

Two circumstances stand out for their contribution to the intergenerational elasticity. The first one is parental education, measured as the number of years of education of the parent with the highest education. This preceding circumstance accounts for 20% of the intergenerational elasticity and it is the single largest contribution among all of the circumstances. The second circumstance is the family savings when the offspring was growing up, measured as a binary variable for people with above-median savings. This is a mediating circumstance that accounts for around 10% of the intergenerational elasticity.

I also find that the intergenerational elasticity does not capture much of the influence of some circumstances. Parental education has a strong influence on parental income, but only half of its influence is captured by the intergenerational elasticity of income. On the other hand, the IQ of the parent and their ethnicity are circumstances that are mostly accounted for by the intergenerational elasticity of income. In sum, circumstances play an important role in explaining intergenerational income persistence, but their influence – as accounted for by inequality of opportunity estimates – goes beyond is not fully captured by it.

## 5.2 Discussion and implications

Chapter 2 explores the issue of omitted circumstances and lower bound estimates of inequality of opportunity. It proposes a simple solution, over estimate the influence of circumstances by using all time invariant factors as a proxy of circumstances. Under this definition, inequality of outcomes and inequality of opportunity are very close (with the latter accounting for up to 90% of the former in some cases) and strongly correlated, both over time and across countries. The lower bound estimates of inequality of opportunity suggest that circumstances account for 2 to 20% of inequality of outcomes. The upper bound estimates, on the other hand, suggest that almost all inequality of outcomes can be attributed to differences in circumstances.

A question that arises from this approach is the importance of time-invariant efforts. In other words, how much higher are these estimates than the real level of inequality of opportunity? If time-invariant efforts are more important than omitted circumstances, then the lower bound estimate of inequality of opportunity will be closer to its real level. The converse is true if omitted circumstances explain most of the gap between the upper and lower bound estimate. While it is impossible to provide a certain answer, I explore this question through two empirical exercises in Chapter 2.

The first exercise increases the lower bound estimate. I include additional circumstances to the lower bound estimation of IOp to check how close they get to the upper bound. This analysis shows that the new circumstances provide little additional information, except perhaps for the inclusion the parent's country of birth for a few countries like Austria, Cyprus or Spain. The second exercise attempts to decrease the upper bound estimate by removing some of the influence of time invariant efforts by controlling for effort variables when estimating my measure of circumstances. The change in the IOp level is negligible except for specific years in a few countries. These exercises reflect how complicated it is to provide accurate estimates of IOp (i.e., close to a 'real' level) using either the lower or upper bound approach.

While both the upper and lower bound estimates of inequality of opportunity suffer from problems, reporting both allows a more nuanced analysis of the importance of circumstances. The lower bound estimate misses the influence of some circumstances while the upper bound estimate captures more than just circumstances. In addition, my findings suggest that the upper bound provides new information relative to the lower bound, as seen in the change in ranking positions and trends over time, reinforcing the complementarities in reporting the two. By providing both bounds we can frame inequality of opportunity estimates as a range of possible values rather than a single point estimate.

Despite their close relationship, Chapter 3 finds that inequality of opportunity and inequality of outcomes differ in their effect on short-term economic growth. An increase in inequality of opportunity – measured through the upper bound estimate – reduces economic growth while inequality of outcomes shows a much less robust effect, only significant for certain estimates. These findings suggest that it is the time-invariant component of inequality – the component associated with inequality of opportunity – that explains to a large extent the influence of inequality on economic growth.

Contrary to previous estimates of the effect of inequality of opportunity on growth such as Marrero and Rodríguez (2013, 2019), my findings show both inequality of opportunity and inequality of outcomes to have a negative regression coefficient. While it could be argued that this is due to my use of upper bound estimates, the similarities in the size of the effect of inequality of opportunity suggests that this is not the main driver of these differences. Instead, I argue that this is because of the period I study, that includes for the Great Recession and the European Debt crisis. One possible explanation could be that, in this context, efforts appear to be heavily mediated by market conditions (what Dworkin (1981a,b) could refer to as 'later brute luck') such that inequalities due to effort reflect inequalities in access to credit or job opportunities, thus reducing growth rates (albeit less than inequality of opportunity). The fact that the effect of inequality of effort grows when we control for standard measures of effort such as working hours suggest that this might be case.

Chapter 4 studies the role of circumstances in the context of intergenerational mobility. Parental education is the most relevant circumstance and between 20% to

25% of the intergenerational elasticity of earnings and income. In addition, around half of its total influence on the income of the offspring is not accounted for by the intergenerational elasticity, suggesting that – for this particular circumstance – an estimate of inequality of opportunity is a better measure of intergenerational transmission of advantages than the intergenerational elasticity.

Another relevant circumstance for the intergenerational elasticity is having high savings when the children are growing up. Savings act as a buffer against economic shocks such as a health problem or unemployment and are highly concentrated.[1] Savings also allow families to invest in their children's education and wellbeing. However, while both parental education

I group circumstances into those preceding parental income (such as their education) and those that mediate the relationship between parent's and offspring's income (such as parental savings). While each of these groups accounts for a large share of the intergenerational elasticity of income on their own (around 26% and 18%, respectively), there is little interaction between the two. Only 15% of the total influence of circumstances involves both groups, for example reflecting the influence of parental education on parental income, which in turn influences the savings held by that family. My findings point to two unrelated paths through high high income parents have high income children, through parental characteristics such as their education, ethinicity or IQ, or through their assets and choices, such as the place of birth of their children.

The decomposition in Chapter 4 might be specific to the U.S.A. and might not apply to other high-income countries. Corak et al. (2011) compares economic mobility in the U.S.A. and Canada, highlighting how accessible healthcare, higher flexibility in childcare choices and working hours, and higher redistribution in Canada make the children in the U.S.A. much more dependent on their parents' socioeconomic background. Breen (2019) reports that, in contrast with Germany or France, the U.S.A. has had no aggregate educational expansion among cohorts

[1]For example, in the context of the COVID-19 pandemic, 47% of Americans report having enough savings to cover three months of expenses (Parker et al., 2020). Among lower-income households that share goes down to 23%.

born after the 1950s, which partly explains why intergenerational persistence has not decreased over time. My results might not hold in other high-income countries, but they could be similar to countries with weaker social safety nets. Further research on different countries will help provide a more comprehensive view of the determinants of intergenerational immobility.

While my focus has been on the measurement of inequality of opportunity and its relationship to growth and mobility, this does not mean that equality of opportunity should be the only principle of justice that determines the distribution of a certain outcome. For the case of poverty, Bourguignon et al. (2006) defines equity as two principles: equal opportunities and avoidance of extreme deprivation. They expand on what Roemer (1998) calls an equal opportunity policy – a policy that maximises the utility of the worse off type – by subjecting it to a restriction consistent with the absence of deprivation. Hufe et al. (2018) provide estimates for such an approach for the U.S.A. and other European countries, showing upward corrections in the extent of unfair inequality when accounting for poverty aversion.

Inequality of effort can also be excessively high. Frank (2016) gives a thorough review of what he calls 'winner-take-all' markets, which assign all rewards to whoever gets the position and nothing to the rest. Common examples of jobs in winner-take-all markets are CEO and upper management positions in the tech, consultancy and financial sectors. Other markets include the music and film industries, as well as professional football. In these markets, the return to efforts is non-linear: small differences in effort result in considerable differences in the outcome. In cases such as this, Frank (2016) argues that it is luck that explains most of the differences in rewards. Winner-take-all markets suggest that luck and other circumstances are involved even when rewards are attributed to differences in effort.

The literature on inequality of opportunity has also studied the treatment of inequalities of effort. From a theoretical point of view, Ramos and Van de gaer (2016) recognise that some differences in effort could result in large differences in the outcome. They propose a reward principle that allows for some inequality aversion from differences due to effort. They also justify this principle on empirical grounds, as most inequality of opportunity estimates attribute the influence of omitted circumstances to effort. To that I add, from an intergenerational point of view, that factors that we consider as acceptable for one generation can become sources of circumstances later in life. In Chapter 4 I discuss this in the context of intergenerational immobility, but the same is true for differential effort. This idea goes back to the point made in World Bank (2006) and Atkinson (2015), where inequality of outcomes (or indeed, inequality of effort) in one generation becomes inequality of opportunity for the next, and inequality of opportunity, in turn, results in higher inequality of outcomes.

## 5.3 Further research

There are aspects of the research on inequality of opportunity that should be further developed. Most of them are related to the estimation of inequality of opportunity in the presence of omitted circumstances but there are also other avenues to pursue, particularly regarding the relationship between inequality of opportunity and other topics in the economic inequality literature. Together, this research would contribute to understanding the drivers behind inequality of opportunity and, more generally, the intergenerational transmission process.

Regarding the estimation of inequality of opportunity, one direction to pursue is to look for ways to narrow the gap between the upper and lower bound estimates of inequality of opportunity. One way to do this is by studying the determinants of why some countries have very small differences between their upper and lower bound estimates while others have large differences. My estimates show that among the countries I measure a gap, Greece has the smallest gaps while the Netherlands shows the largest gap. What are the differences between these countries that result in Greece better accounting for the 'whole picture' with just a few numbers of circumstances? Are there omitted circumstances that matter more for the Netherlands? These are possible questions to address in regards to the gap between upper and lower bound estimates.

Another way to explore what is behind the gap between bounds is to increase the lower bound estimate through the inclusion of new circumstances. I provide examples of how to do so in Chapter 2. However, I show that including new circumstances to increase the lower bound or to account for efforts to reduce the upper bound make small differences in inequality of opportunity estimates. Additional variables, either circumstances or efforts, can contribute to better estimates if they provide information additional to that of the previously included variables. For example, on one hand, the inclusion of whether parents held managerial positions makes little difference in the lower bound estimate of inequality of opportunity as occupation and education are highly correlated with having a managerial position. On the other hand, the country of birth of the parents reduces the gap substantially for Mediterranean countries like Cyprus, Greece, and Spain, suggesting that it identifies additional mechanisms that go above and beyond what is captured by previous circumstances.

A second line of research in the study of upper bound estimates of inequality of opportunity is the importance of time-variant circumstances and more generally, inequality of opportunity dynamics. It could be determined whether the upper bound estimate is an upper bound at all, as the upper bound approach assumes that all circumstances are time-invariant, but that might not be the case. Moramarco et al. (2020) propose a dynamic approach to quantifying inequality of opportunity over the life cycle that allows for circumstances that vary over time. They do this by defining an intertemporal measure of inequality of opportunity which is then aggregated across time. They include one time-variant circumstance, which is whether the parent provided material or financial support during the previous year, while all other circumstances are constant when the outcome was measured (place of birth, gender, and parental education). However, and just like any additional circumstance, for time-variant circumstances to play an important role they would need to account for income differences above and beyond those accounted for by time-invariant circumstances.

Future research should also study inequality traps in the context of inequality of opportunity. One way to do this is by identifying the worse off types and determining if, over time, it is always the same group. Another way to do this is to adapt growth incidence curves – summary measures of economic growth across percentiles of the income distribution – to compare growth pattern across types. One example of such an approach is the Opportunity Growth Incidence Curves proposed in Peragine et al. (2014) and using this approach to identify types with low growth rates. Identifying the evolution of the most advantaged and disadvantaged types could provide new insights into the relationship between inequality of opportunity, inequality of outcomes, and economic growth. These approaches could help quantify the extent of 'inequality of opportunity traps', where parents in the worse off types have offspring that also fall within the worse off types.

A third avenue to pursue regarding the estimation of inequality of opportunity is the interaction between circumstances. As most estimates use linear regression to account for the importance of circumstances, their interactions are typically omitted. In Chapter 4 I find that there is little interaction between what I call preceding and mediating circumstances, but I do not explore the interactions within each of the two groups of circumstances. These interactions might represent substantive disadvantages, for example, the interaction between gender and ethnicity or more generally, the interaction between parent's cultural and financial capital in highly unequal societies. These interactions could help improve the omitted circumstance problem and provide a better understanding of the role of previously studied circumstances.

Most of these lines of research reinforce the data-demanding nature of inequality of opportunity research. Even in the presence of multiple circumstance variables, small sample sizes result in issues of overfitting (Brunori et al., 2018). Data constraints are particularly problematic when applying these methods to low and middle-income countries where exhaustive socioeconomic surveys are less common and where long-running panel data are scarce. The solutions to this problem include the use of short panels to estimate upper bound estimates of inequality of opportunity as discussed in Chapter 2 and by Hufe et al. (2019), using linked administrative data (Correa et al., 2019) or using pseudo-panels constructed from repeated cross-section surveys (Cuesta et al., 2011). The innovative use of existing data as well as new data sources are required to go beyond lower bound estimates of inequality of opportunity.

Inequality of opportunity research could also gain from studying its relationship with inequality perceptions and demands for redistribution. One of the motivations behind the measurement of inequality of opportunity is to communicate how 'unfair' is the current distribution of income (or other outcomes), expecting them to inform and update demands for redistribution. However, Brunori (2017) finds a weak correlation between perceived and measured inequality of opportunity (measured using lower bound estimates for Europe). Factors like experiences of upwards social mobility, unemployment and living in urban areas are much more closely related to perceived inequality of opportunity. These results suggest that lower bound estimates of inequality of opportunity do not capture the individual experiences and characteristics that are associated with perceptions of fairness. Future research should study this relationship, for example through the use of different measures of inequality of opportunity that could account for the factors that are correlated to perceptions on inequality.

One of the arguments in favour of improving inequality of opportunity estimates is that they can better inform policy and justify demands for redistribution. Future research should look at why the relationship between perceptions and estimated inequality of opportunity is so weak. Is it a measurement problem? Are there important confounders behind this relationship? For example, could this relationship have weakened amid the European Debt crisis, with high unemployment and fewer experiences of upward mobility? Mijs (2019) reports that rising inequality of outcomes is followed by a higher belief in meritocracy. Perhaps it is inequality of outcomes that shapes perceptions of fairness. If we expect inequality of opportunity estimates to justify demands for redistribution, we need to understand the relationship between objective and subjective measures of unfair inequality.

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