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Rafael Carranza

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Upper and lower bound estimates of inequality of opportunity: A cross-national comparison for Europe [‡]

Rafael Carranza
Department of Social Policy, LSE

Abstract

I provide lower and upper bound estimates of inequality of opportunity (IOp) for 24 European countries, between 2005 and 2011. Previous estimates of IOp are lower bounds of its true level and provide a partial view of the importance of involuntarily inherited factors. Upper bound estimates of IOp are much larger than their corresponding lower bound estimates. While the lower bound estimates of IOp account for up to 31% of total inequality, the upper bound estimates account for up to 90.5%, suggesting that IOp can be as high as total inequality of outcomes. Indeed, inequality of outcomes has a higher correlation with the upper bound estimates of IOp than with the lower bound estimates, both cross sectionally and over time.

Keywords: Circumstances, equality of opportunity, equivalized household income, inequality, MLD index, upper bound estimate.

JEL Classification: D63.

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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, in order to be able to pursue the goal of equal opportunity, we first require an appropriate measurement of unequal opportunities. I provide upper bounds estimates of inequality of opportunity – a much less common way of quantifying inequality of opportunity – for 24 European countries between 2005 and 2011, complemented with standard lower bound estimates of IOp.

The literature on inequality of opportunity (IOp) states that sources of inequality matter from an ethical point of view (see, e.g., Cohen (1989)). In particular, what matters is the distinction between morally legitimate sources, commonly called 'efforts', 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 (Roemer and Trannoy, 2016; Bourguignon, 2018).

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; Balcázar, 2015). On their own, lower bound estimates can be problematic for three reasons. First, we do not know how far these estimates are from the 'real' level of IOp (Ferreira and Peragine, 2016). Second, lower bound estimates can be misinterpreted as the real level of IOp, reducing redistributive efforts from policy makers (Kanbur and Wagstaff, 2016). Third, misinterpreting lower bound estimates of IOp can diminish the perceived importance of structural causes of inequality, which increase concerns about inequality (Mijs, 2019). 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 true extent of IOp could be well above the levels shown by the latter. I also show that upper

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

bound estimates provide new information, as both country rankings and trends over time show different patterns when looking at the two different estimates. Lastly, by measuring the difference between the upper and lower bound estimates, I show that the importance of the circumstances omitted by the lower bound estimate differs greatly between countries. Overall, these results 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 paper 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 describes the results by showing the general results, the differences between the two estimates, and the gap between them. Section 5 explores robustness checks. Section 6 concludes.

2 Estimating lower and upper bounds of IOp

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 used for a comparison with previous studies, [2] as well as to avoid issues with cross-country differences in labour market participation, particularly among women.

IOp measures the importance of circumstances when determining income. Circumstances are morally illegitimate factors, and characterize what we consider to be outside of an individual's control. Usual examples include gender, the education of the parents, household composition when growing up, etc. IOp is measured as the level of inequality that can be explained by differences in circumstances.[3] I use two approaches to estimate IOp, resulting in lower bound and upper bound esti-

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

[3]For a discussion on what is considered a circumstance and the different interpretations of IOp, see Roemer (2004) or Cohen (2009).

mates. These two approaches differ in how the set of circumstances is constructed: the lower bound approach is limited by the available variables in the data, while the upper bound approach attempts to capture all circumstances while capturing other factors as well. The real level of IOp will be somewhere in between these bounds, closer to one or the other, depending on the importance of the circumstances omitted by the lower bound estimate and the other factors captured by 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 for the lower and upper bound estimates of IOp.

I assume that circumstances are constant over time. This assumption follows from the idea that circumstances are predetermined factors, such as the place of birth or the investment made by parents. There is also a separate determinant of income, referred to as 'efforts' in the literature, that reflect aspects that are within our control, for which we should be held responsible. Efforts can be fixed or vary over time. The income of individual i in time period t is determined by a combination of circumstances and efforts, as shown in equation 1:

$$log(Y_{it}) = \alpha_0 + \beta_0 C_i + \gamma_0 E_{it} + \eta_0 E_i + \mu_t + \varepsilon_{it}. \tag{1}$$

C_i is a vector of circumstance variables, E_{it} and E_i are effort variables (time varying and time invariant, respectively), μ_t a year fixed-effect, and ε_{it} the error term. As the focus of the measurement of IOp of Y_{it} is on the role of individual characteristics, the time effect (for example, a year with a particularly high rate of unemployment) is neither a circumstance, as it is not an individual factor, nor an effort, as it is not a choice or preference.

Circumstances affect income directly, but they also have an indirect effect through efforts. Efforts are determined by circumstances and a separate component, with only the latter being a source of legitimate inequality.[4] Efforts are modelled as a linear combination of circumstances and an error term that represents the part of efforts that is not influenced by circumstances:

$$E_{it} = \alpha_1 + \beta_1 C_i + v_{it}. \tag{2}$$

$$E_i = \alpha_2 + \beta_2 C_i + u_i. \tag{3}$$

[4]This is a common assumption in the IOp literature. However, some authors consider the effect of circumstances on effort to be within the space of personal responsibility (Jusot et al., 2013).

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 gender can affect income directly through labour market discrimination, but also indirectly through labour market choices. The error term provides a measure of 'autonomous' or 'relative' effort that is not determined by circumstances. By substituting these two equations into equation 1, we get:

$$\begin{aligned} log(Y_{it}) = \quad & (\overbrace{\alpha_0 + \gamma_0\alpha_1 + \eta_0\alpha_2}^{\tilde{\alpha}}) + \mu_t + \overbrace{\eta_0 u_i}^{\tilde{u}_i} \\ & +(\underbrace{\beta_0 + \gamma_0\beta_1 + \eta_0\beta_2}_{\tilde{\beta}})C_i + (\underbrace{\varepsilon_{it} + \zeta_0 v_{it}}_{\tilde{\varepsilon}_{it}}). \end{aligned} \tag{4}$$

This equation includes all effects of circumstances on income, both direct and indirect, as shown in equation 5.

$$log(Y_{it}) = \tilde{\alpha} + \tilde{\beta}C_i + \mu_t + \tilde{u}_i + \tilde{\varepsilon}_{it}. \tag{5}$$

Income is determined by circumstances (C_i), a time effect (μ_t), an individual fixed effect that stems from efforts ($\tilde{u}_i$), and an error ($\tilde{\varepsilon}_{it}$). If we fix time to a particular year ($t = \tau$), we get equation 5.

$$log(Y_{it}) = (\tilde{\alpha} + \mu_\tau) + \tilde{\beta}C_i + (\tilde{u}_i + \tilde{\varepsilon}_{i\tau}) = \tilde{\tilde{\alpha}} + \tilde{\beta}C_i + \tilde{\tilde{u}}_i \tag{6}$$

Equation 5 is the standard equation to estimate IOp from cross-sectional survey data. $\tilde{\tilde{\alpha}}$ includes both the constant α and the time effect μ_τ, while $\tilde{\tilde{u}}$ includes the two types of residual efforts, the time invariant $\tilde{u}_i$ and the time variant, in a given year ($\tilde{\varepsilon}_{i\tau}$).

2.2 Measuring IOp: Estimation and prediction

I use equation 6 to estimate both lower bound and upper bound IOp for a given year. I use a parametric or 'regression-based' approach that assumes a functional form to estimate the relationship between income and circumstances (Bourguignon et al., 2007; Ferreira and Gignoux, 2011; Brunori et al., 2013).

The upper bound and lower bound estimates are derived in the same way, but differ in the choice of the circumstance vector. In the lower bound estimation, C_i includes all circumstance variables that are available in the dataset, partly capturing C_i

in equation 6. On the other hand, the circumstance vector in the upper bound approach is the predicted fixed effect from a longitudinal regression. The predicted fixed effect accounts for all circumstances as well as the time invariant component of the effort equation: the upper bound of IOp accounts for both C_i and $\tilde{u}_i$ in equation 5.

In order to measure IOp, we are interested in predicting the conditional mean $E(Y_i|C)$, but equation 6 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 $E(log(Y_i)|C)$, which would require an additional term to get to $E(Y_i|C)$, known as a smearing retransformation.[5]

$$log(\hat{Y}_i) = \hat{\alpha} + \hat{\beta} C_i. \tag{7}$$

IOp is between-type inequality, calculated from equation 7. I use the MLD index, as it can be additively decomposed into within and between group inequalities (with IOp measuring the latter). To summarize, the Inequality of Opportunity Level (IOL) for a given year $t = \bar{t}$ is:

$$IOL = I(\{\hat{Y}\}). \tag{8}$$

A relative measure of IOp can be obtained by dividing the IOL by total inequality. This ratio captures the share of inequality that can be attributed to circumstances, and it is called the Inequality of Opportunity Ratio (IOR):

$$IOR = \frac{I(\{\hat{Y}\})}{I(\{Y\})}. \tag{9}$$

I present estimates for both the IOL and IOR in 2005 and 2011, together with the upper bound estimates for the years 2006, 2007, 2008, 2009, and 2010. Although IOR is more commonly used than IOL, it aggregates IOp and total inequality into a single index. Therefore, an increase in the IOR could be due to an increase in the IOL or a decrease in total inequality (or both). Hence I report total inequality and the IOL separately, together with estimates for IOR in the Appendix.

[5]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 $E(\hat{Y}_i|C) = exp(\hat{\alpha} + \hat{\beta} C_i) \cdot exp(\frac{1}{2}\hat{\sigma}^2)$.

2.3 The lower bound approach

The lower bound approach is one of the most commonly used methods to measure IOp (see e.g., Balcázar (2015); Ramos and Van de gaer (2016)). The goal of this approach is to measure the influence of observed circumstances on a given outcome using equation 6 and then to determine the role played by said circumstances on total inequality. The stronger the influence of the observed circumstances, the higher IOp is.

However, this approach can only account for the importance of observed circumstances. If any circumstance is not included (either by choice or because it is unavailable in the dataset), this approach will not account for it. As some circumstances will inevitably be omitted, the resulting estimation is a lower bound of the true level of IOp. In fact, all omitted circumstances will be included in the error term, together with efforts:

$$\hat{u}_i = log(Y_i) - log(\hat{Y}_i) = log(Y_i) - (\hat{\alpha} + \hat{\beta} C_i). \tag{10}$$

The use of a lower bound estimate on its own 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). Providing lower bound estimates by themselves shows at best an incomplete picture of unequal opportunities.

I estimate the lower bounds of IOp using gender, both parents' education and main activity, the father's occupation, and household composition, all at age 15. This set of circumstances is 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 that the upper bound can pick up.

2.4 The upper bound approach

2.4.1 The two-step approach to deriving upper bound estimates of IOp

The upper bound approach to measuring IOp is a two-step process: estimating the circumstance set, and using the circumstance set to measure IOp. The first step involves the use of long term panel data to capture all time invariant characteristics for each respondent, which are then treated as the circumstance set. 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. Using the estimated circumstance set, the second step is to quantify its role in income in the same way as for the lower bound, but now with a differently defined set of circumstances.

The circumstance vector is obtained from a fixed effect regression that uses all years available, except for the year in which IOp is measured. In the case of Niehues and Peichl (2014), this means at least 5 consecutive years (an average of 7 years) 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.

The fixed effect regression follows from the structural model described in equation 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:

$$log(Y_{it}) = \alpha + \eta_i + u_t + \varepsilon_{it}. \tag{11}$$

If properly estimated, the predicted fixed effect $\hat{\eta}_i$ will capture all time invariant factors. Following equation 5, the fixed effect will be equal to the sum of the effects of time invariant circumstances and residual time invariant effort, on log income:

$$\eta_i = \tilde{\beta} C_i + \tilde{u}_i \tag{12}$$

By using the predicted fixed effect $\hat{\eta}_i$ as the measure of circumstances, we capture all circumstances ($\tilde{\beta} C_i$) together with factors that might not necessarily be considered circumstances ($\tilde{u}_i$). These factors could include determinants of labour market outcomes, such as "*long-term motivation and work effort*" (Niehues and Peichl, 2014). Nonetheless, under a strong definition of inequality of opportunity – one where most intergenerational transmission channels are considered unfair – factors such as long-term motivation could be considered a circumstance or at least strongly determined by circumstances (Roemer, 2004; Cohen, 2009). The upper bound of IOp would reflect the largest possible effect of circumstances on income inequality, if we were to consider all time invariant factors to be circumstances.

I use the predicted fixed effect ($\hat{\eta}_i$) to measure the upper bound of IOp. For a given year s, which is different from the ones used to estimate the fixed effect (i.e.

$s \neq t = \{1, 2, 3\}$), IOp is estimated using equation 13:

$$log(Y_{is}) = \psi\hat{\eta}_i + \omega_{is}. \tag{13}$$

If most circumstances are unobserved in the data, the upper bound estimate of IOp can better describe the total role of circumstances in income inequality than the lower bound. However, it is unable to provide further details on the actual circumstances. While the lower bound estimate of IOp fails to account for all circumstances, it can separately identify the role of each observed circumstance. An upper bound estimate of IOp cannot inform on what circumstances are being included, nor their relative importance.

The upper bound approach has two additional assumptions in order to be able 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 paper to obtain upper bound estimates of IOp over time, I also evaluate whether these assumptions hold empirically. The results are discussed in detail in the appendix (section A.1), and they show that these assumptions hold for the countries in the sample.

The upper bound approach is particularly data demanding, as it uses long term panel datasets to estimate the fixed effects. In fact, 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 my paper 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.

2.4.2 Using the upper bound approach on a short rotating panel

In order to obtain upper bound estimates of IOp for EU SILC countries, and to be able to compare them with the available lower bound estimates, I make two departures from the approach proposed by Niehues and Peichl (2014). The first is to use a shorter panel. 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 a 'prospective' rather than a 'retrospective' approach to estimate the fixed effects.

The first departure, using a shorter panel, allows me to use the EU SILC dataset, as its rotating panel structure follows each respondent for up to 4 consecutive years. The second departure is a choice rather than a requirement, but has the advantage that it allows me to estimate lower and upper bound estimates of IOp for the same two years: 2005 and 2011. The second departure should not result in econometric issues, as the fixed effect (if properly estimated) should capture the same information in both cases.

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). This problem arises from the fact that fixed effects are computed for each respondent using an average over time. Following equation 11, the fixed effect is estimated as:

$$\hat{\eta}_i = \overline{log(Y_i)} - \hat{\alpha} - \overline{u}. \tag{14}$$

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

Unlike the α and β parameters, the fixed effect η_i might not be consistent when N grows (for a given T). Each new observation results in a new η_i parameter, and therefore information does not accumulate on η_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 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. I show in section 5.1 that these departures make little difference to the upper bound estimates of IOp.

3 European data: The cross sectional and longitudinal EU-SILC

This paper 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 (Jäntti et al., 2013). 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 results for the first rotation group in the cross-sectional sample, the same group I use to estimate the upper bound IOp in section 5.2.

I use the cross-sectional sample for the years 2005 and 2011 in order to measure the lower bounds of inequality of opportunity, as these two years include a secondary questionnaire with retrospective information. The choice of circumstance variables is detailed in table 1. To derive the upper bound, I use the longitudinal samples from 2008 to 2014 to estimate IOp from 2005 to 2011.

Table 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 at age 15
d. Technicians	d. Retired	
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	

Although there are several potential circumstances in the EU SILC, we have to limit their number based on two criteria. First, I only include circumstances that are available in both years. This results in the exclusion of circumstances such as the migration status of the parents, which only appears in 2011, or having experienced financial difficulties at a young age, as the possible answers differ across years. Second, the sample size requirements exclude circumstances with low response rates, specifically the occupation of the mother. The final set of circumstances described in table 1 is similar to previous studies that used the same dataset, although not directly comparable. For example Ramos and Van de gaer (2017) include place of birth and mother's occupation, while Hufe et al. (2018) include immigration status but exclude household composition.

The goal of this paper 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, and 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 (Niehues and Peichl, 2014), or including more countries using not necessarily comparable datasets (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).[6] I focus on individuals with positive income, aged 25 to 55. The resulting sample includes 24 countries for which it is possible to estimate upper bounds of IOp between 2005 and 2011. The 24 countries include all countries for which we can estimate lower bounds of IOp for 2005 and 2011, as shown in table **??** in the appendix. 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.

Table 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 sizes vary greatly across countries, going from 367 to almost 5,000 in 2011. Tables **??** and 3 in the Appendix show the number of observations for the cross-sectional and longitudinal sample for all countries - for all countries in the EU-SILC.[7]

Both the cross-sectional and the longitudinal sample lose around 50% of respon-

[6] 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).

[7] Table **??** includes an additional column to show the number of respondents with non-missing data in the circumstance variables.

Table 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

dents, mainly because of the age range. However, it is the cross-sectional sample that loses the larger share of respondents due to the inclusion of 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 over 1,000 repetitions, using random samples with replacement. footnoteDue to the few observations for some countries, researchers have proposed limiting the number of circumstances – via machine learning methods – in order to avoid overfitting (Brunori et al., 2018). I will not explore this method as I am mainly concerned with the upper bound estimates.

4 Upper and lower bound IOp estimates

4.1 Inequality of opportunity by country: 2005 to 2011

The results are shown in two different ways. First, I show all time trends for each particular country (figures 1a and 1b). Secondly, I show all estimates – lower bounds, upper bounds, and total inequality – for all countries, separately for the years 2005 and 2011 (figures 2 and 3). All results are for the IOL using the MLD index. The results for the IOR are shown in the Appendix (figures 14a and 14b).

Figures 1a and 1b show that the upper bound and total inequality appear to move together, 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, showing that IOp could be as high as total inequality.[8]

Over time, countries show a relatively stable level of IOp. Between 2005 and 2011, 10 out of 23 countries showed a decrease in their upper bound estimate, but most of the changes were small.[9] 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).

We see stark differences in the upper bound estimates of IOp. Figure 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 4.2.

The results 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 equation 9 in page

[8]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 14a and 14b in the Appendix..

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

Figure 1a: Inequality of Opportunity level (IOL) by country (MLD)

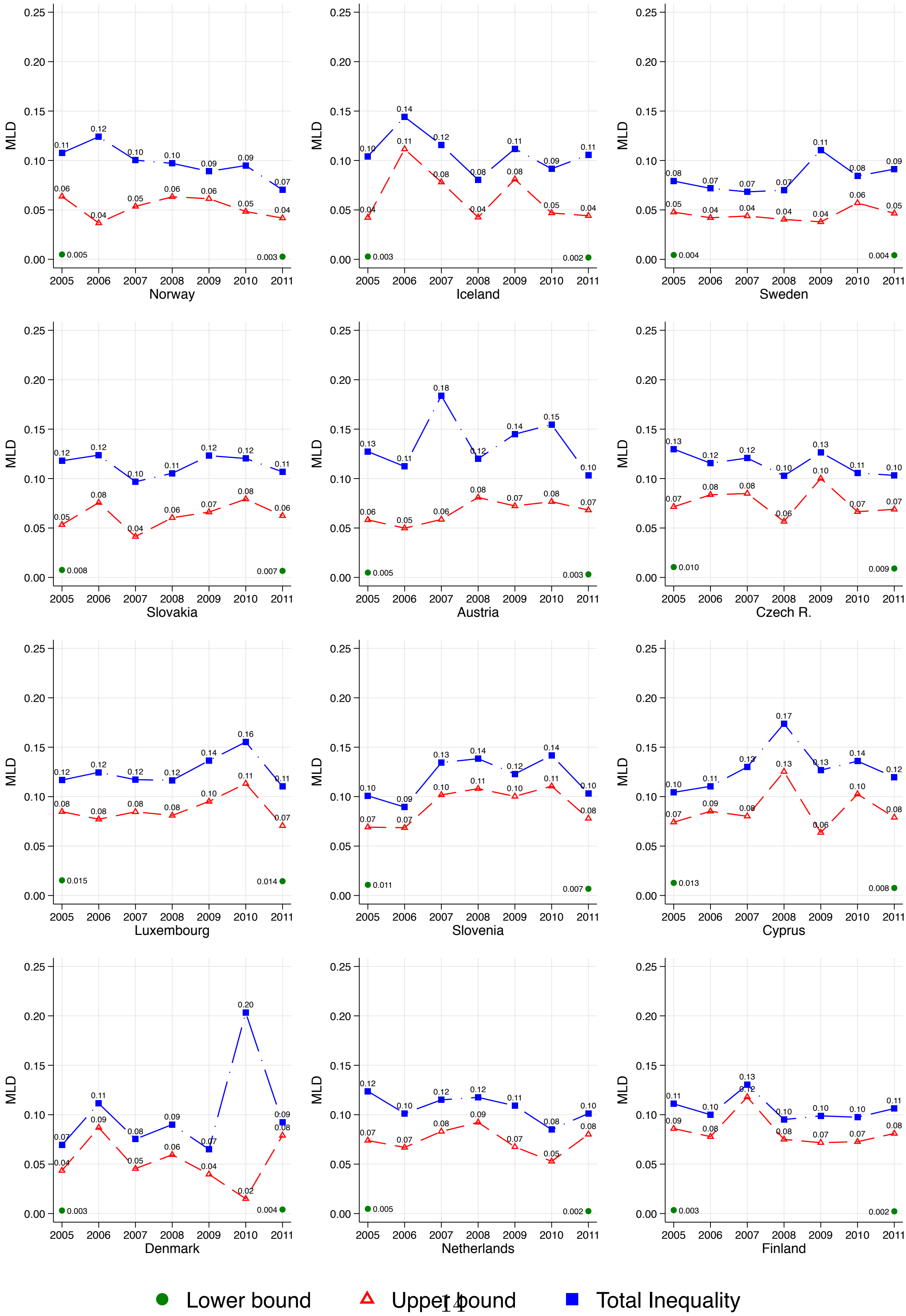
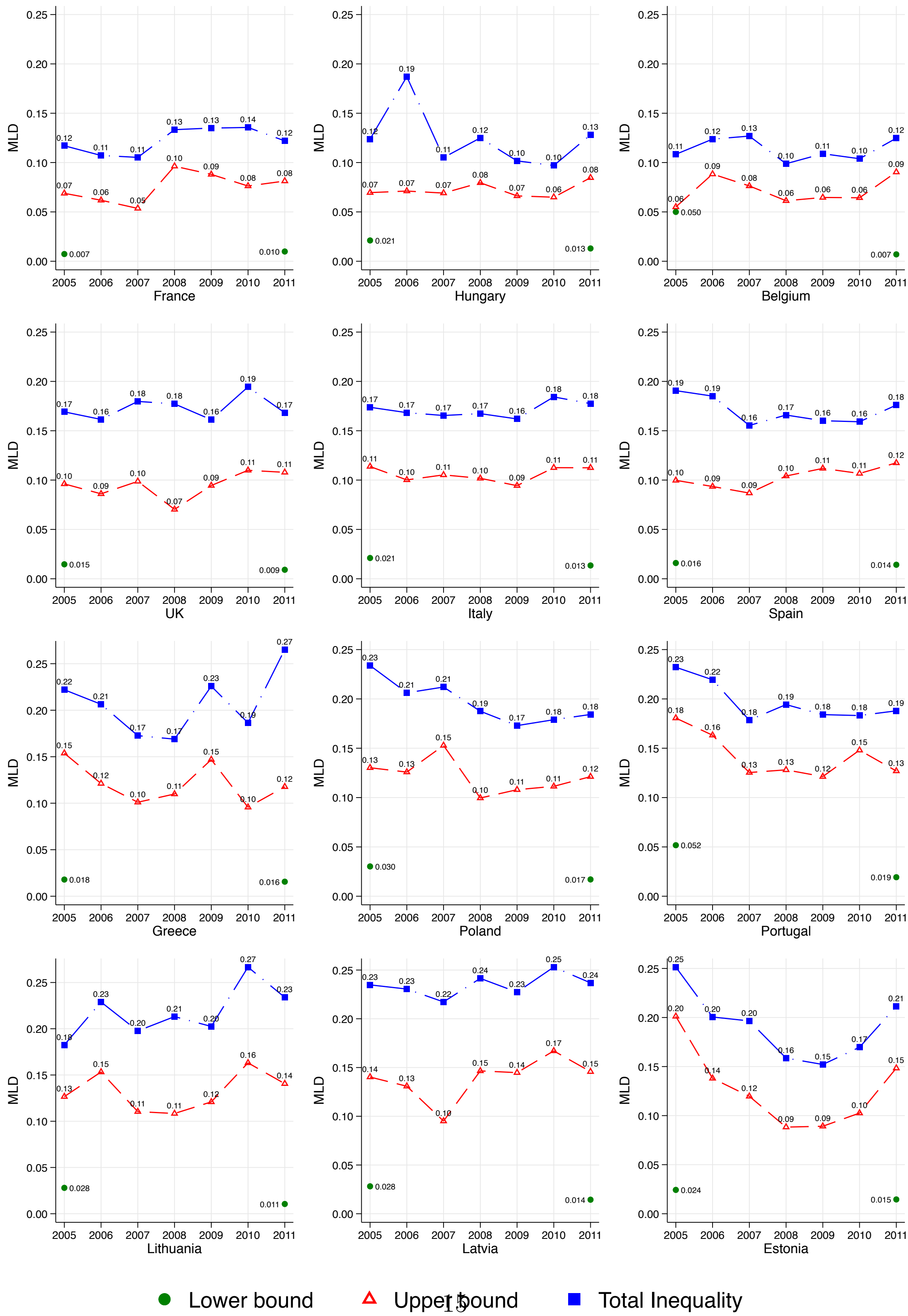
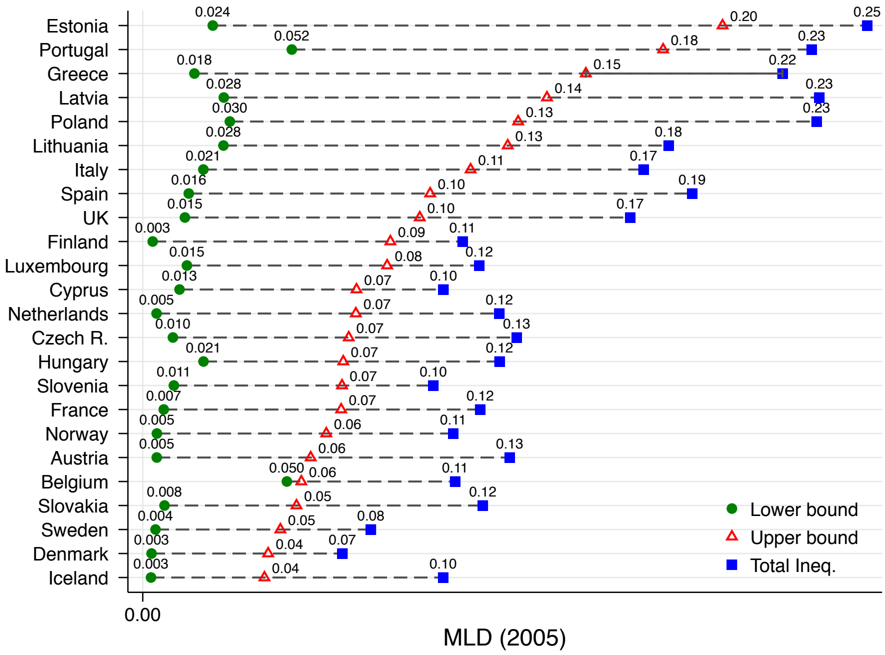


Figure 1b: Inequality of Opportunity level (IOL) by country (MLD)



5), and the corresponding results are shown in the Appendix (figures 14a and 14b on pages 38-39). The lower bound IOp estimates range from 1.7% of total income inequality for Iceland to 11.6% for Luxembourg, while the upper bound estimates go from 41.7% for Iceland or 73.8% for Iceland, to 85.6% of 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 circumstances play a crucial role in determining inequalities.

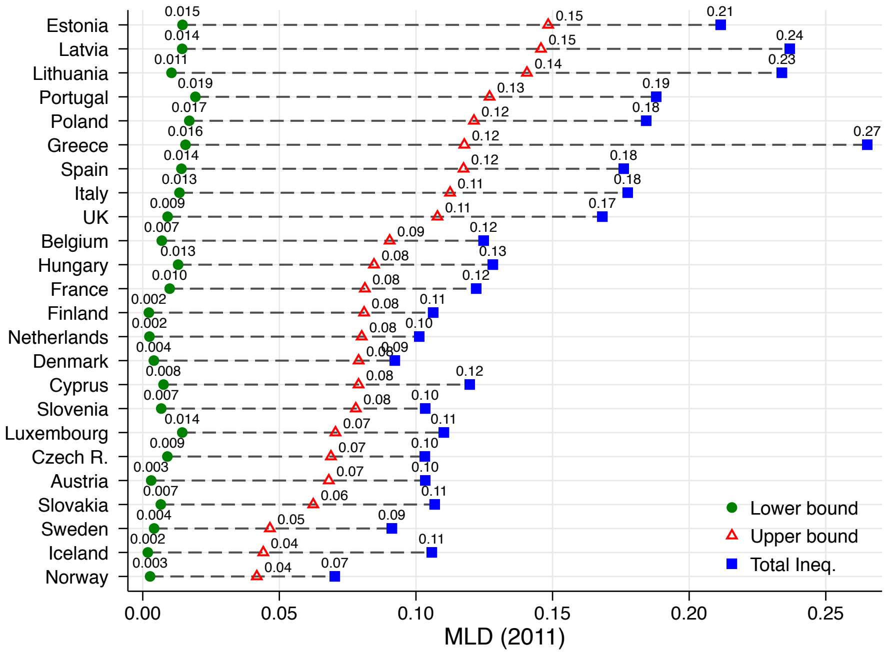
Figure 2: Inequality of Opportunity by country (2005)



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 two bounds are somewhat related, but they do not result in the same country rankings nor show the same trends over time. Furthermore, the upper bound can be estimated in cases where there are no circumstance variables available, but

Figure 3: Inequality of Opportunity by country (2011)



provides no information on what circumstances are being omitted by the lower bound estimates. The following sections explore these issues.

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 4 shows the lower bound estimates (x-axis) and the upper bound estimates (y-axis) for all countries in 2011. The countries are ranked from lower IOp to higher, 1 being the lowest. The diagonal line is the 45° degree line, where countries would be if the rankings did not change. The two dashed lines show changes of at most 5 positions in the rankings. Countries below the 45° line rank worse in the lower bound estimate, while countries above the 45° line rank worse in the upper bound estimate. As the two dashed lines show, most countries differ by at most 5 positions between the two rankings. Finland, the Netherlands, and

Lithuania have better positions in the lower bound ranking, so when we account for omitted circumstances, their relative position worsens. The Czech Republic and Luxembourg have better positions in the upper bound rankings, improving their relative position. These differences show that the omitted circumstances in 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 4: IOp ranking positions for 2011

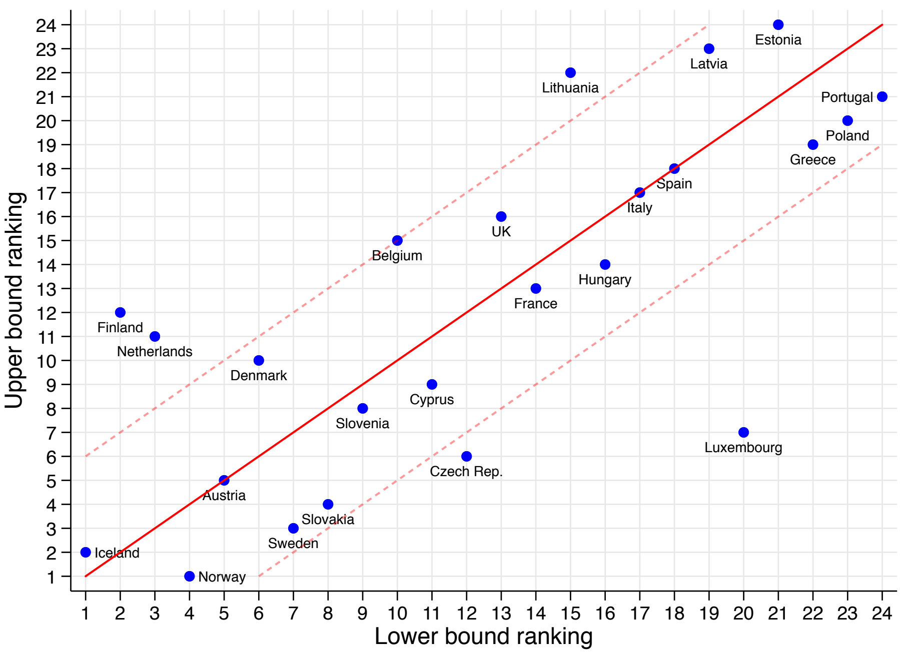
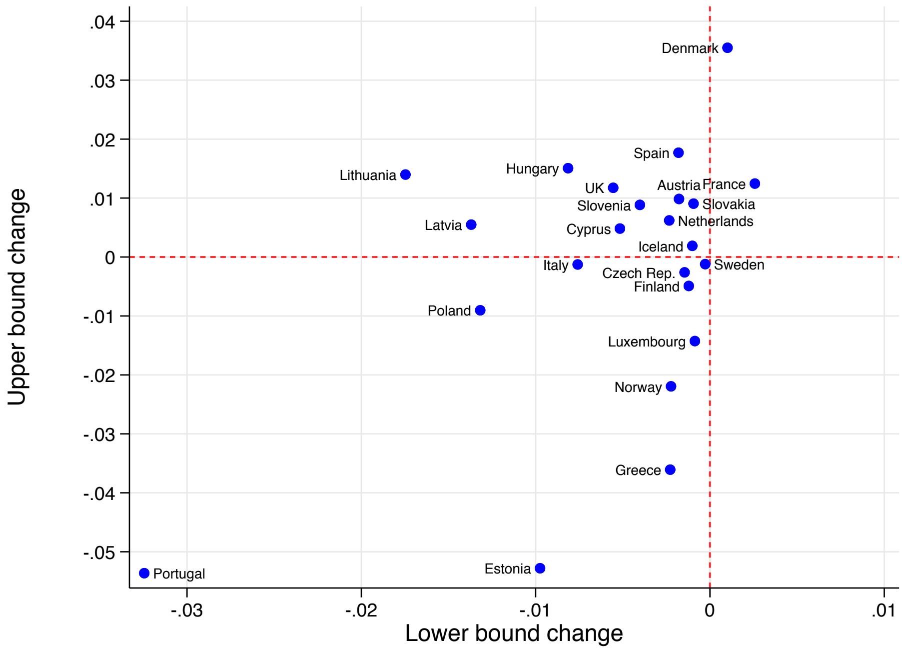


Figure 5 shows the difference between the 2011 and 2005 levels of IOp, both for the lower bound (x-axis) and the upper bound (y-axis) estimates. The red dashed lines indicate no difference between 2011 and 2005. Several countries show an increase in their upper bound estimate of IOp, but only 2, Denmark and France, have an increase in the lower bound estimate. Around half of all countries (12 out of 23) move in the same direction when looking at the lower bound and upper bound estimates.

The upper bound estimates of IOp are not only a 'larger' lower bound, they provide new information, both over countries and over time. The distinct importance of omitted circumstances is what drives these differences, and although we cannot say what these circumstances are, accounting for them matters when making IOp comparisons.

Figure 5: Changes in IOp between 2005 and 2011



4.3 The gap between the upper and lower bound

The gap between the upper and lower bound estimates of IOp can be interpreted as a measure of the relative importance of omitted circumstances, as it accounts for all factors captured by the upper bound, but not included in the lower bound.

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 IOL index defined in equation 8, the gap is computed as the difference between the level of IOp for each bound:

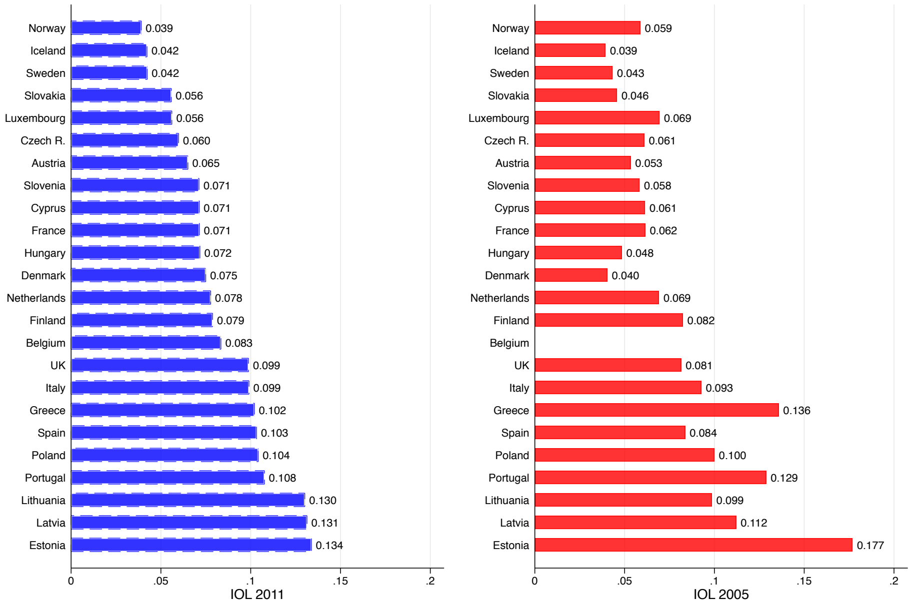
$$Gap = I(\{Y_i^{UB}\}) - I(\{Y_i^{LB}\}). \tag{15}$$

The difference between the two is the level of IOp attributed to all unobserved and time invariant factors. As discussed before, most of these time invariant factors are expected to capture involuntarily inherited characteristics, and should be considered circumstances.

Figure 6 shows the size of the gap for each country for 2005 and 2011, measured in points of the MLD coefficient and sorted by the 2011 gap. The gap goes from

0.4 to 0.18 points of the MLD.[10] Relative to the lower bound, the size of the gap is substantive, representing from 2.3 times the lower bound for Hungary in 2005, to over 30 times to the Netherlands or Finland in 2011.

Figure 6: Gap between lower and upper bounds (IOL)



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 have estimates using 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 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. In my paper, gaps using the MLD index range from 3.9 to 13.4 in 2011, with only 6 countries having a gap of 6 or less. Relative to the results for the US and Germany, the difference in the size of the gaps between their paper and this paper is explained by the lower bound estimates. While the upper bounds are similar, they are able to account for a larger set of circumstances and therefore their lower bound estimates are larger than mine.

Figure 7 shows the gap size by country and year. For 2011, Estonia, Latvia, and

[10] I exclude Belgium in 2005, as it shows a noisy and non-significant lower bound estimate of IOp.

Lithuania show gaps of 0.13 points, suggesting that omitted circumstances play an important role in determining IOp. Omitted circumstances are not as important in Norway, Iceland, and Sweden, with gaps of 0.4 points. Interestingly, countries with small gaps include both high upper bound estimates of IOp countries, like Luxembourg and the Czech Republic, and countries with low upper bound estimates of IOp, like Norway and Sweden. Independent of the level of IOp, these countries share the fact that observed circumstances explain a substantial part of it.

Unlike the lower bound estimate of IOp, the upper bound estimate cannot provide information on the individual circumstances that comprise it. The same is true for the gap, which cannot be decomposed. Given this restriction, I explore the correlation between the gap and total income inequality over countries, as a way of understanding the sources of variation in the omitted circumstances included in the gap.

Figure 7: Gap between bounds vs. total inequality

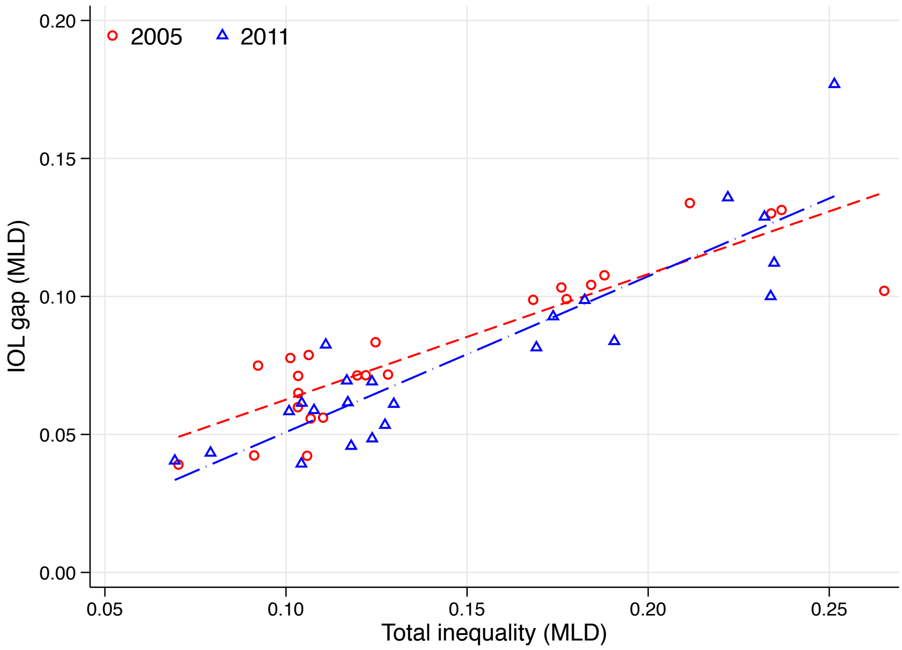


Figure 7 shows a scatter plot between the gap and total inequality, separately for 2011 and 2005, and the linear fit regression line for each case. The figure shows that there is a positive relationship between the gap and total income inequality for both years. A larger gap between the upper and lower bounds of IOp is positively correlated with a higher level of inequality.[11]

Figure 8: Reduction in the gap when including new circumstances

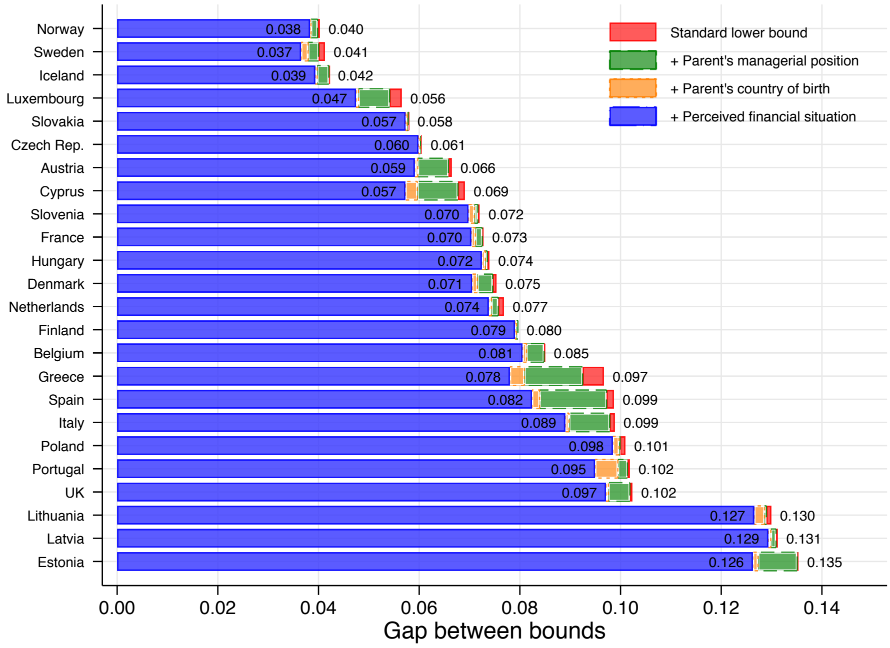


Figure 7 shows a positive relationship between unobserved time invariant determinants of income, and the distribution of that income . This relationship is similar to the one shown in the 'Great Gatsby' curve, which describes the negative relationship between intergenerational mobility and inequality (Corak, 2013). The larger the importance of unobserved circumstances, the larger the level of inequality of outcomes.

Further research should try to understand what is being captured by the upper bound that the lower bound estimates of IOp are not capturing. In order to explore this issue, I use the 2011 cross-sectional EU-SILC to study how the gap decreases when I include additional circumstances that are not included in the analysis, as shown in figure 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

[11]This result holds for a regression with country and year fixed effects, and clustered standard errors at the country level. A similar result is shown using a meta analysis of several different estimates of IOp for different countries (Brunori et al., 2013).

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 could make ends meet.

Figure 8 shows the role played by these new circumstances in reducing the gap. By including the new circumstances, the gap is reduced by between 0.001 points and 0.018 points, with an average of 0.005 points of the MLD index. The decrease is limited, as the new circumstances do not explain a substantial part of the omitted circumstances, but it is an 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, as understanding what is behind the gap will provide a relevant insight for the literature on inequality of opportunity.

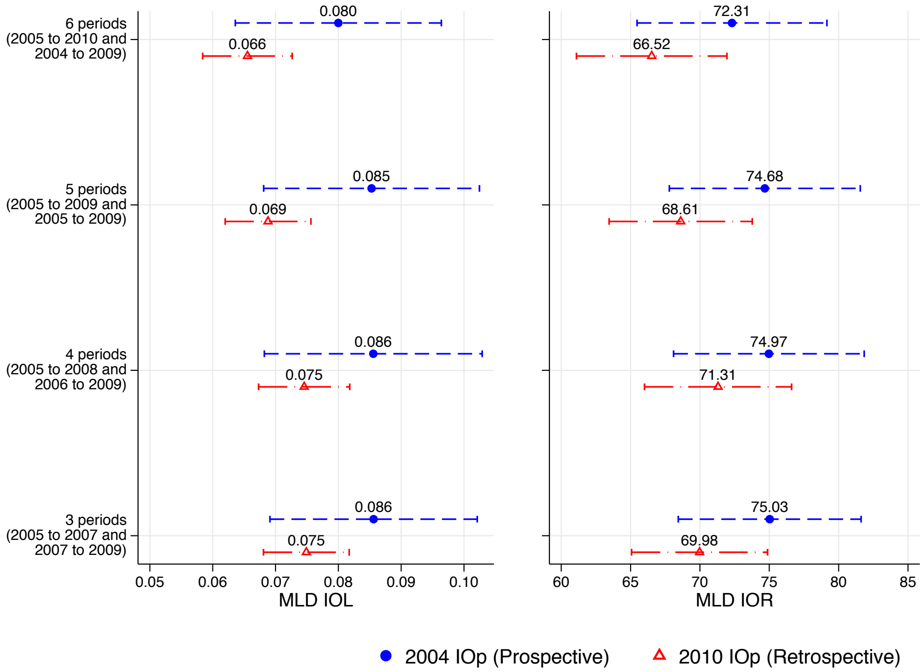
5 Robustness checks

This section explores two departures from the methodological assumptions described in section 2. The first subsection explores how upper bound estimates change when we use a larger period of time to estimate fixed effects. The second subsection explores how lower bound estimates change when we use a sub-sample of the cross-sectional data to improve comparability with the longitudinal dataset. These two departures bridge the space between the original methodology and this paper's application of it, showing that the results do not change substantially with respect to my previous estimates.

5.1 Upper bound estimates and the choice of time window

In order to estimate the upper bounds of IOp, I follow Niehues and Peichl (2014) with two modifications. These two modifications relate to the estimation of the fixed effect in the context of a short panel. On the one hand, while they used the years 2003–2008 to estimate the fixed effect to measure IOp in 2009, I use the years 2010–2012 to measure IOp in 2009, what I call a prospective approach, to contrast with their retrospective approach. On the other hand, they used the

Figure 9: Upper bound IOp for Luxembourg



German SOEP survey, which allowed them to include seven waves to estimate their fixed effects, while in this paper I use only three years.

To explore these departures, I exploit the fact that the 2010 survey includes 7 waves of data for Luxembourg (2004–2010). Using this sample, I estimate IOp for 2004 using my prospective approach (i.e., years 2005 to 2010), and for 2010 using the retrospective approach (i.e., years 2004 to 2009). I also change the number of periods to estimate the fixed effects for both approaches, going from three (as in the rest of the paper) to six periods.

Figure 9 shows the estimates together with their bootstrapped confidence intervals. The y-axis shows the number of periods for each case. For example, the last row shows two estimations using 3 periods: 2004 was estimated using the period 2005–07, while 2010 was estimated using the period 2007–09. The figure to the left shows the results 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. There is a slight decrease in the level of inequality when more periods are included, going from 0.086 to 0.08 for 2004, but they all fall within their confidence interval. The same is true for 2010, where the IOL goes from 0.075 to 0.066.[12] We see something similar for the IOR, with the share of inequality explained by circumstances decreasing slightly for shorter time periods. The choice of approach (retrospective or prospective), as well as the number of periods considered, appear to make almost no difference to the upper bound estimates of IOp.

The 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 as large as the EU average, as well as a higher GDP growth. On the other hand, Luxembourg has levels of inequality and poverty close to the EU average.[13] 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 (Jäntti et al., 2013, pp.189-202).

5.2 Lower bound estimates for a differently defined cross-sectional sample

The upper bound approach requires respondents with at least four waves of data, which constrains the available data, whereas the lower bound estimate uses all respondents. These two datasets cannot be merged to use a common sample. The fact that different subgroups of the survey are being used in each case presents a potential issue of comparability.

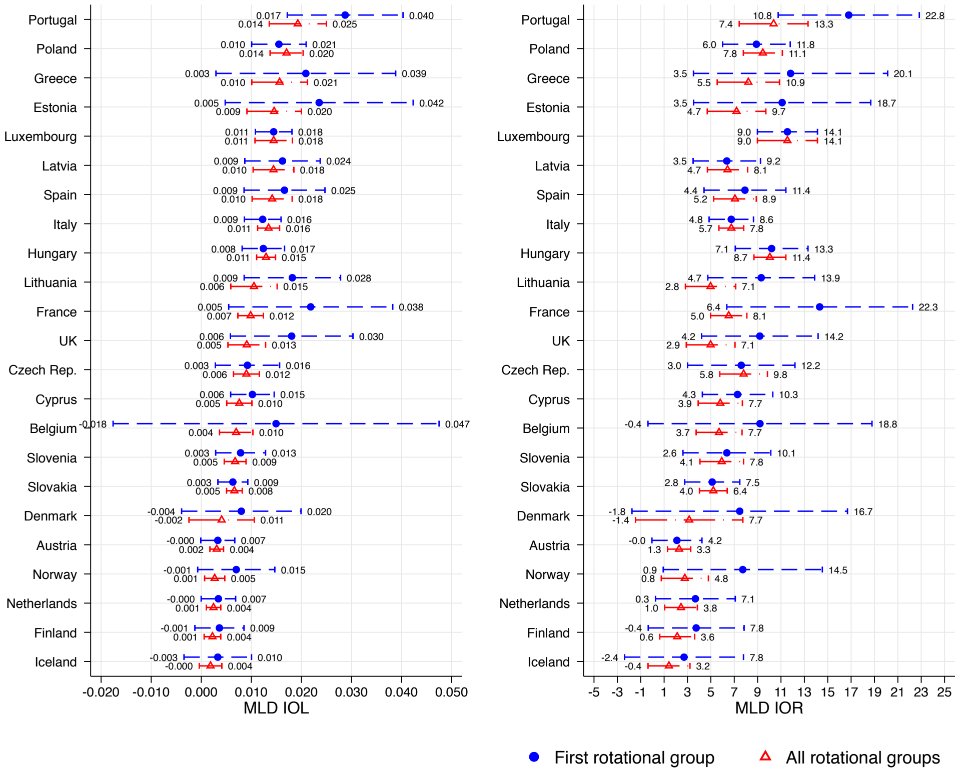
To derive estimates based on a more consistently defined sample, I re-estimate all lower bound estimates of IOp for 2011 using only the first rotational group of that year, i.e., the respondents that were surveyed the first year, and that will be interviewed at least three more times. Given that I use 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. 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.

Figure 10 shows the results for all countries of using both the complete cross-

[12]The 2010 estimate for Luxembourg is not the same as in figure 1a. This is because I am using a 'retrospective' approach here to estimate IOp. However, their 95% confidence intervals overlap.

[13]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.

Figure 10: Lower bound IOp (2011) for the complete and first rotational samples



sectional sample and the first rotational group. The first rotational group excludes Sweden as there are no available observations in that group. Figure 10 shows 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 results 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.

6 Discussion

Inequality of opportunity has gained national and global recognition as an issue that needs to be addressed. However, we know very little about the true extent of unequal opportunity as most methods only show its lowest possible level. I address this problem by providing the upper bounds of inequality of opportunity for income, for 24 European countries between 2005 and 2011. My results show 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 paper to provide the upper bounds for countries over time, which are complemented by the lower bounds for the years 2005 and 2011.

My results show that IOp could 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. The lower bound estimates range from 0.002 for Iceland and Finland, to 0.036 for Romania. The upper bounds range from 0.042 for Norway to 0.203 for Romania. In relative terms, the lower bounds explain between 1.4% (Iceland) and 17.4% (Romania) of total inequality and 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.

The upper bound estimates of IOp are not just a larger version of the lower bound estimates; they provide new insights about IOp trends. Although most countries remain in relatively similar positions, 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 –the ones captured in the upper bound estimate but not in the lower bound– differ in their role across countries.

As the upper and lower bound estimates of IOp do not convey the same information, I explore the gap between them as a measure of omitted circumstances in the latter. I show that the gap varies greatly across countries, ranging from 0.04 points of the MLD index for countries like Norway and Sweden, to 0.16 for Romania (or from 1.15 to 4.9 standard deviations). I also show that this gap is positively correlated with inequality of outcomes, suggesting that omitted circumstances can explain part of the relationship between inequality of outcomes and IOp. Using the few circumstances that are available in the EU-SILC, I provide preliminary results 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 provides 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 relationship is an important one and suggests that both inequalities should be addressed together. In fact, IOp and inequality of outcome can be causally linked over generations: as Atkinson (2015) mentions, inequality of outcomes today affects IOp for the next generation. If we care about IOp, we need to address inequality of outcomes as well.

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A Appendix

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 13 and the $\tilde{\beta}$ coefficient in equation 12 should hold constant for every year. I explore whether these assumptions hold empirically, by estimating both parameters for every country, over time.

Results are shown in figure 11 for the mean fixed effect and on figure 12 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 12. 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 11 and for Estonia in figure 12. Overall, both assumptions appear to hold in a reasonable manner.

Figure 11: Mean Fixed Effect by country

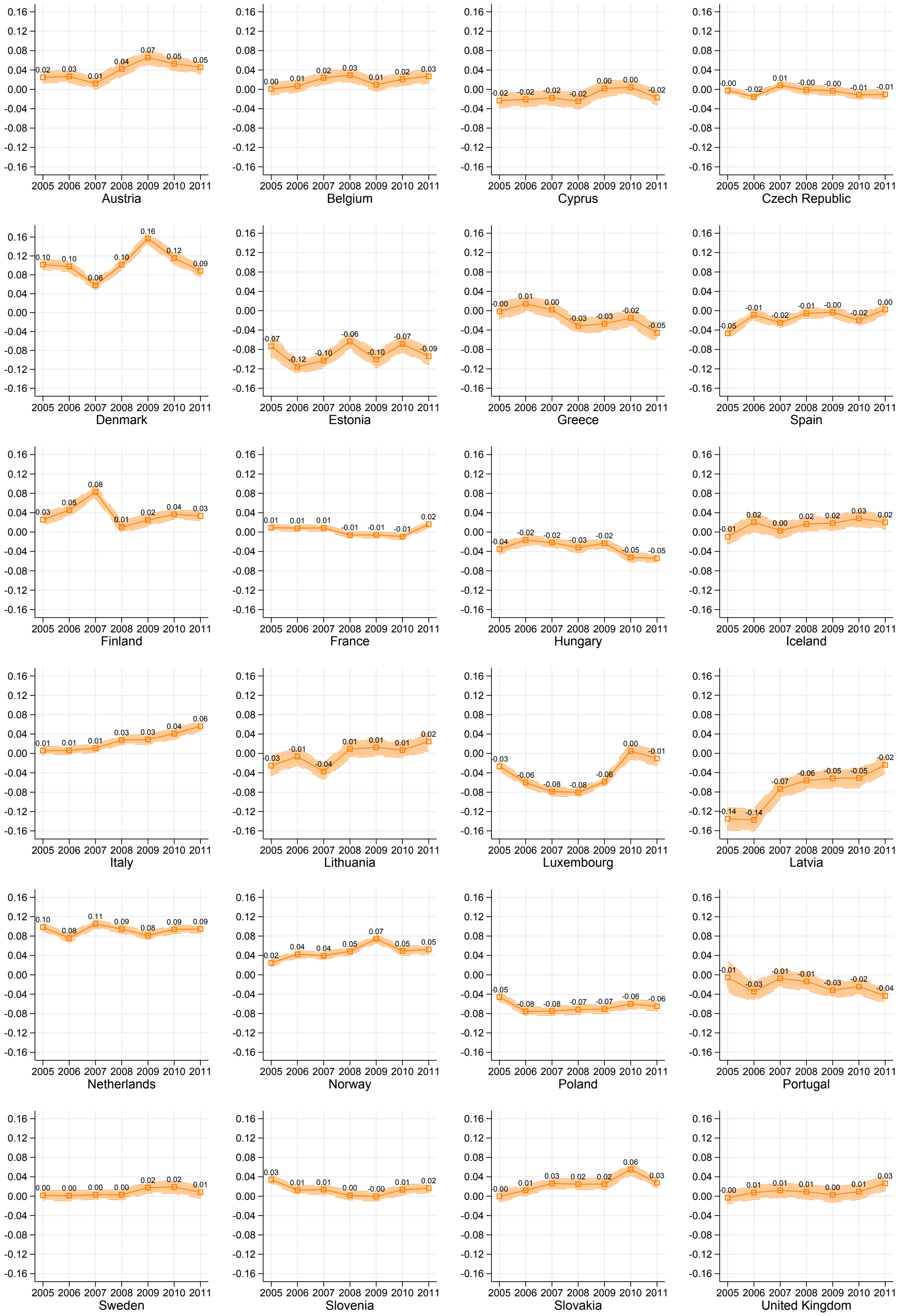
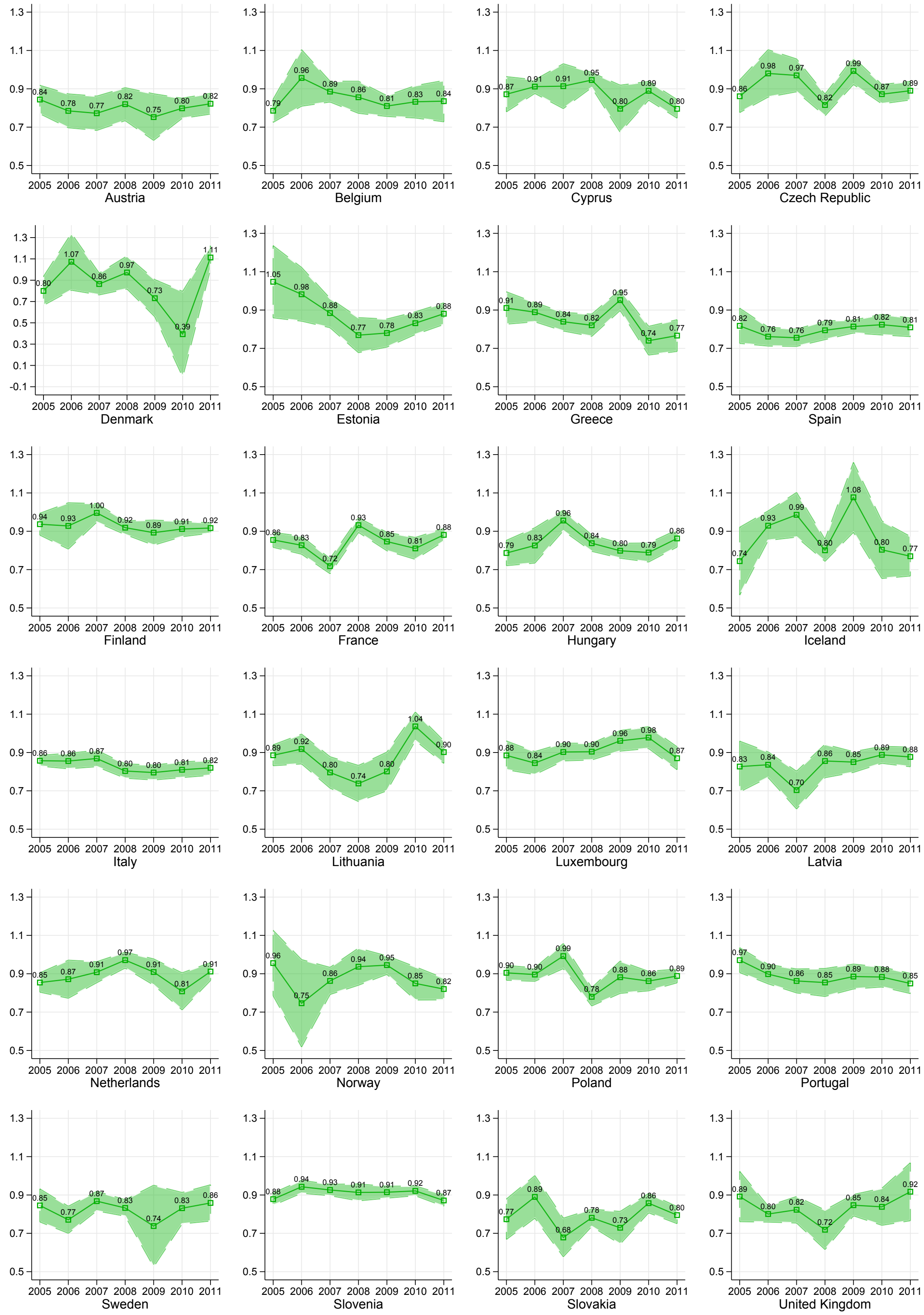


Figure 12: Coefficient of the circumstance variable by country



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

A.2 Number of observations by country

Table 3: 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
Bulgaria	-	827	888	1,466	1,578	1,199	1,154
Switzerland	-	-	-	-	-	-	1,356
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
Ireland	426	384	-	-	353	378	367
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
Malta	-	768	735	705	1,002	980	1,077
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
Romania	-	-	1,948	1,806	1,784	1,922	1,740
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

A.3 IOp estimates with 95% confidence intervals

Figure 13a: Confidence interval for IOL by country (MLD)

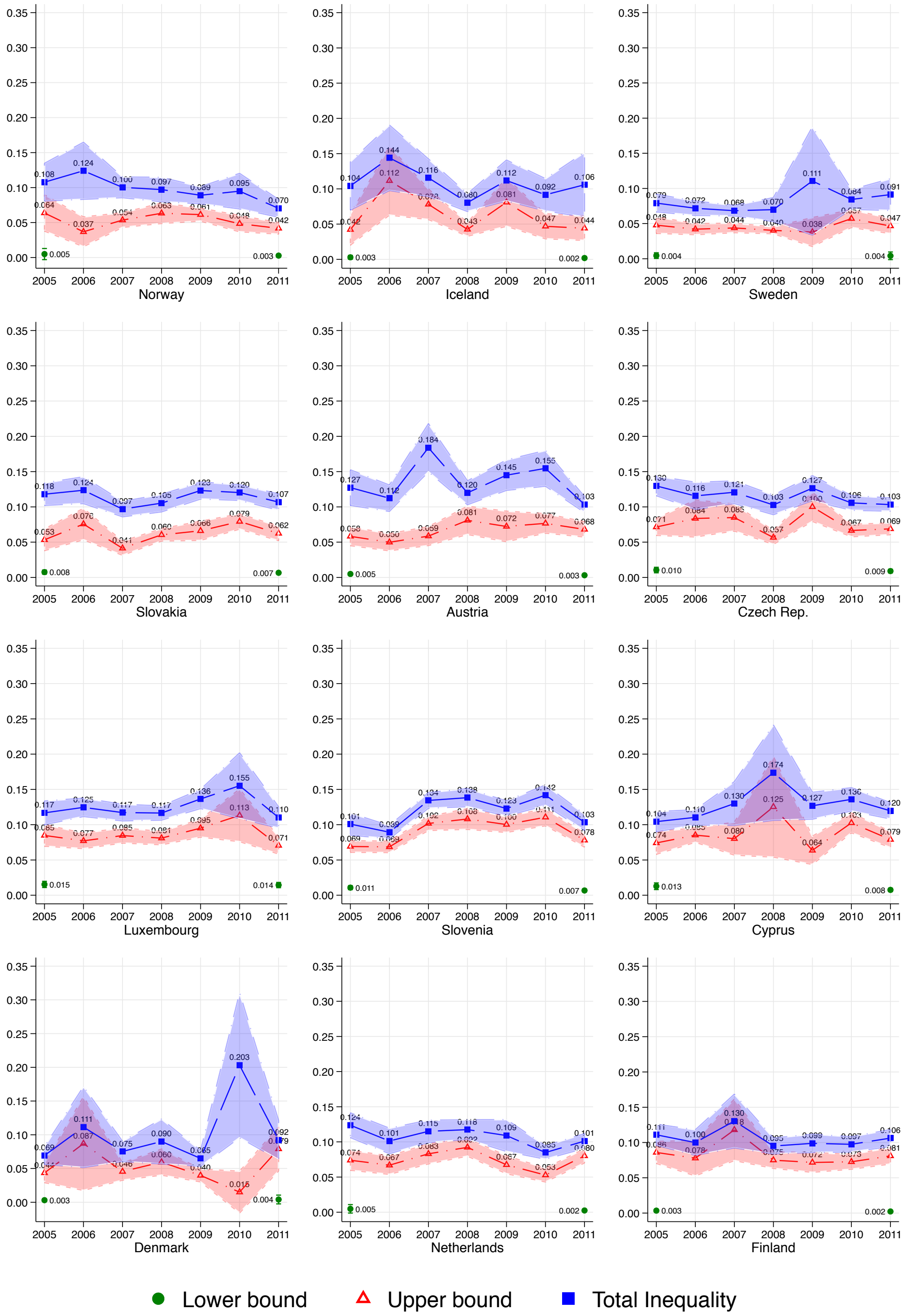


Figure 13b: Confidence interval for IOL by country (MLD)

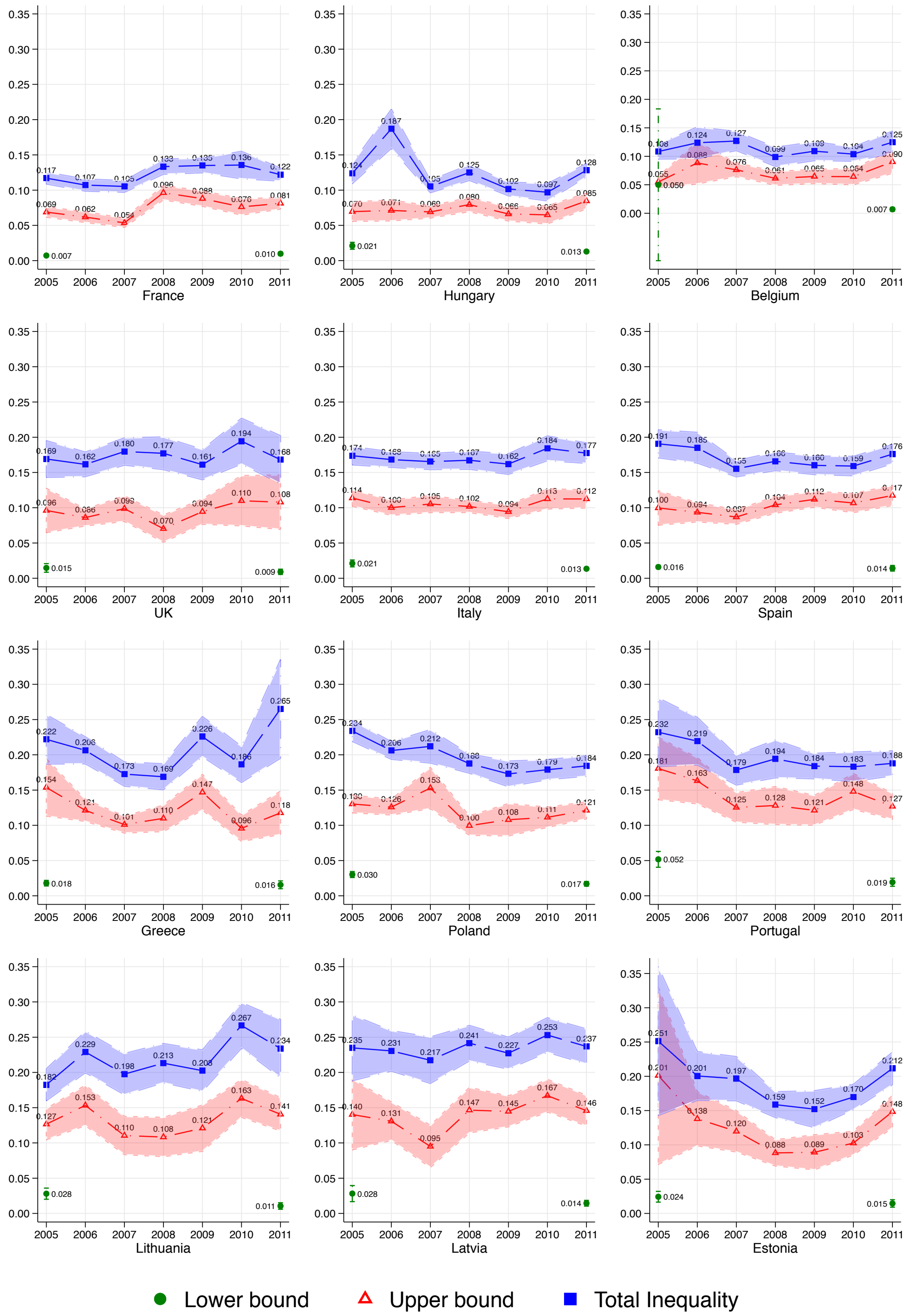


Figure 14a: Confidence interval for IOR by country (MLD)

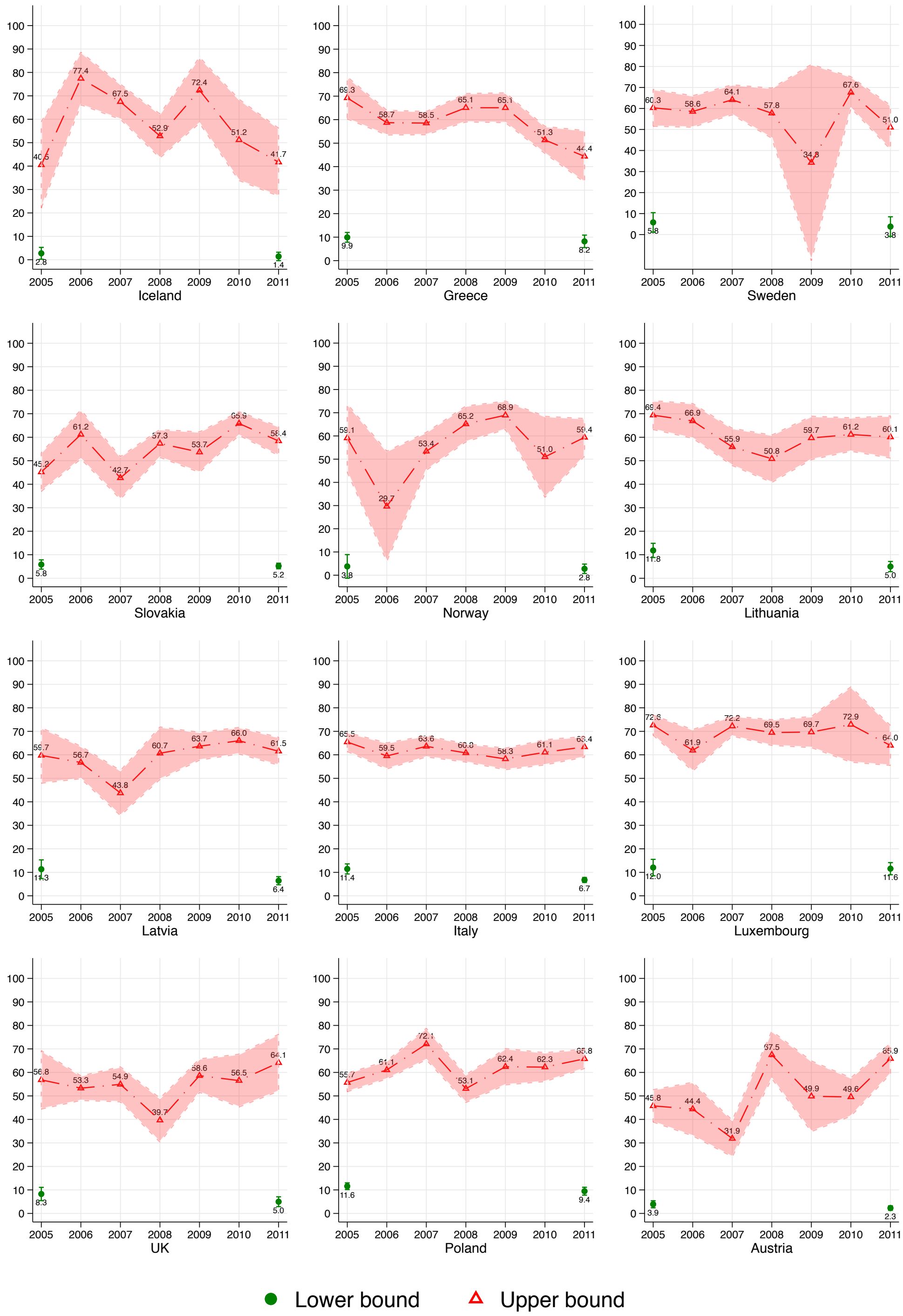


Figure 14b: Confidence interval for IOR by country (MLD)