Pension Reforms and Inequality in Europe*

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Abstract. This paper studies the impact on inequality of pension reforms implemented in 14 European countries between 1990 and 2018. We construct a novel dataset identifying major pension reforms and distinguishing several different types of reforms, thus allowing a high degree of consistency and disaggregation. We identify 54 major and 75 minor pension reforms in the countries involved. We find that major pension reforms significantly affect income inequality by increasing the ratio of top 10% income to median income. On average, reforms of the retirement age and benefits increase inequality, while reforms of contributions and structural aspects of the system reduce it. The increase in inequality seems to be driven by the interaction of the reforms' mechanical effects on accumulation and incomes and the behavioural response of households.

Keywords: European economies, pension reform, inequality
JEL Codes: H55, D63

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1 Introduction

In recent years, European countries have consistently reformed their pension systems. These reforms have aimed at reducing the fiscal costs of population ageing while improving the intergenerational equality of the pension system. In other words, the goal of the reforms is to bring the pension system back on a sustainable path, thus lowering the cost for future generations and creating some fiscal space that can be used to boost public investments or smooth the business cycle. Against this background, some population groups often oppose these reforms, claiming that reductions in the generosity of the system increase inequality. Typically, this often leads governments to soften (or "retrench") the reforms to capture the votes of workers close to retirement, eventually undermining the fiscal sustainability of pension systems that the reform originally aimed at improving.

Despite its relevance for this public debate, there is only scant evidence on the link between pension reforms and inequality. What we know about the effect of pension reforms on inequality is often derived from structural models (e.g., Etgeton, 2018; Andersen et al., 2022) calibrated on microdata for single countries. Despite providing relevant information, these studies are generally limited by country-specific elements in the spectrum of reforms that they can analyse and in their external validity. However, the absence of extended datasets on pension reforms has generally prevented empirical investigations from seriously addressing the effects of pension reforms on inequality.

This paper aims to fill this void. To this end, a key contribution is to construct an original dataset on pension reforms in 14 large European countries (Austria, Belgium, Denmark, Finland, France, Germany, Greece, Ireland, Italy, the Netherlands, Portugal, Spain, Sweden and the United Kingdom) spanning the period 1990–2018. The database relies on narrative identification of the major pension reforms in these countries, which allows a high degree of consistency and disaggregation. Thus, we can investigate several characteristics of the reforms (e.g., type, sign, phasing-in period). Using our dataset, we can pinpoint the effects of pension reforms on income inequality and estimate the role of the various elements composing a pension reform while overcoming country-specific confounding factors.

We find that pension reforms increase inequality. In particular, our estimates show that the ratio between the income of the top 10% of earners and the median income increases after a generic reform. This effect is led by "contractionary" reforms, i.e., reforms aimed at strengthening the sustainability of the system by, for example, increasing the retirement age or reducing benefits, while "other" reforms, i.e. those that have an uncertain impact on the system sustainability, generate a much milder and shorter increase in inequality.

The data also show that the effect of contractionary reforms depends on the pension system characteristic modified by the reform. On average, increases in the retirement

age and in contributions reduce inequality, while a reduction in benefits and increases in the early retirement age increase inequality. Of particular interest is the effect of the reforms that we catalogue as "residual", which are mostly reforms aimed at strengthening the second pillar of the pension system and moving towards a fully funded system: these reforms reduce inequality.

We then investigated the potential transmission mechanism of the distributional effect of reforms. In general, pension reforms mechanically increase inequality because they hinder the redistributive effect of the pension system. We find that the behavioural response of households magnifies this increase when the reforms target benefits, early retirement age, and incentive to work at old age, while sterilizing it when reforms target contributions and statutory retirement age.

Our findings potentially have crucial policy implications. We show that although pension reforms are generally detrimental to inequality, the design of the reform matters for such effects. For example, a pension reform that redesigns the pillars of the pension system while increasing the retirement age and contributions is very likely to persistently reduce inequality. This argument should also reduce opposition to reforms.

The idea of investigating pension reforms by exploiting a narratively identified dataset is not new. Verbič and Spruk (2019) construct a dataset for 34 countries over the period 1970–2013 by reporting only which pillar of the pension system was reformed to analyse which political institution drove the reform. Similarly, Fong and Leibrecht (2020) analyse what drove the introduction of second-pillar defined-contribution schemes in approximately 100 economies in the period 1980–2000. To investigate the drivers of a broader set of reforms, Beetsma et al. (2020) build a dataset containing all pension reforms in 23 OECD countries from 1970 to 2017. Finally, Bi and Zubairy (2023) collect information on reforms in 10 OECD countries between 1962 and 2017 to investigate their effects on the labour force participation rate.

These datasets were either very limited in scope (Verbič and Spruk, 2019; Fong and Leibrecht, 2020) or found to be not orthogonal to business cycle elements (Beetsma et al., 2020; Bi and Zubairy, 2023), potentially because of a misclassification of the reforms. We build on these studies by providing more complete and detailed identification of the pension reforms. Our dataset reports information on all pension reforms implemented in the selected countries, distinguishing between major and minor reforms and the elements of the pension system addressed by the reforms. On the one hand, this helps prevent our labeling government actions consisting of fiscal interventions as reforms (we show below that this may be an issue in Beetsma et al. (2020)) or might depend on the business cycle (we show below that this could be an issue in Bi and Zubairy (2023)). On the other hand, our database allow us to assess which elements of the reforms are more detrimental to the economic system, thus providing crucial evidence to improve the design of pension reforms.

In addition, our dataset covers a set of homogeneous countries, thereby mitigating the possible influence of unobserved differences across units.

We exploit the dataset to investigate whether pension reforms increase or reduce inequality.[1] Despite the relevance of the topic, the number of studies investigating the distributional effects of pension reforms is surprisingly small. Most of these studies use single-country data to explore the distributional effects of specific reforms with structural models (see, for example Huggett and Ventura, 1999; Hairault and Langot, 2008; Etgeton, 2018; Fonseca and Sopraseuth, 2019; Cottle Hunt and Caliendo, 2020; Andersen et al., 2024; Börsch-Supan et al., 2024; Sánchez-Romero et al., 2024). These studies show how pension reforms can increase inequality, particularly when are intended to improve the sustainability of the pension system, a finding in line with our results. However, the information that they provide is often limited to one or two changes in a specific pension system, and the analyses suffer from model uncertainty, as the true model of the economy is unknown (see Furceri and Ostry, 2019). This harms the external validity, making it difficult to generalise the inference they present to different settings. Empirical investigations are scarce, and they focus on how "privatization" of the pension system can increase inequality among retirees (see Van Vliet and Caminada, 2012; Ebbinghaus, 2021). We contribute to this literature by presenting robust and extensive empirical evidence on the distributional effects of a wider set of pension reforms in a cross-national setting, which allows us to overcome the limits of using single-country data.

The paper is organized as follows: the next section presents and details our approach to identifying major pension reforms in our sample of countries and provides descriptive statistics, including a preliminary analysis of the effects of pension reforms on the sustainability of public finances and inequality. Section 4 explains our empirical method; we use local projections to estimate the effects of pension reforms on inequality. In this section, we also address concerns about the endogeneity of pension reforms and compare our dataset to the ones provided by Beetsma et al. (2020) and Bi and Zubairy (2023), while Section 4.2 presents and discusses our results. Section 5 sheds some light on the underpinning mechanism of our results, and Section 6 discusses the most relevant robustness exercises. Finally, we offer our conclusions in Section 7.

[1]The distributional effects of fiscal and monetary policies have received a great deal of attention and have been empirically investigated with narratively identified reforms; see, for example, Furceri and Loungani (2018). While a review of this evidence is beyond the scope of this paper, the reader might consult Nolan et al. (2019).

2 The pension reforms

Our dataset comprises reforms identified by means of a narrative procedure. Similarly to the authors of other studies in the field (Verbič and Spruk, 2019; Fong and Leibrecht, 2020; Beetsma et al., 2020; Romp and Beetsma, 2022; Bi and Zubairy, 2023), we gathered information from several sources to determine relevant actions taken by governments to modify the pension system. Then, we classified these actions based on several elements: intensity, sign, length of the phasing-in, and characteristics that the intervention aimed at modifying (e.g. the retirement age).[2]

We focus on 14 European countries: Austria, Belgium, Denmark, Finland, France, Germany, Greece, Ireland, Italy, the Netherlands, Portugal, Spain, Sweden, and the United Kingdom, for a sample period ranging from 1990 to 2018. The dataset coverage is consistent with our goal of providing evidence on the distributional effects of pension reforms in European countries. To this aim, we concentrate on large countries, thus excluding small ones where pension reforms and the pension systems are largely characterized by idiosyncratic elements. In addition, we decided to start our dataset in 1990 for several reasons: data quality on income inequality lowers dramatically before that year, as well as other statistics that have been harmonized by Eurostat, contractionary reforms are virtually non existent before the 90s, and the EU was created in that decade.[3]

Our primary source of information is the OECD country surveys series for the major European economies starting from 1990. These surveys are at annual or biannual frequency and review the countries' economies and the reforms approved. They also suggest possible strategies for strengthening the countries' economies. This feature allows us not only to pinpoint the date of the pension reforms but also to have a detailed description of the approval process and the content of the reforms. In addition to being extensive, the information from the surveys is largely comparable given that the format is the same for all countries and consistent over the considered period. In a second step, we gathered information from secondary sources. Specifically, we used five editions of the OECD Pensions at a Glance (OECD, 2007, 2013, 2015, 2017, 2019), the ILO-NATLEX database, which reports most laws regarding pension systems; the European Commission PensRef and LabRef databases, which report the structure of pensions and the labour market in European countries, respectively; and other publications, namely, Bisciari et al. (2009), Börsch-Supan and Coile (2018), Beetsma et al. (2020), and Bi and Zubairy (2023). We used the secondary sources to cross-check and integrate the information from the OECD

[2]The dataset also reports a short description of the intervention to cross-check the information. This makes it easy to compare our dataset with other sources and check its validity.

[3]According to Beetsma et al. (2020) data set, there were 10 contractionary pension reforms in our sample of countries during the period 1970-1990. Most pension reforms implemented during the 1970s and in the 1980s were classified as expansionary.

country surveys when necessary: despite being very similar, different sources might refer to different details on the reforms, thus allowing a better identification of the reform characteristics.[4]

To categorize a government action relative to the pension system as a major reform, we took into account several elements: the expected impact of the action, the number of changes introduced, how many times the reform was discussed by different sources, and the motivation of the reform. When an action is characterized by a large expected impact (i.e. involves a large share of the population, ideally the entire working-age population), several significant changes introduced (at least 2), discussed by different sources, and willing to reform the pension system and not only by fiscal needs, then it was categorized as major, while other actions reported by our sources were classified as minor.[5]

This allows us to distinguish between proper reforms and changes to the pension system that were in fact fiscal policy changes – which are probably conflated in other available datasets, as we discuss below – while maintaining an encompassing definition of reform. Additionally, the inclusion of minor actions is relevant, as they often constitute retrenchments of major reforms, changes that extend previous reforms to specific sectors of the economy, or fiscal actions: by excluding them from the analysis, we might be susceptible to omitted variable bias. Using this narrative approach, we identify 54 major pension reforms and 75 minor reforms. These additional reforms address certain aspects of the pension system but do not constitute systematic changes.

In addition, we reported (and investigated) several aspects of the reforms. First, we classified the reforms based on their sign: following other studies such as Beetsma et al. (2020) and Bi and Zubairy (2023) our dataset reports whether a reform is contractionary (i.e., when it aims to reduce pension outlays, e.g., by increasing the retirement age or reducing benefits), expansionary (i.e., when it aims to increase pension outlays), or other (i.e., when it contains both expansionary and contractionary elements).

Second, we classified the reforms based on the element of the pension system that they affect: the statutory retirement age, the level of benefits (this also includes changes in indexation or accrual factors), the level of contributions, the early retirement age (including minimum years of work), the incentives to work longer, and a residual category that includes organizational changes and other system-specific actions. This 6-type classification is a novelty in the literature and constitutes a strength of our dataset.

Third, following Bi and Zubairy (2023), we distinguished reforms based on the length of their phasing-in, which can be short if the implementation is immediate (the reform

[4]The Appendix details and exemplifies our identification strategy.

[5]A change is classified as significant when it represents a major shift in a specific area of the pension system and does not merely extend, apply, or accelerate previous reforms. For example, the decision to link the pension age to life expectancy represents a significant change, while updating the pension age according to previous reform is not.

is fully implemented in the fiscal year), medium if its full implementation (or phase-in) takes up to 5 years, and long if it takes more than 5 years to be fully implemented. Often, different elements of the reforms have different phasing-in lengths and signs: in this case, we departed from previous works which classified entire reforms based on the element with the longest phasing-in or report only the overall sign of the reform by reporting the phasing-in and sign of the different elements. This cross-classification also represents a novelty compared to previous approaches and proved to be an invaluable advancement to provide detailed evidence on the effects of pension reforms.

As a further robustness check, we also created an index of intensity where minor actions are labelled 1, major reforms as 2, and very major reforms (for example, reforms that completely reshaped the system such as the one in the UK in 2000 or reforms composed of several subsequent acts, each of which could be a major reform such as those in Portugal between 1993 and 1995).

Our identification strategy allows us to overcome the limitations characterizing the datasets developed in previous studies. Some of them proved to be limited in scope: Verbič and Spruk (2019) limited their analysis to the pillar reformed, while Fong and Leibrecht (2020) analysed only the reforms of the second pillar of the pension system. Other adopted an identification strategy that induced, instead, a misclassifications of pension reforms. Beetsma et al. (2020) classified every policy action concerning pensions, for example, a freezing of pension contributions as a 'reform', thus labelling as pension reforms 458 government actions, of which 333 affected a large share of the population (indicated as 'Many' in their classification). Considering their sample (23 countries for 47 years), this implies that a pension reform is approved approximately every 2 years, and that one every 3 years concerns a large share of the population. As we will show below, they included actions that can be considered as fiscal policy not as a structural change in the pension system, and we also show that these reforms are correlated with business cycle fluctuations. In our classification, these actions will be generally regarded as 'minor' to focus on the effects of structural changes of the pension system.

Similarly, Bi and Zubairy (2023) have misclassified some minor changes in the pension system as major pension reforms. One example of this is the reform approved in Denmark in 1992, which introduced "the transitional benefits program (overgangsydelse) as an early exit route for long-term unemployed in their 50s, closed to further entry in 1996, and finally phased out in 2006" (Börsch-Supan and Coile, 2018). Despite its relevance, we consider the opening and closing of this program a minor reform because of its limited scope and the share of population involved, plus the fact that some observers, as the OECD, classified this mostly as a labour market policy (OECD, 1993). Another case is the reform approved in Italy in 1997, where "[to respond] to the fact that pension expenditures were continuing to rise more rapidly than expected, the Prodi Agreement of 1997 accelerated

the increase in the early retirement age, brought forward the harmonisation of public and private pension regimes and increased pension contributions paid by the self-employed" (OECD, 1998). Again, this reform being an acceleration of a previously approved one, we deemed it to be a minor change of the pension system. These misclassifications might have produced some correlation between reforms and cyclical elements, as we will show below. However, their main results remain intact when replicated with our dataset.[6]

As aforementioned, another deviation from previous papers is that we reported additional dimensions of pension reforms. This proved to be a crucial factor to identify the drivers of pension reforms' effects. Finally, we also differ from other datasets in terms of coverage: we investigate a subsample of the data covered by Beetsma et al. (2020), while we have a lager cross-section compared to Bi and Zubairy (2023) that studies 7 of the 14 countries we analyse. On the time dimension, Beetsma et al. (2020) covers the sample starting in 1970 whereas Bi and Zubairy (2023) starts in 1962. Even though these papers collect an extensive data set, they only use estimates based on a sample starting in 1980.

2.1 The distribution and characteristics of pension reforms.

Table 1 reports the distribution by country for the 54 major reforms we have identified. The geographical distribution of pension reforms suggests a relatively balanced panel, with countries on average implementing approximately two pension reforms during the sample period. The largest number of reforms adopted is for Portugal (7 reforms during the sample period), whereas Ireland and Sweden implemented only 1 reform each. The table also distinguishes the aim of the pension reforms, namely, whether they are classified as contractionary, expansionary or other. In total, we identified 41 major pension reforms where the main motivation was to reduce pension outlays.

Only two out of 54 reforms can be classified as expansionary: the 1998 reform in Germany and the 2007 reform in the United Kingdom. The pension reform approved in Germany in 1998 is classified as expansionary for several reasons. The reform focused on the retirement age, contributions to the pension system and indexing of pensions. The pension policy of the new government proceeded on two tracks. The new government withdrew savings measures put in place by the old government, as they were deemed socially unacceptable. In addition, the government removed the life expectancy adjustment formula and the combination of pensions for individuals with a reduced capability to work. New measures targeting both the revenue and the spending side of the system were also adopted. The long-term aim was also to stabilize the pension contribution rate at approximately 19 percent by 2010. The government furthermore proposed a lowering of pension contribution rates, mainly for the purpose of lowering non-wage labour costs.

[6]The results for this replication exercise are available upon request.

On the spending side, the government decided to suspend the pension adjustment formula based on net wages for 2000 and 2001, indexing pensions to inflation instead. This move was motivated by the fact that net wages were boosted by the 1999 income tax reform. For these reasons, we classified this reform as expansionary.[7]

The 2007 pension reform in the United Kingdom focused on contributions and indexation. The 2007 Pensions Act proposed that the basic state pension should become widely available, which would increase the contributory credits available for caring responsibilities, and the minimum number of contributing years was lowered to 30 years. Moreover, from 2012, the basic state pension would increase on the basis of increases in average earnings instead of prices. In addition to making the basic state pension more generous, this would significantly reduce the number of people who would qualify for means testing in the future.

A second pension bill presented later in the same year introduced a private pension reform to be in place in 2012. This reform was based on automatic enrolment, a mandatory 3% employer contribution, and a new low-cost scheme of personal accounts that was thought to encourage higher take-up of private pensions and ensure that earners (particularly low- to middle-income earners) would have access to a simple, low-cost pension scheme in which to save. The overall aim of the reform was to increase benefits both through incentives to increase contributory credits and to introduce a new indexation of pensions that would tend to increase the benefits. Therefore, we classified this reform as expansionary.[8]

Beyond these pension reforms, we found it impossible to classify 11 reforms as either contractionary or expansionary. Typically, they involve elements that would decrease the pension age, provide incentives to work longer in life but at the same time increase benefits and make indexation more generous.

Figure 1 reports the distribution of major pension reforms across time. As is evident, the pension reforms are quite balanced over time, with at least one reform being adopted every year apart from 1991, 2005, and 2009 and a maximum of 5 reforms being adopted in 2014. In Figure 1, we also distinguish types of pension reforms, that is, whether they are classified as expansionary, contractionary or other. The classification is the same as the one that we use in Table 1 above. Here, we again note that pension reforms classified as both contractionary and expansionary were implemented in the same year in different countries. Reforms focusing on restricting pension systems dominate. There are also a number of additional pension reforms approved and implemented in our sample

[7]Beetsma et al. (2020) also classified this reform as expansionary. Bi and Zubairy (2023) do not include Germany in their data set.

[8]In this case, we differ from the classification made by Beetsma et al. (2020) classifying this reform as both contractionary and expansionary and Bi and Zubairy (2023) classifying the reform as a major contraction.

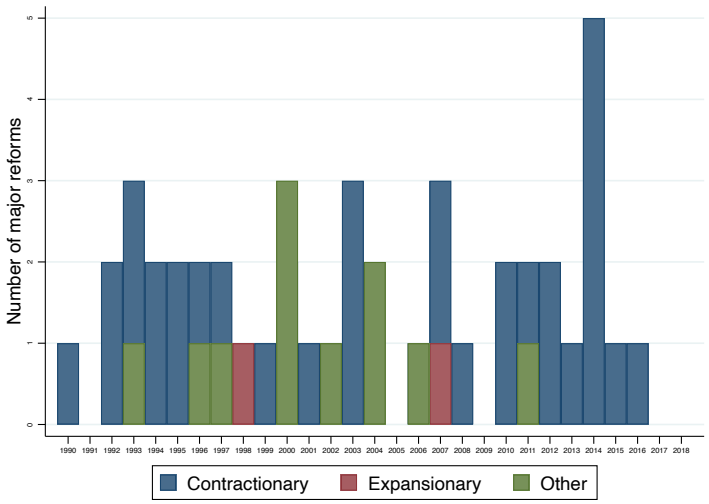
Table 1: Geographical distribution of major pension reforms in 14 European countries. The table reports the approval year.

Country	Reforms	Contractionary	Expansionary	Other
Austria	5	2003		1993, 1997, 2000, 2004
Belgium	5	1996, 2012, 2015		2000, 2006
Denmark	2	2006		2011
Germany	6	1992, 1997, 2001, 2007	1998	2004
Greece	4	1990, 2008, 2010, 2016		
Finland	6	1993, 1994, 1996, 2000, 2003, 2014		
France	4	1993, 2003, 2010, 2014		
Ireland	1	2014		
Italy	4	1992, 1995, 2004, 2011		
Netherlands	3	2014, 2012		1996
Portugal	7	1993, 1994, 1995, 2000, 2007, 2014		2002
Spain	4	1997, 2007, 2011, 2013		
Sweden	1	1999		
United Kingdom	2		2007	2000
Total	54	41	2	11

Note: The table reports the total number of major pension reforms and the year of approval. We distinguish between contractionary, expansionary and reforms that are impossible to classify according to our definitions.

of countries, and we classified all those reforms as minor. In total, we found 75 pension reforms that are not classified as major but address some aspects of pensions.

Figure 1: Number of major pension reforms adopted in a given year.



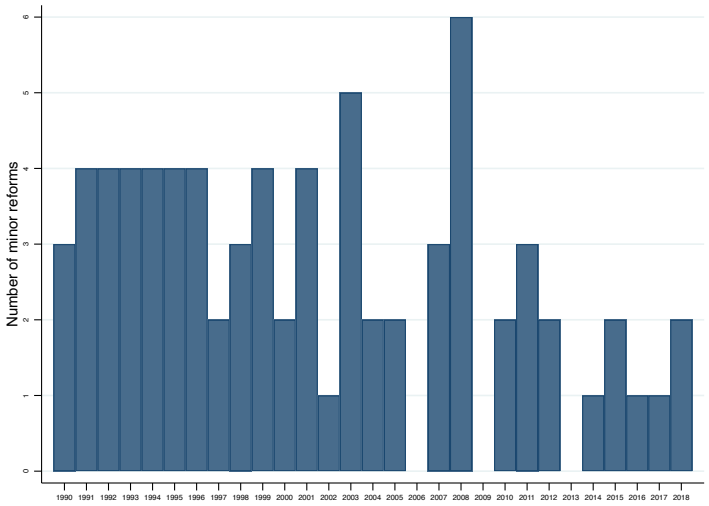
Note: The graph reports the number of major pension reforms adopted each year according to our definitions of expansionary, contractionary and other pension reforms.

Figure 2 documents the distribution across time. Like the major reforms, minor pension reforms are distributed in an almost even pattern across time. On average, there are

between two and three minor reforms implemented each year. The frequency of reforms tends to fall towards the end of our sample period. This could be because many countries had already implemented both major and minor pension reforms up until the financial crisis.

With regards to the motivation of reforms, we were able to identify various aims of adopting major pension reforms: fiscal sustainability, the will to reduce public or pension expenditures, the need to align public and private sector requirements and benefits, the aim to increase pension savings, and the necessity of implementing reform in the present to avoid costlier reforms in the future. The main motivation declared by governments is fiscal sustainability. Other arguments, such as the incentive to work in old age and system sustainability, are mentioned, but it appears that fiscal sustainability totally dominates. In a few cases, a clear motivation was not detected, or we identified several main motives.

Figure 2: Number of minor pension reforms adopted in a given year.



Note: The graph reports the number of minor pension reforms adopted each year according to our definition.

Figure 3 describes how reforms are distributed according to the phase-in period: in our sample, reforms can have an immediate phase-in when fully implemented within a year, a medium phase-in when implemented within 1 to 5 years, and a long phase-in when implemented within more than 5 years. Like Bi and Zubairy (2023) we label reforms according to the tool with the longest phase-in period when multiple tools with multiple phase-in periods are present. As Figure 3 reports, for more than half of the observations in our sample (33 reforms), the reforms had a long phase-in, while there are 9 cases of reforms with an immediate phase-in and 5 with a medium phase-in. In 7 cases, a clear phase-in period could not be identified, either because the sources gave conflicting information or the information was missing.

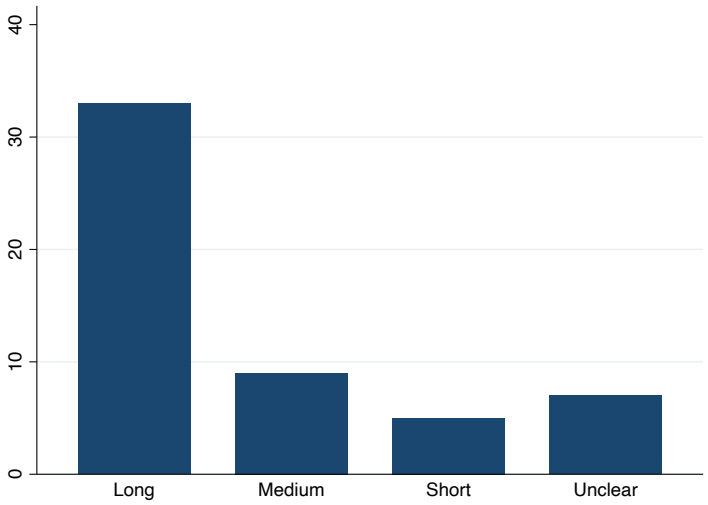


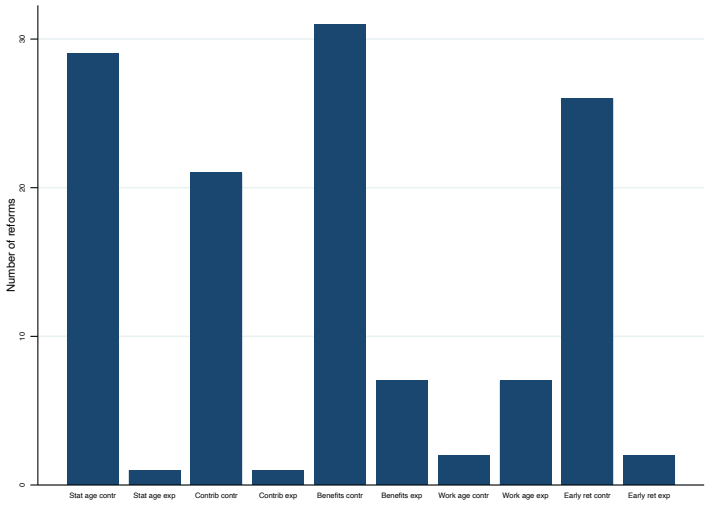
Figure 3: Major pension reforms by phase-in period.

Note: The graph reports the number of major reforms by length of the phase-in. Long: The reform was implemented more than 5 years after approval. Medium: The reform was implemented between 1 and 5 years after approval. Short: The reform was implemented less than a year after approval. Unclear: The phase-in period was not specified. When a reform consists of multiple legs and each of them had different phase-in periods, the one with the longest period defines the phase-in for the entire reform.

Finally, Figure 4 reports the distribution of reforms by part of the pension system reformed. Because most packages reform more than one feature of the pension system, the graph counts the number of times that a specific feature is targeted. It is common for major pension reforms to target more than one feature of the system, and therefore, the sum of the targets does not add up to the total number of major reforms. We also distinguish reforms that are contractionary, for example, those increasing the pension age, and those that are expansionary, for example, those decreasing the pension age. As we can see from the graph, we find 30 reforms that modified the pension age, 29 of which increased the pension age and are thus classified as contractionary. Similar patterns emerge for reforms aimed at contributions. Out of the total of 22 reforms, 21 increased the contributions. Benefits were reduced in 31 reforms out of a total of 38 that included changes to the benefit system. A total of 28 reforms included changes in the early retirement age, with 26 of these increasing the minimum retirement age. Finally, some reforms included incentives for workers to continue working in old age, but only two of them increased these incentives.

In the next section, we will use this unique dataset on major and minor pension reforms to study whether these have consequences for inequality. We start by introducing the data that we will use, and then we document several descriptive statistics before estimating local projections where we condition on a standard set of control variables.

Figure 4: Number of major reforms by main targets.



Note: The graph reports the number of reforms by main target. Each reform usually targets more than one feature of the pension system, and therefore, the numbers do not add up to the total number of major reforms. Stat age contr: The reform increases the minimum retirement age. Stat age exp: The reform decreases the minimum retirement age. Contrib contr: The reform increases the level of contributions. Contrib exp: The reform decreases the level of contributions. Benefits contr: The reform decreases the level of benefits. Benefits exp: The reform increases the level of benefits. Work age contr: The reform increases the incentive to work in old age. Work age exp: The reform decreases the incentive to work in old age. Early ret contr: The reform decreases the incentive for early retirement. Early ret exp: The reform increases the incentive for early retirement.

3 Pension reforms and inequality: A first look

The dataset that we constructed in the previous section can be used to analyse several aspects of pension reforms, including those studied in the earlier literature.

Table 2: Descriptive statistics of the effects of pension reforms on pension outlays, the structural balance and inequality measures.

Variable	Obs	Mean	Std. dev.	Min	Max
Poverty	275	18.47	7.43	4	44
Gini	254	31.39	3.56	22.90	39.60
Income Top 10%	378	0.33	0.03	0.24	0.40
Income Bottom 50%	378	0.21	0.02	0.15	0.26
Wealth Top 10%	331	0.56	0.05	0.45	0.72
Wealth Bottom 50%	326	0.04	0.03	-0.05	0.10

Note: The table reports the distribution of wealth, income, Gini coefficient, and poverty coefficient, in our sample.

Here, we will focus on the effects of reforms on inequality, an issue that has not been

studied empirically in the previous literature.[9] We consider six measures of inequality. The Gini coefficient was downloaded from the World Development Indicators. Poverty is the share of individuals 65 years and over who are at risk of poverty with a cutoff point of 60% of median equalized income after social transfers from Eurostat. Then, we downloaded four inequality measures from the World Inequality Database: the top 10% and bottom 50% shares of pretax national income and the top 10% and bottom 50% shares of net personal wealth. Table 2 reports descriptive statistics for these inequality measures.

In Table 3,we provide a first look at the question of whether major pension reforms affect inequality. We compare observations for countries not implementing a major pension reform with observations for countries implementing at least one reform during the sample period. To this end, we construct a dummy variable equal to one for the year that the first major reform was implemented and for all subsequent observations. We do not take into account whether the first reform is classified as contractionary, expansionary or other. The table reports means and formally tests whether our inequality measures change significantly using simple fixed effects panel data regressions with robust standard errors. We also construct another dummy variable equal to one on the date when the first contractionary pension reform was implemented and for all subsequent observations. This allows us to compare the effects of any major pension reform with those of reforms aimed at making the pension system more robust, which we call contractionary reforms.

Consider first Panel A, where we compare the averages across periods with and without any type of pension reform. In general, the results suggest that there is a significant difference in the means of the poverty rate and in the income distribution, with both the top 10% and bottom 50% of the distribution significantly affected. There is an increase in top 10% income and a fall in bottom 50% income, implying that the ratio between the top 10% and the bottom 50%, which is a measure of the difference between the poorest half and the highest earners within a population, increases in countries implementing a major pension reform.[10] However, we also find that the poverty rate falls significantly. This could be due to the aim of many pension reforms to provide incentives for older workers to remain in the workforce and increases in the pension age. As a result, we expect that the risk of poverty falls. Neither the Gini coefficient nor the two measures of wealth distribution are significantly affected. Overall, it seems that pension reforms of

[9]We found that a majority of pension reforms were aimed at restoring public finance sustainability. In the online supplementary material, we present descriptive statistics addressing this finding. We consider the cyclically adjusted primary balance as a share of GDP and cyclically adjusted pension outlays as a share of both GDP and potential GDP. We find, in accordance with previous literature, that pension reforms significantly affect public finance sustainability.

[10]In the supplementary material we show that pension reform increase the top 10%-bottom 50% ratio by plotting its dynamic effect

any type significantly affect inequality.

Table 3: Descriptive statistics of the effects of pension reforms pension outlays, structural balance and inequality measures.

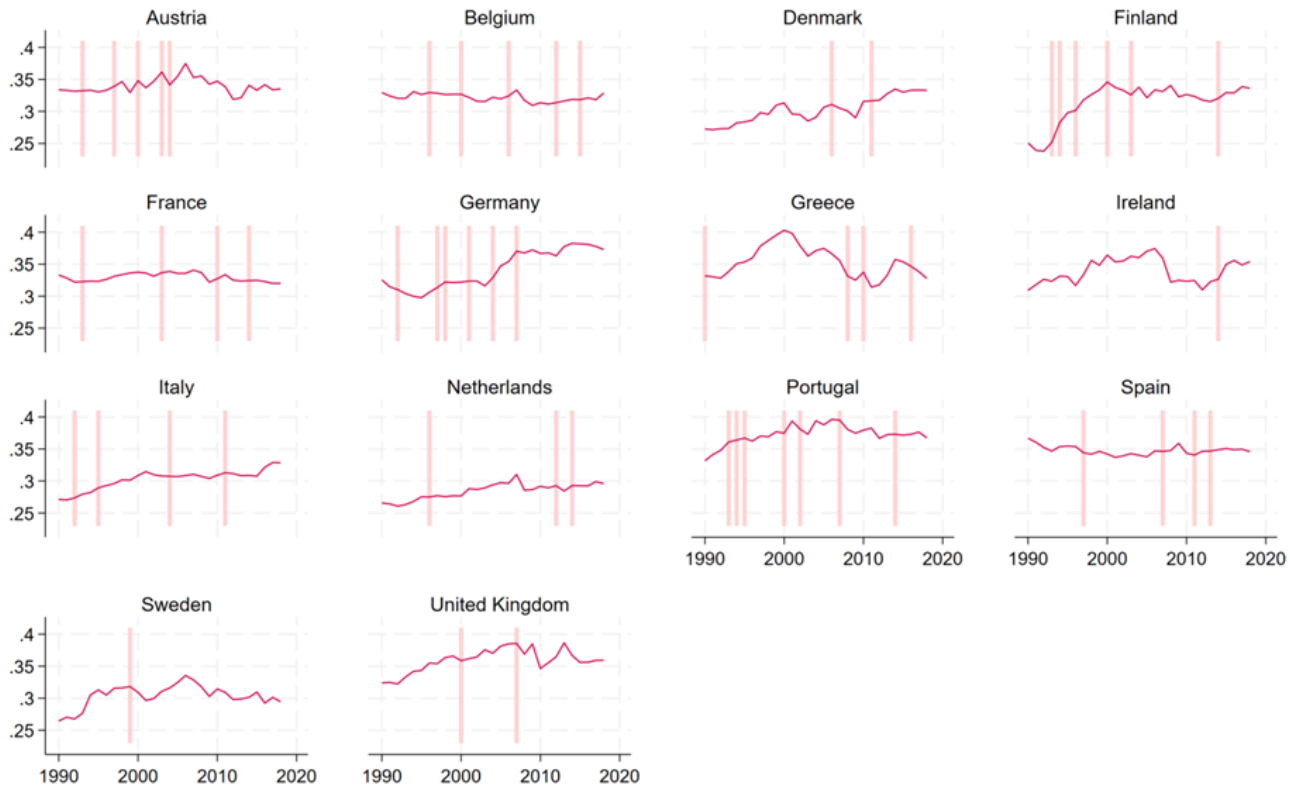
Panel A: No pension reform vs. at least one reform					
	Mean no reform	Mean with reform	H_0 : equal mean	Obs without reform	Obs with reform
Poverty	23.92	17.76	0.053	31	246
Gini coefficient	31.29	31.39	0.371	40	246
Income Top 10%	0.32	0.33	0.006	94	286
Income Bottom 50%	0.22	0.21	0.008	94	286
Wealth Top 10%	0.59	0.56	0.941	63	270
Wealth Bottom 50%	0.02	0.05	0.748	58	270
Panel B: No pension reform vs. contractionary reforms					
	Mean no reform	Mean with reform	H_0 : equal mean	Obs without reform	Obs with reform
Poverty	19.73	18.07	0.042	67	208
Gini coefficient	31.66	31.23	0.040	75	184
Income Top 10%	0.32	0.33	0.004	162	241
Income Bottom 50%	0.22	0.21	0.001	104	227
Wealth Top 10%	0.57	0.56	0.913	104	227
Wealth Bottom 50%	0.03	0.05	0.748	99	227

Note: H_0 : equal mean refers to a t-test of equal mean based on estimates of a fixed effects panel data regression with robust standard errors. Only p-values are reported.

Similar results hold when we compare only contractionary pension reforms with no reforms; see Panel B in Table 3.The Gini coefficient appears to fall significantly after contractionary pension reforms, while the other results in Panel B are consistent with what we found in Panel A. The conclusion from these descriptive statistics is that inequality is significantly affected after the implementation of major pension reforms. However, the statistics computed in the table do not take into account other possible channels through which inequality can be affected that might have occurred in the same period. Moreover, the descriptive statistics presented above consider the behaviour of only a select number of indicators and cannot reveal any causal link or the influence of other macroeconomic or institutional factors. In the next section, we apply local projection analysis using the pension reform data as exogenous shocks and add control variables reflecting the state of the economy and institutions.

Figure 5 shows the development in the ratio of top 10% income over median income for all 14 countries together with major pension reforms. The figure reports the approval of a reform with a light red vertical line, while our variable of interest, the share of top 10% income over median income, is the red line. It is difficult to judge from this figure whether inequality is increasing or decreasing; movements in inequality are in general moderate, but comparing across countries, we find that there is a slight increase in inequality.

Figure 5: Major pension reforms and inequality in 14 European countries.



Note: The graph shows the year of approval of a major pension reform (light red vertical line) and the developments in the ratio of top 10% income over median income (red line).

4 Empirical analysis

To assess the dynamic effect of pension reforms, we employ local projections (Jordà, 2005). The underlying assumption of this methodology is that the pension reforms captured by our measure are exogenous to other economic innovations and structural differences across countries that can affect inequality. To examine whether pension reforms can be treated as exogenous, we start by testing whether our major pension reform indicator is independent of economic and demographic factors. Given these results, we then turn to the estimation of local projections.

4.1 Endogeneity of pension reforms

Although we will use a number of control variables in our estimations of local projections, we must address possible endogeneity issues. It could well be that pension reforms are implemented as a result of concerns about fiscal sustainability, and the timing could well be affected by the phase of the business cycle, with electorates finding reforms more

acceptable during economic upturns. For example, Beetsma et al. (2020) and Romp and Beetsma (2022) showed that pension reforms are driven by the business cycle. However, their dataset includes any government action related to pensions, including temporary freezing of increases in contributions or augments in benefits during crisis periods. As a consequence, they include elements that can be considered more similar to fiscal policy than to structural changes in the pension system. Similarly, misclassification might have generated correlations to the business cycle for Bi and Zubairy (2023) measure.

To formally test this possibility, we regress Beetsma et al. (2020)'s dataset, Bi and Zubairy (2023) dataset, and our data set on measures of discretionary fiscal policy. We restrict the sample to countries and the time horizon that we explore in this paper. Table 4 reports the results of this experiment, showing the results for a panel logit regression with fixed effects. In the table, columns (1) and (4) have our measure as the dependent variable, while columns (2) and (5) have Beetsma et al.'s (2020) measure, columns (3) and (6) have Bi and Zubairy (2023)'s dataset. To control for the possible role of the business cycle, we include the output gap; changes in demography are controlled for with the growth in the age dependency ratio, which also enters in levels to check for possible differences across countries.

We control for discretionary fiscal policy using a structural balance measure and the consolidation measure proposed by Alesina et al. (2019). This latter variable reports changes in the government balance aimed at reducing government debt or the structural balance and is narratively identified. A strength of the consolidation measure is its ability to separate changes based on expenditure cuts from those based on tax increases. It must be noted that, despite pension reforms can be a part of a fiscal plan, we observe that only 12 pension reforms out of the 47 approved until 2013 are also accompanied by a consolidation as defined by Alesina et al. (2019).[11] This implies that the two variables should not be correlated. In our setting, all variables enter in contemporaneous and lagged values, except the age dependency ratio, which enters in levels. The specification is similar to the one in Alesina et al. (2019).

The results in Table 4 indicate that our and Beetsma et al. (2020) measures of pension reforms are driven by the business cycle or the structural balance, while Bi and Zubairy (2023) is correlated to the lagged output gap. However, columns (4), (5), and (6) show that Beetsma et al. (2020)'s data are significantly correlated with the consolidation measure, while our and Bi and Zubairy (2023) data are uncorrelated with consolidations. In addition, these two measures are correlated with changes in age dependency, while the Beetsma et al. (2020) measure is not. This suggests that Beetsma et al. (2020) measure on pension reforms contain fiscal elements that correlate with expenditure cuts, Bi and Zubairy (2023)'s measure is correlated to the business cycle, while our dataset is driven

[11]The sample in Alesina et al. (2019) covers only until 2013.

by demographic elements only.[12] This result is reassuring and suggests that our dataset captures major changes in the pension system approved to strengthen its sustainability and are not related to unexpected fiscal policy.

To investigate the presence of endogeneity of our measure further, we explored a different setting where: 1) we add one more lag to the equation, and; 2) we include some lags of a measure of inequality. This exercise is reported in Columns (1) and (2) of Table 5. Particularly, Column (1) adds two lags of ratio between top 10% average income and the median income, which is our favorite measure of inequality, while Column (2) adds two lags of the growth in the Gini index, which is widely regarded as a good measure of inequality. In both cases, the variables are not significant, thus suggesting that the pension reforms in our dataset are not driven by changes in inequality.[13].

In addition, Table 5 explores other potential drivers of the major reforms as well, i.e. the growth of real gdp (col. (3)), change in unemployment rate (col. (4)), inflation (col (5)), and change in the debt-to-GDP ratio (col. (6)). These variables replace the output gap (cols. (3) - (5)) and the structural balance (col. (6)) to avoid collinearity, as the new variables cover the same economic concept. The results suggest that also these variables cannot predict our measure of pension reforms, thus supporting their independence from business cycle fluctuations and fiscal policy.

4.2 Local projections

In the previous section, we showed that our measure is relatively independent of economic factors but is still related to demographic elements, which must, therefore, be controlled for in our estimates. Another relevant issue in estimating the effects of reforms is the presence of foresight (Duval et al., 2020), because this can generate non-fundamentalness of the estimated shocks and bias the results (Leeper et al., 2013). A possible solution for this is the inclusion of economic variables that, by reflecting agents' behaviour before the reform approval, can integrate agents' foresight into the information set that we control for (Forni and Gambetti, 2016).

This form of anticipation represents a smaller concern in our analysis than in extant research focusing on the business cycle effects of pension reforms. This is related to

[12]We have run a number of additional panel logit regressions allowing for longer lags of the unexpected changes in taxes and expenditures as well as adding other control variables such as government efficiency, debt ratio, the number of years in office, structural balance and pension outlays as a share of GDP. Our main conclusion drawn from Table 4 are unaffected. The (Beetsma et al., 2020) indicator of pension reforms is determined by unexpected government expenditures and in some cases also significantly determined by unexpected tax changes. These results are available upon request from the authors.

[13]We have also controlled for different measures of inequality, as the share of income of the top 1% and the bottom 50%. Our main conclusion drawn from Table 5 remain unaffected. These results are available upon request

Table 4: Major pension reforms vs. Beetsma et al. (2020) and Bi and Zubairy (2023).

	(1) Our measure	(2) Beetsma et al.	(3) Bi and Zubairy	(4) Our measure	(5) Beetsma et al.	(6) Bi and Zubairy
Age Dep. Lev.	-5.726	-0.502	-3.589	0.0979	2.594	-2.572
	(-1.34)	(-0.16)	(-0.75)	(0.01)	(0.63)	(0.740)
Output Gap	0.0192	-0.0400	0.0868	0.0644	0.00743	0.155
	(0.21)	(-0.63)	(0.71)	(0.56)	(0.10)	(0.287)
Output Gap t-1	-0.0637	0.0387	-0.217*	-0.135	0.0410	-0.274*
	(-0.68)	(0.59)	(-1.78)	(-1.21)	(0.52)	(0.052)
Age Dep. Growth	71.28	22.11	145.88	129.6**	34.38	159.92**
	(1.34)	(0.54)	(2.12)	(1.98)	(0.72)	(0.043)
Age Dep. Growth t-1	-31.81	-42.03	-137.59*	-166.5**	-83.75	-138.24
	(-0.56)	(-1.00)	(-1.81)	(-2.27)	(-1.61)	(0.120)
Struc. Bal.	0.110	0.0977	0.1242			
	(0.99)	(1.27)	(0.77)			
Struc. Bal. t-1	0.0235	0.0158	-0.1026			
	(0.22)	(0.21)	(-0.67)			
Tax Un.				0.795	0.644	0.067
				(1.53)	(1.61)	(0.925)
Tax Un. t-1				0.384	0.324	0.528
				(0.74)	(0.79)	(0.407)
Exp. Un.				0.289	0.604**	0.384
				(1.09)	(2.13)	(0.206)
Exp. Un. t-1				-0.0107	-0.155	-0.222
				(-0.04)	(-0.66)	(0.500)
Observations	345	331	178	252	273	147
Pseudo R2	0.028	0.013	0.070	0.056	0.044	0.098

Note: * indicates significance at the 10 percent level, ** at the 5 percent level and *** at the 1 percent level. T-statistics are shown within parentheses below each estimate. Logit panel regression of the dummy for our measure of major pension reforms for all columns. All columns control for the level of the age dependency rate and the contemporaneous change of the rate, plus two lags. The model controls for contemporaneous and two lagged values of: the output gap (col. (1), (2), and (6), age dependency growth (all columns) structural balance (col. (1) - (5)), growth of real GDP (col. (3)), change in unemployment rate (col. (4)), inflation (col (5)), and change in the debt-to-GDP ratio (col. (6)). Column (1) controls also for two lags of the income share of top 10% (our favorite measure of inequality), while Column (2) controls for two lags of the Gini index. The age dependency ratio is defined as the share of population older than 65 over the share of population younger than 65. All regressions include a full set of country fixed effects.

Table 5: Endogeneity of major pension reforms.

	(1) Our Measure	(2) Our Measure	(3) Our Measure	(4) Our Measure	(5) Our Measure	(6) Our Measure
Output Gap	0.0607	0.0669				0.00885
	(0.57)	(0.61)				(0.08)
Output Gap t-1	-0.124	-0.0881				-0.0838
	(-0.85)	(-0.61)				(-0.60)
Output Gap t-2	0.0496	-0.0117				0.0679
	(0.49)	(-0.13)				(0.70)
Age Dep. Growth	66.60	52.99	50.74	40.63	47.67	51.62
	(1.12)	(0.89)	(0.86)	(0.69)	(0.82)	(0.84)
Age Dep. Growth t-1	13.31	14.77	14.16	31.49	36.00	9.874
	(0.15)	(0.17)	(0.16)	(0.36)	(0.41)	(0.11)
Age Dep. Growth t-2	-58.86	-55.59	-48.82	-59.68	-68.97	-60.19
	(-1.01)	(-0.96)	(-0.84)	(-1.02)	(-1.11)	(-1.02)
Struc. Bal.	0.149	0.161	0.161	0.153	0.146	
	(1.24)	(1.34)	(1.34)	(1.24)	(1.26)	
Struc. Bal. t-1	0.0186	0.0245	0.0368	0.0251	0.0542	
	(0.17)	(0.23)	(0.35)	(0.23)	(0.49)	
Struc. Bal. t-2	-0.105	-0.106	-0.0997	-0.101	-0.0521	
	(-1.03)	(-1.05)	(-1.03)	(-1.05)	(-0.50)	
Inc. Share Top 10% t-1	8.487					
	(1.26)					
Inc. Share Top 10% t-2	5.185					
	(0.84)					
Gini Index t-1		7.740				
		(0.87)				
Gini Index t-2		-0.638				
		(-0.07)				
Real GDP Growth			7.529			
			(0.97)			
Real GDP Growth t-1			-0.343			
			(-0.04)			
Real GDP Growth t-2			-6.524			
			(-0.89)			
Unemp. Rate Growth				-0.205		
				(-1.01)		
Unemp. Rate Growth t-1				0.104		
				(0.52)		
Unemp. Rate Growth t-2				0.156		
				(0.96)		
Inflation					0.137	
					(0.66)	
Inflation t-1					-0.263	
					(-1.26)	
Inflation t-2					0.0465	
					(0.32)	
DebtGDP Growth						-0.0356
						(-0.98)
DebtGDP Growth t-1						-0.00629
						(-0.19)
DebtGDP Growth t-2						0.0402
						(1.10)
Observations	331	331	334	334	334	340
Pseudo R^2	0.044	0.038	0.039	0.044	0.039	0.027

Note: * indicates significance at the 10 percent level, ** at the 5 percent level and *** at the 1 percent level. t statistics are shown within parentheses below each estimate. Logit panel regression of the dummy for our measure of major pension reforms for all columns. All columns control fro the level of the age dependency rate and the contemporaneous change of the rate, plus two lags. The model controls for contemporaneous and two lagged values of: the output gap (col. (1), (2), and (6), age dependency growth (all columns) structural balance (col. (1) - (5)), growth of real gdp (col. (3)), change in unemployment rate (col. (4)), inflation (col (5)), and change in the debt-to-GDP ratio (col. (6)). Column (1) controls also for two lags of the income share of top 10% (our favourite measure of inequality), while Column (2) controls for two lags of the Gini index. The age dependency ratio is defined as the share of population older than 65 over the share of population younger than 65. All regressions include a full set of country effects.

the complexity of pension reforms: even if agents can effectively anticipate the timing of reforms, their final design is difficult to predict.[14] Furthermore, reforms usually have lengthy implementation processes and present a strict set of rules. These are, even in the presence of anticipation, difficult for agents to circumvent.

We evaluate the robustness of our results to this form of anticipation performing two separate exercises. First, we add minor reforms to our specification model. The underpinning assumption is that minor changes in the pension system can signal the passage of future major reforms. Second, we validate the robustness of the baseline estimates to the inclusion of leads and lags of the reforms dummy. In this way, we also condition on the correlation among inequality and future reforms, which would prove difficult otherwise. As a matter of fact, it is not obvious which variables can capture this information in our setting, as most of this information is embedded in nonquantifiable elements such as newspaper discussions. The inclusion of leads addresses these issues, thus representing a straightforward solution to control for anticipation. The inclusion of leads and lags also consistent with the one suggested to estimate local projections in case of staggered treatments in Dube et al. (2025), that shows a magnitude and a persistence in the increase of inequality larger than our baseline.

Another form of anticipation is the one stemming from the delay between the length of the reforms' phasing-in period. As Bi and Zubairy (2023) shows, the reaction of households to the approval of a pension reform can crucially depend on the delay between the reform approval and its full implementation. This is because households can decide, for example, to leave the labour force earlier if remaining active will increase their pension benefits only marginally – or will cost several years of labour more. We explicitly investigate this issue by estimating the effects of reforms depending on the length of their phase-in period. The baseline model that we use to estimate the effects of pension reforms is described in the following equation:

$$ineq_{i,h} = \alpha_i^h + \eta^h T_t + \beta^h ref_{i,t} + \theta^h X_{i,t} + u_{i,t}^h \tag{1}$$

where subscripts i and t indicate, respectively, the country and year, $ineq$ is our measure of inequality, ref is our measure of major pension reforms with a horizon-specific coefficient β^h, and X is our set of controls with horizon-specific coefficients θ^h. The model also includes a full set of fixed effects, country effects indicated by α and time fixed effects indicated by T while u are the residuals. The model is estimated with Driscoll-Kraay standard errors, which are robust to heteroskedasticity plus auto- and cross-correlations (Driscoll and Kraay, 1998).

In our baseline model, we use the (log) ratio between the top 10% average income

[14] Alesina et al. (2018) found that this is also true for consolidation plans.

and median income as the dependent variable. Other studies have focused on the Gini coefficient (e.g. Furceri and Loungani, 2018). However, as discussed in Atkinson et al. (2011) and the related literature, the Gini coefficient can show only minor variations even in the presence of large fluctuations in top incomes. In this case, the use of the Gini coefficient might produce a biased inference.

Therefore, we opted to use the Gini coefficient (and other measures of inequality) for our robustness analysis and the share of top 10% income as our (main) dependent variable. These measures are calculated for the entire population. We instead excluded a measure of poverty at old age as available measures, like the one provided by Eurostat, are available only from early 1996 and suffer from missing data in the early 2000s for all countries, thus markedly reducing the size of our dataset.

Our set of controls, X, includes contemporaneous values and two lags of changes in pension outlays over potential GDP, the structural balance, and the age dependency ratio, plus the level of the output gap. In addition, it includes two lags of the dependent variable. The set of parameters θ is meant to capture the horizon-specific relationship between our controls and the measure of inequality. This strips our estimates of possible endogeneity with respect to business cycle innovations and the anticipation of reforms that they potentially drive, plus unobserved country-specific factors and differences in inequality trends not captured by the country and time fixed effects.

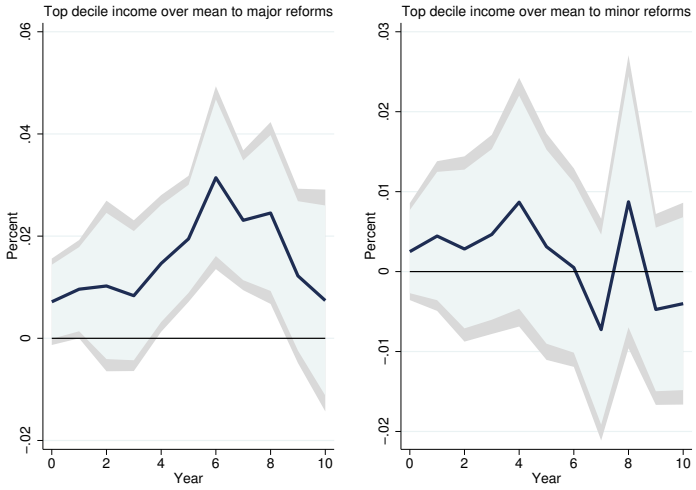
The main parameter of interest is β^h. It represents the effect of the pension reform at horizon h, i.e. after h periods. Therefore, this is the dynamic multiplier for the "reform shock" if we consider the reforms exogenous to all the economic innovations and the structural differences across countries not explicitly included in the model via the controls and the fixed effects.To obtain the cumulative multipliers, we follow the literature on local projections (e.g. Jordà et al., 2020; Ramey and Zubairy, 2018) and computed our dependent variable at horizon h as the difference between the (log) value of the variable at time $t+h$ and the one at time $t-1$, i.e. as $ineq_{i,h} = ineq_{i,t+h} - ineq_{i,t-1}$.

The model described in equation (1) complements the preliminary investigation above (in Table 3 and Figure 5) by investigating the dynamic effects of pension reforms. The estimated cumulative dynamic effects can be plotted in a graph, representing an impulse response function.[15] Figure 6 plots this graph, reporting point estimates with a blue line and the Driscoll-Kraay confidence intervals in light blue (90% level) and gray (95% level) areas.

Figure 6 shows how a generic pension reform increases the share of top incomes. Pension reforms increase inequality only marginally on impact, but they have a large

[15]To analyse the relationship between local projections and reduced-form models as the vector autoregression, see, among the others Ramey (2019), Plagborg-Møller and Wolf (2021), and Montiel Olea and Plagborg-Møller (2021).

Figure 6: The effect of major and minor pension reforms on top decile income over median income.



Note: The graph shows the dynamic effects of a pension reform on the share of top 10% income over median income. Point estimates are reported with a blue line, while the light blue (grey) area represents 90% (95%) confidence levels based on Driscoll–Kraay standard errors. Estimates are based on a panel fixed effects regression with Driscoll–Kraay standard errors.

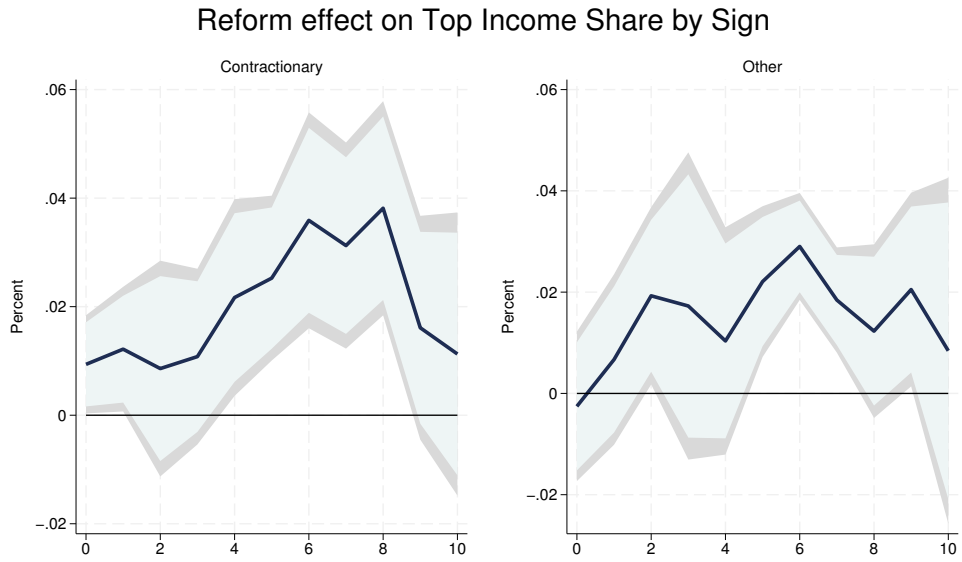
effect after 3 years and reach a maximum 6 years after reform approval, stabilizing for a couple of years and then vanishing after approximately 10 years. This is reassuring about the irrelevance of anticipation and endogeneity effects, as inequality reacts only in the medium term. In terms of magnitude, the income share of the top 10% grows by more than 3 percentage points after 6 years. For comparison, the figure 6 also reports the response of inequality to minor pension system changes, which have no significant effects at any horizon.

As we have discussed before, pension reforms differ in many ways, which can determine different inequality effects. For example, we may surmise that expansionary reforms increase inequality because of their intergenerational effect while contractionary reforms have nonsignificant or short-term impacts. This first relevant distinction is explored in Figure 7, which reports the estimates of equation (1) when we split the reforms according to their sign. We present the results for only two categories, contractionary and other, because we have only two expansionary reforms, which is too small a sample to allow robust estimations.[16] Figure 7 clearly shows that contractionary reforms increase inequality more prominently than the average case. Reforms without a clear sign increase inequality, but their effect is smaller and less significant than that of contractionary reform.

A second distinction that we can make is the one among the tools that a pension

[16]Note that the we included all three categories in the model contemporaneously and these are mutually exclusive.

Figure 7: The effect of contractionary and other major pension reforms on top decile income over median income.

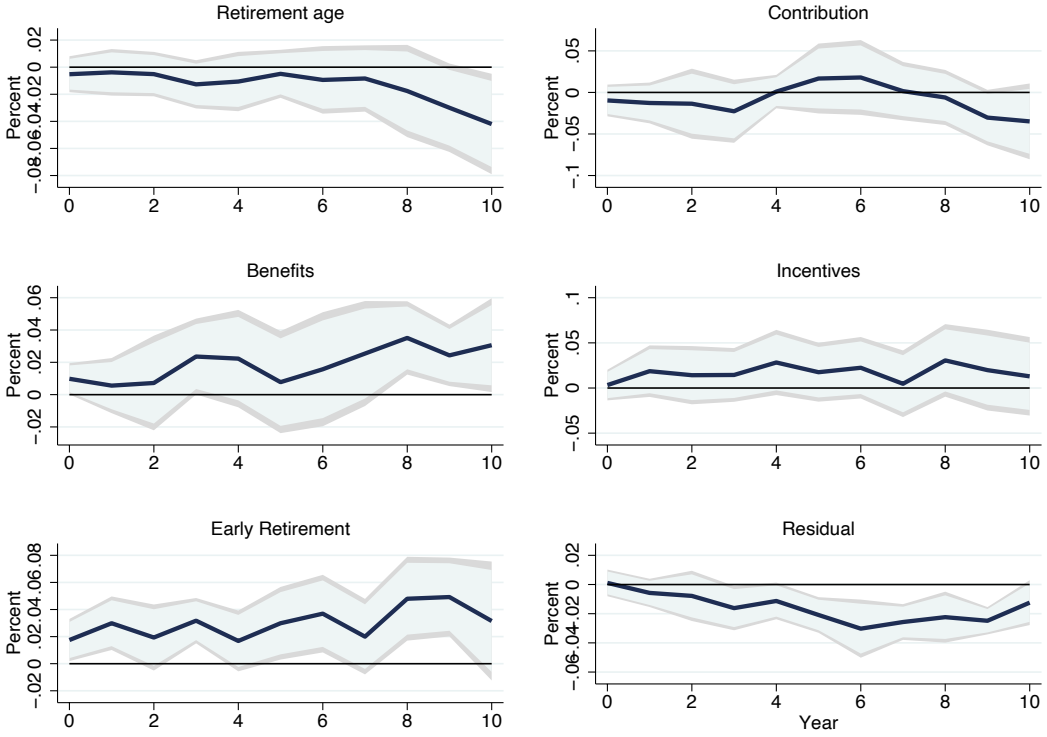


Note: The graph shows the dynamic effects of expansionary (left panel), contractionary (central panel), and other (right panel) pension reforms on the share of top 10% income over median income. Point estimates are reported with a blue line while the light blue (gray) area represents 90% (95%) confidence levels based on Driscoll-Kraay standard errors. Estimates are based on a panel fixed effects regression with Driscoll-Kraay standard errors.

reform implements, which is reported in Figure 8. Also, in this case, equation (1) is enriched with dummies for reforms changing the statutory retirement age (top-left panel), contribution rates (top-right panel), benefits, including changes in indexation (central-left panel), incentives to retire later (central-right panel), reforms of early retirement legislation, including the minimum working age (bottom-left panel), and residual types (bottom-right panel). As mentioned above, this latter type can be considered a proxy of reforms modifying the structure of the pension system, for example, introducing or strengthening specific pillars or speeding up the transition from a pay-as-you-go to a fully funded system (or vice versa). In this specification, reforms with a contractionary sign enter as 1, while reforms with an expansionary sign enter as -1. Therefore, we can interpret the panels as the response to contractionary reforms.

The results in Figure 8 show that the increase in inequality generated by contractionary pension reforms is mostly related to a reduction in benefits and an increase in the early retirement age, and with a more uncertain significance to increases in the incentives to work at old age. Increases in contributions have uncertain effects, resulting in statistically insignificant changes. Two reforms seem to reduce inequality: increases in the pension age (despite only in the very long-term), and, interestingly, reforming the structure of the pension system.

Figure 8: The effect of main targets of major pension reforms on top decile income over median income.



Note: The graph shows the dynamic effects of changes in retirement age (top-left panel), contribution rates (top-right panel), benefits – also including changes in indexation – (central-left panel), incentives to retire later (central-right panel), and residual types (bottom-left panel). Pension reforms on the share of top 10% income over median income. Point estimates are reported with a blue line while the light blue (gray) area represents 90% (95%) confidence levels based on Driscoll-Kraay standard errors. Estimates are based on a panel fixed effects regression with Driscoll-Kraay standard errors.

Finally, we divide our reforms based on the length of the phasing-in. The idea is that the longer the phasing-in of a reform is, the greater the number of workers who exit the labour market in anticipation of the effects of the reform on their retirement (Bi and Zubairy, 2023). As a result, the response of inequality might also change. However, the predicted sign of this change is unclear: inequality might increase if all workers speed up their retirement to avoid the effect of reforms or decline if only the wealthier leave the labour market, as they will have lower benefits. These effects should have an opposite sign if the reforms generate an immediate change, as workers cannot respond to this.

Figure 9 shows the outcomes for this experiment. It reports impulse responses to reforms for short (immediate), medium (up to 5 years), and long (over 5 year) phasing-in periods of the different elements of the reforms. A first relevant result is that the role of phasing-in is highly uncertain for the distributional effects of pension reforms, as the impulse responses are barely significant. This might reflect that different types of reforms do not relate to different phasing-in lengths unequivocally, thus implying that the role of phasing-in could capture opposite distributional effects of a convolution of different reforms. However, despite being more uncertain, the evidence suggests that reforms with short and medium phasing-in periods increase inequality, while those with a long phasing-in reduce it. This is because, as we have discussed above, reforms that reduce inequality (those targeting the system, contributions, or statutory retirement age) on average require a longer phasing-in than reforms that increase inequality (those targeting benefits and the early retirement age).

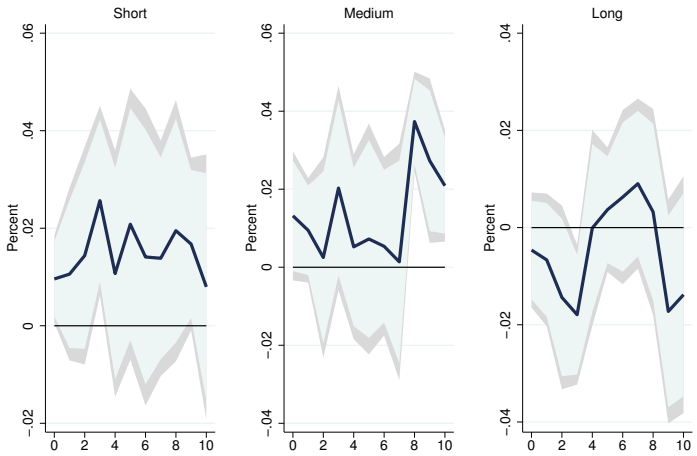
4.3 The role of public debt

Different factors can shape the distributional effects of pension reforms. For example, we can surmise that government credibility and effectiveness can play a crucial role because, in countries where governments are not credible, the likelihood that the reforms will be softened or retrenched in the near future is high. Alternatively, we might believe that countries with higher public debt may experience larger effects on their income distributions because their governments cannot implement policies that can smooth the increase in inequality given the smaller available fiscal space.

This section explores the role of public debt in shaping the distributional effects of pension reforms.[17] This aspect can be explored by letting β^h vary along the share of public debt over GDP. The estimated equation then becomes:

[17]Public debt is not the only factor we have explored. In particular, we found that the share of pension outlays over GDP and over total expenditure plays a similar role to public debt, while the dependency ratio and government effectiveness do not have any significant role. For the sake of brevity, here we decided to report and discuss the public debt as we believe it encompasses pension outlays and public expenditures. Results for other measures are available upon requests to interested readers.

Figure 9: The effect of phasing-in period of major pension reforms on top decile income over median income.



Note: The graph shows the dynamic effects of pension reforms when they have a short (left panel), medium (central panel), or long (right panel) phasing-in period. Regressions are of the share of top 10% income over median income on pension reforms. Point estimates are reported with a blue line, while the light blue (grey) area represents 90% (95%) confidence levels based on Driscoll–Kraay standard errors. Estimates are based on a panel fixed effects regression with Driscoll–Kraay standard errors.

$$ineq_{i,h} = \alpha_i^h + \eta^h T_t + \beta^h ref_{i,t} + \beta_{debt} debt_{i,t} + \beta_{debt}^h [ref_{i,t} * debt_{i,t}] + \theta^h X_{i,t} + u_{i,t}^h \qquad (2)$$

where *debt* is the share of public debt over GDP. The cumulative multiplier is now determined by the average effect β^h, and the marginal effect dependent on public debt β_{debt}^h. Also, equation (2) controls for the share of public debt over GDP to avoid mean effects. This methodology consents to exploring non-linearities in impulse responses while preserving the entire distribution of the interacted variables. Therefore, we avoid possible biases that dichotomization — which is still the standard approach to explore non-linearities in local projection, (see, for example Ramey and Zubairy, 2018; Cacciatore et al., 2021) — can produce by implicitly imposing a threshold (Tafuro, 2023).

The results for this experiment are reported in Figure 10, where we report the effects of general pension reforms for high (80th percentile of the variable distribution) in red and low (20th percentile of the variable distribution) levels of public debt in blue. These two approximately conclude with a country with a debt-to-GDP ratio of 95.1% (a high-level ratio) and 37.5% (a low-level ratio). Although the two responses are statistically indistinguishable, this experiment provides two relevant results. First, the distributional effect of pension reforms is more uncertain in low-debt countries than in high-debt countries. This supports that the larger fiscal space of low-debt countries helps their governments

implement policies that can offset or, at least, smooth the increase in inequality. Second, inequality in low-debt countries increases faster in the short term than that in high-debt countries. related to a stronger reliability of reforms in these countries, which drives workers close to the pension age to bring forward their retirement at the cost of a lower pension.

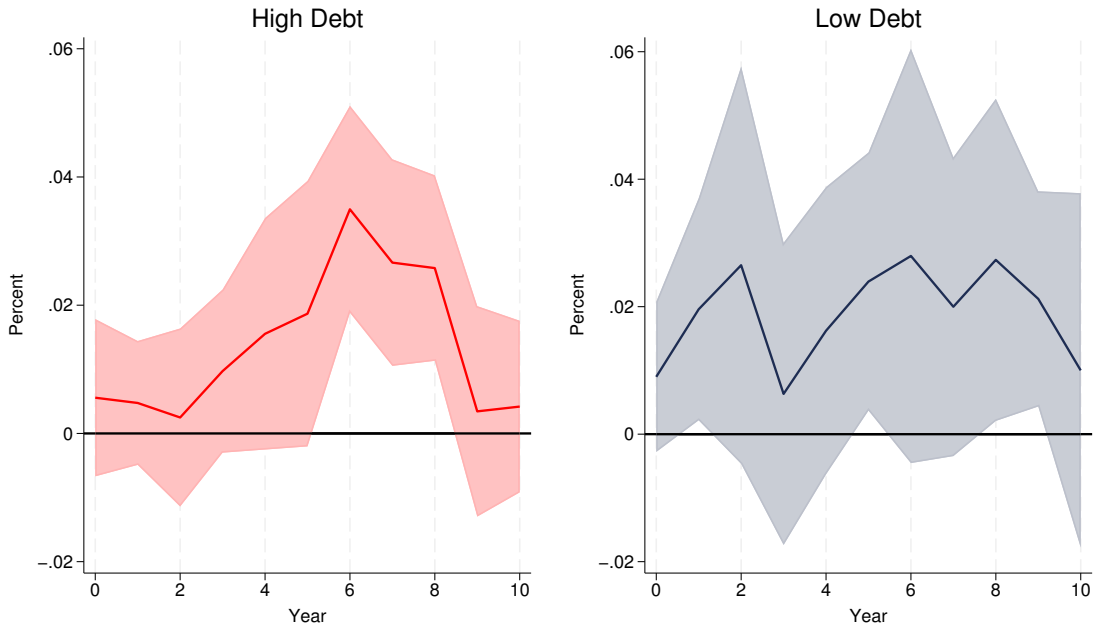


Figure 10: The effect of major pension reforms on top decile income over median income, the role of government efficiency.

Note: The graph shows the dynamic effects of pension reforms on the share of top 10% income over median income in high-debt (left panel) and low-debt (right panel) countries. Point estimates are reported with a blue (red) line while the gray (red) area represents 95% confidence levels for low- (high-)debt countries. Estimates are based on a panel fixed effects regression with Driscoll–Kraay standard errors.

5 Analysis of the transmission mechanism

This section explores the potential transmission mechanisms driving the increase in inequality we have estimated following the approval of a major pension reform. To interpret our results we propose a simplified analytical framework based on Börsch-Supan et al. (2024), that divides the mechanisms driving the distributional effect of reforms as composed of two mechanisms: a "mechanical effect" and a "behavioural reaction".

The mechanical effect consists of the direct impacts that pension reforms exert on pension accumulation and disbursements. Therefore, reforms automatically drive a change in inequality, that we assume to be negative (thus they increase inequality) when they

make the pension criteria more restrictive. This assumption is supported by Hairault and Langot (2008) where it is shown that inequality is increased if reforms involves cutting benefits and increasing contributions, due to the fact that these reforms moderate the redistributive power of the pension system.[18] In addition, Etgeton (2018) demonstrates that reforms aiming at increasing labour attachment, like the ones targeting the retirement age (early or statutory) and increasing the incentives to work longer, have negative distributional effects. This is because the increase in labour supply is more than compensated by the different probability of job loss among households, that is higher for low-incomers.

The behavioural reaction consists in how households modify their labour supply when a reform is approved. In other words, the pension reforms can modify the incentives households have to remain in the active population.[19] We expect that an increase (reduction) in labour force lowers (augments) inequality for two reasons: first, we expect that high-incomers to react more to changes in incentives because they have the physical capacity to work additional years, as their jobs are generally less physical intensive, and the income capacity to leave the labour force earlier, as they generally accumulated more savings and differentiated their income sources. If relative more high incomers are leaving (staying) in the labour force, then their pension income will reduce relatively more (less), thus lowering (increasing) inequality. Second, as we discussed above for the mechanical effect, even if high-incomers and low-incomers react in the same way an increase in labour force can increase inequality because of the relatively higher probability of job loss among low-incomers.

The correct assessment of these two channels would require the creation of counterfactuals that account for one mechanism at the time (see, for example Börsch-Supan et al., 2024). Such an exercise needs data with a larger granularity than the one used in our analysis. We therefore decided to proceed as follow: first, we assess the effects of reforms on the labour supply by using the labour force participation rate as a proxy to investigate the behavioural reaction of households. In line with Bi and Zubairy (2023), the idea is that is that households will try to adjust their labour supply to reforms mainly on the extensive margin, by exiting or entering the labour force. Then, we comment our estimates in light of the results already available in the literature on the mechanical effect of different reforms to speculate on the role of the two mechanisms.

Figure 11 shows the results of this experiment, reporting the usual impulse responses for only the major reforms, considering the entire population (left panel) and the popu-

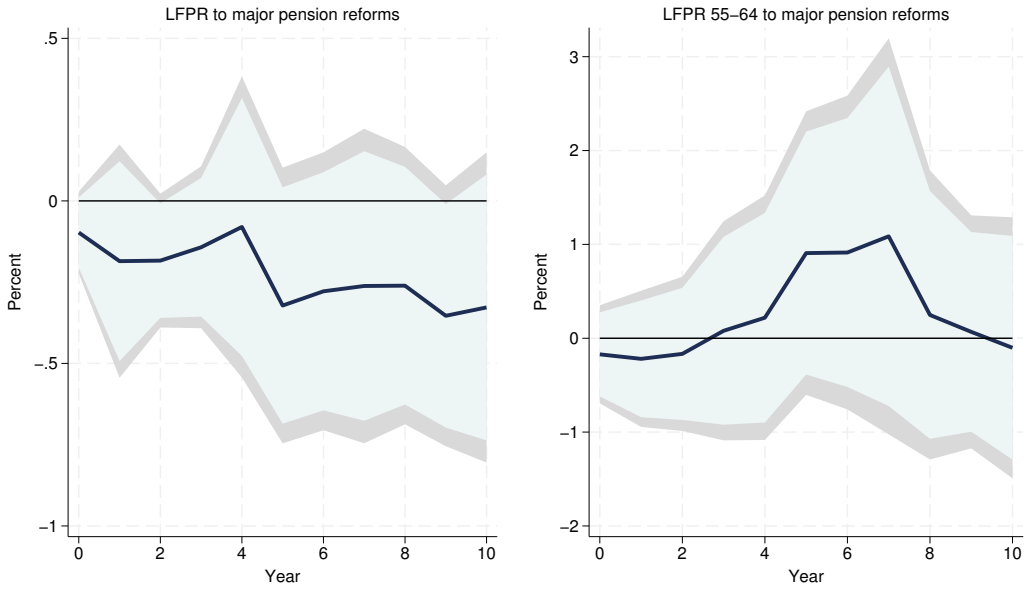
[18]The redistributive role of pension schemes is due to the fact that benefits (contributions) generally increase less (more) than proportionally to pre-retirement income.

[19]For example, benefit reductions lower the incentive to work for additional years, potentially leading households to leave the labour market earlier.

lation included between 55 and 64 years old (right panel).[20] For the experiment, we use data provided by Eurostat and we estimated Equation (1) with this as dependent variable and including the relative lags among explanatory variables.

Our estimates show that major reforms do not have a significant impact on labour supply. This suggests that the mechanical transmission channel might be the one driving the distributional effect of reforms, in line with Börsch-Supan et al. (2024) and with our assumption that the reforms reduce the redistributive power of the pension systems. However, it might also be the case that different types of reforms have different driving mechanisms, that in some cases offset one another. Therefore, we investigate how labour supply is impacted by the different types of reforms.

Figure 11: The effect of major pension reforms on the labour force participation rate.



Note: The graph shows the dynamic effects of a pension reform on the labour force participation rate. Point estimates are reported with a blue line, while the light blue (grey) area represents 90% (95%) confidence levels based on Driscoll–Kraay standard errors. Estimates are based on a panel fixed effects regression with Driscoll–Kraay standard errors.

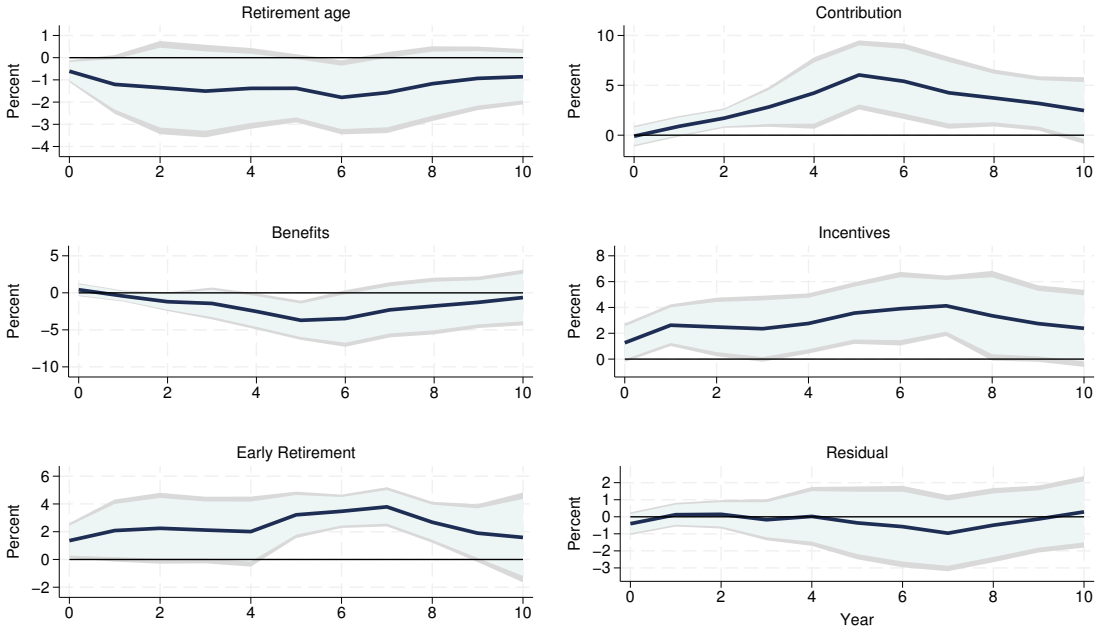
This is reported in Figure 12, that displays the reaction of the labour force participation rate for the total population to major reforms.[21] Similarly to what we observed for inequality, reforms targeting different areas of the pension system affect labour supply differently. Consistently with their goal to increase the attachment to the labour, in-

[20]This cohort is the most relevant for our study because it is the one consisting of households who are about to retire, and therefore might be more sensitive to changes in the pension system.

[21]We decided to report only the results on the population 55 to 64 years old to facilitate the discussion, as there is virtually no difference with the ones we obtained for the entire population. The results for the entire population are available upon request.

creases in incentives and early retirement age have a positive impact on the labour force. Also augments in contributions increases the labour force: our intuition for that is that increases in contributions lower disposable income, leading marginal households to enter the labour force (for example by moving from a part-time to a full-time job).

Figure 12: The effect of main targets of major pension reforms on the labour force participation rate (55-64)



Note: The graph shows the dynamic effects of a pension reform on the labour force participation rate of 55 to 64 years old population. Point estimates are reported with a blue line, while the light blue (grey) area represents 90% (95%) confidence levels based on Driscoll–Kraay standard errors. Estimates are based on a panel fixed effects regression with Driscoll–Kraay standard errors.

Reductions in benefits and increments of the statutory retirement age instead reduce the labour force. The rationale behind this is that cutting benefits reduce the incentive for households to work an additional year. Increasing the statutory retirement age can reduce labour force earlier because, as suggested by Bi and Zubairy (2023), they have a long phasing-in period and households exit the labour force just before the increase in retirement age comes into force in exchange of a lower pension. Residual reforms have a neutral effect on labour supply, indicating that these do not trigger any specific behavioural response.

We can now try to interpret our the distributive impacts of the various types of reforms also in light of the response of labour supply. Increases in early retirement age produced the strongest increase in inequality. This effect is potentially driven by a large increase in labour supply, which is expected to increase inequality, interacted with a mechanical effect

with a similar sign. The latter is motivated by both of the elements discussed above: high-incomers are more likely to benefit from these schemes in that they have larger physical capacity and lower probability of dismissal. This leads to higher accumulation and income during both the latter period of working life and retirement.

Reducing benefits lowers the labour force, that should lead to a reduction in inequality according to our framework should. However, our results show that these reforms increase inequality, thus suggesting that the behavioural effect is more than compensated by a negative mechanical impact. This conclusion is in line with previous studies (Hairault and Langot, 2008) showing how benefit cuts are characterized by a large negative mechanical effect, due to the fact that these reforms hamper the redistributive mechanism of the pension system which strongly penalizes low-incomers during retirement.

According to our estimates, reforms increasing contributions do not significantly affect inequality. However, according to our framework these reform have a negative mechanical effect due to they impact on the redistributive power of the pension system, and generate a negative behavioural reaction due to the increase in labour force they produce. A possible explanation to this puzzle is that the increase in labour force is lead by marginal workers, thus sterilizing the short-term increment in inequality that an increment of a non-progressive tax might exert and increasing their saving accumulation, which, eventually, generates the reduction in inequality that we observe.[22]

Increases in the statutory retirement age lead to a reduction in both inequality and the labour force. We interpret this result as a combination of a behavioural response of households here reduces inequality and more than compensate the mechanical effect described in (Etgeton, 2018). Our intuition behind this is that the high-incomers are the ones leading the reduction in labour force, in that an additional year of work increases their pensions proportionally less than those of lower incomers and they have the capacity to endure such a loss in income. As such a positive impact would require several years to mature, this explanation is also consistent with the delay characterizing the reduction in inequality we observe in our estimates.

Reforms of the pension system structure moderately reduce the labour supply. However, such a response should play a marginal role in the distributional effect of these reforms, which should instead be driven by the increase in savings and the diversification of investments, as also suggested in Andersen et al. (2024). Finally, an increase in incentives to work in old age effectively increases the labour supply. However, this increase potentially concerns only the high skills-high incomers, therefore moderately raising inequality.

[22]It is worth noting that we use a pre-tax definition for all of our measures of inequality. This definition can affect the "mechanical effect" driving the distributional impact of contribution reforms, as contributions – both paid by the employee and by the employer – are part of the pre-tax income.

6 Robustness

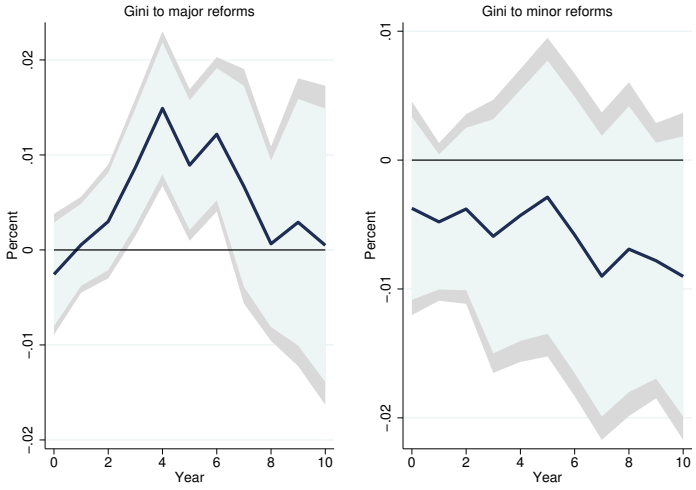
In this section, we present and discuss the most relevant robustness checks for our baseline specification. In particular, we will show that our estimates are robust to the choice of the dependent variable, to the inclusion of elements able to capture anticipation in the specification, to the use of a measure of intensity of reforms instead of dummies and to the instrumenting of our dataset with that of Beetsma et al. (2020). The other robustness checks that concern the most common model perturbations (e.g., different lags, controls, country exclusion, etc.) are reported in the appendix. Our results are very robust to model perturbations: among the one in the appendix, the most relevant are the inclusion of different trends and the exclusion of countries. The former shows that the presence of an increasing trend in inequality is not the main driver of our results, a concern also partially addressed when we showed that some type of reform reduces inequality. The latter demonstrates that our results are not driven by a specific country or a specific set of reforms.

Figures (13)-(15) show the effect of pension reforms on the Gini index (figure (13)), on the share of the bottom 50% over total pretax income (figure (14)), and on the share of the top 1% over total pretax income (figure (14)). We find evidence that major pension reforms increase inequality in all the measures we employ: they increase the Gini index and the share of 1% top incomers, and they reduce the share of bottom 50% — i.e. they reduce the national income share of individuals that receive up to the median income. In addition, we also find that minor changes in the pension system increase the top 1% share of national income. This result confirms, on the one hand, the relevance of changes in pension systems for income inequality and, on the other hand, that aggregate measures of inequality cannot fully capture the effect of policies on top income earners (Atkinson et al., 2011). The result for top 1% earners shows, in fact, a larger sensibility of top earners to policy shocks which have smaller relevance because of their size and goal.

Then, we modify our baseline equation to better control for possible anticipation effects. This is done by including leads and lags of our reform indicator: specifically, when we estimate equation (1), we include among the controls leads of our dummy up to time $t + h$. In this way, we wash out from the estimation the correlation between the changes in inequality and future reforms. In addition, by including leads, we account for the possibility that other countries (which are our control units) could have reformed their pension system between period $t + 1$ and $t + h$ (Dube et al., 2025). In turn, the inclusion of lags is needed to control for possible autocorrelation in pension reforms, which may be particularly relevant in countries that divided a single reform into packages to approve in different years, e.g., Portugal.

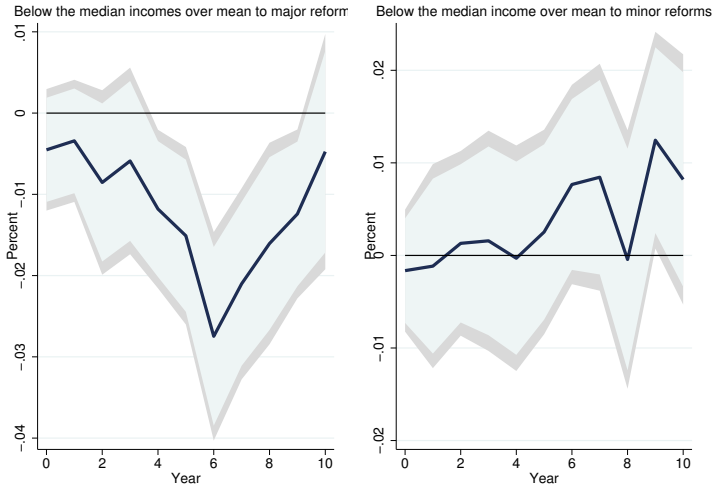
Figure (16) reports the results of this experiment, by iteratively estimating equation

Figure 13: The effect of major pension reforms on Gini index.



Note: The graph shows the dynamic effects of a pension reform on the Gini coefficient. Point estimates are reported with a blue line, while the light blue (grey) area represents 90% (95%) confidence levels based on Driscoll–Kraay standard errors. Estimates are based on a panel fixed effects regression with Driscoll–Kraay standard errors.

Figure 14: The effect of major pension reforms on the bottom 50% income share over median income.



Note: The graph shows the dynamic effects of a pension reform on the bottom 50% income share over median income. Point estimates are reported with a blue line, while the light blue (grey) area represents 90% (95%) confidence levels based on Driscoll–Kraay standard errors. Estimates are based on a panel fixed effects regression with Driscoll–Kraay standard errors.

(1), where now $X_(i,t)$ includes also $ref_{(i,t+i)}$ with $i = 1, .., h$ If anything, we find that the distributional effects of pension reforms might be even larger than what we find in our baseline scenario. In fact, this experiment suggests that the top 10% income share

Figure 15: The effect of major pension reforms on top 1% income over median income.

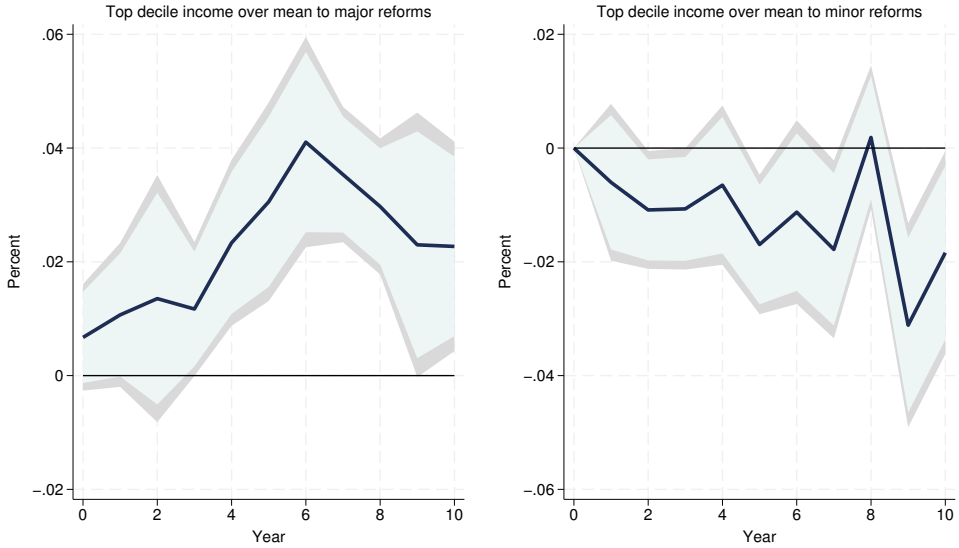
Note: The graph shows the dynamic effects of a pension reform on the share of top 1% income over median income. Point estimates are reported with a blue line, while the light blue (grey) area represents 90% (95%) confidence levels based on Driscoll–Kraay standard errors. Estimates are based on a panel fixed effects regression with Driscoll–Kraay standard errors.

can increase by approximately 5% (against 3% in our baseline) and that this effect is still significant even after 10 years (while, in the baseline, the effect vanishes after 8 years). In addition, minor reforms now seem to mildly reduce inequality, in line with the assumption that these consists mostly in retrenchments from previous major reforms.

Another way in which we can control for anticipation is through the information contained in minor reforms. Intuitively, minor reforms can be a strategy, for governments, to delay a necessary reform or to retrench from a previous reform, thus paving the way for a future reform. Therefore, we estimated a model where we included both major and minor reforms. The results are reported in figure (17), which does not show any relevant difference compared to our baseline. A final experiment to check the robustness of our estimates to endogeneity and anticipation consists in the employing our dataset to instrument the Beetsma et al. (2020)'s one. The rationale behind this is that, by instrumenting the Beetsma et al. (2020)'s data, we estimate the relation between our smaller set of reforms and a wider set of changes related to the pension system, which can carry relevant information of future and previous reforms. Again, the results do not show any significant difference to the baseline (figure (18)). Thus, we can conclude that anticipation and endogeneity not major concerns to estimate the distributional effects of pension reforms.

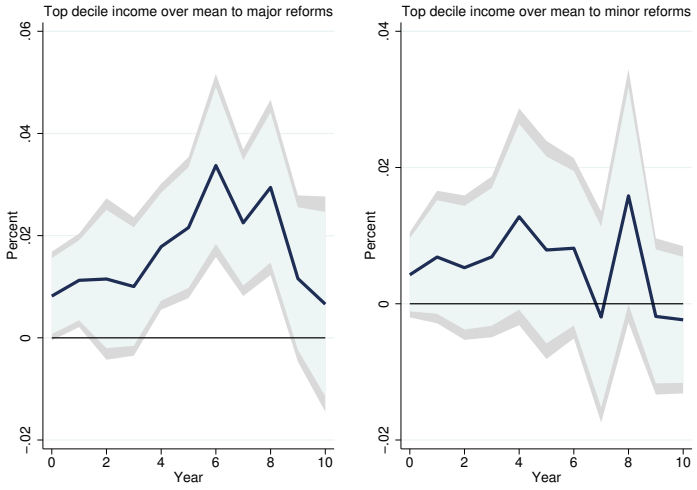
The final experiment that we discuss in this section is the use of a measure of reform intensity to replace the measure of pension reforms (figure (19)). As aforementioned, this index encompasses both major and minor reforms by labeling minor reforms as 1, major

Figure 16: The effect of major pension reforms on the top 10% income share over median income with controls for the lead and lagged effects of major reforms.



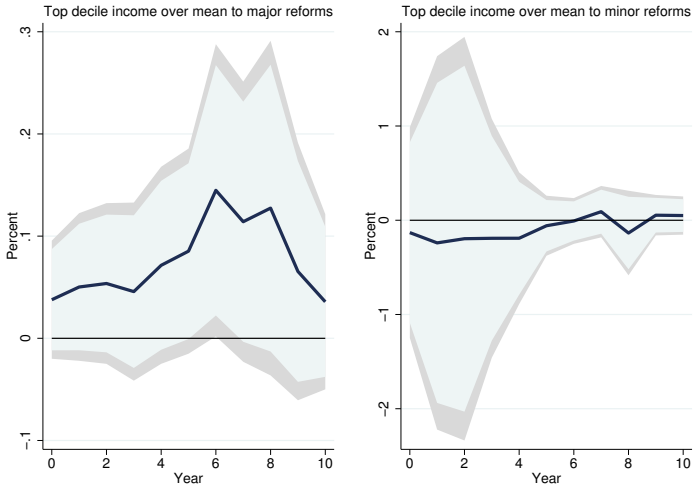
Note: The graph shows the dynamic effects of a pension reform on the top 10% income share over median income when we control for leads and lags of pension reforms. Point estimates are reported with a blue line, while the light blue (grey) area represents 90% (95%) confidence levels based on Driscoll–Kraay standard errors. Estimates are based on a panel fixed effects regression with Driscoll–Kraay standard errors.

Figure 17: The effect of major pension reforms on top 10% income over median income controlling for minor pension reforms.



Note: The graph shows the dynamic effects of a pension reform on the share of top 10% income over median income when we control for minor pension reforms. Point estimates are reported with a blue line, while the light blue (grey) area represents 90% (95%) confidence levels based on Driscoll–Kraay standard errors. Estimates are based on a panel fixed effects regression with Driscoll–Kraay standard errors.

Figure 18: The effect of major pension reforms on top 10% income over median income instrumenting major reforms using the Beetsma et al. (2020) data as instruments.



Note: : The graph shows the dynamic effects of a pension reform on the top 10% income share over median income when we control for minor pension reforms. Point estimates are reported with a blue line, while the light blue (grey) area represents 90% (95%) confidence levels based on Driscoll–Kraay standard errors. Estimates are based on a panel fixed effects regression with Driscoll–Kraay standard errors.

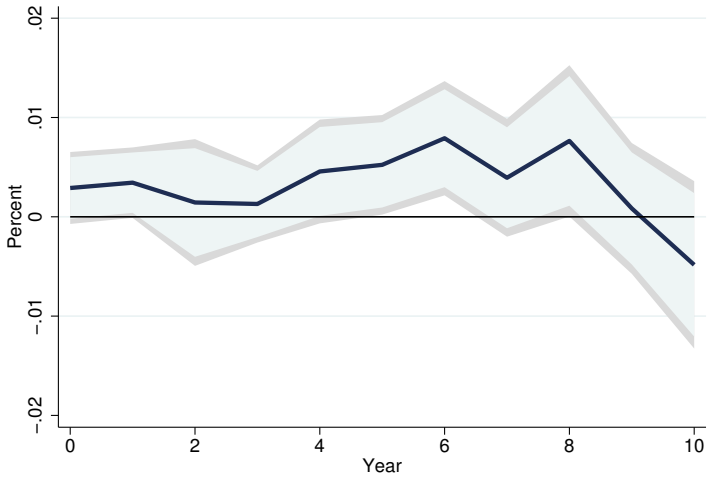
reforms as 2, and very major reforms (for example, reforms that completely reshaped the system — like the one in UK in 2000 — or reforms that are constituted of several subsequent acts each of which could be a major reform — like Portugal between 1993 and 1995). In this way we can account for the heterogeneous intensity of the reforms, and understand if the most relevant changes in the pension system drive the distributional effects of the reforms. As figure (19) shows, there is no relevant difference compared to the baseline, thus suggesting that our estimates are not severely biased by the reforms' heterogeneity.

7 Conclusion

This paper investigates the effects of pension reforms on inequality. We construct a new dataset on major and minor pension reforms in 14 European countries (Austria, Belgium, Denmark, Finland, France, Germany, Greece, Ireland, Italy, the Netherlands, Portugal, Spain, Sweden, and the United Kingdom) over the sample 1990–2018 using a narrative approach. Our dataset builds on a recent study exploiting pension systems with narratively identified datasets (Verbič and Spruk, 2019; Fong and Leibrecht, 2020; Beetsma et al., 2020; Bi and Zubairy, 2023).

We identify 54 major and 75 minor pension reforms adopted and implemented in the

Figure 19: The effect of the intensity of major pension reforms on the top 10 income share over median income.



Note: The graph shows the dynamic effects of a pension reform on the top 10% income share over median income when we control for minor pension reforms. Point estimates are reported with a blue line, while the light blue (grey) area represents 90% (95%) confidence levels based on Driscoll–Kraay standard errors. Estimates are based on a panel fixed effects regression with Driscoll–Kraay standard errors.

14 countries that we focus on. In our dataset, we also distinguish pension reforms that are contractionary (reforms aimed at strengthening the sustainability of the pension system), expansionary (reforms that weaken the sustainability) and other reforms that cannot be identified as either contractionary or expansionary. We further classify major reforms into different categories based on different features: their phase-in period and whether they addressed pension benefits, the retirement age, pension contributions, and incentives for older workers to remain in the workforce.

We then use our dataset to analyse the effects of major and minor pension reforms on inequality. Although our focus is on top 10% income, we also consider measures of poverty, the Gini coefficient and bottom 50% income. Our main results suggest that inequality increases significantly following a major pension reform whereas minor reforms do not affect inequality significantly. Top 10% income increases significantly after the implementation of a major pension reform, and the bottom 50% income share falls such that the gap between the top and the bottom of the distribution increases.

In particular, our results suggest that the distributional effect is almost entirely led by contractionary reforms, i.e., reforms aimed at strengthening the sustainability of the system. In addition, we show that expansionary reforms generate a much milder and often insignificant reduction in inequality. These effects are mostly long-term fluctuations, thus suggesting that they reflect changes in inequality among retirees.

Not all reforms have the same distributional effect. Increases in the retirement age and

in contributions reduce inequality, while reductions in benefits and increases in the early retirement age increase inequality. Of particular interest is that reforms changing the structure of the pension system reduce inequality. These differences are due to different interactions between the mechanical effect of reforms and the behavioural response of households, with the former being the main contributor for the distributional impact of an average reform.

Therefore, the design of the reform matters for its effect on inequality. This result can help the policy makers planning these reforms target inequality and, therefore, opposition to them, and it has the potential to ease the reform approval process, facilitating timelier adjustment of the pension system. In our robustness tests, we find some evidence of a role of government effectiveness in determining the distributional effects of the reforms. This finding seems to suggest that the behavioural response of workers is an essential element for inequality to arise. This is an open question that future research should investigate.

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Appendix A: Pension reforms in Finland and in the United Kingdom

To better understand how pension reforms have been classified, we will present examples from several countries.

A.1 Examples of major reforms

A.1.1 Finland 1993-1996

Between 1993 and 1996, Finland approved and implemented a wide set of pension reforms. These reforms aimed at incorporating OECD recommendations and, despite being approved in different steps, represent a single reform and should be evaluated as such. The broader share of the reform was approved in 1996. The 1997 OECD economic survey on Finland stated that the Finnish pension system underwent a far-reaching reform and that it incorporated many of the recommendations that the OECD had proposed in its earlier economic surveys. These reforms were very ambitious and aimed at making the system more sustainable. In 1993, the retirement age was increased, accrual rates were reduced for the public sector, and new indexation was introduced. In 1994, the retirement age was increased, while there was some moderation on the indexation side. In 1996, accrual rates were diversified to incentivize working in old age, the number on which pension benefits were calculated was increased to reduce benefits, and the indexation was changed again. The following table details some of the changes in the pension system.

Table A.1: The 1993-1996 pension reform in Finland.

	Retirement age	Accrual factors	Pensionable wage	Indexation
1993	Retirement age in the public sector increases from 63 to 65 years, the same as in the private sector	Accrual rates in the public sector reduced from 2.2% to 1.5% per year, the same as in the private sector		New index for pensionable wage introduced based on 50% weights of CPI and the earnings index net of employees' pension contribution
1994	Minimum age for full-time and part-time early retirement changed from 55 and 60 years respectively changed to 58 years and minimum age for unemployment pension raised from 55 to 60 years.		Earnings losses after 55 years disregarded when calculating the pensionable wage.	Indexes of the pensionable wage and of the post-retirement pension frozen for 1994.
1996		Accrual factor of active workers of over 60 years increased from 1.5% to 2.5%. Accrual rates of early-retirement and disability beneficiaries reduced from 1.5% to 1.2% for the 50-60 years age group and from 1.5% to 0.8% for the 60-65 age group.	Pensionable wage based on the last ten years of every employment contract changed to the last four years.	Indexation of post-retirement pension based on a 80% weight of the CPI and a 20% weight of the earnings index net of employees' pension contributions.

Source: 1997 OECD Economic Survey on Finland.

In addition to the measures detailed in the table above, Finland introduced other measures. Pensions were not adjusted for inflation in 1994, implying a one-time permanent

reduction in pension expenditures. Since 1997, pension funds have been allowed to invest in more risky assets, such as shares increasing the rate of return on their investments and increased funding.

We classify these reforms as major. Moreover, the reform strengthened the linkages between preretirement income and benefits, reduced incentives for early retirement and strengthened incentives for labour force participation. For these reasons, we classify this reform as contractionary, in contrast to Beetsma et al. (2020), who classify it as "both". The description of the reform makes the 1993 and 1996 legs seem contractionary, while it is true that the 1994 part of the reform contains both expansionary and contractionary elements. However, we label the reforms contractionary because we consider them part of a single large reform that has a clear contractionary stance.

A.1.2 UK 2007

The United Kingdom implemented a major reform in 2007 aimed at making the system more sustainable. As the 2007 OECD economic survey on the UK describes, through the approval of the Pension Act, the government reformed a wide range of aspects: it broadened the availability of the public pension, and it changed the indexation by switching from prices to earnings. A follow-up reform in 2008 included a statutory duty on employers to automatically enrol eligible workers into workplace pension schemes that must meet minimum qualifying requirements and the public provision of a trust-based nonemployer pension scheme for those otherwise without access to a qualifying scheme. The introduction of the employer duty to auto enrol eligible workers was planned for 2012. The 2007 reform was preceded by an increase in the public sector pension age from 60 to 65 for existing workers from 2012. The union agreed to this reform since it was allowed to negotiate other reforms that supposedly would recover benefits equivalent to those lost.

The reform was reported by all sources (e.g., NATLEX and Beetsma et al. 2020). We classify this reform as expansionary, in contrast to Beetsma et al. (2020), who classify it as both expansionary and contractionary. In fact, from the description of the reform, a clear expansionary aspect emerges related to the change of indexation from prices to earnings that increased benefits and the broadening of public pension availability through the increase in the contributory credits for caring responsibilities and the decrease in the minimum number of contribution years to 30. Moreover, aspects of the reform related to changes in workers' eligibility may have an unclear effect. Finally, the automatic enrolment provision, the mandatory 3% employer contribution and the new low-cost scheme of personal accounts were thought to encourage workers to supplement state pensions with private pensions and ensure that low- to middle-income earners have access to low-cost pension schemes in which to save. The fiscal implications of the reform were clearly expansionary overall. This might explain the choice in Beetsma et al. (2020) to classify the

reform as "both". Some features of the reform were implemented immediately, whereas others were delayed by 3 or 5 years depending on the specific change.

A.2 Examples of minor reforms

A.2.1 Finland 1992

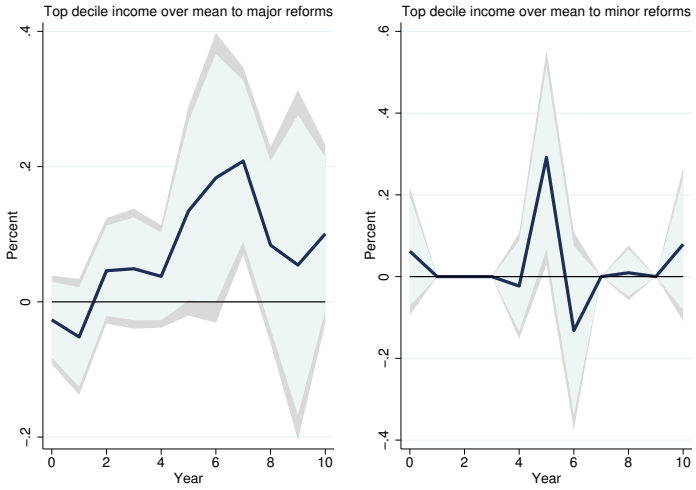
In 1992, Finland introduced a minor change in the pension system. This reform, which we classify as minor, consisted only of a change in the employee's contribution to the occupational pension. The contribution to the occupational pension system was lowered from 16.9% to 14.4%. As complements, the reform also included a lowering of employers' contribution to the national pension. In addition, employers' unemployment and sickness insurance contribution was increased, as was the contribution to the national pension. These changes did not affect the overall system or the size of contributions. This reform is also reported in other databases, such as Beetsma et al. (2020) and Natal. We classify this reform as minor.

A.2.2 France 2007

In 2007, the French government decided to approve an incentive for keeping elderly people in work. In particular, the government raised the quarterly pension premium 5% per year beyond the age of 60, the same as the target rate for the early retirement penalty. In addition, the government relaxed the rules for combining employment and pension income for those aged 60 and above. This reform, reported by (Bisciari et al., 2009), is classified as minor.

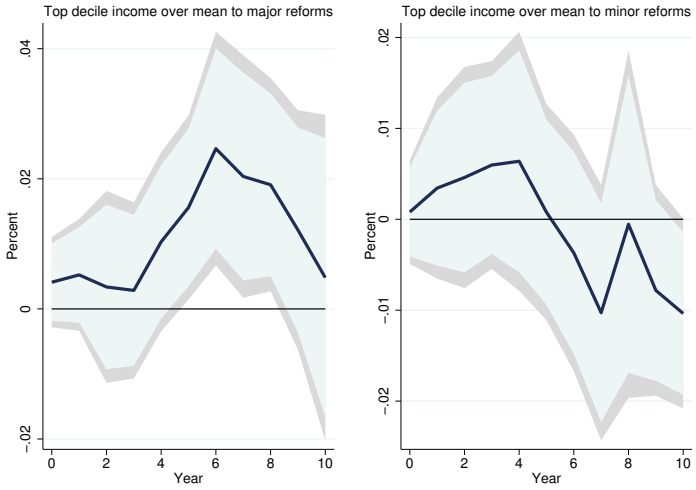
Appendix B: Additional Robustness Checks

Figure B.1: The effect of pension reforms on top 10% income over median income, GMM estimator.



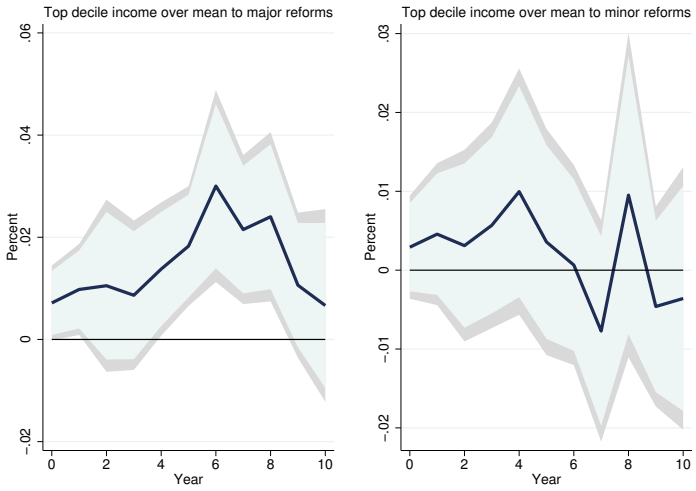
Note: The graph shows the dynamic effects of a pension reform on the top 10% income share over median income when we use the GMM estimator. Point estimates are reported with a blue line, while the light blue (grey) area represents 90% (95%) confidence levels based on Driscoll–Kraay standard errors. Estimates are based on a panel fixed effects regression with Driscoll–Kraay standard errors.

Figure B.2: The effect of pension reforms on top 10% income over median income, no control variables.



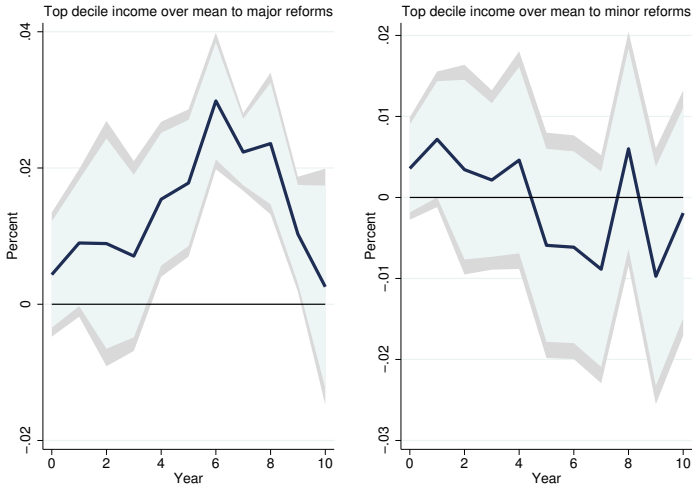
Note: The graph shows the dynamic effects of a pension reform on the top 10% income share over median income when we do not add controls. Point estimates are reported with a blue line, while the light blue (grey) area represents 90% (95%) confidence levels based on Driscoll–Kraay standard errors. Estimates are based on a panel fixed effects regression with Driscoll–Kraay standard errors.

Figure B.3: The effect of pension reforms on top 10% income over median income, additional control variables.



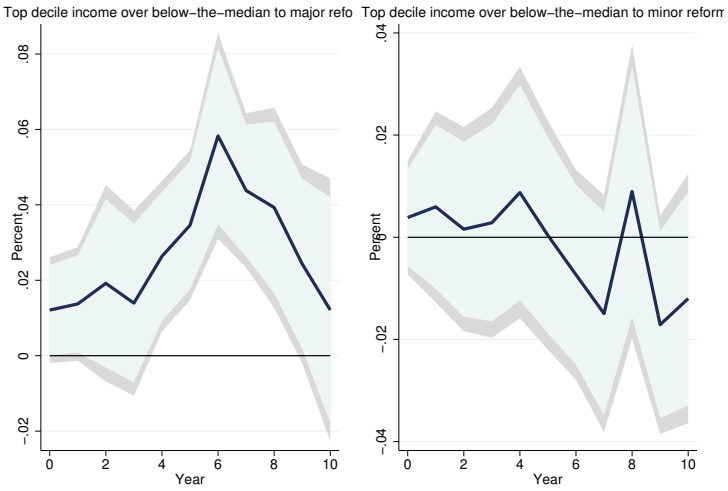
Note: The graph shows the dynamic effects of a pension reform on the top 10% income share over median income when we include, as additional controls, the change in population and a measure of government effectiveness provided by the ICRG Point estimates are reported with a blue line while the gray area represents 95% confidence levels. Estimates are based on a panel fixed effects regression with Driscoll-Kraay standard errors.

Figure B.4: The effect of pension reforms on top 10% income over median income, 4 lags of control variables.



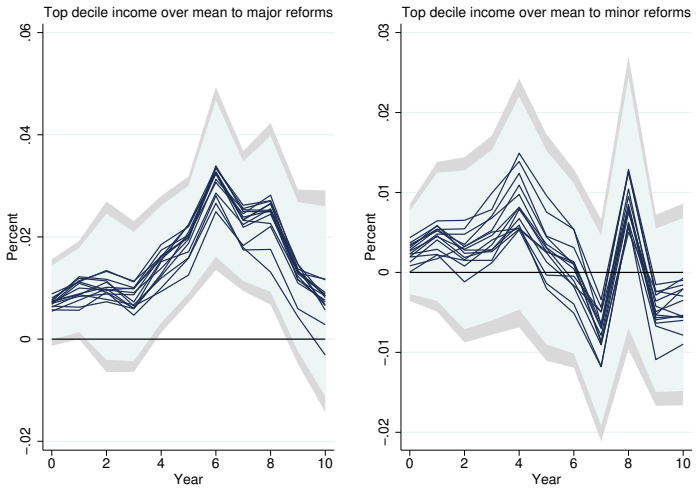
Note: The graph shows the dynamic effects of a pension reform on the top 10% income share over median income when we add up of 4 lags of controls. Point estimates are reported with a blue line, while the light blue (grey) area represents 90% (95%) confidence levels based on Driscoll–Kraay standard errors. Estimates are based on a panel fixed effects regression with Driscoll–Kraay standard errors.

Figure B.5: The effect of pension reforms the share of income of top 10% earners over bottom 50%



Note: The graph shows the dynamic effects of a pension reform on the income share of the top 10% of earners over the bottom 50%. Point estimates are reported with a blue line, while the light blue (grey) area represents 90% (95%) confidence levels based on Driscoll–Kraay standard errors. Estimates are based on a panel fixed effects regression with Driscoll–Kraay standard errors.

Figure B.6: The effect of pension reforms on top 10% income over median income excluding one country at the time.



Note: The graph shows the dynamic effects of a pension reform on the top 10% income share over median income when we add a linear trend. Point estimates are reported with a blue line, while the light blue (grey) area represents 90% (95%) confidence levels based on Driscoll–Kraay standard errors. Estimates are based on a panel fixed effects regression with Driscoll–Kraay standard errors.

Figure B.7: The effect of pension reforms on the top 10% income share over median income including a time linear trend

Note: The graph shows the dynamic effects of a pension reform on the top 10% income share over median income when we include a country-specific trend and a full set of leads and lags. Point estimates are reported with a blue line, while the light blue (grey) area represents 90% (95%) confidence levels based on Driscoll–Kraay standard errors. Estimates are based on a panel fixed effects regression with Driscoll–Kraay standard errors.