

The political process and phase-in period of pension reforms

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Abstract

We construct a novel database of pension reforms in 23 countries from 1970 to 2023. Based on a narrative identification and automated text filters we identify 1,828 legislated reform packages and determine whether those reforms improve or worsen the financial position of the current participants and the length of the phase-in periods. The research contributes to understanding the political economy of pension reforms and their implementation timelines. Reforms making pension systems less generous have longer phase-in periods, but there is no significant relation between demographic conditions and length of the phase-in period.

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Data availability

The full dataset with all reforms underlying this analysis will be downloadable from [Netspar.nl](https://netspar.nl)

1 INTRODUCTION

In December 2022, the House of Representatives of The Netherlands voted in favour of a major redesign of the funded pillar of the Dutch pension system. This new pension law is the culmination of (at least) 12 years of negotiations between social partners, the government, and parliament. This reform has a phase-in period until 2028. The Netherlands is not the only country which is in the process of implementing a major redesign of its pension system; Italy's government started new negotiations with labour unions in January, and the French government encountered massive protests unified in the yellow vests movement. Those countries that did successfully redesign their pension systems, usually did this after external pressure (e.g. Greece) or after a long political process (e.g. Germany and Sweden).

Recently, Bi & Zubairy (2023), Beetsma, Klaassen, Romp & Van Maurik (2020), and Romp & Beetsma (2023) use self-constructed databases of pension reforms to analyse the timing of those reforms and the response of workers close to retirement. Bi & Zubairy found that the phase-in period has a significant effect on the labour force participation rates of the elderly. Their analysis requires that the pension reforms are unexpected exogenous shocks. Beetsma and co-authors, however, concluded that the nature of pension reforms was driven by demographic developments and that the timing of those reforms was determined by the business cycle. The predictability of the ageing process, and the fact that many pension reforms were preceded by years of negotiations, make that those reforms can hardly be classified as a surprise to those close to retirement.

This raises the question whether an ageing society and business cycle conditions affect the length of the political process and the phase-in period of pension reforms? **Do countries with a more pressing ageing problem implement reforms with a shorter phase-in period?** If so, then the ever increasing old-age dependency ratio will result in more unexpected reforms, with shorter phase-in periods, which increases the uncertainty surrounding retirement planning.

In this paper, we will answer this question. We will first combine the dataset used by Bi & Zubairy (2023) with the unpublished database with all reforms underlying Beetsma et al. (2020) and extend the sample to 2023. Crucially, we extract data on phase-in periods from the descriptions and remove double entries from the raw data. This allows us to focus on individual reform measures instead of country, year specific reform regimes as is common in the literature. In the second part we will relate the length of the phase-in periods to the demographic conditions and the state of the business cycle. We find that reforms with a contracting nature – reforms which make the pension system less generous for current participants – on average have a phase-in period which is on average 4.8 years longer than politically easier to implement expanding reforms. We find no statistically significant relation between the demographic conditions, the state of the business cycle, and the length of the phase-in periods.

This paper relates to the literature started by Romer & Romer (2010) who use narrative identification to date and classify economic shocks and reforms using more textual oriented data sources. Duval et al. (2018a,b), Bi & Zubairy (2023), Beetsma et al. (2020) and Romp & Beetsma (2022) are merely four examples. Duval et al. focuses on product market regulation, employment protection legislation and unemployment benefit systems, the other three papers focus on pension reforms. All publications

used reforms extracted using narrative identification from sources such as ILO's NATLEX database, OECD's Economic Surveys and Pension at a Glance, the European Committee's LABREF and PENSREF databases, and the ISSA database. Those sources always include a short description of each reform, but provide little to no classifications. Also, in these databases, actual reforms are often mixed with merely plans for reform and white papers. In this paper, we distinguish between actual pension related reforms and other (non-pension related) reforms, plans for reform, and white papers using a country-specific text filter and successive careful reading of the remaining reforms and the underlying sources.

This paper provides three contributions to the existing literature. First and most important, we construct and make available a database of pension reforms in 23 countries used in Beetsma et al. (2020) from 1970 to 2023. This new database is a combination of those used by Bi & Zubairy and Beetsma et al.¹. The first dataset covers 10 countries, whereas the second dataset covers 23 countries but lacks information on motivation and phase-in periods. We extend the sample and add the years 2018 to 2023. Beetsma et al. only include reforms which affect the financial stance of the government. We also include reforms that have little effect on public finances, but clearly do affect old age income, such as reforms to occupational funded pension schemes. These reforms are mostly missing from the two underlying databases, even though they clearly affect old-age income and probably affect retirement behaviour. Each record includes a short description of the reform and a set of characteristics based on a careful reading of the relevant background information. Second, this paper focuses on individual reforms, not on country-year combinations. Since a country can implement multiple reforms in a certain year, pooling all reforms legislation in a country in a specific year destroys relevant information. We revisit the underlying raw databases and merge records which refer to the same reform. As a third main contribution we identify the phase-in periods of each reform, which is used to answer the main question: Do countries with a more pressing ageing problem implement reforms with a shorter phase-in period?

2 THE DATA

2.1 Pension reforms

Our database spans 23 OECD countries over the period 1970 to 2023 and covers all reform measures, both smaller and more fundamental reforms that affect the pension contributions and pension benefits of individuals². The raw database underlying Beetsma et al. (2020) is the starting point of our analysis. We extend the database of Beetsma et al. (2020) in three dimensions. First, Beetsma et al. only includes pension reforms which directly affect the government's fiscal stance. We also include all reforms which affect the contributions and benefits from the participants' perspective, the main addition are reforms which affect occupational pension schemes. Second, Beetsma et al. (2020) end their sample in 2017, we extend it to 2023. Third, we merge records from the underlying databases

¹ Huixin Bi shared the full dataset underlying their analysis. I also have full access to the data and tech underlying Beetsma et al. (2020) including the textual descriptions of all reforms from 1970 to 2017.

² The full dataset will be available from www.netspar.nl

which cover the same reform. This allows us to focus on legislated reform packages, instead of country-year specific reform regimes.

Closely related to the work by Beetsma et al. (2020, 2023) is Bi & Zubairy (2023) who published a similar database running from 1962 to 2017. They base their dataset fully on publications by the OECD. Their dataset contains ten of our 23 countries: Australia, Belgium, Denmark, Spain, Finland, France, United Kingdom, Italy, Japan, and New Zealand. Most of their reforms were also included in Beetsma et al. (2020). The Bi & Zubairy database was especially useful since it includes data on implementation lags.

To extend the database of Beetsma et al. to include pension reforms which do not directly affect the government's fiscal stance we revisited their underlying databases: the NATLEX database of the International Labor Organization (ILO, 2019), information provided by the International Social Security Association (ISSA, 2019), publications by the OECD (2007, 2009, 2011, 2012, 2013, 2014, 2015, and 2017), and the European Commission's LABREF database (2019). Pension reforms from 2018 to 2023 were collected by research assistants with country specific knowledge using web search, official publications of national governments and the information in OECD (2019, 2021, 2023). Whenever necessary to properly date and categorise the main reforms, we used other texts such as various country specific chapters in Feldstein & Siebert (2002) and other sources (all listed in the dataset).

All databases together contain approximately 15,000 records for our 23 countries. Each record typically has a short description of a reform, a year (usually the year of legislation), and sometimes additional information such as a precise date of implementation. The ISSA database, the LABREF database, and the records from ad hoc sources often contain additional background information, a more extensive description of the reform, and sometimes a brief history of the events leading up to that reform. The records from the various OECD publications lack a precise year, but the description, the caption of the table, and combining adjacent OECD publications, the moment of legislation could usually be narrowed down to a period of one or at most two years. Switzerland has direct referenda on pension related reforms. For that country, we use the year in which parliament legislated the reform, regardless of whether parliament's decision is ultimately confirmed by a referendum.

To clean the data, we first made sure that all information is in English. Especially the NATLEX database contained many records in other languages (mostly German, French, and Spanish). Using the google translate extensions in google sheets, we determined the original language of each record and if not English, we automatically translated that field to English.

Second, we removed all non-pension related records from the dataset. The NATLEX, ISSA, and LABREF databases also contain records describing agreements between social partners, labour market reforms, and health related reforms. In our analysis, we use only reforms which are old-age pension income related and legislated by a national government. This implies that we had to exclude health related reforms (also old-age health), survivors' pension, agreements between social partners, and reforms related to pension supervision and pension management. We also excluded non-financial reforms such as the educational US "Savings are vital to everyone's retirement act of 1997" and the "Social Security Retirement Planner" of 2000. Finally, we excluded international agreements since those are typically the result of negotiations between (at least) two different governments.

Removing these unrelated records was done in two steps. In a first step, we use a country specific text filter. For each country we created a list of keywords with a keyword specific score. For example, if the term “retirement” occurs in the description of the reform, we grant that record a high score. Terms like “occupational disease” and “birth” are indicative of a labour market reform which was not pension related and we give those terms a negative score. Also terms like “management”, “supervision”, etc... which are clearly related to purely supervisory changes, advisory boards, and international agreements carry a negative score. The total score of a record is the sum of all scores of the occurring keywords and we dropped those records with a negative score. The filter system is country specific. General terms like “retirement” and “old age” are always indicative of a pension related reform and carry a positive weight for all countries. To improve the filter, we added country specific terms, using descriptions provided by the OECD and the European Commission’s PensRef database. The best example of these country specific filters is Germany. In Germany, the welfare system is legislated in the Sozialgesetzbuch (SGB), which consists of 14 chapters. The German old age pension system is covered by chapter VI, so all terms involving SGB I-XIV, except SGB VI are discarded. This automated filter was applied to the 11,009 records from NATLEX and reduced the number total number of records in our databases by 5,929³.

After we identified and removed non-relevant records using these textual filters, we processed the remaining 8,653 records manually as follows:

- 1) Clear or unclear: is the description sufficient to determine what was done? If not, then classify as unclear and discard. If clear, then
- 2) Relevant or irrelevant: is the record old-age pension related according to the criteria mentioned above and legislated by the country’s government, then classify as relevant. All records classified as relevant may be used later to be combined with other records to form a complete picture of what happened that year. If not, then classify as irrelevant. If relevant, then
- 3) Clear or unclear: is the description in the record sufficient to determine the key characteristics such as the year of legislation, possibly the nature of the reform in that record, possibly combine it with other records, and determine the years of implementation? If not, then classify as unclear and discard.

After these steps, 2,908 records remained. These 2,908 records include records describing the same reform; a direct consequence of combining overlapping datasets. Beetsma et al (2020), solved this problem by focusing on country-year reform regimes. In their aggregation from individual records to country-year observations, they automatically lost information on individual reforms. For their analysis, this was sufficient, since they merely focused on whether a country implemented at least one reform in a given year. In the empirical analysis below, we focus on individual reforms, so we need to merge records from different datasets referring to the same reform. Based on a careful reading of all records in a specific country-year combination – and often the year just before and immediately after –we merged double entries into reform packages. Specific names of reforms, serial numbers of legislated laws, dates, and specific values of e.g. adjusted contribution rates, indexation rates, ages,

³ The full set of generic and country-specific terms is available upon request.

etc... were particularly useful in this time-consuming stage⁴. Ultimately, we identified 1,828 separate pension reforms packages.

For each reform package we try to determine 1) whether the reform makes the system more generous for the current participants or whether the pension system becomes less generous for the current participants, 2) the first year in which the reform changes the pension system, and 3) the last year in which the reform changes the pension system.

For all reform packages, we determine whether the reform package makes the pension program more generous (expanding), less generous (contracting), or whether the budgetary effect is unclear for the current participants. Clear examples of “expanding” reforms are lowering the retirement age, increasing benefits, increasing coverage, and lowering contribution rates. Besides these trivial cases, we also classify reforms which clearly make pensions less risky as “expanding”. Finally, this category contains reforms which in expectation make the system more generous such as linking benefits to a wage index instead of a price index. Contracting reforms are the opposite of expanding reforms, so increasing the retirement age, lowering benefits directly or by providing less generous indexation, etc... Following Beetsma & Romp (2023) we classified reform packages consisting of multiple measures which work in opposite directions as “Contracting”. A careful reading of those reform packages clearly shows that in those mixed packages the contracting elements dominates. These are usually system wide reforms where the dominant contracting elements are partly offset to protect some vulnerable groups in society. For the sake of clarity, we label reform packages with a solely expanding nature “Expanding only”.

A third group consists of reform packages where the budgetary effect on current participants is unclear, sometimes because the budgetary effect is truly uncertain, but often because the description is too short. An example of a reform package where the budgetary effect is truly uncertain is the Dutch “Future of Pension Act” (Wet Toekomst Pensioenen) of 2023. Under that law, the funded second pillar is transformed from a mix of collective defined contribution with defined benefit aspects to a mostly individual defined contribution system. Reform packages like this and reform packages where the description is uninformative are classified as “Relevant, but unclear”.

The classifications of “Expanding only” and “Contracting” are closely related to the classifications with the same name in Beetsma et al. (2023). The key difference is that they focus on whether the reform is expanding or contracting from the government’s perspective. Often the labels corresponds since reforms which are expansionary from the government’s point of view typically make the pension more generous for current participants. Bi & Zubairy (2023) use a classification similar to ours and we use the classification in those databases wherever possible.

Table 1 shows the distribution of these 1,828 pension reform packages over the 23 countries in our dataset. This table shows that the number of identified reforms varies strongly per country. The number of identified reform packages, however, is not a perfect indicator of how often countries change their pension systems. Differences between countries can also arise for other reasons:

⁴ Attempts to automate this process using AI (ChatGPT 3.5) were to no avail. The links between various records were too subtle for general AI at this stage.

- 1) We cannot always properly pool records in our dataset. The descriptions in ISSA database and LABREF are often extensive and clearly describe a set of measures which belong to one reform package. Larger reforms often contain five or more individual measures, which are mentioned separately in NATLEX, the OECD publications, and in the Bi & Zubairy dataset. If such an extensive description is missing, then we had to include clearly relevant individual legislated measures as separate reforms.
- 2) Differences between countries also arise because some countries provide a better description in the extensive NATLEX database, so it was easier to determine whether those records were relevant. Those countries therefor have more reforms than others. The description in NATLEX for those countries does allow us to determine whether a record is relevant, but the nature of the reform (expanding or contracting) is unfortunately often unclear.
- 3) Pension systems differ between countries and the greater the number of pension systems a country has (e.g. a separate system for civil servants, miners, agricultural workers, bankers, seafarers, etc... like France used to have), the more pension reforms will be legislated. This is particularly relevant for France and Belgium.
- 4) Some countries have automatic adjustments mechanism, other countries have to legislate adjustments e.g. due to changing prices and ongoing ageing on a yearly basis. This is one of the explanations for why the Netherlands has relatively few reforms: public pensions follow the minimum wage since 1974. In other countries, the yearly changes to public pensions are a discretionary decision, often based on some cost-of-living index. Austria for example, nowadays has a yearly "Pensionsanpassung (PAG)".

The third column shows the number of reform packages which are only mentioned in the NATLEX database. This column explains why Denmark is clearly an outlier with 345 reform packages; 266 of those 345 packages come from NATLEX. The Danish NATLEX records clearly indicate whether the reform was related to old age pensions, but those records did not contain sufficient information to be combined with other records into large reform packages legislated together. The fourth column shows the number of reform packages which are "Relevant but unclear". This column clearly correlates with the third column "From NATLEX only" for reasons mentioned above: descriptions in NATLEX might indicate that the reform is relevant for our purposes, but the information in NATLEX focuses on legislation instead of the economic implications.

The last two columns show the number of "Expanding only" reform packages and "Contracting" reform packages. Of the 1,828 reform packages, we managed to determine the budgetary nature of 1,010: 562 are purely expanding, 448 are contracting or mixed. Belgium and especially France legislated most pension reform packages where we could identify the budgetary nature. This is mostly due to the complexity of their pension systems, with many sector specific pension arrangements, each requiring their own reforms.

Figure 1 shows the reforms over time, split into the same three categories "Expanding only", "Contracting", and "Relevant but unclear". Unfortunately, the reforms classified as "Relevant, but unclear" dominate, especially in the first half of the sample. Starting in the late 90s, more and more reforms are properly described in the ISSA database and the LABREF database. Also shown in this figure is the number of reforms which are mentioned only in the NATLEX database. Just as in the

country dimension, the number of reforms only mentioned in NATLEX also correlates with the number of “Relevant but unclear” in the time dimension.

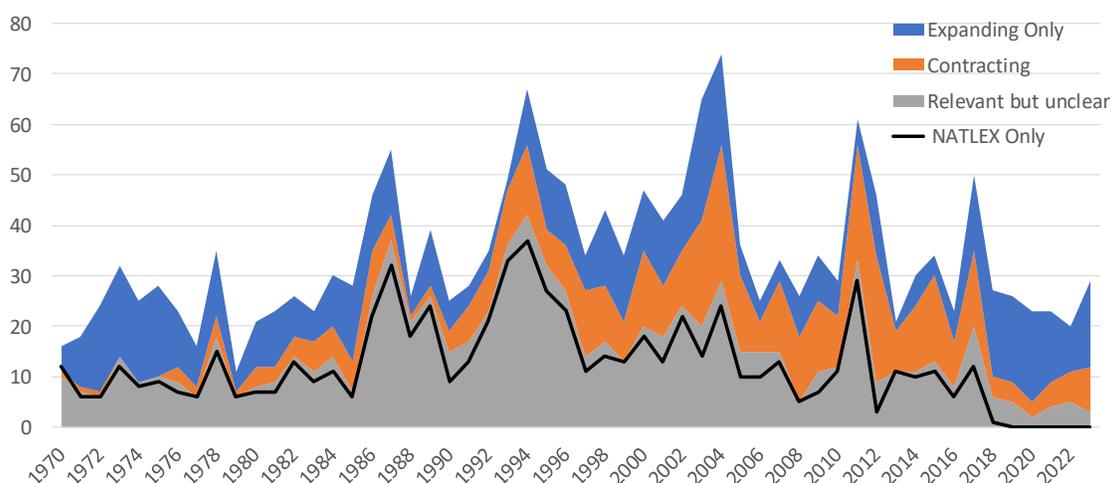
Table 1: Number of reforms per country

Country	Number of reforms	From NATLEX only	Relevant but unclear	Expanding only	Contracting
Australia	97	21	34	39	24
Austria	83	39	55	15	13
Belgium	114	36	41	43	30
Canada	67	25	33	23	11
Denmark	345	266	278	36	31
Finland	88	13	23	31	34
France	157	32	42	65	50
Germany	153	73	91	40	22
Greece	34	6	11	11	12
Iceland	11	2	3	6	2
Ireland	62	28	38	17	7
Italy	48	0	4	13	31
Japan	58	7	11	24	23
Luxembourg	21	6	8	9	4
Netherlands	47	27	28	12	7
New Zealand	42	4	5	15	22
Norway	27	8	12	11	4
Portugal	70	11	16	30	24
Spain	78	5	12	44	22
Sweden	28	4	5	8	15
Switzerland	76	21	27	18	31
United Kingdom	92	25	31	40	21
United States	30	8	10	12	8
Total	1,828	667	818	562	448

Notes: the column “From NATLEX only” shows the number of reform packages which were only mentioned in the NATLEX database. The column “Relevant but unclear” shows the number of reform packages of that country of which the nature (expanding or contracting) was unclear. “Expanding only” shows the number of reform packages which were purely of expanding nature. “Contracting” shows reform packages which are purely of contracting nature, or mixed

The number of “Expanding only” reform packages is low during the early 90s and from 2004 until 2017. The low number of expansionary reforms in the early 90s might be related to the high number of unclear reforms. Some of those reforms for which we could not determine the budgetary nature might be expanding. The low number of expansionary reform packages from 2004 to 2017 are clearly related to increasing demographic pressure; in the same years, the number of contracting reform packages was high. As Beetsma et al. (2020) document, the likelihood of a contracting reform package being legislated by a government increases during recessions. This explains the spikes in the number of contracting reform packages after the dot-com crisis in 2002 and during the financial crisis and following sovereign debt crisis from 2009 until 2017. Countries implemented emergency reforms to ensure short term and long term financial sustainability of their pension systems.

Figure 1: Number of reforms, 1970-2022, by classification



The two other characteristics we identify for each reform package are the first year that the pension system changes due to this reform and the last year that the system changes. The first year where a legislated reform has an effect is usually clear from at least one of the descriptions in the dataset, some NATLEX records even have a separate field for it. Often this is the year of legislation or the year immediately after. Usually only a structural reform packages and reform packages which increase the retirement age are implemented in a later year. Most reforms have a “once and for ever” nature; they change a parameter such as a contribution rate, change the coverage of the pension system, or increase pension benefits by a certain percentage and this change is permanent. Later changes to those parameters require new legislation, so are classified as other reforms. In those cases, the first year in which the system changes coincides with the last year. This also includes reforms which introduce an “automatic adjustment” of pension parameters to economic conditions such as linking indexation to a cost-of-living index, or linking the retirement age to life expectancy.

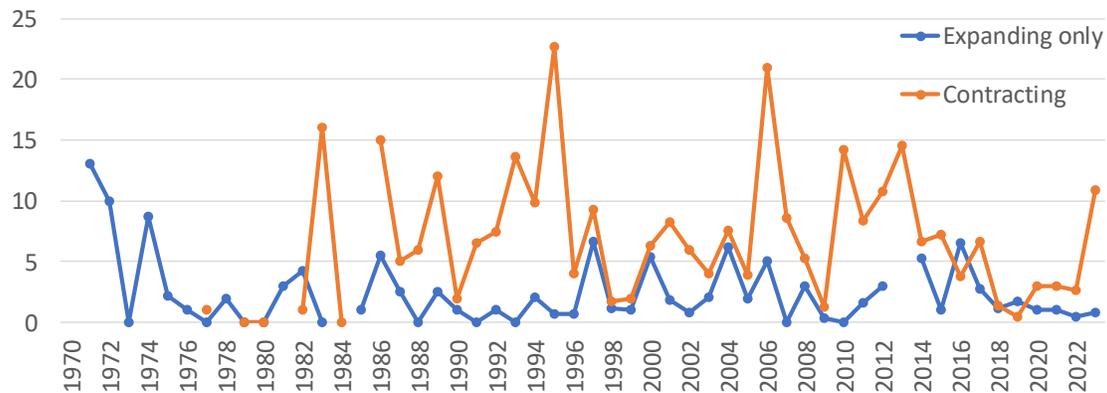
Some reforms have a (much) longer phase-in period. Especially changes to the retirement age are phased in slowly. For those reforms, the last year where the reform has an effect differs from the first year. Where the last year of change is unclear or debatable, we leave this field empty and ignore these reforms in analyses where we use this field. For our analysis, we define the phase-in period as the time between legislation and the last moment a pension parameter changes due to this reform.

As an example: the change in contributory minimum in France legislated on 21 August 2003 was implemented in January 2004 and will be reviewed in 2006 and 2008. This pension reform has 2003 as legislation year and 2004 as first and last year in which this reform changed the pension system, so a phase-in period of one year. The reviews in 2006 and 2008 have no effect on pension parameters, so are not included in the phase-in period.

Figure 2 shows the length of the phase-in periods of “Expanding only” reforms and “Contracting” reforms over time. We could not identify the nature of each reform, so not all years have reforms of each type, which causes gaps in the graph. The overall picture, however, is that contracting reforms typically have longer phase-in periods than expanding only reforms. The average time until a

contracting reform is fully implemented is seven years, which is much longer than the 2.25 years of an average expanding reform.

Figure 2: Mean phase-in periods over time and by type



Notes: the length of the phase-in period is measured in years. The phase-in period is the time between legislation and the last moment a pension parameter changes due to this reform.

Table 2: Mean phase-in periods by country and by type

Country	Mean phase-in of all reforms	Mean phase-in of “Expanding only” reforms	Mean phase-in of “Contracting” reforms
Australia	0.16	0.25	0.00
Austria	2.24	0.73	8.18
Belgium	1.97	1.20	6.88
Canada	2.35	2.75	9.00
Denmark	1.27	1.62	6.50
Finland	3.45	4.40	6.73
France	2.94	1.18	4.56
Germany	4.14	5.07	12.87
Greece	2.61	1.45	5.36
Iceland	5.00	7.25	1.00
Ireland	2.53	3.17	6.00
Italy	6.46	2.40	8.35
Japan	4.80	1.00	10.50
Luxembourg	5.00	0.20	14.33
Netherlands	1.48	1.25	4.67
New Zealand	5.63	2.50	9.75
Norway	0.33	0.00	
Portugal	1.57	0.41	3.05
Spain	5.72	1.56	11.38
Sweden	2.00	1.00	5.00
Switzerland	2.43	4.13	2.95
United Kingdom	5.30	4.91	8.65
United States	11.00		22.00
Average	2.85	2.24	7.08

Table 2 provides a similar picture: in most countries, “Contracting” reform packages have longer phase-in periods than “Expanding only”. Luxembourg and Germany have particularly long implementation phases of contracting reforms. These outliers are driven by three reforms to increase the official retirement ages and tax-deductibility of pension in those countries. In 2004 the German government legislated the Alterseinkünftegesetz which has a long phase-in period for one part of the reform. Pensions would be fully liable to income tax by 2040; a phase-in period of 35 years. Also the increase of the retirement age from 65 to 67 legislated in 2007 was announced well in advance; the retirement age would reach 67 in 2029. Luxembourg accepted a new pension law in 2012 which – among others – increased the qualification period for a full pension and contribution rates. All changes would be fully in place in 2052.

3 REGRESSION ANALYSIS

In this paper we relate the length of the phase-in period to the demographic situation, economic conditions, and the nature of the reform. The underlying hypothesis is that countries where the demographic situation or the state of the business cycle threatens the financial feasibility of the pension system legislates reforms with shorter phase-in periods. Specifically, our baseline regressions is

$$PhaseIn_{i,t}^j = \alpha_i + \beta' BASEVAR_{i,t} + \gamma^E EXP_{i,t}^j + \gamma^C CON_{i,t}^j + \varepsilon_{i,t}^j$$

Where α_i captures country fixed effects and β is a coefficient vector of appropriate dimensions to capture the country-time specific conditions. Finally, γ^E and γ^C measure the additional effect of resp. the expanding only nature of the reform and the contracting nature, compared to the reforms which have an unclear nature. $EXP_{i,t}^j$ and $CON_{i,t}^j$ are dummies indicating whether the reform is expanding only or contracting. As country specific baseline variables we use

$$BASEVAR_{it} = (\overline{OAD}_t, OADDEV_{i,t}, GROWTH_{it}, DEF_{it}, UNEMPL_{it}, CRISIS_{i,t})'$$

The old age dependency ratio enters the regression split into the cross-country average in year t \overline{OAD}_t and the country specific residual $OADDEV_{i,t} = OAD_{i,t} - \overline{OAD}_t$. Beetsma et al. (2020) used this split and found that the cross country mean of the old age dependency ratio was the main driver behind the probability of a specific reform. The country specific residual was not statistically significant in their analysis. This specification is a generalisation over including merely the old-age dependency ratio since that would impose similar coefficients β on the cross-country mean and the deviation from this mean. Further, $GROWTH_{it}$ is the growth rate of GDP per capita, DEF_{it} is the government’s budget deficit as a share of GDP, and $UNEMPL_{it}$ is the unemployment rate. Together, these three variables are intended to capture the state of the business cycle. The old age dependency rates, growth rates, unemployment rates, and public deficits are measured as rates or proportions, not percentages (so, for example, 0.02 instead of 2 percent). The final baseline variable is $CRISIS_{i,t}$ which is a dummy variable indicating whether the country suffers from a banking crisis, a currency crisis, or the Covid-19 pandemic.

One potential concern is whether the length of the phase-in period has feedback effects on the baseline variables. The effect on the old-age dependency ratio is negligible by construction since the old-age dependency ratio uses fixed thresholds (15-64 for the active population, 65+ for the elderly). The large reforms such as changing the retirement age and contribution rates are (almost) always implemented with at least one year delay and after that slowly phased in. This makes it unlikely that the legislation of the reform, and thereby the phase-in period of that reform, has an immediate effect on the state of the business cycle.

Table 3 shows the main regression results of this paper. The consistent difference between the coefficients on the “expanding only” dummy and the “contracting” dummy confirms what we already observed above: contracting reforms have a significantly longer phase-in period. The phase-in period of “Expanding only” reforms is 1.49 to 2.00 years longer than the phase-in period of “Relevant but unclear” reforms. The phase-in period of contracting reforms is more than six years longer, even if we correct for other variables. None of the other coefficients is statistically significant, although ageing has the expected sign (a higher old-age dependency ratio is expected to shorten the phase-in period).

Table 3: Baseline regressions, dependent variable: length of the phase-in period

	(1)	(2)	(3)	(4)
$EXP_{i,t}^j$	2.00***	1.49***	1.76***	
	(0.51)	(0.48)	(0.49)	
$CON_{i,t}^j$	6.27***	6.34***	6.30***	
	(0.50)	(0.50)	(0.50)	
\overline{OAD}_t	-11.9		-10.4	-6.11
	(6.35)		(5.86)	(6.93)
$OADDEV_{i,t}$	-13.6		-12.6	-20.4*
	(9.21)		(9.11)	(10.1)
$GROWTH_{it}$	-2.97			-11.2
	(8.77)			(9.56)
DEF_{it}	-0.24			4.19
	(7.61)			(8.31)
$UNEMPL_{it}$	14.9			12.5
	(7.96)			(8.45)
$CRISIS_{i,t}$	0.40			0.72
	(0.67)			(0.73)
Observations	813	843	816	813
R ²	0.25	0.24	0.24	0.095

Note: standard errors in parentheses, * $p < 0.05$, ** $p < 0.025$, *** $p < 0.01$.

Table 4 shows the relation between the phase-in period if we split the sample by reform type. The first two columns show the results for the expanding only reforms. None of the coefficients is close to being statistically significant. The last two columns show the results for the contracting reforms. Only unemployment has a statistically significant effect: a one percentage point higher rate of unemployment increases the phase-in period by six months. A potential explanation for this finding could be due to the nature of contracting reforms when unemployment rates are particularly high. The main contracting reforms in the late 90s and 2000s involve reducing early retirement regimes and later increasing normal retirement ages. Those reforms were implemented during the dot-com crisis

of 2001, the financial crisis of 2008-9 and especially the sovereign debt crisis in 2011-2013. Reforms which change the retirement age typically have long phase-in periods.

Table 4: Regressions per regime, dependent variable: length of the phase-in period

	Phase-in period of expanding only reforms		Phase-in period of contracting reforms	
	(1)	(2)	(3)	(4)
\overline{OAD}_t	-8.21 (8.94)	-9.72 (7.67)	-7.00 (22.8)	-7.63 (22.1)
$OADDEV_{i,t}$	1.22 (14.0)	-0.24 (13.6)	-46.4 (30.6)	-49.2 (30.3)
$GROWTH_{it}$	3.86 (13.3)		-5.93 (29.5)	
DEF_{it}	1.47 (12.9)		13.7 (25.9)	
$UNEMPL_{it}$	-10.8 (12.7)		59.3** (24.2)	
$CRISIS_{i,t}$	-0.43 (1.07)		0.19 (1.97)	
Observations	208	211	216	216
R ²	0.20	0.20	0.19	0.15

Note: standard errors in parentheses, * $p < 0.05$, ** $p < 0.025$, *** $p < 0.01$.

Potential outliers in our dataset are the reforms in Greece in 2010-2013 and Portugal in 2012, since those reforms were enforced by the European Central Bank, the International Monetary Fund, and the European Commission. To qualify for various bailout packages, the Greek and Portuguese governments had to implement contracting pension reforms to encourage workers to retire closer to the normal retirement age, increase the official retirement age and to lower indexation of existing pensions. Although the Greek reform of 2010 (see Kangur, Kalavrezou & Kim, 2021 for details) was part of the bailout package, it did not have a particularly short phase-in period; the measures were to be implemented between 2011 and 2018. These measures were extended and partly accelerated in the 2012 and 2013 reforms, but also the changes to normal and early retirement ages in those reforms had phase-in periods up to seven years. The Portuguese reform of 2012 was indeed an outlier. The decree of April 5 2012 immediately suspended the rules for early retirement for most people between 55 and 65, levels of pension benefits were frozen and a new special contribution on high pensions was introduced in the same year (see European Commission, 2014). Excluding this outlier does not change the results significantly.

A potential problem with the regressions presented in Tables 3 and 4 is the heterogeneity of the reforms. In Table 3, all reforms are pooled and Table 4 only makes a distinction between expanding only and contracting reforms. This still pools simple reforms which change the pension benefit and contribution rates with more structural system reforms and changes in the retirement age. Most simple reforms have no phase-in period or are effective the next year; many of those simple reforms are legislated in December and effective in January. Reforms which change the retirement age and more structural reforms, on the other hand, typically have longer phase-in periods. This last class of reforms usually receive extensive media coverage. Especially increasing the retirement age is

politically highly sensitive. Moreover, changing the retirement age is a natural response to longevity shocks, so those reforms are – a priori – more likely to be caused by demographic changes.

To solve this issue of heterogeneity, Table 5 presents the same regression as Table 3, but only for reforms which change the retirement age, including the eligibility age of early retirement schemes, and changes to minimum years of contribution. This coefficient on the “Expanding only” dummy shows that reforms which lower the retirement age have a very short phase-in period. Reforms which increase the retirement age, on the other hand, have an average phase-in period of 10 years. This is also the only statistically significant finding; all other coefficients are not significant. The difference in phase-in periods between increasing the retirement age and lowering could be explained by the implications for participants. If the retirement age decreases, then workers may still retire at the age they had in their mind before the reform. A higher retirement age might require a re-evaluation of one of the key decisions in a worker’s life since the originally planned retirement age is not feasible anymore, or at least implies a serious financial penalty. Splitting the sample in “Expanding only” and “Contracting” reform packages related to the retirement age provide a similar non-result as above. This regression table is omitted from this paper.

Table 5: Baseline regressions, dependent variable: length of the phase-in period

	(1)	(2)	(3)	(4)
$EXP_{i,t}^j$	1.41 (3.86)	1.35 (3.66)	1.13 (3.71)	
$CON_{i,t}^j$	10.7*** (3.72)	10.5*** (3.52)	10.5*** (3.57)	
\overline{OAD}_t	-35.5 (25.2)		-22.1 (22.9)	-12.9 (28.1)
$OADDEV_{i,t}$	-30.8 (46.0)		-37.3 (44.3)	-21.4 (51.7)
$GROWTH_{it}$	6.52 (38.0)			-33.9 (41.3)
DEF_{it}	-24.8 (35.2)			10.4 (38.3)
$UNEMPL_{it}$	43.9 (27.5)			47.8 (31.1)
$CRISIS_{i,t}$	2.08 (2.56)			-0.29 (2.85)
Observations	118	120	118	118
R ²	0.59	0.56	0.57	0.46

Note: standard errors in parentheses, * $p < 0.05$, ** $p < 0.025$, *** $p < 0.01$.

4 DISCUSSION AND CONCLUSION

In this paper, we presented a novel dataset with pension reforms and relevant characteristics of those reforms such as the sign of the pension changes from the participants perspective and the length of the phase-in period. The empirical analysis in this paper finds no relation between ageing and the length of the phase-in period of reforms. There is anecdotal evidence which suggests that during crises painful reforms are legislated under high pressure and have shorter phase-in periods, but those crises are not directly related to ageing,

The main deliverable of this paper is this new database; the full dataset is available online. A few examples of projects that could make use of this dataset are projects that study the trust (or lack of trust) in the pension system before, during, and after major pension reforms. Also projects which study labour force participation rates will benefit since pension reforms are a source of exogenous shocks (as in Ramey, 2011). Those studies could pick only those reforms which were truly unexpected, triggered for example by a financial crisis, and those reforms with a very short phase-in period. Finally, the indicators on the phase-in period are relevant for those interested in regulatory aspects of pension reforms.

Our results are in line with those in Beetsma et al. (2020): business cycle indicators are important for the timing a pension reform measures: a worsening makes contractionary measures more likely and expansionary measures less likely. The demography matters only in the sense that the OECD-wide demography explains the general reform trend for a country. These results are rationalised in Romp & Beetsma (2023). They provide an economic rationale for the observed correlation between business cycle and the timing of pension reforms using a political economy model with two-sided adjustment costs. The crux is that government realise that the ageing population requires a politically costly pension reform, but those ultimately necessary reforms can be postponed until change is optimal due to recessions and booms.

Their explanation of fixed adjustment costs could be one explanation for the observed asymmetry between phase-in period of expanding reforms and contracting reforms. Expanding reforms typically have smaller political costs; the main costs are due to implementation delays. Contracting reforms, on the other hand, carry high political costs. The (sometimes very) long phase-in periods might reduce those political costs and make those reforms politically feasible.

A closer look at this new dataset with individual reforms shows that this narrative has one major shortcoming. Once legislated, many reforms with long phase-in periods are accelerated by new legislation. Examples of reforms which were accelerated later are the plans to increase the official retirement ages in Italy, France, Portugal, and the Netherlands. From a pure economic perspective, this is a puzzle, since hardly any new information arrived between the original reform and the new reform to shorten the phase-in period.

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