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POLICY UNCERTAINTY AND INFORMATION FLOWS: EVIDENCE FROM PENSION REFORM EXPECTATIONS*

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We examine how workers' expectations about pension reform vary with proximity to reforms, information availability and worker characteristics. Using newly collected data, we find that (1) expectations about reform are revised upward by about 10 percentage points in the year prior to a reform, from a median of 50%, regardless of whether the reform is announced; (2) expectations increase more the stronger the media activity about imminent reforms; (3) the effect of information on expectations varies systematically with characteristics that proxy cognitive ability and information value; (4) expectations do not converge as a result of reform announcements or implementations.

Recent and ongoing events—such as the COVID-19 pandemic, the global financial crisis, climate change, trade wars and Brexit—epitomise public concerns about policy uncertainty worldwide. Policy uncertainty plays a significant role in individual's welfare, especially when it involves measures that affect important life-cycle decisions and that are not easy to insure against, such as future taxes, monetary policy and public pensions. To mitigate such uncertainty, individuals can exploit a plethora of information about when and how new reforms might be enacted by governments. Since the early 1990s, economists have increasingly collected data on subjective expectations from survey respondents (Manski, 2004; Hurd, 2009; Delavande, 2014). Across several events and settings, an empirical regularity is the presence of substantial heterogeneity in beliefs. We know little, however, about how individuals use available information in practice to elaborate their policy expectations (Manski, 2004; 2018). In this paper, we present novel micro-empirical evidence on expectations about pension reforms from older workers in Europe and analyse how they vary with naturally occurring information over a long period of time.

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Pension reforms are particularly relevant, as continuing population ageing, the legacy of the 2008 credit crunch and the resulting uncertain sustainability of social security programs have left individuals across developed countries uncertain about what will be available to them and when (OECD, 2017). Despite this uncertainty, individuals must form expectations about both future generosity and timing of pension income to prepare successfully for retirement. Importantly, pension reform expectations at the individual level are systematically associated with economic behaviour, such as wealth accumulation and insurance holding (e.g., Bottazzi *et al.*, 2006; Guiso *et al.*, 2013), suggesting that the expectations we focus on have considerable economic relevance to older workers.

We examine how pension policy expectations vary with proximity to reforms, information availability and worker characteristics. For this, we construct a new pan-European dataset of pension reform implementations and announcements covering ten European countries either side of the financial crisis. We combine this information with representative individual-level data on probabilistic expectations about future reform events from workers aged 50 and over in the Survey on Health, Ageing and Retirement in Europe (SHARE) and country-level data on media activity, including internet searches through *Google Trends* and information available in news print from the Nexis database. The latter two provide an aggregate measure of the informational intensity with which pension reforms are discussed in public media and social milieux. The presence of reform announcements creates plausibly exogenous variation in the cost of acquiring information.

We analyse expectations about whether governments will increase the national retirement age or decrease pension benefits and find evidence of considerable pension reform uncertainty. Over the sample period, spanning 2004 to 2013, fewer than one-third of respondents express no uncertainty about the event of a reform before they retire. Conversely, almost two in five workers in the sample reveal substantial uncertainty by reporting a probability between 30% and 70%. Average beliefs that either type of reform will occur rose sharply over the sample period, from about 45% in 2004 to nearly 60% in 2013, possibly reflecting concerns over public finances related to the 2008 banking collapse.[1] This pattern is in line with the results found in other studies measuring probabilistic expectations about pension uncertainty (e.g., Delavande and Rohwedder 2011; Guiso *et al.*, 2013; Bissonette and van Soest, 2015).

One of our main contributions is to investigate how reform expectations vary around reform enactments. Our first main result is that individuals' expectations adjust upward in the year leading up to a reform, consistent with information gathering and processing that shape policy expectations in the period leading up to a reform. In particular, individuals interviewed in the 12 months prior to pension benefit cuts report expectations of a reform that are nearly 9 percentage points higher compared to workers who do not experience such a policy change in the near future, from a mean (and median) close to 50%. The increase in expectations about retirement-age reforms is of a similar magnitude. Our results are robust to conditioning on macroeconomic variables, fixed-effect models and to accounting for rounding of expectations as in Manski and Molinari (2010).

The data we collect allow us to distinguish imminent reforms that are announced from those that are, at the time of interview, *unannounced*. Our second main result is that reform expectations are revised similarly in the period leading up to a reform with and without announcement. Reform announcements can be considered as information shocks that reduce the cost of acquiring

[1] Throughout the paper, we use the terms 'expectations' and 'beliefs' interchangeably.

information, as they are typically followed by ample media coverage. Our second result hence suggests under-reaction to information shock possibly due to inattention and/or cognitive limitations as, despite official government announcements of a reform, individuals revise their beliefs upward by only 11 percentage points on average. Our second result also suggests that, even with no formal announcement, individuals can acquire and process information that is as meaningful as a formal government policy proclamation (and the associated information acquisition that may follow it).

Our third main result is that expectations about reform respond strongly to information available in the media, either in newsprint or through online search. Focussing on online search as a measure of wider media discussions, we investigate how such information and reform announcements interact in the evolution of beliefs.[2] We find that these sources are substitutes: online information generates belief revisions more in periods when reforms have not yet been announced. This finding implies that, in this context, individuals obtain substantial information through informal channels, and also explains the similar updating seen whether or not announcements are made that we document in our second result.

Our fourth main result is that the effect of information on expectations varies systematically with characteristics that proxy for information value and cognitive ability. We analyse how expectation formation differ by observable characteristics. Workers with a university degree increase their expectations of a reform more than their less educated counterparts following an announcement. This is consistent with more educated workers being better at processing information that is easily accessible or cheaper. In contrast, older workers increase their expectations in the 12 months prior to a reform more than younger workers, but here the effect is driven by *unannounced* reforms. Compared to their younger counterparts, older workers may acquire more information, even when the costs are higher—or pay more attention to ambient information related to pension reform—because this information is likely to be more valuable to them; older workers are closer to retirement and so, when a reform occurs, they have less time to adjust their behaviour. This is important because it is unclear whether the heterogeneity in expectations we observe in many contexts is driven by heterogeneity in information acquisition or heterogeneity in information processing. Our fourth result suggests that both are likely to play a role.

Consistent with this heterogeneity, the period leading up to a reform does not typically result in belief convergence. In fact, when retirement-age reforms are announced, the distribution of expectations *diverges*. Making information cheaper through a formal announcement does not reduce the cross-sectional dispersion of expectations.

Related literature. Our main contribution is to provide evidence on expectation formation in response to naturally occurring information. Understanding exactly how people search for and use information to revise their expectations is critical to many areas of social sciences. Yet, we have only limited knowledge of how individuals process information about real-life events to formulate subjective expectations relevant to their decision making (e.g., Manski, 2004; 2018; Bernanke, 2007). The nature of the expectations considered is relevant to the analysis of the revision process. Conceptually, subjective expectations can be classified according to the degree of control over the relevant event. At one extreme, there are expectations over uncontrolled events, such as inflation, stock market returns and the pension reforms studied here. At the other extreme, there are expectations over future choices for which individuals have full control, such as whether

[2] Over the last 20 years, digital news and, more generally, online information searches have become one of the most prominent sources of information acquisition around the world (e.g., Kennedy and Prat, 2018; see also Newman *et al.*, 2019).

or not to purchase an item. Our work focuses on pension reform, an event over which individuals have arguably no direct control. For these types of event, heterogeneity in expectations must be explained by differential attention to public information, and differences in processing public information across individuals.

Psychologists and experimental economists have long analysed how subjects update probabilities in highly stylised situations where the information signals come from simple data generation processes, and have identified systematic departures from Bayes' theorem (e.g., Tversky and Kahneman, 1974; Grether, 1980; El-Gamal and Grether, 1995). It is, however, not entirely clear how these findings apply to expectations formation in real-life situations with a much more complex data generating process of information.

One approach has been to rely on randomised information provision, often within surveys that elicit priors and posteriors about economically salient outcomes. Recent evidence indicates that individuals revise their expectations in the direction provided by the information they receive, resulting generally in convergence in beliefs, even if there is heterogeneity in the revision process that is not always consistent with Bayesian updating (Delavande, 2008; Cavallo *et al.*, 2014; Armantier *et al.*, 2015; 2016; Wiswall and Zafar, 2015; Ben-David *et al.*, 2018; Armona *et al.*, 2019; Roth and Wohlfart, 2020). Some of this work explicitly studies a more realistic information acquisition environment (e.g., Fuster *et al.*, 2022; Roth *et al.*, 2022) and shows that demand for information depends on its expected benefits, and that there is heterogeneity in the type of information individuals prefer. While this approach has the advantage of allowing researchers to precisely account for the information used by individuals to revise their expectations, it does not examine how individuals respond to information that would have not been made artificially available in a survey setting. It also typically focuses on short-term revisions. An important contribution of our analysis is that we assess the evolution of beliefs in conjunction with various information flows arising in a real-world context, over a long period of time.

Another approach is to analyse revisions of expectations collected in panel data surveys and, more generally, to exploit belief variation over time (Bernheim, 1990; Dominitz, 1998; Hurd and McGarry, 2002; Bottazzi *et al.*, 2006). In all such cases, expectation revisions are responsive to new information. Analysing stock market returns expectations, Dominitz and Manski (2011) concluded that, while there is extensive heterogeneity in revision processes, individuals use interpersonally variable, but intrapersonally stable, processes to form their expectations.[3] In the context of macroeconomic expectations, Coibion and Gorodnichenko (2012; 2015) investigated how expectations from experts and other agents respond to macroeconomic shocks and economic conditions, and found evidence of information rigidities. In line with these papers, we exploit variation in expectations over time. We offer a richer picture by additionally exploiting variation in information cost generated by government announcements, media information and characteristics that proxy for information value and cognitive ability.[4]

[3] Hurd *et al.* (2011) also looked at stock markets. Their results suggest that individuals focus on recent stock market performance when assessing future stock market returns. See also Heiss *et al.* (2019). In a related paper, Kezdi and Willis (2011) provided evidence indicating that heterogeneity in stock market expectations comes from different learning histories, with those who can gain more from learning, more likely to gather information. In the context of inflation expectations, Carroll (2003) showed that a model in which the typical household's expectations are updated probabilistically toward the views of professional forecasters captures well the representations of expectations elicited in surveys, and found that household inflation forecasts are better when there is more news coverage. Using inflation forecast error, D'Acunto *et al.* (2022) concluded that high-IQ men overreact to macroeconomic news, while the evidence for low-IQ men is noisy.

[4] Another distinct approach, undertaken mostly by psychologists, is to ask respondents to think out loud about the factors they take into consideration when reporting their expectations. Chin and de Bruin (2017) found that individuals

Finally, we contribute to existing research on policy uncertainty. An important question here is how policy uncertainty should be measured. One strand of the literature estimates variability in past policy changes (McHale, 2001; Nataraj and Shoven, 2003), whereas another relies, as we do, on survey questions about future policy (Giavazzi and McMahon, 2012; Guiso *et al.*, 2013).[5] A different focus is to relate the estimated policy uncertainty to behaviour or welfare (e.g., Morris and Shin, 2002; Luttmer and Samwick, 2018). There has been little analysis, however, on the formation of individuals' policy uncertainty and how it varies with government announcements and other information. This is our primary contribution to this specific strand of the literature.

We organise the rest of the paper as follows. Section 1 discusses the individual-level data from SHARE and the sources used to construct our indicators of reforms, reform announcements and online search. We then illustrate the estimation procedure and identification issues in Section 2. Section 3 presents our key results, while Section 4 discusses belief convergence and documents the extent of heterogeneity in expectations revisions.

1. Data

1.1. *The Survey on Health, Ageing and Retirement in Europe*

Our analysis uses panel data from the Survey on Health, Ageing and Retirement in Europe (SHARE). In 2004/05, SHARE collected data on a representative sample of individuals aged 50 and over and their co-resident partners across 12 European countries. Since then nine additional countries have joined the survey, and the next five sweeps of data have been collected at approximately two-year intervals (2006/07, 2008/09, 2011, 2013, 2015). Of these, wave 3 (2008/09) contains mostly retrospective data on sample members' lives, and is thus not used. Similarly, wave 6 contains few data on pension beliefs, because the relevant questions were asked only to new respondents, although we use it in several contexts for auxiliary analysis.

Of the countries that took part in the first wave, we exclude two, Greece and Israel, which did not participate in all of the subsequent rounds. We then use data on the remaining countries, i.e., Austria, Belgium, Denmark, France, Germany, Italy, Netherlands, Spain, Sweden and Switzerland, over the relevant survey waves.[6]

In estimation, we use different samples depending on the focus of the analysis. Our main work is based on employed and self-employed men and women observed aged less than 65 in waves 1, 2, 4 and 5. This leads to a sample size of 20,366 individuals, for a total of 27,881 person-wave observations.[7] Few individuals (slightly more than 3%) are observed for three or four waves, while 30% are observed twice.[8]

Table 1 reports the summary statistics of the key variables. The average age of 56 years is fairly stable across waves, because SHARE is usually refreshed from one wave to the next with younger

vary in the number and type of issues they consider when forming stock market expectations. For example, about three-quarters of consumers interviewed report considering 'the state of the economy', while less than a third considered 'interest rates on loans and mortgages'.

[5] Baker *et al.* (2016) analysed the text content of media explicitly to measure policy uncertainty.

[6] In wave 4 (2011) the questions of interest were asked only of new respondents (the 'refreshment' sample). Two countries, Germany and Sweden, did not have a 'refreshment' sample in this wave and so only provide three waves of expectations data.

[7] To obtain this final sample, we drop observations with missing values in the variables that are used in the main analysis. Moreover, as our main indicators for pension reform vary by country and month of interview, we consider only country×month cells that contain at least 30 observations. For a breakdown of the sample by country and wave, see Table A.3 in Online Appendix A.

[8] This implies that the fixed effects regressions used in later parts of the paper are close to first differences.

Table 1. *Summary Statistics.*

Variable	Mean	SD	Median	Min	Max	Obs.
Age (years)	55.62	3.75	55.00	50.00	64.00	27,881
Female	0.49			0	1	27,881
Employment status (base: private sector employee)						
Civil servant	0.21			0	1	27,881
Self-employed	0.15			0	1	27,881
Employed part time	0.20			0	1	27,881
Education (base: primary)						
Lower secondary	0.15			0	1	27,881
Upper secondary	0.36			0	1	27,881
University and more	0.39			0	1	27,881
Citizen	0.97			0	1	27,881
General health (base: excellent)						
Very good	0.30			0	1	27,881
Good	0.39			0	1	27,881
Fair/poor	0.14			0	1	27,881
Marital status (base: married/cohabiting)						
Divorced/separated	0.12			0	1	27,881
Never married	0.07			0	1	27,881
Widowed	0.03			0	1	27,881
Household size	2.43	0.92	2.00	1	4	27,881
Log household income (000s, 2005 euros PPP)	3.57	0.97	3.72	0.00	7.98	27,881
Log net wealth (000s, 2005 euros PPP)	5.01	1.63	5.37	0.00	10.21	27,881
B_{NRA}	0.51	0.36	0.50	0.00	1.00	27,881
B_{PB}	0.53	0.34	0.50	0.00	1.00	27,881
NRA: $I^A(-12, -1)$	0.05			0	1	27,881
NRA: $I^U(-12, -1)$	0.19			0	1	27,881
NRA: $I(0, 12)$	0.13			0	1	27,881
PB: $I^A(-12, -1)$	0.05			0	1	27,881
PB: $I^U(-12, -1)$	0.15			0	1	27,881
PB: $I(0, 12)$	0.06			0	1	27,881
$G(6)$	0.08	0.13	0.03	0.00	0.68	24,394
$News(6)$	0.09	0.08	0.06	0.02	0.59	23,219
Numeracy	0.00	0.95	0.18	−2.82	1.18	26,791

Notes: Income and wealth are expressed in 2005 euros and are converted using PPP exchange rates for all countries (reference: Germany). For both variables, we use imputed values (averaged across the multiple imputation values provided by SHARE) and censor the logs at 0 (including negative wealth and income values). This censoring affects 3.98% of the income observations and 2.19% of wealth observations. Household size is censored above at 4, affecting 4.39% of the sample. Here B_{NRA} is the belief about the chances (on a 0–1 scale) that the government raises the national retirement age before the respondent retires and B_{PB} is the belief about the chances it reduces pension benefits before the respondent retires; $I^A(-12, -1)$ is an indicator for the individual being within 12 months before an announced reform, $I^U(-12, -1)$ is the same before an unannounced reform and $I(0, 12)$ is an indicator for the individual being within 12 months after a reform; $G(6)$ is the *Google Trends* index of search intensity about the keyword 'pension reform', averaged over the semester ending in the month of the interview; $News(6)$ is the *Nexis* index of monthly count of news articles about 'pension reforms', rescaled by the maximum monthly count by country (over 2003–13) and averaged over the semester ending in the month of the interview. 'Numeracy' is demeaned and based on the score to a series of questions about numerical cognitive skills averaged across all available waves in the selected sample (see Online Appendix Section A6 for more details).

members. The sample is equally split between men and women, and about 97% are citizens of the country where they are interviewed. Two-thirds of workers are in a full-time private sector job and about 15% are self-employed. Nearly 40% of the sample has at least post-secondary education qualifications, although there is sizeable cross-country variation in schooling, with

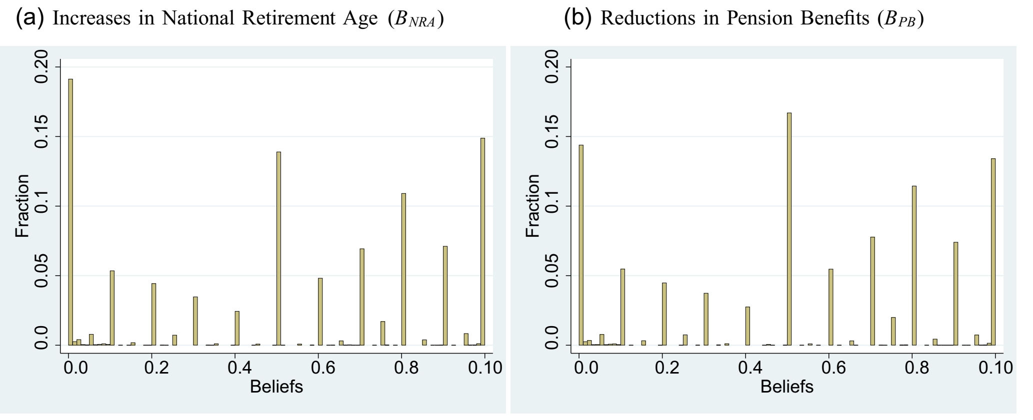

Fig. 1. *Distribution of Beliefs about Pension Reforms.*
Source: SHARE, waves 1, 2, 4 and 5, covering the years 2004–15. The number of person-wave observations used in each figure is 27,881.

Denmark having the largest fraction of workers in the top education group (52%) and Italy the lowest (22%). Four in five respondents are married or in a cohabitation, and again four in five report to be in good or very good health conditions.

1.2. *Pension Reform Expectations*

We use data on individual-specific pension reform beliefs from two questions that SHARE asks to all employed and self-employed individuals.[9]

(i) B_{NRA}: 'What are the chances that before you retire the government will raise your retirement age?'
(ii) B_{PB}: 'What are the chances that before you retire the government will reduce the pension which you are entitled to?'

Table 1 shows that individuals on average assign a 51% chance to the event that the government will raise their retirement age and a 53% chance to the event that the government will reduce their pension benefits. The SD of both measures is high. This can also be seen from Figure 1, which displays the distribution of the two belief measures for all individuals over the whole sample period. The dispersion remains large even after we account for a wide set of individual demographic and socioeconomic characteristics (see Online Appendix Figure A.1). As is common with this elicitation format, responses tend to be rounded to the nearest 5% or 10% (Manski and Molinari, 2010; Giustinelli *et al.*, 2019).

Irrespective of the type of reform, Figure 1 documents that fewer than one-third of respondents express no uncertainty at all by reporting either a 0% chance (certainty there will be no

[9] The same two reform domains are used in the counterfactual experiments by van der Klaauw and Wolpin (2008) and Haan and Prowse (2014). Responses are recorded on a [0–100] scale, but we re-scale them between 0 and 1 (included). Online Appendix Section A1 discusses a number of data collection issues related to such questions. Here we just note that in wave 4 SHARE does not ask them to longitudinal respondents, but only to individuals who are in the refresher samples.

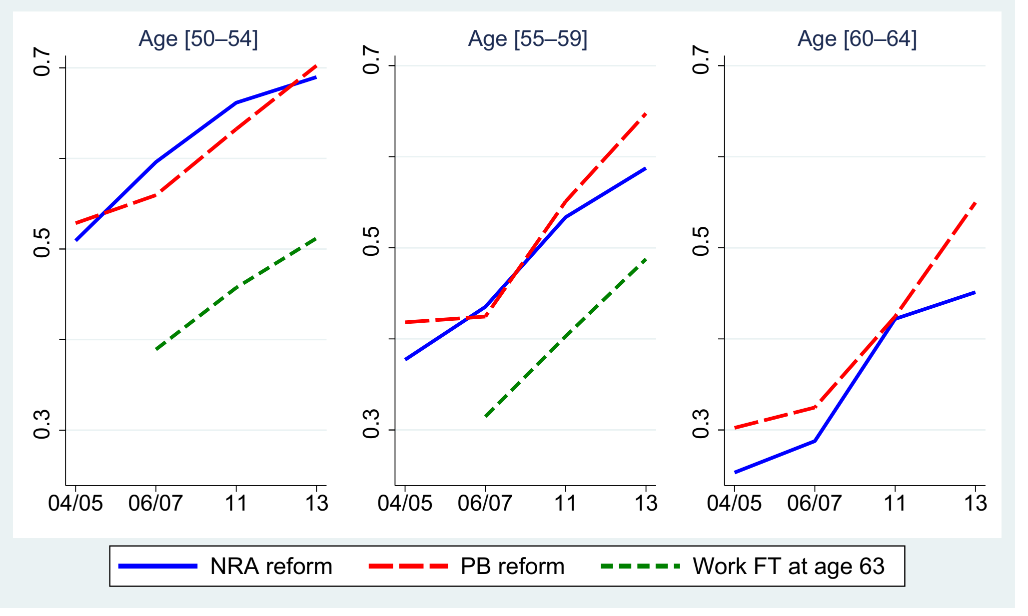

Fig. 2. *Beliefs about Pension Reforms, by Time and Age.*
Notes: Each observation is weighted by total employment by cell defined on country, gender and age group (50–54, 55–59, 60–64) divided by sample size in the same cell. (*Source for total employment*: Eurostat, lfsa_egan table, available at http://appsso.eurostat.ec.europa.eu/nui/show.do?dataset=lfsa_egan&lang=en.) We include only observations used in the main sample (see Tables 1 and 2), apart from Germany and Sweden that we exclude because they do not have observations in the 2011 wave. The panels (except the panel on the right) also show the average expectation that workers will still be working full time by age 63.

reform) or 100% chance (certainty there will be a reform) of a policy change before their retirement. About one in seven workers report 50% chance of either reform, which constitutes the maximum uncertainty level. Almost 40% of the individuals in the sample reveal substantial uncertainty by reporting a probability between 30% and 70%. Although not shown here, cross-country differences are large: fewer than one in six German workers in 2006 fall in the 30%–70% probability band, as opposed to nearly half of Spanish and Swedish workers in 2007.

Figure 2 indicates that average beliefs are consistent with changes in the macroeconomic environment. After the 2008 financial crisis, expectations that a reform will take place increase sharply on average. Beliefs also vary by age: reported chances of a reform are lower for older people, presumably because the remaining time they face before retirement is shorter, and there is consequently a smaller chance that a reform affecting them will be introduced. For individuals aged 50–54, mean expectations move up from around 50% to 70%, suggesting that they are overall becoming more certain that a reform will take place. But, for individuals aged 60–64, mean expectations also move up from approximately 30% to 50%, implying a general rise in uncertainty. Uncertainty remains large for individuals aged 55–59, with average beliefs ramping up from about 40% to 60%.

Figure 2 also shows that beliefs about pension reforms are strongly positively correlated with individual's expectations of working by age 63. These work expectations have also increased over the sample period, in part reflecting changes in the macroeconomy. Although we do not focus on the relationship between reform beliefs and behaviour in this paper, recent empirical studies provide substantial evidence of effects across several domains (see, e.g., Bottazzi *et al.*, 2006; Guiso *et al.*, 2013; Delavande and Kohler, 2016; Armona *et al.*, 2019; Delavande and Zafar, 2019; Debets *et al.*, 2022). Indeed, Online Appendix Table A.5 shows that higher pension reform expectations at wave t are associated with a higher propensity to work full-time at wave $t + 1$. These results suggest that the beliefs we focus on have considerable economic relevance to older workers.

1.3. *Data on Reforms and Reform Announcements*

We compile a comprehensive pan-European inventory of pension reforms, covering 1998 to 2015. This allows us to establish the exact date (year and month) of each reform for each country over the relevant time period. We use a wide range of sources, including European Union reports, OECD publications, articles and books (e.g., Immergut *et al.*, 2007). To fully reflect the expectations data described earlier, we distinguish between reforms that change retirement age and reforms to pension benefits. Overall, and across the ten countries in the analysis, we end up with a total 46 main reforms over the sample period. The detailed inventory is available in Online Appendix B, together with a comprehensive list of references and sources. As discussed in the appendix, all reforms are substantial in scope. Besides the exact timing of enactment—and, as discussed below, announcement—of each type of reform, we chose to neglect other details, such as the specific group of workers affected (in terms of year of birth, gender, sector of employment, seniority and contribution), since these details are hard to identify precisely. Including such details with inaccuracy or ignoring them as we do here may equally lead to measurement error, and in either case, produce estimates that are likely to be attenuated.[10]

Figure 3 shows the timing of the reforms by type and country.[11] Most countries introduced at least one reform of either type during the sample period. Many countries (such as Austria, France, Italy and Spain) experienced multiple reforms over time, sometimes in close succession. Others (such as Germany and Switzerland) instead passed fewer pension reforms, while Sweden introduced none, presumably because of its system of continuous adjustment. We examine the influence of individual countries on our results in robustness analyses reported in the Online Appendix (see in particular Tables A.1 and A.7).

We further assess whether each reform we consider was announced by the relevant government prior to its enactment. For this purpose, using all the available country-specific published records of government activities, we define as the announcement date the earliest month in which one

[10] From this broad inventory, the small number of reforms that lowered national retirement age or increased pension benefits are excluded to match the survey questions on expectations. Online Appendix Table A.2, panel E shows that results are broadly unchanged when we instead include these reforms and control for them with additional binary indicators. Finally, we also exclude one reform in France that enacted a minor modification of some special pension schemes at the end of 2008. In fact, including this French reform in the analysis has no effect on our results because it falls a long way from observations in SHARE.

[11] The empty circles refer to country-specific subsamples obtained after we drop individuals in year-month cells in which there are fewer than 30 observations, as discussed earlier in this section. In a number of robustness checks, we performed our analysis without making this selection and found results that are in line with the baseline estimates. See Section 3.

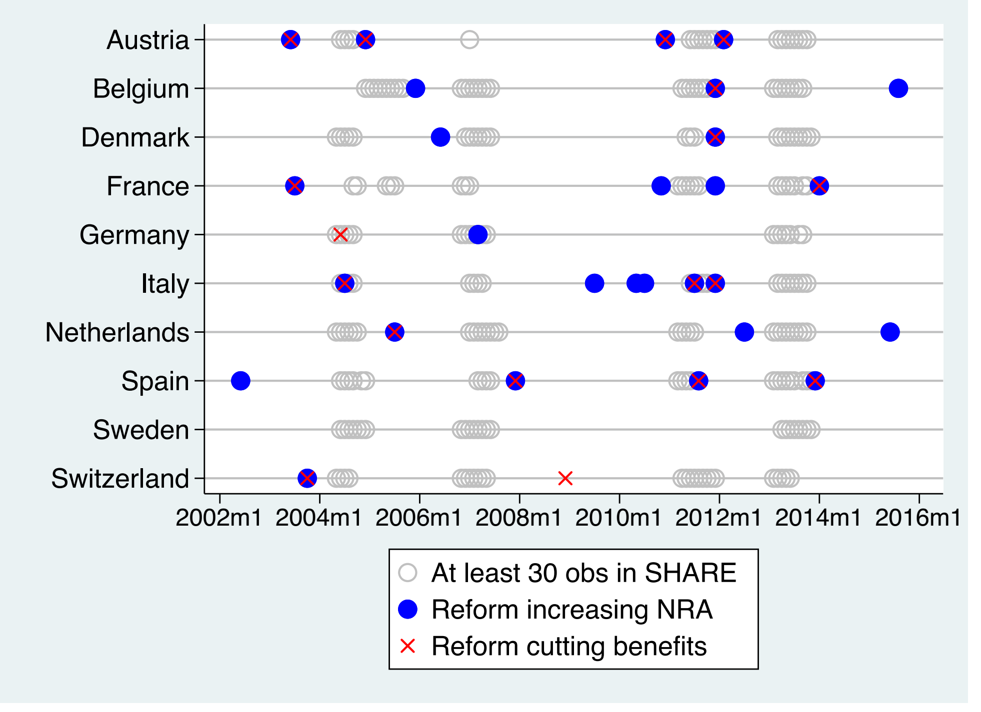

Fig. 3. *Reforms' Timing, by Country and Reform Type.*

of the following three events occurred: (*i*) the government presented a draft or made a formal proclamation (e.g., Denmark in May 2011); (*ii*) a bill was submitted to Parliament (e.g., Spain in October 2013); (*iii*) there was an agreement between different parties and/or trade unions (e.g., Germany in July 2006). When such an announcement has not yet been made, the reform is treated as unannounced. To emphasise, being 'unannounced' in our analysis does not imply that an announcement was never made. In a given country and wave of data, interviews are carried out in different months, some of which happen to fall before the announcement and others after.[12] In fact, we have few truly unannounced reforms in our panel. Yet, these are not necessarily marginal. For instance, the 2005 reform analysed by De Grip *et al.* (2012) led to a major change in the Dutch pension system and was not announced. Neither was the Monti-Fornero reform in Italy in December 2011, which was implemented by the newly formed technical government as an emergency measure to counteract the public debt crisis.

Building on Figure 3, Figure 4 plots the time profile of announcements for each recorded reform. The figure highlights considerable variation across countries and time periods. As detailed in Section 2, we use this variation in a difference-in-difference set-up to identify the impact of announced versus unannounced pension reforms on beliefs.

[12] In some cases we have observations before the announcement, but not between it and the actual implementation of the reform. An example is the Austrian 2004 reform, which was submitted (and hence 'announced') to Parliament in October and approved in December; in this case, our sample contains only observations up to September of that year and then only from 2006 onwards.

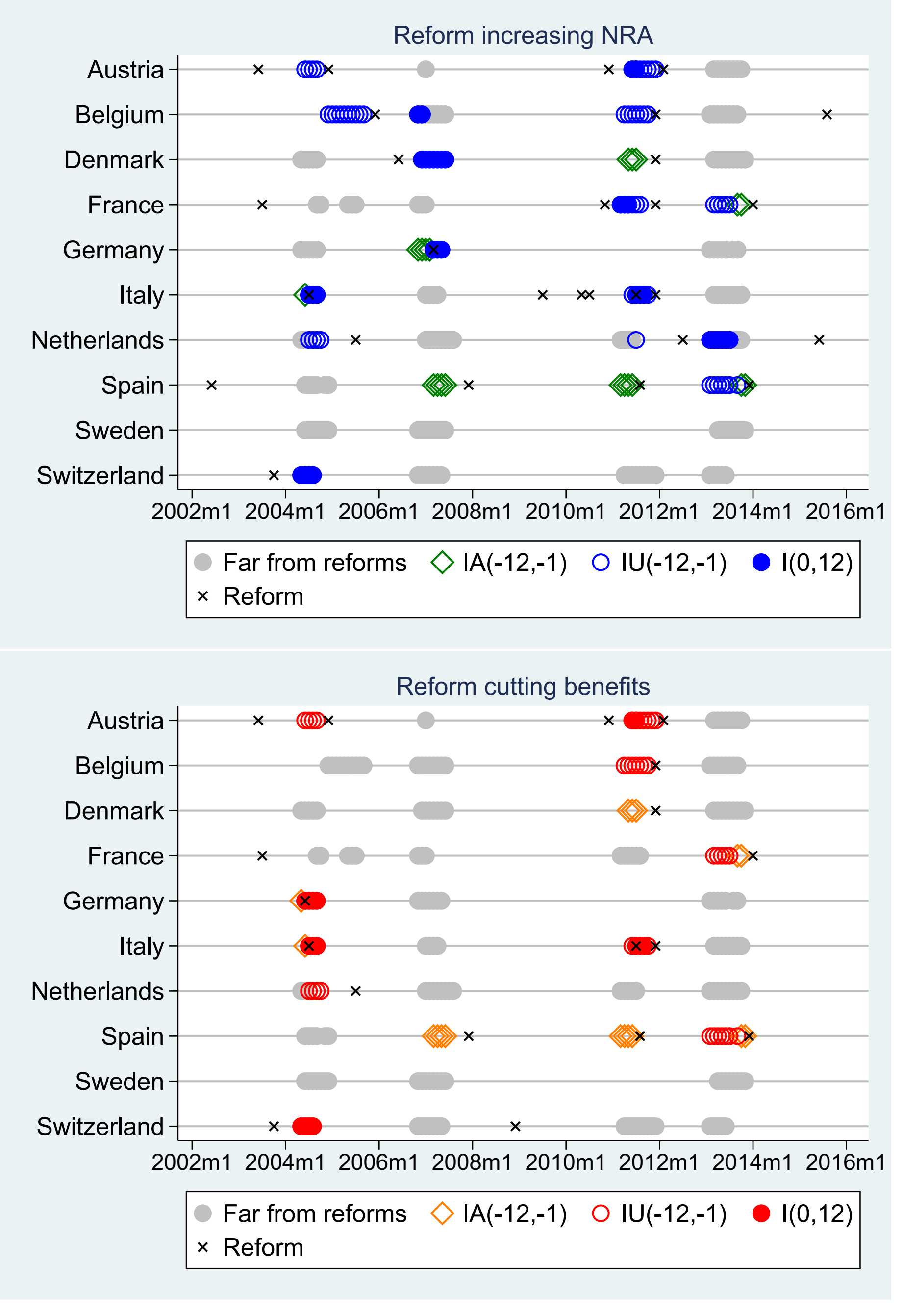

Fig. 4. *Reforms' and Announcements' Timing, by Country and Reform Type.*

Notes: Here $I^A(-12, -1)$ is equal to 1 if the month of interview is between 12 months and one month before a reform and the reform had already been announced, and 0 otherwise; $I^U(-12, -1)$ is equal to 1 if the month of interview is between 12 months and one month before a reform and the reform had not been announced yet, and 0 otherwise; $I(0, 12)$ is equal to 1 over the 12 months following a reform (including the month of the reform), and 0 otherwise.

1.4. *Information Available from the Media*

1.4.1. *Online search data from Google Trends*

Our final measures of information availability are based on data from media sources. We start with a *Google Trends* index of online search. This provides us with an ideal tool to observe information-seeking activities in our context. Not only does it offer readily comparable data across countries and over time, but it also systematically captures the intensity of public opinion, which is presumed to be salient to beliefs and behaviour (Jun *et al.*, 2018).[13]

We focus on monthly searches of the words 'pension reform', translated in each country's language(s) (see Online Appendix A4), from January 1, 2004 (the start of *Google Trends*) to December 31, 2013 (the end of the sample period). The raw data give the number of web searches including our specific keyword in a given country over a predefined period of time (one month in our case), divided by the total number of searches for the same country and month. Let this search share for month m and country j be labelled S_{jm}. The raw search count data are not publicly available for privacy reasons. Google scales the index to 100 in the month in which it reaches its maximum level, while the index in the other months is expressed as a proportion of the maximum, so that higher values represent higher fractions of total searches. The index is 0 if there is no search or the search is too little to obtain reliable data. Thus, the *Google Trends* index for month m and country j is given by $100\, S_{jm}/\max_{k\in K}(S_{jk})$, where K is the time span in months over which online searches are considered. To make the index comparable to the expectation measures, we rescale it between 0 and 1, taking value 1 in the month featuring the most country-specific searches, and 0 if there is no search.

Our main variable of interest for the supply of information is the cumulative past search intensity that captures accumulated knowledge and discussions in social milieu. For the sample of workers in country j interviewed at time t (year and month), we define $G(6)_{jt}$ as the mean of the index computed over the six months preceding the interview date. Although not our main focus, we also look in some part of the analysis at measures of information demand and rely on monthly search to measure current search. In particular, we define $G(0)_{jt}$ as the *Google Trends* index for the month in which the interview takes place. The unconditional distributions of the contemporaneous measure, $G(0)$, and the six-month historic mean index, $G(6)$, are displayed in Online Appendix Figure A.2. Both measures are skewed toward zero as online search tends to be concentrated in specific times and is otherwise relatively limited.[14]

1.4.2. *Newsprint data from LexisNexis*

To complement the data from *Google Trends*, our final measure of information supply is that captured in newsprint, obtained from the LexisNexis ('Nexis') database. Nexis contains a variety of media content across many platforms starting from around 1970.

To use these data, we select specific and suitable newspapers within each country, focussing as far as possible on 'newspapers of record'. We then aggregate up to the country level, before

[13] A wealth of existing research, in fact, documents that online search—among other things—affects purchases and innovation adoption, and has a strong predictive power of influenza diffusion, private consumption, stock prices, trading volumes in financial markets, job search intensity and unemployment (Shim *et al.*, 2001; Ginsberg *et al.*, 2009; Kotler and Keller, 2009; Vosen and Schmidt, 2011; Choi and Varian, 2012; Preis *et al.* 2013; Baker and Fradkin, 2017; D'Amuri and Marcucci, 2017).

[14] Note that $G(6)$ is generally always less than 1 simply because of the way it is constructed. In the six months prior to interview, the index could reach the maximum value of 1, but this is averaged with other values that are less than 1 by definition. Note also that the month in which $G(0)$ takes its (country-specific) maximum value of 1 may not map to any individual observation in the SHARE sample.

performing our main analysis across countries. Similarly to *Google Trends*, we perform an article count by searching within each newspaper using the relevant translation of 'pension reform' or appropriate alternatives. We obtain a count index averaging across available newspapers. Analogously to *Google Trends*, we then rescale the average count by the maximum monthly count observed over 2003–13 for the country. We finally compute $News(6)_{jt}$ as the mean of the index over the six months preceding the interview date t in country j to measure recent supply of information in newsprint. Full details of our use of the data, including a complete list of titles, are included in Online Appendix A5. The distribution of articles is shown in Figure A.2, while Figure A.3 plots the joint relationship with Google searches across date and country.

A complication when using this database, as with similar datasets, is that the subscription to different newspaper sources is patchy over time. Some news sources are only available for 90 days prior to an analyst's search, others are available much earlier, but may equally not cover up until the present day. Additionally, the types of newspapers available, both to the public and in the database, are not uniform across countries. It is for these reasons that we favour the use of historic *Google Trends* as our main measure of information available from the media, and we use the Nexis data mainly to provide supporting evidence.

2. Empirical Strategy

Our aim is to assess how information flows affect beliefs in the proximity of a pension reform. To define notation, let $I(-s,-1)_{jt}$ be an indicator function taking value 1 if workers in country j at time t (given by year and month) are interviewed up to s months *before* the reform implementation. Similarly, let $I(0,s)_{jt}$ take value 1 for interviews conducted between the implementation date and up to s months *after*. These two indicators are mutually exclusive because we define them relative to the closest reform, but in Section A2 of Online Appendix A we also discuss results using an alternative definition for reforms that are contiguous.[15]

We examine announcements in the pre-reform period by defining two different indicator functions. In particular, let $I^A(-s,-1)_{jt}$ take value 1 if individual i in country j at time t is interviewed up to s months before a reform has been announced, and let $I^U(-s,-1)_{jt}$ be defined equivalently for a reform that is as yet unannounced. Accordingly, by definition, $I(-s,-1)_{jt} = I^A(-s,-1)_{jt} + I^U(-s,-1)_{jt}$. Again note that, in the benchmark definitions, $I(0,s)_{jt}$, $I^A(-s,-1)_{jt}$ and $I^U(-s,-1)_{jt}$ are all pairwise mutually exclusive.

In its more general form, our estimation is based on the specification

$$\begin{aligned} y_{ijt} = {} & \rho_0 + \rho_{11} I^A(-s,-1)_{jt} + \rho_{12} I^U(-s,-1)_{jt} + \rho_2 I(0,s)_{jt} \\ & + \delta G(\tau)_{jt} + \psi_{11} I^A(-s,-1)_{jt} \times G(\tau)_{jt} + \psi_{12} I^U(-s,-1)_{jt} \times G(\tau)_{jt} \\ & + \psi_2 I(0,s)_{jt} \times G(\tau)_{jt} + \mathbf{X}'_{ijt}\Theta + \varphi_t + \lambda_j + \varepsilon_{ijt}, \end{aligned} \tag{1}$$

where $y_{ijt} = \{B_{NRA,ijt}, B_{PB,ijt}\}$ with $B_{NRA,ijt}$ referring to the chances that the government raises the national retirement age before worker i in country j at time t retires, and $B_{PB,ijt}$ referring to the chances that the government reduces pension benefits before i retires. Here $\mathbf{X}_{ijt}$ is a vector of individual-specific control variables (including age, gender, education, marital status, employment status, income and health), φ_t denotes time (month $\times$ year of interview) fixed

[15] If the distance s to two successive reforms is exactly the same then we impose $I(0,s)_{jt} = 1$ and $I(-s,-1)_{jt} = 0$. This happens for 103 cases in July 2011 in Austria. Our results do not change if we either redefine the indicator functions in the opposite way or drop those observations from the analysis.

effects, λ_j denotes country fixed effects, τ is the time length over which media information (here online search) is accumulated and ε_{ijt} is a stochastic error component.[16] We estimate (1) using ordinary least squares (OLS), but to take account of the nature of the expectation variables that vary between 0 and 1, we also use the fractional response estimator of Papke and Wooldridge (1996).[17] Standard errors are clustered at the (country $\times$ month-of-interview) level, which is the level at which the variation in the variables of interest occurs.

As we include both country and month-of-interview fixed effects, the identification of the ρ, δ and ψ parameters comes from variation in the introduction of reforms within countries, and net of time-specific aggregate effects across countries. Figure 4 graphically illustrates the abundant variation in $I^A(-12, -1)$, $I^U(-12, -1)$ and $I(0, 12)$ over the sample period.[18]

Our baseline estimates are obtained from a simplified version of (1) in which we do not consider announcements, and instead use $I(-s, -1)$ rather than $I^A(-s, -1)$ and $I^U(-s, -1)$. Following the discussion in the previous section, the corresponding parameter, $\rho_1(= \rho_{11} = \rho_{12})$, captures the total effect that includes both announcements and information flows from other sources. Simple intuition suggests that expectations are revised up ($\rho_1 > 0$) whenever useful information is available. In the more detailed specification, we look separately at the responsiveness of beliefs to reform announcements (captured by ρ_{11}) and other information available prior to an announced reform (ρ_{12}). Post reform, ρ_2 identifies the change in expectations as a result of the reform enactment. If the reform is not considered by workers to be once-and-for-all then expectations need not collapse to zero. Indeed, if workers believe that reforms are positively serially correlated then beliefs should increase. We also estimate variants of (1) that we describe in more detail below.

3. Main Results

3.1. *Baseline Evidence from Reform Implementations*

Table 2 presents least squares estimates for a version of (1) with simplifications in two directions. We include just a single pre-reform indicator without distinguishing announced from unannounced reforms, and we also abstract from online search. In our notation, therefore, we set $\rho_{11} = \rho_{12} = \rho_1$ and $\delta = \psi_{11} = \psi_{12} = \psi_2 = 0$. Here we focus on the relationship between expectations and reforms using a time window around reforms of 12 months.

Focussing first on our preferred specification without macro controls, column (ii) shows that the expectations of a reform that increases national retirement age ('NRA') rises by a little under 10 percentage points, from a median of 50%, just before its implementation. The same is true for reforms that reduce pension benefits ('PB', column (v)). Figure 5 plots these regression results (left panels), together with results from a similar regression where the reference period is shifted to more than 24 months away from a reform (right panels), and illustrates the increase in pension reform expectations prior to a reform. Individuals, therefore, adjust their beliefs leading up to

[16] It is useful to reiterate that $G(6)$ is an average computed over the six months prior to interview and is not pegged to any specific reform. Thus, if an individual is interviewed before the introduction of a reform then $G(6)$ will cover the pre-reform period only. Similarly, if the interview date takes place at least six months after the enactment of a reform, $G(6)$ will cover only the post-reform period. If the date of interview is instead less than six months after the reform implementation, $G(6)$ will cover some of the pre-reform period as well as some of the post-reform period.

[17] Since the results found with this estimator are virtually identical to the OLS estimates, we report them in the Online Appendix (Table A.6) and do not comment on them here.

[18] In some cases these indicators also vary within country and wave, since individuals are interviewed in different months. In the Online Appendix (Table A.4), we provide a breakdown of the sample along these dimensions.

Table 2. *Beliefs and Reforms.*

		(i) Sample mean	(ii)	(iii)	(iv) Sample mean	(v)	(vi)
			B_{NRA}			B_{PB}	
$I(-12, -1)$	(ρ_1)	0.249	0.096***	0.054***	0.199	0.085***	0.031***
			(0.016)	(0.012)		(0.017)	(0.010)
$I(0, 12)$	(ρ_2)	0.135	−0.030**	−0.042***	0.061	0.078***	−0.010
			(0.013)	(0.011)		(0.028)	(0.020)
Unemployment rate				1.061***			0.437
				(0.366)			(0.348)
Log(GDP per head, constant price PPP)				−0.146			−1.398***
				(0.283)			(0.227)
Long-term government bond yields				3.345*			−2.760***
				(1.819)			(0.961)
General gov. gross debt (frac. GDP)				−0.109*			0.220***
				(0.059)			(0.055)
Gen. gov. net lending (+) /net borr. (−) (frac. GDP)				−1.216***			−1.926***
				(0.420)			(0.352)
Mean of dep. var.			0.514			0.535	
N			27,881	27,881		27,881	27,881

Notes: Here B_{NRA} refers to the beliefs an individual has about the chances that the government raises the national retirement age before the respondent retires; B_{PB} refers to the beliefs an individual has about the chances that the government reduces pension benefits before the respondent retires. Chances are defined on the [0–1] scale. Variable $I(-12, -1)$ is equal to 1 over the 12 months before a reform, and 0 otherwise; $I(0, 12)$ is equal to 1 over the 12 months following a reform (and in the month of the reform), and 0 otherwise. The definitions of the $I(\cdot, \cdot)$ variables depend on the margin of reform analysed, so they refer to reforms increasing retirement age in the B_{NRA} case and to reforms cutting benefits in the B_{PB} case. The table reports least squares estimates, obtained from a sample of employed and self-employed individuals aged 50–64, and observed in SHARE waves 1, 2, 4 and 5. All cases for which the total number of country-month-year observations is fewer than 30 are excluded. Estimates in this and other tables also make use of Stata modules by Baum *et al.* (2010). All regressions include a constant, log of household income and log of net wealth, household size and indicator variables for age, sex, education, employment status (civil servant, self-employed; base: private sector employee), part-time employment status, citizenship, self-reported health, marital status. They also include country and time (month and year of interview) fixed effects. Macro variables are from OECD (GDP per head; long-term government bond yields, which refers to government bonds maturing in ten years; general government net lending (+)/net borrowing (−)) and World Bank (general government gross debt). All macro variables apart from log(GDP per head) are in the same scale as beliefs; hence, the coefficient can be interpreted as the percentage point increase in beliefs for each percentage point increase in the macro variable. We denote by N the number of person-wave observations. The standard errors, in parentheses, are clustered at the country × month-year of interview level (237 groups). * Significant at 10%; ** significant at 5%; *** significant at 1%.

policy changes, suggesting that the pre-reform period carries valuable messages about the state of the world. However, expectations are on average still quite far from certainty that a reform will happen (given by the objective probability of 100%), suggesting that information rigidities are important. This is true even if we exclude older respondents who may be retired before the reform and for whom the objective probability is 0 (see Online Appendix Table A.7, panel A). Overall, this means that existing messages are noisy and/or that individuals are unable to process them accurately.

We also look at expectations *after* a reform has been enacted. Interestingly, for PB reforms (column (v)), the time that follows a reform seems to have the same effect on the expectation as the time that precedes it. However, the estimates in column (ii) reveal that NRA reforms work differently. In the 12 months after implementation, individuals *reduce* their expectations of further reform by 3 percentage points. This asymmetry between PB and NRA reforms indicates that individuals believe that governments find it easier to repeat the first type of reform than the second, either due to bureaucratic administration or political acceptability. The asymmetry in

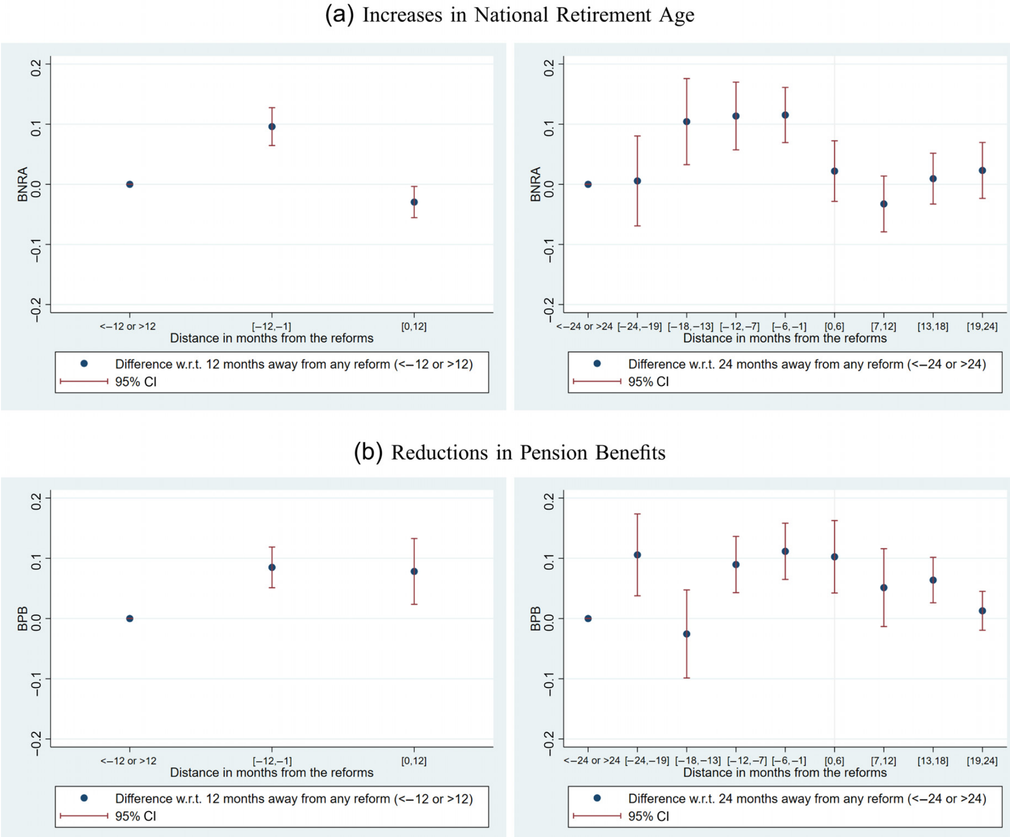

Fig. 5. *Beliefs and Reforms.*

Note: Figures on the left reproduce the regression results in Table 2, by plotting the coefficients on $I(-12, -1)$ and $I(0, 12)$, which capture the change in beliefs with respect to periods more than 12 months away from a reform (which is the reference category in the regressions). The figures on the right plot coefficients from similar regressions, in which the reference period is shifted to more than 24 months away from a reform and the dummy variables for the periods before and after the reform are split into single semesters (a dummy for each semester is included in the regression). The regressions include all the covariates as in Table 2 and standard errors are clustered at the country × month-year of interview level.

updating further indicates that individuals make important distinctions about the nature of the reform and their different underlying data generating processes.

Pension reforms are, of course, partly determined by wider macroeconomic outcomes. We explore this by next augmenting the main specification with a set of country-specific, time-varying, macroeconomic indicators, such as unemployment rate, GDP *per capita*, government debt as a percentage of GDP, government borrowing and long-term government bond yields. The results are shown in columns (iii) and (vi) of Table 2. As expected, these indicators are strongly correlated to beliefs. The deterioration of the national economy, or of public finances, leads to an increase in expectations of a new pension reform. This suggests that people are aware of macroeconomic trends and incorporate them in their policy beliefs. But even after controlling for macroeconomic trends, pension reforms themselves continue to be a powerful predictor of expectations before implementation, suggesting that information about impending reforms is important for belief updating over and above the knowledge of the state of the economy. Post reform, however, interesting differences emerge. In particular, in the case of pension benefit reductions, we no longer see a significant impact on beliefs over the 12 months following their introduction. With regards to the asymmetry compared to retirement age reforms, shown in the base specification, it seems that much of the difference is driven by the macroeconomic cycle. The implication is that workers believe that policy makers turn to different types of reform in different macroeconomic conditions.

The results in Table 2 are robust to a wide range of sensitivity checks, such as isolating regional differences across Europe (specifically Italy and Spain; see Online Appendix Table A.7); varying the time windows around reforms (Online Appendix Table A.8) and allowing each type of reform to 'cross-affect' the other belief domain (Online Appendix Table A.9), as well as those we describe below. A series of additional sensitivity checks is discussed in Online Appendix Section A2, Tables A.1 and A.2.[19]

3.1.1. *Rounding of probabilistic expectations*

As shown by Figure 1 and typical of probabilistic expectation data, many individual responses seem to be rounded to focal answers, such as to 50%. To test robustness of estimates accounting for this possible rounding, we employ the approach of Manski and Molinari (2010), who posited that answers given at focal points reflect an underlying belief from a broader range. The belief range is modelled as depending on the individual's type (or rounding practice). We specify types by examining answers to all expectation questions in the survey, such as the chances of being sunny tomorrow or of leaving an inheritance (see the exact questions and type definition in Online Appendix Table A.10). For example, a response of 10% for someone who only ever answers in multiples of 10 is modelled as reflecting a 'true' latent belief of between 5% and 15% (see Manski and Molinari, 2010 for a description of the modelling, and full justification).

This approach enables partial identification of the effect of the reform indicators and so generates bounded estimates. The results are shown in Table 3, in the fourth and fifth columns, underneath the heading 'Entire sample'. Importantly, for the PB and NRA pre-reform indicators, the lower bound is a little over 0, and the upper bound is roughly double the point estimate (repeated from Table 2 in the first column of Table 3). This suggests that there is indeed a positive effect ranging from above 0 to about 0.17 for the imminence of reform on the formation of probabilistic beliefs, even when rounding is accounted for. For the post-reform indicators, the bounds are similarly wide, but now both include 0 comfortably.

The extreme right-hand columns of Table 3 show results when we examine types who are successively more precise in their belief formulations. Our less restrictive sampling excludes individuals who only ever respond with 0%, 50% or 100%. Our final, most restrictive sampling (used for the final two columns) excludes individuals who only ever respond with multiples of 10, and never reply with more precise numbers, such as 98% or 99%. For this latter group, the bounds

[19] The additional exercises include (*i*) dropping one country at a time from estimation; (*ii*) adding country-specific timing and results of general elections as additional explanatory factors (using data from the ParlGov database by Döring and Manow, 2016); (*iii*) changing sample selection to include individuals even in year-month cells in which there are fewer than 30 observations; (*iv*) defining the pre-reform and post-reform dummies not as mutually exclusive; (*v*) controlling for reforms that increase benefits or extend retirement age; and (*vi*) controlling for time to national retirement age on top of age. The results from all such exercises are in line with the evidence found in the baseline analysis.

Table 3. *Beliefs and Reforms, Set Estimates.*

		Point estimates			Set estimates					
		Entire sample	Excluding respondents always answering to probabilistic questions with multiple of:		Entire sample		Excluding respondents always answering to probabilistic questions with multiple of:			
			50	10			50		10	
					LB	UB	LB	UB	LB	UB
Panel A: dependent variable B_{NRA}										
$I(-12, -1)$	ρ_1	0.096	0.097	0.097	0.013	0.174	0.019	0.169	0.054	0.138
		[0.064, 0.128]	[0.065, 0.128]	[0.064, 0.130]	[−0.020, 0.206]		[−0.014, 0.202]		[0.012, 0.180]	
$I(0, 12)$	ρ_2	−0.030	−0.029	−0.021	−0.106	0.049	−0.100	0.044	−0.063	0.021
		[−0.056, −0.003]	[−0.055, −0.002]	[−0.048, 0.006]	[−0.130, 0.073]		[−0.125, 0.069]		[−0.094, 0.052]	
p-value for H_0: coefficient equal to the entire sample			0.552	0.575						
Panel B: dependent variable B_{PB}										
$I(-12, -1)$	ρ_1	0.085	0.086	0.081	0.001	0.165	0.007	0.160	0.037	0.124
		[0.051, 0.119]	[0.053, 0,119]	[0.048, 0.115]	[−0.032, 0.197]		[−0.022, 0.190]		[−0.001, 0.162]	
$I(0, 12)$	ρ_2	0.078	0.078	0.085	−0.048	0.199	−0.039	0.190	0.018	0.149
		[0.023, 0.133]	[0.023, 0.132]	[0.028,0.142]	[−0.104, 0.256]		[−0.094, 0.245]		[−0.046, 0.213]	
p-value for H_0: coefficient equal to the entire sample			0.607	0.703						
N		27,881	27,457	12,781	27,881		27,457		12,781	

Notes: LB and UB are lower and upper bounds. In square brackets the 95% confidence interval for the point estimates (standard errors are clustered at the country × month-year of interview level), while for the set estimates, we report the confidence set that asymptotically covers the entire identification region with 95% probability, based on Beresteanu and Molinari (2008) and estimated using their programmes with bootstrapping adjusted for clustering at the country-month level (we performed 1,000 bootstrap replications). The set estimates are obtained following Manski and Molinari (2010). Each respondent is classified in a rounding type on the basis of his/her answers to all probabilistic questions (of any type in any wave). Depending on the type of respondent (see the text and Table A.10 in the Online Appendix), each observation is turned into an interval. For instance, the answer 0 from a respondent that always answered 0 or 100 to expectation questions (of whatever type in any wave) is assigned to the interval [0,50]. The answer 20 from a respondent that at least once answered with a multiple of 10 (different from 0, 50 or 100) is assigned to the interval [15,25]. The method defined by Beresteanu and Molinari (2008) then allows us to bound the best linear probability coefficient for any single regressor. All the regressions include the controls that we use in the main regression (Table 2). The p-value for coefficients equal to the entire sample refers to a Wald test for the joint hypothesis that the coefficients on $I(-12, -1)$ and $I(0, 12)$ are jointly equal to their counterparts in the entire sample.

Table 4. *Beliefs and Reforms, Estimates on the Longitudinal Sample.*

		(i) B_{NRA}	(ii) B_{PB}I
Panel A (OLS—individuals with at least two observations)			
$I(-12, -1)$	ρ_1	0.078***	0.053***
		(0.009)	(0.009)
$I(0, 12)$	ρ_2	−0.044***	0.033**
		(0.010)	(0.017)
N		14,319	14,319
Panel B (individual fixed effects)			
$I(-12, -1)$	ρ_1	0.076***	0.024**
		(0.011)	(0.010)
$I(0, 12)$	ρ_2	−0.025**	−0.022
		(0.011)	(0.020)
N		14,319	14,319
Panel C (first difference)			
$I(-12, -1)$	ρ_1	0.064***	0.030**
		(0.017)	(0.012)
$I(0, 12)$	ρ_2	−0.045***	−0.047**
		(0.015)	(0.024)
N		5,861	5,861

Notes: In panels A and B we include only individuals with at least two observations in the main sample; standard errors are clustered at the level of the household of the individual (the household refers to the first household where the individual is observed). Panel A includes all controls as in Table 2. In panel B the regression includes individual fixed effects and all time-variant covariates used in Table 2. In panel C the regression is estimated in first difference (covariates included); the first differences are calculated before removing the country-year-months where the total number of cross-sectional observations is less than 30. Standard errors are clustered at the (country × month-year of interview) level. The first difference is with respect to the previous available wave with beliefs on pension reforms; hence, for wave 4, the difference is with respect to wave 2. The number of observations in panel C refers to the number of observed changes. See Table 2 for other comments. ** Significant at 5%; *** significant at 1%.

are tighter and indicate a sizeable belief revision before implementation. To show consistency, the left-hand columns of Table 3 reveal that the point estimates for these narrower samples are almost identical to those for the entire sample and with similar confidence intervals.

Overall, this exercise therefore indicates that an agnostic approach to modelling response behaviour yields conclusions that support our main analysis.

3.1.2. *Individual revisions of expectations*

The longitudinal nature of the SHARE data allows us to estimate individual fixed effect (FE) models controlling for time-invariant individual-specific characteristics. Here, however, attrition due to retirement (after which the pension reform expectations are not asked), and ill health or death reduces the estimation subsample to half the size of the baseline sample. The results from this analysis are given in Table 4. The FE estimates for the beliefs that the government increases retirement age are remarkably close to their least squares counterparts. The estimates for the expectations that the government reduces pension benefits show a smaller increase when we look at the pre-reform period compared to the baseline estimates. But least squares models run on the same subsample used for the FE estimation also yield estimates that are smaller than our baseline results. The post-reform indicator, $I(0, 12)$, instead has a statistically insignificant impact on beliefs, whereas its corresponding least squares estimates reveal a positive, significant effect. Finally, this table also shows estimates from regressions in first differences, with broadly similar results.

Table 5. *Beliefs and Reforms, Controlling for Lagged Beliefs.*

	B_{NRA}		B_{PB}	
	(i)	(ii)	(iii)	(iv)
$I(-12, -1)$	0.167***	0.060*	0.181***	0.079*
	(0.026)	(0.032)	(0.042)	(0.042)
$I(-12, -1)_{w-1}$	0.077***	0.027	0.096***	0.049*
	(0.022)	(0.022)	(0.027)	(0.025)
$B_{.,w-1}$	0.352***	0.249***	0.369***	0.294***
	(0.020)	(0.020)	(0.020)	(0.017)
$B_{.,w-1} \times I(-12, -1)$	−0.016	0.022	−0.059	−0.054**
	(0.030)	(0.029)	(0.039)	(0.025)
$B_{.,w-1} \times I(-12, -1)_{w-1}$	−0.052*	−0.016	−0.068**	−0.025
	(0.029)	(0.026)	(0.031)	(0.029)
Constant	0.261***		0.280***	
	(0.013)		(0.018)	
N	5,861	5,861	5,861	5,861
Controls		X		X

Notes: Here $w-1$ indicates the previous wave; $B_{.,w-1}$ are the lagged beliefs (referring to B_{NRA} for columns (i)–(ii) and B_{PB} for (iii)–(iv)). As we do not use SHARE wave 3 in the analysis (it does not contain beliefs on pension reforms), for wave 4, $w-1$ refers to wave 2. The sample includes only the observations from Table 2 that refer to individuals observed also in the previous wave. For all the other details, including the full list of controls in columns (ii) and (iv), see Table 2.
* Significant at 10%; ** significant at 5%; *** significant at 1%.

We further take advantage of the panel dimension to investigate whether baseline expectations influence the updating process. To do this, we run a regression of beliefs on lagged beliefs interacted with both the pre-reform indicator and the reform indicator's lag. This regression picks up the weight that workers put on their prior (lagged beliefs) in addition to the reform's presence. The lagged reform indicator is included to control for the presence of reforms in the preceding period, while the interactions capture heterogeneity in updating. The results are reported in Table 5. Focussing on the estimates with full controls, columns (ii) and (iv) show that the reform indicators are slightly lower than for our baseline specification, but still significant across much of the distribution of prior beliefs, despite a substantially reduced sample size. There is some evidence that those with lower prior beliefs of PB reforms update more (as captured by the negative coefficient on the interaction of lagged beliefs with the reform indicator), but the magnitude is small (0.5 percentage point more updating for those with a 10 percentage point lower posterior). No similar evidence however emerges for NRA reforms.

3.2. *Expectation Formation and Information*

3.2.1. *Official announcements*

To better understand the drivers of the updating process in the period leading up to a reform, we distinguish announced from unannounced reforms. Announcements create plausibly exogenous variation in the cost of information gathering. We think of them as information shocks that are followed by periods of high media coverage, making information less costly to acquire. The results are summarised in panel A of Table 6, which reports only the estimates on the pre-reform indicators. The ρ_2 estimates on the post-reform period do not change and are thus omitted.[20]

A striking result emerges unambiguously: the impact of *announced* reforms on beliefs is nearly identical to the impact of *unannounced* reforms. In the 12-month window considered here,

[20] Distinguishing the post-reform period into periods of announced reforms and periods of unannounced reforms is not feasible, since we have too few cases of totally unannounced reforms.

Table 6. *The Effect of Announcements and Media Activity on Beliefs.*

		(i) Sample mean	(ii) B_{NRA}	(iii) Sample mean	(iv) B_{PB}
Panel A (N = 27,881)					
$I^A(-12, -1)$	(ρ_{11})	0.055	0.109***	0.046	0.082***
			(0.037)		(0.036)
$I^U(-12, -1)$	(ρ_{12})	0.194	0.091***	0.153	0.086***
			(0.016)		(0.017)
Panel B (N = 24,394)					
$G(6)$	(δ)	0.082	0.298***	0.082	0.517***
			(0.067)		(0.062)
Panel C (N = 23,219)					
News(6)	(δ)	0.090	0.198*	0.090	0.335***
			(0.111)		(0.098)

Notes: Here $I^A(-12, -1)$ is equal to 1 if the month of interview is between 12 months and one month before a reform and the reform had already been announced, and 0 otherwise; $I^U(-12, -1)$ is equal to 1 if the month of interview is between 12 months and one month before a reform, and the reform had not been announced yet, and 0 otherwise; $I(0, 12)$ is equal to 1 over the 12 months following a reform (including the month of the reform), and 0 otherwise; $G(6)$ is the *Google Trends* index on the intensity of online search about the keyword 'pension reform' averaged over the six-month period prior to interview. In panel B the sample is the same as in Table 2, but Sweden is not included in this analysis, as there are not enough *Google Trends* data available (see Online Appendix A4), and the first semester of 2004 cannot be included because *Google Trends* are available since January 2004. Index *News*(6) is the *Nexis* index of monthly count of news articles about 'pension reforms', rescaled by the maximum monthly count by country (over 2003–13) and averaged over the semester ending in the month of the interview. Sweden is not included because of limited newspaper coverage in LexisNexis and paucity of data (see Online Appendix A5). For all the other details, including the full list of additional controls, see the notes to Table 2. * Significant at 10%; *** significant at 1%.

individuals revise their expectations up by 8.2 percentage points in the case of an announced PB reform and by 8.6 percentage points in the case of an unannounced PB reform (column (iv)). This similarity is also true when we look at NRA reforms (column (ii)).[21]

These findings are important. On the one hand, they show that, despite official government announcements that a reform will take place, individuals revise their beliefs upward by no more than 11 percentage points on average, from mean and median beliefs close to 50%. This is evidence against a benchmark of frictionless full information updating, and suggests either inattention to these announcements or difficulty in processing the information they convey.[22] A related interpretation is that workers may not believe that the reform will be enacted, despite the formal announcement, because they may expect strong hostility from the general public, or labour unions, or opposition parties that could lead to a policy reversal. On the other hand, the results show that, even without announcements, individuals are able to acquire and process information about the occurrence of a reform that is as informative as a pension reform announcement and the associated information dissemination that may follow it. Recall that some reforms that arrived without announcement were large and presumably followed increased discussions and expectations: the 2005 Dutch reform and the 2011 Italian reform are prominent examples.

[21] The estimates in Table 6 are robust to excluding all individuals from Switzerland, for which the definition of announcement is complicated by the presence of referenda after law approvals. They also hold whether we consider a pre-reform window of six or 18 months (Online Appendix Table A.8).

[22] A suitable benchmark is that workers should update their beliefs to 100% after an announcement.

3.2.2. *Media activity*

We now turn to the estimates on media activity, exploring the role of newspaper coverage and online search activity. To reiterate the discussion in Section 1.4 on the latter channel, six-month average online search activities, which are highly correlated to public opinion and social sentiments on a wide variety of issues, proxy the intensity of discussion and information available on social media, and social milieu more generally, related to pension reform. This information in turn affects expectations over and above reform announcements and implementations.

To explore the role of search, we start with another simplified version of (1) in which we do not explicitly consider the timing of reforms or announcements themselves, i.e., $\rho_{11} = \rho_{12} = \rho_2 = \psi_{11} = \psi_{12} = \psi_2 = 0$. Panel B in Table 6 reports the estimates by belief domain of δ found using $G(6)$. The results reveal that periods of more intense online search are associated with significantly greater expectations of reform. This link is strong: a one-SD increase in the number of monthly online searches is associated, on average, with a 3.8 percentage point increase in B_{NRA} and with a 6.6 percentage point increase in B_{PB}.[23]

A concern with our G measure is that online search for the keyword 'pension reform' could be too restrictive and unable to pick up the variety of information on policy changes that workers may attentively weigh in while forming their expectations. As a check, therefore, we re-estimate the same simplified version of (1) as before, where we also include the six-month pre-interview average of the *Google Trends* index for the keyword 'austerity', $G^a(6)$, in addition to $G(6)$. This alternative measure is likely to capture interests that go beyond, but are still related to, government actions around pension reforms. We show the results in panel B of Online Appendix Table A.11. The relationship of $G^a(6)$ with expectations is similar to that of $G(6)$, just slightly weaker in the case of beliefs about pension benefit reductions. This suggests that people also base their pension reform beliefs on broader information about government measures to reduce public expenditure. Nevertheless, the magnitude of δ on $G(6)$ declines only marginally, remaining large and statistically significant.[24]

Turning to the role of newspaper coverage, we employ the same simplified version of (1), but this time using a standardised score of the average coverage over the previous six months in newsprint. As shown in panel C of Table 6, the results for $News(6)$ confirm those for $G(6)$ across both domains, even though the estimates are slightly smaller. These results therefore support our findings that supply of information from the media is strongly associated with increased beliefs of future reform.

3.2.3. *The joint effect of reforms, announcements and media activity*

The introduction of a reform, its announcement (or lack of it) and the acquisition of information from media sources have each been shown to affect belief formation separately. In what follows we analyse how all such channels work as posited in (1) and analyse their joint effects. As

[23] The SD of $G(6)$ is 0.13 (see Table 1).

[24] We perform two additional exercises of note, both reported in Online Appendix Table A.11. First, it is possible that online search in other domains is also associated with our two expectations measures. For this reason, we repeat the analysis including, alongside $G(6)$, the variable $G^{oth}(6)$, which is the six-month pre-interview mean of the *Google Trends* index for the keyword 'reform', i.e., excluding the word 'pension'. This exercise is performed in the spirit of a placebo test. Estimates are shown in Table A.11, panel C. The effects of $G(6)$ are close to those reported previously. The $G^{oth}(6)$ estimates, however, are 4 to 7 times smaller, and at most ever marginally significant. A second consideration is that Google searches and online activity trended strongly upwards over the sample period, from a comparatively small base. Even though we account for time trends with the fixed effect structure, it is possible that differential time trends across countries or age groups may bias our results. However, the bottom panel of Table A.11 shows that, when we control for a measure of country-level internet penetration, the effect of our $G(6)$ variable is unaltered.

Table 7. *The Effect of Interactions Between Announcements and Online Search on Beliefs.*

		B_{NRA}		B_{PB}	
		(i)	(ii)	(iii)	(iv)
$I^A(-12, -1)$	(ρ_{11})	0.079**	0.134**	0.029	0.096**
		(0.036)	(0.060)	(0.034)	(0.041)
$I^U(-12, -1)$	(ρ_{12})	0.091***	0.043***	0.084***	0.052***
		(0.014)	(0.016)	(0.012)	(0.013)
$I(0, 12)$	(ρ_2)	−0.038***	−0.039***	0.042*	0.102***
		(0.013)	(0.013)	(0.024)	(0.024)
$G(6)$	(δ)	0.284***	0.232***	0.496***	0.642***
		(0.053)	(0.047)	(0.059)	(0.129)
$I^A(-12, -1) \times G(6)$	(ψ_{11})		−0.203		−0.351*
			(0.222)		(0.184)
$I^U(-12, -1) \times G(6)$	(ψ_{12})		0.775***		0.541***
			(0.233)		(0.208)
$I(0, 12) \times G(6)$	(ψ_2)		−0.042		−0.297**
			(0.113)		(0.129)

Notes: The sample is the same as Table 6, panel B; the number of person-wave observations is equal to 24,394 in all regressions. For all the other details, including the full list of additional controls, see the notes to Tables 2 and 6. * Significant at 10%; ** significant at 5%; *** significant at 1%.

discussed in Section 1.4, and mainly for reasons of data consistency, we focus on online search as the media channel, using the *Google Trends* index.

Before discussing these joint effects, we briefly discuss how contemporaneous online search, which we view as a proxy for information gathering, itself evolves in the period leading up to a reform. We have argued that government announcements reduces the cost of information acquisition. It is nevertheless possible that they also affect the perceived benefits of information such that their effect on information gathering is unclear. We provide suggestive evidence by performing a simple event-study analysis of the $G(0)$ index around reforms, controlling for country fixed effects, and separating the effects for periods with and without announcements. The results presented in Online Appendix Figure A.4 show clearly that contemporaneous internet search peaks at the time of the reform itself, suggesting that the reform announcement itself stimulates greater information gathering, building up in the preceding period. This build-up is higher when reforms have been announced than when they are currently unannounced. Post-reform, search declines steadily for around four months, until it reaches a steady state.

Table 7 presents the results of the joint effects of reforms, announcements and online search. Columns (i) and (iii) of the table present estimates when the interactions between online search and reforms are set to zero, i.e., $\psi_{11} = \psi_{12} = \psi_2 = 0$. Columns (ii) and (iv) report the estimates for the full specification (1), using 12-month reform windows, and the *Google Trends* index computed over the six months preceding the interview date.

The results in column (i) and (iii) show that the main proxies for information we use leading up to a reform are positively associated with expectations, although the coefficient associated with announced reforms is reduced somewhat when online search is controlled for. These results are consistent with new information driving expectations upward in the time leading up to reforms.

Columns (ii) and (iv) refer to the specifications in which we also estimate the ψ parameters, and so allow for full interactions. The results are similar for both reform domains. At low levels of internet search, announcements raise beliefs upwards ($\rho_{11} > 0$), but unannounced reforms instead have less effect on beliefs (ρ_{12} is closer to 0). This finding is intuitively sensible: with no announcements and no online search, workers have no information with which to modify their beliefs.

From this baseline, increasing the level of search has differing effects across environments. When reforms are unannounced, online search strongly increases expectations, i.e., $\delta + \psi_{12} > 0$ for both reform types. Conversely, when reforms have been announced, internet search has no positive effect on beliefs, i.e., $\delta + \psi_{11}$ is insignificantly different from zero across both domains. These estimates show that, when reforms are imminent, but have not yet been announced, online search is powerful in revealing the true state of the world. Yet, when reforms have been announced, online search, although more frequent in quantity, has far less information content about the probability of reform itself. Having said that, online search here might change beliefs about reform details, for which however we have no data.

We show this feature in a slightly different way in Online Appendix Figure A.5, where we plot the effects of switching on $I^A(-12, -1)$, $I^U(-12, -1)$ and $I(0, 12)$ across the entire $G(6)$ distribution. The profile for $I^A(-12, -1)$, for example, shows $\rho_2 + \psi_2 \times G(6)$. As online search becomes more intense, the effects of announced and unannounced reforms converge. Again, these results indicate the strong interacting effects of different information sources.

Turning to the period after reform implementations, the change in expectations, measured by ρ_2, is similar to that found in Table 2. The introduction of an NRA reform has a negative impact on beliefs that there will be a subsequent NRA reform (column (ii)). But beliefs about PB reforms are revised up by over 10 percentage points (column (iv)). Increasing search intensity post reform further increases the expectations of a reform across both reform domains, though more so in the case of pension benefit cuts. That is, $\delta + \psi_2$ is greater than zero for both domains, but quantitatively lower for retirement age.[25]

4. Heterogeneity and Convergence

4.1. *Heterogeneity in Expectations: Revisions by Characteristics*

Of particular relevance to understanding expectations formation is assessing heterogeneity in the revision process according to key characteristics. Here we look at four characteristics or ‘cues’: (*i*) education, (*ii*) numeracy, (*iii*) age and (*iv*) gender. The first two cues focus on a set of individuals with presumably more cognitive skills and who can therefore more easily process complex information.[26] Older individuals may find information about reform more valuable, for they are closer to retirement and hence have less time to adjust their behaviour following a reform. Existing studies shows that women tend to have different expectations than men, even for events over which they have no control over, such as stock market returns, inflation and home prices (e.g., Dominitz and Manski, 2011; Armantier *et al.*, 2016; Armona *et al.*, 2019).[27]

To keep the analysis simple, in this exercise we define education as a dichotomous indicator that takes value 1 if an individual has a university degree or higher qualification, and 0 otherwise. Numeracy is based on the score to a series of questions about numerical cognitive skills averaged

[25] We carried out sensitivity checks, by adding macro controls and estimating fixed effects models. The estimates, shown in Table A.12 of the Online Appendix, are always similar to those presented in Table 7.

[26] More broadly, several papers have shown a relationship between cognitive function and real pension and retirement outcomes. See, for example, Banks *et al.* (2010).

[27] Similarly, Roth *et al.* (2020) found that men have more confidence in their prior beliefs about macroeconomic employment risks, and consequently respond less to new signals about these risks.

across all available waves in the selected sample, with higher scores indicating greater numeracy skills. (See Online Appendix Section A6 for more details.)

We performed several exercises re-estimating simplified versions of model (1) and interacting the main explanatory variables separately with each of the cues we are interested in.[28] The key results are summarised in Table 8, which only reports the estimates found on each characteristic we consider (labelled 'Cue(k)', with k being either university degree, numeracy, age or female) and on the relevant interaction terms.[29] Panel A, which focuses on the interaction of reforms and announcements, is derived from the regression

$$
\begin{aligned}
y_{ijt} = {} & \theta_0 + \theta_{11} I^A(-12,-1)_{jt} + \theta_{12} I^U(-12,-1)_{jt} + \theta_2 I(0,12)_{jt} + \gamma_1 \mathrm{Cue}(k)_{ijt} \\
& + \gamma_{11} I^A(-12,-1)_{jt} \times \mathrm{Cue}(k)_{ijt} + \gamma_{12} I^U(-12,-1)_{jt} \times \mathrm{Cue}(k)_{ijt} \\
& + \gamma_2 I(0,12)_{jt} \times \mathrm{Cue}(k)_{ijt} + \mathbf{X}'_{ijt}\Lambda + \varphi_t + \lambda_j + \nu_{ijt},
\end{aligned} \tag{2}
$$

while panel B, which looks at online search, comes from the specification

$$
y_{ijt} = \alpha_0 + \alpha_1 G(6)_{jt} + \beta_1 \mathrm{Cue}_{ijt} + \beta_2 G(6)_{jt} \times \mathrm{Cue}_{ijt} + \mathbf{X}'_{ijt}\Pi + \varphi_t + \lambda_j + \epsilon_{ijt}, \tag{3}
$$

where most of the terms in (2) and (3) are the same as those defined in Section 3. The θ_{11}, θ_{12}, θ_2 and α_1 estimates for all four cue-specific regressions, and both types of reforms are in line with the estimates reported in Tables 3, 4 and 5, and are thus not shown.

Of particular interest is the heterogeneity that emerges in panel A where we distinguish announced reforms, i.e., a period in which information acquisition is cheaper, from unannounced reforms. In the 12 months prior to either type of reform, workers with a university degree increase expectations more than their counterparts, but only when the reform is *announced* ($\gamma_{11} > 0$, column (i)). This result is consistent with more educated workers being better able to more easily process accessible information (Cavallo *et al.*, 2014; Fuster *et al.*, 2022). In contrast, older workers have higher expectations in the 12 months that precede reforms, but now the result is driven by reforms that are *unannounced* ($\gamma_{12} > 0$, column (iii)). The expectation revisions when reforms are unannounced are likely to be driven by costly endogenous information acquisition from indirect sources. Compared to their younger counterparts, older workers may acquire more information, even when the costs are higher, or pay more attention to ambient information related to pension reform, because this information is likely to be more valuable to them as they approach retirement (Kézdi and Willis, 2011). This interpretation is strengthened by the estimates in panel B, where $\beta_2 > 0$ (column (iii)). Female workers are less likely to update, but only when reforms are unannounced ($\gamma_{12} < 0$, albeit statistically significant at the 10% level; column (v)), which suggests that women are less likely to acquire information when it is more costly.

The estimates in panel B indicate that better educated, more numerate, older individuals revise up their expectations during periods of high online search ($\beta_2 > 0$; columns (i)–(iii)), while women are no more likely to update beliefs.

[28] SHARE does not collect data on how respondents acquire information about retirement and pension issues. In waves 4 and 5, however, individuals are asked if they read magazines and newspapers (something that can proxy ambient information flows in the model of Section 2) and if they use the internet for a number of purposes, including searching for information. Using the answers to these questions, we constructed two proxy measures of information acquisition and estimated linear probability models of these on the same four cues we analyse here and the same covariates. We find that highly educated workers and workers with high cognitive skills are more likely to read and to use the internet. These features tie in well with the results below.

[29] To ease interpretation, age is rescaled at 50 years and numeracy is demeaned.

Table 8. *Belief Heterogeneity Around Reforms.*

	University degree (i)	Numeracy (ii)	Age 50 (iii)	Female (iv)
Panel A (reforms and announcements)				
Dependent variable: B_{NRA}				
Cue(k)	0.008	0.003	−0.034***	0.011*
	(0.010)	(0.003)	(0.001)	(0.006)
Cue(k) × $I^A(-12, -1)$	0.085***	0.018	0.007	0.002
	(0.029)	(0.016)	(0.006)	(0.017)
Cue(k) × $I^U(-12, -1)$	0.011	−0.008	0.010***	−0.022*
	(0.011)	(0.007)	(0.003)	(0.012)
Cue(k) × $I(0, 12)$	0.001	0.001	0.008***	0.000
	(0.014)	(0.008)	(0.002)	(0.013)
Dependent variable: B_{PB}				
Cue(k)	0.022**	0.008***	−0.023***	0.003
	(0.010)	(0.003)	(0.001)	(0.005)
Cue(k) × $I^A(-12, -1)$	0.048**	0.001	0.007	0.017
	(0.024)	(0.014)	(0.005)	(0.020)
Cue(k) × $I^U(-12, -1)$	−0.007	−0.012*	0.005**	−0.021*
	(0.013)	(0.007)	(0.003)	(0.011)
Cue(k) × $I(0, 12)$	0.036**	0.026**	−0.001	0.002
	(0.018)	(0.010)	(0.003)	(0.013)
Panel B (online search)				
Dependent variable: B_{NRA}				
Cue(k)	0.016	−0.000	−0.033***	0.001
	(0.011)	(0.003)	(0.001)	(0.006)
$G(6)$ × Cue(k)	0.075*	0.040*	0.032***	−0.009
	(0.039)	(0.020)	(0.009)	(0.031)
Dependent variable: B_{PB}				
Cue(k)	0.034***	0.004	−0.026***	−0.008
	(0.010)	(0.003)	(0.001)	(0.005)
$G(6)$ × Cue(k)	0.108**	0.059***	0.025***	0.033
	(0.043)	(0.019)	(0.008)	(0.033)

Notes: The table reproduces the results from Table 6, but including interactions with individual characteristics. The characteristics, referred to as 'Cue(k)', are listed in columns (i)–(iv), with $k = \{$university degree, numeracy, age 50, female$\}$. For their explanation, see the text. Numeracy is demeaned. Each regression includes the same controls as in Table 2, apart from column (ii), in which we do not include education to avoid capturing the effect of numeracy, and column (iii), in which the age dummies are replaced by the continuous variable (age 50). Only the coefficients on Cue(k) and the interaction term are displayed. In column (ii) the number of observations is lower than in the other columns because of missing data in numeracy; for panel A, column (ii), $N = 26{,}791$ and for panel B, $N = 23{,}347$. In the other columns, $N = 27{,}881$ in panel A and 24,394 in panel B. For all the other details, see the notes to Table 6. * Significant at 10%; ** significant at 5%; *** significant at 1%.

In sum, expectations about the introduction of pension reforms vary substantially across workers, and systematically with age, education and in ways that are consistent with both costly access to information and imperfect processing.

4.2. *Do Pension Reform Expectations Converge?*

We now investigate the extent of expectations convergence or divergence in the data. In a canonical setting with frictionless and full information availability, we would expect convergence in beliefs when the reform is imminent. However, belief divergence is also possible in the presence of frictions and costly information acquisition. Indeed, in light of the heterogeneity in revisions just documented, we might consider divergence in beliefs to be more likely in this context.

Table 9. *Belief Dispersion in the Proximity of Reforms and Announcements.*

	$SR(B_{NRA})$	$SR(B_{PB})$	$SR(B_{NRA})$ Accounting for heterogeneity	$SR(B_{PB})$
	(i)	(ii)	(iii)	(iv)
Panel A: reform indicator only				
$I(-12, -1)$	0.009**	−0.000	0.008**	0.000
	(0.004)	(0.003)	(0.004)	(0.003)
$I(0, 12)$	0.017***	−0.009	0.017***	−0.009
	(0.003)	(0.005)	(0.003)	(0.006)
Panel B: with announcement indicator				
$I^A(-12, -1)$	0.026***	0.005	0.025***	0.005
	(0.007)	(0.006)	(0.007)	(0.006)
$I^U(-12, -1)$	0.003	−0.002	0.002	−0.002
	(0.003)	(0.003)	(0.003)	(0.003)
$I(0, 12)$	0.018***	−0.009	0.017***	−0.009
	(0.003)	(0.005)	(0.003)	(0.006)

Notes: In columns (i)–(ii), $SR(z)$ denotes the squared residuals for $z = \{B_{NRA}, B_{PB}\}$ obtained from the two regressions whose results are reported in Table 2 (for panel A above) and from the two regressions whose results are in panel A of Table 6 (for panel B above). In columns (iii)–(iv) we account for heterogeneity by including, in the first-stage regressions used to obtain the residuals, the interaction between the $I(\cdot, \cdot)$ indicators, a dummy for university degree, age (continuous) and gender. The number of person-wave observations is 27,881. For all the other details, including the full list of additional controls, see the notes to Table 2. ** Significant at 5%; *** significant at 1%.

For this analysis, we first measure belief dispersion by computing the squared residuals from earlier regressions, namely, the two regressions reported in Table 2 (in which we look at the time before/after reforms over a 12-month period), and the two regressions reported in panel A of Table 6 (in which we distinguish the 12-month pre-reform bandwidth into announced and unannounced reform periods). These squared residuals are then regressed on the corresponding pre- and post-reform time indicators and the standard set of controls used so far.

The estimates of this analysis are summarised in Table 9, in the first two columns. Column (i) shows that, in the NRA case, belief dispersion increases in the 12 months before a reform. This is especially true when reforms are announced, reflecting more heterogeneous updating, possibly driven by the increased information search that follows a reform announcement. On the other hand, the estimates in column (ii), which refer to PB reforms, indicate neither increased dispersion nor convergence. Individual expectations also become more dispersed in the 12 months after the implementation of the same reforms. It is worth emphasising that these results are entirely consistent with those shown earlier in Table 5: only for PB reforms did we find evidence that those with lower prior beliefs systematically update their beliefs more leading up to a reform; likewise, Table 9 indicates that, for PB reforms, there is less evidence of belief convergence.[30]

We extend our analysis to capture the extent of belief divergence between and within key groups. To do this, we include in our first-stage regressions interactions of the reform indicators with the characteristics shown in Table 8: education, age and gender.[31] The results are shown in Table 9 columns (iii) and (iv), and are virtually identical to those in the first two columns, indicating heterogeneity in revision processes within the groups discussed above.

[30] Also see Online Appendix Figure A.6, which explores the distribution of updating further by plotting the average change in beliefs across the distribution of prior beliefs.

[31] The numeracy variable misses some values, and we therefore omit it here to keep the sample consistently constant across columns. Results are nearly identical with numeracy included.

This analysis suggests that making information cheaper does not decrease the cross-sectional dispersion of expectations, presumably because workers collect heterogeneous pieces of information. The process of expectations revision seems more continuous over time, in contrast to the notion of sticky information that would predict more convergence when information is cheaper to acquire.

Overall, the patterns of beliefs revision we uncover throughout the paper are consistent with models of rational inattention (see Maćkowiak *et al.*, 2018 and Gabaix, 2019 for recent reviews). The limited expectations revision following an announcement is evidence that individuals underreact to shocks. Cost of information and stakes matter: there is more aggregate search when the cost of acquiring information is lower (e.g., after a reform announcement), and individuals with higher stakes revise their expectations more when information is costly (e.g., older individuals who are closer to retirement and in the absence of announcements).

5. Conclusion

Across life domains, individuals revise their beliefs when presented with new and valuable information. Ours is the first paper to examine revisions to expectations about pension reforms, and to investigate individuals' use of available information, in the time around the reforms themselves. To perform the analysis, we construct a novel pan-European dataset of reform implementations and announcements, and combine it with individual-level data on beliefs about future reforms and country-level data on media activity. We identify the impact of the information acquisition on beliefs, taking advantage of the rich variation in the timing of 46 reforms across ten countries.

We find that individuals revise their expectations in the periods leading up to reform enactments. Responses are similar irrespective of whether reforms raise age at retirement or cut pension benefits, and regardless of whether they are announced or not. We further document that the periods surrounding reforms brim with online search that leads to belief updating. In terms of their effects on expectations, announcements and online search are substitutes in the months prior to an impending reform. While there is overwhelming evidence that workers revise up their expectations in the periods leading up to reforms, there is neither evidence of complete uncertainty resolution nor of belief convergence.

Most of these findings are hard to reconcile with canonical frictionless full information updating. Instead, they suggest that workers are inattentive and have different information sets and heterogeneously engage in active information acquisition. The presence of these informational rigidities leads workers to have heterogeneous priors and heterogeneous revision processes. In fact, we do find substantial heterogeneity by observable characteristics. For instance, individuals with a university degree, who may be better at processing freely available information, tend to have greater revisions to beliefs in the period preceding an announced reform compared to their low-education counterparts. The same is true for older workers, who may be searching more intensively for information when it is very valuable to them and reforms are not yet announced. This has important implications from a policy communication standpoint. In particular, it suggests that making a universal policy announcement widely available and readily accessible may not be enough to inform the public about impending reforms. Our findings imply that differentiated messages targeted to specific groups, who may have different levels of vested interests and ability to process complex messages, might be more successful at ensuring people make informed decisions about their future.

Our findings indicate several avenues for further research and data collection.[32] A first avenue is to consider other countries and more recent years, so that we could look at the dynamics of belief updating and information acquisition over the full economic cycle and the pandemic crisis. Another avenue is to analyse additional sources of informative signals other than reforms and announcements, such as industry expert forecasts and pension advisor's reports, which may or may not align with the way in which workers update their expectations. Moreover, another important worker characteristic that should affect beliefs is the individual-specific *exposure* to the reform, which should likely differ by age, gender and occupation. Although this is likely relevant we have not explored this channel here. Additionally, it would be interesting to explore any differences in outcomes by announcement *type*: some formal announcements may be transparent, while others may be more opaque and induce workers to seek additional information. Finally, it may be insightful to elicit from workers the extent of ambiguity, or 'deep uncertainty', about policy, by asking, for example, for the minimum and maximum probabilities of reform (e.g., as in Giustinelli *et al.*, 2022). This suggests the importance of collecting new survey data to better understand expectations formation about public policy across several domains.

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Additional Supporting Information may be found in the online version of this article:

Online Appendix
Replication Package

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