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Emanuele Ciani
Bank of Italy

Adeline Delavande
University of Essex

Ben Etheridge
University of Essex

Marco Francesconi
University of Essex and IZA

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IZA – Institute of Labor Economics

Schaumburg-Lippe-Straße 5–9
53113 Bonn, Germany

Phone: +49-228-3894-0
Email: publications@iza.org

www.iza.org

ABSTRACT

Policy Uncertainty and Information Flows: Evidence from Pension Reform Expectations*

Subjective expectations about future policy play an important role in individuals' welfare. We examine how workers' expectations about pension reform vary with proximity to reforms, information cost, and aggregate information acquisition. We construct a new pan-European dataset of reform implementations and government announcements, and combine it with individual-level representative survey data on expectations about future reforms and country-level data on online search. We find: (1) Expectations are revised upward by about 10 percentage points in the year leading up to a reform, from a median of 50%, regardless of whether the reform is announced; (2) Aggregate online search increases after announcements, when the cost of information is lower; (3) Reform announcements and online information gathering are substitutes in the formation of expectations; (4) Expectations do not converge as a result of announcements or implementations; (5) The effect of information on expectations varies substantially across workers and systematically with observed characteristics that proxy cognitive ability and information value. These findings, interpreted using a model of rational inattention, reveal substantial informational rigidities, with welfare costs that run into trillions of Euros.

JEL Classification: C8, D84, D91, J14

Keywords: expectations, retirement, pension reform uncertainty, reform announcement, online search, rational inattention

Corresponding author:
Marco Francesconi
Department of Economics
University of Essex
Wivenhoe Park
Colchester CO4 3SQ
United Kingdom
E-mail: mfranc@essex.ac.uk

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1. Introduction

Recent events — such as the global financial crisis, climate change, trade wars, and Brexit — epitomize public concerns about policy uncertainty worldwide. Policy uncertainty plays a significant role in individuals' welfare, especially when it involves policies that affect important life-cycle decisions and that are not easy to insure against, including future taxes, home prices, inflation, and pensions. To mitigate uncertainty, individuals can exploit a plethora of information about when and how new reforms might be enacted by governments. We know little, however, about how individuals use this 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 analyze how they vary with proximity to reforms, information cost, and aggregate information acquisition.

Pension reforms are particularly relevant, as demographic changes and the legacy of the 2008 credit crunch 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. Since the early 1990s, economists have increasingly collected data on subjective expectations from survey respondents (Dominitz and Manski 1997; Manski 2004; Hurd 2009; Armantier et al. 2013; Delavande 2014; Wiswall and Zafar 2015; Fuster et al. 2019). Across several events and settings, an empirical regularity which has emerged is that there is substantial heterogeneity in beliefs. This heterogeneity may be explained by private (costly) information held by individuals, differential attention to public information, and different ways of processing public information.

To examine pension policy expectations, we construct a new pan-European dataset of pension reform implementations and announcements covering 10 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, Aging and Retirement in Europe (SHARE) and country-level data on internet search through Google Trends. The latter provides an aggregate measure of information search as well as 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 analyze expectations about whether governments will increase the national retirement age (NRA) or decrease pension benefits (PB) 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 there will be 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, Jappelli,

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

and Padula 2013; Bissonette and van Soest 2015). Importantly, pension reform expectations at the individual level are systematically associated with economically salient subsequent behavior, such as employment, parental care, and financial transfers, suggesting they are relevant to older workers' decisions.

One of our main contributions is to investigate how reform expectations vary around reform enactments. Individuals interviewed in the twelve months prior to PB reforms report expectations that are nearly 9 percentage points higher compared to workers who do not experience a reform in the near future, from a mean (and median) close to 50%. The increase in expectations about NRA reforms is of a similar magnitude.

The data we collect allow us to distinguish imminent reforms that are announced from those that are, at the time of interview, *unannounced*. Reform announcements can be considered as information shocks that reduce the cost of acquiring information, as they are typically followed by ample media coverage. Interestingly, expectations are revised similarly in the period leading up to a reform with and without announcement. On the one hand, this similarity shows strong information rigidities: Despite official government announcements, individuals revise their beliefs upward by only 11 percentage point on average, consistent with either substantial inattention from the public to these announcements, or difficulty in processing this information. On the other hand, this suggests that individuals can forecast reforms without formal announcement: They can acquire and process information which is as meaningful as a formal government policy announcement and the associated information acquisition that may follow it.

To provide a more complete picture of the formation of expectations and the associated endogenous information acquisition that may drive it, we explore the relationship between effected reforms, official government announcements, and aggregate online search.[2] We show that online search increases around the time of government announcements. This heightened search is driven by the announcement itself and not by the impending reform: Online search does not increase with reforms that have not yet been announced. This is consistent with the idea that a lower fixed cost for acquiring information increases information gathering.

Building on these results, we then examine how patterns of search relate to expectations themselves. In particular, we uncover how reform announcements and online search interact in the formation of beliefs. Announcements turn out to be redundant during periods of high online search, while high online search facilitates belief formation when reforms are unannounced. As we discuss through a formal model, this substitutability can be reconciled with the evidence on volume of search. Although the volume of search responds predominantly to government announcements, online search generates belief revisions more in periods when reforms have not yet been announced.

These results are instructive about the information rigidities in the context of policy uncertainty, and reveal important nuances between information acquisition, information content, and revision of expectations. Various models focusing on belief formation with information rigidity about macroeco-

[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., Newman et al. 2019; see also Kennedy and Prat [2018]). Our specific measure uses one of the leading indicators of online information collection, summarizing searches performed through the Google search engine website and collected by Google Trends. See Section 2 for details.

nomic variables have been put forward. These include models in which individuals are continuously updating based on signals that are exogenously noisy (Lucas 1972; Woodford 2002), as opposed to sticky information models in which individuals update infrequently due to fixed costs associated with information acquisition and processing (Mankiw and Reis 2002). Our findings, made possible by the twin availability of expectations data and aggregate information search, suggest that a greater gathering of information need not be accompanied by more belief updating. Furthermore, while we find evidence of costs for changing the information set, the fact that the revision of expectations for an unannounced reform is similar to the revision with announcement implies that the process of belief updating is rather continuous: A model in which individuals only update infrequently when information is cheaper (or more valuable) implies that the effect of announcements should be relatively larger. Our evidence therefore indicates high variability of the effect of information gathering on expectations formation.

Although we find an increase in expectations in the period leading up to a reform, this increase is not accompanied by a convergence in beliefs. Actually, when NRA reforms are announced, the distribution of expectations *diverges*. Making information cheaper does not reduce the cross-sectional dispersion of expectations. This result again suggests that the revision of expectations is continuous, since a model of sticky information would predict more convergence when information is cheaper to acquire.[3]

To better understand this lack of convergence, we analyze how expectation revisions vary according to 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 twelve 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 behavior. Individuals with lower sophistication in probabilistic thinking tend to have lower expectations than their counterparts both for announced and unannounced reforms, suggesting they are searching less intensively for new information and process it less accurately. Overall, our analysis reveals that heterogeneity in expectations comes from heterogeneity in both information gathering and information processing.

In all our analysis we also pay attention to the period *after* a reform. We find the enactment of a reform itself has repercussions on belief formation, although in this case belief updating differs by the reform type. Individuals interviewed up to twelve months after an NRA reform expect another similar reform to be *less* likely to affect them in the future. However, individuals interviewed twelve months after a PB reform continue to expect another similar reform in the future to be *more* likely. This asymmetry by reform type could be due to the fact that individuals believe governments find it

[3]This evidence is in line with findings in Fuster et al. (2019). It also mirrors the analysis in Coibion and Gorodnichenko (2012) who use agents' disagreement following a shock to discriminate between inattention models. They also find evidence against sticky information models.

easier to reduce benefits than to extend age at retirement. It also suggests that, when forming their expectations, individuals make subtle distinctions about the nature of the reform and its possible sequential nature.

We interpret our empirical results within a simple Bayesian framework of belief formation with both exogenous and endogenous information provision. The model is one of rational inattention à la Sims (1998, 2003) and Matějka and McKay (2015). In this environment, announcements and reforms are seen as key channels that reveal meaningful, salient information to workers, who can then gather additional information through online search. We use the model to show how endogenous information acquisition (online search) varies following the exogenous provision of information (reforms), and also how these different information sources interact in belief updating.

After reviewing the relevant literature, we organize the rest of the paper as follows. Section 2 discusses the individual-level data from SHARE and the sources used to construct our indicators of reforms, reform announcements, and online search, and provides evidence on the effect of beliefs on a wide range of expected and actual behaviors. Section 3 formulates the model of rational inattention. We then illustrate the estimation procedure and identification issues in Section 4. Section 5 presents our key results, while Section 6 discusses belief convergence and documents the extent of heterogeneity in expectations revisions. Section 7 provides a simple back-of-the-envelope calculation of the welfare cost due to pension reform uncertainty and summarizes our main findings. An extensive Online Appendix includes additional information on the data, robustness exercises, and further details on the model together with a numerical example.

Related Literature

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 gather information about real-life events and process it 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.[4] Our work focuses on pension reform, an event over which individuals have arguably no direct control.

Psychologists and experimental economists have long analyzed how subjects update probabilities in highly stylized 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; Kahneman and Tversky 1982; Camerer 1987; 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 randomized 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

[4] 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 or not to purchase an item.

convergence in beliefs, even if there is heterogeneity in the revision process that is not always consistent with Bayesian updating (Delavande 2008; Cavallo, Cruces, and Perez-Truglia 2014; Wiswall and Zafar 2015; Armantier et al. 2015, 2016; Armona, Fuster, and Zafar, 2019; Ben-David et al. 2018; Fuster et al. 2019).[5] 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 salient in a survey setting. 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.

Another approach is to analyze 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, Jappelli, and Padula 2006). In all such cases, expectations revisions are responsive to new information. Analyzing stock market returns expectations, Dominitz and Manski (2011) conclude that while there is extensive heterogeneity in revision processes, individuals use interpersonally variable, but intrapersonally stable, processes to form their expectations.[6] In line with these papers, we exploit variation in expectations over time. We offer a richer picture by exploiting in addition variation in information cost generated by government announcements and aggregate search data, which provide useful complementary information on endogenous information gathering.[7]

The patterns of beliefs revision we uncover are consistent with models of rational inattention (see Maćkowiak, Matějka, and Widerholt [2018] and Gabaix [2019] for recent reviews). The limited expectations revision following an announcement substantiates under-reactions 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 respondents with higher stakes revise their expectations more when information is costly (e.g., older respondents who are closer to retirement in the absence of announcements). Although information costs are relevant, high levels of uncertainty and the lack of belief convergence following an announcement lead us to reject the notion of a simple sticky information model.

The evidence of increased aggregate search following an announcement also goes against noisy information models without endogenous information acquisition. As such, our paper complements the empirical literature testing various predictions of inattention models using expectations data (Coibion and Gorodnichenko 2012; Andrade and Le Bihan 2013; Cavallo, Cruces, and Perez-Truglia 2014;

[5]Earlier evidence of changes in risk assessment (about on-the-job risk and climate change) concluded that individuals revised their risk perception in a manner consistent with Bayesian updating (Viscusi and O'Connor 1984; Smith and Johnson 1988; Viscusi and Hakes 1997; Cameron 2005).

[6]Hurd, van Rooij, and Winter (2011) also look 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) provide evidence indicating that heterogeneity in stock market expectations come 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) shows 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 finds that household inflation forecasts are better when there is more news coverage.

[7]A final 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 Bruine de Bruin (2017) find that individuals vary in the number and type of issues they consider when forming stock market expectations. For example, about three-quarter of consumers interviewed report considering "the state of the economy", while less than a third considered "interest rates on loans and mortgages".

Khaw, Stevens, and Woodford 2017). This literature primarily relies on expectations data without any complementary data on information search. An exception is Fuster et al. (2019) who use a within-survey information experiment that allows participants to buy information signals and find evidence consistent with endogenous information acquisition. Like us, they find that a lower cost of information does not lower the dispersion in expectations and explain it by the fact that people acquire different pieces of information. We complement this line of research by studying expectations about policy uncertainty and, importantly, by providing direct evidence outside of the lab on endogenous aggregate information acquisition and how it responds to cost. Our new results show that more information search need not be accompanied by greater belief revisions, since policy announcements and online information gathering are substitutes in the formation of expectations. There is also considerable heterogeneity in belief updating, with university-educated workers, older individuals, and those with more sophistication in probabilistic thinking revising more in the months preceding a reform. Besides age, cognitive limitations appear to be an important source of information frictions.

Finally, we contribute to existing research on policy uncertainty. An important challenge is how to measure policy uncertainty. A strand of the literature estimates variability in past policy changes (McHale 2001; Nataraj and Shoven 2003), whereas another one relies, as we do, on survey questions about future policy (Giavazzi and McMahon 2012; Guiso, Jappelli, and Padula 2013).[8] A different focus is to relate the estimated policy uncertainty to behavior or welfare (e.g., Morris and Shin 2002; Luttmer and Samwick 2018), something we respectively examine in the next section and, briefly, in the conclusion. 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.

2. Data

2.1 The Survey on Health, Aging and Retirement in Europe

Our analysis uses panel data from the Survey on Health, Aging and Retirement in Europe (SHARE).[9] In 2004/05, SHARE collected data on a representative sample of individuals aged 50 and over and their co-resident partners across twelve European countries. Since then, there have been other five sweeps of data at approximately two-year intervals (2006/07, 2008/09, 2011, 2013, 2015) and nine additional countries have joined the survey. Wave 3 (2008/09) contains mostly retrospective data on sample members' lives, and is thus not used. Wave 6 instead contains few data on pension beliefs, because the question was asked only to the few new respondents, although we use it in several contexts

[8] Baker, Bloom, and Davis (2016) analyse the text content of media explicitly to measure policy uncertainty.

[9] The data are from SHARE waves 1, 2, 3 (SHARELIFE), 4, 5 and 6 (DOIs: 10.6103/SHARE.w1.600, 10.6103/SHARE.w2.600, 10.6103/SHARE.w3.600, 10.6103/SHARE.w4.600, 10.6103/SHARE.w5.600, 10.6103/SHARE.w6.600), see Börsch-Supan et al. (2013) for methodological details. In our work, we use the 6-0-0 release of the data. The SHARE data collection has been primarily funded by the European Commission through FP5 (QLK6-CT-2001-00360), FP6 (SHARE-I3: RII-CT-2006-062193, COMPARE: CIT5-CT-2005-028857, SHARELIFE: CIT4-CT-2006-028812) and FP7 (SHARE-PREP: N° 211909, SHARE-LEAP: N° 227822, SHARE M4: N° 261982). Additional funding from the German Ministry of Education and Research, the Max Planck Society for the Advancement of Science, the U.S. National Institute on Aging (U01_AG09740-13S2, P01_AG005842, P01_AG08291, P30_AG12815, R21_AG025169, Y1-AG-4553-01, IAG_BSR06-11, OGHA_04-064, HHSN271201300071C) and from various national funding sources is gratefully acknowledged (see www.share-project.org).

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.[10]

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.[11] Few individuals (slightly more than 3%) are observed for three or four waves, while 30% are observed twice.[12]

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 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 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.

2.2 Pension Reform Expectations

Expectations Data about Pension Reform and Policy Uncertainty — We use data on individual-specific pension reform beliefs from two questions that SHARE asks to all employed and self-employed individuals:[13]

(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 the government will reduce their pension

[10]In wave 4 (2011) the question was collected only for 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.

[11]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.1 in Online Appendix A.

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

[13]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). Section A1 in Online Appendix A 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.

benefits. The standard deviation 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 Figure A.2 in the Online Appendix). As is common with this elicitation format, responses tend to be rounded to the nearest 5 or 10% (Manski and Molinari 2010; Giustinelli, Manski, and Molinari 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 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%. 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%. Beliefs about pension reforms are also strongly positively correlated with individuals' expectations of working by age 63, which have increased over the sample period, in part reflecting changes in the macroeconomy. This is one of the features of the expectations data that we focus on next.

Pension Reform Expectations and Labor Market Decisions — Since beliefs are central to our analysis, it is useful to ascertain whether they are in fact related to individual labor market (expected and actual) decisions. Specifically, we assess how $B_{NRA,ijt}$ and $B_{PB,ijt}$, for individual i residing in country j at wave t, are associated with: [1] the individual-specific subjective probability of working full time after age 63 reported at wave t, $P_{ijt}(FT63)$, using a sample of employed and self-employed individuals aged 50–60 at the time of interview; [2] the subjective expected age at collection of public pension benefits reported at wave t, $ExpAge_{ijt}$, using a sample of employed and self-employed individuals who are entitled to receive a public pension and aged 50–64 at the time of interview; [3] an indicator for being employed in the subsequent wave $t+1$ (which in most cases is a couple of years after the current wave), $Emp_{ij,t+1}$; and [4] an indicator for being employed in a full-time job in $t+1$, $EmpFT_{ij,t+1}$. Specifically, we estimate the following:

$$Y_{ijt} = \phi_0 + \phi_1 B_{NRA,ijt} + \phi_2 B_{PB,ijt} + \mathbf{Q}'_{ijt}\Gamma + \mu_i + \xi_{jt} + u_{ijt}, \tag{1}$$

where $Y_{ijt} = \{P_{ijt}(FT63), ExpAge_{ijt}, Emp_{ij,t+1}, EmpFT_{ij,t+1}\}$, $\mathbf{Q}$ is a vector of controls (such as age, gender, and education), μ_i denotes individual fixed effects, ξ_{jt} are country $\times$ year-of-interview effects, and u_{ijt} is a random shock.[14]

Panel A of Table 2 reports the values of ϕ_1 and ϕ_2 for each of the first two outcomes covering expected labor market behavior. The correlation between beliefs about reforms, on the one hand, and expected behavior, on the other, is very strong. For instance, the top left entry indicates that moving from certainty of no reform of the national retirement age ($B_{NRA} = 0$) to certainty of reform ($B_{NRA} = 1$) leads to a 16 percentage point increase in the expectations of working full time after age 63. The third column shows that the same change in beliefs leads to an increase in the expected age at collection of public pension benefits of 0.58 years. The impacts from changes in B_{PB} are similar, albeit smaller. The estimates from fixed effects (FE) regressions are also smaller, but again highly statistically significant.

The bottom panel of Table 2 shows the values of ϕ_1 and ϕ_2 when assessing actual subsequent labor market choices. The correlation between beliefs and outcomes is invariably strong. Moving from $B_{NRA} = 0$ to $B_{NRA} = 1$ translates into a 9 percentage point increase in the probability of working or working full time in the subsequent wave (see the top row of the first and third columns). Likewise, when considering cuts to pension benefits, moving from $B_{PB} = 0$ to $B_{PB} = 1$ leads to a 3–4 percentage point increase in the probability of working full time in the next wave.[15]

Aside from these four outcomes, we investigated whether B_{NRA} and B_{PB} are related to a number of other domains. The results are summarized in Online Appendix A (Table A.5). We find that greater expectations of a reform of the pension system are significantly associated with a greater propensity to provide informal care to elderly parents and inter vivos financial transfers to young adult children (see, among others, Cox [1990], Becker [1991], and Rosenzweig and Wolpin [1994]). There is instead no evidence of effects on the likelihood of contributing to an occupational or private pension, the probability of providing care to grandchildren, or intended bequests.

Taken together all these results confirm recent empirical evidence in related and other life domains (e.g., Bottazzi, Jappelli, and Padula 2006; Guiso, Jappelli, and Padula 2013; Delavande and Kohler 2016; Delavande and Zafar, forthcoming; Armona, Fuster, and Zafar, 2019; Debets et al. 2018) and suggest that the beliefs we focus on have considerable economic relevance to older workers. They shape key expectations over future work and pension collection; and they shape observed decisions over actual future (full-time) employment, parental care, and intra-household financial transfers.

[14] We abstract from the possibility that labor market decisions later in life can be driven by the joint retirement behavior of couples (e.g., Blau 1998; Coile 2003; Gustman and Steinmeier 2004). Likewise, we do not consider joint retirement decisions affecting beliefs. This is an interesting area which we leave for future work. Moreover, recent research has emphasized that expectations data are likely to be subject to response (measurement) error which can lead to *downward* biased estimates in regressions like (1). See, for instance, Americks et al. (2018). In the main analysis of Section 5, however, this attenuation bias is not a concern since we use beliefs as our dependent (rather than independent) variable.

[15] Similar findings emerge with FE models. The same results hold for men and women separately, although the FE estimates for men are less precisely estimated in the case of an increase in B_{PB}. See Table A.4 in the Online Appendix.

2.3 Data on Reforms and Reform Announcements

We compile a comprehensive pan-European inventory of pension reforms. 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 increase retirement age and reforms that reduce pension benefits, and focus exclusively on policy reforms that are relevant and significant.[16] Across the 10 countries in the analysis, we end up with a total 46 main reforms (out of 65 counts of policy changes) over the sample period. The detailed inventory is available in the Online Appendix B, together with a comprehensive list of references and sources.

Figure 3 shows the timing of the reforms by type and country.[17] 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. This lower (or lack of) frequency may simply reflect greater institutional change in other periods, especially just before the start of the SHARE sample. In the empirical analysis, therefore, we account for such differences by cumulating past reforms over a number of years.

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 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 trades union (e.g., Germany in July 2006). When such an announcement has not yet been made, the reform is treated as unannounced. To emphasize, 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. This variation is important for identification.[18] In fact, we have few truly unannounced reforms in our panel. Yet, these are not necessarily marginal. For instance, the 2005 reform analyzed by De Grip, Lindeboom, and Montizaan

[16]Such data are corroborated with additional information collected from online sources about pension reforms in each country for every year from 1998 to 2015. 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 effect estimates that are likely to be downward biased.

[17]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 5.

[18]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 the 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.

(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 4, we use this variation in a difference-in-difference setup to identify the impact of announced versus unannounced pension reforms on beliefs.

2.4 Online Search

Our final measure of information acquisition is based on 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 and interests about pension reforms, which is expected to be salient to beliefs and behavior (Jun, Yoo, and Choi, 2018).[19] Clearly, unlike the announcement and enactment of reforms, this source is endogenous to individual attention.

We focus on monthly searches of the words "pension reform", translated in each country's language(s), from 1 January 2004 (the start of Google Trends) to 31 December 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 is 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 $\frac{100}{\max_{k\in K}(S_{jk})}S_{jm}$, where K is the time span in months over which online searches are considered. To make the index comparable to the expectations 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.

For the sample of workers in country j interviewed at time t (year and month), we define $G(0)_{jt}$ as the Google Trends index for the month in which the interview takes place.[20] To allow for information diffusion over a longer time period, we also consider $G(6)_{jt}$ as the mean of the index computed over the six months preceding the interview date. This latter measure may pick up greater effort in information collection than $G(0)$, but is also likely to be correlated to other online searches (with different keywords), which could reinforce or attenuate the information obtained on pension reform.

[19]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; Kotler and Keller 2008; Ginsberg et al. 2009; Vosen and Schmidt 2011; Da, Engelberg, and Pengjie 2011; Choi and Varian 2012; Preis, Moat, and Stanley 2013; Baker and Fradkin 2017; D'Amuri and Marcucci 2017).

[20]It is worth stressing that relating this aggregate measure of internet search data to our individual expectations data is novel, as most of the existing studies cited above use Google Trends indexes to forecast aggregate macro-variables such as changes in consumption, moves in the stock market, or labor market transitions.

The unconditional distributions of the contemporaneous measure, $G(0)$, and the 6-month mean index, $G(6)$, are displayed in Figure 5. Both measures are skewed toward zero, especially $G(0)$, as online search tends to be concentrated in specific times and is otherwise relatively limited.[21]

3. Theoretical Framework

In this section, we formulate a simple model of information processing and belief formation using the framework developed by Sims (1998, 2003) and, more recently, Matějka and McKay (2015). This provides us with a coherent setup to guide the empirical analysis and interpret our findings. The model focuses on the period *before* a reform, but it could be re-oriented to conceptualize the post-reform period too. Here, we provide an overview. The model is discussed in further detail in the Online Appendix C, where we also develop an example with numerical results.

Basic Setup

The model economy is characterized by N states of nature, ω_i, forming set $\Omega = \{\omega_1, ..., \omega_N\}$. These states of nature capture the presence, or not, of an impending reform. They also include the presence, or not, of formal reform announcements. Relating to the data discussion in Section 2, the states of nature therefore capture all relevant decisions made by the government. The state of the world is important to workers, because it determines the utility they obtain from their actions, such as saving and labor supply. Workers, however, do not observe the true state of nature but only noisy messages. Therefore, they condition their actions on the messages and not the state itself. Before any message is received, workers forms prior beliefs over states, $g(\omega)$.

For simplicity, we assume that each of the sets of messages and actions has N elements. We denote the set of messages by $\mathcal{M} = \{\widetilde{m}_1, ..., \widetilde{m}_N\}$, and the set of actions by $\mathcal{A} = \{a_1, ..., a_N\}$. Without loss of generality, we assume that, given state ω_i, the most likely message is $\widetilde{m}_i$. The worker's utility is equal to 1 if her action is correctly aligned with the state of the world, and 0 otherwise, i.e.,

$$U(a_i, \omega_j) = \begin{cases} 1 & \text{if } i = j \\ 0 & \text{if } i \neq j. \end{cases}$$

Thus, in the case of a single message, the worker takes action a_i following message $\widetilde{m}_i$. More generally, this specification implies that the worker gains greater ex-ante expected utility the higher the probability that the states of nature generate their associated messages.

Endogenous Information Gathering

Suppose the worker in fact receives two sequential messages from set $\mathcal{M}$. The initial message m_0 arrives with some exogenous and known probability $f_0(m_0|\omega)$. Given this initial message, prior beliefs and this information structure, the worker forms posterior beliefs about the state, given by $h_0(\omega|m_0)$.[22]

[21]Notice $G(6)$ is bounded away from 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 which 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.

[22]Notice $h_0(\omega|m_0) = f_0(m_0|\omega)\, g(\omega) / b(m_0)$, where $b(m_0) = \sum_\omega f_0(m_0|\omega)\, g(\omega)$.

We abstract from heterogeneity for now, and so workers are never differentiated by subscripts. As this section develops, however, we first introduce heterogeneity in messages, before discussing heterogeneity in other dimensions, such as updating rules.

The other message m_1 arrives after endogenous information gathering choices, summarized by $\varsigma \in [0,1]$, which depend on m_0. The message m_1, therefore, depends on the initial message m_0 through choice ς. However, conditional on state ω and choice ς, we assume that m_0 and m_1 are independent. Thus, m_1 arrives with probability $f_1(m_1|\omega, \varsigma(m_0))$. One may imagine that the initial message depends on ambient (or background) information flows that we do not measure in the data, such as information obtained from television, radio, and newspapers, while ς captures more focused information gathering which we partially observe, namely, online search.

In general, the worker chooses a final action based on both messages, m_0 and m_1, and the chosen level of internet search ς. This choice of search is our particular focus here. The worker chooses search to maximize expected utility, which is given as follows:

$$V(\varsigma|m_0) = \sum_{\omega \in \Omega} \left(\sum_{m_1 \in \mathcal{M}} U(a(m_0, m_1, \varsigma(m_0)), \omega) f_1(m_1|\omega, \varsigma) \right) h_0(\omega|m_0).$$

For convenience, and without loss of generality, we assume that message m_1 is more accurate than m_0, and that regularity conditions on the information structure ensure that the worker conditions her actions on the second message only.[23] Following the initial message m_0 and choice ς, expected utility is then given by:

$$V(\varsigma|m_0) = \sum_{i=1}^{N} f_1(\widetilde{m}_i|\omega = \widetilde{\omega}_i, \varsigma) h_0(\omega = \widetilde{\omega}_i|m_0).$$

Intuitively, given state ω, the worker gets higher payoff the higher is the probability of the associated message, $f_1(\widetilde{m}_i|\omega = \widetilde{\omega}_i, \varsigma)$. The worker's expected utility is then formed by summing over all states weighted by their posterior probability $h_0(\omega|\widetilde{m})$. We assume that $V_\varsigma > 0$ because search improves messages' accuracy, and so allows the worker to take appropriate actions more often. We further assume concavity in the payoff from search, i.e., $V_{\varsigma\varsigma} \leq 0$.

Search effort is costly to the worker (Caplin and Dean 2015; Hébert and Woodford 2018). Costs depend on the initial message and are increasing and convex in ς. The worker therefore chooses search effort following each initial message to maximize her expected utility net of costs. Optimal choices of search can then be characterized as a vector of size N, denoted by $\widehat{\varsigma} = \langle \varsigma_1, ..., \varsigma_N \rangle$.

Assuming the population has unit mass and messages sent to individuals are i.i.d. according to $f_0(m_0|\omega)$, we can sum over all possible messages to compute aggregate search as a function of the

[23]We assume that $f_1(\tilde{m}_i|\omega_i, \varsigma) \geq f_0(\tilde{m}_i|\omega_i), \forall \omega_i, \varsigma$. We also invoke additional regularity conditions to eliminate information structures in which, following sequential messages $\widetilde{m}_i$ and $\widetilde{m}_j$, with $i \neq j$, the worker infers that the state is most likely ω_k, with $k \neq i$ and $k \neq j$. The model can be generalized to incorporate these cases, but for exposition purposes we rule them out.

initial state:

$$G^*(\omega) = \sum_{i=1}^{N} \varsigma_i f_0(\widetilde{m}_i|\omega) \tag{2}$$

Finally, given observed messages m_0 and m_1, and given prior beliefs and the information structure determined in part by ς, the worker forms posterior beliefs about the underlying state $y(\omega|m_0, m_1, \varsigma)$.[24] As we describe next, due to data availability, our empirical work focuses on aggregate search, G^*, and individual posterior beliefs, y.

Interfacing the Model to the Empirical Analysis

In what follows, we describe how we map the above model into our empirical analysis. In the model, the worker does not observe the state of the world but observes private messages. In the data, instead, we do not observe the messages but observe the state of the world. We also partially observe individual posterior beliefs as well as aggregate, but not individual, search behavior. As mentioned, we interpret the states of nature not just as the presence or not of the reform, but also as official announcements and other government actions.

Specifically, we use data on workers' beliefs about pension reform and, in terms of states, we break down time periods according to their proximity to a reform. These include periods without an imminent reform, periods in which a reform is imminent and has been announced, and periods with an imminent reform that has not been announced. In keeping with the empirical framework presented below, we label these states D, I^A, and I^U, respectively. The presence of a reform is then captured by $I \equiv I^A \cup I^U$.

The main empirical analysis is based around regressions of individual expectations about these states of nature (and additional covariates). Of particular interest in our case is how announcements and online search interact in the formation of beliefs. In the model, aggregate online search is pinned down by the state entirely, and so including this search as a predictor induces no extra useful variation. However, we can extend the model to allow for additional orthogonal variation in online search by assuming the cost function is subject to aggregate shocks, η.

In Online Appendix C, we describe how a general form for the regression of beliefs on the reform state and observed search is given by:

$$\mathbb{E}_{m_0,m_1}\Big\{ y\big[I|m_0, m_1, \varsigma(m_0, \eta(\omega, G))\big]|\omega, G\Big\}, \tag{3}$$

where the conditional mean function, $\mathbb{E}(\cdot|\cdot)$, is taken over all messages, which we have assumed to be i.i.d. across the population. In the empirical analysis, the state of the world ω is captured by a categorical dummy variable. Thus, given ω, we empirically treat online search, G, as exogenous in the usual econometric sense.[25]

[24]Note y can be computed from f_0, f_1, h_0 and g using Bayes' rule and using the assumed independence of messages conditional on the underlying state and search choices. See the Online Appendix for more discussion.

[25]Note that, in our framework, the worker's information set consists of the messages she receives. When we move to the empirical analysis, we use the term 'information' less formally to cover the fundamental information sources we measure directly, principally formal reform announcements and aggregate online search.

Beliefs, Reforms, and Channels of Information

The model allows for rich patterns of belief formation and endogenous behavior. We shall discuss these in Section 5, grounding the analysis on a linear characterization of (3) and the regression implied by (2). In anticipation of that discussion, here we illustrate some of the model's main implications.

First, any form of coherent updating rules implies that reform expectations increase when reforms are impending (state I), as long as messages are not purely noisy and do embody some informational content. This condition is quite likely satisfied in our context, because individuals have access to two readily available sources of information: announcements and online search. In the language of the model, and abbreviating notation, this implication can be formally stated as $\mathbb{E}\left[y\left(I\right)|\omega=I\right] > \mathbb{E}\left[y\left(I\right)|\omega=D\right]$, where the conditional mean operator, $\mathbb{E}$, is taken over messages, as above.

Second, the volume of online search itself can vary substantially according to the underlying state. This is because of variation in search costs, which can be psychic. This variation is plausible: When pension reforms are formally announced (state I^A), more workers are primed for search upon receiving message $\widetilde{I^A}$. If their psychic costs of online search are low, then the average volume of online search is high. That is, $G^*\left(I^A\right) > G^*\left(I^U\right)$ and $G^*\left(I^A\right) > G^*\left(D\right)$.

Third, and putting these first two implications together, we anticipate that reform expectations are higher when the volume of online search is also higher.

Fourth, when reforms are impending but have not been formally announced (state I^U), online search costs may vary depending on, for example, the extent to which reforms are being discussed in the traditional media (e.g., television and newspapers). It is interesting to consider this case, particularly when the costs of online search are sufficiently high that we observe little online search activity. In this case, information is scarce and we predict beliefs not to increase much at all, that is, $\mathbb{E}\left[y\left(I\right)|\omega=I^U,G=0\right] \approx \mathbb{E}\left[y\left(I\right)|\omega=D,G=0\right].$

Fifth, the model allows for a given volume of search to have different *effectiveness* in each state. In particular, we might anticipate a given volume of search to change expectations most when reforms are impending but have not been announced. Put differently, the return to search is highest in the state I^U. Of course, this higher return is also consistent with a low volume of online search, because individuals make choices of search based not on the state, but their observed messages, and search costs may be relatively high after message $\widetilde{I^U}$. Overall, therefore, the model easily allows for online search to be more prevalent after an announcement, but to be more effective when announcements are absent, i.e., $\frac{\partial}{\partial G}\mathbb{E}\left[y\left(I\right)|\omega=I^U,\overline{G}\right] > \frac{\partial}{\partial G}\mathbb{E}\left[y\left(I\right)|\omega=I^A,\overline{G}\right]$, for relevant $G=\overline{G}$.

We will come back to these implications when we interpret the empirical results. As mentioned, the Online Appendix provides a concrete example which generates all these relationships.

Heterogeneity and Convergence

So far, we have allowed for heterogeneity in one dimension only: When shocks are i.i.d., individuals receive different messages and so have different information sets. However, we could extend the model to allow for heterogeneity more widely, for example by allowing for heterogeneous cost functions. These would induce different expected information quality across individuals as well as heterogeneous posteriors even for workers observing the same ex-post messages. Both this heterogeneous cost mechanism and the assumption of i.i.d. messages cause posterior beliefs to diverge from prior beliefs. And both of these mechanisms imply that at least some individuals face information costs that are non-negligible.

Alternatively, expectations would converge if workers have heterogeneous priors, but have homogeneous updating rules and the messages they receive are highly correlated. These latter two requirements themselves imply that information can be processed cheaply, because they typically imply that messages are highly correlated with the underlying state. Therefore, analyzing convergence itself has implications for the size of information costs. For instance, if expectations do not converge even after an announcement, then this information source is, on average, costly to process. This is exactly the reason why, in the empirical analysis, we test for convergence.

4. Empirical Strategy

Our aim is to assess how information flows affect beliefs in the proximity of a pension reform. In accordance with the previous 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 A3 of Online Appendix A we also discuss results using an alternative definition for reforms that are contiguous.[26]

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 $I^U(-s,-1)_{jt}$ 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 following specification:

$$\begin{aligned} y_{ijt} \; = \; & \rho_0 + \rho_{11} I^A(-s,-1)_{jt} + \rho_{12} I^U(-s,-1)_{jt} + \rho_2 I(0,s)_{jt} \\ & + \delta G(\tau)_{jt} + \psi_{11} I^A(-s,-1)_{jt} \times G(\tau)_{jt} + \psi_{12} I^U(-s,-1)_{jt} \times G(\tau)_{jt} \\ & + \psi_2 I(0,s)_{jt} \times G(\tau)_{jt} + \mathbf{X}'_{ijt}\Theta + \varphi_t + \lambda_j + \varepsilon_{ijt}, \end{aligned} \tag{4}$$

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 $B_{PB,ijt}$ to the chances that the government reduces pension benefits before i retires. $\mathbf{X}_{ijt}$ 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 effects, λ_j denotes country fixed effects, $\tau = \{0, 6\}$, and ε_{ijt} is a stochastic error component.[27] This specification can be considered the empirical analogue of

[26] 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.

[27] It is useful to reiterate that $G(6)$ is an average computed over the 6 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 6 months after the enactment of a reform,

expression (3). We estimate (4) using ordinary least squares (OLS), but to take account of the nature of the expectations variables that vary between 0 and 1, we also use the Papke-Wooldridge's (1996) fractional response estimator.[28] 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 the variation induced by the introduction of the 46 different reforms over time and across countries.[29] Figure 4 graphically illustrates the abundant variation across countries in $I^A(-12,-1)$, $I^U(-12,-1)$, and $I(0,12)$ over the sample period.[30]

Our baseline estimates are obtained from a simplified version of (4) 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, ρ_1 ($=\rho_{11}=\rho_{12}$), captures the total effect that includes both announcements and information flows from other sources. The model suggests 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 (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.

In another simplified version of (4) in which interactions are removed (so the ψ parameters are set to zero), responsiveness of beliefs to online search is captured purely by δ. Moreover, we assess whether online search strengthens or weakens the informational content of the reform process by including in (4) the ψ parameters. These capture the extent to which online search complements (or substitutes) announcements, and, for future reforms, the present reform itself. For instance, $\psi_{12} > 0$ would indicate that online search raises beliefs when a reform has not yet been announced, but is imminent.

In (4) workers' expectations are linked to reforms in the corresponding domain (either retirement age or pension benefits). In extensions, we also allow for each belief type to be affected by both sorts of reforms together. In other extensions, we estimate variants of (4) which include individual-specific fixed effects. Likewise, we analyze further specifications in which we allow for different s time lengths before and after the reform implementation as well as for cases in which the pre- and post-reform periods are captured asymmetrically.

$G(6)$ will cover only the post-reform period. If the date of interview is instead less than 6 months after the reform implementation, $G(6)$ will cover some of the pre-reform period as well as some of the post-reform period.

[28]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 Figure A.3) and do not comment on them here.

[29]Our identification strategy is similar to the difference-in-difference approach used by Autor (2003), Havnes and Mogstad (2011), and Kline (2012), who take advantage of temporal and spatial variation in, respectively, exceptions to the common law doctrine of employment at will across American states, child care coverage across Norwegian municipalities, and curfew laws across US cities. But unlike these other studies, in our application the introduction of a pension reform in a given country is generally followed by another reform in the same country, often over a short period of time.

[30]In some cases these indicators also vary within country and wave, since individuals are interviewed in different months. In the Online Appendix (Table A.2), we provide a breakdown of the sample along these other dimensions.

5. Main Results

5.1 Baseline Evidence from Reform Implementations

Table 3 presents least squares estimates for a version of (4) 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 $\delta = \psi_{11} = \psi_{12} = \psi_2 = 0$.

In panel A, we focus on the relationship between expectations and reforms six months before and six months after each reform, while we extend the length of time s to/from a reform to 12 months in Panel B. Our analysis reveals that the expectations of a reform that reduces PB (column iv) or increases NRA (column ii) rise by 8–9 percentage points, from a median of 50%, just before its implementation using either time frame. Individuals, therefore, adjust their beliefs in anticipation of 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). 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, the time that follows a reform has the same effect on expectation as the time that precedes it. However, the estimates in column (ii) reveal that NRA reforms work differently. In the twelve months after implementation, individuals *reduce* their expectations of further reform by 3 percentage points. If the time window after the introduction of the reform is instead restricted to six months, we find no significant change in beliefs. This asymmetry between PB and NRA reforms could be because 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 updating further indicates that individuals make important distinctions about the nature of the reform and their different underlying data generating processes.

Panel C of Table 3 confirms the previous results when we extend the time bandwidth to 18 months around each reform. Panel D shows that we find similar results if we use triangular kernel functions, PtR(before) and PtR(after) instead of the two indicator variables $I(-s, -1)$ and $I(0, s)$.[31] The results in Table 3 are also robust to a wide range of sensitivity checks presented in the Online Appendix.[32]

[31] PtR(after) is defined as taking value 1 in the month of the reform, declining linearly at a rate of 1/25 for every month after the nearest reform, and eventually taking value 0 more than 2 years after the nearest reform. PtR(before) is defined analogously for the period before the reform. These variables are defined in this way so to have the same integral over time as $I(-12, -1)$ and $I(0, 12)$.

[32] In Section A3 of Online Appendix A (Tables A.18–A.20), we conduct a series of other exercises, including (i) dropping one country at a time from estimation; (ii) using indicators of cumulated past reforms; (iii) adding country-specific timing and results of general elections as additional explanatory factors; (iv) changing sample selection to include individuals even in year-month cells in which there are fewer than 30 observations; and (v) defining the pre-reform and post-reform dummies not as mutually exclusive. The results from all such exercises are in line with the evidence found in the baseline analysis. We also analyze cross-country differences, isolating Italy and Spain from the other eight countries. We find little evidence of cross-country response heterogeneity pre-reform (see Table A.8). Post-reform, we detect no significant updating for both types of reform in the two southern countries, implying no updating asymmetry between PB and NRA

We augment the previous specification with a set of country-specific macroeconomic indicators, such as unemployment rate, GDP *per capita*, government debt as a percentage of GDP, government borrowing, and long-term government bond yields (see Online Appendix Table A.9). As expected, these indicators are strongly correlated to beliefs. But even after controlling for them, pension reforms themselves continue to be a powerful predictor of expectations both before and after their implementation, suggesting that information about reforms is important for belief updating over and above the knowledge of the state of the economy.[33]

We also investigate whether both types of reform together are relevant to each belief domain. To account for this possibility, we re-estimate our simplified specification and use pre- and post-reform indicators for both reforms jointly to predict each belief separately. The results are shown in Online Appendix Table A.10. Both types of reform are relevant in shaping up expectations about retirement age. Individuals have greater expectations of an increase in the retirement age 12 months before the introduction of either reform. In the 12 months afterwards they reduce their beliefs following NRA reforms, while they increase further their beliefs following reforms to PB. It seems, therefore, that people use PB reforms as a signal about a future extension in the retirement age. In the case of expectations about benefit reductions, instead, we find that workers continue to revise up their expectations within 12 months after PB reforms are introduced, but not after NRA reforms. Individuals perceive NRA reforms as being more 'final', while PB reforms indicate continued policy revisions in the future.

The longitudinal nature of the SHARE data allows us to estimate individual fixed effects (FE) models controlling for time-invariant individual-specific characteristics. Here, however, attrition due to ill health or death reduces the estimation subsample to half the size of the baseline sample. 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 (Online Appendix Table A.12).

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 Online Appendix Table A.13. Focusing 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

reforms in Italy and Spain. Although these are interesting results, they do not represent the focus of our present work and their analysis therefore is deferred to future research.

[33] The only exception emerges in the case of beliefs that the government reduces pension benefits, for which we find that reforms no longer have a significant impact on beliefs over the 12 months following their introduction.

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). No similar evidence however emerges for NRA reforms.

5.2 Evidence from 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 an information shock that is followed by a period of high media coverage, making information less costly to acquire. The results are summarized in Table 4, 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.[34] We show results for three different time windows s, up to 6, 12, and 18 months before the reform, in panels A, B, and C, respectively.

A striking result emerges unambiguously: The impact of *announced* reforms on beliefs is nearly identical to the impact of *unannounced* reforms. For instance, looking at twelve-month spans pre-reform, 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 true also when we look at NRA reforms (column (ii)) and whether we consider a pre-reform window of six or 18 months.[35]

These findings are remarkable. 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 point on average, from a mean and median beliefs close to 50%. This suggests either substantial inattention to these announcements, or difficulty in processing this information. 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 labor unions, or opposition parties that could lead to a policy reversal. On the other hand, the results show that, in case of no announcement, 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 acquisition that may follow it. We shall come back to the implications of this point in terms of expectations formation in subsection 5.4.

5.3 Online Search

We now turn to the estimates on the Google Trends index. To re-emphasize the discussion in subsection 2.4, online search activities, which are highly correlated to public opinion and social sentiments on a wide variety of issues, proxy information-seeking behavior and the aggregate demand for information related to pension reform. Online search activities are also likely to be correlated with information sup-

[34]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.

[35]Although in the case of NRA reforms for the six month window, ρ_{12} is smaller in magnitude than ρ_{11}, the difference is not statistically significant from zero (p-value=0.223). The estimates in Table 4 are robust to excluding all individuals from Switzerland, for which the definition of announcement is complicated by the presence of referenda after law approvals.

ply available on social media, which in turn affects expectations over and above reform announcements and implementations.

We start with another simplified version of (4) in which we do not explicitly consider the timing of reforms or announcements themselves, i.e., $\rho_{11} = \rho_{12} = \rho_2 = \psi_{11} = \psi_{12} = \psi_2 = 0$. Table 5 reports the estimates by belief domain of δ found using $G(0)$ and $G(6)$ in panels A and B, respectively. The results reveal that periods of more intense online search are associated with significantly greater expectations of reform. This link is strong. From the estimates on $G(6)$, a one-standard deviation 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}.

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 (4) as before where we also include the 6-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. The results are shown in panel C of Table 5. 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. More importantly, the magnitude of δ on $G(6)$ declines only marginally, but remains large and statistically significant. Taken together, a one-standard deviation increase in monthly searches for both "pension reform" and "austerity" leads to 6.1 and 8.3 percentage point updates in B_{NRA} and B_{PB}, respectively. These imply 60 and 25% increases over the updates found earlier.[36]

It is possible that online search in other domains be also associated with our two expectations measures. For this reason, we again repeat the analysis but this time, beside $G(6)$, we include $G^{oth}(6)$, the 6-month pre-interview mean of the Google Trends index for the keyword "reform", thus excluding the word "pension". This exercise is performed in the spirit of a placebo test. The estimates in panel D of Table 5 show that the effects of $G(6)$ are close to those reported in panel B. The $G^{oth}(6)$ estimates, however, are 4 to 7 times smaller, and either statistically insignificant (as in the case of B_{NRA}, column (ii)) or significant only at the 10 percent level (as in the case of B_{PB}). Related online search activities therefore are largely uncorrelated to pension reform expectations, and although placebo tests cannot be definitive, these results provide support to the strong predictive power of our online search measure.

5.4 The Joint Effect of Reforms, Announcements, and Online Search

The introduction of a reform, its announcement (or lack of it), and the acquisition of information proxied through internet search have been shown each to affect belief formation separately. In what follows we analyze how all such channels work as posited in (4) and analyze their joint effects.

Before discussing these joint effects, we investigate how online search itself evolve in the period leading up to a reform to get a better understanding of endogenous information acquisition. We do

[36] Although we view online search as a proxy for various information sources, this result echoes the findings by Belot, Kircher, and Muller (2019) according to which broadening salient online search activities of individuals (unemployed job seekers in their case) is associated with more favorable labor market outcomes. In our case, broader search may provide more precise or wide-ranging information, leading workers to revise their expectations more than otherwise.

this by estimating regressions of the $G(\tau)$ indices on the indicator functions for pre- and post-reform periods, controlling for a standard set of covariates as well as time and country fixed effects. These regressions can be considered the empirical analogue of equation (2). The results for $G(0)$ and G(6) are summarized in Table 6.

We find that $G(0)$ is not correlated with any of the reform indicators, possibly because most of the internet activities have already happened before the month of interview. For $G(6)$, instead, we detect a positive and significant correlation between announcement and online search, suggesting that the reform announcement itself stimulates greater information gathering. The estimates imply an average 71 and 94% increase in internet search for announcements of NRA and PB reforms, respectively. There is instead no evidence of increased search if reforms are currently unannounced. Post-reform, online search increases after pension benefits are cut, but not after an increase in retirement age, indicating that pension cuts alone generate more speculation and information search. A simple event-study analysis, reported in the Online Appendix (Figure A.4), confirms both findings: Pre-reform online search is high when a reform is announced, while it is lower if a reform has not yet been announced, although it intensifies in the two months preceding the reform enactment.

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 (4), with panel A having 6-month pre- and post-reform time windows and panel B 12-month windows. In all cases, we include the Google Trends index computed over the six months preceding the interview date.

The results in column (i) and (iii) show that all the proxies for information we use leading up to a reform are positively associated with expectations, although the coefficient associated with announced reforms is less precisely estimated now that internet search activity is accounted 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 in panel A. At low levels of internet search, announcements raise beliefs upwards ($\rho_{11} > 0$), but unannounced reforms instead do not affect beliefs ($\rho_{12} \approx 0$). This finding tallies with the fourth model implication discussed in Section 3: With no announcements and no online search, workers have no information with which to increase 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.

We show this feature in a slightly different way in Figure 6, where we plot the effects of switching on $I^A(-6,-1)$, $I^U(-6,-1)$ and $I(0,6)$ across the entire $G(6)$ distribution. The profile for $I^A(-6,-1)$, 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, combined with those from Table 6, are consistent with the fifth model implication discussed in Section 3: When reforms are imminent but have not yet been announced, online search is powerful in revealing the true state of the world. But when reforms have been announced, online search, although more frequent in quantity, is redundant in terms of information content about the probability of reform itself — although it might change beliefs about reform details, for which we have no evidence (and no data).

Turning to the six months after reform implementations, the change in expectations, measured by ρ_2, is the same as that found in Table 3. The introduction of an NRA reform has no impact on beliefs that there will be a subsequent NRA reform (column (ii)). But beliefs about PB reforms are revised up by nearly 9 percentage points (column (iv)), a result that is consistent with persistent uncertainty. Increasing search intensity post-reform further increases the expectations of a reform in the case of pension benefit cuts, but not in the case of reforms to retirement age. That is $\delta + \psi_2$ is greater than zero for benefit cuts, but insignificantly different from zero for retirement age.

Extending the time bandwidth to twelve months pre- and post-reform confirms most of the previous results (see columns (ii) and (iv) in panel B). Some nuanced differences emerge, however, and are worth stressing. Across both policy domains, unannounced reforms now have a stronger effect on beliefs, both at low levels of internet search ($\rho_{12} > 0$) and, as indicated by the high coefficient on ψ_{12}, when internet search is intense. These results suggest that the longer run-up time to reform picks up more predictive information flows. The post-reform effects, ρ_2, show a noticeable response asymmetry between reform types: The implementation of PB reforms leads to an increase in B_{PB} by 10 percentage points at low levels of internet search, while the introduction of NRA reforms induces a reduction in B_{NRA} of 4 percentage points. However, intense post-reform online search activities now raise the expectations of subsequent NRA reforms, i.e., $\delta + \psi_2 > 0$. We carried out several sensitivity checks, such as including macroeconomic indicators into the analysis, estimating fixed effects models, and adding the G^a index. The estimates, shown in Tables A.14–A.16 of the Online Appendix, are always similar to those presented in Table 7.

5.5 Taking Stock of the Results So Far

The information rigidities we uncover could be driven by many factors. Various models of belief formation about macroeconomic variables have been put forward. These include models with exogenously fixed levels of noise in which individuals use noisy signals to update continuously, as well as sticky information models in which individuals update infrequently due to fixed costs associated with acquiring and/or processing information.

Our estimates on online search and belief updating reveal clear information rigidities in the context of policy uncertainty, and indicate important nuances between information gathering, information content, and revision of expectations. First, the fact that policy announcements, which presumably make information acquisition cheaper, generate more online search suggests that individuals are able to control the precision of the information they receive, subject to cost, and that models with fixed levels of noise do not capture important complexities of the information gathering process.

Second, the fact that the revision to reform expectations is similar with or without announcements implies that individuals engage in a rather continuous process of belief updating (Dominitz and Manski 2011). A model in which individuals only update infrequently when there is an information shock (e.g., a reform announcement) or when information is cheaper or more valuable to acquire (e.g., the period following an announcement) would instead imply a relatively larger effect on beliefs following announced reforms. This finding provides evidence against a sticky information approach.

Third, the fact that the revision to expectations is similar with or without announcements further shows that more endogenous information gathering need not be accompanied by more belief updating. In fact, in our case, the additional information coming from online search following an announcement seems to be redundant in terms of the expectations measured here. Indeed, Table 7 provides evidence that announcements and online search are substitutes in the formation of expectations. This evidence indicates high variability of the effect of information gathering on expectations formation (Coibion and Gorodnichenko 2015; Fuster et al. 2019).

6. Convergence and Heterogeneity

6.1. Do Pension Reform Expectations Converge?

If individuals revise their expectations up in the few months preceding, and sometimes following, an actual reform, it is conceivable to surmise some sort of belief convergence over time in the population. In a canonical Bayesian setting with widespread public information and frictionless information availability, we expect *all* workers to revise up their beliefs about the occurrence of reform to similar levels (and possibly close to 1) when the reform is imminent. However, as discussed in Section 3, belief divergence is entirely possible in the presence of frictions and costly information acquisition.

Here we investigate the extent of expectations convergence in the data. Specifically, we measure belief dispersion by computing the squared residuals from earlier regressions, namely, the two regressions reported in panel B of Table 3 (in which we look at the time before/after reforms over a 12-month period), and the two regressions reported in panel B of Table 4 (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 main estimates of this analysis are summarized in Table 8. 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, indicating more heterogeneous updating, possibly driven by the increased information search that follows a reform announcement. Making information cheaper, therefore, does not decrease the cross-sectional dispersion of expectations (Fuster et al. 2019), possibly because workers collect heterogenous pieces of information. Individual expectations also become more dispersed in the 12 months after the implementation of the same reforms. This suggests again that the process of expectations revision is continuous, in contrast to the notion of sticky information which would predict more convergence when information is cheaper to acquire. On the other hand, the estimates in column (ii), which refer to PB reforms, indicate neither increased dispersion nor convergence. In summary, we find no evidence of belief convergence.

6.2. Heterogeneity in Expectations Revisions by Characteristics

To gain a better understanding of what types of heterogeneity in expectations formation drive the lack of convergence, we assess how the revision process varies according to a range of observable characteristics. We look at five cues: (i) education; (ii) numeracy; (iii) probabilistic thinking; (iv) age; and (v) gender. The first three cues focus on a set of individuals with presumably more cognitive skills and who can therefore more easily process complex information.[37] Older individuals may find information about reform more valuable, for they are closer to retirement and hence have less time to adjust their behavior 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, Fuster, and Zafar, 2019).

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, which is meant to pick up aspects of human capital not otherwise captured by formal educational attainment, is based on the score to a series of questions about numerical cognitive skills averaged across all available waves in the selected sample (with higher scores indicating greater numeracy skills). Following Lillard and Willis (2001), probabilistic thinking is an index given by the propensity each individual has in providing answers of "0" , "50:50" , or "100" over all available waves to two survey questions: "What do you think the chances are that it will be sunny tomorrow?" and "Thinking about the next ten years, what are the chances that you will receive any inheritance, including property and other valuables?" The idea is that heaping of responses at such focal answers is associated with respondents' ambiguity or uncertainty about true probabilities and correspondingly lower sophistication in probabilistic thinking.

We performed several exercises re-estimating simplified versions of model (4) and interacting the main explanatory variables separately with each of the cues we are interested in.[38] The key results are summarized in Table 9, which only reports the estimates found on each characteristic we consider (labelled 'Cue(k)', with k being either university degree, numeracy, age, probabilistic thinking, or female) and on the relevant interaction terms.[39] Panel A, which focuses on the interaction of reforms

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

[38] 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 3) and if they use 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 five cues we analyze here and the same covariates. The results are in the Online Appendix (Table A.17). Highly educated workers and workers with high cognitive skills are likely to read and to use internet. Conversely, individuals who have low probabilistic sophistication read less and use internet less. These features tie in well with the results below.

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

and announcements, is derived from the following regression:

$$\begin{aligned} y_{ijt} \;=\; & \theta_0 + \theta_{11} I^A(-12,-1)_{jt} + \theta_{12} I^U(-12,-1)_{jt} + \theta_2 I(0,12)_{jt} + \gamma_1 \text{Cue}(k)_{ijt} \\ & + \gamma_{11} I^A(-12,-1)_{jt} \times \text{Cue}(k)_{ijt} + \gamma_{12} I^U(-12,-1)_{jt} \times \text{Cue}(k)_{ijt} \\ & + \gamma_2 I(0,12)_{jt} \times \text{Cue}(k)_{ijt} + \mathbf{X}'_{ijt}\Lambda + \varphi_t + \lambda_j + \nu_{ijt}, \end{aligned} \tag{5}$$

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

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

where most of the terms in (5) and (6) are the same as those defined in Section 4. The θ_{11}, θ_{12}, θ_2 and α_1 estimates for all five 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 process more easily accessible information (Cavallo, Cruces, and Perez-Truglia 2014; Fuster et al. 2019). 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 expectations revisions when reforms are unannounced are likely to be driven by costly endogenous information acquisition. 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)).

Individuals with lower sophistication in probabilistic thinking tend to have lower expectations than their counterparts following both announced and unannounced reforms (both $\gamma_{11} < 0$ and $\gamma_{12} < 0$, column (iv)). Plausibly, they search less intensively for new information and process it less accurately. Female workers are also less likely to update but only when reforms are unannounced ($\gamma_{12} < 0$, albeit statistically significant at the 10% level, column (v)), which suggests 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 workers with poorer sophistication in probabilistic thinking and women are no more likely to update beliefs.

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

7. Conclusion

Welfare Implications — Before we summarize our main contributions, we provide a back-of-the-envelope calculation of the welfare implications of the expectations revisions we estimate. This will give a concrete idea of the consequences of some of our findings. Consider $\rho_1 + \rho_2$ in panel B of Table 3. This suggests belief updates from a mean of 0.51 to 0.58 and from a mean of 0.53 to 0.69 in the case of NRA and PB reforms, respectively, with a combined 24-month window around reforms.

We use the results found by Luttmer and Samwick (2018), according to which people are willing to forgo 6% of their future benefits in order to remove the policy uncertainty associated with their entitlements. We also take net pension wealth as a measure of the stock of future discounted flows of pension benefits after taxes and social contributions, with the official OECD estimate of net pension wealth averaged over the 10 countries in our sample being 13.1 times their net earnings (OECD 2017). With annual individual earnings averaging €50,200 (in 2005 prices) over the sample period, net pension wealth per capita is approximately €660,000 (13.1 $\times$ €50,200). So the per capita welfare cost is 6% of €660,000, which equals about €40,000.

Now, suppose uncertainty is fully resolved at 0 or 1 for the individuals aged 50–64 in our sample. The anticipation and enactment of reforms eliminate only 2.5% and 14% of baseline uncertainty in the case of NRA and PB reforms, respectively.[40] This means that, on average, each of the 10 European governments could cut per capita future pension benefits by approximately €39,000 (i.e., (1–0.025)$\times$40,000) or €34,000 (i.e., (1–0.14)$\times$40,000) in present value terms without making workers worse off using either NRA or PB reforms respectively, if it somehow could remove all policy uncertainty surrounding future benefits. In the aggregate, with about 37.5 million people aged 50–64 across the 10 countries in the sample, these figures translate into €1.5 trillion and €1.3 trillion, respectively. The upshot is that removing pension reform uncertainty will lead to huge welfare improvements across Europe.

Final Remarks — 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 online search. We identify the impact of the information acquisition on beliefs taking advantage of the rich variation in the timing of 46 reforms across 10 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. Periods that follow reform implementations also lead to significant changes in beliefs. After a reform that increases age at retirement, workers expect another similar reform to be less likely in the future, while after a reform that reduces pension benefits they expect another similar reform to be more likely. This asymmetric response could reflect

[40] These are the percentage declines in the variance of the Bernoulli random variable for NRA and the PB reforms, respectively. The probability of 'success' for the NRA reform variable goes from 0.51 to 0.58, while, for the PB reform, the probability goes from 0.53 to 0.69.

workers' beliefs that governments find it easier to reduce benefits again than to further extend age at retirement. The periods surrounding reforms also brim with online search that leads to belief updating. We find that online search becomes more intense after announcements. 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. And, for the 10 countries in our analysis, the welfare cost of pension reform uncertainty is estimated to run into trillions of Euros.

Most of these findings are hard to reconcile with canonical frictionless Bayesian updating. Instead, they can be rationalized in a simple model of attention where workers 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.

Our findings indicate several avenues for further research and data collection. 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. Another avenue is to analyze additional sources of informative signals other than reforms and announcements, such as industry expert forecasts and pension advisors' reports, which may or may not align with the way in which workers update their expectations. This in turn suggests the importance of collecting new survey data on a wider range of information sources which individuals may pay attention to, as well as heuristics they might use, in forming their beliefs about public policy across several domains.

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Table 1: Summary Statistics for the main sample

Variable	Mean	S.D.	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 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	27881
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
$G(0)$	0.09	0.17	0.01	0.00	1.00	24,623
$G(6)$	0.08	0.13	0.03	0.00	0.68	24,394
Numeracy	0.00	0.95	0.18	-2.82	1.18	26,791
Probabilistic thinking	0.24	0.25	0.25	0.00	1.00	27,851

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. 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; 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; $I(0,12)$ is an indicator for the individual being within 12 months after a reform. $G(0)$ is the *Google Trends* index of search intensity about the keyword "pension reform" in the month of interview; $G(6)$ is the averaged over the semester ending in the month of the interview. 'Numeracy' is based on the score to a series of questions about numerical cognitive skills averaged across all available waves in the selected sample (the variable has been demeaned). 'Probabilistic thinking' is an index given by the propensity each individual has to give focal answers of "0", "50:50", or "100" over all available waves in the selected sample to two survey questions on the chances of being sunny the following day and the chances of leaving an inheritance.

Table 2: The Relationship of Beliefs with Expected and Observed Labor Market Behavior

		OLS	FE	OLS	FE
Panel A [Beliefs → Expected behavior]					
		$P_{ijt}(FT63)$		$ExpAge_{ijt}$	
B_{NRA}	(ϕ_1)	0.162***	0.120***	0.579***	0.178***
		(0.009)	(0.017)	(0.052)	(0.076)
B_{PB}	(ϕ_2)	0.086***	0.073***	0.429***	0.213***
		(0.009)	(0.019)	(0.053)	(0.080)
N		21,129	7,986	23,880	11,096
Panel B [Beliefs → Observed decisions]					
		$\Pr(Emp_{ij,t+1})$		$\Pr(EmpFT_{ij,t+1})$	
B_{NRA}	(ϕ_1)	0.091***	0.061***	0.086***	0.062***
		(0.009)	(0.015)	(0.010)	(0.017)
B_{PB}	(ϕ_2)	0.030***	0.034**	0.044***	0.032*
		(0.009)	(0.015)	(0.010)	(0.018)
N		20,380	11,061	16,325	8,394

Notes: 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. Standard errors (reported in parentheses) are clustered at the household level. Expected age at collection of benefits, $ExpAge_{ijt}$, refers to public pensions (if the country j questionnaire allows for two different codes, it is the minimum between the ordinary and the anticipated pension benefit collection age). Regressions in all panels include a constant, log of household income and log of net wealth (both expressed in 2005 Euros, and converted into Euros when needed using PPP exchange rates), household size, age, and indicator variables for sex, education, employment sector, self-employed status, part time employment status, citizenship, self-reported health, marital status, country × year of interview fixed effects. In panel B we also include age, self-reported health, and household size observed in the subsequent wave of data. In all FE regressions, we include only individuals with at least 2 observations, standard errors are clustered at the level of first-wave household, and time-invariant controls are dropped. and we drop time-invariant controls. Estimates in both panels are obtained from a sample of employed and self-employed individuals aged 50 to 64 (50–60 in the first two columns of panel A) and observed in the SHARE waves 2, 4–6 (or 1–2, 4–6 in the last two columns of panel A). N is number of person-wave observations.

* Significant at 10%; ** significant at 5%; *** significant at 1%.

Table 3: Beliefs and Reforms

		(i) Sample mean	(ii) B_{NRA}	(iii) Sample mean	(iv) B_{PB}
Panel A					
$I(-6,-1)$	(ρ_1)	0.145	0.078***	0.113	0.085***
			(0.022)		(0.024)
$I(0,6)$	(ρ_2)	0.054	–0.012	0.046	0.093***
			(0.026)		(0.033)
Panel B					
$I(-12,-1)$	(ρ_1)	0.249	0.096***	0.199	0.085***
			(0.016)		(0.017)
$I(0,12)$	(ρ_2)	0.135	–0.030**	0.061	0.078***
			(0.013)		(0.028)
Panel C					
$I(-18,-1)$	(ρ_1)	0.267	0.101***	0.209	0.097***
			(0.017)		(0.018)
$I(0,18)$	(ρ_2)	0.272	–0.012	0.170	0.073***
			(0.013)		(0.017)
Panel D					
PtR(before)	(ρ_1)	0.200	0.133***	0.159	0.134***
			(0.023)		(0.023)
PtR(after)	(ρ_2)	0.164	–0.024	0.107	0.110***
			(0.023)		(0.029)
Mean of dep. var.			0.514		0.535
N			27,881		27,881

Notes: 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. $I^(- s, -1)$ is equal to 1 over the s months before a reform, and 0 otherwise; $I(0, s)$ is equal to 1 over the s months following a reform (and in the month of the reform), and 0 otherwise The definition of the $I(.,.)$ variables depends on the margin of reform analyzed, so they refer to reform increasing retirement age in the B_{NRA} case and to reform cutting benefits in the B_{PB} case. The table reports least squares estimates and, in parentheses, the standard errors are clustered at the country $\times$ month-year of interview level (237 groups), obtained from a sample of employed and self-employed individuals aged 50 to 64 and observed in the SHARE waves 1, 2, 4, and 5. All regressions include a constant, log of household income and log of net wealth, household size, age, and indicator variables for sex, education, employment sector, self-employed status, part time employment status, citizenship, self-reported health, marital status, country fixed effects, and time (month and year of interview) fixed effects. All cases for which the total number of month-year observations is fewer than 30 are excluded. N is number of person-wave observations.

** Significant at 5%; *** significant at 1%.

Table 4: Beliefs and Reform Announcements

		(i) Sample Mean	(ii) B_{NRA}	(iii) Sample Mean	(iv) B_{PB}
Panel A					
$I^A(-6,-1)$	(ρ_{11})	0.045	0.110***	0.036	0.091**
			(0.040)		(0.040)
$I^U(-6,-1)$	(ρ_{12})	0.100	0.056**	0.077	0.080***
			(0.023)		(0.023)
Panel B					
$I^A(-12,-1)$	(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 C					
$I^A(-18,-1)$	(ρ_{11})	0.055	0.118***	0.046	0.084**
			(0.037)		(0.036)
$I^U(-18,-1)$	(ρ_{12})	0.212	0.093***	0.163	0.101***
			(0.018)		(0.019)

Notes: Number of person-wave observations, mean of the dependent variables and included additional controls are the same as in Table 3. $I^A(-s,-1)$ is equal to 1 if the month of interview is within s and 1 months before a reform and the reform had already been announced, and 0 otherwise; $I^U(-s,-1)$ is equal to 1 if the month of interview is within s and 1 months before a reform and the reform had not been announced yet, and zero otherwise; $I(0,s)$ is equal to 1 over the s months following a reform (including the month of the reform), and 0 otherwise. For all the other details, see the notes to Table 3.

** Significant at 5%; *** significant at 1%.

Table 5: Beliefs and Online Search

		(i) Sample mean	(ii) B_{NRA}	(iii) B_{PB}
Panel A [N=24,623]				
$G(0)$	(δ)	0.088	0.129**	0.165**
			(0.056)	(0.071)
Panel B [N=24,394]				
$G(6)$	(δ)	0.081	0.306***	0.524***
			(0.068)	(0.063)
Panel C [N=24,394]				
$G(6)$	(δ)	0.081	0.227***	0.466***
			(0.066)	(0.061)
$G^a(6)$		0.197	0.264***	0.195***
			(0.074)	(0.066)
Panel D [N=24,394]				
$G(6)$	(δ)	0.081	0.323***	0.482***
			(0.072)	(0.063)
$G^{oth}(6)$		0.333	–0.046	0.115*
			(0.060)	(0.059)

Notes: N = number of person-wave observations. $G(0)$ and $G(6)$ are the Google Trends indexes on the intensity of online search about the keywords "pension reform" averaged over the month of interview, the 6-month and 12-month periods prior to interview, respectively. They are all measured on a scale from 0 to 1. $G^a(6)$ refers to the Google Trends index on the intensity of online search about the keyword "austerity". See Section A4 of the Online Appendix for further details. $G^{oth}(6)$ refers to the Google Trends index on the intensity of online search about the keyword "reform", excluding the word "pension". The sample is the same as in Table 3, but Sweden is not included in this analysis, as there are not enough Google Trends data available. In Panels B–D, the sample size shrinks with respect to panel A because the Google Trends index is available since January 2004, hence we lose observations when we calculate the average over previous months. For all the other details, including the full list of additional controls, see the notes to Table 3.

* Significant at 10%; ** significant at 5%; *** significant at 1%.

Table 6: The Relationship between Online Search and Reforms

	NRA		PB	
	$G(0)$	$G(6)$	$G(0)$	$G(6)$
$I^A(-12,-1)$	0.025	0.057**	0.005	0.076***
	(0.038)	(0.027)	(0.045)	(0.028)
$I^U(-12,-1)$	0.047	0.008	0.034	0.020
	(0.034)	(0.020)	(0.037)	(0.018)
$I(0,12)$	0.010	0.008	0.014	0.141***
	(0.032)	(0.017)	(0.082)	(0.052)
N	24,623	24,394	24,623	24,394

Notes: The table reports least squares estimates and, in parentheses, standard errors, which are clustered at the country × month-year of interview level. NRA refers to reforms in which the government raises the national retirement age; PB refers to the reforms in which the government reduces pension benefits. The sample is the same as in Table 5, panel A. For all the other details, included the full list of additional controls, see the notes to Table 3-5.

** Significant at 5%; *** significant at 1%.

Table 7: Beliefs, Reforms, Announcements, and Online Search

		B_{NRA}		B_{PB}	
		(i)	(ii)	(iii)	(iv)
Panel A					
$I^A(-6,-1)$	(ρ_{11})	0.076*	0.208***	0.009	0.130**
		(0.040)	(0.067)	(0.043)	(0.052)
$I^U(-6,-1)$	(ρ_{12})	0.045**	0.001	0.046**	0.013
		(0.022)	(0.022)	(0.019)	(0.020)
$I(0,6)$	(ρ_2)	–0.019	0.020	–0.006	0.085**
		(0.025)	(0.033)	(0.033)	(0.036)
$G(6)$	(δ)	0.268**	0.329***	0.523***	0.735***
		(0.066)	(0.063)	(0.075)	(0.151)
$I^A(-6,-1) \times G(6)$	(ψ_{11})		–0.652**		–0.673***
		(0.262)		(0.221)	
$I^U(-6,-1) \times G(6)$	(ψ_{12})		0.491***		0.312
			(0.182)		(0.207)
$I(0,6) \times G(6)$	(ψ_2)		–0.295**		–0.362**
			(0.142)		(0.172)
Panel B					
$I^A(-12,-1)$	(ρ_{11})	0.080**	0.136**	0.030	0.097**
		(0.036)	(0.061)	(0.034)	(0.042)
$I^U(-12,-1)$	(ρ_{12})	0.089***	0.036**	0.079***	0.040***
		(0.014)	(0.017)	(0.012)	(0.013)
$I(0,12)$	(ρ_2)	-0.038***	–0.039***	0.044*	0.103***
		(0.013)	(0.013)	(0.024)	(0.024)
$G(6)$	(δ)	0.281***	0.231***	0.489***	0.639***
		(0.054)	(0.048)	(0.060)	(0.134)
$I^A(-12,-1) \times G(6)$	(ψ_{11})		–0.193		–0.345*
			(0.223)		(0.188)
$I^U(-12,-1) \times G(6)$	(ψ_{12})		0.810***		0.575***
			(0.246)		(0.216)
$I(0,12) \times G(6)$	(ψ_2)		–0.035		–0.296**
			(0.115)		(0.133)

Notes: The sample is the same as Table 5, panel B; the number of person-wave observations is equal to 24,394 in all regressions. For all the other details, included the full list of additional controls, see the notes to Tables 3 and 5.

* Significant at 10%; ** significant at 5%; *** significant at 1%.

Table 8: Belief Dispersion in the Proximity of Reforms and Announcements

	$SR(B_{NRA})$	$SR(B_{PB})$
Panel A		
$I(-12,-1)$	0.009**	–0.000
	(0.004)	(0.003)
$I(0,12)$	0.017***	–0.009
	(0.003)	(0.005)
Panel B		
$I^A(-12,-1)$	0.026***	0.005
	(0.007)	(0.006)
$I^U(-12,-1)$	0.003	–0.002
	(0.003)	(0.003)
$I(0,12)$	0.018***	–0.009
	(0.003)	(0.005)

Notes: $SR(z)$ denotes the squared residuals for $z = \{B_{NRA}, B_{PB}\}$ obtained from the two regressions whose results are reported in panel B of Table 3 (panel A above) and from the two regressions whose results are in panel A of Table 4 (panel B above). The number of person-wave observations in all regressions is 27,881. For all the other details, included the full list of additional controls, see the notes to Table 3.

** Significant at 5%; *** significant at 1%.

Table 9: Belief Heterogeneity Around Reforms

	University degree (i)	Numeracy (ii)	Age–50 (iii)	Probabilistic thinking (iv)	Female (v)
Panel A [Reforms and announcements]					
B_{NRA}					
Cue(k)	0.008	0.003	–0.034***	–0.001	0.011*
	(0.010)	(0.003)	(0.001)	(0.013)	(0.006)
Cue(k)$\times I^A(-12,-1)$	0.085***	0.018	0.007	–0.116	0.002
	(0.029)	(0.016)	(0.006)	(0.072)	(0.017)
Cue(k)$\times I^U(-12,-1)$	0.011	–0.008	0.010***	–0.062**	–0.022*
	(0.011)	(0.007)	(0.003)	(0.030)	(0.012)
Cue(k)$\times I(0,12)$	0.001	0.001	0.008***	0.056*	0.000
	(0.014)	(0.008)	(0.002)	(0.032)	(0.013)
B_{PB}					
Cue(k)	0.022**	0.008***	–0.023***	0.004	0.003
	(0.010)	(0.003)	(0.001)	(0.012)	(0.005)
Cue(k)$\times I^A(-12,-1)$	0.048**	0.001	0.007	–0.200***	0.017
	(0.024)	(0.014)	(0.005)	(0.068)	(0.020)
Cue(k)$\times I^U(-12,-1)$	–0.007	–0.012*	0.005**	–0.106***	–0.021*
	(0.013)	(0.007)	(0.003)	(0.029)	(0.011)
Cue(k)$\times I(0,12)$	0.036**	0.026**	–0.001	0.002	0.002
	(0.018)	(0.010)	(0.003)	(0.040)	(0.013)
Panel B [Online search]					
B_{NRA}					
Cue(k)	0.015	–0.000	–0.033***	–0.002	0.001
	(0.011)	(0.003)	(0.001)	(0.014)	(0.006)
$GT(6)\times$Cue(k)	0.083**	0.043**	0.032***	–0.090	–0.008
	(0.038)	(0.020)	(0.009)	(0.092)	(0.031)
B_{PB}					
Cue(k)	0.034***	0.004	–0.026***	–0.021*	–0.008
	(0.010)	(0.003)	(0.001)	(0.011)	(0.005)
$GT(6)\times$Cue(k)	0.112**	0.061***	0.025***	–0.016	0.033
	(0.044)	(0.019)	(0.008)	(0.084)	(0.033)

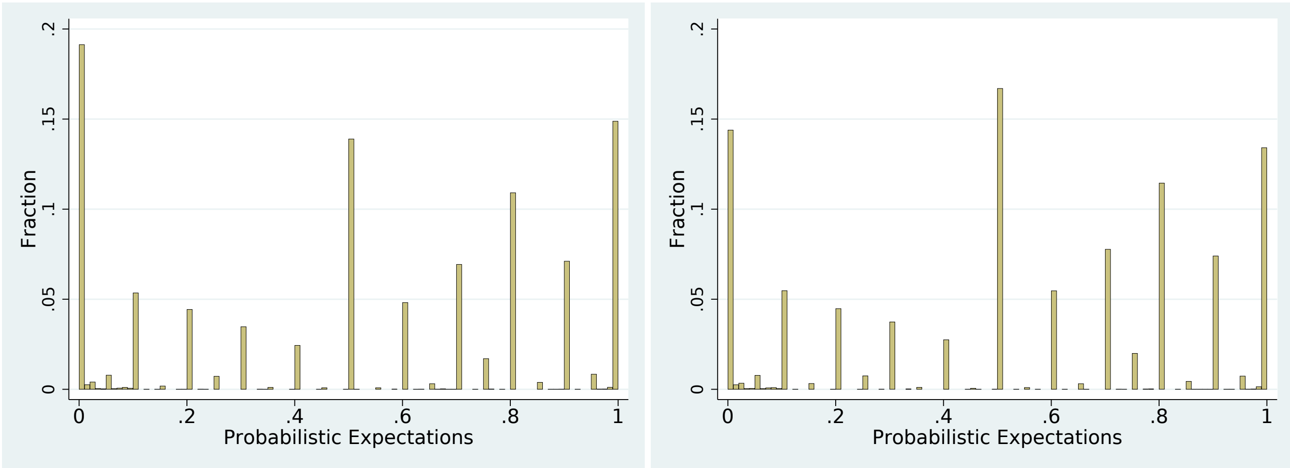
Notes: B_{NRA} refers to the beliefs an individual has about the chances that the government raises the national retirement age before the respondent retires; B_{PB} refers to the beliefs an individual has about the chances that the government reduces pension benefits before the respondent retires. "Cue(k)" refers to one of the five characteristics listed in columns (i)–(v), and k = {university degree, numeracy, age–50, optimism, probabilistic thinking, female}. For their explanation, see the text. Numeracy is demeaned. $GT(6)$ is the *Google Trends* index averaged over the previous 6 months. Each regression includes the same controls as in Table 3, 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). In columns (ii) and (iv) the number of observations is lower than in the case without heterogeneity because of missing data in numeracy and probabilistic thinking. For panel A, columns (ii) and (iv), N=26,791 and 27,851, respectively; for panel B, columns (ii) and (iv) N=23,347 and 24,364. 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 4.

* Significant at 10%; ** significant at 5%; *** significant at 1%.

Figure 1: Distribution of Expectations about Pension Reforms

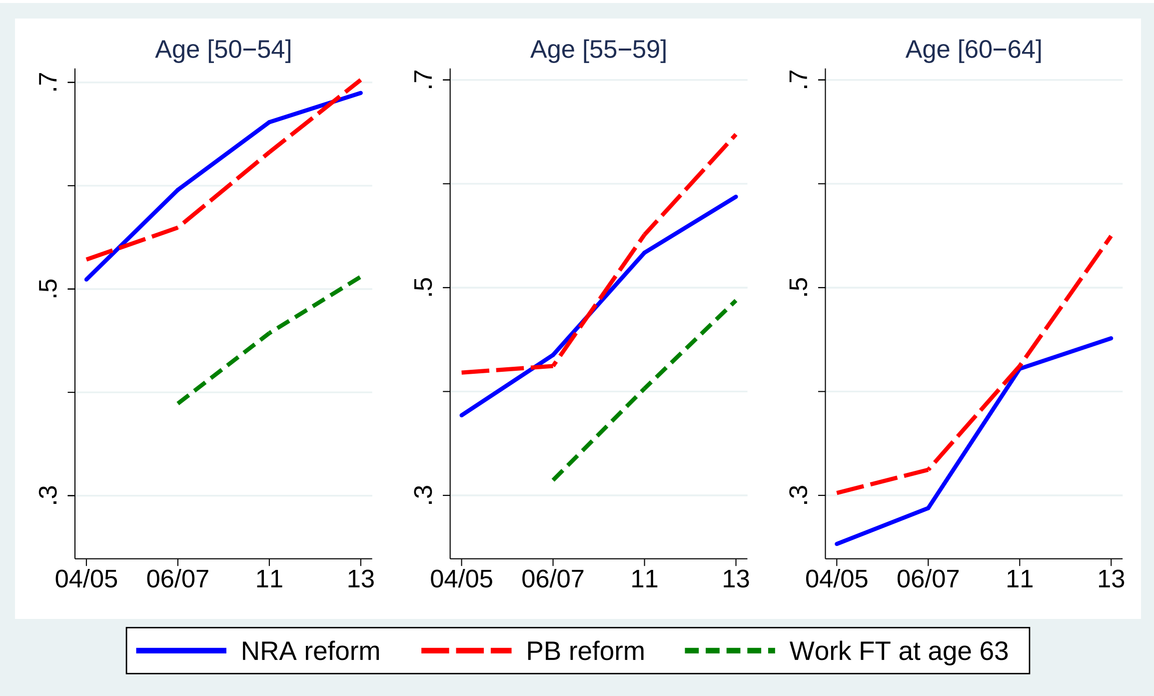
(a) Increases in National Retirement Age (NRA)

(b) Reductions in Pension Benefits (PB)



Source: SHARE, waves 1, 2, 4, and 5, covering the years 2004–2015 . The number of person-wave observations used in each figure is 27,881.

Figure 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>). The panels (except the panel on the right) also show the expectations that workers have to be working full time by age 63. We include only observations used in the main sample (see Tables 1 and 3), apart from Germany and Sweden which are excluded because they do not have observations in the 2011 wave.

Figure 3: Reforms' Timing, by Country and Reform Type

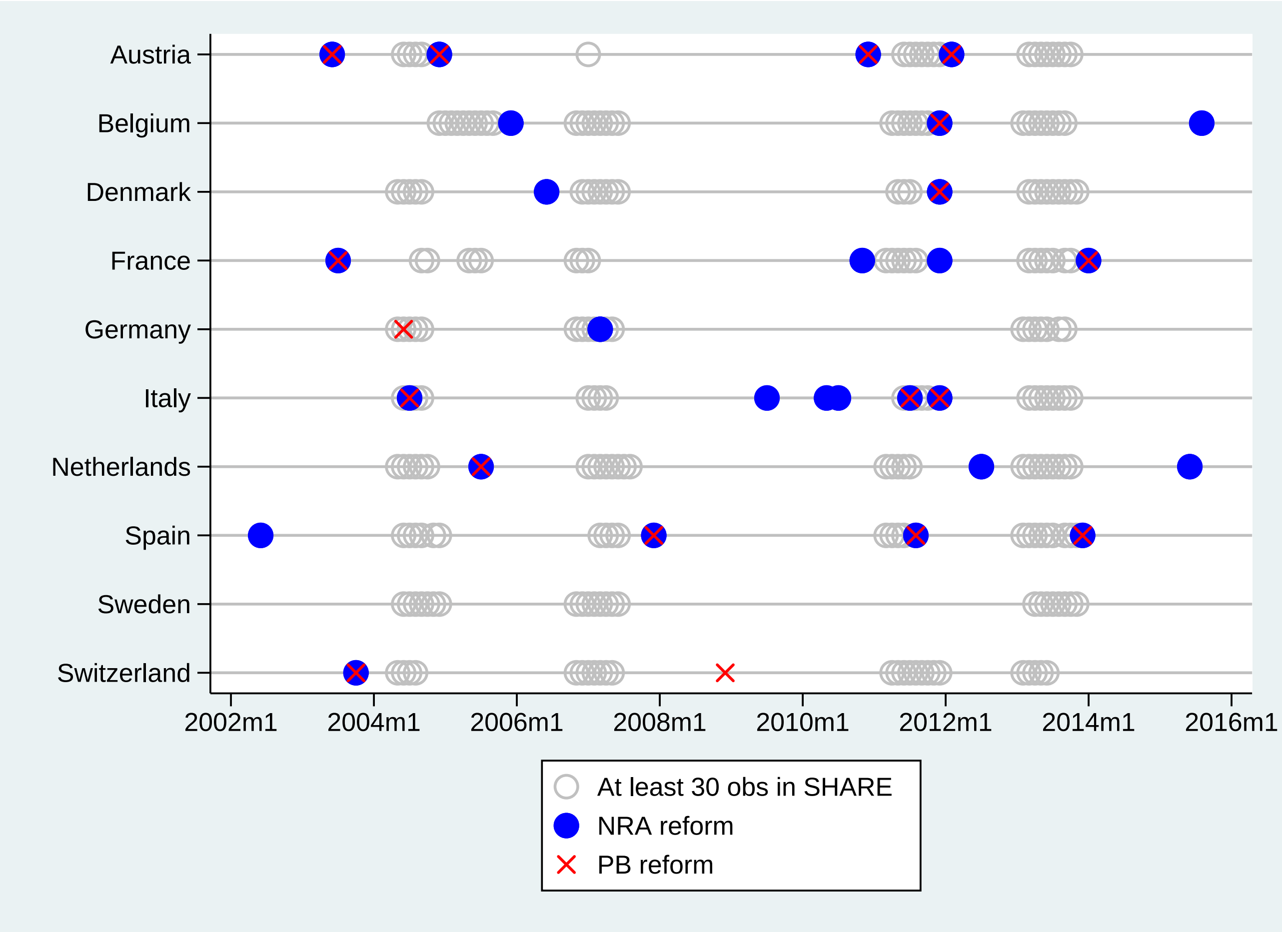
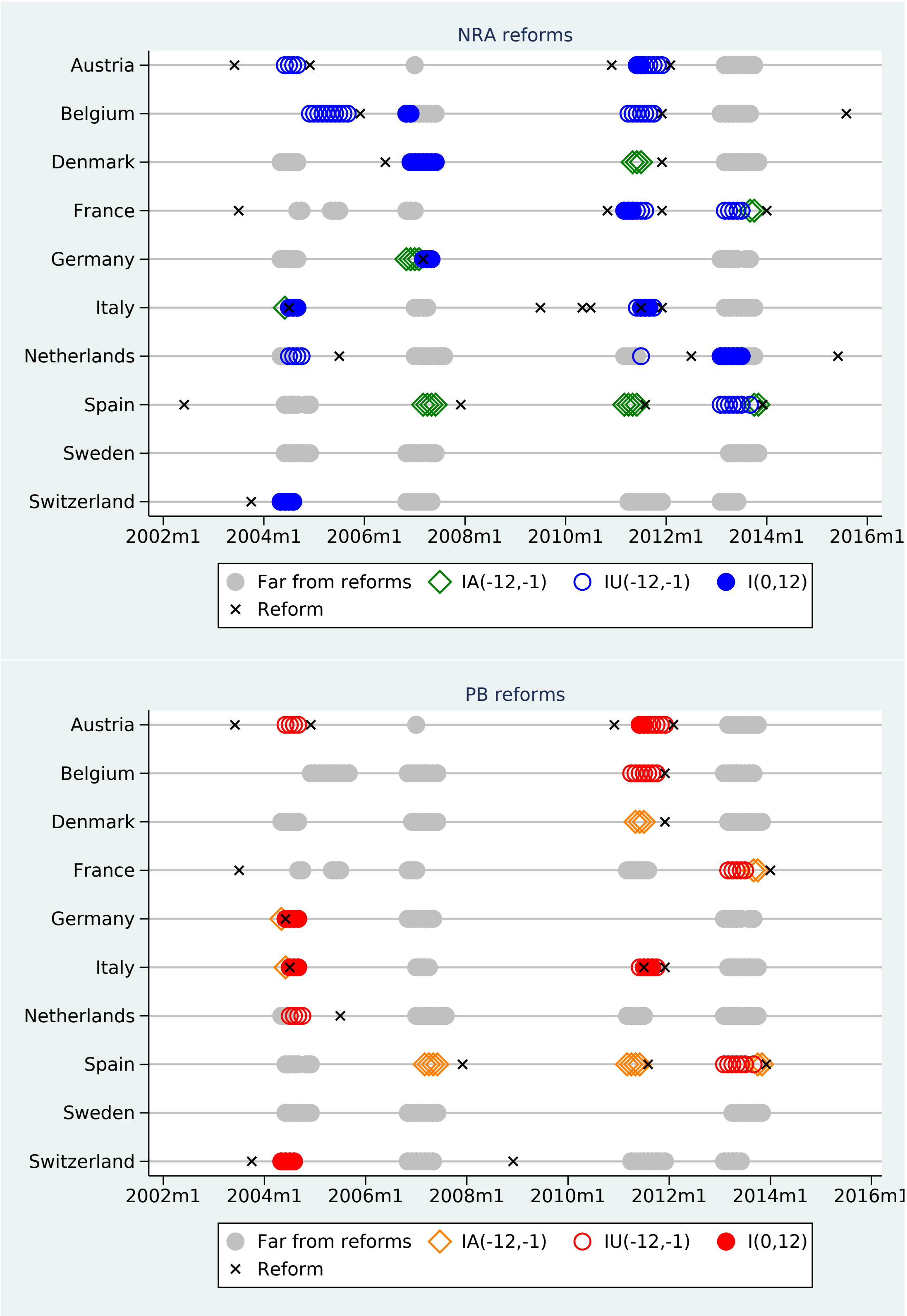


Figure 4: Reforms' and Announcements' Timing, by Country and Reform Type



Notes: $I^A(-12,-1)$ takes value 1 for workers who are interviewed up to 12 months before a reform has been announced, and 0 otherwise; $I^U(-12,-1)$ is equal to 1 for workers who are interviewed up to 12 months before a reform that is as yet unannounced, and 0 otherwise; $I(0,12)$ takes value 1 for workers observed up to 12 months after the reform implementation, and 0 otherwise.

Figure 5: Distributions of $G(0)$ and $G(6)$

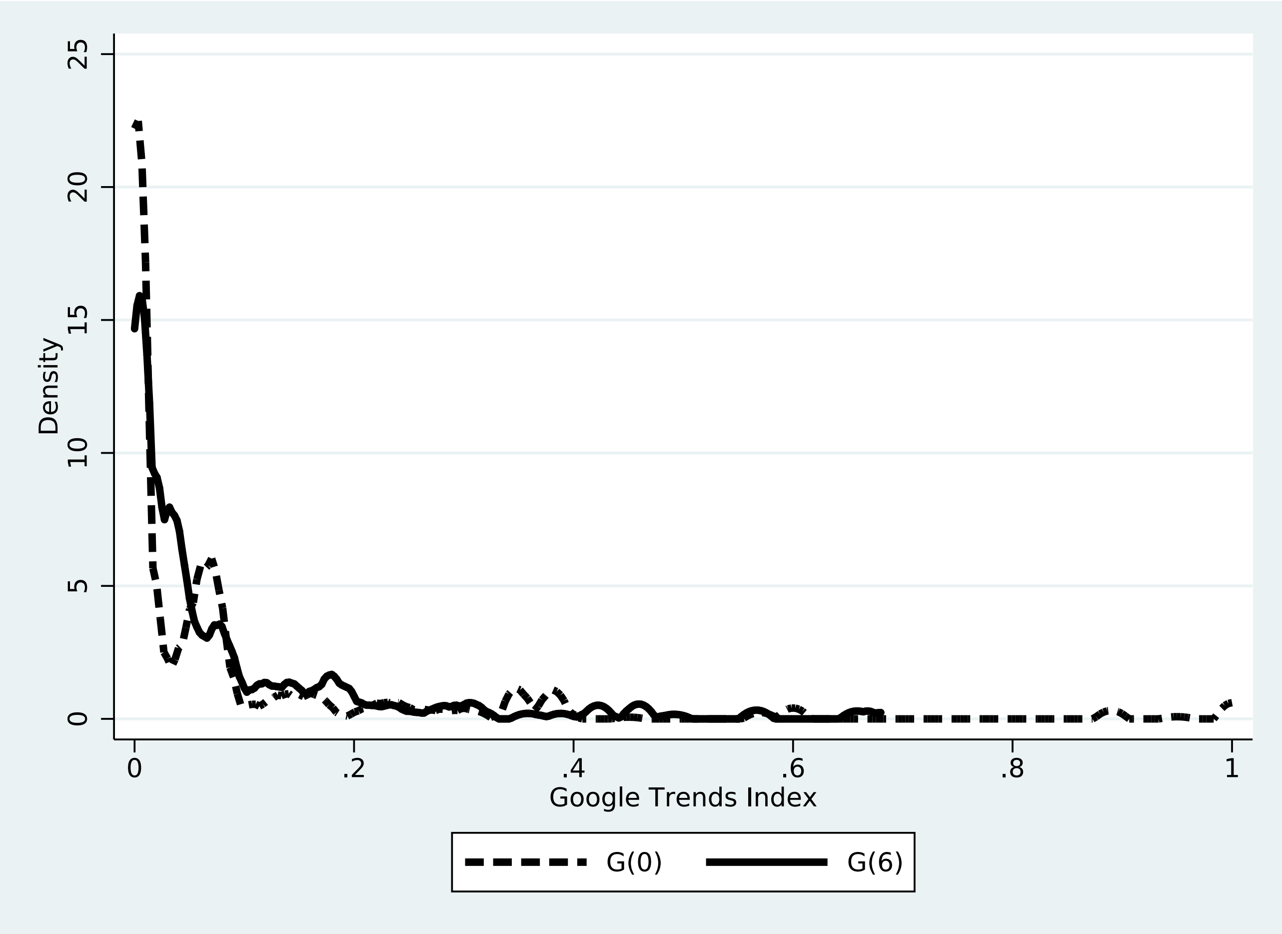
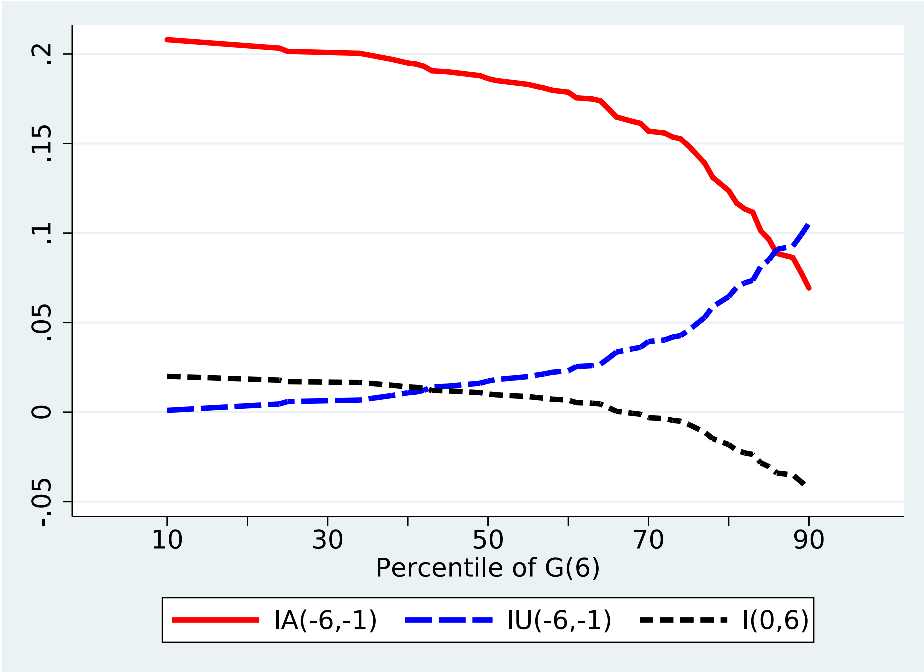
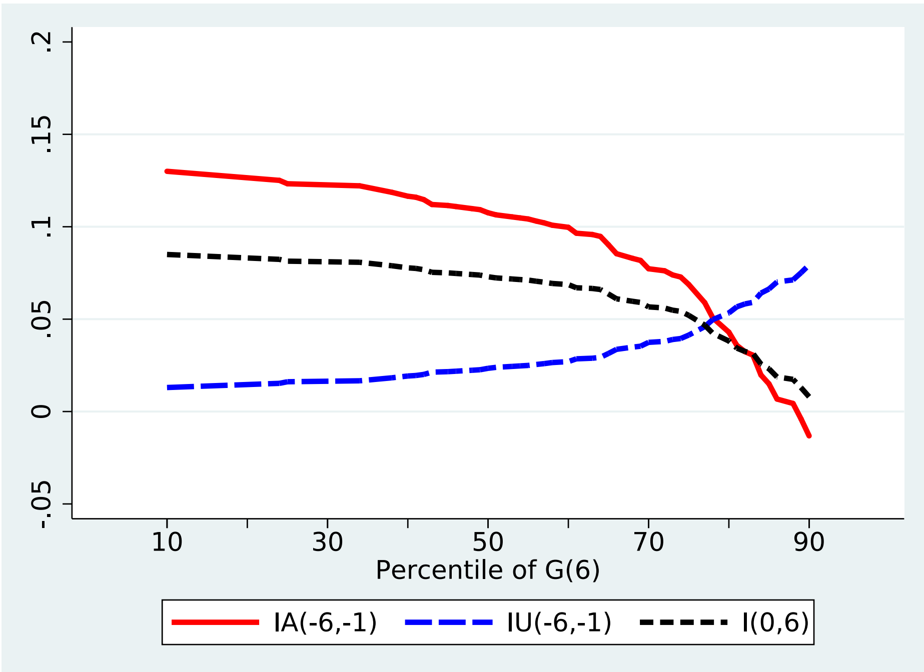


Figure 6: Effects of $I^A(-6,-1)$, $I^U(-6,-1)$, and $\mathrm{I}^U(0,6)$ on Expectations Across the Distribution of $G(6)$

(a) Expectations about increases in age at retirement, B_{NRA}



(b) Expectations about reductions in pension benefits, B_{PB}



Notes: The figure shows $\rho_{11} + \psi_{11} \times G(6)$, $\rho_{11} + \psi_{12} \times G(6)$, $\rho_2 + \psi_2 \times G(6)$ at different percentiles of the distribution of $G(6)$ using the estimates reported in Table 7. We consider values from the $10th$ percentile of $G(6)$ (which is equal to 0) to the $90th$ percentile of $G(6)$, since at the very top of the distribution the estimated effects become implausibly large (given the skewness in the $G(6)$ distribution, this is likely to be driven by nonlinearities which are not accounted for by (4)).