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Contested reform in the European Monetary Union: Lessons from the Netherlands

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ACADEMISCH PROEFSCHRIFT

ter verkrijging van de graad van doctor
aan de Universiteit van Amsterdam
op gezag van de Rector Magnificus
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ten overstaan van een door het College voor Promoties ingestelde commissie,
in het openbaar te verdedigen in de Aula der Universiteit
op 29 januari 2021, te 11 uur

door Jante Parlevliet
geboren te Tilburg

PROMOTIECOMMISSIE

For my parents

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Amsterdam, November 2020

CO-AUTHOR LIST AND PUBLICATIONS

- Chapter 2, entitled 'What drives public acceptance of reforms? Longitudinal evidence from a Dutch pension reform', is single-authored and has been published in *Public Choice*, October 2017, Volume 173, Issue 1–2.

- Chapter 3, entitled 'Populist attitudes, fiscal illusion and fiscal preferences: evidence from Dutch households' is joint work with Prof. dr. M. Giuliodori, Universiteit van Amsterdam. I furthermore received valuable advice from dr. M. Rooduijn, Universiteit van Amsterdam.

- Chapter 4, entitled 'Backlash to pension reform: the role of grievances, fiscal illusion and populist attitudes' is single-authored.

- Chapter 5, entitled 'Staggered wages, unanticipated shocks and firms adjustments' is joint work with Francesco Caloia (University of Venice, Vrije Universiteit, De Nederlandsche Bank) and dr. Mauro Mastrogiacomo (Vrije Universiteit, De Nederlandsche Bank).

- Chapter 6, entitled 'Frontal assault versus incremental change: A comparison of collective bargaining in Portugal and the Netherlands' is joint work with dr. A. Hijzen, Organisation for Economic Cooperation and Development (OECD) and Prof. dr. P.S. Martins (Queen Mary University of London). It has been published in the *IZA Journal of Labor Policy*, November 2019, Volume 9, Issue 1.

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INTRODUCTION

1.1   MOTIVATION

The case for structural reforms has been made recurrently on the European continent. Referring to all measures that lead to structural increases in labour utilisation and productivity, structural reforms have been on the agenda of European governments for decades. During the 1970s and 1980s, the main source of concern was the high level of unemployment in European countries compared to the United States. This 'eurosclerosis' was attributed to excessive regulation of markets and disincentives to work due to generous welfare states. In the 1980s, the advent of the Single Market provided new impetus to reform. And since the early 2000s, the ageing reports of the European Commission have cautioned against the macroeconomic effects of ageing. With roughly a doubling of the population above 65, from around a quarter of the working age population in 2000 to half of it by 2050 (EC, 2001), these forecasts put reform of pension systems and other age-related policies prominently on the policy agenda of European policymakers.[1]

For the countries in the euro area, the case for reform has been accompanied by a plea for increased flexibility in labour and product markets. Many of the candidate members had fragile labour markets, with high unemployment and/or little dynamism (Blanchard and Jimeno, 1995). By joining a currency union consisting of very diverse economies, the single monetary policy could not be tailored to country-specific conditions and countries could no longer resort to competitive devaluations in case of a shock. What is more, in the 1992 Maastricht Treaty, member countries agreed on strict limits to their public deficits and debt (i.e., the Stability and Growth Pact), as well as on a prohibition of monetary financing and bailout of national governments. At the same time, for political reasons, the single currency was not matched with a European fiscal capacity. Hence, by restricting monetary and fiscal tools at the national level, flexible labour and product markets – in which relative prices and wages would guide economies to their new equilibria – were seen as necessary mechanisms to deal with country-specific shocks.

For a while, some economists speculated that this loss of policy space would provide countries with an incentive to make their economies more flexible (Calmfors, 2001), although many acknowledged this was a risky strategy (Van Poeck and Borghijs, 2001). Indeed, research soon showed that that this hope was idle (Bednarek-Sekunda *et al.*, 2010; Duval and Elmeskov, 2006; Van Poeck and Borghijs, 2001). When the Great Recession put the Economic and Monetary Union (EMU) and its then sixteen national economies to an unprecedented test, the costs that had been dreaded became painfully clear. Especially

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1. In the latest projections issued by Eurostat, this so-called old-age dependency ratio is estimated at 52% in 2050 and 57% in 2100.

in the southern European economies, unemployment soon rose to levels that most of these countries had never experienced before; in some cases affecting over a quarter of the workforce (see Figure A.1.3 in the Appendix). Against the backdrop of a sovereign debt crisis, which threatened the very existence of the EMU, several southern European economies underwent an adjustment program that entailed a host of structural reforms. Furthermore, spurred by heightened levels of fiscal stress, many other EU countries also implemented major reforms to pensions, which are commonly the largest spending category on government budgets (Natali and Stamati, 2014; Natali, 2015; Natali, 2018; Beetsma *et al.*, 2020).[2]

Where does this leave us today? Even before the COVID-19 pandemic hit the European continent in early 2020, with a deep economic contraction ensuing in its wake, the structural performance of many European economies was weak. For one thing, differences in living standards between EMU countries remain large. In fact, the relative income position of Greece and Italy has even deteriorated relative to the situation in 1980 and at the start of the euro, especially due to poor productivity growth (see Figure A.1.1 in the Appendix). In 2019, a year in which the euro area economy overall was judged to perform above its structural potential, unemployment exceeded 10 percent in Greece, Italy and Spain (see Figure A.1.3 in the Appendix). While there is potential for catch-up growth for the poorer economies, many economists are not optimistic about the prospects for productivity growth for advanced economies as productivity growth seems to have halted across the Atlantic (Gordon, 2012; Gordon and Sayed, 2019; Bergeaud *et al.*, 2016; Decker *et al.*, 2017). Furthermore, whereas increased labour market participation has significantly contributed to economic growth in the last decades, it is predicted that during the coming decades, demographic trends will instead put a drag on growth in several European countries, even when measures to abolish early retirement and increases in the retirement age are taken into account. In the countries most affected by ageing – i.e. Germany, Portugal and Italy – this discount is expected to measure in the magnitude of half a percentage point of annual growth (see Figure A.1.2 in the Appendix). In the context of an economic contraction many times larger and with public finances being in a bleaker state than when the Great Recession hit ten years ago, in the coming years reforms to increase labour utilisation and productivity growth may be even more important.

Yet, the last decades have also taught us that political elites that wish pursue reforms face a delicate task. The literature has highlighted a myriad of sources of resistance. Reforms

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2. In 2017, spending on pensions in the euro area averaged 12.4 percent of GDP and constituted by far the largest social spending item. The pension budget was followed by health (7.1 percent GDP), education (4.5 percent GDP) and sickness and disability pensions (2.8 percent GDP) (ECB, 2019).

can be blocked owing to opposition by powerful groups that have much to lose from these reforms (Olson, 1965; Sinn and Uebelmesser, 2003). Yet, resistance to reforms can also be rather broad-based. For one thing, the gains from reforms may be uncertain so that the people who will benefit from reforms may fail to embrace such measures (Fernandez and Rodrik, 1991). Relatedly, universal welfare programs tend to be rather popular with large segments of the electorate (Pierson, 1996; Brooks and Manza, 2006; Vis, 2016) and the public may often not feel that reforms are necessary (Boeri *et al.*, 2001; Blinder and Krueger, 2004; Boeri and Tabellini, 2012; Caplan, 2002). Governments therefore may be reluctant to propose reforms because of expected electoral risks. In the purported words of former Eurogroup president Juncker, '*We all know what to do but we don't know how to be re-elected once we have done it*' (The Economist 2007, quoted in Buti *et al.*, 2009).

Against this backdrop, this thesis aims to shed light on the political economy of two sets of economic reforms. The first part of the thesis focuses on fiscal reforms, i.e. reforms that are one-to-one linked to the government budget. Chapter 2 analyses the attitudes of Dutch households ten years in the run-up towards a major pension reform in 2012, the increase in the retirement age from 65 to 67, linking it to life expectancy thereafter. Because this reform has been challenged prominently by so-called populist parties, also in other countries, I also study the role of anti-elite sentiment of households as a driver of their attitudes towards fiscal policy in general (Chapter 3) and towards the increase in the retirement in particular (Chapter 4). Section 1.2 gives a more detailed introduction of the approach taken in these chapters and the lessons learned.

In the second part of this thesis I focus on another policy area that has been subject to fundamental reform, namely collective bargaining. While the words of Juncker may suggest that reforms always go in one direction, in many policy domains we see fundamental discussion about the desired direction of reform. In order to better align wage setting policies with experiences at the firm level, decentralisation has been 'the name of the game' in reforms of collective bargaining for decades (Visser, 2013b). In various countries, this decentralisation has meant that bargaining itself was also moved to the firm level, which has the disadvantage that the bargaining power of workers may be weakened, especially when unions are poorly organised at the firm level. In the Netherlands, bargaining is still predominantly organised at the sector level and in the second part of this thesis I will provide some lessons on how sector-level bargaining can deal with challenging economic conditions, such as the large shock that hit the economy at the end of 2008. Section 1.3 gives a more detailed account of the approach taken and the lessons learned in these last two chapters. Furthermore, in Section 1.4 I draw some lessons for the governance of structural reforms in the euro area.

1.2 THE POLITICAL ECONOMY OF FISCAL REFORMS

As mentioned, the last decades have seen a surge of pension reforms all over the OECD (Natali, 2015; Beetsma *et al.*, 2020; OECD, 2019a). The 2008 financial crisis and the fiscal stress that ensued in its wake have heightened the pace of such reforms.[3] In the Netherlands, perhaps the most significant reform of the last decade concerns the increase in the retirement age from 65 to 67, a measure that had been debated for years and was finally adopted in 2012. The aim of Chapter 2 is to disentangle the role of individual-level and collective attitudes in the run-up to this reform. The chapter exploits a unique longitudinal dataset on the attitudes of Dutch households regarding pension reform in the 2003–2013 period. Our findings offer various new insights. First, while age and income play a large role in political economy models on support for pension reform (Mulligan and Sala-i-Martin, 1999; Tabellini, 2000; Sinn and Uebelmesser, 2003; Galasso, 2014), in our analyses they appear to be of minor importance. Instead, we find that education, occupational status and psychological traits were the most systematic drivers of reform preferences at the individual level. Second, we find that the 'year effects' were the main driver of respondents' acceptance of reform. While in 2004 less than 20 percent of respondents would choose an increase in the retirement age as first preferred reform option, by 2012 this had increased to well over 40 percent. Much of this increase took place well before the crisis. In line with the model of Rodrik and Fernandez (1991) about uncertainty about the gains of the reform, we interpret the time pattern of reform attitudes as evidence of a collective learning process whereby households gradually updated their expectations and reform preferences in light of new information.

Reforms to pensions, and other policy domains, however, did not prove uncontested. For a number of decades now, countries across the world have witnessed a surge in the vote share of parties that challenge political elites. They have been dubbed populist, in reference to rhetoric that promotes the notion of an antagonistic division between the 'ordinary people' and an 'evil elite' which is not acting according to the popular will (Mudde, 2004; Akkerman *et al.*, 2014). Initially, many populist parties had a narrow agenda focusing mainly on immigration, law and order and European integration. Yet more recently, many of them have also come to challenge socio-economic policies. Economists have traditionally perceived populism as a threat to sound economic policymaking (Dornbusch and Edwards, 1991). It is well documented that the public is often poorly informed about economic facts and mechanisms. Hence, many economists and policymakers fear that populist parties can exploit the poor understanding of voters and attract votes by promising simple solutions that are not sustainable in the long run (Dornbusch and Edwards, 1991;

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3. See Table A.1.1 in the Appendix for the development of government debt in selected euro area economies.

Andersen *et al.*, 2017; Guiso *et al.*, 2017; Davidson, 2018). When it comes to fiscal policy, the 'populist economic agenda' is said to be characterized as expansionary, while neglecting the negative consequences of debt accumulation and inflation (Andersen *et al.*, 2017). These concerns of economists rest on the assumption of fiscal illusion, which holds that voters appreciate spending programs but underestimate the (future) costs in terms of taxation or debt (Buchanan and Wagner, 1977; Alesina and Perotti, 1995). What could reinforce the concern of economists about ill-guided policies is that people who hold strong populist attitudes might also be skeptical of experts, as manifested in discussions in the wake of the Brexit referendum as well as the election of Donald Trump. Indeed, recent research has confirmed that populism strongly correlates with anti-intellectualism, a measure that includes attitudes towards economists (Merkley, 2020).

Against this backdrop, Chapter 3 studies whether people who hold strong populist ideas also report significantly more expansionary fiscal preferences, and whether populist attitudes reinforce the risk of fiscal illusion. Our analysis is based on an analysis of fiscal preferences in a real-life setting, i.e. the question on how to use windfalls in tax revenue in the summer of 2017. We measure populist attitudes of respondents with a tested index developed by political scientists (Akkerman *et al.*, 2014). Furthermore, we assess the role of fiscal illusion by including a measure of the literacy of respondents, and by means of an experiment in which we treat a random share of respondents with information about public debt dynamics. The chapter makes three contributions to the literature. First, we regress fiscal preferences on populist attitudes at the individual level. To our knowledge, this has never been done before, and we find that populist attitudes of respondents prove a very relevant predictor of their fiscal preferences. Because this regression may suffer from endogeneity due to, for example, reverse causation, we also employ an instrumental variable (IV) estimation. When instrumenting populist attitudes of respondents with the trust in politics and in the management of financial firms that respondents had reported prior to the crisis, our results remain valid. Second, in line with previous research, we find that literacy and information provision – which have the potential to alleviate the occurrence of fiscal illusion – indeed contribute to less expansionary fiscal preferences. Third, we find that the effect of literacy is conditional on the level of populist sentiment. In particular, we find that poorly literate respondents report significantly higher support for tax relief only when they hold strong populist attitudes, but not when they have more favourable attitudes towards the elite. Furthermore, we find that the effect of information provision is larger for respondents with stronger populist attitudes, suggesting that information provision can also be effective in alleviating fiscal illusion also with voters who are skeptical of the establishment.

Chapter 4 extends the analyses in Chapter 2 and Chapter 3. While the analysis in Chapter 2 closes in 2013, a year after the increase in the retirement age was implemented, in subsequent years backlash against the reform mounted, expressed most prominently by populist parties. In other European countries, populist parties also campaigned to bring the retirement age down again (e.g. the Lega Nord and Five Star movement in Italy, Front National in France and Podemos in Spain). Our data allow us to test several hypotheses on the drivers of this reform backlash. First of all, we assess to what extent grievances about postponed retirement – such as lower longevity and poorer working conditions – drive reform resistance. Secondly, we test to what extent poor literacy can explain reform resistance. Thirdly, we test to what extent populist attitudes drive reform resistance. Our results shed light on the mechanisms that lie at the basis of resistance to the retirement age increase, and at the basis of support for reform reversal. First, we find evidence that grievances that have been left unaddressed in government policies play a role, most notably differences in longevity and lower job satisfaction. Second, we also find evidence that poor literacy explains part of the resistance to reform. Third, we find that populist sentiment in itself spurs reform resistance. This effect is large and is also robust to a set-up in which we instrument populism with pre-reform trust in national politics and the management of financial firms.

1.3 REFORM OF COLLECTIVE BARGAINING SYSTEMS

The second part of this thesis discusses some questions around what is perhaps the most contentious type of reform adopted in countries within EMU in recent years, i.e. reforms to collective bargaining. As noted, by entering a currency union, individual countries give up on an important policy instrument to adjust to shocks, i.e. control over their exchange rate. The loss of this exchange rate instrument was not trivial, as especially southern European economies relied on substantial competitive devaluations well into the 1990s (see Figure A.1.4 in the Appendix). When the Great Recession put EMU and its then sixteen national economies to an unprecedented test, it became clear that some of these economies were not flexible enough to deliver the massive adjustment of relative prices that suddenly appeared necessary.

Clearly, too little wage adjustment is by no means the only factor to blame for the substantial increase in unemployment, since other important factors such as a tightening of credit conditions and fiscal consolidation played their part (Comunale and Hessel, 2014; Bentolila *et al.*, 2018; Terzi, 2020), while ultimately wages did show a substantial drop (OECD, 2014). Yet, studies did point out that rigid wages impeded a downward adjustment of wages and were at the basis not only of a paucity of new jobs being created,

but also of jobs being destroyed (Martins, 2014; Carneiro *et al.*, 2014; Guimaraes *et al.*, 2017). In the same period, Germany experienced a drop in unemployment, falling from 11.3 percent in 2005 to 5.3 percent in 2013 (and further down thereafter, see Figure A.1.3 in the Appendix). Many factors have been found to be at play, from reforms to social security to the creation of flexible employment contracts (Parlevliet and Verbruggen, 2015). Yet, according to some economists, reforms to wage bargaining – giving firms more scope to deviate from sector-level agreements – were pivotal in Germany's new emergence as an 'economic superstar' (Dustmann *et al.*, 2014).

When some of the southern European countries entered into a program, the consortium of the EC, ECB and IMF – typically dubbed 'the troika' – also scrutinised their collective bargaining systems and proposed several reforms thereof. In these reforms, a large emphasis was placed on moving collective wage bargaining from the sector level to level of the firm, with the intention of making wages more responsive to firm-level conditions. Yet these reforms proved particularly contentious and according to some even constituted a 'frontal assault' on collective bargaining (Marginson, 2015; Van Gyes and Schulten, 2015). This controversy had several sources. For one thing, reforms to wage bargaining are by nature controversial as they can have important implications for the balance of power – and hence bargaining outcomes – both between workers and firms, and within these two groups. Despite the praise for Germany's employment miracle, decentralisation of bargaining has also been held responsible for the decline in bargaining coverage, increasing wage inequality and labour market duality (Dustmann *et al.*, 2009; Hassel, 2014). In addition, there were concerns that bargaining systems were still too poorly understood to motivate such drastic measures (Blanchard *et al.*, 2014; Koukiadaki and Grimshaw, 2016; Brandl and Ibsen, 2017). For one thing, research has highlighted various examples of countries, including the Netherlands, that have rigid bargaining systems but that have managed fairly well to restrain wage growth and maintain external competitiveness (Johnston and Hancké, 2009; Vandenbroucke, 2017).

Unlike Germany, the Netherlands has so far held on to sector-level bargaining. Although various large firms have their own collective agreement, firms cannot easily opt out of sector-level agreements and the large majority of the workforce is covered by a sector agreement. Although in recent decades there have been continuous adjustments, the Dutch system still contains some strong rigidities. For instance, some collective agreements have durations of two or even three years, hampering wage adjustment in the case of a shock. Research on Spain has pointed out that such long contracts were a source of job losses in the aftermath of the Great Recession (Díez-Catalán and Villanueva, 2014). Yet, there is little research on the impact of such rigidities in a mature corporatist setting, where possibly mechanisms have emerged to alleviate such rigidities. Chapter 5

assesses to what extent the staggered adjustment of economic shocks into wages has also affected employment in the Netherlands. We use the large drop in growth and inflation expectations in the aftermath of the collapse of Lehman Brothers, after which wage growth fell from 3.5 percent to 1 percent in two quarters, to look at the differential effect on employment in firms that operated under 'old' contracts versus firms that had been able to timely adapt wages downwards. As the starting dates of agreements are predetermined, our estimates can be interpreted as causal. Interestingly, we find that there was no difference between employment in anticipating and non-anticipating firms. Instead, we find that non-anticipating firms had been able to reduce the wage bill of their workforce by cutting bonuses and benefits and so-called incidental pay components. This means that rigidities in the setting of sector-level CAOs do not necessarily lead to employment losses, when firms have enough discretion over other wage components.

The Dutch experience with sector-level bargaining can also offer lessons for countries that are reluctant to move bargaining to the level of the firm. In Chapter 6, we compare the reforms to collective bargaining that Portugal undertook under the 2011 adjustment programme to the collective bargaining system in the Netherlands. In the late 1970s and early 1980s the Dutch economy was in a dire state, with poor firm profitability and high unemployment (see Figure A.1.3 in the Appendix), while resistance increased against the occasional use of competitive devaluations vis-à-vis the D-mark (see Figure A.1.4 in the Appendix).[4] After the 1982 Wassenaar agreement, in the wake of concerted social dialogue, the Netherlands gradually and incrementally reformed its bargaining system. In Portugal, prior to the crisis, high inflation had facilitated real wage adjustments (Blanchard, 2007; Carneiro *et al.*, 2012). With the onset of EMU, it was widely acknowledged that bargaining systems in Portugal and other southern European countries were in need of change, as they were very rigid, highly dependent on state support and characterised by adversarial relations (Fajertag and Pochet, 2000; Natali and Pochet, 2009; Molina, 2014). In 2011, Portugal entered into an adjustment program under which it agreed to adopt reforms to collective bargaining. As happened in other countries, these reforms led to strong controversies and some of them were reversed once the program had ended (Moury and Afonso, 2019).

Our comparison between the Dutch and Portuguese systems offers several insights. First, introducing greater flexibility in collective bargaining systems has been possible in the Netherlands without undermining sectoral bargaining through more customization within agreements. Second, coverage extensions can support a level playing field, but

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4. For a description of the economic problems at hand and the reforms that were taken in the run-up up of the emergence of the Netherlands 'Dutch miracle', see Knot (2018).

in the Netherlands they have also been made subject to representativeness criteria. And finally, effective coordination between bargaining units and high-quality labour relations can improve the performance of collective bargaining. The Dutch experience suggests that the government can promote responsible bargains by offering both 'carrots' (e.g. fiscal subsidies) and 'sticks' (e.g. the threat of not granting extensions).

1.4 IMPLICATION FOR THE GOVERNANCE OF REFORMS IN THE EMU

By sharing a single currency, there can be important spillover effects from countries' economic policies. From the start of EMU, there has been a sequence of initiatives to better coordinate economic policies while respecting national diversity (Vandenbroucke, 2017; Verdun and Zeitlin, 2018). Whereas the Stability and Growth Pact from the start provided for sanctions in the case of non-compliance, until recently the governance of broader economic policies mainly relied on monitoring and peer review. Since the crisis, European authorities have been granted some more traction over domestic policies, although not all of these mechanisms have been used so far. Since 2011, the Macroeconomic Imbalances Procedure has granted the European Commission the ability to challenge member countries' economic policies in case of macroeconomic imbalances, backed up by the ability to impose sanctions. Furthermore, access to lending facilities that have been set up to assist distressed countries is contingent on adjustment programs including the adoption of structural reforms. This is not the place to review specific governance proposals. However, this thesis does offer some lessons for the viability of such attempts to foster the adoption of reforms.

First, this thesis has highlighted the importance of literacy and information provision. From a European perspective, the Netherlands can be seen as an upper bound for the susceptibility of the public to be convinced of the need for reforms. The Netherlands, first of all, has a long tradition with a fiscal council, the Netherlands Bureau for Economic Policy Analysis (CPB), which plays an important role in providing policymakers and the public alike with independent information about the rationale for reforms (van Geest and van Vuuren, 2018). In the wake of the European debt crisis, fiscal councils have been set up across the European continent. Fiscal councils have been found to support fiscal transparency especially when they operate independently from politics and when they have a strong presence in the public debate (Debrun and Kinda, 2017). Furthermore, the Netherlands has a relatively highly literate population (see Table A.1.3 in the Appendix). While in a large scale survey conducted by the OECD, 18 and 17 percent of Dutch adults score at the highest level of proficiency for literacy and numeracy, respectively. In

Italy, these percentages are only 3 and 5 percent, with similar scores in Greece and Spain.[5] This calls for massive investments in skills upgrading, benefiting both productivity growth as well as voters' readiness to reform (Fornero, 2014). Of course, such investments take considerable time to take effect, in the meantime making it particularly challenging for elites in these countries to pave the way for reforms.

Second, the adoption of reforms is greatly facilitated by compensating policies. Economic policy reforms can involve substantial costs for specific groups, both in terms of material conditions as well as economic insecurity. Just as is the case with globalisation, reforms may therefore need to be embedded in a set of compensating policies that can offset such costs (Ruggie, 1982; Burgoon, 2009). When it comes to the reforms studied in this thesis, these may be generous supporting fiscal policies, like the ones that were adopted as part of the 1982 Wassenaar agreement to restrain wage growth, and grandfathering clauses, like the 2019 decision to slow down the increase in the retirement age for the oldest cohort of workers. Turning to the governance of economic policies in EMU, an implication is that the implementation of structural reforms is best accompanied by fiscal space, such as is envisioned with the Budgetary Instrument for Convergence and Competitiveness (BICC).

Third, the rise of populist parties and the pervasiveness of populist sentiment among voters fundamentally complicates the pursuit of reforms. It is still debated what factors are at the root of the success of populist parties, ranging from supply-side factors (e.g., cognitive mobilisation and the dissolving of religious and ideological ties between the electorate and the elite) to poor responsiveness of mainstream parties to concerns over immigration or economic hardship. Yet, whatever its cause, it is clear that the political constraints to reform are even larger in an era of populist dynamics. If people do not believe that the elite acts in their interest, compensating policies and careful design of policies become even more important to signal that the elite is trying to serve the public at large. At the same time, it is dangerous to equate the populist economic agenda as merely short-termist and unsustainable, as it can reinforce the idea that the elite is too easily dismissing voters' concerns, while it may also inhibit debates about fundamental economic policy questions, complex as they may be (Piketty, 2020).

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5. At the low end of the distribution, 12 percent (13 percent) of the Dutch adult population scores at or below the lowest proficiency level in reading (numeracy). This is 28 percent for literacy and 32 percent for numeracy in Italy, with again very similar scores in Greece and Spain. Closer inspection of the scores per age cohort lead to similarly worrisome conclusions. In Greece and Italy, the average literacy proficiency of respondents below 24 is equal to or even below the score of Dutch respondents aged between 55 and 65. What is more, in Greece, literacy proficiency has hardly increased in more than a generation.

A fourth lesson is that an appropriate balance needs to be struck between what is imposed by monetary unification, and the socio-economic goals of national societies, such as a generous welfare state and a balanced distribution of income (Vandenbroucke, 2017). Finding this balance is particularly complex when it comes to collective bargaining. Whereas prior to the crisis it was widely acknowledged that bargaining systems in southern European countries were in need of reform (Fajertag and Pochet, 2000; Natali and Pochet, 2009; Molina, 2014), the Dutch experience shows that firm-level bargaining is not a necessary condition for the pursuit of responsive wage setting and external competitiveness (see also, Johnston and Hancké, 2009; Vandenbroucke, 2017). Yet, for sector-level bargaining to become more effective at these tasks in southern European countries (and remain so in the Netherlands), social partners will have to increase their representativeness, and they will have to show they are able to strike responsible bargains. The Dutch experience suggests that governments can promote self-regulation, for example, by offering both 'carrots' and 'sticks'. Looking forward, virtually all countries where collective bargaining is prominent also face a number of common challenges related to the decline in union density and the emergence of new forms of work, which threaten to undermine the legitimacy of collective agreements, especially in some sectors and among younger cohorts.

Finally, and relatedly, external pressure may be a tipping point for the adoption of reforms, but it is domestic political deliberation that is crucial for its long-run sustainability. In the Netherlands, it was the threat of breaching European deficit rules that acted as a catalyst for the 2012 decision to increase in the retirement age to 67. Yet before that moment, reports by the Dutch fiscal council (CPB) and extensive political deliberation had been paving the way for the reform for years, while the Dutch public had had at least a decade to get used to the idea of a prolonged working life. Elements of the Dutch reform of 2012 that were most controversial were reversed in the subsequent years. Also in Portugal and other countries that underwent an adjustment programme, controversial reforms have been reversed once the programs had ended (Moury and Afonso, 2019). While evidently situations of acute crisis do not always allow for extensive domestic deliberation, it is worthwhile where possible to take the time to design reform packages that address the main grievances among the electorate so that reforms can endure.

PART I:
THE POLITICAL ECONOMY OF FISCAL REFORMS

Chapter 2

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WHAT DRIVES PUBLIC ACCEPTANCE OF REFORMS?

LONGITUDINAL EVIDENCE FROM A DUTCH PENSION REFORM

2.1 INTRODUCTION[6]

Governments often find it hard to pursue economic reforms. Two general sets of considerations have been put forward to explain this. A first view points toward distributional conflicts between different socio-political groups with opposing interests. Such conflicts can take time to be resolved because, for instance, it can pay to outwait the other party in a 'war of attrition' (see, e.g., Alesina *et al.*, 2006). By the same token, reforms can be blocked due to opposition by powerful groups that have much to lose from these reforms (Olson, 1965; Sinn and Uebelmesser, 2003). A second view stresses that resistance to reforms is in fact rather broad-based. In particular, universal welfare programs tend to be rather popular with large segments of the electorate (Pierson, 1996; Brooks and Manza, 2006; Vis, 2016) and the public may often not appreciate that reforms are necessary (Boeri *et al.*, 2001; Blinder and Krueger, 2004; Boeri and Tabellini, 2012; Caplan, 2002). In this context, voters who eventually gain may resist change (Pitlik *et al.*, 2014). Governments therefore may be reluctant to propose reforms because of expected electoral risks. In the purported words of former Eurogroup president Juncker, '*We all know what to do but we don't know how to be re-elected once we have done it*' (The Economist 2007, quoted in Buti *et al.*, 2009).

This chapter aims to disentangle the role of individual-level and collective resistance to reform using a case study of a major Dutch policy change implemented in 2012, namely the increase in the statutory retirement age from 65 to 67 in 2023. From the perspective of policymakers, this reform measure increasingly was seen as the most promising option for adjusting the public pay-as-you-go (PAYG) pension system to the Dutch population's ageing (as in other countries, see World Bank, 1994). The rise in the retirement age already had been considered at the scheme's inception in 1957, and had been advocated by policy committees in the 1980s and 1990s. Nevertheless, it was politically controversial (as it was and is elsewhere in Europe, see EC, 2012; Arza and Kohli, 2011), and various governments instead resorted to more passive measures, such as raising payroll taxes – and, since the late 1990s, transfers from the general budget – and temporary suspensions of the indexation of benefits. Building on a unique longitudinal dataset over the years 2003-2013, we evaluate the explanatory powers of the expectations and preferences of Dutch households towards a later retirement age in the run-up to the actual reform. Controlling for a large set of covariates, we assess the extent to which opposition to the reform was driven by various individual characteristics – such as age and income – or was broader in nature.

Our analysis makes two contributions to the literature. First, given information on a large set of individual characteristics and a large number of observations (over 12,000) our data enable us to assess systematically the relative importance of various explanatory variables that have been suggested in the literature, such as age, employment status, income, education and deeper-rooted psychological traits. Second, the time dimension of our data is unique and enables us to trace preferences for reform over a time span of ten years in the run-up to an actual reform. This time dimension turns out to play a critical role.

Our most important findings can be summarized as follows. First, among the individual covariates, we find that education, psychological traits (such as patience and locus of control) and employment status were the most robust drivers of attitudes towards the reform. On the other hand, despite the diverging interests of young and old respondents, age explains only a minor share of the variation in reform attitudes. We also do not find that income played a significant role in attitudes toward pension reform, even though the PAYG schemes entails considerable redistribution across income groups. Second, we find that the year effects in our regressions are dominant in driving respondents' acceptance of the reform. In the ten years preceding the reform, households seem to have gradually updated their expectations and preferences in light of new information and political debates (e.g., public discussions concerning the ending of early retirement schemes). This evidence suggests that distributional and generational clashes were not at the heart of the observed resistance to raising the retirement age.

The next section presents an overview of the factors the literature has proposed as driving attitudes to policy reforms, focusing on PAYG pensions. Section 2.3 describes the Dutch pension system and the most important phases in the reform process, while Section 2.4 describes our data. Section 2.5 discusses our results. Given the important role of time, Section 2.6 will inspect the coefficients of our year dummies, qualitatively linking them to some key events in the reform process. Section 2.7 presents several robustness checks, after which Section 2.8 concludes.

2.2 LITERATURE ON THE POLITICAL ECONOMY OF PENSION REFORM

A large theoretical and empirical literature has highlighted a variety of factors that shape individuals' attitudes towards PAYG pension reform. While some authors examine support for raising the retirement age specifically (van Els *et al.*, 2004; Cremer *et al.*, 2008; Scheubel *et al.*, 2013), most studies have considered reforms that reduce the amounts of PAYG transfers. These reforms are of course related: when the level of benefits is kept

unchanged, postponing the retirement age also reduces PAYG transfers. However, in at least two respects raising the retirement age differs fundamentally from other reform margins (larger contributions or smaller benefits). First, for these options to be identical even in an actuarially neutral system, strong assumptions have to be made about the consistency and stability of household preferences and households' use of all available information, conditions that are not easily met (Fatas *et al.*, 2007). [7] Second, and relatedly, several studies have confirmed that the statutory retirement age acts as an important 'focal point' for actual retirement decisions (Behaghel and Blau, 2012; van Rooij *et al.*, 2014). For some households, following the accepted practice even entails significant economic costs (Lumsdaine *et al.*, 1996). Where relevant, we therefore adapt the arguments in the literature to a context wherein the retirement age has a particular 'reference role' for retirement decisions. Below, we will in turn discuss the roles of age, employment status, income, demography, ideology, education, knowledge and the political process itself.

Age plays a predominant role in political economy models addressing the reform of PAYG pensions (for a review, see Galasso, 2014). In the case of demographic pressures on the PAYG scheme, young workers might be less willing to support a retirement program from which they themselves might not benefit. However, because the old grow in number as the population ages, they can block reforms by majority voting (e.g., Sinn and Uebelmesser, 2003) or by lobbying activities (Mulligan and Sala-i-Martin, 1999). On the basis of surveys among Italian, Spanish, French and German respondents, Boeri *et al.* (2001, 2002, 2012) report that the young are indeed more keen to make PAYG pensions less generous, although not all of the evidence is unambiguous (Blinder and Krueger, 2004; Kohli, 2008; EC, 2012). On the basis of a 2003 survey in The Netherlands, Van Els *et al.* (2004) find that support for raising the retirement age was weak overall, while respondents' ages had no significant impact on such support.

Of course, age is only one factor that determines one's stake in PAYG systems and *income* and *employment status* are also likely to be important. As to the former, flat-rate PAYG systems entail a substantial within-cohort redistribution; such is the case in The Netherlands (Bonenkamp and Ter Reele, 2013). Higher-income respondents therefore would be better off when the scheme becomes less generous (Tabellini, 2000). Similarly, one's labour market status is expected to influence preferences towards pension reform (see, e.g., Drissen and van Winden, 1991). Empirical evidence has confirmed this (Boeri *et al.*, 2001; Boeri *et al.*, 2002; Boeri and Tabellini, 2012; Blinder and Krueger, 2004). Boeri *et al.* (2001; 2002), however, find mixed results for the effects of income.

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7. Cremer *et al.* (2008) furthermore show that as soon as a PAYG system entails redistribution, such as in the Netherlands, it is unavoidable that the system includes distortions towards early retirement.

Demographic factors also can shape preferences for pension reforms because decisions on retirement often are made in a family context (Leroux *et al.*, 2011; van Rooij *et al.*, 2014). Indeed, empirical studies have confirmed that married respondents are less supportive of pension reform (see, e.g., Grip *et al.*, 2012; Boeri and Tabellini, 2012). Furthermore, other studies have found that women are less likely to favour pension reforms that reduce public transfer payments (Boeri *et al.*, 2002; Blinder and Krueger, 2004; Boeri and Tabellini, 2012; Scheubel *et al.*, 2013). Goerres and Tepe (2010) find that older respondents are more likely to support public child-care provision – which they pay for, but will not use – if they are in close contact with their adult children. Likewise, people with grandchildren could be more likely to value the future sustainability of public pension programs. Finally, support for pension reform may also depend on respondents' health status (McGarry, 2004; Scheubel *et al.*, 2013).

Empirical studies of attitudes towards pension reform also often include a self-reported measure of *ideology* on the left-right scale. Boeri *et al.* (2001; 2002) find that political ideology systematically drives preferences for reform; Blinder and Krueger (2004) even conclude that ideology is the main factor underlying reform preferences. Ideology can be a rational basis for voting as it can simplify choices in the context of a multidimensional issue space. Most obviously, left-wing parties typically are seen to cater to the working class (although, according to some, this class divide has become less pronounced, see, e.g., Jansen *et al.*, 2013).

Recent research also has found that deep-rooted *psychological traits* influence ideological positions, which are sometimes at odds with economic self-interest (Jost *et al.*, 2009; Gerber *et al.*, 2011). For instance, studies have shown that individuals ranking high on 'conscientiousness' – the extent to which people think of themselves as organized and keeping their promises, one of the so-called 'Big Five' personality traits – tend to be more conservative, both in social and economic terms. Although still in its infancy, this strand of literature links such traits directly to political attitudes and outcomes. For example, Alesina and La Ferrara (2005) hypothesize and confirm empirically in a US sample that *risk averse* individuals value redistribution more than others do (see also Verbon and Van Winden, 1985). Furthermore, on the basis of both the US and World Values Survey, Alesina and Giuliano (2011) find that respondents who believe that luck is a more important driver of success than hard work, have stronger preferences for redistribution. These results are corroborated by Kouba and Pitlik (2014).

Empirical studies typically also find that *education* helps explain preferences for pension reform, holding income constant (see, e.g., Boeri *et al.*, 2001). Education might influence preferences for pension reform through several channels. More highly educated

individuals might enjoy greater *job satisfaction* and might be more optimistic about their future employment prospects (Scheubel *et al.*, 2013). Another factor correlated with education is *knowledge* about the pension system. When respondents simply are unaware of rising pressure on the public pension system because, for instance, they do not realize that employers offer lower wages to help finance their own contributions to the program, they might well support the status quo of a stable retirement age and stable benefits, even if that is at odds with their interests (Browning, 1975; Cremer and Pestieau, 2003). Boeri *et al.* (2001; 2002) confirm that households underestimate the costs of public pensions, sometimes quite dramatically so.

Households' lack of knowledge creates opportunities for discretion for policymakers. In the more stylized median voter models, politicians merely mirror the preferences of their constituencies in order to maximize their votes. Yet, political elites can be expected to have better information on the urgency of budgetary pressures than the electorate does and, as such, can support unpopular policies (Pierson, 1996; Cukierman and Tommasi, 1998). At the same time, of course, politicians seek votes. The fact that retrenchment of broadly valued welfare programs happens nonetheless, has inspired a large body of literature on the conditions under which governments can overcome resistance to reforms, such as 'blame avoidance' and careful framing of the need for reform (see, e.g., Pierson, 1996; Cox, 2001; Green-Pedersen, 2002; Arza and Kohli, 2011; Vis, 2016). Elected public officials and bureaucrats may influence the reform process. On the one hand, bureaucrats can hamper reforms when, for instance, they are motivated to maximize their budgets (Niskanen, 1971). For the US social security program, Congleton and Shughart (1990) find that the scope for budget-maximization of PAYG is limited, given the relatively simple and transparent structure of its administration. On the other hand, bureaucrats can facilitate reforms when elected politicians use them to avoid the blame for unpopular measures (Marier, 2005). In The Netherlands, politicians used a similar strategy to reform the disability insurance scheme by, e.g., referring to reports by independent agencies and by delegating programmatic design changes to trade unions and employer organisations (Vis and Van Kersbergen, 2013).

Finally, scholars also have investigated the extent to which economic crises help or hinder the adoption of reforms. On the one hand, in crisis times voters may find the social safety net to be more valuable than otherwise (Vis *et al.*, 2011; Galasso, 2014). On the other hand, politicians can use a budgetary crisis to avoid blame for unpopular reform initiatives (see, e.g., Vis, 2016). Furthermore, if voters punish politicians for an economic downturn (retrospective economic voting), elected officials may be more likely to pursue unpopular

reforms because re-election already is less likely (Hollanders and Vis, 2013). Empirically, studies tend to confirm that crises facilitate the adoption of reforms (Pitlik and Wirth, 2003; Agnello *et al.*, 2015).

Our dataset allows us to complement this large body of literature as follows. First, given information on a large set of individual characteristics and a relatively large number of observations (over 12,000) we systematically can disentangle the marginal effects of various individual covariates mentioned above. Second, and importantly, we can trace the development of household preferences for reform over a ten-year time span. Our data start in 2004 when fiscal subsidies for early retirement were being phased out and cover several years of political discussions about raising the retirement age, until its implementation in 2012. This time dimension plays a critical role, and although we cannot make bold statements about the causal impact of particular events, these time effects give us some suggestions on decisive moments in the public acceptance of reforms.

2.3   THE DUTCH PUBLIC PENSION AND THE REFORM PROCESS

The focus of this chapter is the flat-rate public pension system (*Algemene Ouderdomswet*, 'AOW'), which is financed on a pay-as-you-go basis. In addition to the AOW, almost 90% of all workers are covered by a funded earnings-related occupational pension that takes the AOW benefits into account in their pension plans. At the start of the AOW in 1957, the statutory retirement age was set at 65. Yet, until the early 2000s most workers left the workforce sooner, supported by fiscal incentives with the ill-guided goal of releasing jobs for the young (Gruber and Wise, 2008). In the meantime, demographic tensions put ever more pressure on PAYG. The old-age dependency ratio rose from just below 16% in 1957 to 20% in 1985, 25% in 2010 and 30% in 2015. That demographic trend was not unanticipated and already at the time the AOW was created, the finance minister noted that, '*should the burden become too heavy in the future, then relief is possible by raising the statutory age of retirement, an option that well suits the increase in the average age and a better physical condition of the elderly*'.[8] Yet, it was not until more than 50 years later, in 2009, that the Dutch government seriously proposed raising the retirement age.

To appreciate the reform process, some features of the Dutch political processes are worth mentioning. The Netherlands has a multi-party system wherein coalition governments generally consist of two or three parties. In the period under study (2003-2013), six different coalition governments were in place, consisting of one or two central parties (the Christian-democrats, conservative liberals and social-democrats) and, typically, the support

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8. Own translation of Explanatory Memorandum, Kamerstukken II 1954-1955, 4009, No. 3, chapter 8.

of one smaller party either to the left or right. According to Green-Pederson (2002), the centrist parties facilitated the emergence of inter-party policy consensus, thereby reducing the risk that parties' justifications for retrenchment would be challenged by other parties. It is also worth noting that labour unions and employer organizations are consulted by the government on major socioeconomic reforms. The government is obliged to give a formal appreciation of the proposals made by a tripartite body of trade unions, employer organizations and independent experts, the Social and Economic Council (SER). That requirement also applies to the reports of other important advisory bodies, such as the Scientific Council for Government Policy (WRR) and the Bureau for Economic Policy Analysis (CPB), which play important roles in analysing the economic and budgetary effects of reforms under discussion (Hemerijck and Visser, 1997; den Butter and Mosch, 2003).

Raising the retirement age has been on the political agenda on several previous occasions. In the mid-1980s, a high-level committee advised raising the retirement age (Commissie Drees, 1987). In 1993, the WRR echoed this advice (WRR, 1993). However, political parties were not keen on proposing changes to the AOW and instead resorted to more passive reforms, such as raising employees' and employers' contributions – and since the late 1990s, transfers from the general budget – and suspensions of the policy of indexing pension benefits. According to Green-Pederson (2002), one reason why no serious reforms were put on the table in the pension sphere, as opposed to other contentious policy domains, was that the dominant discourse for retrenchment in the 1990s was to combat retirement *before* the statutory age.

In the 2000s, raising the retirement age received renewed attention. In 2006, well before the financial crisis, both CPB, WRR and a high-level committee of civil servants issued reports on the budgetary costs of population ageing, several of which hinted at raising the retirement age. In the general elections later that year, raising the retirement age for the first time featured in two party platforms – by the smaller social-liberal party and the Green party. Furthermore, in November 2007, the minister of Social Affairs invited a high-level committee to propose measures to strengthen labour-market participation (Bakker Committee). In June 2008 this committee advised raising the retirement age gradually to 67 in 2040. In its official response later that month, the responsible minister concurred that raising the retirement age 'seemed inevitable' (in 1993 he had in fact chaired the WRR Committee that had advised raising the statutory retirement age gradually). Yet, he maintained that the government's stance was to 'avoid this inevitability' by, e.g., raising labour-market participation rates before the age of 65 and improving public finances by other means.

In 2009, the government did propose raising the retirement age as part of a reform package to weather the economic crisis, but it took some time before the measure was finally agreed upon. After protests by the main trade union federation, relevant interest groups were first invited to formulate an alternative that would realize the same budgetary gains. Partly due to strong opposition from the trade unions' constituencies, they failed to deliver by their deadline of October 1. A few days later, the government proposed raising the retirement age to 66 in 2020 and to 67 in 2025. However, in early 2010 the coalition government fell and interest groups again started to negotiate in anticipation of new government plans. In early June of that year, several days before the elections, they reached an agreement to raise the retirement age in one step to 66 in 2020 and – if needed – to 67 in 2025. The new (minority) government supported that plan, even though it relied on the support of a populist party (PVV) that had opposed the reform strongly before the elections.[9] The final act of the reform process started in spring 2012, when the government had to pursue consolidation measures to comply with European budgetary rules. With the support of two other parties, the government proposed raising the retirement age in small increments already in 2013. It was this 'Spring Agreement' that was finally implemented.

A preceding series of reforms is also worth mentioning. Given the high frequency of early retirement, a previous step involved ending fiscal subsidies for such action. In the early 2000s, the government first suggested phasing out fiscal subsidies, while starting negotiations with relevant constituencies. After massive trade union protests in October 2004, pressure groups and the government struck a deal to end subsidies for early retirements, using lenient transition periods to limit the costs for the unions' older constituents.

Figure 2.1 summarises the most heated debates in the reform process, highlighting Google search activity on some key related search terms. A first peak occurs in summer 2005, when following the phasing out of fiscal subsidies for early retirement schemes, trade unions and the government reached an agreement on a transition scheme for civil servants (see Grip *et al.*, 2012). Furthermore, in September 2006 search activity for 'ageing' reaches a peak, just after the publication of several reports on the impact of ageing on social safety net programs (see above). Furthermore, search activity on the 'statutory retirement age' peaks in October 2009, after various interest groups failed to propose an alternative to raising the retirement age. Finally, searches for 'retirement age' peak in November 2012, following the 2013 launch of an awareness publicity campaign.

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9. Some powerful members of the main trade union federation (*Federatie Nederlandse Vakbeweging*, 'FNV') remained opposed to this agreement and demanded the resignation of the federation leader, which happened in 2012.
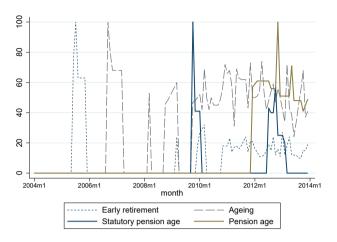
Figure 2.1 Monthly Google search activity on pensions, 2004-2013

Notes: Each line shows the relative search activity on the respective term over time. A peak of 100 indicates when in the 2004-2013 period the term was used most.
Source: Google trends (available from January 2004).

2.4 DATA DESCRIPTION

We use data from the DNB Household Survey (DHS), conducted by CentERdata at Tilburg University. The panel includes approximately 2,000 households, which are representative of the Dutch population, from which one or more household members take part. A majority of respondents stays in the panel year after year; in case of attrition, new participants with similar characteristics are recruited. Throughout the year, respondents in the panel are presented with various surveys that include questions on a broad set of issues such as demographics, occupational status, education, earnings, wealth, health and psychological attitudes. Among other things, the panel has been used to study financial market and savings behaviour (see, e.g., Guiso *et al.*, 2008).

Our main area of interest concerns respondents' answers to a question on reforming the public pension scheme, which has been posed since 2004: '*To make sure that the general old-age pension remains affordable certain measures have to be taken. Which of the following measures appeals to you most?'* Three answers are possible: i) '*a lower general old-age pension at the age of 65*', ii) '*an increase in the old-age pension premium for people working*', and iii) '*increase the age by two years (from 65 to 67 years of age) at which one will receive the general*

old-age pension'.[10] Respondents could list both a first and a second option. As discussed above, we are mostly interested in the attitude of respondents towards an increase in the retirement age (option iii). Unlike the other two measures, the retirement age has an important reference value in retirement decisions (Section 2.2). Furthermore, while in the past policymakers had resorted to both raising contributions and cutting benefits, politicians started to seriously consider raising the retirement age only in the late 2000s (see Section 2.3). In addition, as an auxiliary variable we will also analyse respondents' expected retirement ages (available from 2003), as that answer can help complement our understanding of the reform process. For instance, if respondents over the years systematically expect to work longer, this may indicate that they are aware of (or expect) policy adjustments. The wording of the question is: '*At what age do you expect to retire, or make use of the early retirement arrangement?*'.

Table 2.1 reports summary statistics for our dataset. The first variable, *preference_aowage_up*, is a dummy taking the value of 1 when the respondent lists raising the retirement age as the first preferred option for AOW reform (in Section 2.7, we will also discuss the results for the other reform options). On average, this comprises 29% of the observations. As to the second variable, *expected_retirementage*, the sample is smaller, as this question is not posed to all respondents.[11] Table 2.1 shows that of those 9,278 respondents listing an expected age of retirement, the average is 63.5. Table 2.1 also lists the summary statistics of all of our empirical models' explanatory variables, while Table A.2.1 in the Appendix offers a description of all variables used and Table A.2.2 presents the summary statistics by year. Before discussing the individual covariates, it must be noted that some values are missing. These missing values are associated with other modules of the questionnaire that respondents have not answered. Where possible, we have imputed missing values by entering the mean of the observations of the respondents from previous and following years when they participated in the same survey. This particularly applies to the psychological traits, which were not asked of respondents in all years.[12] While at most 8% of values remain missing for a single variable, with all these missing values combined in our full

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10. In 2012 – when the government had decided to raise the statutory retirement age – the last option was asked without reference to 65 and 67; in 2013, reference is made to the recent increase in the statutory retirement age.

11. In addition, some respondents indicate that the question is not applicable to them. We focus on respondents who are still active in the labour market or who still have not entered it. Furthermore, we report missing values in some rare cases for which the expected retirement age was below 40 (this does not influence our results).

12. In 2003, none of the psychological concepts feature in the questionnaire. Furthermore, *locus of control* is not included in the years 2004, 2008, 2010 and 2012. Finally, starting from 2010, the module on *patience* is only asked to respondents who have not yet answered it. When applying a stricter imputation method - imputing missing values only with the value observed in the closest year this question was asked - our sample is reduced by around a third. In the regression of the expected retirement age some variables lose statistical significance (e.g., the dummies for the third wealth quartile, higher vocational training and risk aversion). All other results are largely the same.

model we end up with subsamples of 87% (in the case of the expected retirement age) and 89% (in the case of preferences for AOW reform) of the total sample. In Section 2.7, we test whether the treatment of missing values influences our results.

Turning to the individual covariates in Table 2.1, most are straightforward and have been used in other studies reviewed in Section 2.2 so we restrict our discussion to the less obvious ones at the bottom of the table. First and foremost, a unique feature of our dataset is the inclusion of respondents' scores on four personality traits, all of which have been

Table 2.1 Summary statistics, 2003-2013

	count	mean	s.d.	min	max
Dependent variables					
preference_aowage_up	13723	0.29	0.46	0	1
expected_retirementage	9278	63.51	3.78	40	98
Covariates					
age	14722	46.31	11.61	16	65
employee	14722	0.69	0.46	0	1
self_employed	14722	0.06	0.24	0	1
household	14722	0.09	0.29	0	1
benefits	14722	0.12	0.32	0	1
other	14722	0.04	0.20	0	1
grossincome (1,000 EUR)	14003	32.50	24.20	0	582.5
financial_wealth (1,000 EUR)	14259	30.90	83.23	-68.9	3702.1
female	14722	0.46	0.50	0	1
married	14722	0.64	0.48	0	1
child	14722	0.54	0.50	0	1
grandchild	14722	0.13	0.33	0	1
health	14322	3.90	0.67	1	5
low	14722	0.23	0.42	0	1
preuniversity	14722	0.09	0.29	0	1
vocational_med	14722	0.21	0.41	0	1
vocational_high	14722	0.29	0.45	0	1
university	14722	0.18	0.38	0	1
riskaversion	13900	5.25	1.00	1.3	7
patience	14089	4.13	0.58	1.5	6.8
conscientiousness	14085	4.42	0.58	1.8	5.8
locus_of_control	13503	4.55	0.63	1.8	7
frequency	14722	3.51	2.54	1	11

Notes: In 2003 respondents were inquired about their expected age of retirement (999 observations) but not yet about their preferences for AOW reform.
Source: DHS, 2003-2013.

constructed and validated in psychological research. For all four concepts, respondents need to rate (on a one-to-seven scale) the extent to which they agree with several statements (some are reverse coded). See Table A.2.3 in the Appendix for the statements used.
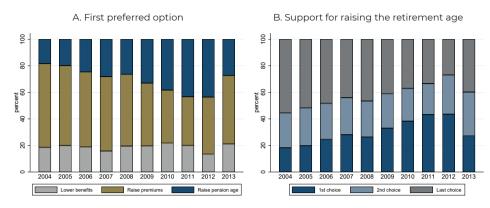
Table 2.1 reports the average score on all questions/statements. Although not all concepts have been linked systematically to PAYG reform, the literature reviewed in Section 2.2 offers inspiration for our hypotheses. First, following Alesina and La Ferrara (2005), we can expect *risk averse* individuals to attach more value to redistribution and, hence, to prefer raising payroll contributions over raising the retirement age (and over cutting benefits). The role of the statutory retirement age as a focal point is also likely to be more relevant for risk-averse individuals. On the other hand, *patient* individuals have been found more systematically to save some of their own earnings (Sutter *et al.*, 2013; Hastings and Mitchell, 2020). As such, the importance of the official retirement age as a focal point might be less important and they might be less opposed to a later retirement age. Third, as noted in Section 2.2, *conscientious* individuals attach more value to keeping obligations and display stronger support for the status quo (no change to the retirement age).[13] Also, given the role of the retirement age as a focal point for retirement, we expect more conscientious individuals to display less support for postponing that age. Fourth, *locus of control* measures the extent to which an individual believes in his or her own control over lifetime economic outcomes, in contrast to having those outcomes determined by luck or fate. Following Alesina and Giuliano (2011) and Kouba and Pitlik (2014), such individuals can be expected to attach less value to redistribution and, hence, to prefer raising the retirement age (or cutting benefits) over raising contributions. As with patience, the official retirement age as a focal point might be less important.

The last row of Table 2.1 shows that respondents on average participated in 3.5 surveys ('frequency'), while some individuals even participated over the entire period (11 years). This feature of the data allows us to correct for the possibility that year effects are driven by changes in the composition of the sample.[14] On the other hand, it is also possible that individuals who remain in the panel are 'treated' by answering this question several times. We will test for these possibilities in Section 2.7.

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13. Conscientiousness is one of the so-called 'Big Five' personality traits. The other Big Five personality traits only featured in DHS as of 2013, and are included in the analyses reported in Chapters 3 and 4.

14. One concern is that until 2012, the mean age of respondents rose gradually. In 2013, the average age dropped by several years, owing to CentER's oversampling of younger households to correct for the ageing of the sample in prior years.

Figure 2.2 Preferences for AOW reform

A. First preferred option   B. Support for raising the retirement age

Notes: Panel (a) shows the percentage of respondents listing each option as their first choice for AOW reform. Panel (b) shows the percentage of respondents listing an increase if the retirement age as 1st, 2nd or last preferred reform option.

Finally, a crucial dimension of our dataset is that we can trace households' expectations and preferences over ten years. Focusing on preferences, the first panel of Figure 2.2 shows the first preferred option of respondents for ensuring sustainability of the public pension scheme. The figure shows a gradual increase in support for a higher retirement age. This greater support for raising the retirement age came especially at the expense of support for raising payroll tax premiums; the support for lower benefits remained remarkably constant at around 20%. In 2013, when the statutory retirement age was raised, support for a further increase in the retirement age dropped, although staying at a higher level than in 2004. The second panel of Figure 2.2 lists the support for raising the retirement age as respondents' first, second and last option. When the second preferred option also is entered, support for raising the retirement age increased from more than 40% in 2004, to more than 70% in 2012.

2.5   MAIN RESULTS

Specification

We will now assess the impact of various covariates on both the expected retirement age and support for raising the retirement age. In both regressions, we enter the same characteristics as covariates, except the dummies for working in the household and receiving benefits; not all of those individuals are asked about their expected retirement age (see footnote 11). We pool observations on all respondents and years together, exploiting both variation between respondents and over time. We include time dummies in all regressions and

cluster standard errors at the household level. The expected retirement age is a continuous variable and is estimated by OLS. The preference for raising the statutory retirement age is measured on a two-unit scale (1 = first preference for reform; 2 = the second preference) and is estimated with a probit regression, for which we report marginal effects at the variable's mean. In Section 2.7, we test whether those results are robust to a fixed effects panel regression of the individuals who take part in the survey for several years, as well as several other robustness checks.

Of all the covariates listed in Table 2.1, most have low correlations and of the more than 200 pairwise correlations, only one exceeds 0.5, the threshold often used as a rule of thumb for problems of multicollinearity. That correlation is between being married and having a child (R^2 = 0.81). We decided not to enter the dummy for having a child, while we keep the dummy for being married and for having a grandchild.

Finally, we note some choices on the functional form of our regressors. First, we include age, income, wealth and education in dummy categories as they are not necessarily related linearly to expectations and preferences. Second, we standardize the scores on the psychology traits and on health so that they have zero mean and a standard deviation of one.[15]

Results

The results of our two regressions are reported in Table 2.2. Coefficients for the time dummies are suppressed but will be discussed in the next section. We first discuss the results for *age*, which plays a dominant role in political economy models of PAYG reform. Several results stand out. First, respondents below 35 of age expect to retire significantly earlier than the reference group of respondents, aged from 35 to 45. This result could suggest that young households were more naïve in their assessments of future retirement policies.[16] Preferences do not differ among the youngest respondents and the reference age group (35 to 45), while those aged between 45 and 55 report significantly less support for raising the retirement age than the younger respondents, in line with their economic interests. Interestingly, this finding does not hold for respondents aged 55 to 65. The

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15. Note that we standardize variables by taking the mean and standard deviation *per year*. Although personality traits typically are assumed largely to be time-invariant, some evidence exists that they can fluctuate mildly, particularly when it comes to risk aversion (see chapter 2 of Salamanca, 2015).

16. When running the regression of the expected age of retirement in two sub-periods (before and after 2007) we find that in the first period young respondents (below 35 years of age) were expecting to retire significantly earlier than the reference group (35-45 years), while this effect is not statistically significant for the years after 2007. This is consistent with the idea that economic and political events gave young people, in particular, a 'wake-up call' on what future retirement policies they could expect.

relative indifference of this latter group to raising the retirement age might be explained by the fact that a significant share of them already is in early retirement. Alternatively, they may not be affected owing to the transition periods featured in the reform proposals.

The results furthermore show that *occupational status* matters for reform preferences (but not for expectations). Current employees (the reference group) not surprisingly are most hostile to raising the retirement age, while the self-employed and respondents who receive benefits (for either disability or for retirement) are significantly more favourable toward raising the statutory retirement age. Those effects are rather large (10 and 11 percentage points, respectively). Self-employed respondents might be particularly hostile to higher payroll contributions, as they are more aware of the premiums they pay than are employees. Respondents who receive (disability or old-age pension) benefits might worry about the benefit level of their pensions and the eroding support for larger pension contributions among the active workforce (in Section 2.7, we report results from probit regressions on the other two reform options, confirming that both groups are especially concerned about larger payroll contributions). Finally, income and wealth do not seem to matter,[17] except for respondents in the two highest wealth quartiles, who expect to retire significantly earlier than respondents in the quartile with the lowest wealth (including negative wealth).

Turning to *demographic factors*, females expect to retire a year earlier than men. This can be explained by women having older partners and planning to retire at the same time. Married respondents also expect to retire earlier and also are significantly more hostile to raising the retirement age. Having grandchildren, however, strengthens support for postponing the statutory retirement age (though only at the 10% level). This finding can indicate that respondents with grandchildren attach more importance than others to the future sustainability of the pension scheme. Furthermore, healthier respondents expect to retire at older ages and, hence, more often support raising the statutory retirement age.

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17. Note that 'wealth' potentially suffers from endogeneity, as private savings can be tapped for earlier retirement (although the most important pension savings – those in occupational pension schemes – are not included in this variable). We have run the same regressions without the wealth dummies. The results are identical, except for the estimated coefficients on some age dummies in the OLS regression of the expected retirement age, whereby the dummies for older respondents seem to pick up wealth effects.

Table 2.2 Regression results for retirement expectations and support for reform

	(1) Expected retirement age		(2) Increase in the retirement age first reform preference	
	OLS		Probit	
Age (reference group: aged 35 to 45)				
below35	-0.48**	(0.23)	-0.01	(0.02)
age45to55	-0.19	(0.16)	-0.06***	(0.02)
age55to65	-0.21	(0.18)	-0.02	(0.02)
Employment status (reference group: employees)				
self_employed	0.35	(0.37)	0.10***	(0.03)
household			0.04	(0.03)
benefits			0.11***	(0.02)
other	-0.52	(0.37)	0.02	(0.03)
Income/wealth (reference group: Q1)				
incomeQ2	0.16	(0.37)	-0.01	(0.02)
incomeQ3	0.18	(0.37)	-0.01	(0.02)
incomeQ4	-0.36	(0.39)	-0.02	(0.03)
wealthQ2	-0.15	(0.19)	-0.02	(0.02)
wealthQ3	-0.41**	(0.19)	-0.02	(0.02)
wealthQ4	-0.77***	(0.19)	-0.03	(0.02)
Demographics				
female	-1.01***	(0.15)	-0.02	(0.01)
married	-0.73***	(0.16)	-0.03**	(0.02)
grandchild	0.12	(0.23)	0.04*	(0.02)
health	0.20***	(0.08)	0.02***	(0.01)
Education (reference group: low education)				
preuniversity	0.24	(0.29)	-0.00	(0.03)
vocational_med	-0.17	(0.22)	-0.01	(0.02)
vocational_high	0.57***	(0.22)	0.02	(0.02)
university	0.78***	(0.25)	0.11***	(0.02)
Personality traits				
riskaversion	0.12*	(0.07)	-0.02***	(0.01)
patience	0.07	(0.08)	0.02**	(0.01)
conscientiousness	-0.17***	(0.07)	-0.01**	(0.01)
locus_of_control	-0.13	(0.08)	0.02***	(0.01)
_cons	63.45***	(0.49)		
N	8103		12176	
(Pseudo) R^2	0.126		0.0563	
Time dummies	Yes		Yes	

Notes: In parentheses we report standard errors, which are clustered at the household level. For the probit regression (column 2) we report marginal effects measured at the mean. The symbols *, ** and *** denote significance at the 10, 5 and 1 percent confidence level, respectively.

Furthermore, with respect to *education*, respondents who attended university or have more vocational training expect to retire seven to nine months later than others. Interestingly, support for raising the retirement age is 11 percentage points higher among university graduates than among the lowest-educated respondents (the reference group), but does not significantly differ for other educational groups. In line with the discussion in Section 2.2, university graduates might be more supportive of a later retirement age because of greater job satisfaction, but also because of more knowledge about the budgetary pressures of an ageing population (note that income is held constant).

The last lines of Table 2.2 show regression results for our four measures of *personality traits*, which are in line with our hypotheses outlined above. First, risk-averse respondents are less supportive of raising the retirement age. Second, conscientious individuals expect to retire earlier and also are less supportive of raising the retirement age. Furthermore, patience and locus of control also matter for reform preferences. To be precise, someone who has one standard deviation more locus of control (patience) than the average, is 2% more likely to support postponing retirement.

Goodness of fit

The goodness of fit of our models is not very high, as is often the case with this kind of survey data (Boeri *et al.*, 2001; Boeri *et al.*, 2002). Clearly, it is difficult to attribute expectations and preferences to observed characteristics even with a rich set of respondent characteristics. Still, we have identified above many factors that affect expectations and preferences significantly. Table 2.3 summarises the goodness of fits of both models and the explanatory shares that can be attributed to various subsets of covariates. The regressors' shares in the total R^2 are calculated using the Shorrocks-Shapley decomposition (Chávez Juárez, 2012). For both the expected retirement age and preferences for PAYG reform, the year fixed effects clearly are the strongest predictors (67% and 50%, respectively). Furthermore, demographic factors explain most of the remaining variation in the expected retirement age, followed by income/wealth and education. As for reform preferences, education, occupational status and personality traits account for most of the remaining variation, while age and demography play a more limited role.[18]

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18. Due to collinearity the *gross* contributions to overall variation (i.e., when not including controls) of these factors can be higher. For instance, when looking at the contribution of age to reform preferences without controls, its contribution increases to 8.9%. Yet, in relative terms the explanatory power of age still ranks 6th after the year dummies, education, personality, occupational status and demographic factors. The results are also by and large the same when we exclude the year 2013, when the reform was already implemented.

Table 2.3 Decomposition goodness of fit

Contribution to goodness of fit of statistically significant regressors in models presented in Table 2.2

	(1) Expectations		(2) Preferences	
	R²	% full model	Pseudo R²	% full model
Age	0.002	2%	0.003	6%
Occupational status	0.001	1%	0.007	12%
Income/wealth	0.008	7%	0.001	2%
Demographic	0.021	17%	0.004	6%
Education	0.008	6%	0.008	14%
Personality	0.004	4%	0.006	11%
Time	0.080	64%	0.027	48%
Total	0.126	100%	0.056	100%

Notes: Decomposition of goodness of fit of the regressions of the expected retirement age and support for an increase in the retirement age. The (pseudo) R^2 is decomposed using the Shorrocks-Shapley method (calculated with Stata's 'shapley2' command, see Chavez Juarez 2015).

2.6 FOCUS ON YEAR EFFECTS

Figure 2.2 already indicated that support for raising the retirement age displayed an upward trend in the 2004-2013 period. In fact, in our regressions, the year fixed effects account for two thirds of the explained fit of the model of the expected retirement age, and half of the explained fit of the preferences for AOW reform. It is therefore informative to look at the coefficients for the time dummies in greater detail. Unfortunately, with ten survey events it is difficult to attribute the rising trend in a coefficient to a single event econometrically. Our approach, hence, is qualitative. We first plot the coefficients, identify some key patterns and then assess what possible explanations would fit these.

Figure 2.3 shows the year fixed effects of the regression explaining the expected retirement age (Panel A) and support for raising the retirement age (Panel B). Note that the year fixed effects are entered relative to the base year 2004 (2003 for the expected retirement age) and that the regression controlled for all the statistically significant covariates discussed above (as in Table 2.3). Furthermore, in addition to the confidence bands, we also have tested whether the estimated year coefficients differed statistically significantly from the previous year. That is the case for all years on the x-axis that are marked in black (i.e., in the left panel, the coefficients for 2003-2004 and 2004-2005 do not differ statistically, but those for 2005-2006 and later do). Lastly, in the interpretation of the year fixed effects, it is informative to know that the surveys were administered in April/May every year.

The panels display some interesting patterns. First, the first panel of Figure 2.3 shows that the expected age of retirement of the respondents was stable over the surveys of 2003-2005, but witnessed a continuous upward trend thereafter, for which the year-to-year increase was statistically significant. Second, support for raising the statutory retirement age (panel b) rose during 2005-2007, but halted in spring 2008, rising thereafter to stabilize in spring 2012. In the 2013 survey, support for a further postponement of the retirement age dropped significantly, although the level of support remained higher than in 2004-2005. Plotting the time pattern for different subgroups (see Figure A.2.1 in the Appendix) actually yields very similar patterns, which suggests that events in this period influenced the majority of respondents in the same way.
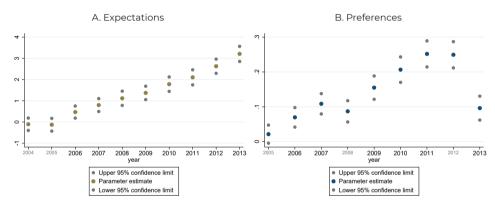
Figure 2.3 Year effects expected retirement age and reform preferences, 2003-2013

Notes: Panel (a) shows the year coefficients from the regression of the expected age of retirement. Panel (b) from the probit regression on preferences for AOW reform (base years are 2003 and 2004, respectively). Grey dots show lower and upper confidence bounds. When the years at the x-axis are marked black, this indicates that the year coefficient is statistically significantly different from the coefficient in the previous year.

What factors can account for these patterns? In Section 2.3, we highlighted some key events in the reform process. Apart from those, economic conditions could also play a role. First, given that part of Dutch PAYG pensions are paid from the general budget, worsening economic conditions and public finances might influence respondents' expectations and preferences for AOW reform. Second, as the first and second pillar pensions are linked (see Section 2.3), it is possible that funding problems in the second pillar affect attitudes towards the PAYG system. Table 2.4 therefore provides an overview of economic conditions just before our surveys were conducted (in the first quarters of every year). It shows that until the 2008 survey, overall economic conditions were developing favourably. Between the

2008 and 2009 surveys, however, GDP dropped substantially and, in consequence, the debt-to-GDP ratio also worsened. Furthermore, the financial positions of second pillar pension funds worsened significantly because of large investment losses, falling interest rates and upward revisions in life expectancy at retirement.

Table 2.4 GDP, government debt and second pension pillar funding rate

Quarter	GDP growth	Government debt	Second pillar funding rate
2003q1	0.9	49.3	114
2004q1	1.1	50.3	121
2005q1	1.3	49.4	134
2006q1	3.1	48.3	144
2007q1	3.6	44.7	140.9
2008q1	3.6	43.3	132.3
2009q1	-4.2	57.3	91.8
2010q1	0.1	57.3	108.2
2011q1	2.6	59.1	111.8
2012q1	-1.6	62.2	98.6
2013q1	-0.9	66.4	106.5

Notes: Year-on-year quarterly growth rate of GDP, government debt as percentage GDP and the average funding rate of Dutch second pillar pensions is expressed as % of liabilities (for years 2003-2006 only annual figures were available).
Source: Statistics Netherlands and De Nederlandsche Bank.

Combining Figures 2.1, 2.3 and Table 2.4 allows us to speculate on the factors that could cause the year fixed effects for respondents' preferences to trend upward. First, greater support for raising the retirement age in the years 2005-2007 is likely related to public discussions around the phasing out of the Dutch early retirement scheme – including the public attention to the reform of the early retirement scheme for civil servants in summer 2005 – and public reports on societal ageing in 2006. Second, Google search activity on ageing and retirement issues was relatively subdued in 2007-2008, which might help explain why the stronger support for a later statutory retirement age ended in early 2008.[19] Third, the gradually rising support for postponing the retirement age after the global financial crisis hit probably reflects a combination of greater awareness of fiscal constraints and more intense public discussions. Finally, after the raising of the statutory retirement age had been decided upon and communicated publicly, respondents expected

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19. An analysis of newspaper articles confirms that in this period public discussions of old age pensions had virtually halted (van der Wiel, 2009).

to retire even later than they had beforehand. At the same time, support for postponing the statutory retirement age further fell, although it remained at levels higher than in 2004-2005.

2.7 ROBUSTNESS ANALYSIS

This section describes several checks on the robustness of the results obtained above. A first concern is that sample attrition is non-random. While we can correct for respondents' observed characteristics, those who participated more than once might also differ on characteristics that we cannot detect. Hence, it is possible that the year fixed effects pick up compositional changes in our sample. To check this, we can exploit the fact that a majority of respondents took part in the survey several times and estimate the time-wise fixed effects with a panel regression wherein we can single out all time-invariant attributes. Figure A.2.2 in the Appendix compares the time coefficients reported above with the time effects resulting from this fixed effects panel regression. Although coefficients differ somewhat, the time patterns described above are the same.

Second, we include a broad set of covariates and given that there are some obvious examples of multicollinearity (e.g., between income and education), the coefficients in our analysis are dependent on the inclusion of other covariates. This means that some coefficients can fluctuate substantially, depending on the model specification, while the coefficients of other variables may be less dependent what other variables are included. To test the stability of our coefficient, we add variables one by one in our regression for support for raising the retirement age (see Tables A.2.4a and A.2.4b in the Appendix). The results reveal that the regression coefficients – and significance levels – fluctuate considerably for the income and wealth dummies and some of the demographic variables. Stability of regression coefficients is observed, conspicuously, for education (i.e., having completed university), occupational status and all four psychological traits.

Third, it also is possible that respondents are 'treated' by answering our questions. For instance, when respondents are required to think about options for AOW reform several times, they could develop concerns about the scheme's sustainability. When the 'frequency of participation' of respondents is entered (see last row in Table 2.1) in our regression, we do not find evidence of such a 'treatment effect' (see Table A.2.5 in the Appendix).

Fourth, we assess the extent to which the year 2013 matters for our conclusions. A rise in the retirement age had been adopted in 2012; hence, at the time of the 2013 survey respondents faced a new situation (in the survey, the question was updated accordingly; see footnote 10). Furthermore, in 2013, a larger proportion of young people participated

in our survey (see footnote 14), who might also differ on non-observable characteristics. However, when 2013 is excluded from the sample, our results essentially are the same (see Table A.2.6 in the Appendix; for an analysis of the years beyond, see Chapter 4).

Fifth, we focused above on support for raising the statutory retirement age. Respondents also could report their support for two other reform margins, namely cutting retirement benefits and raising payroll contributions. Our focus was motivated by the fact that in the period under study, raising the statutory pension age was the most salient reform option, both economically and politically (see also Sections 2.1, 2.3 and 2.4). Yet, the interpretation of our results could be influenced by changes in preferences regarding the two other options. Therefore, we also ran similar probit regressions for them. Overall, the results indicate that more support for postponing the statutory retirement age came especially at the expense of support for larger payroll taxes (see Table A.2.7 in the Appendix). This additional analysis first of all applies to estimates of the year coefficients. Corroborating Figure 2.2, the year effects of the probit regression explaining support for a later retirement age (Figure 2.3, panel (b)) mirror almost perfectly the year coefficients of the probit regression explaining support for raising contributions. Support for postponing the retirement age also comes at the expense of support for increasing payroll taxes when it comes to most individual covariates. Two notable exceptions are age and marital status. While respondents below 35 years of age and over 55 did not display significantly more or less support for a later statutory retirement age, the younger respondents were somewhat more favourable toward cutting pension benefits, while the older respondents tended to favour raising payroll taxes over pension benefit cuts. Furthermore, while married respondents were less anxious about raising the retirement age, they tended to be indifferent towards the payroll taxes, but preferred reducing public pension benefits.

2.8   CONCLUSION

Governments often find it hard to pursue economic reforms, even if they ultimately benefit a majority of voters. The literature is still divided on whether resistance to reform is explained mainly by distributional conflicts between different economic classes – for instance, dividing the young and the old in the case of pay-as-you-go public pension programs – or whether resistance to reforms is instead rather broad-based. Exploiting a unique longitudinal dataset, this chapter has evaluated the explanatory powers of reform attitudes by means of a case study: the 2012 raising of the Dutch statutory retirement age from 65 to 67.

Our most important findings can be summarised as follows. First, individual covariates were important drivers of reform attitudes, but not always as much as expected *a priori*. First and foremost, while age plays an important role in political economy models of PAYG reform proposals that postpone statutory retirement age, it was not found herein to be a major dividing line. Respondents aged 45 to 55 did display relatively strong resistance to raising the retirement age, but that effect is smaller than those of some other covariates. The age of respondents to annual surveys about pension-reform options explained only a small fraction of the observed variation in support for a two-year increase in the official retirement age. On the other hand, we find that at the individual level, education, employment status and personality traits appear to be the most robust drivers of acceptance of reform. Those factors all exert a significant impact on pension reform preferences, explain a fair share of the total variation among respondents, and the estimated coefficients were relatively insensitive to the inclusion of other explanatory variables. Especially for respondents' personality traits, this finding invites further theoretical and empirical work on the precise channels in play, as the literature on personality traits and policy preferences is still in its infancy.

Second, and what is most important, our year fixed effects explain most of the variation in respondents' expectations and preferences about policy reform. During the 2004-2013 period, public opinion gradually grew more favourable toward later retirement. Already in the years 2004-2006, when economic conditions and public finances were still benign, support for raising the retirement age grew steadily. We attribute this finding to the publication of various reports by independent agencies on the budgetary costs of ageing and to public discussions on the ending of subsidies for early retirement. When the global financial crisis set in, the Dutch government did for the first time propose postponing the statutory retirement age as part of a long-term budget consolidation package, igniting several years of public discussion. In those years, expectations and preferences continued to grow increasingly favourable towards working longer and retiring later. Estimating the year effects by age cohorts confirms that a gradual increase in support for that reform materialized among all age groups. These findings suggest that distributional clashes were not at the heart of the observed resistance to postponing the statutory retirement age in The Netherlands. Instead, we interpret our results as a collective learning process in which respondents gradually updated both their expectations and preferences on the statutory retirement age in response to new information and public debates.

Our research offers several guidelines for policymakers. First, in terms of the reform of social safety nets like publicly financed pensions, the most important challenge for reform-minded policymakers seems to be to convince the public at large, rather than countering opposition from particular pressure groups which stand to lose the most. Second, our

results underline the usefulness of reliable and easily understandable information on the financial position of the pension system. Such reports can convince the public of the need for reform and offer vote-seeking politicians an opportunity to justify unpopular public policy measures. Third, our results indicate that personality traits are important for public acceptance of reforms. Given that such traits largely are time-invariant, Kouba and Pitlik (2014) conclude that welfare state reforms can be facilitated only in the long run by educational and social policies that support the independence and self-confidence of people. Yet, appreciation of fundamental attitudes towards reform can offer some lessons in the short run as well. For instance, when people resist reforms because they feel they would lose control of their lives and livelihoods, policymakers should address those concerns by offering the public new perspectives in return. Finally, our results underscore the usefulness of incremental steps in the reform process. Although the Dutch government did propose raising the statutory retirement age only in 2009, the phasing out of early retirement schemes in 2004-2005 appears to have prepared households for a later retirement age as well.

Chapter 3

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POPULIST ATTITUDES, FISCAL ILLUSION AND FISCAL PREFERENCES:

EVIDENCE FROM DUTCH HOUSEHOLDS

3.1   INTRODUCTION[20]

For some decades now, countries across the world have witnessed a surge in the vote share of parties that challenge the political elite. The success of these so-called populist parties has resulted in a fast-growing literature on what defines populist parties and what determines their success. According to political scientists (Mudde, 2004; Mudde and Rovira Kaltwasser, 2017) it is the populist idea set[21] that best defines these challenger parties, which are found at both the right and the left of the political spectrum (Taggart, 2000). This set of ideas holds that there is an antagonistic division between the ordinary people and an 'evil elite' which is not acting according to the popular will (Mudde, 2004; Akkerman *et al.*, 2014). The literature has highlighted several drivers of the success of these parties, both at the supply side (the political structure) and the demand side (voters' grievances). On the supply side, cognitive mobilization and commercialisation of the media have made it less self-evident that people merely follow the political elite (Mudde, 2004). In addition, with the decreasing importance of ideology and religion, mainstream parties have become more similar, thereby creating opportunities for (populist) challenger parties to distinguish themselves (Keman and Pennings, 2006). On the demand side, unaddressed grievances of the electorate have been found to give rise to populist voting. What are the most important grievances is still debated, although anti-immigration and anti-EU sentiment are very common among populist parties and their voters (Ivarsflaten, 2008; Otjes and Louwerse, 2015; Spruyt *et al.*, 2016).

While initially focusing on the causes of the populist success, political scientists have recently started to assess its consequences. Overall, their starting point is agnostic. On the one hand, by catering to unaddressed grievances, the rise of populism has the potential to act as a corrective mechanism and even improve the functioning of democracy (Rovira Kaltwasser, 2012; Mudde and Rovira Kaltwasser, 2012). At the same time, by placing a large weight on 'the will of the people', populism can have negative consequences for democratic checks and balances and the position of minorities (Rooduijn *et al.*, 2016). Furthermore, research has highlighted that the populist rhetoric can be reinforcing. Whatever its cause, once the idea becomes widespread that the elite does not act in the interest of the ordinary people, this will also cause people to view politics in such a way (Rooduijn *et al.*, 2017; Hooghe and Dassonneville, 2018). Furthermore, with the increasing role of populist parties in Parliament and government, political scientists have

started to assess the consequences of populism for a diverse set of policy domains. A few papers have also assessed the impact on welfare state policies (Afonso, 2015; Otjes and Louwerse, 2015; Röth *et al.*, 2018).

Inspired by episodes of exploding public debt and hyperinflation in most notably Latin America, economists have traditionally characterised 'populism' as a threat to sound economic policymaking (Dornbusch and Edwards, 1991). It is well documented that the public is often poorly informed about economic facts and mechanisms (Boeri *et al.*, 2001; Caplan, 2002; van der Cruijsen *et al.*, 2015; Guiso *et al.*, 2017). Hence, many economists and policymakers fear that populist parties can exploit the poor understanding of voters and attract votes by promising simple solutions that are not sustainable in the long run (Dornbusch and Edwards, 1991; Andersen *et al.*, 2017; Guiso *et al.*, 2017; Davidson, 2018). As such, according to Acemoglu *et al.* (2013), the populist policy agenda ultimately hurts the economic interests of the majority. When it comes to fiscal policy, the 'populist economic agenda' is said to be characterized as expansionary, while neglecting the negative consequences of debt accumulation and inflation (Andersen *et al.*, 2017). What could reinforce these concerns, is that people who adhere to the populist rhetoric, might also be sceptical of experts (Taggart, 2000), as manifested in discussions in the wake of the Brexit referendum as well as the election of Donald Trump. Indeed, recent scholarship has found that populism is correlated with anti-intellectualism – a measure that includes attitudes towards economists – and that respondents with strong anti-intellectualist sentiment might even increase their opposition when confronted with expert advice (Merkley, 2020).

Despite the concerns of economists over the unsustainable nature of the populist economic agenda, the literature has so far not yet rigorously studied what the surge of populist sentiment – understood as the pervasiveness of ideas of an antagonistic division between the elite and the people – implies for fiscal preferences. A more rigorous assessment is needed for at least two reasons. For one thing, the concern that populist leaders might win votes with short-term policies rests on the assumption of fiscal illusion, i.e. voters underestimating the (future) costs of public programs in terms of taxation or debt (Buchanan and Wagner, 1977; Alesina and Perotti, 1995). Yet, while in some countries politicians may indeed succeed in misleading their voters with expansionary policies that are not sustainable, the empirical support for fiscal illusion – and the deficit bias that goes with it – is not strong, especially in those countries where voters are more sophisticated (Eslava, 2011; Alesina and Passalacqua, 2016). Secondly, recent research has highlighted various examples of economic policies that may have been unresponsive to the concerns of a large share of voters. This goes, most notably, for the disruptive labour market effects of trade with China, which has been found to play a causal role in the electoral success of populist parties (Autor *et al.*, 2016; Colantone and Stanig, 2018; Colantone and Stanig,

2019). In a similar vein, populist parties may also cater to voters who hold intrinsically more expansionary preferences than the political elite. In line with this, Piketty (2020) mocks the tendency of mainstream political actors to label parties 'populist' merely because they propose policies that they deem too radical.[22]

The aim of this chapter is to assess whether people who hold strong populist ideas also report more expansionary fiscal preferences, and to what extent populist attitudes reinforce the risk of fiscal illusion. Our survey is set in the Netherlands, a country with a strong tradition of prudent fiscal policy and a literate population which on average holds relatively conservative fiscal preferences. We measure fiscal preferences by asking respondents how they would use the tax windfalls that were foreseen at the time of the survey (September 2017), for debt reduction, tax relief and/or more spending. In turn, we measure the extent to which individuals adhere to populist ideas by a tested index of individuals' 'populist attitudes' (Akkerman *et al.*, 2014). We assess the role of fiscal illusion by including a measure of the literacy of respondents, and by means of an experiment in which we treat a random share of respondents with information about public debt dynamics.

This chapter makes three main contributions to the literature. First, we regress fiscal preferences on populist attitudes at the individual level. To our knowledge, this has never been done before, and we find that populist attitudes of respondents prove a very relevant predictor of their fiscal preferences. Our results are robust to the use of an IV estimation, in which we instrument populism with pre-crisis trust in politics and the financial sector. Second, in line with previous research, we find that literacy and information provision – which can alleviate the occurrence of fiscal illusion – contribute to more prudent fiscal preferences. Third, to assess whether populist attitudes reinforce the risk of fiscal illusion, we evaluate to what extent populist sentiment moderates the effect of literacy and information on fiscal preferences. We find that the effect of literacy is conditional on the level of populist sentiment when it comes to support for tax relief. To be precise, poor literacy only spurs support for tax relief when respondents hold relatively strong populist attitudes. Furthermore, when it comes to support for more spending, we find that

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22. As an example, in the resistance of the French 'gilberts jaunes' to a gasoline tax increase to finance climate measures, protesters contend that the political elite forgets that many people have trouble making ends meet. 'Some people can afford to think about the end of the 'system', but most of us just worry about how to cope until the end of the month.' (The Guardian, 12 Dec 2018). According to Piketty (2018), the fact that the French government at the same time abolished the wealth tax, makes the protest of the yellow vests a strive for fiscal justice. These same yellow vests, together with the Lega Nord and M5S parties in Italy, have also proposed referenda on the cancellation of debt. While Piketty does not deem a referendum the most adequate means, he sees the idea as a necessary and unavoidable impetus to a public debate about ways to reschedule public debt in Europe (Piketty, 2020: p. 963).

the effect of our information experiment is larger for respondents with stronger populist attitudes, suggesting that information provision can also alleviate fiscal illusion with voters who are sceptical of the establishment.

This chapter proceeds as follows. The next section reviews the literature on populist attitudes and fiscal preferences and presents our hypotheses. Section 3.3 describes our research design. Section 3.4 presents the main results of this chapter, i.e. the relationship between populist attitudes and fiscal preferences, as well as the impact of literacy and information provision. Among other robustness tests, Section 3.5 assesses whether our results are robust to an IV estimation in which we instrument populism with pre-crisis trust in national politics and the financial sector. Section 3.6 assesses to what extent populist attitudes reinforces the effect of literacy and information provision on fiscal preferences. Section 3.7 concludes.

3.2   SELECTED LITERATURE REVIEW AND HYPOTHESES

Attitudes towards the political elite and fiscal preferences

The starting point for our hypothesis on the relationship between respondents' populist attitudes and fiscal preferences is given by the model of Cukierman and Tommasi (1998). It consists of three premises that we deem rather realistic. First, public policy is complex, as outcomes do not only depend on government policies but also on external circumstances. Second, politicians have a comparative advantage in economic policymaking, as they have more information and access to expert judgement on the state of the world. Last, the model holds that voters cannot observe the ideology of politicians.[23]

In such a setting, voters will condition their evaluation of economic policy proposals on their judgement of the political elite's ideology. When people believe that the political elite acts according to the people's will, they are more likely to support their plans. Arguably, this mechanism greatly facilitated the job of political and economic elites in the days when there were strong ideological and religious ties between the electorate and the elite (e.g. in the period of strong 'pillarization' in the Netherlands). Yet, when for whatever reason people believe that the political elite is not acting in the people's interest as it should, they may be sceptical of the elite's policy proposals, especially if these policy proposals align with their views of the elite. Models in the public choice literature typically consider politicians as self-serving, trying to extract from public funds as much rents as possible (Tullock *et al.*,

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23. The model of Cukierman and Tommasi continues with the prediction that under certain circumstances, 'unlikely' parties will be more successful in implementing substantial policy reform, as their proposal of such policies can be more credible.

2002). If voters fear that elites will use public funds for their own means, they may hence favour a lean government (Hayo and Neumeier, 2017; Otjes *et al.*, 2018; Roth et al., 2020). However, the opposite may also be true. When voters are especially worried that the elite is pursuing a neoliberal agenda mainly catering to the interests of big business, as in the model of Acemoglu *et al.* (2013), the elite will have to propose expansionary fiscal policies in order to be credible. It is the latter prediction that is consistent with popular discourse on the populist economic agenda, and that will guide our hypothesis.

> **Hypothesis 1 (H1).** *When people hold strong populist views, they are more likely to support expansionary fiscal policy.*

Importantly, there is also evidence – albeit not at the individual but more aggregate level – suggesting that the relationship between populist attitudes and fiscal preferences goes in the other direction. Analysing how fiscal consolidation undertaken in the aftermath of the Great Recession affected welfare spending at the regional level, Fetzer (2019) finds that the support for Brexit was especially large in districts where the cuts to welfares spending were largest. Similarly, Guiso *et al.* (2019) hypothesize that in euro area countries the crisis has spurred frustration over the loss of economic policy space, and find support that the resort to populist parties was stronger in countries in the eurozone than outside. This literature hence warrants for an empirical set-up to correct for endogeneity of populist attitudes.

Fiscal illusion and the role of literacy and information

There is a large literature in the public choice tradition on 'fiscal illusion', which holds that voters appreciate spending programs but underestimate the (future) costs in terms of taxation or debt (Buchanan and Wagner, 1977; Alesina and Perotti, 1995). This can, for instance, arise when people observe the fruits of public spending, but do not observe the costs when spending is paid for with an increase of public debt. This would cause fiscal policies to be biased towards deficits. While the concept of fiscal illusion is intuitively very powerful and is often taken for granted, also when it comes to the populist economic agenda, scholars have put forward that fiscal illusion is at odds with the fact that voters often actually support politicians with fiscal conservative agendas (Alesina and Perotti, 1995; Eslava, 2011).

The financial sophistication of the public, and collective learning by the public and the media to judge fiscal policies, have been put forward as mechanisms that can mitigate fiscal illusion. Indeed, empirical studies have highlighted that a large share of the public is ill-aware of economic facts and mechanisms (Boeri *et al.*, 2001; Caplan, 2002; Blinder and Krueger, 2004; van der Cruijsen *et al.*, 2015). When public policy is complex and

budgets are non-transparent, poorly literate voters are less suited to judge economic and fiscal policies. In turn, as voters are more literate and receive information on the public budget, they are less prone to deficit bias.

On the basis of a study on Germany, Hayo and Neumeier (2017) indeed find that the more knowledgeable respondents are, the more they favour conservative debt policies. Furthermore, a few studies have used randomized information experiments and have confirmed that exposure to factual information can spur public support for pension reforms (Boeri and Tabellini, 2012) and alter fiscal preferences (Roth *et al.*, 2020). In particular, this latter study finds that respondents become more supportive of debt reduction, although they do not update their preferences on taxation.

> *Hypothesis 2 (H2). People with poor literacy skills will be more favourable to expansionary fiscal policy.*

> *Hypothesis 3 (H3). When people receive information about the intertemporal budget constraint of the government, they will become less favourable to expansionary fiscal policy.*

The moderating effect of populism on the effect of literacy and information

To the best of our knowledge, there is no literature explicitly linking literacy and populist attitudes in relation to fiscal policy preferences. Yet, from the model of Cukierman and Tommasi (1998) presented above we can derive predictions on this. The model first of all holds that it is more difficult for voters to evaluate economic policy than for politicians due to less information and access to expert judgement. Yet, of course, this does not apply to all voters to the same extent, as some voters are more literate and have gathered more information on economic policymaking than others. Likewise, support for economic policies depends on people's judgement of the elite's ideology. Yet, also here, voters differ in their evaluation of the political elite's ideology. These two effects can be expected to reinforce another. When voters are particularly suspicious of the elite's ideology, especially poorly literate individuals may demand expansionary policies to be convinced the elite is acting in their interest. Yet, when they are reassured that the elite is acting in the people's interest, poor literacy may not lead to a demand for expansionary fiscal policies.

When it comes to information, a similar mechanism may be in place. Yet, there is also a possibility that people who are sceptical of the motives of the political elite also tend to be sceptical of expert advice and third-party information in general. Indeed, recent research

has found that populism is correlated with anti-intellectualism – a measure that includes attitudes towards economists. What is more, people with strong anti-intellectualist sentiment might even increase their opposition when confronted with expert advice (Merkley, 2020). Yet, we do not expect that this latter effect is strong enough to counter the negative effect of information provision on preferences for expansionary fiscal policies.

> **Hypothesis 4 (H4)**. *The positive effect of poor literacy on support for expansionary fiscal policy is larger, if people hold stronger populist views.*

> **Hypothesis 5 (H5)**. *The negative effect of information on support for expansionary fiscal policy is larger, if people hold stronger populist views.*

Controls of fiscal preferences

A large theoretical and empirical literature has highlighted a host of determinants of fiscal preferences that we need to control for in our regression of fiscal preferences.

Gender, age and children. First of all, it is well established that since several decades women in industrialized countries have grown to be more supportive of redistribution and government spending (Alesina and La Ferrara, 2005; Alesina and Giuliano, 2011; Dassonneville, 2020).[24] Furthermore, most empirical evidence suggests that support for redistribution increases with age (Alesina and Giuliano, 2011; Gärtner *et al.*, 2017). According to Ricardian equivalence models, older people should favor higher debt levels than younger people since their remaining longevity is shorter, and hence their cumulative tax payments smaller (Heinemann and Hennighausen, 2012). However, empirical research does not find support for this prediction (Heinemann and Hennighausen, 2012; Stix, 2013). According to the same reasoning, people with children may instead be less supportive of high debt due to intergenerational concerns. Yet, the evidence is not conclusive (Heinemann and Hennighausen, 2012; Hayo and Neumeier, 2017).

Education. On the basis of the data from both the US General Social Survey and the World Values Survey, Alesina and Giuliano (2011) find that that lower-educated people demand more redistribution, also controlling for income. However, when interacted with a leftwing ideology, higher-educated people are more supportive of redistribution (Alesina

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24. This 'gender gap' has invoked a large literature on possible explanations, ranging from more egalitarian attitudes, a stronger inclination of helping others and a higher likelihood to be employed in the public sector (Howell and Day, 2000; Cavalcanti and Tavares, 2011). On the other hand, according to experimental evidence, women are more supportive of redistribution because they are less self-confident about the position they take up in the income distribution (Buser *et al.*, 2016).

and Giuliano, 2011).[25] Lower-educated people have also been found to be less favorable to debt reduction, again controlling for income (Stix, 2013). What the channels are behind this education gradient has not been extensively studied. Alesina and Giuliano (2011) attribute it to the higher prospect of upwards mobility.

Economic position. According to the Meltzer-Richard hypothesis, one's position in the income distribution is the key driver of support for government spending (Meltzer and Richard, 1981).[26] Likewise, in the model of Cukierman and Meltzer (1989) people who are more financially constrained are more supportive of borrowing from future generations. Empirical studies have confirmed that higher-income individuals are less supportive of redistribution (Alesina and Giuliano, 2011), and that financially constrained people are more opposed to debt consolidation (Stix, 2013). Furthermore, Alesina and Giuliano (2011) report that people who have experienced an unemployment spell are more supportive of redistribution.

Personality traits. The literature has also highlighted how several personality traits affect fiscal preferences. First of all, and most straightforwardly, more risk averse individuals demand more redistribution (Alesina and La Ferrara, 2005; Gärtner *et al.*, 2017). Second, studies have found that myopic individuals are more tolerant of debt (Stix, 2013; Hayo and Neumeier, 2017). Third, studies have highlighted that people who believe in control over lifetime economic outcomes, rather than luck or fate, attach less value to redistribution (Alesina and Giuliano, 2011; Kouba and Pitlik, 2014). To our knowledge, there is no literature on other personality traits and fiscal preferences.[27]

Attitudinal predispositions. Finally, fiscal preferences have been linked to a variety of predispositions held by individuals. Almost by definition, people who place themselves to the left of the political spectrum are more keen on redistribution (Alesina and Giuliano, 2011).[28] Furthermore, in the European context, fiscal policy is highly influenced by

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25. Piketty (2020) documents that in fact low-educated people in advanced economies, including the Netherlands, have voted more and more for the right, while higher-educated people have voted more for left-wing parties. Yet, this does not mean that lower-educated people demand less redistribution as they may vote for right-wing parties for other reasons (e.g. immigration policies, involvement of left parties in retrenchment policies).

26. The Meltzer-Richard hypothesis also posits that if inequality grows, in a median voter model there will be more redistribution. The evidence for this prediction, however, is weaker (for a discussion, see Borge and Rattsø, 2004).

27. There is literature, though, that links personality traits to household saving and debt (Webley and Nyhus, 2001) which finds that people who score high on emotional stability are more likely to save, while extraversion is related to lower debt.

28. While scholars heavily rely on left-right self-placement as a summary indicator of political ideology, it has been found that this measure in fact captures attitudes towards various issues such as cultural freedom, income equality, the role of markets and immigration (Bauer *et al.*, 2017; Laméris *et al.*, 2018). Given that we have variables for both support for redistributive policies and left-right orientation, we prefer to include these as separate regressors.

European rules. Attitudes towards the European Union may therefore drive fiscal preferences, as people who are supportive of the EU may be more supportive of fiscal consolidation efforts to comply with European rules. At the same time, there is also evidence that Euroscepticism has grown as a result of fiscal consolidations implemented during the crisis, which lends support to the view that one's attitude towards the EU could be an endogenous regressor (Armingeon *et al.*, 2016; Guiso *et al.*, 2019; Fetzer, 2019).

3.3   RESEARCH DESIGN AND DATA DESCRIPTION

Context of our study

Our study is set in the Netherlands a decade after the start of the Great Recession. With government debt at 43.0 percent of GDP in 2007, its public finance position was relatively strong.[29] Yet, government debt rose to 67.8 percent of GDP in 2014, despite expenditure cuts and tax rises that were taken from 2011 onwards. In Spring 2012, the government was at the brink of non-compliance with the Stability and Growth Pact (SGP) rules, and implemented a large package to meet the 3 percent deficit threshold (the so-called Spring agreement, see also Chapter 2). Since 2014, growth turned positive again and government debt has been on a downward path.

Since the early 1990s, Dutch fiscal policy follows the principle of trend-based budgeting. This consists of expenditure ceilings, prudent budgeting and a strict separation between the expenditure and revenue side of the budget, which restricts the use of revenue windfalls for additional expenditure (Beetsma *et al.*, 2013a). Trend-based budgeting can be at odds with the SGP rules, as in a downturn the deficit can quickly deteriorate. This is why authorities aim to have some margin with respect to the SGP thresholds (MinFin, 2016). In Summer 2017, the economic forecasts for the years to come were better than assumed in the initial budgetary projections. In the run-up of the budget for 2018 – that was presented to the public in September 2017 – there was discussion on how to allocate the tax windfalls that would result from this, for debt reduction (consistent with budgetary rules) or for tax relief or more spending. This is precisely the question that we ask respondents in our September 2017 survey.

It is furthermore worth mentioning that, compared to other European countries, Dutch households appear to be relatively financially literate as well as fiscally conservative. As to literacy, according to the analysis of Fornero and Prete (2019), the Dutch public has one of highest rates of financial literacy, which they find facilitates pension reforms (see also Table

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29. Numbers come from CPB (2019).

A.1.3 in the Appendix). Furthermore, when it comes to fiscal policy, the Netherlands was the first country to have a fiscal council, the Netherlands Bureau for Economic Policy Analysis (CPB) (Debrun and Kinda, 2017; van Geest and van Vuuren, 2018). Research has demonstrated that CPB's forecasts are relatively unbiased (Beetsma *et al.*, 2013a; Beetsma *et al.*, 2013b). Arguably, this tradition has made Dutch voters one of the most sophisticated audiences for judging fiscal policy. When it comes to their fiscal preferences, together with more Northern European countries, Dutch households are on average relatively debt averse. In a 2010 Eurobarometer poll, 77 percent of Dutch households agreed that measures to reduce public deficit and debt could not be delayed (EC, 2010). This was only 3 percentage points higher than the EU average; yet, the Netherlands had also a relatively low debt level (59 percent of GDP, against an EU average of 80 percent).

Furthermore, the Netherlands is also a suitable case for studying the influence of populism. First, next to right-wing populists (PVV, LPF), also left-wing populists (SP) have been successful in this country. Second, it has been demonstrated that populist attitudes can be measured validly in this country (Akkerman *et al.*, 2014). Third, the populist message is relatively widespread in the Netherlands (Rooduijn, 2014). The main surveys used for this study, on populism and on fiscal preferences, were held just several months after the Parliamentary elections in which populist parties fared relatively well.

Surveys used

We employ various modules of the DNB Household Survey (DHS), conducted by CentERdata at Tilburg University. DHS is a panel dataset that includes approximately 2000 households from which one or more household members may take part. The panel is designed to be representative of the Dutch population and includes questions on demographics, occupational status, education, earnings, wealth, health and psychological concepts, in various modules spread out over the year. In addition to recurrent questions, additional questions can be added to the questionnaire on an *ad hoc* basis.[30] In a special module of the September 2017 DHS survey, respondents were presented several questions on fiscal policy. The survey was presented to 2773 members of the panel, and completed by 2299 of them (i.e., the response rate was 82.9%). Furthermore, in June 2017, we conducted a special module in the DHS on political attitudes of households (this survey was also used in Rooduijn *et al.*, 2017). This survey was presented to 3035 members of the panel, and completed by 2358 of them (i.e., the response rate was 77.7%). Furthermore, to instrument populism with two indicators of trust, we draw from the trust survey that is administered by De Nederlandsche Bank and embedded in the DHS modules

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30. The panel has been used extensively to study financial market and savings behaviour (see e.g. Guiso *et al.*, 2008; van Rooij *et al.*, 2012).

of 2006 and further, which are presented to the panel in the first quarter of the year (for more information, see van der Cruijsen *et al.*, 2016). See Table A.3.1a and A.3.1b in the Appendix for an overview of all variables used and the respective module.

Dependent variables

Our dependent variables capture the support of respondents for three margins of fiscal policy. To be precise, we asked respondents to what extent they would prefer to use the tax windfalls that were foreseen at the time for i) debt reduction, ii) tax relief and iii) increased spending (see Table A.3.1a in the Appendix for the exact wording of our survey questions). Respondents could answer all three questions on a 1 (do not agree at all) to 4 scale (strongly agree). Clearly, a score of 1 or 4 on all three dimensions would be inconsistent, which is why we delete those cases. [31] Furthermore, as some respondents report more extreme answers than others, we divide the scores per item by the the total scores awarded. This adjustment results in three continuous variables, that we transform to a scale from 0 to 10. We will use these three variables – '*support_debtreduction*', '*support_ taxrelief*' and '*support_morespending*' – as our dependent variables throughout the chapter. Correcting for the total scores awarded is not trivial, however, as the total scores awarded is significantly correlated with populism (0.17). Yet, in the robustness section we show that our results are similar without this correction. Table 3.1 below presents summary statistics for all variables used, restricting the sample to respondents for whom we observe their fiscal preferences. From an inspection of the means of our three dependent variables, we can see that respondents were on average most likely to support more spending and to a lesser extent debt reduction, while support for tax relief was substantially lower. To test our hypotheses, we interpret support for debt reduction as less expansionary, and support for tax relief and more spending as more expansionary.

Regressors of main interest

While political scientists initially studied the ideas of political parties, recently they have started to measure to what extent individuals are prone to the populist idea set. To be precise, this set of ideas comprises i) a distinction between the ordinary people and an evil elite, ii) an antagonistic relationship, and iii) the premise that politics should follow the general will and respect popular sovereignty (Mudde, 2004; Akkerman *et al.*, 2014).[32] Following the work of Akkerman *et al.* (2014), in the June 2017 survey we asked respondents to rank their agreement with six statements on political elites (see Appendix

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31. Together, this concerns 31 cases.
32. It has been demonstrated that the index of populism, also used in this research, measures different underlying attitudes than older concepts such as political trust and external political efficacy (i.e. the extent to which respondents feel they can influence the political process)(Geurkink *et al.*, 2020).

Table A.3.1a for the exact wording). As in other studies, the scores on these statements are strongly related, allowing us to integrate the scores into a composite index which is internally consistent.[33] This composite indicator, '*populism*', is a continuous variable ranging from 1 to 5.

Furthermore, to measure the numerical sophistication of respondents we use an index for probability literacy ('*prob_literacy*').[34] This index traces to what extent respondents can account for uncertainty, such as an event like job loss (Hudomiet *et al.*, 2018). The index is based on four numerical questions, where respondents have to select the right answer on an answering scale from 0 to 100. We reward each correct answer with 1 point, leaving us with an index running from 0 (all questions answered incorrectly) to 4 (all questions answered correctly).

Last, to assess the effect of information provision on fiscal preferences we conduct a survey experiment, presented to DHS respondents just before the question on fiscal preferences in the September 2017 survey. In the experiment, half of the respondents was presented with information about the intertemporal budget constraint of the government, whereas the other half received no message. To be precise, our message is as follows: *'In case in one year a government spends more money than she receives by taxation, the government runs a budget deficit. To finance this, the government must borrow money. Due to this, the total debt of the government ('government debt') will increase. The government cannot let government debt rise endlessly. If the government keeps on borrowing, eventually she will have to raise taxes or cut expenditures so as to stop government debt from increasing.'* The variable '*debt_experiment*' takes the value of 1 if respondents received the information text, and a value of 0 if they received no text.

Controls

Due to the embedding of our questions in the DHS survey, we can merge our own survey questions with a rich set of controls: age, income, educational level, personality traits and the financial situation of the household (e.g. whether they have a hard time making ends

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33. The Cronbach alpha of the six statements is 0.85.
34. We also considered using two measures for financial literacy available in the DHS, a parsimonious one based on three questions as designed by Lusardi and Mitchell (2014) and an extensive one based on sixteen questions as designed by Van Rooij *et al.* (2012). However, the questions to construct these indices were not asked in 2017. Using the index of another year would result in a loss in the number of observations (with 75% in the case of the extensive measure). The correlation of our probability numeracy index with the parsimonious and the extensive financial literacy indicator is 0.33 and 0.43 respectively. When running our baseline regressions with the literacy measures in a smaller sample, our conclusions are the same.

meet).[35],[36] For the precise description of all variables, see Table A.3.1a in the Appendix or the DHS codebook.[37] It furthermore should be noted that we have imputed some values in order to minimize the loss of observations due to merging various modules, as in Chapter 2. For the personality traits, that are found to be relatively stable (Cobb-Clark and Schurer, 2012; Cobb-Clark and Schurer, 2013; Salamanca, 2018), we have imputed the average value of the available observations in the entire period. For the variable '*hard_to_getby*' we used a stricter imputation method and imputed the value of the observation in the previous or next year only, or the average thereof, if both were available.

Moving further in Table 3.1, '*rightwing*' is the self-placement of respondents on a left-to right scale, ranging from 0 (extreme left) to 10 (extreme right). Furthermore, the table shows respondents' agreement (on a 1 to 5 score) with the statement that the government should take measures to minimize income differentials ('*equality*'). Both variables come from the June 2017 survey.

Turning to the end of the table, under the 'auxiliary variables' we report the summary statistics for variables that are used for robustness purposes. First of all, pre-crisis trust in national politics and the management of financial institutions refer to the trust that respondents reported in the years 2006, 2007 and 2008 (as respondents stay in the panel for a limited number of years, this leads to a strongly reduced sample size). We will use these as instruments for populism in Section 3.5. Furthermore, in our robustness section we test whether the results hold when we include a regressor that we suspect to be endogenous to fiscal preferences, i.e. respondents' attitudes towards a strengthening of EU cooperation ('*eu_cooperation*').

Finally, Table 3.1 highlights that, due to nonresponse and the merging process, for some variables there are quite a few missing values. As we shall see, in our baseline regressions we end up with a sample of 1636 observations, which is 71 percent of the full sample. In the robustness section, we test whether the smaller sample selection that we end up with results in different estimates.

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35. Since 2013, in the DHS survey features all so-called 'Big Five' personality traits. These are openness, conscientiousness (which featured in DHS surveys before 2013), extraversion, agreeableness and neuroticism.
36. Table A.3.3 in the Appendix displays pairwise correlations of all our regressors. It can be derived that the highest correlation between covariates in our baseline regression is -0.42, i.e. the correlation between left-right self-placement (rightwing) and support for income levelling (equality). Furthermore, there is a similarly high correlation between populist attitudes and attitudes towards EU cooperation (0.42), which is not included in our baseline regression but in one of our robustness checks.
37. For the DHS codebook see: https://www.dhsdata.nl/site/users/login .

Table 3.1 Summary statistics

	count	mean	s.d.	min	max
Dependent variables					
support_debtreduction	2268	4.37	1.66	0	10
support_taxrelief	2268	2.93	1.41	0	10
support_morespending	2268	4.70	1.34	0.3	10
Regressors of main interest					
populism	1926	3.20	0.74	1	5
prob_literacy	1995	2.51	1.10	0	4
debt_experiment	2268	0.50	0.50	0	1
Controls					
age	2268	54.40	16.84	16	92
female	2268	0.48	0.50	0	1
child	2268	0.32	0.47	0	1
low	2267	0.26	0.44	0	1
preuniversity	2267	0.11	0.31	0	1
vocational_med	2267	0.25	0.44	0	1
vocational_high	2267	0.25	0.43	0	1
university	2267	0.13	0.34	0	1
grossincome (in 1000 EUR)	2268	2.75	21.03	0	1000
hard_to_getby	2172	2.54	0.84	1	5
riskaversion	2160	5.18	0.98	1.5	7
patience	2239	4.11	0.65	1.5	6.3
locus_of_control	2218	4.53	0.68	1.9	7
openness	2234	3.42	0.55	1.2	5
conscientiousness	2241	3.63	0.53	1.7	5
extraversion	2234	3.06	0.66	1	5
agreeableness	2234	3.89	0.57	1.7	5
neuroticism	2234	2.48	0.69	1	4.9
rightwing	1927	5.14	2.08	0	10
equality	1932	3.51	0.98	1	5
Auxiliary variables					
trust_politics_pc	824	2.22	0.60	1	4
integrity_finance_pc	821	3.43	0.62	1	5
eu_cooperation	1932	3.24	1.04	1	5
N	2268				

Notes: See Appendix Table A.3.1a for descriptions of all variables and Table A.3.1b for their source.

3.4 MAIN RESULTS

We now turn to our baseline results, allowing us to test hypotheses 1, 2 and 3. Our dependent variables are the scores of respondents on the three options to use tax windfalls that were at the time discussed in the policy debate, namely for debt reduction, for tax relief or for more spending. For each of the three margins, we show three models. First, we include only populist attitudes and our controls. Second, we add our literacy measure. Third, we also add the dummy variable taking the value of 1 in case respondents received our information treatment. As our dependent variables are continuous, we use OLS. In all regressions, we cluster standard errors at the household level to control for the possibility that errors correlate among members of the households, capturing e.g. exposure to the same media and acquaintances that shape populist attitudes and/or policy preferences. We standardize the personality traits and use dummy variables for income and education.

Table 3.2 reports the results of our main regressions. The first row shows our main result: populist attitudes yield a strongly significant coefficient on all three fiscal preferences. The directions are as expected: respondents with strong populist attitudes are less debt averse, more inclined to favor tax relief and more supportive of spending, in line with hypothesis 1. Importantly, this effect holds when adding our literacy and information measures. In line with hypothesis 2, we find that our literacy measure exerts a highly significant effect on debt reduction and tax preferences. Yet, there is no effect on support for more spending. Last, treatment with our educational message causes respondents to report more support for debt reduction and less support for more spending. In line with the findings of Roth *et al.* (2020), the effect on tax relief is not significant at the convenient significance level of 5 percent.

We will not discuss all other results in detail, but will mention some results that stand out. First of all, support for more spending does not differ among educational groups, yet more education goes hand in hand with more support for debt reduction and less support for offering tax relief. Second, in contrast to the prediction of the Meltzer-Richard hypothesis, one's income position does not matter for fiscal preferences. Yet, in line with the model of Cukierman and Meltzer (1989), we do find that respondents who have a hard time getting by are less supportive of reducing public debt (although this result is significant at 10 percent only once we add our literacy measure).[38] Third, several of the personality traits appear highly significant regressors of fiscal preferences, although their impact differs across our three fiscal policy margins. The results for risk aversion, patience and locus of control are broadly in line with previous research. As to the 'Big Five' personality traits, we

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38. We also included an actual unemployment spell as a regressor; yet the results were not significant.

Table 3.2 Regression of fiscal preferences

Extent to which respondents would be in favor of using foreseen tax windfalls for the following purposes exclusively:

	Debt reduction			Tax relief			More spending		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
populism	-0.50***	-0.47***	-0.46***	0.33***	0.30***	0.30***	0.17**	0.17**	0.16**
prob_literacy		0.20***	0.20***		-0.17***	-0.17***		-0.04	-0.04
debt_experiment			0.28***			-0.11			-0.17**
age35to45	0.00	0.04	0.04	-0.17	-0.20	-0.20	0.17	0.16	0.16
age45to55	0.04	0.07	0.08	-0.16	-0.19	-0.19	0.12	0.12	0.11
age55to65	-0.06	0.00	0.00	-0.09	-0.14	-0.14	0.15	0.14	0.14
age65plus	-0.15	-0.03	-0.04	-0.14	-0.23	-0.23	0.28*	0.26*	0.27*
female	-0.33***	-0.27**	-0.27**	0.12	0.07	0.07	0.21**	0.20*	0.20*
child	-0.24*	-0.23*	-0.24*	0.16	0.16	0.16	0.08	0.08	0.08
vocational_med	-0.10	-0.11	-0.10	0.00	0.02	0.01	0.09	0.09	0.09
preuniversity	0.26	0.20	0.22	-0.30*	-0.25	-0.26*	0.04	0.05	0.04
vocational_high	0.13	0.05	0.07	-0.26*	-0.20	-0.20	0.13	0.15	0.14
university	0.49**	0.33*	0.35*	-0.50***	-0.37**	-0.38**	0.02	0.04	0.03
incomeQ2	-0.05	-0.04	-0.01	0.14	0.13	0.12	-0.09	-0.09	-0.11
incomeQ3	-0.14	-0.12	-0.10	0.14	0.12	0.12	0.00	-0.00	-0.02
incomeQ4	-0.07	-0.07	-0.04	0.12	0.12	0.11	-0.05	-0.05	-0.07
hard_to_getby	-0.11*	-0.09	-0.09	0.06	0.05	0.04	0.05	0.05	0.04
riskaversion	0.04	0.03	0.03	-0.10**	-0.09*	-0.09*	0.06	0.06	0.06
patience	0.11*	0.10*	0.10*	-0.07*	-0.06	-0.06	-0.04	-0.04	-0.04
locus_of_control	0.11*	0.10*	0.09	-0.04	-0.03	-0.03	-0.06	-0.06	-0.06
openness	0.04	0.03	0.03	0.04	0.04	0.04	-0.07	-0.07	-0.07
conscientiousness	-0.07	-0.06	-0.06	0.12**	0.11**	0.11**	-0.05	-0.05	-0.05
extraversion	-0.04	-0.02	-0.02	0.06	0.05	0.05	-0.03	-0.03	-0.03
agreeableness	-0.01	-0.03	-0.03	-0.14**	-0.13**	-0.13**	0.15***	0.16***	0.16***
neuroticism	-0.03	-0.03	-0.04	0.05	0.05	0.05	-0.02	-0.02	-0.02
rightwing	0.07**	0.07**	0.07**	0.01	0.01	0.00	-0.08***	-0.08***	-0.08***
equality	-0.05	-0.04	-0.04	-0.09*	-0.10*	-0.10*	0.14***	0.14***	0.14***
_cons	6.36***	5.58***	5.36***	2.03***	2.68***	2.77***	3.60***	3.74***	3.87***
N	1636	1636	1636	1636	1636	1636	1636	1636	1636
R²	0.145	0.158	0.165	0.098	0.111	0.112	0.112	0.112	0.116

Notes: Fiscal preferences are measured on a 0 to 10 scale, and are estimated by OLS. Reference groups: aged below 35, male, low educational level and first income quartile. Standard errors (which are clustered at the household level) are suppressed due to space constraints. The symbols *, ** and *** denote significance at the 10, 5 and 1 percent confidence level, respectively.

find that conscientious respondents are significantly more supportive of tax relief, while individuals who score high on agreeableness are less supportive of tax relief and more supportive of increasing spending. Fourth, there appear to be some interesting nuances when it comes to the differences between rightwing self-placement and support for income levelling. When it comes to debt reduction, it is rightwing respondents that stand out with a significantly higher support for of debt reduction. When it comes to support for tax relief, it is supporters of income levelling that stand out, reporting significantly lower levels of support for tax relief. When it comes to support for more spending, both dimensions prove significant regressors.

Table 3.3 Decomposition goodness of fit

	Debt reduction		Tax relief		Spending	
	Pseudo R^2	**%**	**Pseudo R^2**	**%**	**Pseudo R^2**	**%**
Populism	0.047	28%	0.027	25%	0.009	8%
Literacy	0.030	18%	0.024	21%	0.003	3%
Information	0.007	4%	0.002	2%	0.004	3%
Demographics	0.014	8%	0.005	5%	0.016	14%
Education	0.020	12%	0.019	17%	0.003	2%
Income	0.013	8%	0.005	5%	0.007	6%
Personality	0.023	14%	0.023	20%	0.032	27%
Rightwing	0.005	3%	0.004	3%	0.020	17%
Equality	0.006	4%	0.003	3%	0.022	19%
Total R^2	0.165	100%	0.112	100%	0.116	100%

Notes: Shorrocks-Shapley decomposition calculated with Stata's shapley2 command (Chávez Juárez, 2012).

Table 3.3 reports the results of a decomposition of the explained variance. First and foremost, it shows that populist attitudes are not only a significant regressor for fiscal preferences, they also prove very important in material terms. When it comes to support for debt reduction and tax relief, a quarter of the explained variation can be attributed to populist attitudes. Its contribution is bigger than many variables that play a central role in the literature on fiscal preferences, such as income and right-wing ideology. The relevance of populist attitudes for support for more spending is more limited, whereas the role of ideology is much bigger here. Second, a similar pattern can be observed when it comes to our literacy measure. Whereas literacy accounts for about a fifth of the fit of our models of debt reduction and tax relief, this is only 3 percent when it comes to attitudes towards spending. Third, while our information treatment has a significant effect on two out of three fiscal preferences, it can only account for a very small portion of the model fit. When

it comes to our controls, it stands out from Table 3.3 that personality traits play a very important role in explaining attitudes towards fiscal policy. Taken together, they are even the most important predictor of support for more spending.

3.5   ROBUSTNESS ANALYSIS

Endogeneity

As mentioned, our main methodological concern about the regression presented in Table 3.2 is that various sources of endogeneity would cause the outcomes in Table 3.2 to be biased. Our main regressor of interest, populist attitudes, could be endogenous because of three reasons. First, there could be an omitted variable bias, i.e. fiscal preferences and populism are both influenced by another factor, such as feelings of vulnerability. A second concern is that the relation is simultaneous, i.e. there is reverse causation from fiscal preferences to populism. In our case, we cannot exclude the possibility that respondents with more expansionary fiscal preferences have grown populist sentiments when the government consolidated in the midst of a large recession. Finally, it would be problematic if errors in the measurement of our variables – which by themselves are inevitable in observational research – would not be random. For instance, a respondent's mood can influence the answers given in the same survey in similar ways. This is particularly a concern when variables are taken from a common source.

In our baseline regression, several elements of our estimation strategy already mitigated some of these sources of endogeneity. For instance, we employ a very rich set of regressors, diminishing the bias resulting from omitted variables. Likewise, our questions come from surveys held at various moments in time, which alleviates concern over common source bias. Most importantly, this holds for our main regressor of interest populist attitudes, which were recorded in June 2017, and our dependent variables (fiscal preferences), which were recorded three months later. Nonetheless, there remains a risk of endogeneity due to other omitted variables, the possibility of reverse causation and measurement error that affect the answering in all surveys (e.g. a tendency to social desirability).

A more general solution to control for endogeneity is to employ an instrumental variable approach. Given the possibility of reverse causation, we prefer to use instrumental variables that are gathered with a considerable lag from the moment that we asked respondents about their fiscal preferences, ideally before the economic crisis. As the measurement of populist attitudes was first done in June 2017, a lagged variable of populism is not an option. Yet, as our survey was embedded in the rich DHS survey environment there are other lagged variables that we can consider. In particular, since 2006 each year DHS

respondents are asked about their trust in various public entities, including national politics. In addition, respondents were asked whether they had faith in the expertise and integrity of the management of financial firms (see Table A.3.1a in the Appendix for the exact wording).[39]

Trust in politics is typically understood by political scientists as an evaluation of how well politicians fulfill people's expectations (van der Meer, 2018) and is a different concept than populism (Geurkink *et al.*, 2020). For instance, according to populist discourse, politics should follow the 'will of the people', which is not a necessary condition for people to trust their government. Yet, low trust and populism have in common that they both question the motivations and/or the performance of the political elite. Likewise, respondents' attitudes to the management of financial institutions are likely to pick up adverse sentiment towards elites. To rule out the possibility that our instruments capture feelings of anger over fiscal consolidation measures taken in the aftermath of the crisis, we take the average of the available observations that were available in 2006 to 2008, before the crisis hit. This limits our sample to just 610 observations. Yet, even with this time lag, our instruments could still be endogenous to our dependent variables (fiscal preferences) as there could unobserved fixed individual characteristics that influence both attitudes towards fiscal policy and political views. When it comes to trust in politics, there is literature that links fiscal preferences to trust in politics (Stix, 2013; Hayo and Neumeier, 2017). Yet, we are not aware of theoretical and empirical linkages of pre-crisis attitudes towards the financial sector and fiscal preferences. Table A.3.4 in the Appendix shows the correlation between fiscal preferences of respondents in 2017 and their trust in national politics and the management of financial firms in the years before the crisis. It reports a significant correlation of pre-crisis trust in national politics and fiscal preferences in 2017, also when controlling for other relevant regressors. Yet, there is no significant correlation of fiscal preferences with pre-crisis trust in financial sector management. When including controls, the remaining partial correlation is very low (from a maximum of -0.09 for support for tax relief to 0.02 for support for spending). This gives us reassurance that according to this statistical test, at least one of our two instrumental variables is exogenous to our dependent variables, which is a necessary condition for the Sargan over-identification test that we perform later to be reliable.

In addition to these two auxiliary instruments, we include all controls from the second stage in our first-stage regression. In the regression of fiscal preference, our second stage, we include the full set of regressors of Table 3.2 (i.e. columns 3, 6 and 9). We first run the

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39. This variable has also been used in a recent paper on financial literacy and trust in financial institutions (van der Cruijsen *et al.*, 2019).

same regressions but then with our limited sample size (N=610) so that we can attribute any differences in the estimated coefficient to the estimator and not to differences in the sample. We then run a two-stage-least square (2SLS) regression where we instrument populism as discussed above.

Table 3.4 shows the results of the explanatory regression, as well as the highlights of the first stage (i.e. the coefficients of our pre-crisis trust variables) and the relevant test statistics. The table shows that our two instrumental variables, pre-crisis trust in politics and the financial sector management, turn out to be highly significant regressors. The partial R^2 of the first stage is 0.159 and the F-statistic 55.1, which largely exceeds the threshold of 10 which is widely used to test the relevance of an instrument.

Furthermore, as we have more than one instrument, we can run a Sargan test of overidentifying restrictions to test the exogeneity condition of our instrument set. At the significance level of 5 percent, we can reject the null of invalidity of our instrument set for all our three fiscal preferences. The results of the second stage regression confirm that our coefficient for populism is robust to the use of an IV estimator; hence our estimates are unbiased. Yet, as OLS is a much more efficient estimator we also assess to what extent an IV estimation is really needed.[40] Comparing OLS and 2SLS results, the Wu-Haussman test scores formally rejects endogeneity of populist attitudes. Hence, while our IV estimation is valid and the results support that populism is a significant predictor of fiscal preferences, we may just as well rely on our OLS regression.

Inclusion of attitudes towards the European Union

As noted in the literature review, attitudes towards the European Union (EU) might be another factor driving fiscal preferences. Yet, attitudes towards the EU might also be influenced by fiscal preferences, e.g. due to frustration over consolidations during the crisis. This would make it an endogenous regressor, that in turn is also highly correlated with populist attitudes (0.42, see Table A.3.3 in the Appendix). Including multiple endogenous covariates can severely confound our regressions, which is why we chose not to include attitudes towards the EU in our baseline regression, while correcting for endogeneity via an IV regression instrumenting populist attitudes with pre-crisis trust levels. Yet, it is possible that also these pre-crisis populist attitudes partly pick up the effect of (pre-crisis) attitudes towards the EU. In Table A.3.5 in the Appendix we run the same regressions as in

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40. Another drawback is that our IV estimate is strictly speaking only applicable to those respondents for which our instruments properly predict the level of populism. In more technical terms, the IV models estimate the 'local average treatment effect' (LATE) for those people that positively respond to the IV ('the compliers'), while the models without IV estimate the 'average treatment effect' (Cameron and Trivedi, 2009: 884-5).

Table 3.4 IV Estimation results for fiscal preferences with lagged trust as instrument

Extent to which respondents would be in favor of using foreseen tax windfalls for the following purposes exclusively:

	Debt reduction		Tax relief		More spending	
	OLS (1)	2SLS (2)	OLS (3)	2SLS (4)	OLS (5)	2SLS (6)
populism	-0.49***	-0.81***	0.27***	0.37*	0.22**	0.44**
prob_literacy	0.10*	0.09	-0.13**	-0.12**	0.03	0.04
debt_experiment	0.14	0.12	0.05	0.05	-0.18*	-0.17
age35to45	0.48	0.59	-0.25	-0.28	-0.23	-0.31
age45to55	0.27	0.39	-0.20	-0.24	-0.07	-0.15
age55to65	0.20	0.38	-0.13	-0.19	-0.06	-0.19
age65plus	0.10	0.29	-0.15	-0.20	0.05	-0.08
female	-0.40***	-0.43***	0.14	0.15	0.26**	0.28**
child	-0.36**	-0.35**	0.42**	0.42***	-0.06	-0.07
vocational_med	0.01	-0.04	-0.10	-0.09	0.10	0.13
preuniversity	0.40	0.31	-0.34	-0.31	-0.06	0.01
vocational_high	0.31	0.20	-0.48***	-0.44**	0.16	0.24
university	0.40	0.22	-0.56**	-0.50**	0.16	0.28
incomeQ2	0.20	0.21	0.00	-0.00	-0.20	-0.21
incomeQ3	-0.06	-0.09	0.08	0.09	-0.02	0.00
incomeQ4	0.12	0.06	0.11	0.13	-0.23	-0.19
hard_to_getby	-0.14	-0.12	0.06	0.06	0.07	0.06
riskaversion	0.07	0.08	-0.10	-0.10	0.03	0.02
patience	0.02	0.02	-0.06	-0.06	0.03	0.04
locus_of_control	0.17**	0.18**	-0.05	-0.05	-0.12*	-0.12**
openness	0.03	0.02	0.09	0.10	-0.12*	-0.12*
conscientiousness	-0.22***	-0.20**	0.23***	0.22***	-0.00	-0.02
extraversion	0.03	0.03	-0.07	-0.07	0.04	0.04
agreeableness	0.11	0.12	-0.21***	-0.21***	0.10	0.09
neuroticism	-0.08	-0.08	0.10	0.10	-0.02	-0.02
rightwing	0.07*	0.10**	0.00	-0.01	-0.07**	-0.09***
equality	-0.13	-0.11	-0.05	-0.06	0.18***	0.17**
_cons	5.94***	6.70***	2.52***	2.28**	3.54***	3.02***
First stage (populism)						
trust_politics_pc	-0.39***		-0.39***		-0.39***	
integrity_finance_pc	-0.11**		-0.11**		-0.11**	
(other controls suppressed)						
Partial R² / F(2,581)	0.159/55.1		0.159/55.1		0.159/55.1	
Sargan (score) chi²(1)	1.95 (p=0.16)		0.00 (p=0.96)		3.08 (p=0.08)	
Wu-Hausman F(1,581)	1.91 (p=0.17)		0.23 (p=0.63)		1.34 (p=0.25)	
N	610	610	610	610	610	610
R²	0.200	0.187	0.132	0.130	0.145	0.134

Notes: For each fiscal preference, the table first reports ordinary least squares (OLS) regression (same as in Table 3.2, but with a smaller sample) and two-stage least squares (2SLS) regression whereby populism is instrumented by pre-crisis trust in national politics and pre-crisis trust in financial sector management plus all the controls used in the second stage. Due to space limitations, standard errors are suppressed. The symbols *, ** and *** denote significance at the 10, 5 and 1 percent confidence level, respectively.

Table 3.2, but now also including attitudes towards EU cooperation. While the coefficient of populist attitudes is somewhat smaller, it remains highly significant and all other results also hold.

Loss of observations

As noted in Section 3.3, using variables from different DHS modules comes at the cost of a loss of observations. In our baseline regressions we end up with a sample of 1636 observations, which is 71 percent of the full sample of fiscal preferences. To test whether restricting the sample influences our results, we repeat the regressions with a smaller set of controls and hence with larger samples. In Table A.3.6 in the Appendix we repeat the regression of Table 3.2 for debt reduction (the first of the three fiscal preferences) with a smaller set of regressors for which we have substantially more observations. Comfortably, we find that all coefficients are remarkably similar. The only difference is that in one case (i.e., a dummy for respondents aged 65 and over) the coefficient is only statistically significant in the larger sample, and not in our baseline sample.

Correction of scaling

As mentioned in Section 3.3, we have corrected the scales of our dependent variables for the total number of scores awarded so as to filter out the effect of more extreme answering. Yet, this correction is not trivial as the answering is correlated with populist attitudes. We therefore test whether our main results are robust to using the uncorrected scale. Table A.3.7 in the Appendix compares the results of Table 3.2 (full model) with fiscal preferences, when no correction is made (to compare the coefficients, we adjust the original scale to the 0 tot 10 scale that we employ in the baseline regression). When we do not correct the scaling, the results for populist attitudes are even stronger.

3.6   INTERACTION EFFECTS

Moderating effect of populism on effect of literacy

To test hypotheses 4 and 5, this section extends the regression of Table 3.2 with interaction effects of populism with literacy and information provision, respectively. Figure 3.1 graphically reports the results of the interaction analysis of populist attitudes and literacy. The y-axis depicts the marginal effect of literacy on fiscal preferences (y-axis) at different levels of populism (x-axis). As such, the chart reports the combined effect of the coefficients of literacy and the interaction term of literacy and populist attitudes. The brown bars display the distribution of populist attitudes. The chart in panel (a) shows that the effect of literacy on debt reduction – which on average yielded a highly significant coefficient

of 0.20, see Table 3.2 – does not vary much with populism, although at very low levels of populism the effect of literacy on support for debt reduction is no longer significant. The effect is stronger when it comes to tax relief (panel (b)). On average, we found a highly significant coefficient of -0.17 of literacy on support for tax relief. Yet, panel (b) shows that the effect of literacy on support for tax relief is not significant at low levels of populism, but is highly significant at high levels of populist attitudes, lending support to our hypothesis 4. The effect of literacy on support for spending does not vary significantly with the level of populism (panel (c)), which is not surprising as there was also no significant overall effect of literacy on support for more spending in the first place. These results are robust to using the uncorrected scale (results available upon request).
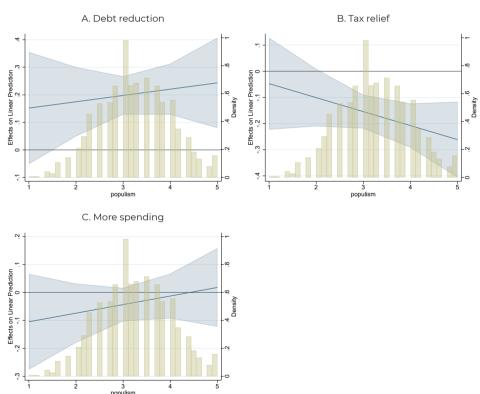
Figure 3.1 Moderating effect of populism on the effect of literacy on fiscal preferences

Notes: Panels show at the y-axis the marginal effect of literacy on fiscal preferences (i.e., debt reduction, tax relief and more spending) at various levels of populism (x-axis). Shaded area covers the 90% confidence interval. Brown bars display the distribution of populist attitudes (right axis).
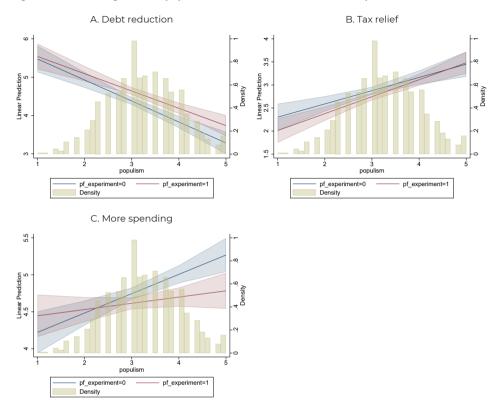
Figure 3.2 Moderating effect of populism on effect information on fiscal preferences

Notes: At the y-axis is the level of support for our three fiscal policy margins (i.e., debt reduction, tax relief and more spending) at various levels of populism (x-axis) for respondents in the treatment group of our information experiment (red line) and those in our control group (blue line). Shaded area covers the 90% confidence interval. Brown bars display the distribution of populist attitudes (right axis).

Moderating effect of populist attitudes on effect of information

Figure 3.2 reports the results of the models in which we include an interaction term of the information experiment with populist attitudes. The figure shows the predicted level of fiscal preferences (i.e., support for the fiscal policy margin on a scale of 1 to 10), both for people who have been treated with the information experiment (red line) as well as for those in our control group (blue line). In Table 3.2 we estimated the average effect of the information experiment over the entire sample, yielding a significant coefficient for debt reduction (0.28, i.e., more support debt reduction) and spending (-0.17, i.e., less support for more spending). Panel (a) and (b) show that the effect of the information experiment on support for debt reduction does not vary along respondents' level of populist attitudes.

Panel (c), however, shows that the information experiment reduces support for more spending especially when populist attitudes of respondents are stronger. This is especially so because support for more spending is already lower at low levels of populist attitudes. In line with hypothesis 5, information provision can hence alleviate fiscal illusion also especially with voters who are sceptical of the establishment.[41]

3.7   CONCLUSION

This chapter's aim was to assess whether populist attitudes lead to more expansionary fiscal preferences, and populist attitudes reinforce the risk of fiscal illusion. Our analysis has first of all demonstrated that populist attitudes are indeed a highly significant and materially important predictor of fiscal preferences. People who hold strong populist sentiments are less supportive of debt reduction, and more supportive of tax relief and more spending. For debt reduction and tax relief, the explanatory power of populist attitudes by far exceeds that of covariates that feature prominently in the political economy literature, such as income and left-right ideology. Our IV regressions have confirmed that our estimates are robust to potential endogeneity, e.g. due to reverse causality, and are hence unbiased.

To assess whether populist sentiment reinforces the risk of fiscal illusion, we have also inspected the role of literacy and information and their interaction with populist attitudes. We found that literacy is a statistically significant and materially relevant predictor of support for debt reduction and tax relief. We take this as support of the occurrence of fiscal illusion. What is more, we find that populist attitudes moderate the effect of literacy on support for tax relief (but not on attitudes towards debt and spending). At high levels of populist attitudes, literacy is a significant predictor of support for tax relief, but not at low levels of populist attitudes. Our results hence suggest that populist sentiment reinforces the risk of fiscal illusion that comes with poor literacy. Turning to information, our information experiment confirms that providing information about the intertemporal budget constraint of the government causes respondents to have less expansionary fiscal preferences. We find that respondents with strong populist attitudes, who report significantly higher support for more government spending, also reduce their support for more spending more strongly after being exposed to our information treatment.

Our results offer various lessons to economic policymakers who may view populism as a threat to sound economic policymaking. First of all, our results corroborate the finding that poor literacy spurs the risk of fiscal illusion which calls for investing in the numerical and fiscal policy sophistication of voters (Fornero, 2014). Furthermore, this risk is even

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41. Again, results are robust to using the uncorrected scale. Results available upon request.

larger when people, for whatever reason, have come to believe that the political elite is not acting in their interest as it should. This means that in the current era, in which the religious and/or ideological ties between voters and the elite have become increasingly loose, investing in knowledge and skills is even more important. Second, our results suggest that information provision can alleviate fiscal illusion especially with people with strong populist sentiment, as they are most prone to a deficit bias in the first place. Of course, a precondition is that information can reach such voters, which may complicated by the lower tendency of voters with strong populist attitudes to make use of established news sources (Schulz, 2019).

Yet, our results of our analysis also imply that fiscal illusion is not the full story behind the expansionist 'populist economic agenda', as dubbed by economists. For one thing, after controlling for literacy and information, there remains a very large independent effect of populism on fiscal preferences. Our literature review has highlighted some mechanisms that may be at play here. Most prominently, according to the model of Acemoglu *et al.* (2013), individuals who think the elite is not acting in the people's interest as it should, may deem the political elite's agenda too neoliberal to their taste, catering more to the needs of big business more than to the ordinary people. It is not in the scope of this chapter to judge whether such a view is correct or not. In either case, as put forward by Piketty (2020), it is dangerous to equate the populist economic agenda as merely short-termist and unsustainable, as it can reinforce the idea that the elite is not responsive to the needs of ordinary voters and can also inhibit debates about fundamental economic policy questions, complex as they may be. And in either case, the elite may need to signal better that it is really acting in the interest of ordinary people, e.g., by investing in universal welfare schemes and shifting taxation more to big firms and wealthy households.

We conclude this chapter with some suggestions for further research. First of all, with our dataset we have only been able to analyse fiscal preferences and populist attitudes in a cross-section setting. By instrumenting populist attitudes with pre-crisis trust levels, we ruled out the possibility of reverse causation and have shown that populist attitudes of respondents yield an unbiased regressor of fiscal preferences. Yet, while we showed that one of our instrumental variables was exogenous to fiscal preferences from a statistical point of view, it could be argued that unobserved individual traits may link it endogenously to the dependent variable. This can only be addressed by using longitudinal data, once these become available. Second, the strong empirical relationship between populist attitudes and fiscal preferences warrants more theoretical literature on the mechanisms underlying this relationship.

Chapter 4

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BACKLASH TO PENSION REFORM:

THE ROLE OF GRIEVANCES, FISCAL ILLUSION AND POPULIST ATTITUDES

4.1 INTRODUCTION[42]

In Chapter 2 we have shown that in the decade up to 2012, the support among the Dutch public for an increase in the retirement age gradually grew. After several failed attempts, in Spring 2012 the cabinet successfully adopted legislation to increase the retirement age to 67, increasing with life expectancy thereafter. Around the same time, spurred by heightened fiscal stress, also many other countries have seen major reforms to pension schemes (Natali and Stamati, 2014; Natali, 2015; Natali, 2018; Beetsma *et al.*, 2020). Yet, in the Netherlands and elsewhere the increase in the retirement age did not remain uncontested. In the Netherlands, also after the adoption of the reform much discussion centered on the difficulties that workers in physically demanding occupations face in working longer, as well as the lower life expectancy of lower-income groups due to which an increase in the retirement age would be regressive (Mastrogiacomo *et al.*, 2014; de Beer *et al.*, 2017a; de Beer *et al.*, 2017b; Vandenbroucke, 2018). In the run-up of the 2017 Dutch Parliamentary elections, several parties proposed to reverse the reform of 2012, moving the retirement age back to the age of 65. The main parties that did so (i.e., the PVV and SP) are typically classified by scholars as populist (Akkerman *et al.*, 2017; van der Waal and De Koster, 2018).

In other European countries challenger parties also prominently campaigned to reverse reforms and (partly) lower the retirement age. In Italy, the Lega Nord and the Five Star movement campaigned with a promise to erase the 2011 pension reform, and in 2019 settled a bill that would partly meet this promise. In the 2017 presidential elections in France, the Front National promised to lower the retirement age. Macron won the elections but, after massive protests, in early 2020 withdrew his proposal to raise the retirement age. And while in Germany the Alternative für Deutschland did not go as far as to propose a lowering of the retirement age, its appeal to pensioners did cause the coalition of Merkel to top up pension contributions in an attempt to counter the AfD (Wagstyl, 2016; Natali, 2018).

Expanding on both Chapter 2 and Chapter 3, this chapter assesses the nature of the backlash against pension reform in the Netherlands. As discussed in the previous two chapters, it is well documented that a large part of the public has poor knowledge of the pension system and public finances in general. Hence, many economists and policymakers fear that populist parties exploit the poor understanding and attract votes by promising simple solutions that are not sustainable in the long run. At the same time, political science

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42. I am grateful to Frank Vandenbroucke for valuable comments to the published version of Chapter 2, which provided inspiration for this chapter.

research has pointed out several 'grievances' that populist parties respond to (Ivarsflaten, 2008). If mainstream parties do not address such grievances, the rise of populism has the potential to make policies more responsive and even improve the functioning of democracy (Rovira Kaltwasser, 2012). Also when it comes to an increase in the retirement age, populist parties may cater to grievances of the electorate that the political elite in their view may too easily dismiss. For instance, it has been demonstrated by De Grip *et al.* (2012) that changing the pension age late in the game can lead to strong feelings of discontent. According to some, also some elements of the Dutch pension reforms might have felt like 'theft' (de Beer *et al.*, 2017b).

Our data allow us to test several hypotheses on the drivers of resistance towards the increase in the retirement age. First of all, we extend our longitudinal analysis of Chapter 2 with several new regressors to assess to what extent grievances about postponed retirement – in particular lower longevity and poor job satisfaction – drive reform resistance. Secondly, building on the 2017 survey used in Chapter 3, we test in a cross-section setting to what extent poor literacy and populist sentiment drive reform resistance. We operationalize resistance to pension reform in two ways. The first operationalization is to look at the (least) preferred reform options respondents select in case further reform would be necessary, as we have studied in Chapter 2. The second operationalization is the extent to which respondents agree that the retirement age should be set at the age of 65 again. Other than the first measure, this question does not discipline respondents on how to finance such a measure.

Our results shed light on the mechanisms that are at the basis of resistance towards the increase in the retirement age, and support for reform reversal. First, we find evidence that grievances of, in particular, lower-educated workers play a role, most notably differential mortality and lower job satisfaction. Second, we find evidence that poor literacy makes respondents more likely to favour a reversal of reform, although we do not find that poor literacy inhibits support for reform when respondents are confronted with the opportunity costs. Third, we find that populist sentiment in itself spurs reform resistance.

For policymakers who wish to foster the acceptance of economic policy reforms, this offers several leads for action. In line with Chapter 2 and 3, the acceptance of reforms will be higher when voters are confronted with disciplined choices, something that can be spurred by fiscal councils. Similarly, in line with Chapter 3, investing in the skills and knowledge of voters can diminish the incidence of fiscal illusion. Yet, our results also show that fiscal illusion is not the only challenge to overcome. In particular, reforms will also stand a better chance when policymakers show that they do their utmost to incorporate grievances of large parts of the public in the design of reform policies, for instance by grandfathering

clauses or compensating policies. By doing this, the political elite can also signal to voters that they are trying their best to act in the interest of ordinary voters, which could further alleviate reform resistance.

The remainder of this chapter is structured as follows. The next section complements the literature reviews in Chapters 2 and 3 with a selected overview of studies that have looked into the possible impact of differential longevity, working conditions, literacy and populism on reform attitudes, which will guide our hypotheses. Section 4.3 describes how the political discussions and decision-making on the retirement age has evolved since the adoption of the increase in the retirement that we described in Chapter 2, while Section 4.4 describes our data. Section 4.5 updates the longitudinal analysis of Chapter 2 on support for a higher retirement age with data until 2019, while adding some additional controls to better capture potential grievances, and reports the results of several robustness tests. Section 4.6 presents the results of a cross-section analysis of support for reform reversal, which we compare with the disciplined attitudes towards reform used in our longitudinal analysis, and reports the results of several robustness tests. Section 4.7 concludes.

4.2 SELECTED LITERATURE REVIEW AND HYPOTHESES

It is well documented that higher-educated individuals have a higher life expectancy upon retirement, e.g. due to different lifestyles and less physically demanding work (Ayuso *et al.*, 2017). Higher-educated individuals typically enter the labour market later, and when the retirement age is uniformly set, they also enjoy a longer period of retirement. For the Netherlands, it has been calculated that life expectancy at 65 is in the order of 2.5 years higher for higher-income individuals (Kalwij *et al.*, 2013). In line with this, it has been confirmed that individuals with lower expected longevity would like to retire earlier than individuals with higher perceived longevity (van Solinge and Henskens, 2009). In addition to the fact that lower-educated individuals on average enjoy a shorter retirement, their lower remaining life expectancy also has substantial regressive consequences. According to some studies, the regressive effect of differential mortality can eliminate much (or even fully) the redistribution from higher-income to lower-income households that flat-rate pay-as-you-go systems typically entail (Garrett, 1995; Ayuso *et al.*, 2017).[43] This leads us to our first hypothesis.

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43. As mentioned in Chapter 2, also the Dutch system entails redistribution from higher-income to lower-income households (Bonenkamp and Ter Reele, 2013).

>   ***Hypothesis 1 (H1)****. Respondents with lower expected longevity will be less supportive towards raising the retirement age.*

Besides differences in health outcomes after retirement, it is also well established that there are pronounced differences between educational (and likewise income) groups in physical and mental health experienced before retirement. Lower-educated people typically sort into jobs that come with more physical strain and less control over their jobs, with adverse consequences for their health (Ravesteijn *et al.*, 2018). Furthermore, adverse working conditions have been found to lower job satisfaction and increase the intention to retire earlier (Bockerman and Ilmakunnas, 2017).

>   ***Hypothesis 2 (H2)****. Respondents with worse working conditions and lower job satisfaction will be less supportive towards raising the retirement age.*

In Chapter 3 we reviewed the literature on fiscal illusion. In this literature, poor information and poor sophistication of voters play a critical role. Appreciation of the need for economic reforms also typically involves factual knowledge as well as an understanding of economic mechanisms at play. Hence, it can be expected that literacy facilitates the support for reform. Using country-level data on literacy and the implementation of pension reforms, Fornero and Lo Prete (2019) report evidence that countries where households are on average more financially literate, have pursued more pension reforms.

>   ***Hypothesis 3 (H3)****. Respondents with poorer literacy skills will be less supportive towards raising the retirement age.*

In Chapter 3 we showed that respondents with strong populist preferences are less keen on reducing debt. A possible channel is that respondents who are sceptical about the political elite, are less inclined to accept that reforms are necessary. This is supported by recent research that has found that populism is correlated with anti-intellectualism – a measure that includes attitudes towards economists – and that respondents with strong anti-intellectualist sentiment might even increase their opposition when confronted with expert advice (Merkley, 2020). Hence, we expect that respondents with strong populist attitudes are less inclined to favour reforms.

>   ***Hypothesis 4 (H4)****. Respondents with strong populist attitudes will be less supportive towards raising the retirement age.*

4.3 CONTEXT OF OUR STUDY

In Chapter 2 we traced household attitudes towards a higher retirement age. Our analysis starts in 2004, when raising the retirement age was still very unpopular. As described, although the increase in the retirement had been debated since its inception, it was not proposed by a single political party until well into the 2000s. The liberal democrats (D66) were the first to do so in 2006, after which the pro-reform consensus among parties strengthened. In 2009 and 2010, there had been several proposals that were fiercely debated. In Spring 2012, the government had to pursue consolidation measures to comply with European budgetary rules. With the support of two other parties, the government proposed raising the retirement age in small increments already in 2013. It was this 'Spring Agreement' that was finally implemented. In Chapter 2 we showed that between 2004 and 2012 the public gradually became more reform-minded.

In October 2012, a new government was formed between the Liberal Party (VVD) and the Labour Party (PvdA). In their coalition agreement they decided to implement the increase in the retirement age even more quickly. At the time, the proposal raised little protests, yet when in May 2015 the measure was discussed in the Senate, the public interest was much higher. This is shown in Figure 4.1 which, updating Figure 2.1 in Chapter 2, summarizes Google search behaviour on the statutory pension age ('aow-leeftijd') and the pension age ('pensioenleeftijd'), the latter of which also captures trends in the second pension pillar.

The discussion on the retirement age entered into a new dynamics when in the run-up to the March 2017 Parliamentary elections several parties proposed to reverse the increase in the retirement age. This proposal featured prominently in the electoral campaigns. '*The retirement age should be set at 65 years again*' was one of the four statements presented to the party leaders in one of the large television debates ('Carrédebat' on March 5, 2017) that attracted 1.2 million viewers. The proposal to lower the retirement age back to 65 also featured as one of the 30 statements in the 'Stemwijzer', the largest of the online voting advice websites (in the run-up of the March elections it recorded 6.8 million users, out of almost 13 million eligible voters). Several political parties proposed a reversal of the retirement age, the largest of which (PVV and SP) are typically classified as populist (Akkerman *et al.*, 2017; van der Waal and De Koster, 2018). The elderly party – '50PLUS' – was the most pronounced advocate. Moving the retirement age back to 65 was the first election promise mentioned in their party platform, followed by e.g. the indexation of pension benefits to wages and more spending on home care. While the elderly party is not consistently classified as populist by populism scholars, proposals such as the reversal

of the retirement did earn them the label 'populist' in public discussions.[44] Whereas the Socialist Party's election program had initially let go of previous resistance towards later retirement and had proposed flexible retirement, two months before the election they changed their program and proposed a reversal of the retirement age to 65. In addition to 50PLUS and SP, also the Freedom Party (PVV) promised to reverse the increase in the retirement age (in addition to the Animal Party and three fringe parties that did not enter Parliament). Mainstream parties as well as FvD and GeenPeil (two small parties also considered populist) were not supportive.
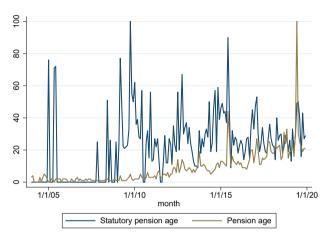
Figure 4.1 Monthly Google search activity on pensions, 2004–2019

Notes: Each line shows the relative search activity on the respective term over time. A peak of 100 indicates when in the 2004–2013 period the term was used most.
Source: Google trends.

The coalition government that took office in October 2017 (VVD, CDA, CU, D66) did not propose any amendments to the first pillar. Yet, the government did announce that they wanted to reach a deal with trade unions and employers on the second pension pillar, in line with proposals that had been made by social partners and independent members of the Social and Economic Council. As especially trade unions were reluctant about the reform, which reduced the security of pension entitlements, in the negotiations over this deal the bargaining was extended to include concessions on the retirement age in the first pillar. After long negotiations, in June 2019 (see the last peak in Figure 4.1) a deal was reached with social partners to reform the second pension pillar. In this deal,

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44. See, e.g., 'De opmars van 50Plus', Algemeen Dagblad, 16 November 2016.

the cabinet agreed to relax the pace of implementation as well as adjust the one-to-one adjustment to life expectancy to a more proportional link between working life (two thirds) and retirement (one third). Furthermore, funds were made available to facilitate early retirement for people in physically demanding professions, up to three years before the statutory retirement age. In an interview at the occasion of the pension agreement, the prime minister classified its former approach to the increase in the retirement age as 'hysteric'.[45]

44. DATA DESCRIPTION

Longitudinal survey (2004-2019)

In Chapter 2 we have used the DHS surveys of 2004 up to 2013 to trace attitudes towards reform of the public retirement system. In the first part of the chapter, we update this analysis to cover the period up to 2019. To be precise, in Chapter 2 we focused on respondents' answers to a question on reforming the public pension scheme: '*To make sure that the general old-age pension remains affordable certain measures have to be taken. Which of the following measures appeals to you most?*' Respondents could select a first and a second choice out of three options: (1) '*a lower general old-age pension at the age of 65*', (2) '*an increase in the old-age pension premium for people working*', and (3) '*an increase the age at which one will receive the general old-age pension by two years (from 65 to 67 years of age)*'. Given that there was a major reform implemented in 2012, the question was amended in the years following the reform. As of 2012, in the question there was no more mentioning of the age of 65. Furthermore, as of 2013 the question referred to the 2012 reform to gradually increase the statutory age of retirement ('*In 2012 it was decided to gradually raise the retirement age of the general old-age pension. To make sure that the general old-age pension remains affordable, possibly more measures shall be taken*').[46] Finally, also as of 2013, the third option – that of raising the retirement age – referred to '*a further increase*' in the retirement age instead of an increase by two years. Table A.4.1 in the Appendix gives the exact wording for each year.

To do more justice to concerns raised in the public debate over the impact of a higher retirement age – in particular differences in longevity between higher and lower educated people as well as their job characteristics – we augment our set of regressors. To capture longevity risk, we use respondents' assessment of the chance that they reach the age of 80

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45. Hendrickx, F. 'Met Rutte III keert het gematigde midden terug: wat eerst 'verstandig' pensioenbeleid was, heet nu "hysterisch" ', *Volkskrant,* 7 juni 2019.
46. In 2013, the text was slightly different: 'Recently it was decided to raise the retirement age of the general old-age pension'.

('*longevity_80*'). In addition, we include a measure that captures how satisfied respondents are (on a scale from 1 to 5) with their job ('*jobsatisfaction*'). Furthermore, we do not have information about the physical demandingness of respondents' jobs, but we do have other indicators for the work environment of respondents. The first, '*work_autonomy*', captures to what extent respondents agree with the following statement '*In my work I make my own decisions*'. The second, *'work_social'*, captures the extent to which respondents interact and cooperate with others in the work place (based on a total of four statements, see Appendix A.4.1). Furthermore, given the finding of Beetsma *et al.* (2019) that economic crises facilitate the acceptance of pension reforms, as a proxy for the economic outlook of the national economy we include a measure that captures to what extent households think the economic situation of their household will worsen or improve in the next five years ('*ec_optimism*').

June 2017 survey

In addition to this longitudinal survey, in June 2017 – just several months after the Parliamentary elections – we conducted a special survey on populism, which we also used in Chapter 3. In the survey, we also asked respondents whether they would support a reversal of the pension reform. To be precise, we asked people whether they agreed with the following statement '*The retirement age should be set at 65 years again*' (on a scale of 1 to 5, where 1 is 'strongly disagree' and 5 is 'strongly agree'). This is in line with the way the reversal of the pension reform was phrased during the election campaigns (see Section 4.3). In addition to this question, we use the index of populist attitudes of respondents based on six statements on the relationship between people and the elite ('*populism*', see Chapter 3 and Table A.4.1 in the Appendix). In addition, in our 2017 analysis we can include a variable to capture the literacy of respondents (unfortunately this control is not available for the times series analysis). We use the same measure as used in Chapter 3, i.e. probability literacy, which is based on the answers to four questions that require respondents to calculate the probabilities of certain events ('*prob_literacy*'). Note furthermore that the two datasets can be merged, so that in the 2017 analysis we can include all variables used in the longitudinal analysis.

Descriptive evidence on attitudes towards the retirement age

Figure 4.2 lists respondents' first preferred reform options for the PAYG pension system (in Figure A.4.2 in the Appendix, we also include the share of respondents who list an increase in the retirement age as their second and last reform choice). In panel (a) we show the results for the period in the run-up to the reform, when the option proposed to respondents was to increase the retirement age with two years. This panel corresponds to panel (a) in Figure 2.2 in Chapter 2, and documents the steady increase in the support

for a higher retirement age in the run-up of the reform. Panel b) shows the result for the period after the reform, when the reform option was relaxed to '*a further increase*' in the retirement age. Even without reference to an increase of 'two years', support for a further increase in the retirement age clearly dropped after the reform, and also decreased markedly thereafter.
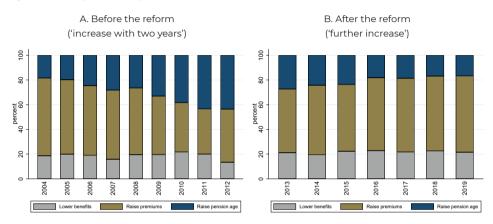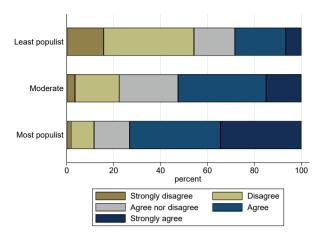
Figure 4.2 First preferred option for AOW reform

Notes: The panels show the percentage of respondents listing each option as their first preferred choice for AOW reform, before the reform (2004-2012, panel (a) and thereafter (2013-2019, panel (b)), as in panel (a) of Figure 2.2. Note that prior to the reform to option of an increase in the retirement age noted an increase of two years, after the reform the option was 'a further increase'.
Source: DHS, 2004-2019.

We now present some descriptive evidence on support for moving the retirement age back to 65. Overall, over 50% of the respondents in our 2017 survey supports a reversal of the age of retirement back to 65, out of which 18% strongly so. Less than a third of respondents disagrees with such a reversal. This is somewhat at odds with the 40% of respondents that mentioned the increase in the retirement age as the first preferred option for reform in 2012. An obvious explanation for the difference is the fact that in the 2017 study our respondents are presented with an 'undisciplined' choice without reference to the opportunity costs (level of premiums or a cut in benefits). Figure 4.3 gives a preview of the role of populist attitudes when it comes to the support for reform reversal, as we split the sample in three quantiles of populism (least populist, moderately populist and strongly populist). Of the respondents who have the least strong populist attitudes, less than 30% supports the reform reversal. This is almost three quarters (73%) for the most populist share of respondents.

Figure 4.3 Support for moving the retirement age back to 65
By three quantiles of populist attitudes

Note: The figure shows the distribution of responses to the statement 'The retirement age should be set at 65 years again', by three quantiles of populist attitudes.
Source: DHS 2017.

Summary statistics

Table 4.1 summarises both the dependent variables and the regressors of our two datasets (in Table A.4.2 in the Appendix we display the means of the longitudinal dataset by year). The first series of columns display the descriptive statistics of the dataset used for the update of our longitudinal analysis, while the last series of columns show the data for the 2017 analysis. The first lines are our dependent variables. The first, '*aowage_up_first*', is our dependent variable in the longitudinal analysis and indicates that respondents chose an increase in the retirement age as their first preferred reform option. Second, '*aow_age_65*' is based on the question of whether respondents would like to reverse the retirement age to 65. Note that the number of observations in 2017 is larger than the previous one because this second question was asked to all respondents, and not just to respondents of working age. Third, '*resistance_aowage_up*' traces the resistance of respondents towards an increase in the retirement, as measured with the disciplined question on AOW reform. It takes the value of 1 if respondents chose an increase in the retirement age as their first preferred reform option, 2 if it is their second preferred option and 3 if an increase in the retirement age is their last preferred option for reform. We use this variable as a benchmark for the results on support for reform reversal.

Table 4.1 Summary statistics

	Longitudinal survey (2004-2019)				Special survey on populism (2017)			
	count	mean	min	max	count	mean	min	max
Dependent variables								
aowage_up_first	22443	0.25	0	1	1370	0.19	0	1
aow_age_65					2359	3.32	1	5
resistance_aowage_up					1370	2.29	1	3
Covariates								
age	22443	46.62	16	72	2370	53.48	17	94
employee	22443	0.67	0	1	2370	0.48	0	1
self_employed	22443	0.07	0	1	2370	0.05	0	1
household	22443	0.08	0	1	2370	0.07	0	1
benefits	22443	0.12	0	1	2370	0.30	0	1
other	22443	0.05	0	1	2370	0.10	0	1
grossincome	17331	32.64	0	583	2370	2.70	0	1000
financial_wealth	19285	30.91	-69	3702	1980	37.00	-69	2617
female	22443	0.49	0	1	2370	0.48	0	1
partner	22443	0.74	0	1	2370	0.68	0	1
child	22443	0.51	0	1	2370	0.31	0	1
low	22439	0.20	0	1	2368	0.26	0	1
preuniversity	22439	0.09	0	1	2368	0.10	0	1
vocational_med	22439	0.24	0	1	2368	0.27	0	1
vocational_high	22439	0.29	0	1	2368	0.24	0	1
university	22439	0.18	0	1	2368	0.13	0	1
riskaversion	21289	5.20	1	7	2250	5.18	1.5	7
patience	21262	4.13	1.5	6.8	2331	4.12	1.7	6.3
conscientiousness	21903	3.64	1	5	2334	3.62	1.5	5
locus_of_control	21273	4.55	1.5	7	2311	4.53	2.1	7
openness					2326	3.41	1.2	5
agreeableness					2326	3.89	1.2	5
extraversion					2326	3.06	1	5
neuroticism					2326	2.48	1	4.9
health	21718	3.87	1	5	2234	3.80	1	5
longevity_80	21717	6.01	0	11	2082	7.01	0	11
jobsatisfaction	18517	3.99	1	5	1529	3.93	1	5
work_autonomy	17250	5.59	1	7	1571	5.47	1	7
work_social	17250	5.50	1	7	1571	5.40	1	7
ec_optimism	20963	3.09	1	5	2236	3.08	1	5
frequency	22443	4.59	1	17	2051	6.63	1	15
populism					2360	3.20	1	5
prob_literacy					2087	2.51	0	4
N	22443				2370			

Notes: The table reports summary statistics for our longitudinal sample (2004-2019) and our June 2017 special survey. The sample of the longitudinal survey is the same as the one in Table 2.1 but then updated with data for 2014-2019. *Source*: DHS, 2004-2019.

Most covariates are the same as in Chapter 2, although we made some slightly different choices regarding the imputation of missing values.[47],[48] Furthermore, when it comes to demographic characteristics, we now used a dummy for respondents that have a child (either in or outside the home) instead of having a grandchild, and use a dummy for having a partner instead of being married. In addition to this, we expanded the age group of '55 to 65' to '55 to 67', in line with the increase in the retirement age.[49] Last, in our 2017 analysis, we now include all 'Big Five' personality traits that are available in DHS since 2013 (while in the longitudinal analysis we only included 'conscientiousness'). We have discussed most covariates before, so we will not discuss our 'old' controls. Before turning to our new controls, however, it is important to note that in 2014 only a small share of the respondents answered the question on their preference for reform of the retirement age (see Table A.4.2 in the Appendix), which is due to an error in the routing. For this reason we should exert caution in the interpretation of the year effects of the pooled analysis, and should give greater value to the year effects of a fixed effects regression which abstracts from changes in the composition of the sample, also those unobservable characteristics we cannot control for (see Section 4.5).

Turning to our new control variables, when looking at the values for '*longevity_80*' over time, we can clearly see an increase in the probability that respondents assign to reaching the age of 80, from a low of 5.6 (on a scale of 0 tot 11) to a peak in 7.0 in 2017. This is in line with releases about increased longevity in the years after the reform. Furthermore, from Table A.4.2 we can also learn that optimism about the financial situation of the household ('*ec_optimism*') fluctuates over the business cycle, from a first local peak in 2007 (3.12 on a scale of 1 to 5) to a through in 2012 (2.89) to a record-high 3.25 in 2018 (Table A.4.2 in the Appendix).

Finally, Table 4.1 shows that despite imputing some missing values, for several variables there are quite some missing values due to the fact that not all questions had been asked

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47. As personality traits are found to be relatively stable (Cobb-Clark and Schurer, 2012; Cobb-Clark and Schurer, 2013; Salamanca, 2018), we have imputed the average value of the available observations in the entire period as we did in Chapter 2. For the variables that are more likely to vary – for instance, one's work environment – we used a stricter imputation method and the imputed value of the observation is the one for the next or previous year only, or the average thereof, if both were available. We have not imputed numbers for total income and wealth as we did in Chapter 2, but have instead imputed one's quartile position in the income and wealth distribution so as to filter out time effects (not reported in Table 2.1 but included in the regressions).
48. Furthermore, we corrected an error made in Chapter 2 regarding the scaling of the variable '*conscientiousness*'. This variable runs from 1 to 5, and not from 1 to 7 like the other personality traits, which we had assumed when adjusting the reverse-coded items. Last, we also chose a slightly different way to code the 'frequency of participation' of respondents.
49. The question about reform preferences was also asked to people of age 66 and 67 as of 2017.

to all respondents (questions on the work environment, for example, were only asked to respondents who work) or respondents did not participate in all modules of the survey. This means that our sample decreases the more controls we include.

4.5 ATTITUDES TOWARDS THE RETIREMENT AGE BEFORE AND AFTER THE REFORM

In Chapter 2, we studied attitudes of households vis-à-vis the retirement age in the run-up of the 2012 reform. In this section, we extend this analysis with household attitudes in the post-reform years (2013-2019), while using several additional controls. As noted before, it is important to keep in mind that the question we use to construct our dependent variable – i.e. support for raising the retirement age – has changed: previously it considered an increase from 65 to 67, while in 2013 and beyond people were asked about their support for a *further* increase in the retirement age.

Pooled regression

Like in Chapter 2, in our baseline regression we pool all observations together, whereby we now distinguish between attitudes before the reform (2004-2012) and after the reform (2013-2019). As in Chapter 2, we run a probit regression where a value of 1 means the respondent lists an increase in the retirement age ('*with two years*' before the reform, and '*a further increase*' thereafter) as his/her first preferred reform option. To make our coefficients comparable across regressions, in all regressions we restrict our sample to the group of respondents for whom we also have the new controls. This implies that we leave out respondents that have not been active on the labour market, and hence do not answer the questions on job satisfaction and job characteristics. To facilitate interpretation, as in Chapter 2 and 3, we standardise the scores on the psychology traits, self-reported health, job satisfaction, literacy and populist attitudes so that they have zero mean and a standard deviation of one. Table 4.2 shows our results. In column 1 and 3 we include the controls we used in Chapter 2 ('old controls'), while in columns 2 and 4 we add the additional controls just discussed (subjective longevity, work environment).

When comparing the two periods, we find that overall the same regressors turn out significant across periods, although there are some slight differences.[50] Turning to our new controls, we find that in the period before the reform subjective longevity and job satisfaction appear significant regressors at the 5% and 1%, respectively. Respondents with a lower expected longevity are less supportive of a higher retirement age, which confirms

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50. Our focus on a smaller sample means that our results for the pre-reform period can differ from the results presented in Chapter 2. We indeed see some differences, most notably for women and wealthy individuals.

our hypothesis 1. The same goes for respondents that are less satisfied with their job, confirming hypothesis 2. At the same time, the subjective health score that we included in Chapter 2 is no longer significant. Furthermore, the regressors that capture autonomy and social contacts on the work floor are not significant, as is the indicator of economic optimism about the financial situation of the household.

We now turn to the period after the reform. Here we look at the respondents that indicated that a further increase in the retirement age is their first preferred reform option, should more measures need to be taken to keep the pension system sustainable. Of our new controls, only subjective longevity comes out as a significant regressor – be it only at the 10% level – in the direction of our hypothesis. Furthermore, after the reform respondents that are more optimistic about the economic situation of their household actually prove slightly more in favour of a further increase (significant at 10% level). This at odds with the findings of Beetsma *et al*. (2019) that support for pension reforms increases when economic conditions worsen, and could signal that our measure is not a good proxy for respondents' perceptions of macroeconomic conditions.

Goodness of fit

Table 4.3 shows a decomposition of the goodness of fit of the full models of the pre- and post-reform period. First of all, our new controls – longevity, work traits, and economic optimism – account for a non-negligible share of the fit of the model, especially before the reform. Furthermore, as in our analyses presented in Chapter 2, education proves a major dividing line for support for a higher retirement age.[51] This is true for both periods, although the role of education becomes even more pronounced in the post-reform period. In addition, there also is a stronger division between the preferences of those with and those without families as well as between different kinds of workers. In addition, as was easily observable also in the descriptive charts, in the period after the crisis the time variation is much smaller.

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51. In the analysis in Chapter 2, occupational status was a rather material predictor of reform preferences, yet as we left out many respondents who do not work, it is logical that the contribution of occupational status is lower.

Table 4.2 Support for increase in the retirement age, before and after reform

	Before the reform (2004-2012) Increase of two years				After the reform (2013-2019) Further increase			
	Old controls (1)		All controls (2)		Old controls (3)		All controls (4)	
below35	0.00	(0.02)	-0.00	(0.02)	0.02	(0.02)	0.01	(0.02)
age45to55	-0.06***	(0.02)	-0.05***	(0.02)	-0.03*	(0.02)	-0.03*	(0.02)
age55to67	-0.03	(0.02)	-0.03	(0.03)	-0.01	(0.02)	-0.01	(0.02)
self_employed	0.10***	(0.03)	0.09***	(0.03)	0.09***	(0.02)	0.08***	(0.02)
household	-0.03	(0.09)	-0.03	(0.09)	0.04	(0.06)	0.05	(0.06)
benefits	0.07**	(0.03)	0.08**	(0.03)	0.10***	(0.02)	0.10***	(0.02)
other	0.01	(0.05)	0.02	(0.05)	0.06	(0.04)	0.06	(0.04)
incomeQ2	0.02	(0.02)	0.02	(0.02)	0.01	(0.02)	0.01	(0.02)
incomeQ3	0.03	(0.03)	0.03	(0.03)	-0.02	(0.02)	-0.02	(0.02)
incomeQ4	0.01	(0.03)	0.01	(0.03)	0.00	(0.02)	0.00	(0.02)
wealthQ2	-0.02	(0.02)	-0.02	(0.02)	0.02	(0.02)	0.02	(0.02)
wealthQ3	-0.03	(0.02)	-0.03	(0.02)	0.01	(0.02)	0.01	(0.02)
wealthQ4	-0.05**	(0.02)	-0.05*	(0.02)	0.01	(0.02)	0.01	(0.02)
female	-0.03*	(0.02)	-0.04**	(0.02)	-0.06***	(0.01)	-0.06***	(0.01)
partner	-0.03	(0.02)	-0.03	(0.02)	-0.02	(0.02)	-0.02	(0.02)
child	0.01	(0.02)	-0.00	(0.02)	-0.03*	(0.02)	-0.03*	(0.02)
vocational_med	-0.02	(0.03)	-0.02	(0.03)	0.02	(0.02)	0.01	(0.02)
preuniversity	-0.00	(0.03)	-0.01	(0.03)	0.04	(0.03)	0.04	(0.03)
vocational_high	0.03	(0.03)	0.03	(0.03)	0.04*	(0.02)	0.04*	(0.02)
university	0.10***	(0.03)	0.10***	(0.03)	0.14***	(0.03)	0.13***	(0.03)
riskaversion	-0.02**	(0.01)	-0.02**	(0.01)	-0.02***	(0.01)	-0.02**	(0.01)
patience	0.02**	(0.01)	0.02**	(0.01)	0.02***	(0.01)	0.02***	(0.01)
locus_of_control	0.03***	(0.01)	0.02**	(0.01)	0.01	(0.01)	0.00	(0.01)
conscientiousness	-0.02**	(0.01)	-0.02***	(0.01)	-0.02***	(0.01)	-0.02***	(0.01)
health	0.03***	(0.01)	0.01	(0.01)	0.01*	(0.01)	0.01	(0.01)
longevity_80			0.01**	(0.00)			0.01*	(0.00)
jobsatisfaction			0.03***	(0.01)			0.01	(0.01)
work_social			-0.00	(0.01)			-0.01	(0.01)
work_autonomy			-0.00	(0.01)			0.01	(0.01)
ec_optimism			0.01	(0.01)			0.01*	(0.01)
N	8027		8027		6815		6815	
Pseudo R^2	0.065		0.070		0.069		0.072	
Time effects	Yes		Yes		Yes		Yes	

Notes: The table reports marginal effects (measured at the mean) resulting from probit regressions of support for raising the retirement age as first preferred option. Column 1 by and large corresponds to column 2 in Table 2.2, except that the sample is restricted 2004-2012 instead of 2004-2013 and to respondents for whom we observe the additional set of controls (see column 2 and 4). In parentheses we report standard errors, which are clustered at the household level. The symbols *, ** and *** denote significance at the 10, 5 and 1 percent confidence level, respectively.

Table 4.3 Decomposition of goodness of fit

	Before the reform		After the reform	
	2004-2012		2013-2019	
	Pseudo R²	%	Pseudo R²	%
Age	0.003	5%	0.002	3%
Occupational status	0.005	7%	0.007	9%
Income/wealth	0.001	2%	0.003	5%
Demographics	0.002	3%	0.009	12%
Education	0.009	13%	0.020	28%
Personality traits	0.009	14%	0.012	16%
Health	0.003	4%	0.001	1%
Work traits	0.004	5%	0.002	2%
Economic optimism	0.002	3%	0.003	4%
Time	0.030	45%	0.013	19%
Total	0.068	100%	0.072	100%

Notes: Decomposition of goodness of models 2 and 4 in Table 4.2. The pseudo R² is decomposed using the Shorrocks–Shapley method (calculated with Stata's 'shapley2' command, see Chávez Juárez 2015)

Interpretation of the year effects

In Chapter 2, we found a very large role for the passage of time, as support vastly increased in the run-up of the reform. As seen above in the descriptive statistics, after the reform the support for a further increase in the retirement age dropped. In Figure 4.4 we take a look at the coefficients of the year dummies in more detail (which were suppressed in Table 4.2). To be precise, we pool both periods together, so that the base year is 2004 in all years (although, again, the question changes from an increase 'with two year' before the reform to a 'further increase' thereafter). Also when controlling for observed characteristics of respondents, we find the same trend as we observed in Figure 4.2: in the years after the reform support for a further increase in the retirement age dropped.[52] A possible explanation for the decreased appetite for a higher retirement age is that the concerns over prolonged careers heightened in the aftermath of the reform, especially when people realised what the effect was of the link of the retirement age to life expectancy.

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52. In 2018 and 2019, the upper confidence bounds were below the lower confidence bounds of 2013-2015.
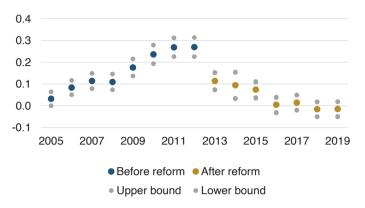
Figure 4.4 Year effects of reform preferences, 2004-2019

Notes: The figure shows the coefficients of the year dummies from probit regression on support for an increase in the retirement age, with all year effects in the same regression (i.e. Table 4.2. columns 2/4 combined). Up to 2012, respondents were asked about their support for an increase in the retirement age from 65 to 67, after this respondents were asked about their support for a further increase. Grey dots show lower and upper confidence bounds.
Source: DHS, 2004-2019.

Robustness

As noted, the time pattern of our results could be driven by compositional effects. Therefore, we estimate the longitudinal analyses with a fixed effects regression. As such, the year dummies are estimated for the same individuals, allowing us to filter out the effects of compositional changes in the panel on the time effects. Figure A.4.2 in the Appendix compares the year coefficients of the fixed effects regression with those from the pooled regression as shown in Figure 4.4. Although the time pattern in the coefficients are by no means identical, the main patterns are similar. In both models, support for an increase in the retirement age increased in the run-up of the reform, while support for further raising the retirement age decreased over the years after the reform.

Second, we assess to what extent respondents' frequency of participation in our survey influences our results. As noted in Chapter 2, when respondents are required to think about options for AOW reform several times, they could develop concerns about the scheme's sustainability. When the 'frequency of participation' of respondents is entered in our regression, we do not find evidence of such a 'treatment effect' (see Table A.4.3 in the Appendix).

4.6   SUPPORT FOR REFORM REVERSAL AND POPULISM

In this section we turn to support for reform reversal and the role of populist attitudes, as we evaluated in our June 2017 survey. As introduced in Section 4.4, we assess this by analysing respondents' answers to the following statement: '*The retirement age should be set at 65 years again*', whereas the response category ranges from 1 (strongly disagree) to 5 (strongly agree). This statement is in line with how the retirement age was discussed during the election campaigns (e.g. a large television debate with party leaders) and how people were informed about party positions on the largest of the online voting advice websites (see Section 4.3). We compare the answers to this question with the resistance of respondents towards an increase in the retirement age in a disciplined setting, as discussed above.

Whereas the analysis above has confirmed the relevance of longevity expectations and some aspects of working conditions (hypothesis 1 and 2), in this section the regressors of our main interest are literacy and populist attitudes, for which we have data for 2017 only. We will also include longevity, but will not include job satisfaction as this comes at the cost of a loss of too many observations, which we want to avoid now that we have a much smaller sample.[53] We drop variables that were not significant in the regression of Table 4.2 in the period we study (i.e. after the reform) and whose inclusion would lead to a substantial loss of observations. On the functional form, as our dependent variable has five response categories we employ an ordered probit regression (although results are almost identical when using OLS).

Results

Table 4.4 below displays our results. The first line of the table confirms our hypothesis 4: respondents with strong populist attitudes are significantly more supportive of a decrease in the retirement age. They would also oppose a further increase the retirement age in a disciplined choice setting, to be financed by either a cut in benefits or an increase in premiums. Our hypothesis 3 is also confirmed: respondents with poor literacy skills are more likely to support a reduction in the retirement age. Interestingly, people with poor literacy skills are not significantly stronger opposed to an increase in the retirement age in a disciplined choice setting. Although we realise that the questions are not the same, i.e. support for earlier retirement does not equal resistance towards higher retirement, given the large difference we take this result as a sign of fiscal illusion. Our results also confirm

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53.   When including job satisfaction in our regression, the sample drops from 1755 to 1283 observations. The coefficient for job satisfaction is not significant.

that respondents with lower longevity are supportive of decreasing the retirement age and also less keen on an increase in the retirement age in a disciplined setting, supporting our hypothesis 1.

As to the other controls, we only discuss some highlights. First, people receiving benefits (pensions and otherwise) are strongly opposed to a lower retirement age, also in a disciplined choice setting. Furthermore, whereas higher education comes with stronger opposition to reform reversal, this is not the case in a disciplined setting. Furthermore, the coefficients for the various age groups are now more pronounced than in the 2004-2019 analysis. In particular, people in their years just prior to retirement now appear strong proponents of a lower retirement age.

To assess the relevance of our various regressors, in Table 4.5 we decompose the R^2 of the regressions in Table 4.4. Turning to unconditional support for a lower retirement age, populist attitudes appears the largest predictor of all regressors, explaining 42 percent of the model fit. Yet, also in the disciplined setting populist attitudes appear to have the strongest predictive power. Furthermore, literacy proves a relevant regressor only when it comes to attitudes towards reform in an undisciplined setting.

Table 4.4 Attitudes towards lowering and raising the retirement age

	Retirement age back to 65 1=strongly disagree, 5=strongly agree		Resistance to increase in retirement age 1=first preferred option, 3=least preferred option	
	Ordered probit (1)		Ordered probit (2)	
populism	0.44***	(0.04)	0.17***	(0.04)
prob_literacy	-0.11***	(0.03)	0.03	(0.04)
age35to45	0.14	(0.10)	0.16	(0.11)
age45to55	0.19*	(0.10)	0.24**	(0.11)
age55to67	0.35***	(0.10)	0.29**	(0.12)
age67plus	0.08	(0.13)		
self_employed	-0.21*	(0.13)	-0.33**	(0.15)
household	-0.26**	(0.12)	-0.15	(0.17)
benefits	-0.50***	(0.09)	-0.47***	(0.11)
other	-0.09	(0.11)	-0.23	(0.17)
incomeQ2	0.09	(0.08)	0.23**	(0.11)
incomeQ3	0.07	(0.09)	0.18*	(0.11)
incomeQ4	0.00	(0.09)	0.15	(0.12)
female	-0.01	(0.06)	0.21**	(0.08)
partner	0.02	(0.06)	0.18**	(0.08)
child	0.14**	(0.07)	0.10	(0.08)
vocational_med	-0.03	(0.08)	0.09	(0.10)
preuniversity	-0.43***	(0.11)	-0.15	(0.14)
vocational_high	-0.29***	(0.09)	0.07	(0.11)
university	-0.73***	(0.11)	-0.21	(0.14)
riskaversion	0.08***	(0.03)	0.10***	(0.03)
patience	-0.02	(0.03)	0.00	(0.04)
locus_of_control	-0.13***	(0.03)	0.01	(0.04)
openness	0.07	(0.06)	-0.08	(0.07)
conscientiousness	0.07**	(0.03)	-0.01	(0.04)
agreeableness	0.08	(0.06)	0.01	(0.07)
extraversion	-0.09*	(0.05)	-0.03	(0.05)
neuroticism	0.04	(0.04)	-0.04	(0.05)
longevity_80	-0.04***	(0.01)	-0.03**	(0.02)
ec_optimism	-0.05*	(0.03)	0.03	(0.04)
N	1755		1209	
Pseudo R²	0.128		0.044	

Notes: The table reports results of ordered probit regressions for support for moving the retirement age back to 65 (column 1) and resistance to further raising the retirement age in a disciplined setting (column 2). In parentheses we report standard errors, which are clustered at the household level. The symbols *, ** and *** denote significance at the 10, 5 and 1 percent confidence level, respectively.
Source: DHS, 2017

Table 4.5 Decomposition goodness of fit

	Move retirement age back to 65		Resistance to increase in retirement age	
	Pseudo R²	%	Pseudo R²	%
Populism	0.054	42%	0.010	22%
Literacy	0.009	7%	0.000	1%
Age	0.007	6%	0.002	5%
Occupational status	0.007	6%	0.009	20%
Income/wealth	0.004	3%	0.002	4%
Demographics	0.002	2%	0.007	15%
Education	0.027	21%	0.006	13%
Personality traits	0.014	11%	0.007	16%
Longevity	0.002	1%	0.001	3%
Economic optimism	0.002	2%	0.000	0%
Total	0.128	100%	0.044	100%

Notes: Decomposition of goodness of fit of models in Table 4.2. The pseudo R² is decomposed using the Shorrocks–Shapley method (calculated with Stata's 'shapley2' command, see Chávez Juárez 2015)
Source: DHS, 2017

Robustness

As was the case in Chapter 3, there are reasons to be suspicious of endogeneity of the regressor of our main interest, populist attitudes. The model could, for example, suffer from simultaneity bias in case preferences towards the retirement age have also led people to grow populist sentiment due to the reform and the quicker pace of the increase. Furthermore, in column 1 in Table 4.4, the dependent variable – support for reform reversal – and 'populism' come from the same survey. Both factors could cause our estimates to be biased. Therefore, we run the same IV regression that we employed in Chapter 3 for our current regressions. In Chapter 3, we found that one of our instruments – pre-crisis trust in the management of financial firms – was uncorrelated with fiscal preferences, which allows us to test the exogeneity condition of our instruments. Yet, this is not necessarily the case for our current dependent variables, support for reform reversal and resistance to a further increase in the retirement age. Table A.4.4 in the Appendix indeed reports a significant correlation of pre-crisis trust in the management of financial firms and support for reform reversal (as well as with pre-crisis trust in politics, as we found in Chapter 3). This means that this variable is only a valid instrument under the rather strong assumption that this correlation runs through populist attitudes only and there is no direct effect from pre-crisis

attitudes towards the financial sector and attitudes towards pension reform. Comfortingly, the correlation between pre-crisis attitudes towards financial sector management and resistance towards an increase in the retirement is virtually zero.

Table A.4.5 in the Appendix reports the results of our IV estimation. In our first stage, we regress populism on pre-crisis trust in politics and trust in the integrity of the management of financial firms and all other controls of the second stage (the coefficients of which are suppressed). Note that using these instruments results in a significantly smaller sample (613 for support for reform reversal, 330 for our disciplined question on reform). The table shows that our two instruments, pre-crisis trust in politics and the financial sector management, again turn out to be highly significant regressors in the first stage. Both 2SLS regressions pass the Sargan test of over-identifying restrictions, although we have reason to be suspicious of the statistic when it comes to our first dependent variable as this test is only valid when at least one of the two regressors is exogenous. Comparing OLS and 2SLS results, according to the Wu-Haussman test we cannot reject endogeneity of populist attitudes in the case of the second regression, which warrants the use of the 2SLS estimation. Yet, also according to our IV estimation populist attitudes exert a significant positive effect on resistance to an increase in the retirement age.

4.7   CONCLUSION

In Chapter 2, we described a successful attempt of the Dutch cabinet to raise the retirement age. Yet, in the years that followed the reform was widely contested. This chapter updated the analysis in Chapter 2 with data up to 2019 and extended the control set to cover several variables that received most attention in the public debate. Furthermore, we inspected attitudes towards a reversal of the 2012 reform, as had been proposed by several challenger parties in the run-up of the Parliamentary elections in March 2017.

The update of our analysis with data up to 2019 corroborates the vast increase in support for an increase in the retirement age that took place in the decade running up to the enactment of the reform (2004-2012), while demonstrating a decrease in appetite for further reform in the years thereafter (2013-2019). Furthermore, in the period after the reform differences in education, demographic characteristics and occupational status have become more pronounced dividing lines of reform attitudes. Augmenting our dataset with an additional set of controls allowed us to test several hypotheses on the drivers of resistance towards the reform that might help explain these trends. First of all, we assess to what extent grievances about postponed retirement – in particular lower longevity and lower job satisfaction – are at the basis of reform resistance. Both in our longitudinal and

2017 analysis we indeed find that differences in longevity appear a systematic driver of attitudes. People with a lower expected longevity are more reluctant to support a higher retirement age, and are more in favour of a reversal of the retirement age. Furthermore, we find that low job satisfaction goes at the expense of support of a higher retirement age. Secondly, in our analysis of our 2017 survey we test to what extent poor literacy can explain resistance to reform. As expected, we find that respondents with poorer literacy skills are more keen on a reversal of the retirement age back to 65, although they do not have markedly different preferences in a disciplined setting. Thirdly, we test to what extent populist attitudes drive reform resistance. As hypothesized, we find that populist attitudes were strongly significant as well as highly relevant predictors of reform attitudes. For respondents' resistance to a further increase in the retirement age, this result was robust to using an IV estimation in which we instrumented populism with pre-crisis trust in politics and the management of financial sector firms.

For policymakers who wish to foster the acceptance of economic policy reforms, this offers several leads for action. In line with Chapters 2 and 3, the acceptance of reforms will be higher when voters are confronted with disciplined choices, something that can be spurred by fiscal councils. Similarly, in line with Chapter 3, investing in the skills and knowledge of voters can diminish the incidence of fiscal illusion. Yet, our results also show that fiscal illusion is not the only challenge to overcome. In particular, reforms will also stand a better chance when policymakers manage to incorporate grievances of large parts of the public in the design of reform policies, for instance by grandfathering clauses or compensating policies. Such measures can have a double dividend. On the one hand, they can make reforms proposals attractive to a larger group of voters. At the same time, many of the grievances mentioned above are most hardly felt by lower-educated (and likewise, low-wage) workers, and less so by the political elite which is overrepresented by higher-educated individuals (Wille and Bovens, 2014).[54] By demonstrating effort to address grievances of lower-educated workers, the political elite can also signal to voters that they are doing their utmost to act in the interest of the public at large.

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54. Furthermore, households with higher earnings typically dispose of more private wealth that they can use to finance retirement (Knoef *et al.*, 2016). Research on the phasing out of early-retirement schemes has highlighted that high-wage workers were far less affected by the reform, as they were able to cushion the impact of the reform by private wealth (Lindeboom and Montizaan, 2018).

PART II:
COLLECTIVE BARGAINING

Chapter 5

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STAGGERED WAGES, UNANTICIPATED SHOCKS AND FIRMS' ADJUSTMENTS

5.1   INTRODUCTION[55]

This chapter evaluates the effect of a particular source of nominal rigidity in labour markets, namely the staggered nature in which wages adjust to changes in the economic environment. Wages are staggered when contract decisions are taken at different points in time and these decisions are valid for a certain number of periods (Taylor, 1979).[56] At any point in time, only some industries reset their wages, and during the bargaining process employers and unions are likely to respond to events which happened in the previous period. Also, as the bargained wage is valid for a number of periods, unions form inflation expectations for the contract period in order to insure workers against real wage losses. Expectations on productivity and profitability are likely to be relevant as well, as unions would make sure that wages grow in line with industry profits. Sudden changes in expectations can therefore induce wage differentials across sectors bargaining in nearby months.

Staggered wages, also known as Taylor contracts, have been introduced in macroeconomics to understand the persistence of inflation dynamics and the transmission of monetary policy.[57] In the most recent macroeconomic literature, staggered wages are the most common method of incorporating labour market rigidities in quantitative macroeconomic models (Taylor, 2016), and represent a nominal friction that helps these models in reproducing the volatile inflation and employment dynamics observed in the data (Christiano *et al.*, 2005; Hall, 2005; Smets and Wouters, 2007; Gertler and Trigari, 2009).

Empirical studies have well documented that wages are indeed reset infrequently. On the basis of a large-scale survey among European firms, it has been documented that in the majority of firms wage resets take place only once a year or even less frequently (Fabiani *et al.*, 2010). At the same time, on the basis of the same survey, it has been documented that firms may be able to adjust their wage bill through other means, e.g. by slowing promotions, cutting bonuses, adjusting working hours or choosing cheaper hires to replace workers who leave the firm (Babecký *et al.*, 2012; Babecký *et al.*, 2019).

56. This wage rigidity is different from downward nominal wage rigidity (DNWR), which refers to the absence of nominal wage cuts. Downward nominal wage rigidity has been studied in Altonji and Devereux (2000), Elsby (2009), Schmitt-Grohé and Uribe (2016).
57. See Friedman (1977), Fischer (1977), Calvo (1983) and Roberts (1995).

Likewise, in case firms offer worker insurance within the firm, they can forego profits to absorb shocks (Guiso *et al.*, 2005). If the scope of such adjustments is large enough, the employment response to staggered wage setting may be more muted.

In this chapter, we take a microeconomic perspective to empirically assess how staggered wages affect firms' wage versus employment adjustments in the Netherlands, where wage resets are established in collective labour agreements (collectieve arbeidsovereenkomst, CAO hereafter) that apply at the sector level, but also at the company level for larger firms. The case of the Netherlands is particularly interesting because nominal rigidities due to contract staggering are very pervasive, due to the large coverage of collective agreements and the relatively long duration of contracts. We use detailed data on professional forecasts specific to the Dutch economy to identify the timing of a big unanticipated macroeconomic shock that created a substantial differential in wage growth among CAOs negotiated in nearby months. We identify this shock in October 2008, after which average wage growth agreed upon in CAOs fell from 3.5 percent to 1 percent in two quarters. We then use a large matched employers-employees dataset obtained from the combination of several administrative data sources to show the effect of contract staggering on a battery of firm-level labour market outcomes. In particular, we consider firms' responses on total and flexible employment levels and the number of vacancies in each firm and adjustments to various pay components (the ordinary wage, bonuses, non-wage benefits, and overtime hours paid to the workers), to understand which adjustment margins firms use the most.

Identifying the effect of collective bargaining in micro-econometric analysis is challenging. One particular concern is that the period in which the parties bargain over wage resets may be endogenous, as uncertainty may cause both parties to delay the renegotiation process (Danziger and Neuman, 2005). To address this, we exploit a particular feature of the Dutch context to generate plausible exogenous variation around the timing of the shock: in each sector or company in our data, the start date of the subsequent CAO always coincides with the expiry date of the previous CAO. By exploiting variation in the staggered and pre-determined start dates around the aggregate shock, our identification strategy ensures that the start date of each CAO is independent on the uncertainty generated by the shock. This allows us to estimate the causal effect of the wage rigidities created by contract staggering on the firms and the workers who signed their CAO before the shock and did not anticipate it.

The paper closest to our own research is Díez-Catalán and Villanueva (2014) who study the effect of widespread nominal wage rigidity in Spain, where contract staggering is caused by the presence of province-sector level agreements with a duration of typically two years or even longer. They examine workers' total wage growth and transition to

unemployment after the Lehman Brothers default, which was an external shock to the Spanish economy, and find that this nominal rigidity increases the probability of transition to unemployment, especially among minimum wage workers. Differently from them, in this chapter we investigate firms' labour cost adjustments after the start of the 2009 recession in the Netherlands. We improve on this study along two main dimensions: first, using data on collective agreements in place in the Netherlands in the period 2006-2012, we provide evidence on how bargaining has changed during the crisis period. Then, using an identification strategy based on CAOs pre-determined start dates (instead of the endogenous agreement dates), we identify the causal effect of nominal rigidities on firms' labour market outcomes.

Our main result is a precisely estimated zero effect of contract staggering on employment. Firms that agreed on high wage growth before the recession did not cut employment levels more than those firms that were able to witness the shock and were able to agree on lower contractual wage growth. Instead, we show that non-anticipating firms have been able to adjust their wage bill by cutting bonuses and benefits and that they seem to have been able to curb the so-called incidental pay component, that we do not observe directly.[58]

This result partially contrasts with macroeconomic models such as those of Hall (2005) and Gertler and Trigari (2009), in which staggered wage bargaining leads to more volatile responses to an aggregate shock of all labour market indicators, including employment. We find significant employment losses only in sectors covered by contracts with duration larger than thirty months, although this effect is not large enough to affect our overall results. Such durations are much higher than what is normally assumed in macroeconomic models for the U.S. economy, such as Christiano *et al.* (2005), Smets and Wouters (2007), Gertler and Trigari (2009), where wage resets often occur every three to four quarters.

Our results have important implications for the role of collective bargaining. Out of concern for job losses, international organizations have often advocated to reduce nominal rigidities in wage setting, for instance by moving bargaining to the firm level. Yet, such reforms have proven contentious and have raised concerns about the overreliance on wage moderation, the erosion of collective bargaining and the consequences on wage inequality (see Dustmann *et al.*, 2009; Blanchard *et al.*, 2014; Boeri and Jimeno, 2015; Addison *et al.*, 2017). Our analysis shows that the nominal rigidities that often result from collective

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58. Incidental pay is the residual of the total wage growth minus contractual wage growth, and is typically estimated at the macro-level. It comprises the pay increases that workers receive due to e.g. promotions and performance ratings as well as compositional changes in the workforce (e.g. replacing workers who leave with cheaper hires).

bargaining do not necessarily come at the cost of employment loss, and suggests that building more room for discretionary pay components in collective agreements may be a way to alleviate such rigidities.

The chapter proceeds as follows. Section 5.2 discusses the institutional framework. Section 5.3 describes the data and the descriptive statistics. Section 5.4 presents the identification strategy being used and the results of the empirical analysis. In Section 5.5 we present robustness checks and examine heterogeneity in contract duration. Section 5.6 concludes.

5.2   INSTITUTIONAL FRAMEWORK

Wage setting in the Netherlands is characterized by a dominant role for collective bargaining. According to the OECD, almost 80 percent of all wage earners in the Netherlands are covered by a collective agreement (CAO), as compared to an average of 32 percent across OECD countries.[59] The high coverage is due to the presence of the so-called *erga omnes* provisions, i.e. agreements that automatically apply to all workers within firms that are members in a signing employer association, as well as government-issued extensions of collective agreements to all firms and workers in a sector (Visser, 2016b; Hijzen *et al.*, 2019).

The great majority of workers are covered by a CAO that applies to the whole sector, although especially large firms can have their own firm-level agreements. As in many other countries, in the Netherlands there has been a shift towards a more decentralized wage setting. The main route to decentralization has been the increasing scope for customization within sectoral agreements. Examples include the use of minimum standards that can be topped up at the firm level and the so-called *à la carte* provisions which allow the bargaining parties of each firm to select and include the wage and non-wage components they prefer. Wage bargaining in the Netherlands is furthermore set in a corporatist setting, where various coordination mechanisms are in place to sustain collective bargaining: to ensure that confederations have a mandate, unions and employers' confederations participate to an annual discussion aimed at setting the guidelines for the next wage increases and other bargaining priorities. This internal coordination is quite strong, especially for sectoral unions that often agree on a minimum or maximum wage demand and can possibly team up with employers against dissident unions. At the same time, since the 1982 Wassenaar agreement ended a severe wage-price spiral, unions have come to adhere to a 'jobs before wages' strategy which has been credited for a sustained decrease in unemployment levels over the 1980s and 1990s (Visser, 1998).

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59. See https://stats.oecd.org/Index.aspx?DataSetCode=CBC

For the purpose of this chapter, it is important to mention some details on the timing and the degree of synchronization of Dutch collective agreements. In theory, wages are bargained according to a specific calendar: based on the forecasts published by the Netherlands Bureau for Economic Policy Analysis (CPB) in September, parties define their wage demands for the following year and set bilateral guidelines for wage increases and other bargaining priorities.[60] In practice, as we will document in the next section, bargaining takes place throughout the year. This is different from Japan or the US where wage bargaining is synchronized across sectors (Olivei and Tenreyro, 2010). Furthermore, while collective agreements in Japan or the U.S. typically have a duration of 12 months, CAOs in the Netherlands can have longer as well as shorter durations. On average, the duration of a collective agreement is about 20 months (Visser, 2019), while the average duration of wages among the 17.000 European firms surveyed for the ECB Wage Dynamic Network initiative was about 15 months (Lamo and Smets, 2009). Hence, the Dutch labour market is characterized by a relatively high degree of contract staggering.

Concerning the contract period of CAOs, it is important to mention that there can be a substantial difference between the start date and the signature date of collective agreements. Like in other countries, the Dutch practice is that if there is no new agreement the old agreement remains valid after expiration (so-called ultra-activity, see also Chapter 6). This implies that the wage level stays the same, i.e. there is no wage increase. Yet, in case a new contract is agreed upon after the pre-determined start date, the resulting wage increase can apply retrospectively (retro-activity) (Hijzen *et al.*, 2019).

Last, it is worth mentioning that collective agreements typically govern most components of workers' wage bill, such as the ordinary wage, other pay components (e.g. the thirteenth month), contribution to pension funds and benefits (e.g. travel costs). However, firms may have also some room for discretion, such as pay components that depend on performance or the granting of promotions.

5.3   DATA AND DESCRIPTIVE STATISTICS

Collective agreements data

Our dataset on collective labour agreements (CAOs) is obtained from the Ministry of Social Affairs (MinSZW). The dataset covers the most important CAOs in terms over worker coverage that were in place in the 2006-2012 period, and contains information on the agreement, the start and the expiration dates of agreements, the contractual wage

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60. CAOs in the Netherlands typically do not contain clauses to index wage growth to inflation.

increase as well as incidental and structural adjustments in other pay components. Also, it contains the Standard Business Classification (SBI) code that identifies the sector of the economic activity of each firm and is used to merge the CAO information with the companies and workers information available in the Matched Employers Employees Data.

Table 5.1 reports descriptive statistics regarding contract durations, agreement delays and contractual wage growth, and shows how the main bargaining characteristics evolved. The duration of collective agreements is the characteristic that has remained most stable. This is due to the fact that in many sectors social partners tend to stick with the same contract duration. Instead, contractual wage growth is more volatile and is closely related to the business cycle: the annualized wage increase and the total annual wage growth (that includes incidental and structural adjustments in other pay components) were around 3 percent in the pre-crisis period, then fell to 1.3 percent during the 2009 crisis and slightly recovered to 1.8 percent in 2011. More importantly, the table shows that collective agreements are signed with a delay of, on average, several months.
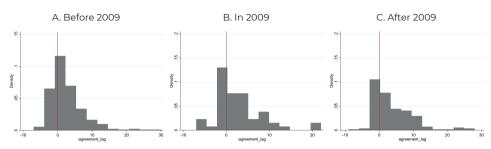
Table 5.1 Summary statistics (CAO Data)

	2007	2008	2009	2010	2011
By CAO start date					
Contract duration (months)	19.4	21.4	18.6	18.6	21.7
Agreement delay (months)	2.3	2.0	4.8	2.4	2.9
Wage growth (%)	2.9	3.0	1.3	1.2	1.7
N° agreements	88	71	74	73	60
By CAO agreement date					
Contract duration (months)	19.7	21.0	18.0	18.7	21.2
Agreement delay (months)	2.3	2.2	2.8	3.8	3.2
Wage growth (%)	2.9	3.1	1.3	1.1	1.8
N° agreements	95	70	56	80	60

Notes: The table reports means and the number of collective agreements by CAO start date (first lines) and by CAO agreement date (last lines). The percentage wage growth is annualized (to normalize over contract duration) and also includes other structural pay increases (e.g. 13th month).

Figure 5.1 shows the distribution of the agreement delay before, during and after the 2009 recession. The figure shows that social partners typically reach an agreement in the months around the expiry date of the previous contract (which corresponds to the start date of the new agreement). Around 25% of contracts are agreed exactly on time, while in many other cases agreements are reached either a few months before the start date, or with a delay of one to several months.

Figure 5.1 Agreement delay

Notes: The figure shows the distribution of the agreement delay of contracts started before 2009 (panel (a)), in 2009 (panel (b)) and after 2009 (panel (c)). The agreement delay is expressed in months.

For the purpose of this chapter, it is interesting to notice that agreement delays have increased, on average, during the crisis period: in 2009 (middle panel) the share of contracts agreed on time has dropped by more than 5% and the overall distribution is more skewed to the right than the distributions before and after 2009. Instead, the distribution of the agreement delays before and after 2009 are very similar to each other. This descriptive evidence supports the idea that agreement dates are endogenous. The parties involved in the bargaining process may have decided to delay their decisions or postpone the whole negotiation process when the shock hit the economy. For this reason, in the robustness section we also perform our analysis using agreement dates instead of start dates.

To study the consequences of the rigidities induced by contract staggering in the recession period, the empirical analysis focuses on the analysis of 'reference CAOs'. We define the reference CAO as the collective labour agreement that was in place during the 2009 recession in each company and sector.[61] Table 5.2 reports the list of all reference CAOs and shows the agreed percentage wage increase and the validity period of each reference CAO.

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61. In case of two CAOs in place during this period, the reference contract has been defined on the basis of the number of months in 2009 in which each of them was in place or on the basis of information availability.

Table 5.2 CAO data

CAO name	Wage increase %	Validity Period	CAO name	Wage increase %	Validity Period	CAO name	Wage increase %	Validity period
ABN-AMRO	3.50	Jan-09 / Mar-10	Gemeentelijk vervoersbedrijf (GVB)	-	Jan-09 / Jan-10	Postkantoren	1.00	Apr-09 / Jan-10
AGF	0.00	Apr-09 / Apr-10	Gemengde- en speelgoedbranche	1.25	Jan-09 / Jan-11	Praxis group	0.29	Feb-09 / Feb-11
ANWB	3.25	Apr-08 / Apr-10	Getronics Nederland	0.00	Jan-09 / Jan-10	Primair Onderwijs	0.84	Jan-09 / Jan-10
Achmea	3.25	Jan-08 / Jun-09	Glastuinbouw	2.5	Jul-07 / Jul-10	Provinciepersoneel	0.70	Jun-07 / Jun-09
Afbouw	2.00	Jan-08 / Jan-10	Goederenvervoer Nederland (KNV)	3.50	Oct-08 / Jan-10	RABO-bank	3.00	May-07 / May-09
Akzo Nobel Pharma bv	3.00	Apr-07 / Apr-09	Grafi-media	2.75	Feb-07 / Feb-10	Recreatie	3.50	Jul-08 / Jul-09
Albert Heijn (distributie org.)	3.50	Oct-08 / Oct-09	Heineken Nederland Beheer BV	2.50	Jul-08 / Oct-09	Reisbranche	1.00	Apr-09 / Apr-11
Algemene Banken	3.00	Jan-08 / Apr-10	Hema	0.75	Feb-09 / Feb-10	Rijkspersoneel	2.30	Jan-07 / Jan-11
Apotheken	3.25	Apr-08 / Apr-10	Hoger beroepsonderwijs (HBO)	1.30	Aug-07 / Aug-10	SNS Reaal	3.25	Jun-08 / Jun-09
Arbo Unie Nederland	1.00	Jul-08 / Jul-11	Holland Casino	0.00	Apr-09 / Jan-11	SNT Nederland	1.13	Jul-08 / May-10
Architectenbureaus	1.00	Apr-09 / Apr-10	Horeca- en aanverwant bedrijf	2.76	Apr-08 / Apr-10	Schilders- en glaszetbedrijf	1.50	Mar-09 / Mar-11
Atos Origin	4.00	Jan-08 / Jan-09	Hoveniersbedrijf	0.60	Mar-09 / Mar-10	Schoonmaak	3.50	Apr-08 / Jan-10
Bakkersbedrijf	3.50	Jun-08 / Aug-09	Huisartsenzorg	1.50	Jan-09 / Apr-10	Selektvracht B.V.	1.50	Apr-09 / Jan-11

Table 5.2 Continued

CAO name	Wage increase %	Validity Period	CAO name	Wage increase %	Validity Period	CAO name	Wage increase %	Validity period
Beroepsgoederenvervoer	3.50	Oct-08 Jan-10	IKEA Nederland b.v.	3.25	Oct-08 Oct-09	Slagersbedrijf	1.00	Apr-09 Apr-11
Beroepsonderwijs	0.07	Feb-09 Apr-11	ING	3.00	Jul-08 Jan-12	Sociale Verzekeringsbank	3.25	Apr-08 Apr-10
Betonproduktenindustrie	1.00	Mar-09 Apr-11	Informatie & communicatie	3.00	Jan-08 Jan-10	Sociale werkvoorziening	3.00	Nov-07 Mar-10
Beveiligingsorganisaties	3.25	Aug-08 Jul-11	Jeugdzorg	3.50	May-08 May-10	TNT N.V.	0.70	Apr-09 Jan-12
Bloemen en planten (groothandel)	3.00	Jan-08 Jul-09	KLM-grondpersoneel	1.25	Apr-09 Apr-10	TNT Post B.V.	2.86	Apr-08 Apr-09
Bloemen- en Plantend-etailhandel	1.40	Jan-09 Apr-11	KPN Contact	2.00	Jul-08 Jan-10	Tankstations en wasbedrijven	0.00	Jan-09 Jan-10
Boekhandel en Kantoor-vakhandel	1.75	Jan-09 Feb-10	KPN N.V.	0.00	Jan-08 Jan-10	Taxivervoer	1.50	Mar-09 Jan-14
Bouwnijverheid	1.75	Apr-07 Jul-09	Kappersbedrijf	1.00	Jan-09 Jul-10	Technische groothandel	3.25	Jan-08 Apr-10
Canon	3.50	Apr-08 Jul-09	Kinderopvang	2.00	May-08 May-09	Technische installatiebedrijven	3.50	Feb-08 Dec-09
Carrosseriebedrijf (metaal)	3.50	Feb-08 Dec-09	Landbouwwerktuigen exploiterende	2.00	Apr-07 Apr-09	Timmerindustrie	1.25	Jan-09 Jul-09
Contract-cateringbedrijf	3.50	Apr-08 Apr-10	Levensmiddelen (groot.)	3.25	Apr-08 Apr-10	UWV (Uitvoeringsorgaan)	2.60	May-07 May-10
Corus Staal B.V.	0.50	Apr-09 Apr-10	Levensmiddelen en/of zoetwaren	2.00	Jul-07 Jul-09	Uitzendbureaus	1.00	Jan-09 Apr-11
DHL Express Nederland	1.50	Apr-09 Jan-11	Levensmiddelenbedrijf	3.25	Apr-08 Apr-10	Unilever	2.00	Mar-09 May-10

Table 5.2 Continued

CAO name	Wage increase %	Validity Period	CAO name	Wage increase %	Validity Period	CAO name	Wage increase %	Validity period
DSM Limburg B.V.	3.50	Jun-08 Jun-09	Metaalbewerking (metaal)	3.50	Feb-08 Dec-09	Universitair Medische Centra	2.10	Jan-08 Mar-11
Defensie-personeel	1.00	Mar-09 Mar-10	Metalektro	3.00	Nov-07 Feb-10	Vroom Dreesman	2.05	Feb-08 Feb-10
Delta Lloyd n.v.	3.00	Jun-07 Jun-09	Meubileringsbedrijven	1.15	Jul-08 Jul-10	VVT	3.25	Jan-08 Mar-10
Dierhouderij	2.50	Jul-07 Jan-10	Mode- en sportdetailhandel	2.00	Jan-08 Jul-10	Verzekeringsbedrijf (binnendienst)	3.00	Jun-07 Dec-09
Doe het zelf branche	2.34	Jan-07 Jul-09	Mode-; interieur-; tapijt- en	3.50	May-08 Jul-10	Vleessector	1.00	Apr-09 Apr-11
Drogisterijbranche	1.00	Apr-09 Oct-10	Motorvoertuigen	3.50	Feb-08 Jan-10	Voortgezet Onderwijs	3.00	Jul-08 Aug-10
Electrotechnische detailhandel	1.58	Jan-09 Jan-12	Nederlandse Spoorwegen (nieuw)	3.00	Apr-07 May-09	Vroom en Dreesmann Food	1.92	Feb-08 Feb-10
Energie	2.80	Jan-08 Jul-09	Oce Nederland B.V.	3.50	Apr-08 Jul-09	Welzijn en maatschappelijke dienst	2.50	May-08 Jan-12
Facilitaire Contactcenters	2.25	May-08 May-10	Ons Middelbaar Onderwijs	3.00	Jul-08 Aug-10	Wonen	2.75	Jan-08 Jan-10
Fortis-bank	3.50	Jan-09 Mar-10	Open Teelten	1.00	Jan-09 Jul-10	Woondiensten	1.50	Jan-09 Jan-11
Geestelijke Gezond-heidszorg	1.00	Apr-09 Mar-11	Openbaar Vervoer	3.50	Jan-08 Jan-09	Ziekenhuizen	1.00	Mar-09 Mar-11
Gehandicaptenzorg	1.50	Jan-09 Mar-11	Philips (nieuw)	-	Jan-09 Jan-10	Zoetwarenindustrie	3.00	Jul-07 Jul-09
Gemeente-ambtenaren	2.20	Jun-07 Jun-09	Politie-personeel	3.50	Jan-08 Jan-12	Zorgverzekeraars	3.00	Jun-07 Jun-09

Notes: The table shows the list of all reference collective labour agreements (reference CAOs) in our sample and reports the agreed percentage wage increase and the validity period stated in the agreement.

The table shows that about 25% of all collective labour agreements in place during the recession consist of company-specific CAOs (as they cover relatively few employees, the coverage of the workforce by company-specific CAOs is much lower). Also, it shows that the agreed percentage wage increases in these reference CAOs range from 0 to 3.5 percent. Furthermore, the table shows that collective agreements can enter into force in various months of the year, although the first half of the year is more common than the second half. When looking at agreement dates (not reported), these are even more spread throughout the year. Finally, the table shows that contracts can have various durations, where around two thirds of them have a duration of 1 year, 18 months, 2 years or 3 years.

Matched employers employees data

The data that we use to analyse employment and wage adjustments is a matched employers employees dataset obtained by combining different administrative sources available from Statistics Netherlands. For the employers part, we combine information available in the General Firm Registry on the size, the municipality and the sector in which the firms operate with balance sheet information coming from the corporate income tax data of the tax authority. For the employees part, we combine contract spells and monthly wage bills from the tax authority with workers characteristics available from the Employee Insurance Agency (UWV) and the General Civil Registry. The Matched Employers Employees Dataset is then obtained by merging employers and employees data via the firms and workers identifiers. Eventually, the final dataset is obtained by further merging the data on collective labour agreements via the SBI codes.[62]

Table 5.3 provides descriptive statistics of the most important employers' and employees' attributes in the Matched Employers Employees Data (MEED). Panel (a) reports summary statistics of some firm-level attributes, while panel (b) provides summary statistics for the main worker-level attributes. Data for employers refer to a total of 90.688 firms that have at least five employees and that have been successfully matched with their corresponding employees data.[63] These firms are also observed in the whole sample period which spans from 2007 to 2009. For all these firms we observe sector-level profitability indicators and aggregate financial statistics linked via the sector codes, while for a subset of 20% of the

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62. In case of company-specific CAOs, we combine this information on the basis of a statistical match based on the SBI code, the municipality where the company is headquartered, and the total number of employees.
63. The General Firm Registry unfortunately contains two different and non-overlapping identifiers, namely the encrypted fiscal number and the firm identifier, and it is possible to match the employers with the employee data only on the basis of the firm identifier. For this reason, our data for employers do not contain the population of firms in the Netherlands. The share of successfully matched companies is about 75% of the total number of firms with at least five employees.

Table 5.3 Summary statistics (MEED)

a) Employers

	2007	2008	2009
Sales (in '000s)	5,768.7	5,448.4	4,561.5
	(142,448.1)	(129,001.9)	(104,524.4)
Total Assets (in '000s)	5,497.3	6,292.5	5,890.9
	(325,994.5)	(322,626.7)	(312,336.7)
Net Profits (in '000s)	652.2	455.8	255.2
	(63,938.7)	(55,102.7)	(21,648.2)
Wages (in '000s)	805.9	794.2	730.4
	(12,943.6)	(14,320.3)	(12,962.7)
N° of Employees	38.7	39.8	38.8
	(572.8)	(634.5)	(582.0)
New workers (Inflows)	18.8	16.9	14.1
	(57.1)	(60.0)	(43.8)

b) Employees

	2007	2008	2009
Net Annual Wage	21,271.1	22,074.4	23,164.3
	(31,001.1)	(24,827.5)	(24,440.0)
N° hours worked	23.4	23.6	23.8
	(14.1)	(14.2)	(14.0)
Bonus	2,120.2	2,303.1	2,525.1
	(15,105.0)	(10,813.9)	(9,963.0)
Bonus share (%)	13.8	13.7	13.2
Bonus share (interquantile range)	6.3	7.1	7.4

Notes: The table shows descriptive statistics of the Matched Employers Employees Dataset. Descriptive statistics of the data for employers are reported in the left panel, while descriptive statistics of the data for employees are reported in the right panel. The table reports the mean and standard deviation (in parentheses) of each reported variable.

firms in the manufacturing sector we observe the actual balance sheet data merged via the employer identifier. For listed companies, that typically have a company-specific collective agreement, we obtain the corresponding balance sheet information from Bloomberg.

For all workers of the selected 90.688 firms, we observe employees' contract spells and monthly wages of employees. Thanks to the available employer identifiers, contract spells data allows us to observe, at any point in time, total employment levels of all companies included in the sample. The total number of workers employed in our sample of firms is more than 6 million. The monthly wage bills data allows us to observe the whole structure of workers' wages, and to obtain a decomposition of their remuneration into the following components: the ordinary wage, bonuses (e.g., performance pay), benefits (e.g., travel costs), compensation for overtime hours and taxes and contributions.[64] In this analysis, we focus on the components on which firms have some discretion. While firms have to take the contractual wage growth set in CAOs as given, they are able to adjust the ordinary wage of their workforce through other means (the so-called 'incidental pay' increases, see footnote 58). In particular, they can delay promotions and reduce or delay performance-related pay increases. In addition, firms can look for cheaper hires to replace workers who leave the firm, which will lower employers' ordinary wage costs through a composition effect. Furthermore, we will assess to what extent employers cut bonuses, benefits and overtime hours. Eventually, we set the data at the firm level and quarterly frequency.

5.4 EMPIRICAL STRATEGY

This section describes our empirical strategy to assess how the 2009 aggregate shock has affected firms' adjustments in the wage bill. Before presenting our identification strategy, we first show that the shock was unanticipated, leading to exogenous variation in wage growth due to staggering in the renewal dates of collective agreements.

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64. To be precise, bonuses include the holiday allowance (this is mandatory for all workers) and, if applicable, the 'thirteenth month', individual and collective performance pay, gratifications and profit sharing. Benefits include travel cost reimbursements, the rental value of a car provided by the employer, the rental value of a service-house provided by the employer, and bonuses in the form of real goods (e.g., holidays or event tickets). Taxes and contributions include health insurance, disability insurance and contributions a pension fund.

Unanticipated shock

The shock that we exploit is the 2009 recession. In terms of GDP, it was the strongest shock to the Dutch economy since World War II (up to 2020) which led to a substantial change in expectations.[65] To identify the timing of the shock in such a way that it was unanticipated, we use professional forecasts data on expectations about consumer prices, industrial production, investments and private consumption.[66] Table 5.4 shows that as of October 2008, professional forecasters were still expecting consumption, inflation and investment to increase, and their one-year ahead forecasts were stable. On the contrary, the recession scenario was already included in their April 2009 forecasts: industrial production and real investments were expected to fall by almost 10%, private consumption was also expected to drop and inflation expectations had been substantially cut. These forecasts are perfectly in line with those published by the Netherlands Bureau for Economic Policy Analysis (CPB) that form the basis in the bargaining process.[67] As the change in expectations took place between October 2008 and April 2009, we set the timing of the shock in October 2008 to make sure that the shock was unanticipated.

Exogenous variation in wage growth

The exogenous variation that we exploit is the variation in the start dates of collective agreements that were in place during the 2009 recession ('reference CAOs'). In fact, in our data the start date of a CAO always coincides with the end date of the previous CAO. This means that once a collective agreement is signed, in principle the start date of the next collective agreement is known. This in turn implies that the contract period of all CAOs was entirely pre-determined when the 2009 recession hit the Dutch economy, and this gives us plausible exogenous variation in the CAO start dates around the timing of the shock. In practice, there can be delays, which is why in the robustness section we test whether our results are robust to the use of agreement dates instead of the (retrospective) start dates.

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65. A change in expectations is a necessary condition to produce wage dispersion in a staggered wage setting, given that unions and employers associations bargain wages also on the basis of their expectations for the time horizon of the collective agreement. Gertler and Trigari (2009) show that when contracts are staggered, the bargained wage not only depends on relative bargaining power, but also on expected future economic conditions. Similarly, Hall (2005) shows that changes in the economic environment shift the boundaries of the bargaining set and changes the employers' incentives to recruit.

66. Consensus data consists of forecasts data at semi-annual frequency, specific for individual countries, obtained by surveying and combining opinions of professional forecasters such as advisors, institutional investors and rating agencies. For more details, see ttps://www.consensuseconomics.com

67. These can be found in the Macroeconomic Outlook (MEV) published in 2008, available at https://www.cpb.nl/en/publication/macro-economic-outlook-2008, and in the Central Economic Plan (CEP) published in March 2009 and available at https://www.cpb.nl/en/publication/central-economic-plan-cep-2009.

Table 5.4 Consensus forecasts

Forecast Horizon	Industrial Production				Forecast Horizon	Consumer Prices			
	Current year	1 year ahead	2 years ahead	mid-term (5 years)		Current year	1 year ahead	2 years ahead	mid-term (5 years)
October 2007	3.6	2.0	2.0	0.8	October 2007	1.7	2.2	2.3	2.0
April 2008	3.8	1.6	1.7	2.6	April 2008	2.4	2.5	2.0	1.9
October 2008	0.6	0.1	1.0	3.3	October 2008	2.6	2.0	1.7	2.3
April 2009	-9.9	1.0	2.8	5.0	April 2009	1.0	1.0	1.0	2.1
October 2009	-9.1	1.6	2.3	3.1	October 2009	0.9	1.0	1.3	1.6
April 2010	3.7	2.0	0.3	-0.5	April 2010	1.1	2.3	1.7	1.6

Forecast Horizon	Real Gross Investments				Forecast Horizon	Private Consumption			
	Current year	1 year ahead	2 years ahead	mid-term (5 years)		Current year	1 year ahead	2 years ahead	mid-term (5 years)
October 2007	4.4	3.4	2.9	1.4	October 2007	2.0	1.9	1.9	1.7
April 2008	3.6	2.0	2.7	2.6	April 2008	2.0	1.7	2.1	1.9
October 2008	5.7	0.0	2.6	3.1	October 2008	1.9	0.7	1.5	2.1
April 2009	-9.3	-2.7	2.0	3.9	April 2009	-0.7	-0.4	1.0	2.1
October 2009	-11.2	-2.8	1.8	2.5	October 2009	-2.7	-0.1	0.7	1.6
April 2010	-3.5	2.4	1.4	2.0	April 2010	-0.1	1.1	1.1	1.4

Notes: The table reports, for each date and forecast horizon, the annual expected growth rate (in percentages) of Industrial Production, Consumer Prices, Real Gross Investments and Private Consumption. Each column and row refer to a different forecast horizon and survey date, respectively.
Source: Consensus data.

To test whether the 2009 recession was sudden and strong enough to create a significant wage differential, we regress the contractual wage increases in CAOs in our CAO dataset (as reported in Table 5.2, but then annualised to correct for contract duration) on a constant and dummy variable equal to one if the start date of the CAO is before October 2008. The associated coefficient identifies the differential in the annualised wage increase of CAOs that were signed before and after the shock. Table 5.5 shows that the unconditional difference in the increase of the ordinary wage is 2.79% (column a). Controlling for differences in profitability across sectors only slightly attenuates the difference to 2.45% (column b). Considering the total annualised wage increase (that includes increases in structural pay allowances) does not change the result (column c and d).

Table 5.5 Wage differential test

	Annualised wage increase (%)		Total annualised wage increase (%)	
	(a)	(b)	(c)	(d)
CAO started before 2008Q4	2.78*** (0.156)	2.45*** (0.168)	2.85*** (0.160)	2.35*** (0.163)
Controls	X	C	X	C
N	101	101	101	101

Notes: The table reports the average (unweighted) differential in the wage increase in CAOs (both the ordinary wage and the total wage) bargained before and after the beginning of the crisis (difference in means). Columns (a) and (c) report the unconditional difference in wage growth, while column (b) and (d) show the same difference after having controlled for sector Return on Assets (ROA) and Return on Equity (ROE) and profit margin. The symbols *, ** and *** denote significance at the 10, 5 and 1 percent confidence level, respectively.
Source: Collective agreements data.

Identification strategy

This observed wage differential across sectors and firms resetting wages around the start of the recession is the key of our identification strategy. The channel is the following: firms covered by a CAO that started before the 2009 recession did not anticipate the shock and committed to pay a high wage increase in a period of a substantial economic contraction. Therefore, they may have incentives to adjust labour cost using the available adjustment margins (employment or pay components over which employers have some discretion).

As our empirical strategy, we employ a quasi-natural experiment approach, in which our treatment group consists of firms that were covered by CAOs that were renegotiated before the shock and hence could not adjust wages to the crisis scenario ($T_s = \boldsymbol{1}$[CAO started before 2008Q4). Our control group consists of firms that were covered by CAOs that were renegotiated after the shock. Our total sample period lasts from 2007Q1 to 2009Q4, yet,

we can evaluate our treatment dummy only for the period after the shock has taken place (i.e., 2008Q4 to 2009Q4). We use two alternative specifications to evaluate the effect of treatment. First, we apply a classical Difference-in-Difference estimator that estimates the treatment effect over the entire treatment period (2008Q4-2009Q4). Second, we apply a two-way-fixed-effects estimator that estimates the treatment effect per quarter. To be precise, we estimate the following equations:
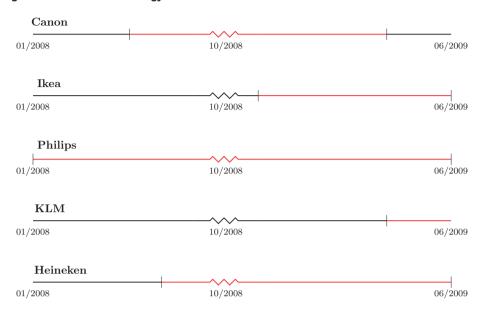
$$y_{j,s,t} = \lambda_t + c_j + \beta \, (T_s * after) + \theta \, newCAO_{s,t} + Z'_{s,t} \, \gamma + \varepsilon_{j,s,t} \tag{5.1}$$

$$y_{j,s,t} = \lambda_t + c_j + \sum_{k=2008Q4}^{2009Q4} \beta_k (T_s * \vartheta_k) + \theta \, newCAO_{s,t} + Z'_{s,t} \, \gamma + \varepsilon_{j,s,t} \tag{5.2}$$

where is the labour market outcome of interest (e.g., employment, wage) of firm j in sector s in quarter t, c_j and λ_t are firm and time fixed effects and is a set of control variables, respectively. The effect of interest is captured by the coefficient β, which identifies the Average Treatment effect on the Treated (ATT) as evaluated over the entire period, and the coefficient vector β_k, which identifies the ATT for all the five quarters k in the treatment period separately (2008Q4 to 2009Q4). The variable *after* is a dummy equal to 1 for all quarter in the entire treatment period (2008Q4-2009Q4), while is a dummy equal to 1 for all five quarters k in the treatment period separately. The variable $newCAO_{s,t}$ equals one in all periods in which the reference CAO is replaced by a new collective agreement.

Figure 5.2 shows a graphic example of the identification strategy we use. The variable $newCAO_{s,t}$ accounts for the fact that, as evident from the figure, some CAOs signed in 2008 were already renegotiated in 2009. Thus, it accounts for the fact that once a new collective agreement has been signed, the contractual wage growth is adjusted to the new economic reality. As a result, the treatment period consists of the crisis period, up to the time in which the reference CAO is still in place. Again, the treatment assignment is based on the start date of the reference CAO, which is already established when the previous CAO was agreed upon, and is thus independent of the uncertainty caused by the crisis.

Figure 5.2 Identification strategy

Notes: Graphical example of the identification strategy used in the empirical analysis. The figure shows, in a time line, the period in which the reference CAO was in place in five well-known companies. The red segment denotes the validity period of the Reference CAO and the *zigzag* line denotes the timing of the shock. The treatment (control) group consist of firms covered by a CAO that started before (after) October 2008. Here, Canon, Philips and Heineken are in the treatment group, Ikea and KLM are in the control group. The treatment period is the period between the (common) shock and the (specific) end date of the reference CAO: in this graphic example, Canon early-exits from treatment.

Parallel trend test

The assumption behind the results obtained from eq. (5.1) and (5.2) is that, without the nominal rigidities induced by contract staggering, all labour market outcomes in the treatment and control group firms would have had the same trend. In formulas, we are able to consistently identify the Average Treatment effect on the Treated (ATT) only if the following condition holds:

$$E(y_{j,s,a}(0) - y_{j,s,b}(0)|\, T_s = 1) = E(y_{j,s,a}(0) - y_{j,s,b}(0)|\, T_s = 0) \tag{5.3}$$

where *a* and *b* stand for a generic period after and before the treatment period, respectively. Since the term on the left-hand side is a counter-factual, it cannot be observed in the data, and the condition cannot be tested. This is usually overcome by testing the so-called parallel trend assumption, i.e. by looking at whether the outcomes in the two groups were following similar trends in the period before the treatment. In Figure 5.3, which we

construct linking all available firms to the CAO dataset described above, we show for the same two groups of firms the trends in employment and in the amount paid in bonuses and benefits in the seven quarters before the shock (2007Q1-2008Q3). The figure shows that the trends were perfectly parallel in the pre-treatment period, and lead us to conclude that the parallel trend assumption holds and that the ATTs estimated in the previous sections are correctly identified.
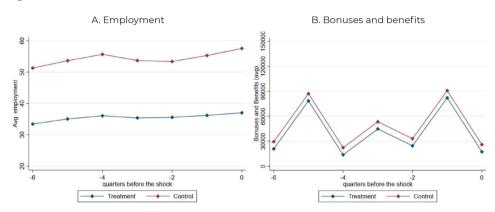
Figure 5.3 Parallel trends test

Notes: The figure shows the pre-treatment period trends in the employment level (a) and in the amount paid in bonuses and benefits (b) for firms that signed their CAO before (blue) and after (red) 2018 Q4.

5.5   MAIN RESULTS

Table 5.6 reports the result of the analysis of firms' adjustments in employment in response to the unanticipated shock. It reports the estimated ATTs and standard errors which are clustered at the three-digit sector level to account for possible latent correlation between firms in the same sector (Cameron and Miller, 2015). For each outcome variable, column (a) reports the results based on a classical two-groups-two-periods Difference in Differences approach (eq. 5.1) and column (b) shows the results by quarter based on eq. 5.2. The outcome variables are expressed in logs and include employment, the number of flexible workers and the number of vacancies.[68]

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68. Note that we use a proxy for the number of vacancies, given by the inflow of new workers in each firm. For a given firm, a worker is defined as a new inflow in period t if he/she is employed in the firm in period t but wasn't employed in the same firm in period $t - 1$. The number of inflows equals the number of vacancies if and only if all vacancies posted by the firms find a match in the labour market. This, despite being a strong assumption in general, is a relatively weaker assumption in crisis periods, when the number of vacancies is typically low and the number of unemployed workers is instead high. The same argument is used in Mortensen and Pissarides (1994) for their assumption of frictional labour market and the functional form given to the assumed matching function.

Results show that, on average, the positive wage differential paid by non-anticipating firms did not induce them to (differentially) adjust labour costs by changes in the workforce. The coefficient in column (a) indicates a point estimate of the ATT equal to 0.0%. The effects on flexible employment (0.4%) and vacancies (0.8%) are also not statistically different from zero. These are usually the drivers of changes in unemployment (Mortensen and Pissarides, 1994), as firms typically stop posting vacancies and do not renew flexible contracts when they plan to cut total employment levels.

Table 5.6 Adjustment of employment

	Employment		Flexible Employment		N° of Vacancies	
	(a)	(b)	(a)	(b)	(a)	(b)
started before× after shock (2008Q-2009Q4)	0.000 (0.010)		0.004 (0.017)		0.008 (0.029)	
started before×2008Q4		-0.002 (0.006)		-0.006 (0.008)		0.008 (0.021)
started before×2009Q1		0.022 (0.014)		0.046** (0.021)		0.010 (0.025)
started before×2009Q2		-0.002 (0.009)		0.013 (0.021)		-0.033 (0.030)
started before×2009Q3		-0.010 (0.009)		-0.013 (0.023)		-0.016 (0.031)
started before×2009Q4		-0.021 (0.013)		-0.029 (0.024)		0.011 (0.039)
N° of observations (*Nt*)	1,043.162	1,043.162	1,043.162	1,043.162	1,043.162	1,043.162
N° of firms (*N*)	90.688	90.688	90.688	90.688	90.688	90.688
N° of CAOs	101	101	101	101	101	101

Notes: The table reports the results of the firm-level analysis based on eq. (5.1) (columns a) and eq. (5.2) (columns b). All outcome variables are expressed in logarithm. The control variables are: dummy variables for small and big enterprises, the agreed percentage wage increase and the length of the CAO, the sector-level return-on-equity (ROE), return-on-assets (ROA) operating profit margin and value added. Clustered robust standard errors are in parentheses. The symbols *, ** and *** denote significance at the 10, 5 and 1 percent confidence level, respectively.

Table 5.7 reports the results of the analysis of firms' adjustment of four different pay components, obtained again from the estimation of a classical DiD regression (eq. 5.1) (column a) and of eq. (5.2) (column b). To be precise, it distinguishes the wage growth attributable to increases in the ordinary wage (a sum of the contractual wage growth mandated by CAOs and incidental pay increases), bonuses, benefits and overtime
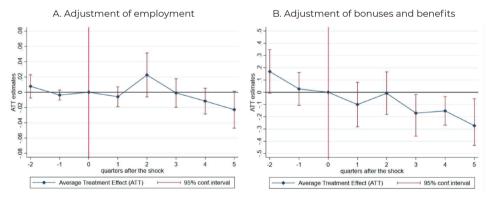
Table 5.7 Adjustment of different pay components

	Base wage		Bonuses		Benefits		Overtime	
	(a)	(b)	(a)	(b)	(a)	(b)	(a)	(b)
started before× after shock (2008Q-2009Q4)	0.004 (0.013)		-0.149*** (0.057)		-0.023 (0.018)		-0.008 (0.033)	
started before×2008Q4		-0.003 (0.007)		-0.131 (0.100)		-0.017 (0.016)		-0.036 (0.024)
started before×2009Q1		0.039** (0.018)		-0.058 (0.066)		0.010 (0.021)		0.008 (0.039)
started before×2009Q2		0.004 (0.012)		-0.170*** (0.078)		-0.015 (0.019)		-0.060 (0.039)
started before×2009Q3		-0.005 (0.013)		-0.161*** (0.060)		-0.064*** (0.017)		-0.072 (0.047)
started before×2009Q4		-0.031* (0.016)		-0.266** (0.129)		-0.043** (0.021)		0.006 (0.045)
N° of observations (Nt)	1.042.830	1.042.830	681.663	681.663	649.466	649.466	260.093	260.093
N° of firms (N)	90.634	90.634	56.805	56.805	54.122	54.122	21.674	21.674
N° of CAOs	101	101	101	101	101	101	101	101

Notes: The table reports the results of the firm-level analysis based on eq. (5.1) (columns a) and eq. (5.2) (columns b). The control variables are: dummy variables for small and big enterprises, the agreed percentage wage increase and the length of the CAO, the sector-level return-on-equity (ROE), return-on-assets (ROA) operating profit margin and value added. Clustered robust standard errors are in brackets. The symbols *, ** and *** denote significance at the 10, 5 and 1 percent confidence level, respectively.

hours.[69] Starting with the base wage, the table shows that non-anticipating firms that were compelled by CAOs to grant their workers a substantial wage increase, did not pay their workers a significantly higher ordinary wage over the entire period we study (2008Q4-2009Q4). The period-specific ATTs in column (b) show that in the first quarter of 2009, firms did pay higher ordinary wages, yet, in the quarters thereafter there is no significant differential. Apparently, these firms have been able to offset the contractual wage increase by curbing incidental pay increases (e.g., slowing of promotions, delays of performance-related pay increases and replacing workers that leave with cheaper hires). In Section 5.7 we repeat this regression at the worker-level, whereby we can filter out composition effects (e.g., cheaper hires).

Furthermore, firms have also been able to adjust to contract staggering by cutting bonuses. Over the entire period, bonuses in non-anticipated firms were 14.9% lower than in firms that had been able to negotiate lower wages. In addition, firms were able to cut benefits in the last half of 2009; yet, this effect is not significant when evaluated over the entire period (2008Q4-2009Q4). Last, we find no difference in the compensation for overtime hours. Figure 5.4 provides a graphical representation of the effect on employment and bonuses plus benefits by plotting the ATTs around the timing of the shock.

Figure 5.4 Comparison of adjustment margins

Notes: The figure shows the estimated Average Treatment Effects on the Treated (ATTs) of contract staggering on total employment (a) and on bonuses and benefits (b).

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69. Please note that not all firms in the Netherlands use bonuses, benefits and, especially, overtime hours. For this reason, the number of observations differs from that in Table 5.6, as the log transformation gets rid of all firms that do not pay these compensations. While firms are compelled to pay a holiday allowances, which is counted as a bonus, if the holiday allowance is the sole bonus components we cannot use these observations in a panel set-up.

Summing up, in sectors where wage resets were agreed upon before the 2009 recession started, firms committed to pay, on average, 2.5% higher contractual wage growth. Unlike what is predicted in macroeconomic models, this rigidity did not result in employment losses. As an explanation, our results demonstrate that these firms were able to offset this higher contractual wage growth with cuts in bonuses and incidental pay. Unfortunately, from this analysis we cannot derive exactly what incidental pay elements were cut as we only observe the aggregate base wage. In Section 5.7 we perform the analysis at the worker-level, allowing us to distinguish between cuts to the wage of the existing workforce (e.g., slowing of promotions and delays of performance-related pay increases) and cuts that arise from changes in the composition of the workforce (e.g., cheaper hires).

5.6   HETEROGENEITY IN CONTRACT DURATION

In this section, we perform a heterogeneity analysis to shed more light on the role of contract staggering as a nominal rigidity. The definition of contract staggering not only requires that contract periods of the various CAOs are not completely in sync (that we exploit in our identification strategy) but also requires that the agreed wage increase remains valid for a certain period (wage stickiness).

Here, we look at the size of the ATTs as a function of contract duration to examine whether firms' labour adjustment cost depend on the degree of the wage rigidity. In sectors where collective agreements last longer, firms commit to a certain wage increase for a longer period, and they therefore have higher incentives to cut labour costs through other means. On the contrary, firms covered by short-lasting CAOs can wait for the next bargaining round to reduce their wage offer in the CAO. As already shown in Table 5.2, the heterogeneity in contract duration is high in the Netherlands, and varies from six months to about three years.

Table 5.8 reports the estimates of the specification in eq. (5.2) on the subsamples of firms covered by CAOs lasting less than 18 months, between 18 and 30 months, and more than 30 months, respectively. Figure 5.5 presents a graphical representation of the results. We find a negative and significant effect on employment throughout the treatment period only for firms covered by CAOs lasting more than 30 months. Furthermore, for the CAOs with shorter durations, we only find a negative and significant effect on employment in the last quarter.

Table 5.8 Adjustment of employment and flexible pay by contract duration

	Employment			Bonuses and benefits		
	(≤ 18m)	(18-30m)	(≥ 30m)	(≤ 18m)	(18-30m)	(≥ 30m)
started before×2008Q4	0.027*	-0.004	-0.017***	0.012	-0.097	-0.098**
	(0.016)	(0.004)	(0.006)	(0.037)	(0.073)	(0.033)
started before×2009Q1	0.087*	0.015	-0.029*	-0.062	-0.008	-0.167***
	(0.045)	(0.015)	(0.016)	(0.094)	(0.041)	(0.025)
started before×2009Q2	0.037**	-0.005	-0.037***	0.119	-0.173	-0.222***
	(0.017)	(0.013)	(0.011)	(0.207)	(0.105)	(0.027)
started before×2009Q3	-0.017	-0.018	-0.041***	0.068***	-0.129*	0.192***
	(0.012)	(0.012)	(0.012)	(0.012)	(0.065)	(0.027)
started before×2009Q4	-0.055**	-0.028*	-0.070***	0.089	-0.147	-0.230***
	(0.025)	(0.016)	(0.017)	(0.144)	(0.094)	(0.078)
N° of observations (*Nt*)	231.307	670.112	141.743	194.191	560.797	106.690
N° of firms (*N*)	22.239	53.781	14.668	16.051	42.999	9.064
N° of CAOs	31	51	19	31	51	19

Notes: The table reports the results of the heterogeneity analysis based on eq. (5.2) for various durations of CAOs separately. All outcome variables are expressed in logarithm. For each specification, the first column refers to the subsample with CAOs lasting at most 18 months, the second column to the subsample with CAOs that last more than 18 and less than 30 months. The third column refers to the subsample with CAOs lasting at least 30 months. The firm-level control variables are the same used in the main specification. Clustered robust standard errors are in parentheses. The symbols *, ** and *** denote significance at the 10, 5 and 1 percent confidence level, respectively.

The results of this heterogeneity analysis confirm the presence of relevant rigidities in staggered wage setting. The non-synchronicity of the contract periods of collective agreements induces a wage differential across sectors negotiating wage resets around the time of an aggregate shock. We showed that the wage increase paid by firms that did not anticipate the 2009 recession was about 2.5% higher than those of firms that reset their wages during the recession. However, the most relevant rigidity is due to the presence of very long contract durations, which imply a commitment to the agreed-upon wage increases. In fact, wages are sticky for the entire contract period of the CAO. We showed firms that did not anticipate the crisis and that were covered by CAOs with the longest durations have also the strongest labour cost adjustments.

At the same time, this effect is not large enough to affect the overall result on employment (Table 5.6). Our main result that contract staggering does not have, on average, effects on employment contrasts with the results of macroeconomic models such as that of Gertler and Trigari (2009). In their model calibration for the U.S. economy, these authors pick a value of the wage reset frequency parameter that implies that firms, on average, reset wages every three quarters. We provide evidence of significant causal effects of an

economic shock on employment only in sectors in which wage resets occur less than once every 10 quarters. Our results suggest that the absence of an overall employment effect is contingent on the possibility to adjust variable pay components, in particular incidental pay and bonuses (see Table 5.7).
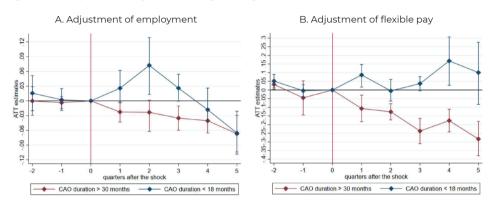
Figure 5.5 Comparison of adjustment margins, long and short contract duration

Notes: The figure shows the estimated Average Treatment Effects on the Treated (ATTs) of contract staggering on total employment (a) and on bonuses and benefits (b).

5.7 ROBUSTNESS ANALYSIS

Agreement versus start dates

Our identification strategy relies on the fact that CAOs start dates, being pre-determined from the moment the previous contract was signed, are independent of the uncertainty caused by the crisis and thus exogenous. However, start dates typically differ from agreement dates that are instead endogenous and, during a crisis period, possibly dependent on the uncertainty surrounding it. In the Netherlands, wage resets can be agreed upon by social partners months after the start date of the collective agreement, and the resulting wage increase can thus apply retroactively (Hijzen *et al.*, 2019). In Figure 5.1 we already showed that the agreement delay has increased during the 2009 recession, as compared to the previous and following period, and this somehow supports the argument of Danziger and Neumann (2005) on the endogeneity of agreement dates.

This issue can potentially affect the causal interpretation of our estimated effects to the extent that firms whose CAO was supposed to start before the crisis, actually agreed the wage increase after the crisis started because of a delay. Once social partners realize that

the economy is hit by a recession, they could delay the negotiation process to gather more information or, given the new scenario, even change their wage offer and demand. In this case, considering start dates instead of agreement dates can cause the treatment group to include firms that, because of a delay, managed to anticipate the crisis.

Table 5.9 shows that out of the 60 sectors whose CAO started before 2008Q4, 10 of them reached an agreement after the crisis because of a delay.[70] This means that social partners might have agreed the wage increase in these sectors under a very different information set. To exclude this possibility, in this section we switch from an identification based on start dates to an identification based on both starting and agreement dates. The crucial difference between the two is that while start dates are pre-determined and thus exogenous, they do not capture firms not anticipating the recession in case of delays in the negotiation process. On the other hand, agreement dates are endogenous but they better reflect the information set under which social parties signed the agreement. Here, we drop the firms covered by the 10 CAOs that started before the crisis but were agreed upon during the crisis and we re-estimate eq. (5.1). In this way, we make sure that the treatment group consists of truly non-anticipating firms and, at the same time, we rule out possible issues of selection into treatment.

Table 5.9 Agreement dates versus start dates

N° of collective agreements		Agreed	
		after 2008Q4	before 2008Q4
Started	after 2008Q4	41	0
	before 2008Q4	10	50

Notes: The table reports the number of collective labour agreements started and agreed before or after 2008Q4.

Note that, thanks to the availability of information on both the starting and agreement dates, the use of the endogenous agreement dates does not bias our result: by excluding the firms that delayed their wage resets, the new treatment group only consists of the firms whose CAO started *and* was agreed upon prior to October 2008. In such a way, we make sure that the treatment group only consists of non-anticipating firms. Table 5.10 shows that re-estimating eq. (5.1) and (5.2) on the basis of the new agreement dates does

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70. The average annual wage increase established in this group is equal to 2.35%, and is mostly in line with those started and agreed before the crisis. Out of these 10 CAOs, only two agreed on wage growth close to zero.

Table 5.10 Identification based on agreement dates

	Employment		Base wages		Bonuses		Benefits	
	(a)	(b)	(a)	(b)	(a)	(b)	(a)	(b)
agreed before× after shock (2008Q-2009Q4)	0.004 (0.010)		0.008 (0.013)		-0.142** (0.060)		-0.023 (0.019)	
agreed before×2008Q4		0.000 (0.006)		0.001 (0.007)		-0.114 (0.107)		-0.018 (0.016)
agreed before×2009Q1		0.024* (0.014)		0.0043** (0.018)		-0.055 (0.073)		0.010 (0.022)
agreed before×2009Q2		0.002 (0.010)		0.006 (0.013)		-0.177** (0.087)		-0.016 (0.020)
agreed before×2009Q3		-0.011 (0.010)		-0.006 (0.013)		-0.184*** (0.066)		-0.062*** (0.018)
agreed before×2009Q4		-0.018 (0.013)		-0.028* (0.017)		-0.204*** (0.129)		-0.045** (0.022)
N° of observations (Nt)	976.294	976.294	976.016	976.016	642.563	642.563	617.178	617.178
N° of firms (N)	85.050	85.050	85.002	85.002	51.718	51.718	51.159	51.159
N° of CAOs	91	91	91	91	91	91	91	91

Notes: The table reports the results of the firm-level analysis based on eq. (5.1) (columns a) and eq. (5.2) (columns b). All outcome variables are expressed in logarithm. The control variables are: dummy variables for small and big enterprises, the agreed percentage wage increase and the length of the CAO, the sector-level return-on-equity (ROE), return-on-assets (ROA) operating profit margin and value added. Clustered robust standard errors are in brackets. The symbols *, ** and *** denote significance at the 10, 5 and 1 percent confidence level, respectively.

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not modify our main results, i.e., there is no overall effect on employment, base wages are significantly higher in 2009Q1 higher but not when evaluated over the entire period. Furthermore, bonuses were cut with a similar magnitude.

Worker-level evidence

The main result of the firm-level analysis is that firms that agreed on high wage growth before the recession did not cut employment levels more than those firms that were able to witness the shock and were able to agree on lower contractual wage growth. Instead, we show that non-anticipating firms have been able to adjust their wage bill by cutting bonuses and benefits and that they seem to have been able to curb the so-called incidental pay component, that we do not observe directly. In this section, we switch to the most granular level of our MEED data linking firms to employment spells, thus a worker-level analysis to test whether workers employed in treated firms eventually enjoyed a pay increase due to contract staggering. This allows us to distinguish between cuts in incidental pay that were enacted at the worker-level (e.g., slowing of promotions and cuts of performance-related wage increases) and cuts that are due to a change in the composition of the workforce (e.g., cheap hires). To this end, we use the employees part of the MEED and look at the wage bills of all workers that were continuously employed throughout the whole sample period (2007-2009) by the 90.688 firms in our sample. Monthly wage bills have been collapsed at quarterly frequency too, so that an analogous specification to that of the firm analysis in Section 5.5 can be obtained at the workers level. The estimated Difference-in-Differences equation is as follows:

$$y_{i,j,s,t} = \lambda_t + c_i + \beta\,(T_s * after) + \theta\,newCAO_{s,t} + Z'_{s,t}\gamma + X'_{i,j,s,t}\delta + u_{i,j,s,t} \qquad (5.4)$$

where $y_{i,j,s,t}$ is the wage of worker i in firm j of sector s at time t, and $X_{i,j,s,t}$ are workers characteristics and *after* is a dummy equal to 1 for all quarter in the entire treatment period (2008Q4-2009Q4). The outcomes we consider are the workers' ordinary and total net hourly wage. The former is equal to the ordinary wage divided by the number of contractual hours, while the latter includes all non-ordinary wage components such as benefits, bonuses and income from overtime hours.[71]

Table 5.11 reports the results of the estimates of eq. (5.4), for both the cases in which the treatment group indicator T_s is determined using the start dates and the agreement dates of CAOs. Results based on the former show that workers employed in non-anticipating firms enjoyed, on average, a 1.4% higher ordinary wage relatively to their control group

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71. We divide the total net wage in a quarter with the total number of hours worked. In this way, an increase in wage can only be due to an increase in wage, but not to an increase in hours worked.

counterparts.[72] This positive differential contrasts with the finding in Table 5.7 that there was no significant differential in the ordinary wage of workers in anticipating and non-anticipating firms. This suggests that also composition effects have played a role, i.e., firms have replaced workers that left the firm with cheaper hires.

Table 5.11 Workers' individual wages

	Dependent variable:			
	Ordinary hourly wage		**Total hourly wage**	
	(a)	**(b)**	**(c)**	**(d)**
started before × after shock (2008Q-2009Q4)	0.014*** (0.003)		0.0032 (0.0041)	
agreed before × after shock (2008Q-2009Q4)		0.008*** (0.003)		-0.007*** (0.004)
new CAO	0.022** (0.009)	0.022** (0.009)	0.0150* (0.0082)	0.0150* (0.0082)
age	0.057*** (0.001)	0.057*** (0.001)	0.0566*** (0.0010)	0.0566*** (0.0010)
age squared	-0.001 ** (0.000)	-0.001*** (0.000)	-0.0005*** (0.0000)	-0.0005*** (0.0000)
experience	0.003*** (0.000)	0.003*** (0.000)	0.0026*** (0.0003)	0.0026*** (0.0003)
maternity leave	-0.0000 (0.003)	-0.0000 (0.003)	-0.0127*** (0.0035)	0.0127*** (0.0035)
sickness leave	-0.035*** (0.004)	-0.035*** (0.004)	-0.0393*** (0.0037)	0.0393*** (0.0037)
permanent contract	0.107*** (0.008)	0.108*** (0.008)	0.1111*** (0.008)	0.1111*** (0.008)
N° of observations (*Nt*)	24.690.219	24.690.219	24.690.219	24.690.219
N° of workers (*N*)	1.541.430	1.541.430	1.541.430	1.541.430
N° of firms	90.688	90.688	90.688	90.688
N° of CAOs	101	101	101	101

Notes: The table reports the results of the worker-level analysis based on eq. (5.4). All outcome variables are expressed in logarithm. The control variables are the same used in the main specification. Cluster robust standard errors are in parentheses. The symbols *, ** and *** denote significance at the 10, 5 and 1 percent confidence level, respectively.

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72. This is lower than the 2.5% average contractual wage increase in CAOs, which is consistent with firms slowing promotions and/or curbing performance-related increases in the ordinary wage. Yet, note that the 2.5% differential in contractual wage increases concerns an ordinary arithmetic mean, i.e., it has not been weighted by the numbers of employees covered by the respective CAO. This can lead to differences with the estimated ordinary wage in the worker-level analysis.

Furthermore, Table 5.11 shows that the differential in the total wage paid to workers (that includes bonuses and benefits) is not statistically significant. Hence, the bonuses and benefits components have further allowed firms to adjust their wage bill. Results based on agreement dates show that after the cut in the discretionary part of the wage bill, the total wage of workers in the treatment group is slightly lower (-0.7%) than that of workers in the control group. We conclude that employees of non-anticipating firms did not eventually earn more as a result of contract staggering and the time of the signature date of their CAO. The fact that individual employers have room for discretion on part of the increase of ordinary wages (e.g. promotions and performance-related pay increases) and on the bonus and benefit components gives firms flexibility to adjust labour cost without changing the workforce.

Other robustness checks

We performed two additional robustness checks. First, we re-estimate eq. (5.1) excluding the banking, insurance and financial sector. In fact, firms in these sectors firms typically pay higher bonuses as a share of the corresponding total wages. Also, a possible drop in the bonuses in these sectors may be driven by a large negative shock in financial markets. Second, as the sharp economic contraction was still ongoing by October 2008, we check whether results are robust to a later definition of the shock by shifting the treatment period onwards from the 2008Q4-2009Q4 period to the 2009Q1-2010Q1 period. The results, not reported but available upon request, show that all estimates are robust to these changes.

5.8 CONCLUSION

This chapter has investigated the consequences of the wage rigidities induced by contract staggering in collective labour agreements in the Netherlands. Contract staggering is a labour market rigidity that arises from the combination of wage stickiness and unsynchronized collective labour agreements. The analysis confirms that the random non-synchronicity of collective agreements can create a considerable wage differential across sectors and firms that reset wages in nearby periods, especially in times characterized by widespread uncertainty and/or sudden changes in expectations, such as the 2009 recession. The descriptive analysis shows that the average wage growth stipulated in CAOs that started before the crisis was 2.5% higher than the wage growth of CAOs starting during the crisis.

In the empirical analysis, our central question was whether and how labour market rigidities induce firms to adjust labour cost at the onset of the 2009 recession. To answer

this question, we exploit the exogenous and staggered start dates of collective agreements to set up a quasi-natural experiment that allows us to identify the causal effect of labour market rigidities. The main finding of this chapter is that, unlike what is predicted in macroeconomic models, the pervasive contract staggering in the Dutch labour market did not result in employment losses, even in the aftermath of an unprecedented shock such as the Great Recession. In contrast, we provide evidence that firms were able to offset this higher contractual wage increases with cuts in bonuses and benefits and, apparently, by curbing incidental wage growth. We find that employment cuts have been significant only among firms covered by very rigid collective agreements lasting more than thirty months, although this effect is not large enough to affect the overall result. Our results are robust to several sensitivity tests. First and foremost, we repeat our analysis using agreement dates instead of official start dates of CAOs, to control for the possibility that our results are biased because some CAOs have in fact been bargained under a different information set. This concerns 10 of 101 CAOs. Wage growth in these sectors was in fact very much in line with those CAOs that were bargained just before the onset of the recession. Furthermore, when dropping those CAOs from our analysis, our results do not change.

Overall, our results suggest that despite widespread rigidity, the Dutch wage setting was flexible enough not to cause employment reductions in the year after the 2008 shock. Hence, this study underscores the importance of taking into account the wider institutional setting in which wage setting takes place. We conclude that the fact that firms, despite being covered by a collective agreement that prescribes rigid wages, still have discretion over part of the base wage as well as over bonuses and benefits allows them to their wage bill without employment losses. This is consistent with the evidence from the Wage Dynamic Network initiative (Babecký *et al.*, 2012; Babecký *et al.*, 2019) showing that slowing promotions, using cheaper hires and cutting bonuses were a common wage adjustment used by European firms in the aftermath of the Great Recession. The fact that this effect was so large, could be due to the increased flexibility within Dutch CAOs and the fact that bargaining takes place in a corporatist setting in which unions have come to adhere to a 'jobs before wages' strategy.

At the same time, of course, this does not mean that more flexible wage setting could not further alleviate the risk of employment losses in response to a shock. Although the 2009 shock was the largest economic shock that hit the Netherlands since World War II, it was small compared to the deep economic recession that ensued in the wake of the COVID-19 outbreak. In the context of an even larger shock such as the current one, the scope for cuts in incidental pay and bonuses and benefits may be too limited to curb employment losses and a further adjustment of base wages could be preferred in order to preserve jobs.

Unfortunately, our data does not allow us to identify the precise mechanisms that firms were able to use to adjust incidental pay increases (e.g. the slowing of promotions) or bonuses (e.g. cuts in performance pay or profit-sharing schemes), and to what elements of CAOs these can be linked to.[73] Further research is needed to understand which details are more important in enhancing the flexibility of collective agreements. Also, due to data unavailability this study is silent on other important margins of adjustment that may have played a role. One is firms' pricing, as previous evidence has highlighted that firms use price-setting as an important adjustment margin to a labour market shock, especially when competition is lower (Bertola *et al.*, 2012). Another interesting possibility is the effect on workers' transition to unemployment (in line with the findings of Diez-Catalan and Villanueva, 2014) and, especially for the Netherlands, workers' transition to self-employment. We leave these questions for future research.

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73. Coding the content of collective agreements is notoriously difficult, as such agreements are often extensive and look very differently. For a sub-sample of our CAO dataset, the Ministry of Social Affairs has gathered information on CAOs that make reference to profit sharing, result agreements and performance pay. For this subsample of CAOs, we constructed dummies for these three various pay components. However, when adding these dummies to our analyses of the adjustment of bonuses, they did not yield significant results.

Chapter 6

FRONTAL ASSAULT VERSUS INCREMENTAL CHANGE:

A COMPARISON OF COLLECTIVE BARGAINING IN PORTUGAL AND THE NETHERLANDS

6.1   INTRODUCTION[74]

Collective bargaining is an important feature of labour markets, especially in Continental Europe where collective agreements (CAs) typically cover more than three quarters of the workforce. Collective bargaining provides voice to workers, and in doing so, has the potential to enhance working conditions, increase productivity, reduce inequality and help minimise industrial conflict (see, e.g., OECD, 2018). At the same time, collective bargaining has sometimes been associated with wage drift or wage rigidity, with adverse consequences for employment. Downward nominal wage rigidity in particular can be an issue for countries without their own monetary policy that are confronted with an adverse demand shock in a low inflation environment. Collective bargaining also has come under pressure as a result of declining coverage and trade union density (see, e.g., OECD, 2017) – partly driven by the expansion of the service sector, individualization, immigration, deregulation, globalisation, and, more recently, the emergence of new forms of work, such as through digital platforms (e.g. Uber).

Traditionally, the policy debate on collective bargaining has tended to concentrate on the *level of bargaining*. An influential view originally proposed by Calmfors and Driffill (1988) held that systems with predominantly sector-level bargaining lead to excessive wage claims relative to productivity. As a result, these systems were associated with weaker labour market performance than either centralised systems, which provide flexibility at the aggregate level by inducing unions and employer associations to internalise the macro-economic effects of wage claims on economy-wide employment, or decentralised systems, which provide wage flexibility at the firm level. However, it has also long been acknowledged that experiences diverged noticeably even among countries where sector-level bargaining is widespread (Traxler *et al.*, 2001; Soskice, 1990). Indeed, the impact of bargaining systems does not only depend on the degree of collective bargaining coverage and the level of negotiation, but also on the specific rules and institutional practices that characterise each system with respect to, for example, the degree of flexibility for firms and the degree of co-ordination across sectors or firms (OECD, 2017).

Despite research showing that there are many other factors that affect the role of collective bargaining systems for labour market performance, reforms in response to the global financial crisis remained by and large focused on the level of bargaining, and more specifically, ways to increase the prominence of firm-level bargaining. Indeed, the Great

Recession and the ensuing surge in unemployment in Southern European countries placed sector-level bargaining – the predominant form of bargaining in these countries – under renewed scrutiny. As in the early work by Calmfors and Driffil (1988), this was seen as a source of wage rigidity, impeding both macro-flexibility (i.e. the ability of the economy to adjust to macroeconomic shocks) and micro-flexibility (the ability for workers and firms to form the most productive match). Portugal and Greece implemented substantial reforms as part of an adjustment program by the 'troika' that sought to weaken sector-level collective bargaining. Most notably, administrative extension of sectoral agreements – that extend their applicability beyond the membership of the social partners in the sector – was reduced or even suspended and in the case of Greece the principle of favourability abandoned.[75]

Reforms to collective bargaining are by nature controversial, as they can have important implications for the balance of power (and hence bargaining outcomes) both *between* workers and firms, and *within* these two groups. Although in Germany, decentralisation of bargaining has been credited with positive employment effects, there have also been concerns about declining bargaining coverage (Addison *et al.*, 2017), increasing wage inequality and labour market duality (Dustmann *et al.*, 2009; Hassel, 2014). Yet, the reforms to collective bargaining systems in crisis countries proved particularly contentious. The fact that such reforms were not home-grown but implemented in the context of an adjustment program most likely added to their controversy. According to some, these reforms even constituted a 'frontal assault' or 'European attack' on collective bargaining (Marginson, 2015; Van Gyes and Schulten, 2015). The sense of attack may partially have resulted from the focus of the reforms on weakening the system of sector-level bargaining at the expense of firm-level or individual bargaining with relatively little attention being paid to ways to improve the functioning of sector-level bargaining systems.

It should be recognised, however, that prior to the crisis it was widely acknowledged that bargaining systems in Southern European countries were in need for change as they were very rigid, highly dependent on state support and characterised by adversarial relations (Fajertag and Pochet, 2000; Natali and Pochet, 2009; Molina, 2014). The rigidity of the collective bargaining system in these countries was exposed during the financial and sovereign debt crisis, prompting the introduction of abrupt and often politically controversial reforms. On the other hand, more mature corporatist economies also underwent reforms, but the reform process took place gradually over the course of several decades following concerted social dialogue based on a shared understanding of the need

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75. See Blanchard *et al.* (2014) for an overview of labour market reforms in IMF programs and Van Ours *et al.* (2016) for a description of the Greek case.

to adapt to new economic realities. Arguably, these incremental reforms also focused more on enhancing the functioning of the existing system of collective bargaining rather than seeking a more systemic change that challenges the core of a country's collective bargaining tradition.

This chapter aims to contribute to the recent policy debate on collective bargaining reforms by comparing the recent reforms to the Portuguese system with the gradual changes since the early 1980s that took place in the Netherlands, while preserving the predominance of sectoral bargaining. A comparison of the Dutch and Portuguese systems is insightful for three main reasons. First of all, the collective bargaining systems of the two countries share many important features: i) sector-level bargaining is dominant; ii) collective bargaining is very pervasive, covering around three quarters of the workforce; iii) trade union density is rather low, representing 20% or less of the workforce; iv) administrative extensions of collective agreements are fairly important. Consequently, one could argue that both systems are relatively centralised.

Secondly, their industrial relations systems differ markedly in their maturity and the way operational practices have evolved over time, with potentially important implications for the quality of industrial relations and the effectiveness of collective bargaining. In the Netherlands, after the Wassenaar agreement of 1982, which ended a wage spiral and an era of tense and often conflictual labour relations, the system has gradually adapted to emerging challenges such as monetary unification, globalization, and population ageing. It has done so in incremental steps, without changing the fundamental features of the system.[76] Portugal, on the other hand, emerged from a long-lasting conservative dictatorship a little more than 40 years ago. While the high-inflation environment of the 1970s and 1980s helped to achieve real wage adjustments when needed, the collective bargaining system had not been put to a proper test in a low-inflation environment until the global crisis of 2008. In the context of an adjustment program, Portugal implemented abrupt and often controversial labour market reforms, including in collective bargaining. Therefore, comparing the Portuguese and Dutch experiences allows us to speculate on the role of reforms induced by gradual, concerted social dialogue instead of crisis and those by external pressure.

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76. According to Visser and Hemerijck (1998), what characterizes the Dutch corporatist tradition is precisely this gradual experimentation with institutional rules and practices, which they dub 'policy learning'. In their view, this policy approach was an important factor behind the strong employment performance at the turn of millennium. In another article, they go even further by saying that the Dutch experience 'shows that a rescue of the European social model is possible, even under the conditions of a more restrictive macro-economic policy environment, and increased pressure on firms to adapt to external market pressures' (Hemerijck *et al.*, 2000).

Thirdly, as a result of the incremental changes in the Netherlands and the relative rigidity of the Portuguese system the two systems developed differently, with respect to the flexibility of the system to respond to changing economic realities with important benefits for the quality of labour relations. This chapter focuses on a number of specific elements of the Dutch system that are likely to have contributed its flexibility and its effectiveness. These are: i) the extent and nature of decentralisation, ii) the extent to which administrative extensions of sectoral agreements take account of social and economic consideration, iii) the application of agreements after expiration or retrospectively (ultra- and retro-activity), and iv) the degree of coordination and cooperation between social partners.

Our analysis suggests that the recent reforms in Portugal have brought the institutional set-up more in line with that in Netherlands, but questions remain about the implications of these changes for actual bargaining practices and the ability of the system to contribute to better outcomes for workers and firms. First, the scope for flexibility at the worker and firm levels within sector-level agreements has increased over time in both countries. While the Dutch system has held on to sector-level bargaining, it has gradually increased the scope for flexibility at the firm or worker level within sector agreements. This is in contrast with the case of Portugal, where decentralisation has only been introduced recently, and, so far, does not appear to have significantly increased the scope for flexibility at the firm level. Second, concerns about the possible adverse employment effects of coverage extensions have led to the introduction of representativeness criteria in both countries. However, as a result of the way these criteria have been introduced and the presence of a clear and transparent framework for exemptions from extensions a strict application of representativeness criteria remains very contentious in Portugal. Third, since the reforms in the aftermath of the crisis the application of retro-activity of collective agreements and their extensions in Portugal is now similar as in the Netherlands. While Portugal has taken additional measures to restrict the ultra-activity of collective agreements and extensions, no such restrictions exist in the Netherlands. However, in the Netherlands ultra-activities is limited to the signatory parties while it applies to both signatory and non-signatory parties in Portugal. Fourth, effective coordination between bargaining units and high-quality labour relations are crucial for high-performance collective bargaining systems. While enhancing the coordination of collective bargaining outcomes and the quality of labour relations is beyond the direct control of policy makers, the Dutch experience points to a number of helpful strategies based on a combination of 'carrots' (e.g. tax concessions, even greater involvement in training activities) and 'sticks' (including the possibility of restricting the extensions of collective agreements). It is unclear what the consequences are of the reforms to collective bargaining in the aftermath of the global financial crisis in Portugal on the quality of industrial relations and the ability of the social partners to contribute good macro-economic outcomes.

The remainder of this chapter is structured as follows, focusing on different components of sectoral collective bargaining. Section 6.2 discusses decentralisation. Section 6.3 examines the role of extensions. Section 6.4 discusses the role of ultra and retro-activity. Section 6.5 zooms in on the role of coordination and trust between the social partners. Finally, Section 6.6 concludes.

6.2 DECENTRALISATION

In many countries with national or sectoral bargaining, decentralisation has been 'the name of the game in industrial relations' (Visser, 2013b). Decentralisation refers to the creation of more space for negotiations over working conditions at the level of the firm, establishment or workplace, in contrast to the sectoral or national levels.[77] Decentralisation is often seen as a way to improve labour market performance through enhanced adaptability and resilience of firms. For instance, Dustmann *et al.* (2014) have argued that decentralisation played a key role in the strong performance of the German labour market in the aftermath of the crisis.

A first type of decentralisation refers to the possibility to deviate from national standards. In the Netherlands, self-regulation by social partners is typically seen as a way to limit the need for detailed regulations by the state (Visser, 2018). Over the last decades several possibilities were introduced for deviating from national labour law (Verhulp, 2003; Heerma van Voss, 2005). Under 'semi-binding law', firms are allowed to deviate from the respective clause when they agree so with their employee in writing. 'Three-quarters law' is a bit stricter, as deviations are only possible when employers and unions negotiate different standards in a collective agreement. Finally, in between these two are clauses with 'two thirds-binding law', whereby deviations are allowed when employers negotiate different standards with their work council. Of course, such deviations are possible with clauses that are translations from European Directives. But they have been introduced for clauses on notice periods (semi-binding law), working time (two-thirds binding law) and probation periods (three-quarters law). Importantly, such deviations can also be less favourable to workers, unless stipulated otherwise. Portugal, on the other hand, has a very legalistic tradition in which the labour code (the collection of all applicable labour law) regulates virtually all aspects of the labour relationship, and there is no use of derogations

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77. Whether or not this increased space is used in practice and results in more differentiated working conditions across workers is a different and more complex question that we do not discuss here. For instance, the legal possibility of firm-level agreements itself can influence the content of sectoral agreements even in the absence of actual firm-level agreements.

therefrom (OECD, 2017). According to various commentators, this legalistic tradition has limited the scope for self-regulation by the social partners (Traxler, 2003; Aghion *et al.*, 2011; Molina, 2014).

In the remainder of this section we focus on deviations not from national law, but deviations from collective agreements. While sector-level bargaining remains predominant in both the Netherlands and Portugal, with sectoral agreements covering around three quarters of employees, both countries have sought to create more scope for bargaining at the firm level. In the Netherlands, these discussions started in the early 1980s – when bargaining parties at the national level ('central parties') played an important role in wage setting – and institutional reform happened incrementally. In Portugal, decentralisation became an agenda item much more recently as a result of the 2011 sovereign debt crisis and the subsequent adjustment program. Consequently, the changes have been more abrupt and also more prone to reversals.

In the Netherlands, an agreement in 1982 by the main union and employer representatives ('Wassenaar agreement') is widely seen as setting the stage for the decentralisation of collective bargaining over wages and working conditions. It effectively signalled the end of active interference by the government in wage setting via wage freezes, even if, in practice, it retained a certain degree of control through the threat of intervention (de Beer, 2013).[78] The more hands-off approach by the government increased the responsibility of the social partners for outcomes at the sectoral level. In the bipartite agreement of 1993 ('Een Nieuwe Koers', 'A New Course'), the central parties emphasised the need for customization and differentiation in wage setting. Moreover, it argued for a greater emphasis on decentralised or firm-level bargaining to achieve this.

In contrast to other countries where decentralisation effectively led to collective bargaining taking place simultaneously at the sector and firm levels (e.g. Germany), in the Netherlands the main route to decentralisation has been to increase the scope for customization *within* sectoral agreements by allowing for more flexibility at the worker level without requiring an additional layer of bargaining at the level of the firm. One reason for this is that, in the Netherlands, unlike in Germany, unions are only weakly affiliated to work councils and trade unions are reluctant to delegate negotiating power to the work councils (Visser, 2016a). In addition, anecdotal evidence suggests that also employers may prefer to negotiate with a professionally organised and macro-oriented union instead of a work council existing of their own employees.[79]

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78. This is discussed in more detail in Section 6.5.
79. See *Financieel Dagblad*, Werkgever gaat zieltjes winnen voor leeglopende vakbonden, 1 October 2018.

Decentralisation within sectoral agreements has taken various forms (SER, 2006; Lier and Zielschot, 2014; Volkerink *et al.*, 2014). First, there has been a gradual shift from standards in collective agreements that specify narrow bounds for pay and working conditions, towards minimum standards that provide more space for issues such as performance-related pay. Minimum standards nowadays characterise the majority of pay clauses in sector-level agreements (Visser, 2013a; Lier and Zielschot, 2014).[80] The negotiation of higher standards is left to worker-level bargaining and in some cases to local unions or work councils (e.g. for working hours, see van Lier and Zielschot, 2014). Second, an increasing number of sectoral agreements include a range of working conditions from which employers and employees can choose, so-called '*à 1a carte*' provisions, that allow trading off pay and other working conditions. For instance, a part of gross salary can be used to finance additional leave or higher pension entitlements (Volkerink *et al.*, 2014). In the latter case, the total budget is still set by sector-level bargaining, but working conditions can be customised to workers' preferences.

Importantly, decentralisation in the Netherlands has offered more flexibility to firms and workers without undermining sectoral bargaining or its coverage (de Beer, 2013). As such, the Dutch case provides an example of 'organised decentralisation' (Traxler, 1995). As of 2014, 78% of employees were covered by a sectoral-level agreement, while the share of workers covered by a firm-level agreement was 8% (de Ridder and Euwals, 2016a). The share of employees covered by any collective agreement has been rather stable over time at around 80%.

In Portugal, decentralisation of collective bargaining is a much more recent and abrupt phenomenon, and it has largely been imposed from the outside as part of the adjustment program rather than by the social partners themselves (as in other Southern European countries, see Marginson, 2015). This is also related to the political developments in the country, which has emerged from a long-lasting conservative dictatorship (that ruled out independent unions) a little more than 40 years ago. The union movement that was born following the 1974 revolution, in a highly politically charged context, has gradually become more moderate. This process involved many steps, including: the break-up of the then single union confederation into two (1978); the creation of a national forum for tripartite dialogue (CPCS, 1984); a first income policy agreement (1986) which sought to control inflation; an important labour reform towards reducing the very high level of employment protection and segmentation that existed at the time (1989, see Martins,

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80. According to a 2014 survey, 52% of collective agreements are of a minimum nature, where deviations are only possible to the upside. In 28% of cases, deviations are possible in both directions. In 7% of CAs, deviations are not allowed (the remaining CAs do not stipulate whether deviations are allowed) (Lier and Zielschot, 2014).

2009); the 2003 introduction of the labour code that also featured the possibility of termination of collective agreements; and a number of tripartite agreements signed since the early 1990s, on average every two years, even if excluding CGTP, in particular those of 1990 and 1992.

While there have always been a number of firm- or holding-level agreements in the private sector, these largely concerned single firms or groups of firms that had previously been part of the public sector and were subsequently privatised.[81] The historical absence of firm-level bargaining in Portugal reflects a number of factors. First, the strict application of the favourability principle reduces incentives for firm-level bargaining. This entails that, in case of diverging standards in different agreements covering the same workers, it is the most favourable conditions across all agreements that apply to employees. Its strict application is a consequence of the strong legalistic tradition in labour matters, also shaped by many years of high inflation, which excluded the possibility of deviating downwards from sectoral standards in firm-level agreements, unless there was a view that, overall, the new terms would be more favourable to (incumbent) workers.[82] Second, strong competition between the two main unions, combined with low levels of membership and lack of trust from employees towards potential union representatives, effectively reduced the scope for firms to engage in firm-level bargaining.[83]

In 2009, a first attempt towards decentralisation was made when work councils in firms with more than 500 employees were given the right to engage in formal collective bargaining, but only if authorised by unions. Moreover, the resulting firm-level agreements were given precedence over sectoral agreements, even if some standards were lower, although presumably only in the context of an overall improvement in working conditions. In subsequent years, the firm-size threshold for collective bargaining by work councils was lowered to 250 in 2011 and 150 in 2012. However, approval from sectoral unions remains a pre-requisite. Moreover, given the competition and lack of trust between work councils

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81. Before the global financial crisis, only around 50-75 of the 300,000 firms in Portugal concluded or renewed a firm- or holding-level collective agreement in any given year. The number of these agreements has remained largely unchanged over the financial and sovereign debt crises. One should also note that, on top of these formal agreements, there is an additional number of informal firm-level agreements, signed with worker councils of large firms. While not qualifying as de jure collective agreements, these correspond as de facto agreements between firms and workers' representatives, setting a large number of work conditions that are respected by both employers and employees.

82. The state traditionally exerts a strong role in the regulation of the labour market, similarly to the French model. This has limited the scope for self-regulation by the social partners (Traxler, 2003; Aghion *et al.*, 2011; Molina, 2014). For instance, the labour code (the collection of all applicable labour law) includes 560 articles that regulate virtually all aspects of the labour relationship. Moreover, the minimum wage is relatively high compared to the median wage, in particular since 2006, reaching Kaitz ratios of around 60% (close to those of France and Slovenia, the highest in Europe).

83. At least in part this is a legacy from the 1928-1974 dictatorship which sought to compensate for the repression of independent trade unions and social dialogue through legal rules.

and unions, sectoral unions have not been keen to delegate bargaining to the former at the firm level. In practice, this has made it difficult for this new tool (collective bargaining conducted by work councils sanctioned by unions) to gain practical relevance.

It is not clear what the impact of these reforms on firm-level bargaining has been. Figures up to 2015 show that the number of firm-level agreements has been largely constant[84], suggesting that the recent regulatory changes did not have a major impact on the prevalence of firm-level bargaining. It also implies that in practice, the new framework has not so far provided much additional space for firms in terms of wage flexibility. In part, this may reflect the fact that such agreements have been largely imposed externally and hence benefited from limited acceptability among incumbent firm and worker representatives who, in any case, have a strong vested interest in sectoral bargaining. Especially when firms can be certain sectoral extensions will be extended anyway, they may not see a benefit from firm-level bargaining. It may also reflect the continued need for approval by sectoral unions, which greatly reduces the scope for signing firm-level agreements with less favourable conditions.

Looking forward, Portugal faces different options to decentralise collective bargaining further. One possibility would be to place more emphasis on bargaining with work councils, e.g. by allowing them to conclude formal firm-level agreements also without the consent of sector unions. However, this would potentially clash with the constitutional provisions that indicate that unions can conduct collective bargaining but do not offer the same rights to work councils. Furthermore, for this route to make bargaining attractive also for workers, work councils are needed that are both representative of the workforce and independent from the employer.[85] On the other hand, Portugal could promote the Dutch approach of increasing flexibility for workers and firms through the inclusion of e.g. *à-la-carte* provisions in sectoral agreements. Portugal could also facilitate additional deviations from the national labour code, by extending the range of subjects that can be adjusted (upward or downward) by bargaining or even restricting somewhat the scope of the labour code itself. These measures would broaden the scope for Pareto-improving agreements, and as such might make bargaining more attractive to a larger set of firms and unions. As in the Netherlands, the government could still influence the resulting agreements by specifying at what level parties are allowed to deviate (individual, work council or trade union) and to what extent deviations can also be less favourable to workers.

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84. This may be an underestimation, as there is an unknown number of informal firm-level agreements established with worker councils – these are not formally collective agreements but in practice play the same role (Palma Ramalho, 2009).
85. A mild version of this route would be to replace the veto of sector unions by a criterion that agreements will need to be confirmed by a referendum among the workforce.

6.3   EXTENSIONS

In both the Netherlands and Portugal, collective bargaining coverage extends well beyond the membership of trade unions and employer associations. A first reason for this, which applies in the case of the Netherlands, is the presence of so-called *erga omnes* provisions. In the Netherlands, collective agreements automatically apply to all workers within firms that sign directly or are subject to the agreement through their membership of a signing employer association. In Portugal, as in some other countries such Germany and Norway, this is not the case. A second reason, which applies to both countries, is the frequent use of administrative (i.e. government-issued) extensions of collective agreements beyond the membership of employer associations to all firms and workers in a sector. In order to have an agreement extended, a request has to be made by one or both signatory parties to the Ministry of Labour. Extensions have been motivated by the desire of creating a level-playing field and, in doing so, limiting the scope for competition on the basis of poor working conditions while enhancing inclusiveness and reducing wage inequality.[86]

One potential concern about extensions, however, is that the signatory parties to the agreement may not be representative of the firms and workers in the sector. As a result, there is a risk that collective agreements may not be well suited to the needs of firms and workers to whom the agreement is imposed by means of an administrative extension. For example, to the extent that larger firms are more likely to be part of an employer association, but also tend to be more productive and willing to pay higher wages, this may result in collectively agreed wage floors that are too high for smaller non-organised firms and therefore reduce employment—particularly of low-productivity firms and workers (Zalm, 1992). It has been argued that non-representative employer associations may even have an incentive to use extensions as an anti-competitive device that seeks to reduce competition from low-wage firms (Haucap *et al.*, 2001; Magruder, 2012; Martins, 2014).

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86. Extensions may further be used as an instrument to internalise any possible public good characteristics of collective agreements, such as sectoral training and mobility schemes that are funded by those subject to collective agreements (de Ridder and Euwals, 2016a). They may also serve to disseminate what may be considered as best practices within in a sector in different worker-related areas, such as personnel management, training, health and safety, technology usage, insurance, retirement packages, or performance-related incentives. Furthermore, in the absence of a national minimum wage, they may help prevent a downward spiral of wages and thereby an erosion of domestic demand. In the aftermath of the 1930s, this was a key motivation of the 1937 Introduction of Extension Act in the Netherlands (Zalm, 1992; Visser, 2018).

In order to alleviate such concerns, administrative extensions are often subject to representativeness criteria or a meaningful test of public interest.[87],[88] In the Netherlands, the 1937 Extension Act stipulated that extensions were only granted when an 'important majority' of workers in the sector was employed by firms which are members of the signatory employer association(s). The exact threshold was not made explicit until in the 1990s extensions were criticized by economists for contributing to wage drift and unemployment among the low-skilled workers (see, e.g., SER, 1992; Zalm, 1992). Following a positive advice by the Social and Economic Council (see, e.g., SER, 1992), the Minister specified that 60% qualified as an important majority, whereas with a proportion of between 55% and 60% of workers additional scrutiny was exercised. These rules are still in place today. Given that employer organisation in the Netherlands is rather high – 85% according to Visser's ICTWSS database – for many sectors this threshold is not prohibitive, although there are also examples of sectors where collective agreements cannot be extended anymore due to declining representativeness (such as in the shipping sector, see Mevissen *et al.*, 2015). The Dutch law also provides for a public-interest test. While political actors frequently call upon this clause to limit extensions to agreements that meet certain conditions (for some recent examples, see Tweede Kamer, 2016), in practice, the government has been reluctant to resort to this clause, fearing its political interference would disturb negotiations between bargaining partners. In a number of instances, the government has implicitly or explicitly referred to the threat of non-extension as a means to discipline the social partners, for instance, to promote wage moderation or to discourage the inclusion of social security provisions that it deemed excessively generous (see Section 6.5).

In Portugal, no representativeness criteria existed until the reform of 2012. Administrative extensions were quasi-automatic, making collective bargaining strongly dependent on government support (as in other Southern European countries, see Traxler, 2003; Molina, 2014). A brief study of the number of workers potentially subject to the extension and their wage increases would be conducted by the Ministry of Labour, using the most recent 'Quadros de Pessoal' data, a matched employer-employee register. However, if these findings on workers and wage increases had any impact on the decision to extend, our understanding is that an extension was even more likely the higher the number of workers affected and the higher their wage increases, not the opposite. After a temporary suspension

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87. Another route is to make membership of an employer organisation compulsory or strengthen incentives to join. Employer associations typically deliver several services to their members. This would effectively suggest a move towards the Scandinavian system of collective bargaining.
88. Germany recently abandoned its 50% representativeness criterion in favour of a test of public interest. The former was increasingly seen as an obstacle to extensions while a test of public interest provides more flexibility. This change was intended to promote the use of extensions.

of administrative extensions from June 2011 (a policy analysed in Hijzen and Martins, 2016), the criteria for extensions were reformed in 2012 by allowing extensions to be issued only if the employer organisation represented firms covering at least 50% of the workforce of the relevant sector. Since the density of employer association membership is rather low – 40% according to our estimates[89] – this has led to concerns that the conditions for extensions were too strict. In July 2014, with effect from January 2015, these requirements were again revised by adding an extra, alternative clause stating that extensions could also be issued if at least 30% of the membership of employer associations (in terms of the total number of firms) consisted of small- and medium-sized enterprises (firms with less than 250 employees). Since this new representativeness clause is met for the large majority of employer associations, this new, alternative criterion effectively represented a return to the situation pre-2012, characterised by a virtually automatic extension of all agreements. More recently, in June 2017, representativeness criteria were fully abandoned, implying a both *de jure* and *de facto* return to the pre-2011 period.

While representativeness criteria help avoid too large systematic differences in the characteristics of firms and workers in the organised (affiliated) and unorganised (non-affiliated) sector, there remains a risk that standards in collective agreements are not in line with the needs of all firms, whether they are affiliated or not. To address this issue, exemptions to administrative extensions can be granted to firms and workers that feel the sectoral agreement does not suit their needs. Importantly, even if such exemptions are not widely used, their existence provides incentives to better adapt the contents of sectoral agreements to the needs of firms that are not affiliated to an employer association.

In the Netherlands, there are two different ways through which firms can get exempted from extensions. Since 2007, the main route is to get dispensation from the social partners which have concluded the agreement (until then, having a firm-level collective agreement was a sufficient condition for dispensation).[90] Since 2014, all sectoral agreements are required – in order to qualify for an administrative extension – to include a transparent exemption framework stipulating objective dispensation criteria and the procedures for obtaining dispensation. The second possibility is to request dispensation from the Ministry. This is only possible if firms or subsectors can make a compelling case that firm-specific conditions justify dispensation and if they have concluded their own agreement

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89. This was calculated by the authors based on the Quadros de Pessoal (2010) data set as the percentage of private sector workers in affiliated firms. In contrast, in Visser's ICTWSS database, employer density was estimated at 65% in 2008, while it reports a figure of 34% for 1995 (Traxler, 2000).

90. This happened after some cases where unions were not deemed independent of the employer (sometimes established on the same day as the signatory date of the agreement). In response, the rules for what constitutes an independent union were strengthened and social partners were made the prime responsible for judging the need for dispensation (Rojer, M. F. P. and van der Veldt, 2010; Stege, 2011).

with an independent union. Traditionally, most conflicts over extensions have arisen in low-cost sectors such as retail, work agencies and cleaning (Visser, 2005). Between 2007 and 2015, the Ministry granted dispensation in 58 cases in response to 191 requests and rejected 77 of them, mostly on material grounds and sometimes on procedural grounds (i.e. no firm-level collective agreement) (MinSZW, 2016).

In Portugal, firms or unions may formally oppose an extension and make a request for their non-application. However, such requests have been issued very rarely, and have typically concerned unions that had not subscribed to the agreement being extended in the first place (but subscribed to an alternative agreement with the same employer association). Individual firms have rarely opposed extensions for a number of possible reasons, including low expectations that a request for non-application would be accepted, concerns about the effects of an application in terms of their reputation with banks and other firms they do business with, and the costs involved in applying for an exception. Non-compliance with the extension may also be an approach adopted by some firms, given potentially low levels of enforcement by the labour inspectorate and imperfect knowledge of labour law and collective bargaining, particularly among smaller and younger firms.

Another argument for extensions is that they reduce the transaction costs of setting working conditions, which may be particularly important for small firms that lack the resources to engage in firm-level bargaining (Blanchard *et al.*, 2014). This argument alone does not provide a sufficient justification for having extensions that impose similar conditions to all firms in a sector, but rather provides an argument for letting individual firms adopt sectoral norms voluntarily, i.e. 'opting in'. One example is the growing practice of 'orientation' in Germany, where employers voluntarily follow pay policies in collective agreements in the sector without being involved in their negotiations (Addison *et al.*, 2012). Compared with extensions, the main advantage of this approach is that it does not impose excessive pay conditions on low-productivity employers and that employers are not bound by the conditions in the collective agreement when economic circumstances change. This increased flexibility for firms comes at the cost of potentially lower labour standards for workers and higher inequality, especially if firms pick only some of the collectively agreed working conditions.[91] Since extensions were quasi-automatic, orientation has not played a role in Portugal, at least until recently. In the Netherlands, some firms voluntarily follow

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91. Addison *et al.* (2012) show that orientation tends to be partial in the sense that it leads to lower wages than in firms that are directly covered but higher than in firms that do not orientate their pay practices to the collectively negotiated wage agreements.

collective agreements ('incorporatiebeding'). However, when making an explicit reference to a collective agreement in an employment contract, the agreement becomes legally binding and firms cannot just 'opt out' as they please.

In the Netherlands, there has traditionally been broad support for extensions. According to surveys, most firms – also those bound to the collective agreements by extension – report being in favour of extensions (van den Berg and Van Rij, 2007; Mevissen *et al.*, 2015). While two parties are currently in favour of abolishing administrative extensions[92], most of the others see the extension procedure as a legitimate way to avoid downwards competition on working conditions. Hartog *et al.* (2002) did not find a significant wage effect of extensions in the Netherlands, while De Ridder and Euwals (2016a) find that wages are higher in sectors with extensions, but this effect was only present in the boom years 2006 and 2007 and not in the crisis years that followed. Given the absence of a wage premium in the crisis years, it is unlikely that extensions have resulted in large job losses as reported for e.g. Portugal (Martins, 2014). At the same time, there has recently been some discussion on whether the current extension and dispensation rules allow for enough customization or could harm outsiders (Mulder and Pikaart, 2016; Hartog, 2016; Gautier, 2016; Grapperhaus, 2016). Furthermore, there are concerns that *non-wage* clauses in sectoral agreements that try to neutralise activating reforms by the government – such as most recently the duration of unemployment benefits – could harm employment when such sectoral agreements are extended to the entire sector (de Ridder and Euwals, 2016b). Hence, suggestions have been made to allow for a stronger material appraisal of dispensation requests and the delegation of this responsibility away from social partners to an independent authority (Grapperhaus, 2016; Hartog, 2016; Mulder and Pikaart, 2016).

In Portugal, both the role of extensions and the need for representativeness criteria remain contentious, with virtually all social partners advocating the former and rejecting the latter. One important reason why representativeness criteria are controversial is that employer associations have too few members in most cases to allow concluding sufficiently representative agreements, at least by the standards of representativeness of countries such as the Netherlands. As a result, the introduction of strict representativeness criteria in 2012 may have played a role in bringing sector-level bargaining to a standstill (although it is difficult to disentangle its effect from that of the economic crisis). While the 2015 reform considerably reduced the stringency of representativeness criteria, it also risks reintroducing the problems associated with non-representative extensions.[93]

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92. These are the liberal party (VVD), which is currently the largest, and the radical right-wing PVV. Together, they currently hold 52 of 150 seats in Parliament.

93. In June 2017, a further reform in Portugal eliminated completely the representativeness requirement. It also required extensions to be issued no later than seven weeks after the request by the subscribing partners.

To solve this conundrum, one possibility may be to fix a timeline for gradually increasing the stringency of representativeness criteria. This should eventually eliminate non-representative extensions, while at the same time providing employer associations time to increase their membership levels, especially among smaller firms, which account for a large share of employment. Another option could be to follow the Dutch practice of a double criterion. For instance, in addition to the current threshold of 50%, Portugal could introduce an additional range – e.g. between 30 and 50% – where the government would grant extensions only when certain additional conditions are met (such as efforts to improve the representativeness of bargaining parties).[94] Furthermore, for both types of extensions, the government could require the existence of a clear dispensation framework, as is required to qualify for extensions in the Netherlands. Table 6.1 summarizes the main rules governing extensions.

Table 6.1 Extensions: conditions and exemptions

	Netherlands	Portugal – pre 2012; and from 2017	Portugal – between 2012 and 2017
Test of public interest	Yes - but rarely called upon explicitly	No, decision is entirely discretionary, not based on objective and verifiable criteria	No
Representativeness criteria	Share of workforce in signatory firms should exceed 60% (or 55% with additional scrutiny and 50% in exceptional circumstances)	No	2012 - 2014: Share of workforce in signatory firms should exceed 50% of total employment in relevant sector. 2015 - 2017: share of workforce in signatory firms should exceed 50%; or at least 30% of employer association members (firms) should have no more than 250 employees (the latter criterion being met in almost all cases)
Exemptions	Application procedure to government or though dispensation rules in collective agreements	No	No

94. I thank Jelle Visser for this suggestion.

6.4   THE CONTINUITY OF SECTOR-LEVEL AGREEMENTS: RETRO AND ULTRA-ACTIVITY

In order to ensure the continuity of rights and obligations in collective agreements, they may enter into force retro-actively, i.e. before their signature date, and/or remain effective ultra-actively, i.e. beyond the date of their expiration. These two instruments are not equivalent from the perspective of workers or firms. Retro-activity mainly matters for wages, since it typically imposes an obligation on firms to pay wage arrears, whereas ultra-activity seeks to preserve the continuity of not only wage floors but also other non-wage working conditions. Consequently, retro and ultra-activity are best seen as complements for ensuring the continuity of collective agreements. As most recent discussions have tended to focus on the pros and cons of ultra-activity we will start with this.

Ultra-activity

Ultra-activity entails that collective agreements remain effective after their date of expiration. In doing so, it provides a form of income security to workers in the medium term in a similar manner as the statutory minimum wage. This can also enhance labour peace and help foster a long-term perspective in collective bargaining. At the same time, ultra-activity tends to reduce incentives for collective bargaining and signing new agreements, particularly when wages may otherwise have to be renegotiated downward, such as, for example, during a recession. This is because it has a tendency to shift the focus of collective bargaining from the distribution of overall rents to that of *additional* rents, which are more limited and may indeed be negative in a recession. The pro-cyclical nature of economic rents creates pro-cyclical incentives for collective bargaining, and these are reinforced in the context of ultra-activity. While the pro-cyclicality of collective bargaining in itself may not be an issue, weak incentives for renegotiation reduce such pro-cyclicality in difficult economic conditions. This may hamper labour market resilience since it reduces the likelihood of finding mutually beneficial solutions in periods where these are most needed.[95]

Ultra-activity reduces the scope for nominal and real wage reductions once collective agreements have expired. While this is unlikely to be an important issue in normal times, it could become an obstacle to wage adjustment in recessions, particularly in a low-inflation environment. In normal economic times, there is little need for downward wage adjustment, while in recessionary periods with high inflation, such as those following

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95. Employers might anticipate that they are unlikely to win major concessions in economic downturns, leading to greater smoothing over the business cycle and more wage moderation during economic booms. However, this may not be enough to cope with unexpectedly large adverse shocks such as the recent global financial and sovereign debt crises.

the oil shocks in the 1970s or currency crises in emerging markets, downward real wage adjustments can be achieved simply through wage moderation without having to cut nominal wages. However, in a low- or even negative-growth and inflation environment such as the recent financial and sovereign debt crisis, ultra-activity can undermine labour market resilience in countries without independent macro-economic policies by increasing the degree of downward nominal wage rigidity – limiting the scope for real wage adjustment to restore external competitiveness and clear the labour market.[96]

Both Portugal and the Netherlands have some form of ultra-activity, but its scope differs (Table 6.2a). In the Netherlands, ultra-activity only applies to organised firms and not to firms covered by an administrative extension. This means that after the expiration date of a collective agreement, non-organised firms are allowed to cut wages as long as wages do not fall below the statutory minimum wage, offering an incentive to bargaining parties to strike a new agreement. In Portugal, ultra-activity related to clauses that determine the renewal of agreements is limited to 18 months from 2009 and 12 months from 2014. However, in continuing employment relationships that started when the collective agreement was in force, employers cannot cut nominal wages or adjust downward most other working conditions unless the worker agrees and, in some cases, the labour inspectorate is involved.[97] Ultra-activity also applies to all covered firms, including those covered through extensions.

The application of ultra-activity to extensions can have important implications for the incentives of bargaining parties, especially when there is a need for downward nominal wage flexibility. Guimaraes *et al.* (2017) show that in the aftermath of the crisis nominal (base) wage cuts have been virtually absent in Portugal, while downward nominal wage rigidity has become binding for the large majority of (continuing) worker-firm matches as reflected by the pervasiveness of nominal wage freezes. While in part this could reflect the role of binding wage floors, supported by ultra-activity, it may also reflect the need for union consent for cutting nominal wages in a context where employment protection is very strict. In any case, the lack of incentives to renegotiate wages downward, possibly

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96. When social partners have a shared understanding of the negative employment consequences, such risks can be alleviated with coordinated support for wage moderation. This underlines the importance of coordination and cooperation, which is discussed in Section 6.5 below.

97. It is also noteworthy that until 2003 collective agreements could only be terminated by agreement between the two parties. Only following a reform introduced in that year it became possible to terminate agreements unilaterally. These earlier domestically-led reforms have also created continuing discontentment with the political parties in the (extreme or radical) left, which represent about 15% of the electorate. These parties, as well as the largest trade union confederation, are still demanding the reversion of those earlier reforms, as they think this would strengthen the bargaining power of workers/unions, and always raise this issue in discussions of collective bargaining themes.

reinforced by ultra-activity, may have contributed to the sharp drop in the number of new contracts – not in the number of contracts in force, however – during the crisis, alongside the collective bargaining reforms mentioned above.

In the Netherlands, both employers and trade unions saw the need for wage moderation in the aftermath of the crisis. More recently, renewing collective agreements has proven difficult in various service sectors, such as retail trade and hospitality (van der Valk, 2016). However, this largely has reflected the role of difficult economic conditions in these sectors and the weak position of traditional trade unions, rather than ultra-activity.

Table 6.2a Ultra-activity

	Netherlands	**Portugal – pre 2012**	**Portugal – since 2012**
Scope	Ultra-activity of collective agreement, not for parties bound by extensions	Ultra-activity applies to both collective agreement and extensions.	Ultra-activity applies to both collective agreement and extensions.
Duration	Unlimited - unless stated otherwise in collective agreement	Time-limited unless stated otherwise in collective agreement: five years up to 2009 and 18 months since 2009	Time-limited unless stated otherwise in collective agreement: 12 months
Application	Workers who were employed by firm prior to expiration	Workers who were employed by firm prior to expiration	Workers who were employed by firm prior to expiration

Retro-activity

In both Portugal and the Netherlands, there is a possibility to activate collective agreements, entirely or in part, retrospectively so as to ensure continuity of rights and obligations. A difference between the two countries is that in the Netherlands this possibility only applies to the signatory parties of agreements and not to those bound by subsequent extension, whereas in Portugal, until the reform of 2012, retro-activity applied to both signatory parties and those bound by extensions. The rationale for retro-actively applying agreements and extensions is to ensure that a level playing is fully preserved, consistent with the spirit of sector-level bargaining and the logic behind extensions.

The application of retro-activity to extensions has been a source of concern, however. The reason is that extensions are typically administered with some delay. This means that the degree of retro-activity tends to be more important for extensions than for the original agreements. For example, Hijzen and Martins (2016) report that in Portugal during the period 2010-2011, the typical delay with which extensions entered into force

relative to the relevant collective agreement tended to be about 6 months. To the extent that collective agreements are publicly documented and there is little uncertainty as to whether or not they will eventually be extended, this should not pose any problems as long as firms have rational expectations and do not face any financial frictions. However, if many firms unexpectedly become liquidity-constrained as a result of a major unforeseen macroeconomic shock, the requirement to retro-actively pay wage increases over a considerable period of time could well lead such firms to lay off workers, with significant adverse implications for aggregate employment. Hijzen and Martins (2016) indeed find evidence that retro-activity contributed importantly to the adverse impact of extensions on employment growth during the crisis in Portugal, as agreements subject to longer retro-activity periods exhibit stronger negative effects.

As part of the labour market reform of 2012, retro-activity for extensions was abolished in Portugal (and has not been reinstated since). This means that retro-activity provisions under the Portuguese and Dutch systems are now very similar (Table 6.2b).

Table 6.2b Retro-activity

	Netherlands	**Portugal – pre 2012**	**Portugal – since 2012**
Scope	Possible to activate (part of the) CA retrospectively, but not for extensions	Possible to have enter CA retrospectively, including for extensions	Possible to activate CA retrospectively, but not for extensions

6.5   COORDINATION AND COOPERATION

Apart from the differences in rules governing the bargaining process discussed above, the Netherlands and Portugal differ in the degree to which the actions of bargaining units are synchronised ('coordination') and the quality of social dialogue between bargaining partners ('cooperation').

Coordination

Coordination among bargaining units can positively influence macroeconomic flexibility (Blanchard and Wolfers, 2000; OECD, 2006; Traxler and Brandl, 2012). Indeed many countries with some form of coordinated bargaining, such as Scandinavian countries, Germany, or Japan, have enjoyed comparatively high and stable employment over the years (IMF, 2016). Coordination can be state-imposed based on statutory controls such as indexation, state-sponsored through social pacts, can take the form of agreements between

or within the central employer and worker organisations, or can be led by trend-setters (resulting in so-called trend or pattern bargaining) (Traxler *et al.*, 2001). The issue of coordination typically arises in countries predominantly characterised by sector-level bargaining but effective coordination can also be achieved in countries with decentralised bargaining systems.

The Netherlands has a long tradition of state-sponsored coordination under which non-binding central agreements between the main employer and union confederations, with or without involvement of the government, have been fairly common since the Wassenaar agreement in 1982. Such agreements can shape expectations and establish norms in relation to collective bargaining and macro-economic policy, without imposing any formal rigidity on the pay policies of firms (Visser, 2013b). This type of coordination requires inclusive and representative employer and union confederations. Since such agreements can be fragile in practice, it is important for them to be underpinned by institutional arrangements that provide a stable support for social dialogue at the national level.

In the Netherlands, to ensure the confederations have a mandate, both union and employer confederations typically have an annual discussion round with their members to set guidelines for wage increases and other bargaining priorities. Especially for trade unions, this internal coordination is quite strong, as sectoral unions for instance agree on a maximal wage demand and can even possibly team up with employers against dissident unions. To support bi- and tripartite agreements at the national level, institutional arrangements also play an important role. The Social and Economic Council (a tripartite council of social partners plus independent members) and the independent Bureau for Economic Policy Analysis (CPB) particularly matter, providing platforms for regular discussion between the social partners and developing a shared understanding of the key challenges (den Butter and Mosch, 2003).

The Wassenaar agreement of 1982 remains the prime example of wage coordination to this day. It effectively broke the wage-price spiral that was paralyzing the economy at the time and heralded a prolonged period of price stability and strong economic growth. The agreement was reached between employer and employee organisations, but was also supported by the government e.g. with tax concessions that dampened the adverse effect of wage moderation on net incomes. Since then, there have been several other instances where social partners at the national level aimed to influence wage setting notably in the late 1990s and early 2000s, often with pressure from the government (including through a threat not to grant extensions). Central recommendations on wages in these years served as an important input for collective bargaining (van Houten, 2008).

Coordination also plays an important role regarding other aspects of collective bargaining. An important example concerns the level of entry wages set in collective agreements. In the early nineties, the government grew increasingly worried about declining employment of lower-skilled workers, which coincided with an increasing gap between the statutory minimum wage and entry scales of collective agreements. In response to government pressure, including the threat not to grant extensions, central parties issued a recommendation to bring the minimum wage scales in collective agreements down to the level of the national minimum wage. This approach proved highly successful. Whereas in 1994, the difference between the minimum wage in CAs and the national minimum wage (NMW) was on average 12%, this came down to 2.2% in 2004 and 1.7% in 2014 (Rojer, 2002; SER, 2006; MinSZW, 2015). Currently, a similar approach is taken to allow for special entry scales to support the participation of (partially) handicapped persons ('Participatiewet'). Other examples include the facilitation of temporary and part-time work, the employability of older workers and the inclusion of exemption procedures in collective agreements (see Section 6.3). Such measures have likely contributed to the strong structural performance of disadvantaged groups in the Dutch labour market.

Traditionally, active wage coordination across bargaining units has been limited in Portugal. Despite the absence of active coordination, the labour market remained relatively resilient until the mid-1990s, partly as inflation was also relatively high by European standards, allowing real wages to respond strongly to changes in unemployment without requiring any adjustment in nominal wages (Martins *et al.*, 2012). As in other high-inflation countries, the prospect of EMU membership also acted as catalyst for several pacts to bring wage inflation down during the 1990s (Fajertag and Pochet, 2000). As the entry into EMU removed this catalyst and inflation came down in the late 1990s, adjustment of real wages was much less automatic and became more reliant on more flexible wage setting mechanisms or coordination by social partners.

The main reason why active coordination did not materialise in Portugal, even during the post-2000 period, was that it proved difficult to reach agreements between all (currently four) employer confederations and the two main union confederations. As discussed above, even when compared to other new democracies, relations between bargaining parties in Portugal had been rather adversarial and parties often lacked a shared understanding of the economic problems at hand (Fajertag and Pochet, 2000; Natali and Pochet, 2009). In the case of the financial crisis, the issue at hand was the need for real wage adjustment in a low-inflation environment. Despite the frequent occurrence of tripartite agreements, one of the two major union confederations (the largest one) rarely subscribed to them. The tripartite agreement of 2012 was notable in that it did bring together the main employer and the smaller union confederations and also included many measures towards

greater labour cost flexibility. However, this may have been an exception reflecting the unique economic context at the time. The 2012 agreement played an important role in garnering support from the social partners for many of the reform measures agreed as part of Portugal's adjustment program.[98]

In the absence of more effective coordination by social partners, the national minimum wage gains particular importance as a coordination device. It is relatively high by European standards in Portugal, following a push during the period 2007-2011, and is set by the government in consultation with the social partners in a discretionary manner. Under such circumstances, the minimum wage has the potential to act an instrument of wage indexation, by setting a benchmark for collectively negotiated wages, even if it would not be largely automatic as in the case of France, for example (Fougère *et al.*, 2016). By contrast, in the Netherlands, the minimum wage has no strong coordinating role as it is considerably below the median and is set through a fixed formula based on collectively negotiated wages. Consequently, the minimum wage also has a more limited impact on the bargaining process.
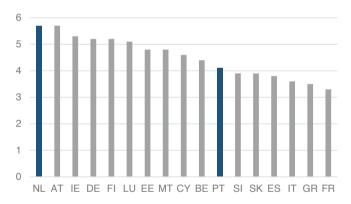
Figure 6.1 Cooperation in labour relations

Notes: The chart shows the average rating of executives of the labour-employer relations in their country, where 1 is 'generally confrontational' and 7 is 'generally cooperative'.
Source: World Economic Forum, The Global Competitiveness Report 2012-2013, Table 7.01.

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98. This agreement went much further than most tripartite agreements, however, since it was about the adjustment of the economy to the global financial and Eurozone debt crises and also included areas such as public administration and taxation.

Cooperation

There are striking differences in the quality of labour relations and the degree of trust between social partners in the two countries. While the quality of labour relations in the Netherlands is typically considered very high, and even the highest among 18 countries in a survey of managers conducted by the World Economic Forum, it is rather low in Portugal, as in other Southern European Countries (see Figure 6.1). Similar insights are obtained when looking at the degree of trust in others or trust in institutions. This suggests that the quality of labour relations is likely to depend on broader societal and cultural factors and not just on the main actors in collective bargaining and its institutional architecture.[99]

While the formation of trust between social partners, and that of the wider public in social partners and their institutions, is a complex process, it seems plausible that certain features of collective bargaining systems can contribute to build trust (IMF, 2016).[100] First, the inclusiveness of bargaining parties, and that of the collective bargaining system more generally, is likely to enhance trust. Bargaining parties are less likely to be inclusive when they are heavily fragmented and confederations are absent or have weak coverage. In the case of extensions, representativeness criteria can make the system fairer and thereby enhance trust—both directly, and indirectly by providing incentives for bargaining parties to reduce fragmentation and expand their coverage. Second, the nature of procedures with respect to opt out and extension can also help, such as the use of objective criteria for processing requests, the availability of accurate and verifiable information for assessing them, and the presence of an independent body in the case of extensions. Third, built-in incentives for regular renegotiation might enhance trust. Such incentives may take the form of time-limited agreements, or alternatively, restricting ultra-activity of agreements to certain non-wage working conditions. This strengthens incentives for the renewal of collective agreements, especially in difficult times. At the same time, rules that would impose collective bargaining may be counter-productive, if there is no shared willingness to reach agreements. Last, mechanisms that make social partners accountable for the effective implementation of collective agreements could foster trust by forcing them to take ownership and reducing the scope for opportunistic behavior. Straightforward ways to make the social partners more accountable include providing transparent, objective and accessible information on the key elements of collective agreements (e.g. a database with coded information of all collective agreements) and relying on independent labour inspectorates to monitor the effective implementation of agreements.

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99. Some scholars trace the emergence of trust between social partners in the Netherlands back to the culture of cooperation and trust that emerged in the late Middle Ages between the various provinces (Prak and Luiten van Zanden, 2013) and the joint fight of the Dutch against the water (den Butter and Mosch, 2003).
100. See Gould and Hijzen (2016) for a recent analysis of the determinants of trust and social capital with a specific focus on inequality.

Importantly, the quality of labour relations is also likely to reflect the success of past experiences and, as such, to be path-dependent (Lorenz, 1999; Blanchard and Philippon, 2004; Visser, 2005; Aghion *et al.*, 2011). For example, employee-employer relations in the Netherlands were much more adversarial at the time of the Wassenaar agreement (1982). According to Visser and Van der Meer (2011), it was only in the decade after Wassenaar that a consensus emerged between unions and employer organisations. Hence, the pact arguably served to demonstrate to bargaining parties that compromises could be mutually beneficial and provided a basis for future collaboration and the build-up of trusting relationships. In Portugal, as noted, the wage bargaining system was only put to a serious test in the 2000s and the aftermath of the Great Recession. As previously social partners had not found each other in (enough) mutually beneficial bargains, at that time the adversarial element was arguably still more present.

In turn, differences in the degree of trust and the quality of labour relations are likely to have important implications for the effectiveness of coordination and, ultimately, economic performance (Blanchard *et al.*, 2014; Blanchard and Philippon, 2004). For example, the improvement in economic performance in the Netherlands following the Wassenaar agreement of 1982, often referred to as the 'Dutch miracle', has in part been attributed to the importance of trust between the social partners and the quality of labour relations (Hemerijck and Visser, 1997; den Butter and Mosch, 2003). Among other things, trust enables social partners to engage in intertemporal efficiency-enhancing deals that are only feasible in a repeated game. For example, trust can contribute to risk sharing whereby firms insure workers against shocks hitting the firm, in return for lower overall wages. By contrast, the breakdown of collective bargaining at sectoral level during the recent economic crisis in Portugal has been linked in part to the lack of trust between trade unions, employer associations and the government (Addison, 2016). In Portugal, more trust between bargaining parties might have facilitated significantly the adjustment process during the 2011-13 recession. For instance, greater coordination in terms of faster and simultaneous adjustments in both prices and salaries could have restored external competitiveness more quickly at a lower cost to employment and living standards.[101]

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101. For instance, Martins (2017) finds that one of the measures included in the labour market reform of 2012 (and in the tripartite agreement of the same year) – greater flexibility in the setting of overtime pay premiums – promoted greater employment resilience. Martins (2019) presents evidence that increased employee representation in firms in Portugal has a positive causal impact on firm performance, in particular through stronger investments in worker training. See also Martins (2018) for an analysis of monopsony power and its wage effects in the case of Portugal.

6.6 CONCLUSION

Following the crisis, several southern European countries have implemented substantial reforms to their collective bargaining systems. As these were often imposed in the context of an adjustment programme, some see these reforms as a 'frontal assault' on collective bargaining. This chapter sought to put the reforms in perspective by comparing the changes to the Portuguese system with the system in the Netherlands, where sector-level bargaining also dominates but its functioning has evolved gradually over time in line with changing economic circumstances. The collective bargaining models of the Netherlands and Portugal share many broad features, including their focus on the sectoral level and the widespread use of extensions. We find that the Portuguese reforms have in fact brought the system more in line with Dutch practices, even if significant differences remain. As such, the reforms also contain elements of gradual policy convergence.

In characterising in more detail the differences between the two countries, our analysis identified a number of more general insights for improving the outcomes of sector-level bargaining systems. We see four main lessons.

First of all, introducing greater flexibility in collective bargaining systems through decentralisation can be achieved without undermining the inclusiveness of sectoral bargaining. The Dutch experience, as well as that of some neighbouring countries, has shown that decentralisation is feasible without undermining sectoral bargaining. Decentralisation in Netherlands has taken place gradually over more than three decades and is still continuing. By allowing deviations from national law, more flexibility was offered to firms and workers, inviting social partners to self-regulate. Decentralisation was also pursued *within* sector-level agreements, by making them less prescriptive, through the inclusion of framework provisions or a menu of options rather than by adding an additional layer of collective bargaining at the firm level. By contrast, in Portugal decentralisation is a more recent phenomenon. Similar to many other European countries with sectoral bargaining systems, it has sought to promote bargaining at the firm level but with limited practical impact on either the scope for flexibility at the firm level or the integrity of sector-level bargaining. Looking forward, the Dutch experience can offer inspiration for three (complementary) broad routes for greater decentralisation. One would involve to facilitate additional deviations from the national labour code, by extending the range of subjects that can be adjusted by bargaining or even restricting somewhat the scope of the labour code itself. This could broaden the scope for Pareto-improving bargains, and as such might make bargaining more attractive to a larger set of firms and unions. As in the Netherlands, the government could still influence the resulting agreements by specifying at what level parties are allowed to deviate (individual, work council or trade union)

and to what extent deviations can also be less favourable to workers. Furthermore, there could be more customization *within* sector-level agreements whereby parties at the local level can trade-off various elements of the agreements. The last route would be to further support firm-level bargaining. A critical question in such an endeavour is whether firms are allowed to form a collective bargaining agreement with a work council also without the consent of unions. For this route to make outcomes also attractive for workers, work councils would be needed that are both representative of the workforce and independent from the employer.

Second, coverage extensions can help promote inclusiveness, but need to be subjected to representativeness or public interest criteria. Whereas both countries never relied on a public interest criterion, since the 1990s the Netherlands did place stronger emphasis on representativeness as a criterion for the extensions of collective agreements than Portugal did. Over the years, the framework for exemptions from extensions has undergone gradual change – both material and procedural – and up to this day measures are being proposed to further strengthen its transparency. Although representativeness criteria preclude extensions in some sectors, they are widely seen as a necessary tool to legitimise extensions to non-signatory parties. Representativeness criteria remain controversial in Portugal mainly because representativeness is typically low: it is difficult to specify criteria that are sufficiently strict to be meaningful, yet sufficiently easy to satisfy to allow an effective role for extensions. To solve this conundrum, one might start with a low threshold but set a time-line for gradually increasing the stringency of representativeness criteria up to a reasonable level. This would eventually ensure that non-representative extensions are eliminated, while also providing time for employer associations (and potentially unions) to increase their membership levels, especially amongst smaller firms. Another option might be to follow the Dutch practice of a double criterion. For instance, in addition to the current threshold of 50%, there could be an additional range – e.g. between 30% and 50% – where the government would grant extensions only when certain additional conditions are met (e.g. efforts to improve the representativeness of bargaining parties). Furthermore, for both types of extensions, the existence of a clear dispensation framework might help – as required to qualify for extensions in the Netherlands.

Third, retro-activity and ultra-activity help ensure the continuity of collectively agreed rights and obligations but are best limited to signatory parties. In both the Netherlands and Portugal, retro-activity applies only to the signatory parties of agreements. In contrast, ultra-activity applies to signatories and non-signatories in Portugal, but only to signatory parties in the Netherlands. While there are good reasons for ensuring the continuity of agreements through retro and ultra-activity, this can become an obstacle to adjustment in

severe recessions. Limiting retro- and ultra-activity to the signatory partners may reduce this potentially adverse impact, while also possibly enhancing incentives for collective bargaining.

Last, effective coordination between bargaining units and high-quality labour relations are crucial for the performance of collective bargaining systems. In the Netherlands, labour relations tend to be consensual, marked by a comparatively high level of trust between social partners and a shared understanding of the main economic problems at hand. Collectively bargained wages tend to be effectively coordinated (both within and between employers and union confederations) and seem well aligned with macro-economic conditions. In Portugal, labour relations have been conflictual, with lower levels of trust between the social partners, and a weak coordination of collective bargaining outcomes. In contrast to the tradition of consensus building in the Netherlands, as mentioned Portugal has a strong legalistic tradition which leaves limited scope for self-regulation by social partners at the sectoral level and may reduce their responsibility and accountability for economic outcomes. Social partners have more frequently placed themselves in a role of opposition to government (and unions in opposition to employers, and vice versa) than in one of partnership, particularly in periods of recession. Of course, improving labour relations and cooperation is a difficult undertaking that is path dependent and for a large part beyond the direct control of policymakers. Yet, the Dutch experience suggests that the government can promote self-regulation, for example by offering both 'carrots' (e.g. fiscal subsidies, greater involvement in training activities, allowing deviation from very prescriptive national law) and 'sticks'. The latter may also include the rules regarding collective agreement extensions, which can help make the content of collective agreements more inclusive.

Looking forward, both countries studied here – as well as most other countries where sectoral bargaining remains prevalent – face a number of common challenges related to the decline in union density and the emergence of new forms of work. Although union density in both countries is not relevant for the decision to grant extensions, its decline nonetheless threatens to undermine the legitimacy of collective agreements, especially in some sectors and among younger cohorts. In the Netherlands, the sharp increase in the number of independent workers – to over a million in 2015 – challenges the legitimacy of collective agreements as their interests can be at odds with those of employees. As a way to improve the representativeness of union demands, the Dutch Social and Economic

Council advises also consulting non-members, including independent workers (SER, 2013).[102] Declining union density may also threaten the bargaining power of unions, thereby possibly undermining collective bargaining as well.[103]

While detailed qualitative comparisons – such as those in this chapter – can be useful in inspiring the directions of policy changes, concrete decisions on specific instruments should also be grounded in empirical research. In Portugal, accurate and reliable information is available to monitor working conditions, as well as trade union membership and (since 2010) employer association membership. These unique datasets, including a matched employer-employee panel, have been made available to researchers in different forms over the last two decades, facilitating the emergence of a large body of high-quality micro-econometric research on different labour market issues, including collective bargaining. This research has been instrumental to create the foundations of evidence-based policy. The availability and use of similar datasets in the Netherlands and other countries could similarly lead to greater insight on the effects of different labour market institutions in various contexts, including particularly collective bargaining, leading to better policy making.

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102. This is also the approach taken by the new Dutch trade union 'Alternative for Trade Union' ('Alternatief voor vakbond', avv). In their so-called 'support model' of representation all stakeholders – whether members or not – can take part in (online) consultations on ongoing negotiations (Mulder and Pikaart, 2016).
103. In the Netherlands, the low union density is seen as one of the reasons that unions in certain non-tradable sectors recently had to agree to less generous working conditions (van der Valk, 2016).

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NEDERLANDSE SAMENVATTING

Dit proefschrift bevat vijf hoofdstukken over de politieke economie van economische hervormingen die bij Europese beleidsmakers hoog op de agenda staan en tegelijkertijd op veel maatschappelijke weerstand kunnen rekenen. In het eerste deel van het proefschrift (hoofdstukken 2 tot en met 4) kijk ik naar de publieke opinie ten aanzien van maatregelen om de overheidsfinanciën beter bestand te maken tegen de gevolgen van vergrijzing, waaronder met name de verhoging van de pensioenleeftijd. Aan de hand van uitgebreide enquêtes onder de Nederlandse bevolking in de periode 2003-2019, kan ik verschillende lessen trekken over de drijfveren van steun voor – en weerstand tegen – dergelijke hervormingen. In het tweede deel van het proefschrift (hoofdstukken 5 en 6) ga ik in op collectieve arbeidsovereenkomsten (cao's). In de nasleep van de Europese schuldencrisis werden landen als Portugal en Griekenland in het kader van aanpassingsprogramma's gevraagd om hervormingen door te voeren om hun loonvorming flexibeler te maken. Deze hervormingen riepen veel controverse op omdat ze tegelijkertijd de positie van vakbonden dreigden te ondermijnen. Op basis van de Nederlandse ervaringen, kan ik een aantal lessen trekken over het nut en noodzaak van hervormingen om de loonvorming te flexibiliseren.

Draagvlak voor pensioenhervormingen

In alle Europese landen voorspellen demografen een substantiële vergrijzing van de bevolking, die deels al gaande is. Terwijl in het jaar 2000 het aantal personen van pensioengerechtigde leeftijd (65 jaar en ouder) nog ongeveer zo groot was als een kwart van de beroepsactieve bevolking (15 tot 65 jaar), zal dit volgens de laatste voorspellingen in 2050 52% en in 2100 57% bedragen. Dit betekent dat pensioenen die volgens het 'omslagmodel' gefinancierd worden, zoals in Nederland de aow-uitkering (Algemene Ouderdomswet), bij een gelijkblijvende pensioenleeftijd door een steeds kleinere groep mensen moet worden betaald, met sterk stijgende premies, een stijgende staatsschuld of dalende uitkeringen als gevolg. Vanwege deze voorspellingen nemen pensioenhervormingen, zoals verhogingen in de pensioenleeftijd, al geruime tijd een prominente plaats in op de agenda's van Europese beleidsmakers.

Pensioenhervormingen zijn tegelijkertijd vaak erg impopulair. De politieke economie literatuur wijst op verschillende bronnen van weerstand. Hervormingen kunnen stuklopen op de oppositie van machtige groepen, die veel te verliezen hebben bij dergelijke hervormingen, zoals oudere werknemers in het geval van pensioenhervormingen. Maar weerstand tegen hervormingen kan ook veel breder leven. Universele voorzieningen van de verzorgingsstaat zijn vaak zeer populair bij grote delen van het electoraat, ook bij groepen die het meest baat zouden hebben van hervormingen, zoals jongeren in het geval

van pensioenhervormingen. Daarmee samenhangend zijn burgers vaak niet goed op de hoogte van de kosten van sociale zekerheid, en zien zij daarom de noodzaak niet om te hervormen. Regeringen zijn dan ook vaak terughoudend met hervormingen, vanwege de daaraan verbonden electorale risico's.

Hoofdstuk 2 zoomt in op een van de meest veelbesproken hervormingen in Nederland van de afgelopen jaren, de verhoging van de aow-leeftijd van 65 naar 67. Deze maatregel is jarenlang bediscussieerd en werd tenslotte in 2012 aangenomen. Het doel van Hoofdstuk 2 is te ontwarren welke factoren de publieke opinie over de verhoging van de pensioenleeftijd kunnen verklaren. Daartoe maak ik gebruik van een unieke longitudinale dataset over de houdingen van Nederlandse huishoudens ten aanzien van de verhoging van de aow-leeftijd (en daarnaast, een verhoging van de premies of een daling van de uitkeringen) in de jaren 2003 tot en met 2013. Mijn bevindingen bieden verschillende nieuwe inzichten. Ten eerste, terwijl leeftijd en inkomensniveau in politieke economie-modellen over steun voor pensioenhervormingen vaak een grote rol spelen, suggereren mijn eigen analyses dat ze van minder belang zijn. In plaats daarvan komen opleidingsniveau, arbeidsmarktstatus en psychologische eigenschappen op individueel niveau als de meest systematische drijfveren uit de bus. Ten tweede blijken de 'jaareffecten' de voornaamste verklarende factor te zijn voor de mate van acceptatie van de hervorming. Terwijl in 2004 minder dan 20 procent van de respondenten zou kiezen voor een verhoging van de pensioengerechtigde leeftijd als eerste hervormingsvoorkeur, was dit in 2012 gestegen tot ruim 40 procent. Ik interpreteer het tijdspatroon in de publieke opinie over de pensioenhervorming als bewijs van een collectief leerproces, waarbij huishoudens geleidelijk aan hun verwachtingen en hervormingsvoorkeuren bijstellen, in het licht van nieuw verworven informatie.

Ook in veel andere Europese landen zijn de afgelopen jaren pensioenhervormingen doorgevoerd. Tegelijkertijd zijn deze hervormingen recentelijk in veel van deze landen ter discussie gesteld, met name door zogenoemde populistische partijen (bijvoorbeeld de PVV en SP in Nederland, de Lega Nord en de vijfsterrenbeweging in Italië, het Front National in Frankrijk en Podemos in Spanje). Deze partijen worden 'populistisch' genoemd, omdat zij een retoriek hanteren waarin er een antagonistische scheidslijn getrokken wordt tussen 'het gewone volk' en een 'kwalijke elite' die niet handelt volgens de wil van het gewone volk. Onderzoek naar de impact van populistische partijen op sociaaleconomisch beleid staat nog in de kinderschoenen. Politieke wetenschappers stellen dat de opkomst van populisme, enerzijds, beleid responsiever kan maken doordat beleidsmakers gedwongen worden meer aandacht te besteden aan zorgen van het gehele electoraat. Tegelijkertijd vrezen vooral economen en beleidsmakers dat populistische partijen munt kunnen slaan uit onwetendheid van kiezers, en stemmen kunnen winnen door simpele oplossingen te beloven die op langere termijn niet houdbaar zijn. Wat deze zorg bij economen zou

kunnen versterken, is de neiging die burgers soms hebben om de kosten van publieke voorzieningen, in termen van een hogere belastingdruk of een oplopende staatsschuld, te onderschatten (zogenaamde 'fiscal illusion' ofwel 'budgettaire illusie').

Tegen deze achtergrond bestudeer ik in Hoofdstuk 3 of mensen die sterke populistische opvattingen hebben ook significant positiever staan tegenover expansief begrotingsbeleid (een hogere staatsschuld, lagere belastingen en/of hogere uitgaven), en of populistische houdingen het risico op 'budgettaire illusie' versterken. De analyse is gebaseerd op de vraag, die in zomer van 2017 actueel was, hoe om te gaan met meevallers in de belastingopbrengsten. Ik meet de populistische gezindheid van respondenten met behulp van een door politieke wetenschappers ontwikkelde maatstaf. De rol van budgettaire illusie bestudeer ik met een maatstaf van de geletterdheid van respondenten, en door middel van een experiment waarin een willekeurig deel van de respondenten wordt 'behandeld' met informatie over de staatsschuld. Het hoofdstuk levert drie bijdragen aan de literatuur. Ten eerste breng ik begrotingsvoorkeuren van respondenten in verband met populistische gezindheid. Voor zover bekend is dit nog nooit eerder gedaan, en mijn bevinding is dat de populistische gezindheid van respondenten een sterke voorspellende waarde heeft voor hun begrotingsvoorkeuren, veel meer dan traditionele verklaringen zoals inkomenspositie of links-rechts oriëntatie. Ten tweede vind ik, in lijn met eerder onderzoek, dat geletterdheid en informatievoorziening – die het mogelijke optreden van budgettaire illusie kunnen verlichten – inderdaad zorgen voor minder expansieve begrotingsvoorkeuren. Ten derde concludeer ik dat het effect van geletterdheid mede afhangt van de populistische gezindheid van respondenten. In het bijzonder blijkt dat weinig geletterde respondenten significant positiever staan tegenover een belastingverlaging als ze sterk populistisch gezind zijn, maar niet wanneer ze een gunstiger beeld hebben van de politieke elite. In de 'populistische tijdgeest', zoals politicologen het huidige politieke klimaat kenmerken, leidt lage geletterdheid dus eerder tot voorkeuren voor expansief begrotingsbeleid.

Hoofdstuk 4 bouwt voort op de analyses in Hoofdstuk 2 en Hoofdstuk 3. De analyse in Hoofdstuk 2 eindigde in het jaar 2013, een jaar na de beslissing tot een hogere pensioenleeftijd. In de jaren daarna nam het verzet tegen de hervorming weer toe en bij de parlementsverkiezingen in 2017 stelden diverse populistische partijen voor om de hervorming weer terug te draaien. Met mijn data kan ik verschillende mechanismes onderzoeken achter het verzet tegen de verhoging van de pensioenleeftijd. Ten eerste tonen mijn uitkomsten aan dat zorgen van met name lager opgeleiden een deel van de weerstand verklaren, aangezien zij harder geraakt worden door een verhoging van de aow-leeftijd vanwege een kortere levensverwachting en een lagere mate van baantevredenheid. Ten tweede kan lage geletterdheid een deel van het verzet tegen de hervorming verklaren.

Ten derde blijkt populistische gezindheid zelf een sterke verklaring van de weerstand tegen de verhoging van de aow-leeftijd, waarmee het doorvoeren van hervormingen een grotere uitdaging blijkt te zijn in een populistische tijdgeest.

De economische impact van cao's

Het tweede deel van dit proefschrift gaat in op misschien wel het meest omstreden type hervormingen in landen van de Europese Monetaire Unie (EMU) van de afgelopen jaren, namelijk hervormingen ten aanzien van collectieve arbeidsovereenkomsten (cao's). Door samen een munteenheid te vormen, leverden individuele landen in 1999 een belangrijk beleidsinstrument in waarmee ze eerder op economische schokken konden reageren, namelijk hun wisselkoers. Het verlies van het instrument van de wisselkoers was niet triviaal, aangezien met name de Zuid-Europese economieën tot ver in de jaren 1990 regelmatig gebruik maakten van competitieve devaluaties. Toen de Grote Recessie de EMU en de toen zestien lidstaten voor een ongeëvenaarde uitdaging stelde, werd al gauw duidelijk dat sommige van deze economieën niet in staat waren om de enorme aanpassing van relatieve prijzen tot stand te brengen die op dat moment nodig leek te zijn.

Toen sommige van de Zuid-Europese landen in de problemen kwamen en een steunpakket kregen, stelde het consortium van de Europese Commissie, de Europese Centrale Bank en het Internationaal Monetair Fonds – vaak samen aangeduid als 'de trojka' – ook een reeks hervormingen voor in cao-stelsels. In deze hervormingen werd getracht meer ruimte te creëren voor onderhandelingen op ondernemingsniveau ten koste van onderhandelingen op sectorniveau, in de hoop dat de loonvorming daarmee responsiever zou worden voor lokale omstandigheden (zoals een dreigend faillissement). Deze hervormingen bleken bijzonder omstreden, en kwamen volgens sommigen zelfs neer op een 'frontale aanval' op collectieve onderhandelingen als zodanig. Deze controverse hangt samen met het feit dat de wijze waarop loononderhandelingen verlopen belangrijke gevolgen heeft voor de machtsbalans tussen arbeiders en bedrijven. Om deze reden wordt decentralisatie van cao-onderhandelingen ook wel verantwoordelijk gehouden voor een toename in de loonongelijkheid en tweedelingen op de arbeidsmarkt.

In deze context is Nederland een interessante casus, omdat sociale partners in Nederland in staat zijn gebleken om de externe concurrentiekracht in stand te houden, terwijl het systeem van loononderhandelingen wel degelijk sterke rigiditeiten kent. Hoewel een reeks grote bedrijven over hun eigen cao's beschikken, is het voor bedrijven niet gemakkelijk om zich aan sectorale overeenkomsten te onttrekken en wordt het overgrote deel van het personeel gedekt door een sectorale overeenkomst. Bovendien hebben sommige collectieve overeenkomsten een duur van twee of zelfs drie jaar, wat een belemmering vormt voor het aanpassen van lonen in geval van een schok.

In Hoofdstuk 5 analyseer ik of de trage wijze waarop de lonen zich in Nederland kunnen aanpassen aan economische schokken van invloed is op de werkgelegenheid. Ik gebruik hierbij de sterke terugval van de groei en inflatieverwachtingen in de nasleep van de val van Lehman Brothers, toen de loongroei binnen twee kwartalen terugviel van 3.5 procent tot 1 procent. Vervolgens kijk ik naar de differentiële effecten op de werkgelegenheid binnen bedrijven die vielen onder 'oude' overeenkomsten (afgesloten voor de val van Lehman Brothers), versus de bedrijven die tijdig de lonen omlaag konden bijstellen. Interessant genoeg constateer ik dat er geen verschil bestaat tussen de werkgelegenheid in bedrijven die de schok konden anticiperen en zij die dat niet konden. In plaats daarvan vind ik dat niet-anticiperende bedrijven in staat waren om de loonkosten van hun personeel te verlagen, door het schrappen van bonussen en het aanpassen van zogenaamde incidentele looncomponenten. Dat betekent dat rigiditeiten in de loonvorming niet per se hoeven te leiden tot werkgelegenheidsverlies, wanneer bedrijven voldoende zeggenschap hebben over andere looncomponenten.

De Nederlandse ervaring met sectorale cao-onderhandelingen kan ook andere lessen bieden voor landen die terughoudend zijn om onderhandelingen te verplaatsen naar het niveau van de onderneming. In Hoofdstuk 6 vergelijk ik de abrupte hervormingen in het cao-stelsel die Portugal pleegde in de context van het aanpassingsprogramma van 2011, met het onderhandelingsstelsel zoals dat zich in Nederland gradueel ontwikkeld heeft in de context van de doorlopende dialoog tussen sociale partners sinds het Akkoord van Wassenaar in 1982. De vergelijking tussen het Nederlandse en het Portugese systeem biedt meerdere inzichten. Ten eerste, door meer maatwerk te bieden in sectorale cao's is de flexibiliteit van de loonvorming in Nederland vergroot zonder sectorale onderhandelingen te ondermijnen. Ten tweede, het algemeen verbindend verklaren van cao's is een nuttig instrument om de dekking van cao's te vergroten, maar ook in Nederland zijn deze administratieve extensies wel onderhevig aan diverse eisen om het risico te beperken dat de loonvorming wordt gedomineerd door een te kleine groep bedrijven. En tenslotte, goede arbeidsrelaties en effectieve coördinatie tussen sociale partners kunnen de prestaties van cao-stelsels verbeteren door welvaartsverhogende uitruilen mogelijk te maken. De Nederlandse ervaring suggereert dat de overheid dit soort uitruilen tussen sociale partners kan bevorderen, door het aanbieden van zowel 'wortels' (zoals bijvoorbeeld belastingsubsidies) als 'stokken' (zoals het dreigement om een eventuele administratieve extensies niet toe te kennen).

APPENDICES

Appendix 1 Figures and tables as background to Introduction

Country codes:

BE   Belgium
FR   France
DE   Germany
GR   Greece
IT   Italy
NL   Netherlands
PT   Portugal
ES   Spain

Table A.1.1 Government debt in selected EA countries, 1980-2019

	1980	1990	1999	2008	2013	2019
BE	76.4	129.6	114.4	92.5	105.5	101.0
FR	20.8	35.6	60.5	68.8	93.4	99.3
DE	n/a	39.0*	60.1	65.5	78.6	58.6
GR	22.5	73.2	98.9	109.4	177.9	176.6
IT	n/a	98.7	109.7	102.4	129.0	133.2
NL	43.6	75.1	57.5	53.8	67.8	49.2
PT	n/a	56.4	51.1	71.6	128.9	117.6
ES	16.6	42.5	62.5	39.4	95.5	96.4

* figure for 1991, the first available figure after German reunification.
Source: IMF WEO database, October 2019.

Table A.1.2 Current account balance in selected EA countries, 1980-2019

	1980	1990	1999	2008	2013	2019
BE	-4.0	1.8	7.7	-1.0	-0.3	-1.1
FR	-0.6	-0.8	4.4	-0.7	-0.5	-0.5
DE	-1.8	3.1	-1.4	5.7	6.6	7.0
GR	-3.8	-3.6	-3.6	-14.5	-2.6	-3.0
IT	-3.5	-1.9	1.1	-2.8	1.0	2.9
NL	-0.4	2.5	3.5	5.0	9.8	9.8
PT	-3.3	-0.2	-8.9	-12.1	1.6	-0.6
ES	-2.3	-3.4	-3.3	-9.3	1.5	0.9

Source: IMF WEO database, October 2019.
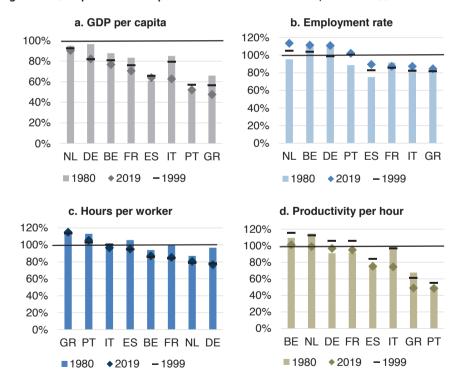
Table A.1.3 Literacy and numeracy proficiency in selected EA countries

Share of respondents with the lowest and highest proficiency level

	Literacy		Numeracy	
	% lowest	**% highest**	**% lowest**	**% highest**
BE	14	12	13	17
FR	22	8	28	8
DE	18	11	18	14
GR	27	5	29	6
IT	28	3	32	5
NL	12	18	13	17
ES	27	5	31	4

Note: The table shows the scores on proficiency levels as measured in the 2014-5 (Greece) and 2011-2012 (other countries) OECD Adult Skills Survey. 'Lowest' refers to level 1 or below, 'highest' to level 4 and 5. Results for Belgium refer to Flanders; Portugal did not participate in the survey.
Source: OECD (2019b), figures 2.6 and 2.9

Figure A.1.1 GDP p.c. and its components in selected EA countries, relative to US

Note: The figure shows the per capita GDP (in 2018 US$) and its three subcomponents for selected EA economies relative to the United States. The employment rate is the total number of employed persons divided by the total population.
Source: The Conference Board Total Economy Database™ (Original version), April 2019
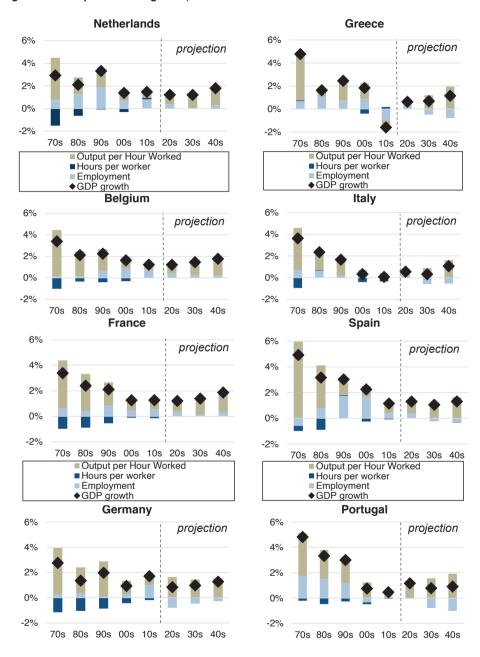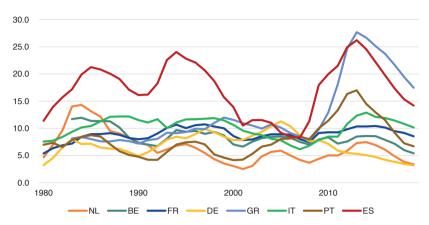
Figure A.1.2 Composition GDP growth, 1970s and further

Note: The figure shows GDP growth and its three subcomponents for selected EA economies. The employment rate is the total number of employed persons divided by the total population. Projections are based on the European Commission's Ageing's forecasts.

Source: The Conference Board Total Economy Database™ (Original version), April 2019 and EC (2018).

Figure A.1.3 Unemployment rate, 1980-2019
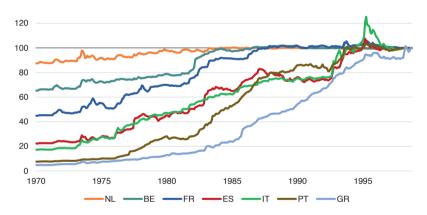
Annual average; share of active population of those aged 15-65

Source: OECD, LFS database

Figure A.1.4 Exchange rate vis-à-vis D-mark, 1970-1999

Index 1998m12=100

Source: Thomson Reuters.

Appendix 2 to Chapter 2

Table A.2.1 Description of survey questions used

Variable	Meaning
Dependent variables	
expected_retirementage	'At what age do you expect to retire, or make use of the early retirement arrangement?'
aow_reform_pref1	'To make sure that the general old-age pension remains affordable certain measures have to be taken. Which of the following measures appeals to you most?'.
	'a lower general old-age pension at the age of 65',
	1. 'an increase in the old-age pension premium for people working',
	2. 'increase the age by two years (from 65 to 67 years of age) on which one will receive the general old-age pension'
	From 2012 onwards – when the government had raised of the statutory retirement age – the last option was asked without reference to 65/67.
aow_reform_pref2	'Which of the two remaining measures appeals the most to you thereafter?' (1, 2, 3)
aowage_up_first (D)	Respondent's 1st choice is to raise the retirement age
cut_benefits_first (D)	Respondent's 1st choice is to cut the level of benefits
raise_premiums_first (D)	Respondent's 1st choice is to raise the contributions paid
Covariates	
age	Approximate age of respondent (year survey minus year of birth)
below35, etc. (D)	Respondent is aged below 35, etc.
employee (D)	Employed on a contractual basis
self_employed (D)	Works in own business, free profession, freelance, self-employed
household (D)	Respondent works in own household
benefits (D)	Respondent is (pre)retired, disabled, or working keeping benefit payments
other (D)	Respondent is busy otherwise (student, looking for work, volunteering)
grossincome	Total annual gross income (in EUR 1,000)
financial_wealth	Total of financial assets (in EUR 1,000, can be negative)
female (D)	Female
married (D)	Married
child (D)	One or more children
grandchild (D)	One or more grandchildren
health	Self-reported health score (5 categories, 5=highest)
low (D)	Special, primary or lower vocational
preuniversity (D)	Pre-university education (HAVO/VWO)
vocational_med (D)	Intermediate vocational training (MBO)
vocational_high (D)	Vocational colleges (HBO)
university (D)	University
riskaversion	Respondent's score on risk aversion (see Table A2.3)
patience	Respondent's score on patience (see Table A2.3)
conscientious	Respondent's score on conscientiousness (see Table A2.3)
locus_of_control	Respondent's score on locus of control (see Table A2.3)
frequency	Frequency of participation in the questionnaire on retirement expectations and preferences (runs from 1 to maximum 11 for each individual)

D = dummy variable

Table A.2.2 Summary statistics by year, 2003-2013

	2003		2004		2005		2006		2007	
	N	mean	N	mean	N	mean	N	mean	N	mean
preference_aowage_up	.	.	1515	0.18	1645	0.20	1547	0.25	1465	0.28
expected_retirementage	999	62.59	949	62.32	1001	62.23	910	62.98	893	63.21
age	999	42.47	1515	45.36	1645	44.00	1547	44.63	1465	45.68
employee	999	0.95	1515	0.68	1645	0.68	1547	0.68	1465	0.67
self_employed	999	0.04	1515	0.05	1645	0.05	1547	0.05	1465	0.05
household	999	0.00	1515	0.11	1645	0.11	1547	0.11	1465	0.11
benefits	999	0.00	1515	0.11	1645	0.11	1547	0.11	1465	0.13
other	999	0.01	1515	0.05	1645	0.06	1547	0.05	1465	0.04
grossincome	922	36.18	1445	31.03	1556	30.06	1473	29.36	1418	29.97
financial_wealth	964	26.43	1482	26.55	1583	26.88	1507	27.75	1449	27.85
female	999	0s.35	1515	0.45	1645	0.48	1547	0.47	1465	0.48
married	999	0.63	1515	0.65	1645	0.64	1547	0.64	1465	0.66
child	999	0.53	1515	0.55	1645	0.55	1547	0.55	1465	0.56
grandchild	999	0.05	1515	0.11	1645	0.11	1547	0.10	1465	0.12
health	964	4.01	1484	3.87	1594	3.90	1506	3.89	1447	3.91
low	999	0.18	1515	0.25	1645	0.23	1547	0.24	1465	0.24
preuniversity	999	0.09	1515	0.09	1645	0.10	1547	0.09	1465	0.10
vocational_med	999	0.21	1515	0.20	1645	0.22	1547	0.22	1465	0.22
vocational_high	999	0.32	1515	0.29	1645	0.29	1547	0.28	1465	0.28
university	999	0.20	1515	0.17	1645	0.16	1547	0.16	1465	0.16
riskaversion	837	5.15	1429	5.23	1552	5.23	1475	5.18	1416	5.15
patience	844	4.13	1451	4.14	1576	4.11	1498	4.11	1438	4.09
conscientiousness	825	4.39	1442	4.40	1575	4.39	1491	4.36	1441	4.40
locus_of_control	714	4.61	1253	4.55	1524	4.56	1467	4.52	1431	4.57
frequency	999	1.00	1515	1.49	1645	2.09	1547	2.79	1465	3.52
N	999		1515		1645		1547		1465	

Notes: The table shows summary statistics for respondents who reported information preferences for AOW reform, of which a subset also was asked about their expected retirement age. In 2003 respondents were not yet asked about their preferences for AOW reform, only the expected retirement age. Given not all respondents respond to this question, summary statistics of 2003 and 2004-2013 are not comparable.

	2008		2009		2010		2011		2012		2013	
	N	mean	N	mean	N	mean	N	mean	N	mean	N	mean
	1359	0.26	1366	0.33	1183	0.38	1187	0.43	1224	0.44	1232	0.27
	830	63.53	864	63.80	727	64.18	638	64.57	710	65.03	757	65.57
	1359	46.92	1366	47.60	1183	49.22	1187	49.83	1224	49.62	1232	45.02
	1359	0.66	1366	0.66	1183	0.65	1187	0.63	1224	0.65	1232	0.73
	1359	0.06	1366	0.07	1183	0.08	1187	0.09	1224	0.08	1232	0.07
	1359	0.10	1366	0.09	1183	0.08	1187	0.09	1224	0.08	1232	0.07
	1359	0.14	1366	0.14	1183	0.14	1187	0.16	1224	0.14	1232	0.09
	1359	0.03	1366	0.04	1183	0.05	1187	0.03	1224	0.04	1232	0.04
	1322	31.10	1305	32.71	1125	34.91	1154	34.82	1158	35.30	1125	35.82
	1333	32.01	1314	33.54	1141	38.73	1157	39.41	1187	35.77	1142	28.04
	1359	0.48	1366	0.46	1183	0.45	1187	0.47	1224	0.47	1232	0.49
	1359	0.67	1366	0.66	1183	0.65	1187	0.65	1224	0.65	1232	0.58
	1359	0.57	1366	0.57	1183	0.54	1187	0.54	1224	0.54	1232	0.47
	1359	0.12	1366	0.15	1183	0.17	1187	0.18	1224	0.16	1232	0.12
	1341	3.91	1328	3.89	1151	3.91	1171	3.90	1187	3.88	1149	3.92
	1359	0.25	1366	0.23	1183	0.24	1187	0.23	1224	0.22	1232	0.16
	1359	0.09	1366	0.09	1183	0.10	1187	0.09	1224	0.10	1232	0.08
	1359	0.21	1366	0.21	1183	0.19	1187	0.20	1224	0.21	1232	0.21
	1359	0.28	1366	0.28	1183	0.29	1187	0.28	1224	0.29	1232	0.33
	1359	0.17	1366	0.18	1183	0.18	1187	0.18	1224	0.18	1232	0.22
	1312	5.25	1286	5.31	1144	5.33	1148	5.33	1168	5.30	1133	5.28
	1297	4.11	1299	4.09	1163	4.13	1165	4.15	1189	4.16	1169	4.18
	1319	4.41	1302	4.40	1163	4.46	1169	4.49	1189	4.48	1169	4.45
	1288	4.52	1285	4.55	1103	4.56	1159	4.57	1120	4.56	1159	4.53
	1359	4.07	1366	4.50	1183	4.58	1187	5.05	1224	5.08	1232	4.97
	1359		1366		1183		1187		1224		1232	

Table A.2.3 Statements used to measure personality traits

Risk aversion

I think it is more important to have safe investments and guaranteed returns, than to take a risk to have a chance to get the highest possible returns

I would never consider investments in shares because I find this too risky

If I think an investment will be profitable, I am prepared to borrow money to make this investment *(reverse)*

I want to be certain that my investments are safe

I get more and more convinced that I should take greater financial risks to improve my financial position *(reverse)*

I am prepared to take the risk to lose money, when there is also a chance to gain money *(reverse)*

Patience

I think about how things can change in the future, and try to influence those things in my everyday life

I often work on things that will only pay off in a couple of years

I am only concerned about the present, because I trust that things will work themselves out in the future *(reverse)*

With everything I do, I am only concerned about the immediate consequences (say a period of a couple of days or weeks) *(reverse)*

Whether something is convenient for me or not, to a large extent determines the decisions that I take or the actions that I undertake *(reverse)*

I am ready to sacrifice my well-being in the present to achieve certain results in the future

I think it is important to take warnings about negative consequences of my acts seriously, even if these negative consequences would only occur in the distant future

I think it is more important to work on things that have important consequences in the future, than to work on things that have immediate but less important consequences

In general, I ignore warnings about future problems because I think these problems will be solved before they get critical *(reverse)*

I think there is no need to sacrifice things now for problems that lie in the future, because it will always be possible to solve these future problems later *(reverse)*

I only respond to urgent problems, trusting that problems that come up later can be solved in a later stage *(reverse)*

I get clear results in my daily work, this is more important to me than getting vague results

Conscientiousness

I do chores right away

I'll leave my things lying around *(reverse)*

I live my life according to schedules

I neglect my obligations *(reverse)*

I have an eye for details

I am accurate in my work

I forget to put things back where they belong *(reverse)*

I am always well prepared

I often make a mess of things *(reverse)*

I like order.

Locus of control

Saving and careful investing is a key factor in becoming rich

Whether or not I get to become wealthy depends mostly on my ability

In the long run, people who take very good care of their finances stay wealthy

If I become poor, it's usually my own fault

I am usually able to protect my personal interests

When I get what I want, it's usually because I worked hard for it

My life is determined by my own actions

There is little one can do to prevent poverty *(reverse)*

Becoming rich has nothing to do with luck

Regarding money, there isn't much you can do for yourself when you are poor *(reverse)*

It's not always wise for me to save because many things turn out to be a matter of good or bad fortune *(reverse)*

It is chiefly a matter of fate whether I become rich or poor *(reverse)*

Only those who inherit or win money can possible become rich *(reverse)*

Note: The table lists the statements for which respondents need to rate to what extent they agree with them or not (on a 1 to 5 or 1 to 7 scale). By averaging the answers (reversing the reverse-coded ones), we construct our composite measures of risk aversion, patience, conscientiousness and locus of control.

Table A.2.4a Support for pension reform - stability of coefficients

	(1)		(2)		(3)		(4)		(5)		(6)	
below35	0.00	(0.02)	0.00	(0.02)	0.01	(0.02)	0.00	(0.02)	-0.01	(0.02)	-0.01	(0.02)
age45to55	-0.08***	(0.02)	-0.08***	(0.02)	-0.08***	(0.02)	-0.08***	(0.02)	-0.07***	(0.02)	-0.06***	(0.02)
age55to65	-0.01	(0.02)	-0.03	(0.02)	-0.04*	(0.02)	-0.04**	(0.02)	-0.03	(0.02)	-0.02	(0.02)
self_employed			0.13***	(0.03)	0.13***	(0.03)	0.13***	(0.03)	0.11**	(0.03)	0.10***	(0.03)
household			0.00	(0.02)	0.02	(0.03)	0.04	(0.03)	0.04	(0.03)	0.04	(0.03)
benefits			0.09***	(0.02)	0.10***	(0.02)	0.11***	(0.02)	0.11**	(0.02)	0.11**	(0.02)
other			0.02	(0.03)	0.03	(0.03)	0.03	(0.03)	0.02	(0.03)	0.02	(0.03)
incomeQ2					-0.00	(0.02)	-0.01	(0.02)	-0.01	(0.02)	-0.01	(0.02)
incomeQ3					0.02	(0.02)	0.01	(0.02)	-0.01	(0.02)	-0.01	(0.02)
incomeQ4					0.05*	(0.03)	0.04	(0.03)	-0.00	(0.03)	-0.02	(0.03)
wealthQ2					-0.01	(0.02)	-0.02	(0.02)	-0.02	(0.02)	-0.02	(0.02)
wealthQ3					-0.00	(0.02)	-0.00	(0.02)	-0.01	(0.02)	-0.02	(0.02)
wealthQ4					0.01	(0.02)	0.00	(0.02)	-0.02	(0.02)	-0.03	(0.02)
female							-0.02	(0.01)	-0.03*	(0.01)	-0.02	(0.01)
married							-0.05***	(0.02)	-0.04**	(0.02)	-0.03**	(0.02)
grandchild							0.03	(0.02)	0.04*	(0.02)	0.04*	(0.02)
health							0.02***	(0.01)	0.02***	(0.01)	0.02***	(0.01)
preuniversity									0.00	(0.03)	-0.00	(0.03)
vocational_med									-0.01	(0.02)	-0.01	(0.02)
vocational_high									0.03	(0.02)	0.02	(0.02)
university									0.12**	(0.02)	0.11**	(0.02)
riskaversion											-0.02**	(0.01)
patience											0.02**	(0.01)
conscientiousness											-0.01*	(0.01)
locus_of_control											0.02***	(0.01)
N	12176		12176		12176		12176		12176		12176	
Pseudo R²	0.033		0.040		0.041		0.045		0.052		0.056	
Time dummies	Yes		Yes		Yes		Yes		Yes		Yes	

Notes: Various sets of covariates (age, occupational status, income/wealth, demographics, education and personality traits) are added one by one. Column 6 is the full model and corresponds to column 2 in Table 2.2. Covariates are highlighted grey when coefficients are significant throughout. In parentheses are standard errors. The symbols *, **, and *** denote significance at the 10, 5 and 1 percent confidence level, respectively.

Table A.2.4b Support for pension reform - stability of coefficients (reverse order)

	(1)	(2)	(3)	(4)	(5)	(6)
riskaversion	-0.02*** (0.00)	-0.02*** (0.01)	-0.02*** (0.01)	-0.02*** (0.01)	-0.02*** (0.01)	-0.02*** (0.01)
patience	0.02*** (0.00)	0.01** (0.01)	0.02** (0.01)	0.02** (0.01)	0.02** (0.01)	0.02** (0.01)
conscientiousness	-0.02*** (0.00)	-0.01** (0.01)	-0.01** (0.01)	-0.01* (0.01)	-0.01** (0.01)	-0.01** (0.01)
locus_of_control	0.03*** (0.00)	0.02*** (0.01)	0.02*** (0.01)	0.02*** (0.01)	0.02*** (0.01)	0.02*** (0.01)
preuniversity		-0.01 (0.03)	-0.00 (0.03)	0.00 (0.03)	0.00 (0.03)	-0.00 (0.03)
vocational_med		-0.02 (0.02)	-0.02 (0.02)	-0.01 (0.02)	-0.01 (0.02)	-0.01 (0.02)
vocational_high		0.01 (0.02)	0.01 (0.02)	0.03 (0.02)	0.03 (0.02)	0.02 (0.02)
university		0.10*** (0.02)	0.10*** (0.02)	0.12*** (0.02)	0.12*** (0.02)	0.11*** (0.02)
female			-0.01 (0.01)	-0.02 (0.01)	-0.01 (0.01)	-0.02 (0.01)
married			-0.04** (0.02)	-0.04** (0.02)	-0.04** (0.02)	-0.03** (0.02)
grandchild			0.07*** (0.02)	0.07*** (0.02)	0.04** (0.02)	0.04* (0.02)
health			0.01* (0.01)	0.01* (0.01)	0.02*** (0.01)	0.02*** (0.01)
incomeQ2				-0.01 (0.02)	-0.01 (0.02)	-0.01 (0.02)
incomeQ3				-0.04** (0.02)	-0.02 (0.02)	-0.01 (0.02)
incomeQ4				-0.04* (0.03)	-0.02 (0.03)	-0.02 (0.03)
wealthQ2				-0.02 (0.02)	-0.02 (0.02)	-0.02 (0.02)
wealthQ3				-0.02 (0.02)	-0.02 (0.02)	-0.02 (0.02)
wealthQ4				-0.03 (0.02)	-0.03* (0.02)	-0.03 (0.02)
self_employed					0.10*** (0.03)	0.10*** (0.03)
household					0.04 (0.03)	0.04 (0.03)
benefits					0.11*** (0.02)	0.11*** (0.02)
other					0.02 (0.03)	0.02 (0.03)
below35						-0.01 (0.02)
age45to55						-0.06*** (0.02)
age55to65						-0.02 (0.02)
N	12176	12176	12176	12176	12176	12176
Pseudo R²	0.038	0.044	0.047	0.048	0.054	0.056
Time dummies	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Various sets of covariates (age, occupational status, income/wealth, demographics, education and personality traits) are added one by one. Column 6 is the full model and corresponds to column 2 in Table 2.2. Covariates are highlighted green when coefficients are significant throughout and blue when only significant in some specifications. In parentheses are standard errors. The symbols *, ** and *** denote significance at the 10, 5 and 1 percent confidence level, respectively.

Table A.2.5 Impact of frequency of participation

	Expected retirement age	Support for raising the statutory retirement age
	OLS	conditional logit
	FE	FE
frequency	-0.08	0.09
	(0.08)	(0.06)
N	8717	6881
(Pseudo) R^2	0.095	0.079
Time dummies	Yes	Yes

Notes: The table shows the results of a panel fixed effects regression of respondents' expected age of retirement and a conditional logit fixed effects model for respondents' support for raising the retirement age, focusing on coefficients for the frequency that respondents answered the question on preferences for reform (from 1st time to at maximum the 11th time). In parentheses are standard errors. The symbols *, ** and *** denote significance at the 10, 5 and 1 percent confidence level, respectively.

Table A.2.6 Regression results with and without 2013

| | Expected retirement age | | Increase in the retirement age first reform preference | |
	All years	Without 2013	All years	Without 2013
below35	-0.48** (0.23)	-0.56** (0.24)	-0.01 (0.02)	-0.02 (0.02)
age45to55	-0.19 (0.16)	-0.20 (0.16)	-0.06*** (0.02)	-0.07*** (0.02)
age55to65	-0.21 (0.18)	-0.21 (0.19)	-0.02 (0.02)	-0.04 (0.02)
self_employed	0.35 (0.37)	0.17 (0.37)	0.10*** (0.03)	0.10*** (0.03)
household			0.04 (0.03)	0.04 (0.03)
benefits			0.11*** (0.02)	0.12*** (0.02)
other	-0.52 (0.37)	-0.52 (0.39)	0.02 (0.03)	0.02 (0.03)
incomeQ2	0.16 (0.37)	0.19 (0.39)	-0.01 (0.02)	-0.00 (0.02)
incomeQ3	0.18 (0.37)	0.17 (0.39)	-0.01 (0.02)	-0.01 (0.02)
incomeQ4	-0.36 (0.39)	-0.40 (0.41)	-0.02 (0.03)	-0.01 (0.03)
wealthQ2	-0.15 (0.19)	-0.16 (0.20)	-0.02 (0.02)	-0.01 (0.02)
wealthQ3	-0.41** (0.19)	-0.47** (0.20)	-0.02 (0.02)	-0.01 (0.02)
wealthQ4	-0.77*** (0.19)	-0.80*** (0.20)	-0.03 (0.02)	-0.03 (0.02)
female	-1.01*** (0.15)	-1.05*** (0.16)	-0.02 (0.01)	-0.01 (0.02)
married	-0.73*** (0.16)	-0.74*** (0.17)	-0.03** (0.02)	-0.03* (0.02)
grandchild	0.12 (0.23)	0.16 (0.23)	0.04* (0.02)	0.04* (0.02)
health	0.20*** (0.08)	0.20** (0.08)	0.02** (0.01)	0.02** (0.01)
preuniversity	0.24 (0.29)	0.26 (0.30)	-0.00 (0.03)	-0.00 (0.03)
vocational_med	-0.17 (0.22)	-0.15 (0.23)	-0.01 (0.02)	-0.01 (0.02)
vocational_high	0.57*** (0.22)	0.58*** (0.23)	0.02 (0.02)	0.02 (0.02)
university	0.78*** (0.25)	0.82*** (0.26)	0.11*** (0.02)	0.10*** (0.03)
riskaversion	0.12* (0.07)	0.12* (0.07)	-0.02*** (0.01)	-0.02*** (0.01)
patience	0.07 (0.08)	0.08 (0.09)	0.02** (0.01)	0.02** (0.01)
conscientiousness	-0.17*** (0.07)	-0.16** (0.07)	-0.01** (0.01)	-0.01* (0.01)
locus_of_control	-0.13 (0.08)	-0.13 (0.09)	0.02** (0.01)	0.02** (0.01)
_cons	63.45*** (0.49)	63.51*** (0.51)		
N	8103	7459	12176	11138
(Pseudo) R²	0.126	0.102	0.056	0.058
Time dummies	Yes	Yes	Yes	Yes

Notes: The table repeats regression of Table 2.2 (column 1 and 3) and compares these with the same regression without 2013 (columns 2 and 4). In parentheses are standard errors. Columns 3 and 4 report marginal effects. The symbols *, ** and *** denote significance at the 10, 5 and 1 percent confidence level, respectively.

Table A.2.7 Probit regression of all three PAYG reform margins

First preferred margin to adjust PAYG system:

	(1) Increase in retirement age		(2) Reduce level of benefits		(3) Increase pension contributions	
below35	-0.01	(0.02)	0.03**	(0.01)	-0.02	(0.02)
age45to55	-0.06***	(0.02)	-0.03*	(0.01)	0.09***	(0.02)
age55to65	-0.02	(0.02)	-0.10***	(0.02)	0.12***	(0.02)
self_employed	0.10***	(0.03)	-0.01	(0.02)	-0.10***	(0.03)
household	0.04	(0.03)	0.02	(0.02)	-0.07**	(0.03)
benefits	0.11***	(0.02)	-0.01	(0.02)	-0.11***	(0.02)
other	0.02	(0.03)	-0.01	(0.02)	-0.01	(0.03)
incomeQ2	-0.01	(0.02)	0.01	(0.02)	-0.01	(0.02)
incomeQ3	-0.01	(0.02)	0.00	(0.02)	0.01	(0.03)
incomeQ4	-0.02	(0.03)	0.03	(0.02)	-0.02	(0.03)
wealthQ2	-0.02	(0.02)	-0.02	(0.01)	0.03*	(0.02)
wealthQ3	-0.02	(0.02)	-0.00	(0.01)	0.02	(0.02)
wealthQ4	-0.03	(0.02)	0.02	(0.02)	0.01	(0.02)
female	-0.02	(0.01)	0.03***	(0.01)	-0.02	(0.02)
married	-0.03**	(0.02)	0.03**	(0.01)	0.00	(0.02)
grandchild	0.04*	(0.02)	-0.04**	(0.02)	-0.01	(0.02)
health	0.02***	(0.01)	0.01*	(0.01)	-0.03***	(0.01)
preuniversity	-0.00	(0.03)	-0.01	(0.02)	0.02	(0.03)
vocational_med	-0.01	(0.02)	0.01	(0.02)	0.01	(0.02)
vocational_high	0.02	(0.02)	0.00	(0.02)	-0.02	(0.02)
university	0.11***	(0.02)	0.01	(0.02)	-0.13***	(0.03)
riskaversion	-0.02***	(0.01)	-0.01***	(0.00)	0.03***	(0.01)
patience	0.02**	(0.01)	-0.01	(0.01)	-0.01	(0.01)
conscientiousness	-0.01**	(0.01)	0.00	(0.01)	0.01	(0.01)
locus_of_control	0.02***	(0.01)	0.00	(0.01)	-0.03***	(0.01)
Year dummies (reference year is 2004)						
2005	0.02	(0.01)	0.00	(0.01)	-0.03	(0.02)
2006	0.07***	(0.01)	-0.00	(0.01)	-0.07***	(0.02)
2007	0.11***	(0.01)	-0.04***	(0.01)	-0.07***	(0.02)
2008	0.09***	(0.02)	-0.00	(0.01)	-0.09***	(0.02)
2009	0.16***	(0.02)	0.01	(0.02)	-0.17***	(0.02)
2010	0.21***	(0.02)	0.04**	(0.02)	-0.25***	(0.02)
2011	0.25***	(0.02)	0.03*	(0.02)	-0.28***	(0.02)
2012	0.25***	(0.02)	-0.04**	(0.02)	-0.22***	(0.02)
2013	0.09***	(0.02)	0.03	(0.02)	-0.12***	(0.02)
N	12176		12176		12176	
Pseudo R²	0.056		0.030		0.053	

Notes: The table repeats regression of Table 2.2 (column 1) and compares this with similar probit regressions of the other two adjustment margin (i.e. a cut in the benefit level and an increase in pension contributions). In parentheses are standard errors. Columns 3 and 4 report marginal effects. The symbols *, ** and *** denote significance at the 10, 5 and 1 percent confidence level, respectively.
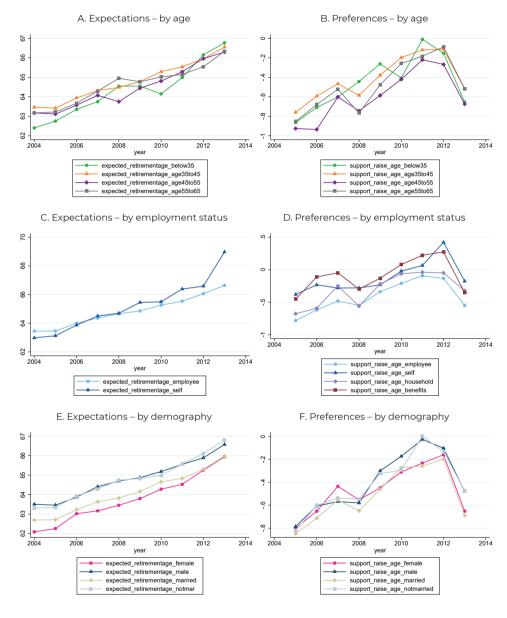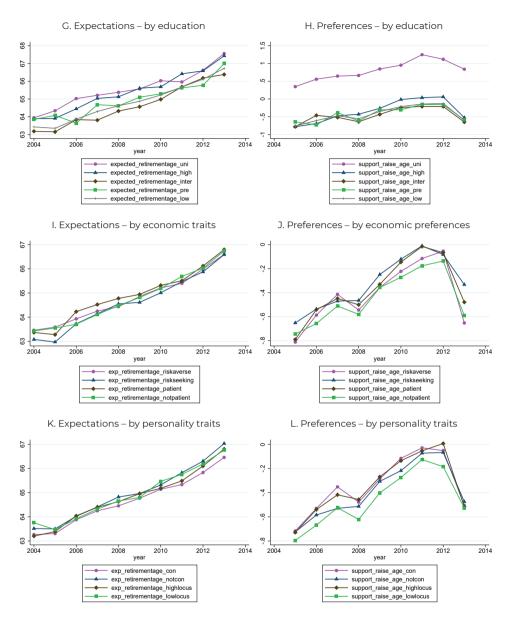
Figure A.2.1 Year effects of retirement expectations and preferences - by group

A. Expectations – by age

B. Preferences – by age

C. Expectations – by employment status

D. Preferences – by employment status

E. Expectations – by demography

F. Preferences – by demography

G. Expectations – by education

H. Preferences – by education

I. Expectations – by economic traits

J. Preferences – by economic preferences

K. Expectations – by personality traits

L. Preferences – by personality traits

Notes: The left panels shows year coefficients from the regression of the expected age of retirement (2003 is base year), right panels from the probit regression on preferences for AOW reform (2004 is the base year). Our reference respondent is aged 35 to 45, employee, male, low-educated and falls in the first income and wealth quarter.

Figure A.2.2 Year effects of retirement expectations and preferences – various methods

A. Expectations

B. Preferences

Notes: Panel (a) shows year coefficients from the regression of the expected age of retirement, panel (b) from the probit regression on preferences for AOW reform (2004 is the base year). The dark dots in both graphs are identical to those in Figure 2.3; the lighter dots show the time effects from the panel fixed effects regression (see Section 2.7). Confidence bands are suppressed to facilitated reading.

Appendix 3 to Chapter 3

Table A.3.1a Description of survey questions used
Number in brackets refers to source, which is shown in Table A.3.1b (overview sources)

Variable	Meaning	Source
Dependent variables		
support_ debtreduction, support_taxrelief, support_ morespending	'It is expected that economic growth with be sustained during the coming years. With the currently planned taxes and expenditures, the government is expected to run a surplus on its budget, while the government debt (measured in euros and as percentage of gross domestic product) is expected to fall. To what extent do you agree with the following statements?' a. It would be a good idea if the government does not adjust expenditures and tax rates, and lets the government debt fall. (*support_debtdown*) b. It would be a good idea if the government lowers tax rates, without adjusting expenditures. (*support_taxrelief*) c. It would be a good idea if the government spends more money on collective services, such as education and health care, without raising tax rates. (*support_morespending*) *Response on a 4-point scale from 1 (not at all) to 4 (strongly agree).* *To elicit true preferences, we correct the scales for the total score awarded, and transform the answers to a 0 to 10 scale.*	[1]
Main covariates		
populism	*Following Akkerman et al. (2014) we present DHS respondents with 6 statements.* 'To what extent do you disagree or agree with the following statements on the role of politicians': 1. The politicians in the Dutch parliament need to follow the will of the people; 2. The people, and not politicians, should make our most important policy decisions; 3. The political differences between the elite and the people are larger than the differences among the people; 4. I would rather be represented by a citizen than by a specialized politician; 5. Elected officials talk too much and take too little action; 6. What people call 'compromise' in politics is really just selling out on one's principles. *All response categories range from 1 (strongly disagree) to 5 (strongly agree). For our variable 'populism' we take the mean value of the six statements, so our variable runs from 1 to 5.*	[2]

Variable	Meaning	Source
prob_literacy	*Following the work of Hudomiet et al. (2018), the 2017 DHS questionnaire includes 4 questions to measure to what extent respondents can account and plan for uncertainty, such as the events like job loss.* 1. Consider that you take one ball from a bowl that holds 10 balls without looking. The bowl has 10 white balls and no red balls. What is the percent chance that the ball you take is red? 2. Now suppose you take one ball from a bowl that holds 10 balls without looking. The bowl has 7 white balls and 3 red balls. What is the percent chance that the ball you take is white? 3. Assume that the weather report accurately reports the chance of rain. Suppose the weather report tells you that the chance it will rain tomorrow is 70%. What is the chance it will NOT rain tomorrow? 4. Suppose that whether it rains in your town and whether it rains in Paris are unrelated. The chance that it will rain in your town tomorrow is 50%. The chance that it will rain in Paris is also 50%. What is the chance that it will rain both in your town and in Paris tomorrow? *Respondents are asked to answer the questions on a scale from 0 to 100, where 0 means a zero chance of outcome mentioned, and 100 means absolute certainty of this outcome. We reward respondents with 1 if they select the right answer, and 0 if not, resulting in an index ranging from 0 to 4.*	[3]
debt_experiment	*In our randomized survey experiment half of the respondents were presented with an educative message on public debt dynamics, whereas the other half was not. In our treatment group (debt_experiment=1), respondents were presented with the text below, whereas respondents in our control group (debt_experiment=0) were not.* 'In case in one year a government spends more money than she receives by taxation, the government runs a budget deficit. To finance this, the government must borrow money. Due to this, the total debt of the government ('government debt') will increase. The government cannot let government debt rise endlessly. If the government keeps on borrowing, eventually she will have to raise taxes or cut expenditures so as to stop government debt from increasing.'	[1]
Controls		
age	Approximate age of respondent (year survey minus year of birth)	[5]
below35, etc. (D)	Respondent is aged below 35, etc.	
female (D)	Female	
child (D)	One or more children in the household	
low (D)	Special, primary or lower vocational education	
vocational_med (D)	Intermediate vocational training (MBO)	

Variable	Meaning	Source
preuniversity (D)	Pre-university education (HAVO/VWO)	
vocational_high (D)	Vocational colleges (HBO)	
university (D)	University	
grossincome	Gross income at personal level (if respondent doesn't know one's income, we set the gross income at 0)	
incomeQ1 (D), etc.	Dummy variables for four gross income quartiles	
hard_to_getby	'How well can you manage on the total income of your household?' *Respondents category ranges from 1 ('it is very hard') to 5 ('it is very easy'). We reverse-coded the variable so that a high score indicates difficulties in getting by.*	[3]
riskaversion, patience, locus_of_control*	For statements used, see Table A.2.3 in Chapter 2.	
openness, conscientiousness, extraversion, agreeableness, neuroticism	So-called 'Big Five' personality traits. For statements used, see Table A.3.2	
right_wing	'In politics sometimes is spoken about right and left wing. When you think about your own political views, where would you place yourself on a scale from 0 to 10, where 0 stands for left and 10 for right?' *(0=extreme left, 10=extreme right)*	[2]
equality	'To what extent do you disagree or agree with the following statement[...]': The government should take measures to reduce income differentials. *Response categories range from 1 ('strongly disagree') to 5 ('strongly agree').*	
Auxiliary variables		
trust_politics	'How much trust do you have in national politics?' *Respondents are asked to answer the questions on a scale from 1 (a lot of trust) to 4 (no trust at all). We reversed the scaling so that 1 represents low trust and 4 represents high trust.*	[4]
integrity_finance	'To what extent do you agree with the following statement: Overall, the managers of financial institutions are knowledgeable and trustworthy.' * *Respondents are asked to answer the questions on a scale from 1 (completely agree) to 5 (completely disagree). We reversed the scaling so that 1 represents low trustworthiness and 4 represents high trustworthiness. Respondents could also indicate they did not know; these observations are reported missing.* * As of 2010, respondents are asked about knowledge and trustworthiness separately. For these years, the average of the two values is taken.	

Variable	Meaning	Source
eu_cooperation	'To what extent do you disagree or agree with the following statement[…]': European cooperation should be strengthened. *Response categories range from 1 ('strongly disagree') to 5 ('strongly agree').*	[2]

D = dummy variable

Table A.3.1b Sources and survey dates of DHS variables

No.	Name	Date presented to respondents
1	DHS special survey on fiscal policy	September 2017
2	DHS special survey on populism	June 2017
3	DHS 2017 psychological module	March 2017
4	DHS trust survey	Each year starting from 2006 in February or March
5	DHS administrative information	Continuous: at each participation in the survey, respondents are presented their demographic information and are asked to update any new information.

Table A.3.2 Statements to measure the 'Big Five' personality traits

Openness
I have excellent ideas*
I have a vivid imagination*
I am full of ideas
I have a rich vocabulary
I have difficulty understanding abstract ideas (*reverse*)
I am not interested in abstract ideas (*reverse*)
I am quick to understand things
I do not have a good imagination (*reverse*)
I use difficult words
I spend time reflecting on things

Conscientiousness
I like order*
I'll leave my things lying around (*reverse*)*
I do chores right away
I live my life according to schedules
I neglect my obligations (*reverse*)
I pay attention to details
I am accurate in my work
I forget to put things back where they belong (*reverse*)
I am always well prepared
I often make a mess of things (*reverse*)

Extraversion
I keep in the background (*reverse*) *
I am quiet around strangers (*reverse*) *
I am the life of the party
I do not talk a lot (*reverse*)
I feel comfortable around people
I start conversations
I have little to say (*reverse*)
I talk to a lot of different people at parties
I do not mind being the centre of attention
I do not like to draw attention to myself (*reverse*)

Agreeableness
I sympathize with others' feelings*
I take time out for others*
I feel little concern for others (*reverse*)
I am interested in people
I insult people (*reverse*)
I am not interested in other people's problems (*reverse*)
I have a soft heart

I am not really interested in others (*reverse*)

I feel others' emotions

I make people feel at ease

Neuroticism

I have frequent mood swings*

I get stressed out easily*

I seldom feel blue (reverse)

I am relaxed most of the time (reverse)

I worry about things

I am easily disturbed

I get upset easily

I change my mood a lot

I get irritated easily

I often feel blue

Notes: Table lists the statements for which respondents need to rate to what extent they agree with them or not (on a 1 to 5 scale). By averaging the answers (reversing the reverse-coded ones), we construct our composite measures.

* Statements used in short version (2016, 2018, 2019).

Table A.3.3 Pairwise correlations between regressors

Variables	populism	prob_literacy	pf_experiment	below35	age35to45	age45to55	age55to65	age65plus	female	child	low	vocational_med	preuniversity	vocational_high	university
populism	1.00														
prob_literacy	-0.31	1.00													
pf_experiment	-0.02	0.01	1.00												
below35	-0.11	0.12	-0.01	1.00											
age35to45	-0.10	0.08	-0.01	-0.16	1.00										
age45to55	-0.04	0.04	-0.02	-0.18	-0.19	1.00									
age55to65	0.06	-0.00	-0.00	-0.21	-0.22	-0.23	1.00								
age65plus	0.13	-0.18	0.03	-0.28	-0.30	-0.32	-0.37	1.00							
female	0.03	-0.10	-0.03	0.10	0.06	-0.00	-0.01	-0.11	1.00						
child	-0.08	0.07	-0.01	0.11	0.28	0.25	-0.09	-0.40	0.06	1.00					
low	0.28	-0.25	0.03	-0.12	-0.16	-0.08	0.06	0.22	0.03	-0.11	1.00				
vocational_med	0.11	-0.11	-0.02	0.04	0.07	0.13	0.01	-0.20	0.00	0.05	-0.34	1.00			
preuniversity	-0.02	0.03	-0.02	0.03	-0.04	-0.02	0.00	0.02	0.03	0.00	-0.21	-0.19	1.00		
vocational_high	-0.15	0.11	-0.01	-0.02	0.08	-0.02	-0.02	-0.01	-0.01	0.06	-0.35	-0.32	-0.20	1.00	
university	-0.30	0.29	0.01	0.10	0.06	-0.01	-0.06	-0.05	-0.06	0.01	-0.24	-0.22	-0.13	-0.22	1.00
incomeQ1	0.10	-0.07	0.03	0.14	-0.08	-0.00	0.02	-0.06	0.28	0.05	0.15	-0.01	0.07	-0.12	-0.09
incomeQ2	0.13	-0.12	-0.02	-0.01	-0.03	-0.03	-0.04	0.08	0.13	-0.08	0.10	0.09	0.02	-0.11	-0.13
incomeQ3	-0.00	-0.03	-0.02	-0.03	0.02	-0.03	-0.00	0.04	-0.11	-0.04	-0.04	0.04	-0.03	0.08	-0.06
incomeQ4	-0.23	0.21	0.01	-0.10	0.09	0.06	0.02	-0.06	-0.30	0.07	-0.21	-0.13	-0.06	0.16	0.28
hard_to_getby	0.23	-0.16	-0.03	-0.00	-0.03	0.03	0.03	-0.03	0.08	0.03	0.12	0.11	0.01	-0.12	-0.15
riskaversion	0.09	-0.06	0.01	-0.09	-0.02	-0.07	0.02	0.11	0.20	-0.06	0.08	0.00	0.02	-0.02	-0.10
patience	-0.20	0.19	0.01	0.11	0.07	0.02	-0.03	-0.12	-0.05	0.06	-0.16	-0.07	-0.04	0.09	0.22
locus_of_control	-0.18	0.18	0.02	0.12	0.06	0.02	-0.06	-0.09	-0.10	0.02	-0.15	-0.03	0.02	0.08	0.12
openness	-0.10	0.13	0.02	0.07	0.07	0.05	-0.04	-0.10	-0.04	0.06	-0.18	-0.03	0.00	0.10	0.16
conscientiousness	0.06	-0.06	0.02	-0.06	-0.03	0.00	0.02	0.04	0.06	-0.03	0.04	0.00	-0.01	-0.01	-0.04
extraversion	-0.02	-0.06	0.01	0.03	-0.02	0.00	-0.01	0.01	0.01	0.03	-0.01	-0.02	-0.00	0.02	0.03
agreeableness	0.03	-0.01	-0.01	-0.02	-0.02	-0.01	0.03	0.02	0.26	-0.02	-0.03	-0.01	-0.01	0.03	0.01
neuroticism	-0.02	0.01	-0.00	0.17	0.12	0.02	-0.06	-0.17	0.13	0.07	-0.02	0.03	-0.01	-0.02	0.02
rightwing	0.17	-0.08	-0.01	0.02	0.03	0.03	-0.05	-0.02	-0.11	0.05	0.07	0.05	0.03	-0.06	-0.10
equality	0.16	-0.11	-0.01	-0.06	-0.04	-0.06	0.04	0.08	0.09	-0.08	0.06	0.05	-0.04	-0.05	-0.04
trust_politics_pc	-0.36	0.05	0.00	0.01	0.10	0.06	-0.06	-0.05	0.01	0.12	-0.12	-0.02	0.02	0.09	0.05
integrity_finance_pc	-0.21	0.04	0.04	-0.00	0.02	-0.02	-0.02	0.03	-0.00	-0.02	-0.07	0.02	0.06	-0.00	0.01
eu_cooperation	-0.42	0.11	-0.00	0.03	-0.01	-0.02	-0.07	0.07	-0.01	-0.05	-0.11	-0.12	0.00	0.11	0.16

Notes: The table shows pairwise correlations between all our regressors. We highlight those correlations that exceed 0.40.

	incomeQ1	incomeQ2	incomeQ3	incomeQ4	hard_to_getby	riskaversion	patience	locus_of_control	openness	conscientiousness	extraversion	agreeableness	neuroticism	rightwing	equality	trust_politics_pc	integrity_finance_pc	eu_cooperation
	1.00																	
	-0.33	1.00																
	-0.34	-0.33	1.00															
	-0.34	-0.33	-0.33	1.00														
	0.14	0.21	-0.06	-0.30	1.00													
	0.07	0.05	0.04	-0.15	0.01	1.00												
	-0.07	-0.09	-0.01	0.16	-0.09	-0.03	1.00											
	-0.07	-0.13	0.03	0.17	-0.29	-0.02	0.20	1.00										
	-0.03	-0.04	-0.04	0.11	-0.04	-0.03	0.16	0.21	1.00									
	0.01	-0.02	-0.00	0.01	-0.09	0.12	0.11	0.17	0.05	1.00								
	-0.02	-0.01	-0.01	0.04	-0.04	-0.04	0.02	0.08	0.25	0.05	1.00							
	0.05	0.05	-0.02	-0.07	-0.02	0.17	0.08	0.12	0.26	0.22	0.23	1.00						
	0.07	0.04	-0.01	-0.11	0.15	-0.02	0.05	-0.15	-0.07	-0.21	-0.19	-0.15	1.00					
	0.00	-0.07	0.01	0.07	-0.03	-0.08	-0.01	0.12	-0.07	0.10	0.05	-0.12	-0.05	1.00				
	0.07	0.17	-0.01	-0.22	0.16	0.12	-0.05	-0.23	-0.02	-0.05	-0.05	0.12	0.06	-0.42	1.00			
	-0.05	-0.04	0.02	0.06	-0.13	-0.04	0.10	0.13	0.01	0.03	0.07	0.06	-0.05	-0.04	-0.05	1.00		
	-0.04	-0.05	0.01	0.08	-0.11	0.01	0.04	0.17	0.00	0.08	0.02	0.08	-0.10	-0.01	-0.05	0.36	1.00	
	-0.04	-0.04	-0.02	0.10	-0.11	-0.08	0.10	0.11	0.10	-0.01	0.04	0.08	-0.05	-0.23	0.13	0.22	0.15	1.00

Table A.3.4 Correlation between dependent variables and instruments

Extent to which respondents would be in favor of using foreseen tax windfalls for the following purposes exclusively:

	Debt reduction					Tax relief				More spending		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
trust_politics_pc	0.51***	0.39***			-0.24**	-0.16*			-0.27***	-0.23**		0.02
integrity_finance_pc			0.18	0.07			-0.16	-0.09			-0.03	
prob_literacy		0.12*		0.12*		-0.14**		-0.14**		0.02		0.02
debt_experiment		0.16		0.17		0.03		0.03		-0.19*		-0.19*
age35to45		0.29		0.28		-0.15		-0.13		-0.14		-0.15
age45to55		0.09		0.08		-0.10		-0.09		0.01		0.01
age55to65		-0.03		-0.09		0.00		0.03		0.03		0.06
age65plus		-0.18		-0.20		0.01		0.03		0.17		0.17
female		-0.36***		-0.35**		0.12		0.12		0.24*		0.24*
child		-0.44**		-0.37**		0.46***		0.43**		-0.01		-0.06
vocational_med		0.02		0.06		-0.12		-0.13		0.10		0.07
preuniversity		0.46*		0.53**		-0.39*		-0.40*		-0.07		-0.13
vocational_high		0.42**		0.48**		-0.54***		-0.56***		0.12		0.08
university		0.61*		0.68**		-0.68***		-0.71***		0.07		0.03
incomeQ2		0.17		0.18		0.02		0.01		-0.19		-0.19
incomeQ3		-0.02		-0.01		0.06		0.06		-0.04		-0.04
incomeQ4		0.23		0.21		0.06		0.07		-0.28		-0.27
hard_to_getby		-0.13		-0.16*		0.06		0.08		0.07		0.08
riskaversion		0.06		0.05		-0.09		-0.09		0.03		0.04
patience		0.02		0.03		-0.06		-0.06		0.04		0.03
locus_of_control		0.17**		0.16**		-0.05		-0.04		-0.12*		-0.12*
openness		0.06		0.04		0.08		0.08		-0.14**		-0.13**
conscientiousness		-0.24***		-0.25***		0.24***		0.24***		0.00		0.01
extraversion		0.03		0.04		-0.07		-0.08		0.04		0.03

Table A.3.4 Continued

	Debt reduction					Tax relief				More spending		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
agreeableness		0.08		0.09		-0.19**		-0.20***		0.12*		0.11
neuroticism		-0.05		-0.07		0.09		0.09		-0.03		-0.02
rightwing		0.03		0.03		0.02		0.02		-0.05		-0.05
equality		-0.16*		-0.16*		-0.04		-0.03		0.19***		0.20***
_cons	3.23***	3.82***	3.74***	4.51***	3.42***	3.55***	3.43***	3.47***	5.35***	4.63***	4.84***	4.02***
N	610	610	610	610	610	610	610	610	610	610	610	610
R^2	0.032	0.183	0.005	0.167	0.010	0.122	0.005	0.120	0.014	0.143	0.000	0.134

Notes: The tables reports results of OLS regression of fiscal preferences and pre-crisis trust in national politics and pre-crisis trust in the integrity of financial sector management, both without and with controls. Standard errors (which were clustered at the household level) are suppressed. The symbols *, ** and *** denote significance at the 10, 5 and 1 percent confidence level, respectively.

Table A.3.5 Regression of fiscal preferences - including attitudes towards EU

Extent to which respondents would be in favor of using foreseen tax windfalls for the following purposes exclusively:

	Debt reduction		Tax relief		More spending	
	(1)	(2)	(3)	(4)	(5)	(6)
populism	-0.46***	-0.42***	0.30***	0.25***	0.16***	0.17***
prob_literacy	0.20***	0.21***	-0.17***	-0.17***	-0.04	-0.04
debt_experiment	0.28***	0.28***	-0.11*	-0.11*	-0.17***	-0.17***
age35to45	0.04	0.04	-0.20	-0.20	0.16	0.16
age45to55	0.08	0.08	-0.19	-0.19	0.11	0.11
age55to65	0.00	0.00	-0.14	-0.14	0.14	0.14
age65plus	-0.04	-0.06	-0.23*	-0.21	0.27**	0.27**
female	-0.27***	-0.26***	0.07	0.07	0.20**	0.20**
child	-0.24**	-0.24**	0.16*	0.16*	0.08	0.08
vocational_med	-0.10	-0.09	0.01	0.00	0.09	0.09
preuniversity	0.22	0.22	-0.26**	-0.25**	0.04	0.04
vocational_high	0.07	0.06	-0.20*	-0.20*	0.14	0.13
university	0.35**	0.34**	-0.38***	-0.37***	0.03	0.03
incomeQ2	-0.01	-0.00	0.12	0.11	-0.11	-0.11
incomeQ3	-0.10	-0.09	0.12	0.11	-0.02	-0.02
incomeQ4	-0.04	-0.04	0.11	0.11	-0.07	-0.07
hard_to_getby	-0.09*	-0.08	0.04	0.04	0.04	0.04
riskaversion	0.03	0.04	-0.09**	-0.10**	0.06*	0.06*
patience	0.10**	0.10**	-0.06*	-0.06*	-0.04	-0.04
locus_of_control	0.09*	0.09*	-0.03	-0.03	-0.06	-0.06
openness	0.03	0.03	0.04	0.04	-0.07*	-0.07*
conscientiousness	-0.06	-0.06	0.11***	0.11***	-0.05	-0.05
extraversion	-0.02	-0.02	0.05	0.05	-0.03	-0.03
agreeableness	-0.03	-0.03	-0.13***	-0.13***	0.16***	0.16***
neuroticism	-0.04	-0.03	0.05	0.05	-0.02	-0.02
rightwing	0.07***	0.08***	0.00	0.00	-0.08***	-0.08***
equality	-0.04	-0.05	-0.10**	-0.09**	0.14***	0.14***
eu_cooperation		0.07		-0.07*		0.00
_cons	5.36***	5.04***	2.77***	3.11***	3.87***	3.85***
N	1636	1636	1636	1636	1636	1636
R²	0.165	0.166	0.112	0.114	0.116	0.116

Notes: The table repeats the regression of preferences towards debt reduction in Table 3.2 (full model, column 3, 6 and 9), but now including attitudes towards the EU. Standard errors (which were clustered at the household level) are suppressed. The symbols *, ** and *** denote significance at the 10, 5 and 1 percent confidence level, respectively.

Table A.3.6 Regression of preferences towards debt - larger sample

Sample	September 2017 only		June 2017 and September 2017	
	Baseline sample	**Larger sample**	**Baseline sample**	**Larger sample**
	(1)	**(2)**	**(3)**	**(4)**
age35to45	-0.07	0.05	-0.01	-0.06
age45to55	-0.05	-0.01	0.02	-0.07
age55to65	-0.30**	-0.24**	-0.12	-0.16
age65plus	-0.44***	-0.42***	-0.23	-0.31**
female	-0.42***	-0.41***	-0.34***	-0.36***
child	-0.29***	-0.27***	-0.27***	-0.22**
vocational_med	-0.05	-0.05	-0.08	-0.07
preuniversity	0.50***	0.42***	0.31*	0.34**
vocational_high	0.47***	0.42***	0.22*	0.22*
university	1.02***	0.97***	0.64***	0.60***
debt_experiment	0.31***	0.20***	0.30***	0.22***
populism			-0.54***	-0.52***
rightwing			0.07***	0.06***
equality			-0.07	-0.07
_cons	4.47***	4.46***	6.06***	6.12***
N	1636	2267	1636	1926
R^2	0.082	0.075	0.137	0.126

Notes: The table repeats the regression of preferences towards debt reduction in Table 3.2 (full model, column 3), but then with a smaller set of controls so as to test whether the results are influenced by a loss of observations due to merging of the sample. We first only use the controls available in the September 2017 survey on fiscal preferences (column 1 and 2), not using the variables from the psychology module of DHS and the June 2017 survey on populism. In column 3 and 4 we use this same set of controls, while adding the regressor from the June survey (populism, and further), not using the variables from the psychology module. Column 1 and 3 first show the results with the baseline sample (N=1636), while column 2 and 4 use all available observations. Standard errors (which were clustered at the household level) are suppressed. The symbols *, ** and *** denote significance at the 10, 5 and 1 percent confidence level, respectively.

Table A.3.7 Regression of preferences - no correction of answer scales

	Debt reduction		Tax relief		More spending	
	Baseline	No correction	Baseline	No correction	Baseline	No correction
	(1)	(2)	(3)	(4)	(5)	(6)
populism	-0.46***	-0.56***	0.30***	0.49***	0.16***	0.34***
prob_literacy	0.20***	0.26***	-0.17***	-0.28***	-0.04	-0.07
debt_experiment	0.28***	0.38***	-0.11*	-0.17	-0.17***	-0.27**
age35to45	0.04	0.16	-0.20	-0.23	0.16	0.35
age45to55	0.08	0.40*	-0.19	-0.04	0.11	0.50**
age55to65	0.00	0.22	-0.14	-0.06	0.14	0.40*
age65plus	-0.04	0.08	-0.23*	-0.18	0.27**	0.58**
female	-0.27***	-0.26*	0.07	0.11	0.20**	0.39***
child	-0.24**	-0.16	0.16*	0.40***	0.08	0.27*
low	-0.22	-0.13	0.26**	0.56**	-0.04	0.23
vocational_med	-0.31*	-0.26	0.27**	0.60**	0.05	0.34
preuniversity	0.00	0.00	0.00	0.00	0.00	0.00
vocational_high	-0.15	-0.13	0.05	0.11	0.10	0.25
university	0.13	-0.15	-0.12	-0.31	-0.01	-0.15
incomeQ2	-0.01	0.00	0.12	0.18	-0.11	-0.06
incomeQ3	-0.10	0.09	0.12	0.28	-0.02	0.21
incomeQ4	-0.04	0.15	0.11	0.30	-0.07	0.15
hard_to_getby	-0.09*	-0.23***	0.04	-0.02	0.04	-0.04
riskaversion	0.03	0.09	-0.09**	-0.10	0.06*	0.16**
patience	0.10**	0.05	-0.06*	-0.16**	-0.04	-0.15**
locus_of_control	0.09*	0.11	-0.03	-0.06	-0.06	-0.10
openness	0.03	0.02	0.04	0.08	-0.07*	-0.08
conscientiousness	-0.06	-0.08	0.11***	0.17**	-0.05	-0.06
extraversion	-0.02	-0.01	0.05	0.09	-0.03	-0.02
agreeableness	-0.03	-0.02	-0.13***	-0.17**	0.16***	0.24***
neuroticism	-0.04	-0.08	0.05	0.08	-0.02	-0.02
rightwing	0.07***	0.11***	0.00	0.01	-0.08***	-0.11***
equality	-0.04	0.04	-0.10**	-0.07	0.14***	0.29***
_cons	5.58***	7.35***	2.51***	3.16***	3.91***	5.00***
N	1636	1636	1636	1636	1636	1636
R²	0.165	0.104	0.112	0.103	0.116	0.110

Notes: The table repeats the regression of preferences towards debt reduction in Table 3.2 (full model, column 3, 6 and 9), but without the correction of the answering scale, while adjusting the original scale to the 0 tot 10 scale that we employ in the baseline. We marked coefficients in grey when they are significant at the 5% level (or higher) in one model, but not significant at all in the other. Standard errors (which were clustered at the household level) are suppressed. The symbols *, ** and *** denote significance at the 10, 5 and 1 percent confidence level, respectively.

Appendix 4 to Chapter 4

Table A.4.1 Description of survey questions used

Variable	Meaning
Dependent variables	
aowreform_pref1 / aowreform_pref2	aowreform_pref1 (aowreform_pref2) lists the option selected first (second) in the questions below.
2004-2012 survey	'To make sure that the general old-age pension remains affordable certain measures have to be taken. Which of the following measures appeals to you most?' Three answers are possible: A lower general old-age pension. 1. An increase in the old-age pension premium for people working. 2. Increase the age on which I will receive the general old-age pension with two years (from 65 to 67 years of age).'
2013 survey	'Recently it has been decided to increase the general old-age pension age. To make sure that the general old-age pension remains affordable certain measures have to be taken. Which of the following measures appeals to you most? 1. A lower general old-age pension. 2. An increase in the old-age pension premium for people working. 3. Further increase in the age on which I will receive the general old-age pension.'
2014 survey and further	'In 2012, it has been decided to increase the general old-age pension age. To make sure that the general old-age pension remains affordable certain measures have to be taken. Which of the following measures appeals to you most? 1. A lower general old-age pension. 2. An increase in the old-age pension premium for people working. 3. Further increase in the age on which I will receive the general old-age pension.'
aowage_up_first (D)	Respondent's 1st choice is to raise the retirement age in the questions above.
aow_age_65	'Can you indicate to what extent you agree with the following statements? [...] The retirement age should be set at 65 years again'. (1=strongly disagree, 5=strongly agree)
resistance_ aowage_up	Based on the two questions above on reform preferences (*aowreform_pref1 / aowreform_pref2*) we constructed a variable measuring resistance to an increase in the retirement age: 1. Increase in the retirement age in respondent's first preferred reform option. 2. Increase in the retirement age is respondent's second preferred option. 3. Increase in the retirement age is respondent's last preferred option.
Covariates	
age	Approximate age of respondent (year survey minus year of birth)
below35, etc. (D)	Respondent is aged below 35, etc.
female (D)	Female

Variable	Meaning
partner (D)	Partner
child (D)	One or more children
employee (D)	Employed on a contractual basis
self_employed (D)	Works in own business, free profession, freelance, self-employed
household (D)	Respondent works in own household
benefits (D)	Respondent is (pre)retired, disabled, or working keeping benefit payments
other (D)	Respondent is busy otherwise (student, looking for work, volunteering)
grossincome	Gross income at personal level (if respondent doesn't know one's income, we set the gross income at 0)
financial_wealth	Total of financial assets (in EUR 1,000, can be negative)
university (D)	University
vocational_high (D)	Vocational colleges (HBO)
vocational_med (D)	Intermediate vocational training (MBO)
preuniversity (D)	Pre-university education (HAVO/VWO)
low (D)	Special, primary or lower vocational
riskaversion*	Respondent's score on risk aversion (see Table A.2.3)
patience*	Respondent's score on patience (see Table A.2.3)
locus_of_control*	Respondent's score on locus of control (see Table A.2.3)
openness, conscientiousness, extraversion, agreeableness, neuroticism	These are the 'Big Five' personality traits. For statements used to measure these traits, see Table A.3.2
health	Self-reported health score (1 to 5, 5=highest)
longevity_80	How likely is it that you will attain at least the age of 80? (0='no chance at all', 10='absolutely certain')
jobsatisfaction	How satisfied are you all in all with your current work? (1= very dissatisfied, 5=very satisfied)
work_autonomy	'The next questions are about your work. Please indicate in which extent you agree or disagree. [...] I make my own decisions' (1='totally disagree', 7='totally agree')
work_social	'The next questions are about your work. Please indicate in which extent you agree or disagree. [...] - I interact a lot with other people - I have to cooperate with others - I am interested in and help out other people - I work with others in a team' (1='totally disagree', 7='totally agree'; for the composite indicator we take the average score on the four statements)
ec_optimism	How do you think the economic situation of your household will be in five years' time in comparison to the current situation? (1= much worse, 5=much better, 'don't know' is reported as missing)

Variable	Meaning
frequency	Frequency of participation in the questionnaire on retirement expectations and preferences (runs from 1 to maximum 17 for each individual)
populism	Score on six statements: - The politicians in the Dutch parliament need to follow the will of the people; - The people, and not politicians, should make our most important policy decisions; - The political differences between the elite and the people are larger than the differences among the people; - I would rather be represented by a citizen than by a specialized politician; - Elected officials talk too much and take too little action; - What people call 'compromise' in politics is really just selling out on one's principles. - Response categories range from 1 (strongly disagree) to 5 (strongly agree). For our variable populism we divide the total score by the number of questions (7), so the variable also runs from 1 to 5.
prob_literacy	*Following the work of Hudomiet et al. (2018), the DHS questionnaire includes 4 questions to measure to what extent respondents can account and plan for uncertainty, such as the events like job loss.* 1. Consider that you take one ball from a bowl that holds 10 balls without looking. The bowl has 10 white balls and no red balls. What is the percent chance that the ball you take is red? 2. Now suppose you take one ball from a bowl that holds 10 balls without looking. The bowl has 7 white balls and 3 red balls. What is the percent chance that the ball you take is white? 3. Assume that the weather report accurately reports the chance of rain. Suppose the weather report tells you that the chance it will rain tomorrow is 70%. What is the chance it will NOT rain tomorrow? 4. Suppose that whether it rains in your town and whether it rains in Paris are unrelated. The chance that it will rain in your town tomorrow is 50%. The chance that it will rain in Paris is also 50%. What is the chance that it will rain both in your town and in Paris tomorrow? *Respondents are asked to answer the questions on a scale from 0 to 100, where 0 means a zero chance of outcome mentioned, and 100 means absolute certainty of this outcome. We reward respondents with 1 if they select the right answer, and 0 if not, resulting in an index ranging from 0 to 4.*

*See Table A.2.3 in the Appendix to Chapter 2 for the statements associated with these personality traits.
D = dummy variable

Table A.4.2 Summary statistics by year, 2004-2019

	2004	2005	2006	2007	2008	2009	2010	2011
aowage_up_first	0.18	0.20	0.25	0.28	0.26	0.33	0.38	0.43
age	45.36	44.00	44.63	45.67	46.92	47.60	49.22	49.83
employee	0.68	0.68	0.68	0.67	0.66	0.66	0.65	0.63
self_employed	0.05	0.05	0.05	0.05	0.06	0.07	0.08	0.09
household	0.11	0.11	0.11	0.11	0.10	0.09	0.08	0.09
benefits	0.11	0.11	0.11	0.13	0.14	0.14	0.14	0.16
other	0.05	0.06	0.05	0.04	0.03	0.04	0.05	0.03
grossincome	29.47	29.41	29.14	29.91	31.72	33.06	35.16	35.24
financial_wealth	25.33	27.87	27.52	28.13	32.97	33.24	37.70	40.74
female	0.45	0.48	0.48	0.49	0.48	0.46	0.45	0.47
partner	0.76	0.76	0.75	0.76	0.78	0.77	0.77	0.76
child	0.55	0.55	0.55	0.56	0.57	0.57	0.54	0.54
low	0.17	0.16	0.16	0.16	0.17	0.18	0.18	0.18
preuniversity	0.29	0.29	0.28	0.28	0.28	0.28	0.29	0.28
vocational_med	0.20	0.22	0.22	0.22	0.21	0.21	0.19	0.20
vocational_high	0.09	0.10	0.09	0.10	0.09	0.09	0.10	0.09
university	0.25	0.23	0.24	0.24	0.25	0.23	0.24	0.23
riskaversion	5.24	5.23	5.18	5.15	5.25	5.31	5.34	5.33
patience	4.14	4.12	4.11	4.09	4.10	4.09	4.13	4.15
conscientiousness	3.60	3.59	3.56	3.60	3.61	3.60	3.66	3.69
locus_of_control	4.54	4.56	4.52	4.57	4.50	4.55	4.54	4.57
health	3.87	3.89	3.89	3.91	3.90	3.89	3.90	3.90
frequency	1.75	2.31	3.02	3.79	4.36	4.77	4.82	5.46
New controls								
longevity_80	5.62	5.75	5.69	5.85	5.62	5.58	5.70	5.58
jobsatisfaction	4.03	3.97	3.98	4.01	4.00	4.04	3.98	3.99
work_autonomy	5.76	5.75	5.73	5.69	5.64	5.65	5.54	5.60
work_social	5.57	5.63	5.65	5.65	5.58	5.51	5.42	5.46
ec_optimism	3.08	3.08	3.05	3.12	3.07	3.03	3.01	2.96
N	1520	1647	1549	1465	1359	1367	1185	1188

Notes: The table shows the means for all variables used in regression in Table 4.2 for those respondents who were asked about their preferences for AOW reform. This is the same sample as for we reported summary statistics in Table A.2.2. Slight differences in the means between these two tables are attributable to some differences in data cleaning (see footnotes 47 and 48).
Source: DHS, 2004-2019

2012	2013	2014	2015	2016	2017	2018	2019
0.44	0.27	0.24	0.24	0.18	0.19	0.17	0.17
49.62	45.02	44.85	47.57	46.00	46.96	46.23	46.95
0.65	0.73	0.73	0.71	0.72	0.70	0.64	0.63
0.08	0.07	0.07	0.07	0.07	0.07	0.06	0.07
0.08	0.07	0.07	0.08	0.07	0.06	0.05	0.06
0.14	0.09	0.11	0.09	0.09	0.13	0.13	0.13
0.04	0.04	0.02	0.05	0.04	0.04	0.11	0.11
36.64	36.87	34.29	34.30	35.93	35.23	30.23	30.92
36.53	28.32	18.32	30.84	34.04	29.37	27.79	31.29
0.47	0.49	0.52	0.50	0.51	0.50	0.51	0.53
0.77	0.77	0.78	0.77	0.76	0.69	0.64	0.64
0.54	0.47	0.54	0.55	0.50	0.41	0.37	0.40
0.18	0.22	0.21	0.17	0.21	0.16	0.17	0.18
0.29	0.33	0.33	0.27	0.29	0.28	0.28	0.29
0.21	0.21	0.21	0.29	0.26	0.33	0.33	0.27
0.10	0.08	0.08	0.08	0.08	0.07	0.07	0.07
0.22	0.16	0.17	0.19	0.15	0.16	0.15	0.17
5.30	5.27	5.23	5.13	5.19	5.11	5.06	5.09
4.16	4.18	4.19	4.17	4.15	4.16	4.13	4.15
3.68	3.65	3.63	3.62	3.65	3.62	3.71	3.77
4.55	4.53	4.54	4.49	4.57	4.56	4.58	4.58
3.88	3.92	3.79	3.89	3.87	3.83	3.83	3.80
5.47	5.32	5.02	5.40	5.61	5.61	6.10	5.21
5.60	5.72	5.90	5.87	6.88	7.01	6.92	6.15
4.00	3.97	3.91	4.02	4.00	3.95	3.94	3.98
5.52	5.53	5.59	5.59	5.55	5.51	5.47	5.51
5.45	5.47	5.65	5.42	5.44	5.42	5.42	5.45
2.89	3.06	3.15	3.08	3.17	3.18	3.25	3.16
1224	1232	396	1467	1432	1697	1677	2056

Table A.4.3 Impact of frequency of participation

	Support for raising the statutory retirement age
	conditional logit
	FE
frequency	0.04
	(0.06)
N	6392
Pseudo R^2	0.096
Time dummies	Yes

Notes: The table shows the results of conditional logit fixed effects model for respondents' support for raising the retirement age. We included all time-varying regressors from Table 4.2, but only report the coefficients for the frequency that respondents took part in the survey of interest (from 1st time to at maximum the 17th time, see Table 4.1). In parentheses are standard errors. The symbols *, ** and *** denote significance at the 10, 5 and 1 percent confidence level, respectively.

Table A.4.4 Correlation between dependent variables and instruments

	Support for AOW age at 65 1=strongly disagree, 5=strongly agree				Resistance AOW age up 1=first preferred option, 3=last preferred option			
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
trust_politics_pc	-0.28***	-0.21***			-0.15***	-0.14***		
integrity_finance_pc			-0.22***	-0.13***			-0.04	-0.04
age35to45		-0.13		-0.15		0.97***		0.98***
age45to55		0.13		0.11		0.94***		0.95***
age55to67		0.23*		0.27**		1.14***		1.17***
self_employed		-0.09		-0.12		-0.09		-0.11
household		-0.08		-0.06		0.13		0.12
benefits		-0.39***		-0.34**		-0.44***		-0.44***
other		-0.02		-0.02		-0.33		-0.34
incomeQ2		0.13		0.12		0.12		0.11
incomeQ3		0.22		0.21		0.18		0.18
incomeQ4		-0.17		-0.17		-0.02		-0.02
female		0.08		0.07		0.08		0.08
partner		0.01		0.01		-0.03		-0.03
child		0.21		0.19		0.28***		0.27**
vocational_med		-0.07		-0.07		-0.04		-0.06
preuniversity		-0.65***		-0.67***		-0.18		-0.20
vocational_high		-0.40***		-0.42***		-0.10		-0.13
university		-0.85***		-0.88***		-0.18		-0.23
prob_literacy		-0.08		-0.09*		0.05		0.05
riskaversion		0.03		0.05		0.08*		0.09*
patience		-0.04		-0.04		0.01		0.01
locus_of_control		-0.13**		-0.11**		0.01		0.01
ppenness		0.05		0.07		-0.00		0.01
conscientiousness		0.16***		0.17***		-0.00		0.00
agreeableness		-0.05		-0.07		-0.03		-0.04
extraversion		-0.11		-0.13		-0.14**		-0.15**
neuroticism		0.02		0.02		-0.07		-0.07
longevity_80		-0.05**		-0.05**		-0.04*		-0.04*
ec_optimism		-0.07		-0.07		-0.00		-0.01
_cons	3.34***	4.21***	3.33***	4.25***	2.28***	2.20***	2.27***	2.24***
N	798	613	793	613	363	331	361	331
R^2	0.054	0.254	0.033	0.239	0.035	0.185	0.002	0.159

Notes: The table reports results of OLS regression of attitudes towards the retirement age on pre-crisis trust in national politics and pre-crisis trust in the integrity of financial sector management, both without and with controls. Standard errors (which were clustered at the household level) are suppressed. The symbols *, ** and *** denote significance at the 10, 5 and 1 percent confidence level, respectively.

Table A.4.5 IV estimation attitudes towards pension reform

	Support for AOW age at 65 1=strongly disagree, 5=strongly agree		Resistance AOW age up 1=first preferred option, 3=last preferred option	
	OLS	2SLS	OLS	2SLS
populism	0.43***	0.61***	0.14***	0.42***
prob_literacy	-0.04	-0.02	0.05	0.07
age35to45	-0.01	-0.14	0.85***	0.66*
age45to55	0.30	0.20	0.85***	0.68*
age55to67	0.41	0.28	1.05***	0.87**
age67plus	0.17	0.03		
self_employed	-0.12	-0.12	-0.14	-0.14
household	-0.16	-0.17	0.07	0.03
benefits	-0.44***	-0.46***	-0.45***	-0.47***
other	-0.13	-0.15	-0.37	-0.43*
incomeQ2	0.17	0.19	0.10	0.14
incomeQ3	0.27*	0.29*	0.15	0.11
incomeQ4	-0.07	-0.02	-0.02	0.02
female	0.13	0.15	0.06	0.04
partner	0.01	0.01	-0.02	-0.00
child	0.15	0.13	0.25**	0.23**
vocational_med	0.04	0.08	-0.02	0.05
preuniversity	-0.52***	-0.44***	-0.16	-0.03
vocational_high	-0.21*	-0.11	-0.05	0.09
university	-0.47***	-0.31	-0.08	0.18
riskaversion	0.04	0.03	0.09*	0.09**
patience	-0.03	-0.03	0.02	0.05
locus_of_control	-0.12**	-0.12**	0.02	0.05
openness	0.11	0.11	0.04	0.08
conscientiousness	0.12**	0.10*	-0.02	-0.06
agreeableness	-0.08	-0.08	-0.05	-0.04
extraversion	-0.10	-0.09	-0.14**	-0.13*
neuroticism	0.03	0.03	-0.06	-0.04
longevity_80	-0.07***	-0.07***	-0.05**	-0.06***
ec_optimism	-0.08	-0.08*	-0.02	-0.02
_cons	3.84***	3.86***	2.24***	2.19***
First stage (populism)				
trust_politics_pc		-0.31***		-0.25***
integrity_finance_pc		-0.09**		-0.12**
(other controls suppressed)				
Partial R²		0.147		0.118
F(2,581)/F(2,299)		50.0		20.0
Sargan (score) chi²(1)		0.17 (p=0.68)		1.60 (p=0.21)
Wu-Hausman F(1,581) /F(1,299)		2.32 (p=0.13)		4.10 (p=0.04)
N	613	613	330	330
R²	0.325	0.310	0.179	0.096

Notes: The table first reports ordinary least squares (OLS) regression and two-stage least squares (2SLS) regression whereby populism is instrumented by pre-crisis trust in national politics and financial sector management plus all the controls used in the second stage. Due to space limitations, standard errors are suppressed. The symbols *, ** and *** denote significance at the 10, 5 and 1 percent confidence level, respectively.
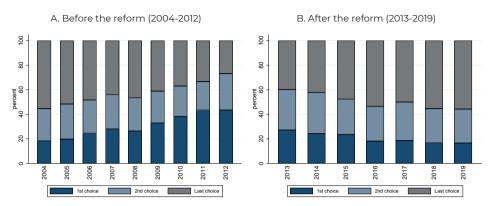
Figure A.4.1 Support for raising the retirement age (further)

A. Before the reform (2004-2012)　　　　　B. After the reform (2013-2019)

Notes: The figures show the percentage of respondents listing an increase in the retirement age as 1st, 2nd or last preferred reform option for adjusting the PAYG system, before the reform (2004-2012, panel (a)) and thereafter (2013-2019, panel (b)).
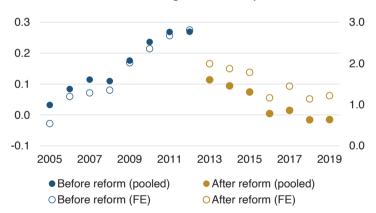
Figure A.4.2 Year effects from fixed effects regression reform preferences

Notes: The figure shows the coefficients of the year dummies from probit regression (filled dots, left axis) and the fixed effects regression (transparent dots, right axis) of support for an increase in the retirement age, in which all year effects have been included in the same regression. The dark dots are identical to those in Figure 4.4. Up till 2012, respondents were asked about their support for an increase in the retirement age from 65 to 67, after this respondents about their support for a further increase. Confidence bands are suppressed to facilitated reading.
Source: DHS, 2004-2019.

On the European continent, the case for structural reforms has been made recurrently. Referring to all measures that lead to structural increases in labour utilisation and productivity, structural reforms have been on the agenda of European governments for decades. Reforms have been called for to alleviate the macroeconomic effects of an ageing population, to facilitate rebalancing within the euro zone and to address segmented labour markets. And as the COVID-19 pandemic hit the European continent in early 2020, sparking a deep economic contraction, the coming years will see a need for structural reforms to help restore public finances and support sustainable growth.

Clearly, political elites that pursue economic reforms face a delicate task. Structural reforms come with fundamental questions about the desired direction of change, and the practical design of reform. Reform proposals may also face opposition from powerful groups that stand to lose from these reforms or, often, by the public at large who may not appreciate that reforms are necessary.

Drawing on the experiences with structural reform in the Netherlands over several decades, this thesis aims to shed light on some particularly controversial reforms that have featured highly on the agenda of European policymakers. The first part of the thesis focuses on public opinion toward fiscal reform. Most notably, it traces the attitudes of Dutch households towards an increase in the age of retirement, a reform that was debated for decades – also in other European countries – and was finally adopted in 2012, while it remained in dispute in the years thereafter. The second part of the thesis focuses on collective bargaining, with lessons on how sector-level bargaining can deal with challenging economic conditions, such as the shock that hit the economy at the end of 2008.

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