The Sources of Pension Reforms in Europe: Domestic Factors, Policy Diffusion, or a Common Shock?

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Abstract

During the 1990s and 2000s, many European Union member states implemented structural pension reforms, but the causal pathway of reform remains inconclusive. Some scholars have attributed steps towards pension privatization to a process of policy diffusion, wherein reforms in one country are linked to similar developments in peer nations. Others have ascribed pension reforms exclusively to domestic political pressures (such as unfavorable demographic developments) that strain fiscal resources. What has been missing the literature is an argument about how common shocks may influence the politics of pension reform. We address this gap by developing hypotheses about how the Maastricht treaty may be causally connected to the decision to privatize pension systems or introduce funded components. Furthermore, this is the first study that systematically tests whether pension reforms during the observed period are due to domestic pressures, policy diffusion, or a common shock. We use a spatial autoregressive probit analysis in an event history setup to distinguish between all three possible causes of pension reform. We also identify the direction and effect size of each mechanism.

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1 Introduction

In times of slow growth, aging populations, and overstretched social security systems, the privatization of public pension systems is commonly viewed as imperative to ease the strain on nations' fiscal resources. Yet, the overhaul of state pension systems is a high-risk political endeavor. Because privatization replaces the principle of public social insurance with individual responsibility for old age income, organized beneficiaries of state pension benefits will vehemently oppose reform. Structural pension reform is also perilous from a financial cost perspective. By channeling pension contributions away from public to private pension funds, the government accumulates a deficit to cover the current pension liabilities during the reform's transition period. Since the costs of pension reform accrue immediately but the long-term benefits – lower fiscal outlays – do not materialize until the distant future, it is hardly surprising that governments pursuing such a strategy often suffer electoral defeat (Pierson 2000). Given the high risks associated with the reform of state pension systems, it is puzzling to observe that this measure was adopted by many European countries between 1980 and 2009. During this period, several European governments have either privatized their pension systems or introduced funded components.

Existing studies do not offer unequivocal conclusions about the causal pathway of reform because they concentrate on a single impetus instead of probing a variety of possible explanations. Studies attributing pension reform purely to domestic pressures, such as rising longevity, low fertility, and concomitant financial exigencies (Taverne 2001; Disney 2003) fail to account for the timing of pension reforms. The growing top-heaviness of the age pyramid has plagued European societies since the 1970s, but the majority of pension reforms occurred in the 1990s. Another influential stream of research locates the source of structural pension reform in processes of policy learning, wherein reforms in one country are linked to similar developments in peer nations (Weyland 2005; Orenstein 2003, 2008). However, temporal and

geographic correlations in the adoption of policy reform may be due to "umbrella causation", whereby an external stimulus – like rain – may induce similar, but spuriously correlated, reactions in multiple subjects – like the opening of an umbrella (Weber 1978, p. 23). In this paper, we argue that a third – and so far overlooked – possible influence on pension reforms constitutes an exogenous shock, such as an international treaty, that may restrict domestic policy choices. The macroeconomic constraints imposed by the Maastricht treaty, which was adopted by twelve European member states in 1992, can be seen as a "common shock" that pushed the member states to overhaul their state pension systems.

The challenge to disentangle these mechanisms is commonly referred to as "Galton's problem" (Jahn 2006; Franzese and Hays 2008). Inadequate modeling of interdependence may lead scholars to exaggerate the effect of common shocks, while failure to control for shocks may induce analysts to overestimate the role of diffusion dynamics and domestic exigencies. Because the underlying causal logic of pension reform shapes actors' political room of maneuver, it is crucial to get the causal story right. Uncovering the factors that drive pension regime transitions is also normatively important as age-related spending constitutes one of the largest items on governments' budgets.

In this paper we demonstrate how scholars can distinguish between these causal pathways. We employ a spatial autoregressive probit analysis in an event history setup to systematically test whether pension reforms in Europe carried out between 1980-2009 are the result of domestic factors, policy diffusion, or a common shock. We establish that each of these mechanisms affected pension reforms in Europe during the period of interest and highlight the conditional and interactive nature of these forces. Demographic aging played an important role for pension reform in some countries, but not others. The Maastricht treaty had an accelerating impact on pension reforms in low-debt countries, but is not causally related to pension reforms in high-debt countries. Finally, we find evidence for the diffusion of policy ideas, but the learning effect is negative. The European countries learned from early

reformers, but were obviously keen on avoiding the mistakes of their peers. These findings have important implications for democratic accountability and research on cooperation in the EU more generally. Although national governments firmly control pension policies, EU treaties may also have far-reaching consequences for old age income provision. As a result, governments may become decoupled from accountability, while external shocks may require painful policy adjustments. Alternatively, governments may resist EU mandated change when the costs of compliance are too high, thereby jeopardizing EU integration efforts. Thus, identifying the sources of policy change is critical to understanding how global economic integration constrains domestic policymaking.

The remainder of this paper discusses the possible causes of pension privatization efforts. We then devise a spatial autoregressive probit model that tests whether the timing of pension reform is the result of domestic pressures, policy diffusion (learning), external shocks, or a combination thereof. The final section concludes with a summary of the main findings and suggestions for future research.

2 Theoretical approaches to the study of pension reform

When and why do countries introduce reforms to their pension systems? While a burgeoning literature has documented the adjustment pressure that demographic aging poses for the European nations, only a few scholars have analyzed the impact of international factors, such as European Union directives, on domestic pension policies (Pochet 2003; Haverland 2007; Hennessy 2008). We contend that the adoption of the Maastricht Treaty and concomitant changes in governments' marginal utility for running high deficits and debt levels – of which unfunded pension liabilities are the largest part – are causally related to the pension reforms in the 1990s and early 2000s. By accounting for the timing of pension reforms we seek to

address shortcomings of several influential lines of research.

Two approaches have focused on domestic factors in explaining pension reform. The literature on population aging has productively identified unfavorable demographic developments as important sources of reform pressure (World Bank 1994; Disney 2003). Two trends are particularly problematic in this respect: increasing life expectancy and declining fertility rates. Due to both factors, the share of people aged 65 years and over will rise dramatically relative to the working-age population (15 to 64 years). The EU Commission estimates that, from 2013, the European working-age population will start to shrink, while the over-60 population will continue to increase by about two million people a year (Commission of the European Communities 2010). From a policy perspective, one could therefore expect unfavorable demographic developments to increase the likelihood of pension reform.[1]

However, demographic aging alone cannot explain the temporal clustering of pension reforms. The growing top-heaviness of the age pyramid has plagued European pension systems for decades, yet the majority of pension reforms were carried out in the 1990s and 2000s. To be sure, growing demographic pressures will cause demands on PAYG pension systems to eventually outgrow the fiscal ability of the state. Once a critical threshold is reached, the results are short-term budget crises and efforts to rein in spending, including cuts in the pension system. However, none of the countries of western Europe were near such a dramatic tipping point in the 1990s. Demographic pressures by themselves therefore cannot explain the timing of the wave pension reforms in these countries.

Other approaches focussing on domestic policy explanations have linked pension reform to party ideology. According to this view, left party power matters for the level of income replacement rates of benefits (Korpi and Palme 2003). Others have denied that political parties have any effect on social policy development (Rose 1984; Pierson 1994). Martin and

[1] Alternatively, elderly voters often represent powerful political constituencies. To the extent that a growing cohort of the elderly exerts political power, policy pressures for reform may go unheeded. Accordingly, governments with greying populations should become less willing to stomach the political costs of reform.

Swank obtained inconclusive evidence, finding that government tenure of social democratic parties is only moderately associated with an increase in social spending (Martin and Swank 2004, p. 600). A different set of scholars found that left parties have a bruising effect on the welfare state. From this perspective, left parties choose unpopular retrenchment policies to position themselves strategically. By moving closer to the center on the ideological left-right party spectrum, left parties hope to gain confidence with the business community and centrist voters more generally (Ross 2000; Kitschelt 2001).

Whatever the "true" effect of party ideology on the nature of pension reforms may be, it remains that the political color of government also fails to account for the timing of pension reforms in Europe during the observed period. Changes in the partisan complexion of government have occurred between 1980-2009 across all countries of western Europe, but it is unclear whether the left or the right is more likely to support privatization. For this reason, we do not have firm expectations with regard to the timing of the observed pension reforms. We leave it to the empirical analysis to uncover partisan effects. However, we will revisit the question of ideological orientation in the context of policy diffusion and learning. As discussed in more detail below, governments may be more likely to learn from their partisan counterparts in other countries than from governments with different ideological leanings.

A different stream of research has argued that pension regime transitions are driven by diffusion processes, wherein the decision to reform is shaped by similar developments in relevant peer nations. The causal mechanism by which the process of diffusion unfolds has been variously specified as one of learning (Heclo 1974; Gilardi *et al.* forthcoming), the appeal of certain ideas (Orenstein 2008) or "soft power" (Keohane and Nye 1989), competition (Simmons and Elkins 2004), the dissemination of norms (Keck and Sikkink 1998), or cue-taking by governments (Brooks 2005, 2007).

Popular perceptions of the European Union as a major agent that seeks to harmonize

policies in Europe through deregulation might induce analysts to attribute pension reforms to policy diffusion. Furthermore, the existence of extensive trade ties between all twelve EU countries in our sample could be an indicator for diffusion dynamics as integrated economies may also experience synchronization of partisan cycles (Kayser 2009). Finally, institutions such as the World Bank have been instrumental in disseminating ideas about pension privatization as a strategy to deepen capital markets and increase domestic savings rates (World Bank 1994; Orenstein 2003).

But as valuable as the diffusion literature has been, it often suffers from "Galton's problem", which denotes the challenge to disentangle peer dynamics from independent government reactions to a common shock. While many scholars have correctly linked pension reforms in Latin America and Eastern Europe to the transmission of social policy ideas (Müller 1999; Weyland 2005; Brooks 2007; Orenstein 2003, 2008), Brooks (2005) has found that diffusion matters little for the decision to privatize within OECD countries.

In cases where countries do not adopt reforms implemented by their peers, these are usually taken to be instances in which policies fail to take hold in other countries. However, it is possible that this is indicative of a negative form of learning. Observing policy reform in other countries allows politicians to better gauge the risks and unanticipated consequences of such a policy move. Anecdotal evidence suggests that implementing reform indeed can cause electoral backlash. During the 1990s, coalition governments in Germany, France, and Italy have lost elections because voters resisted their controversial pension reform plans. In Germany, chancellor Kohl was dethroned after sixteen year in office largely because his government had introduced a demographic factor into the pension formula that was to reduce the replacement rate from 70 percent in 1999 to 64 percent in 2030. Fulfilling a central campaign promise, the red-green successor government initially revoked this reform but later reinstated the demographic factor after realizing the extent to which generous PAYG pensions placed a strain on fiscal outlays (Streeck and Trampusch 2005). In France, prime

minister Juppé lost the election in 1997 after he tried to force pension cuts on private sector employees without any negotiation. In Italy, a general strike organized by the labor unions in 1994 against the government's pension reform plans resulted in the fall of prime minister Berlusconi's center-right government (Bonoli and Palier 2008). In all of these cases, the reform plans were limited to retrenchment measures, while wholesale pension privatization was considered too risky. Consequently, widespread resistance to pension cutbacks should make politicians contemplating more ambitious reforms particularly cautious.

So, how does one know that governments "learned" from one another and that the new information influenced the likelihood of pension reform? In the existing literature hypotheses about the directional effects of learning are often missing. To fully understand how governments use new data to promote policy change we need to have a theory about who learns from whom, and what lessons were drawn. Following the literature on partisan waves (Midtbo 1998; Kayser 2009), we test the proposition that governments with similar ideological leanings are more likely to learn from one another than governments with conflicting partisan views. It has been argued that concurrent shifts in the fortunes of the left or right across multiple countries are to some degree international in origin (Kayser 2009). While distant and dissimilar states would be poor candidates for such a test – few would expect Chile to influence Germany – it makes sense to examine geographically proximate and economically integrated states. The European states meet those criteria. We argue that learning within this group of peer nations can take on a positive direction if the benefits of policy implementation in peer nations appear to outweigh the costs. Conversely, the direction will be negative if reform is associated with electoral defeat or other negative feedback effects that over time may undermine the sustainability of a given pension regime (Weaver 2011).

2.1 The influence of the Maastricht criteria on pension reform

A third possible cause of pension reform – one that has been overlooked by the extant pension literature – is an external shock that induces similar, but spuriously correlated, reactions in several countries. The Maastricht Treaty, which limits the build up of public sector deficits and debt over time, may have acted as external impetus for the implementation of pension reforms in Europe. To meet the famous Maastricht criteria, member states' deficits could be no larger than three percent of GDP, and debt levels could not exceed 60 percent of GDP. In the view of the treaty's authors, the European member states needed a strong disincentive to run excessive deficits in order to preclude monetary and financial instability in the future eurozone. Consequently, the Maastricht criteria may have decreased member states' marginal utility for retaining high deficits and debt levels, of which unfunded PAYG pension liabilities constitute the largest part. Holzmann (2006) has argued that a Maastricht fiscal regime, together with enhanced labor market flexibility, mobility, and labor supply in aging societies, all demand some convergence in the area of pensions. Thus, the Maastricht Treaty may have altered the intertemporal policy trade-off of governments. Without the Maastricht Treaty, governments had incentives to put off reforming their pension system into the future. Why incur costs in the present when the benefits won't accrue until the distant future? However, the Maastricht deficit criteria may have altered political incumbents' time horizon: the longer European governments failed to reduce their future PAYG pension claims, the tougher the actual cuts required to live within the confines of the stability pact would have to be. Therefore, the longer the delay, the higher the likelihood that governments would need to cut entitlements of current beneficiaries (Truglia 2002, p. 4). The closer to the present the actual cuts in benefits are, the greater the risk that governments will suffer electoral punishment. In this respect, Maastricht-induced pressure could be seen as a way of overcoming the time inconsistency–problem political incumbents invariably confront, namely incurring costs in the present to realize a collective good – in this case, a reduced

social security pension deficit – in the future (Pierson 1994; Jacobs 2008).

Another reason why the Maastricht Treaty may be causally related to pension reform is that it changed the level of governments' implicit and explicit debt obligations and as such, affected the perception of their solvency (Fiess 2003, p. 4). By creating the European Central Bank, national governments lost the option of expanding the money supply to meet debt obligations. Consequently, rating agencies may downgrade countries that fail to make their overstretched social security pension systems more sustainable for future generations (Eijffinger and De Haan 2000). Rating agencies provide international financial markets with a common language of risk and carry the "force of law" in many countries around the world (Abdelal 2007, ch. 7). Lower ratings indicate a decrease in a country's readiness and willingness to meet debt obligations duly and therefore deter potential investors. This, in turn, may decrease the number of investment funds which will buy government bonds, and increase the interest rate on government debt. Privatizing a pension system can adversely affect sovereign credit risk particularly during the transition period. By channeling pension contributions away from the government and creating a deficit of resources to cover the current pension liabilities during the transition period, the resulting public debt can negatively affect a sovereign's perceived creditworthiness (Cuevas *et al.* 2008).

Other scholars argue that unfunded pension claims of nation states play only a minor role in assessing sovereign credit risk (Truglia 2002) . Although calculations of future pension liabilities provide a projection of a given scenario, Truglia does not expect this projection to actually materialize: "Large future pension claims have not greatly influenced our ratings of government debt in the industrialized world, even where net present value calculations would indicate very substantial claims on government resources over a 20-30 year time horizon. We simply expect that the government will 'default' in the future on its pension promises as currently written in law in a way that will favor creditors" (Truglia 2002, p. 2-3). The expectation of default implies, however, that the ratings of the affected countries could come

under severe pressure unless appropriate reform steps are introduced.

The downgrading of Greece is a dramatic example of how skepticism about a country's credit worthiness can cause severe financial disruptions. In December 2009, it was revealed that the Greek government had been feigning debt and deficit figures to conceal its violation of the Maastricht criteria. Consequently, skepticism about Greece's ability to bring its growing deficit under control induced the three largest credit rating agencies to cut Greece's sovereign credit rating from A minus to junk status. The Greek debt crisis generated externalities for the eurozone as a whole in the form of a weaker euro and contagion in bond markets spreading to Portugal, Spain, Italy, and Ireland. Although the lack of confidence in Greece's credit worthiness has more to do with domestic corruption than with the country's pension system, the government's austerity measures included highly unpopular pension cuts for civil servants, as well as a radical overhaul of the underfunded state pension system.

However, the European countries may have been oblivious to the danger of rating downgrades before the Greek tragedy hit in 2009. Thus, an alternative perspective may doubt the causal connection between the observed pension reforms and the Maastricht criteria. Brooks (2005, p. 282) has argued that the likelihood of pension reform may in fact decline with growing deficits, as domestic financing constraints preclude governments from shouldering the transitional costs that arise from pension reform. Such costs emerge as individuals begin to divert payroll contributions to privately managed pensions schemes. In the transition period, some governments may offer tax breaks to encourage citizens to set up private pension schemes. But because the government must uphold its commitment to existing pension claims by large elderly populations, the resulting fiscal shortfalls may be enormous, contributing to the so-called "double payment" problem. As a result, high deficits may discourage governments from embarking on pension reform. If this were true, even the commitment to a deficit-restraining mechanism, such as the Maastricht Treaty, should have no discernible effect on the decision to privatize or introduce funded components. Yet, the Maastricht cri-

teria may have had a different effect on low debt countries. While highly indebted countries may have found the costs of reforming their pension system prohibitive, low-debt countries (the ones who defined the criteria in the first place) may have used the treaty stipulations to justify unpopular policies at home. According to this perspective, the pension reforms reflect the political and economic strength of those countries that had critically shaped the treaty's design. Once in place, the Maastricht criteria may have served as a scapegoat for pension reforms that had been planned long before.

The implication is that some pension systems are better placed to deal with demographic difficulties and external shocks than others. Pension scholars usually distinguish between Beveridgean and Bismarckian pension systems, which captures ideological disagreements over the proper role of the state vis-a-vis firms or private schemes in providing retirement income.[2]

Beveridgean pension systems are aimed at poverty prevention. Social security pensions are low and ungenerous, providing either universal flat-rate or means-tested benefits. The majority of people do not expect their state pension to secure their living standard during the retirement phase, and hence have to save for their own retirement. Consequently, Beveridgean pension systems tend to have mature occupational and private pension sectors. Bismarckian pension systems, by contrast, are based on the social insurance principle and provide earnings-related benefits aimed at status maintenance during old age (Esping-Andersen 1996; Schludi 2005; Bonoli and Shinkawa 2005). Beneficiaries rely on statutory

[2]More fine-grained distinctions of pension systems include universal pension systems (Esping-Andersen 1990) and notional defined contribution schemes (NDCs) (Orenstein 2008; Brooks 2007). While our classification may not depict the entire complexity of pension systems, we are most interested in the dichotomy between mature PAYG systems and mature occupational or private pension systems. Esping-Andersen (1990)'s analysis of universal pension systems is less useful for our purposes as he lumps together countries like Finland and the Netherlands, although the Netherlands comprises a mature occupational pension sector whereas Finland relies on a PAYG system. Sweden is the only country in our sample that adopted (and invented) the NDC system in 1998, but we decided not to adopt a separate category. Since the return that contributions earn are not the product of investment returns in the market, but set by the government, NDC schemes are more similar to PAYG systems (Brooks and Weaver 2006).

social security pensions as their primary income during the retirement phase, while occupational and private pensions are underdeveloped.

The following section probes whether structural pension reforms in the 1990s and early 2000s were the result of domestic factors, the diffusion of policy ideas, the reaction to a common shock, or a combination thereof.

3 Empirical Analysis

3.1 Data and Research Design

Our analysis includes 16 western European countries from 1980-2009. The countries and the year in which initial reform was implemented are listed in table 1.

Table 1: Countries in Study, 1980-2009

Country	Year of Reform	Maastricht Member	Mature occupational or private pension sector
Austria	1990	yes[b]	no
Belgium	1995	yes	no
Finland	1992	yes[b]	no
France	—	yes	no
Germany	2001	yes	no
Greece	—	yes	no
Ireland	1990	yes	yes
Italy	1995	yes	no
Luxembourg	2000	yes	no
Netherlands	1993	yes	yes
Norway	—	no	no
Portugal	2002	yes	no
Spain	1999	yes	no
Sweden	1998	no	no
Switzerland	1985	no	yes
UK	1988	yes[a]	yes

[a] The UK opted out of introducing the Euro.
[b] Became member upon accession to EU.

A look at the table shows that four countries implemented major pension reforms prior to the signing of the Maastricht treaty in 1992. These countries are Austria, Ireland, Switzerland, and the United Kingdom. From the group of countries that adopted reform legislation after 1992, only Norway and Sweden refused to sign the Maastricht treaty.[3] Austria and Finland joined the treaty when they acceded to the Union on January 1st 1995. Austria had applied to join the then European Community in 1989, Finland in 1992. Of the Maastricht signatories, only France and Greece failed to implement any form of major pension reform by 2002.

Since we make causal claims about the effects of the Maastricht Treaty, we include a number of countries in the analysis that were not signatories. Unfortunately, the number of potential control cases is small. In order to find cases that are similar in social, political, and economic structures, we concentrate on Western Europe. There are only four states that did not sign the Maastricht treaty in 1992. They are Norway, Sweden, Switzerland and arguably Britain.[4] However, we can exploit temporal variation in the research design. The signatory countries themselves were free of the influence of Maastricht prior to 1992. For these early years, they are therefore part of the control group.

The phenomenon we seek to explain is the timing of pension reform, therefore a natural statistical setup is event history analysis (Box-Steffensmeier and Jones 2004). The dependent variable is pension reform, which we code as one for the year in which a government decided to implement full or partial privatization and zero in other years. Although it is common in the literature to distinguish between pension privatization and notional defined contribution schemes (Orenstein 2008; Brooks 2007), we are more interested in the dichotomy of "reform – no reform," rather than the scope of privatization. Qualitative differences in reform outcomes

[3]Norway decided in a 1994 referendum to stay out of the European Union entirely.

[4]Despite being a signatory to the Maastricht treaty, Britain's initial drive to join the single currency was derailed on 'Black Wednesday,' 16 September 1992, when the pound sterling was forced out of the European Exchange Rate Mechanism through speculative attacks.

are a reflection of varying domestic political realities. What matters for our purposes is the timing of the initial decision to try reform, independent of policy outcomes.

To test for the effects of the Maastricht deficit criteria, we include a dummy variable that is coded 0 for all cases prior to 1992, and 1 for member states afterwards. Since the Maastricht treaty was signed on 7 February 1992, we include this year as first year in which we expect an effect on the probability of pension reform.[5] For non-treaty members, the variable is coded as 0 for all years. To tap into the policy versus politics distinction, we include a general government debt variable, measured as percentage of GDP.[6] We interact this variable with the Maastricht dummy. To recap our theoretical expectations, if Maastricht serves as catalyst for pension reform in countries with a high debt burden and thus strong fiscal pressures, the interaction should be positive. Conversely, if the treaty only has an effect on countries with low debt and therefore low political costs of reform, the interaction should be negative.

One important specification choice in our analysis is how to incorporate learning effects. To this end, we use a spatial autoregressive probit setup (LeSage and Pace 2009). The spatial probit incorporates the dependent variable, i.e. pension reform, on the right-hand side of the equation. The degree to which individual governments influence the decision of other governments is determined by weights that capture the affinity between them. The relevance of these connections is estimated through a connectivity parameter. This parameter

[5]To show that 1992 has a special status, we plan to vary in later iterations of the analysis the start date for the Maastricht dummy between 1988 and 1995.

[6]The main source of this data is the OECD.Stats Extracts Portal (2011). Where necessary, we augmented the data from national statistics. This left some data missing for the early years of the sample. We used the first reported debt value for each country with missing data to fill in these blanks. Since government debt generally follows an upward trend this should overstate the true debt levels, and conservatively lead to smaller estimated effect sizes.

indicates learning effects. Formally, the model can be written as

$$y^*_{i,t} = \rho \sum_{j \neq i} b_{j,i} y^*_{j,t} + \boldsymbol{x}_{i,t}\boldsymbol{\beta} + h(t) + \varepsilon_{i,t}, \tag{1}$$

where y^* is the unobserved latent variable for reform, ρ is the connectivity parameter, $b_{j,i}$ is the affinity between the governments of countries j and i, $\boldsymbol{x}_{i,t}\boldsymbol{\beta}$ is a standard linear additive term of covariates, and $h(t)$ is a baseline hazard. Observed realizations of the dependent variable are

$$Y_{i,t} = 1 \text{ if } Y^*_{i,t} \geq 0, \tag{2}$$

$$Y_{i,t} = 0 \text{ otherwise.} \tag{3}$$

We estimate this model in a Bayesian framework.

Our theoretical expectations drive the choice of affinity weights $b_{j,i}$. We want to test the hypothesis that governments with similar ideological outlook are more likely to learn from each other. Accordingly, we code $b_{j,i} = 1$ if countries i and j have governments with a similar partisan makeup (both left, centrist, or right), and $b_{j,i} = 0$ otherwise.[7]

Since our data structure implies an event history setup with discrete time, we need to model the baseline hazard. We follow Beck *et al.* (1998) who propose to use year dummies to model temporal dependence. A special feature of our model is that we also include the endogenous learning component. This implies that we have to keep countries in the sample that already experienced reform. The dependent variable of these countries is coded as 1 for the remainder of the analysis. Since we try to capture the effects of learning, this is substantively desirable. Once reforms are implemented in a country, the political example

[7]Following common practice in the spatial econometrics literature we also 'row-standardize' the weights, so they add up to 1 for each country i (Franzese and Hays 2007). This implies that the learning component can be interpreted as weighted average of pension reforms in other countries.

is set and remains visible over time. Following Beck *et al.* (1998, footnote 22), we set the first-year time dummy to 1 for countries that already implemented reform in the previous years .[8]

Other independent variables in the analysis are designed to address the influence of domestic political pressures. They include the percentage of the population over 65 years of age, government party ideology, and the type of pension system. The age variable captures the reform pressure European societies face as a result of gains in life expectancy and declining fertility rates, raising the specter of massive funding shortfalls with which to finance rising pension liabilities.

From the financial cost perspective, one might expect the likelihood of pension reform to rise the higher the demographic pressure weighs on a given nation. Conversely, Pierson (1994) has shown that governments tend to resist cutbacks of mature welfare entitlement programs because retrenchment tends to be unpopular with voters. Thus, a "domestic audience cost" theory would predict that cutbacks of mature entitlement programs may prove too dangerous for reelection minded politicians. Through this lens, the likelihood of pension reform should decline with rising demographic pressure.

The population share of people older than 65 varies in the sample between 10.5 percent (Portugal in 1980) and 17.9 percent (Sweden in 1986). Most countries experienced a steady increase of the share starting in the early 1980s and extending throughout the sample period. Two exceptions are Sweden and Norway, which are the only countries that saw the share of over 65 year olds decline over long time periods, starting in Sweden after 1986, and after 1990 in Norway.

The ideology variable captures the influence of party ideology on structural pension

[8]This is somewhat incorrectly suggests that these countries just entered the analysis, and have the chance of experiencing another reform down the road. We are facing a trade-off here, because we model learning, and therefore cannot follow standard practice and discard observations from the analysis after reform first occurred.

reform. It is a dummy variable that takes on a value of 1 for left leaning governments, and 0 for right leaning governments (Keefer 2004). In the sample period, right leaning governments slightly outweighed left leaning governments, with a share of 59.6 percent. Looking at individual countries, we have cases of great ideological stability and those with frequent turnover. Luxembourg had a center-right government from 1980 to 2000, the year it implemented pension reform. In contrast, Italy swung between left and right leaning governments five times before it implemented reform in 1995.

Of the 16 countries in the sample, only four countries (Ireland, the Netherlands, Switzerland, and the UK) correspond to the Beveridgean model. Austria is the only country with a Bismarckian pension system that implemented reform prior to the signing of Maastricht. The other four early reformers – Ireland, Switzerland, and the UK – all have mature occupational and private pension systems.

While pension reform tends to be politically risky, we expect Beveridgean countries to have an easier time implementing reforms vis-a-vis their Bismarckian counterparts. First, the demographic time-bomb is ticking less loudly in Beveridgean systems because the low levels of public pensions keep future costs under control. Secondly, moving further down the path of privatization is considerably easier when people already expect to save for their own retirement, knowing that the low public pension won't secure an adequate standard of living during the retirement phase. Pension privatization efforts in Bismarckian systems, however, tend to be politically toxic because of the double payment problem – at least one cohort will be asked to pay for the public pensions of their parents' generation and for their own.

3.2 Results

We estimate our model using Gibbs-sampling and an uninformative prior. The parameter ρ has a non-standard conditional distribution. To deal with this, we embed a Metropolis-Hastings algorithm within the Gibbs sampler (Le Sage 2000). Because of the connectivity

structure, the MCMC algorithm turns out to be computationally intensive. Below we present preliminary results that are based on three markov-chain runs with 40000 iterations, each of which took about 2 weeks to complete. Standard convergence diagnostics ensure that the main variables in our analysis have reached a stationary distribution. However, because of inefficiencies of Metropolis-Hastings sampling, the learning parameter ρ still shows incomplete mixing behavior, which also affects some of the time dummies. Our results with regard to learning are therefore still preliminary in nature. It should be noted though that all three chains produced negative estimates for ρ that were very similar in magnitude.[9]

When reporting results from the analysis, a first look can reveal the sign of coefficients and the degree of statistical certainty. However, to establish effect sizes we need to account for the binary nature of the dependent variable and the endogeneity of reform. Endogeneity means that the decision to implement reform in a given country is not just a function of the values of coefficient estimates and covariates in this country, but through learning processes also a function of the values of covariates in all other countries. This makes the usual approach not applicable, in which the researcher presents counterfactuals for a hypothetic country with median or mean values on all variables except for those of interest. Instead, we construct counterfactuals for a number of countries, and use true variable values for all other countries. We then generate estimates of the marginal effects for changes in the variables of interest.[10]

Table 2 shows the results from the analysis. A first glance shows that the routine recovers a positive effect of joining the Maastricht treaty. But the interaction term reveals that this effect diminishes with increasing debt levels. Learning effects are statistically significant and negative, i.e. observing governments of similar political leanings implement reform decreases

[9]The chains were started from different starting points.

[10]These do not reflect the conditional effects arising from the changes induced through the connectivity. While theoretically feasible, obtaining these quantities is currently computationally very expensive (Franzese *et al.* 2010).

Table 2: Spatial Probit, Posterior Summary: Pension Reform

Variable	Median Coeff.	95% HPD Interval Lower Bd.	Upper Bd.
Maastricht Signatory	**4.49**	2.86	6.59
Signatory × Debt	**-0.0278**	-0.0539	-0.00447
Funded System	**2.46**	1.48	3.55
Left Government	0.157	-0.817	1.16
Right Government	0.633	-0.455	1.79
Population over 65	**0.714**	0.447	1.03
Government Debt	0.0142	-0.00374	0.0335
Learning	**-0.154**	-0.0219	-0.0864

Gibbs repetitions: 40000, burn-in: 10000, n=480, **bold** coefficients indicate that zero is not contained in 95% HPD interval.

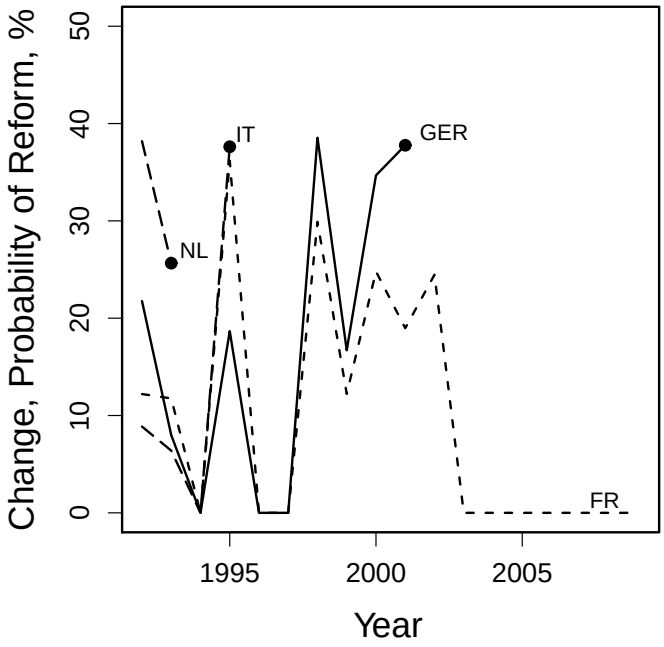
peer nations' willingness to try the same. As expected, funded pension systems reform earlier and aging populations increase the chance of reform. In line with the mixed results in the literature, we do not find an effect of party orientation on the likelihood of reform. Let us next look at these findings in more detail.

Our core argument that external pressures stemming from the Maastricht treaty increase the drive for reform finds solid support in the analysis. Figure 1 shows the effect of treaty accession on the probability of reform for our example countries. These effects were calculated using the actual data for the countries in the analysis, except for the treaty dummy variable. The graph shows that the status of being a signatory added about 40 percentage points to the probability of reform in the years that Italy and Germany reformed, and about 25 percent for the Netherlands.[11] France is a tough case as it is somewhat counterintuitive. It did not introduce major reforms with a capital component until 2010, outside the time frame of our analysis. Insterestingly, our model suggests that Maastricht exerted reform pressures on the French government throughout the 1990s and until about 2003, after which the treaty

[11]Germany and Italy are good examples of Bismarckian pension systems with high and low debt levels, respectively. The Netherlands is one of the only two Maastricht signatories with a funded pension system. Ireland, the other signatory is not sufficiently economically developed to make for a good comparison.

shows no more effect. In this period, the Balladur reforms of 1993 and the Fillon reforms of 2003 changed the pension formula and the indexation mechanism. However, both reforms fell short of introducing a capital based component to the system.

Figure 1: Effect of Maastricht Treaty on Probability of Pension Reform



Maastricht does not have a statistically significant effect for all countries in the analysis. Table 3 shows effect size and 95 percent probability bounds in the year of reform for each country in the sample. Joining the treaty adds between 2.05 percentage points (Finland) and 43.1 percentage points (Portugal) to the probability of reform. For Italy, the effect of Maastricht on reforms is not statistically significant. This is also true for Belgium. Austria and Ireland reform before Maastricht, while France and Greece don't implement reforms in our sample period.

The lack of statistical certainty in the case of Italy illustrates the role of government debt for pension reform. We theorized that membership in the Maastricht treaty could either influence pension reform through the policy pressures brought about by high debt loads, or

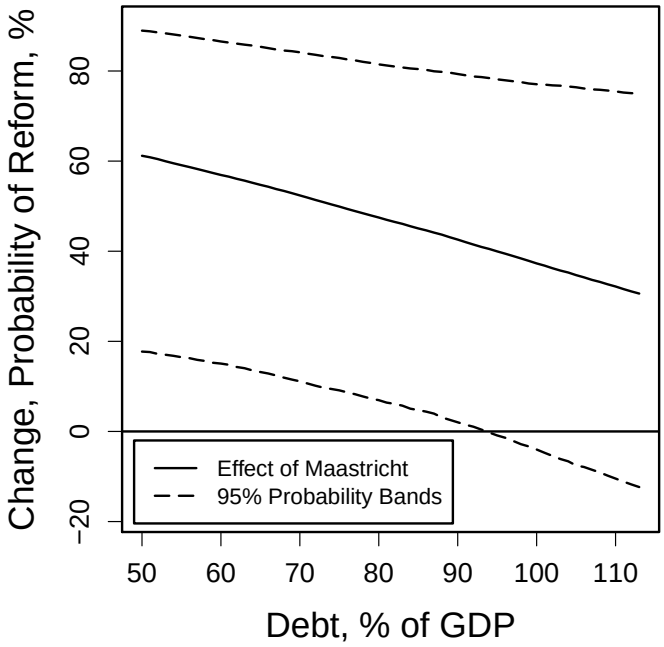
Table 3: Effect of Maastricht Membership in Reform Year

		Effect	95% Bands	
Country	Year	percent	Lower Bd.	Upper Bd.
Austria	1990	n.a.	—	—
Belgium	1995	10.3	-0.0780	44.9
Finland	1992	2.05	0.0464	15.3
France	—	n.a.	—	—
Germany	2001	38.0	4.31	76.8
Greece	—	n.a.	—	—
Ireland	1990	n.a.	—	—
Italy	1995	29.8	-13.4	74.2
Luxembourg	2000	11.1	0.346	56.8
Netherlands	1993	24.9	1.46	75.2
Norway	—	n.a.	—	—
Portugal	2002	43.1	2.67	85.8
Spain	1999	26.4	1.61	75.3
Sweden	1998	n.a.	—	—
Switzerland	1985	n.a.	—	—
UK	1988	n.a.	—	—

by systematically selecting countries in good economic shape into crafting the treaty that would find reform less costly. Our findings produce strong support for the latter hypothesis. Looking again at Italy in figure 2, we see the effect of treaty accession on the probability of pension reform in 1995 (the year of reform), conditional on debt levels. Italy's true debt stood at 113 percent of GDP. We show counterfactual values ranging down to 50 percent. The difference is remarkable. The effect of Maastricht increases from somewhat below 40 percent to over 60 percent, a 20 percent increase. Also, the 95 percent probability bounds cross the origin, thus making the effect statistically significant for – counterfactual – lower debt levels. The overall evidence suggests that pension reforms in light of Maastricht were mainly due to the political and economic strength of those countries that had crafted the treaty to serve their interests.

Next we look at learning effects. To gauge the effect size we look at the Netherlands as one of the early reformers after the treaty was signed. Figure 3 shows the predicted

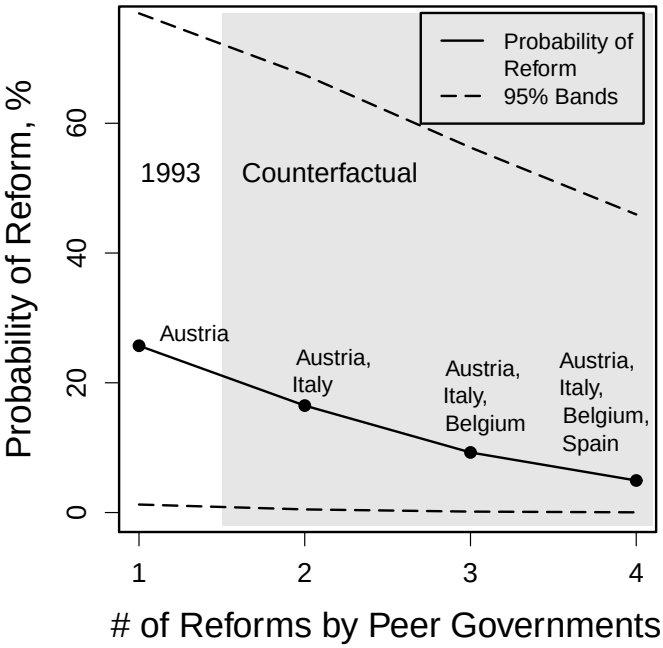
Figure 2: Italy in 1993, Effect of Maastricht Treaty



probability of reform in 1993 at the left, using actual data for all countries in the analysis. The Netherlands had a left leaning government at the time. Among all other countries with left governments, only Austria had reformed. We now show how the predicted probability of reform changes as the number of reformed countries with left governments increases. We first code Italy as having reformed.[12] The negative learning effect from this additional case translates into a 9 percentage point drop the in Netherland's probability of reform, a decrease by more than a third. Adding Belgium and then Spain as additional reformed countries results in further drops of 7 percentage points and 4 percantage points. Thus, with three additional reformed countries in the sample, the Netherland's predicted probability of reform drops overall from 25 percent to 5 percent. This shows that the effect of political learning is sizable and substantively important.

[12]To achieve this, we directly change the latent variable $y^* = 10$ for Italy . This value is high enough to ensure that the predicted probability of reform for Italy approaches 1.

Figure 3: Predicted Probability of Reform in Netherlands



Turning to domestic variables, funded systems are substantively more likely to reform than non-funded system. Looking at the example of the UK reforms in 1988, our model attributes 52.2 percentage points of the overall predicted probability of reform of 53.1 percent to the funding type of the British pension system. For the Netherlands, the impact of having a funded system is similarly big, with almost all of the 25.7 percent predicted probability of reform attributed to its funded pension system. It should be noted that in the case of the Netherlands the effects of having a funded pension system and joining Maastricht are heavily interactive, due to the non-linearity of the probit specification. Each of the two variables can account for almost all of the predicted probability of reform, but only in the presence of the other. Without joining Maastricht, our model predicts that the type of pension system would have a negligent effect on the probability of reform (an increase of 0.0253 percentage points from a basis of 1.35×10^{-9} percent). Vice versa, absent the funded system, the predicted effect of Maastricht is diminishingly small (an increase of 0.08 percentage points

from the same basis). Our analysis therefore provides an answer to why the Netherlands waited much longer than other European countries with mature private pension sector. It took the impulse from Maastricht to implement reform.

Finally, we find that demographic aging exhibits the well-known accelerating effect on pension reform. To gauge effect sizes, consider the country that had the oldest population at the time of reform, Sweden in 1998 (17.3 percent of population over 65 years of age). For each percentage point increase of the population over 65, the model predicts an increase of 3.36 percentage points in the probability of pension reform (with 95% bounds of 0.068% and 22.7%). The numbers are much higher for the country that had the youngest population at the time of reform in the sample, the UK in 1988 (15.5 percent over 65). The marginal effect of a one-point increase in the old age demographic is associated with a 25.6 percentage points increase in the probability of reform (the 95% lower bound is at 3.55 percent, the upper bound at 34.9 percent). The latter example shows that aging plaid an important role for reform in some countries, but not in others. It is beyond the scope of this paper to explore this variation in detail. However, in light of our previous discussion of the overwhelming role of pension system for the reforms in the UK, we have evidence for an interaction effect between aging and the pension system. A younger demographic profile would not only have made reform less likely directly, but also decreased the role of Britain's funded system for early reform.

Overall, our analysis has produced strong support for the hypothesis that pension reforms in western Europe in the 1990s and early 2000s were driven by external factors. Joining Maastricht and preparing for EMU clearly accelerated the drive towards reform. We were also able to tease out the causal pathway trough which this external influence happened. Since Maastricht served as catalyst for reform only for countries with relatively low debt loads, explanations that point to the structural and macroeconomic policy dimensions of monetary union fail. Instead of a common shock, there seems to have been a joint movement

by the strongest countries in Europe to set up the single currency at a time that was both propitious for building international cooperation and attempting domestic reform.

A similar outcome resulted from our analysis of learning effects. Instead of imitating policy innovations, governments primarily appear to have internalized political lessons. Observing another government of the same political colors implement reform made leaders more hesitant to incur the political costs of reform themselves.

4 Conclusion

Existing research on structural pension reform has made important contributions in pinpointing mechanisms through which policy choices may be causally connected. Close correlations in the timing and location of pension reforms have been variously explained by domestic pressures (demographic aging and financial cost of social security systems), or policy diffusion (learning or emulation). Missing in the literature is an argument about how common shocks may affect the likelihood of pension reform. We address this gap by developing a measure of how the debt and deficit constraining criteria recorded in the Maastricht treaty, which was signed by twelve member states in 1992, may be causally related to the observed pension regime transitions.

Existing studies are limited because they fail to consider the possibility of common shocks influencing pension reforms. Secondly, few studies systematically test for more than one causal pathway towards pension reform (but see Brooks 2005, 2007). This is problematic because inadequate modeling of interdependence may lead scholars to exaggerate the effect of common shocks, while failure to control for shocks may induce analysts to overestimate the role of diffusion dynamics and domestic exigencies. Using a spatial probit setup within an event history analysis, this study is the first that systematically tests all three causal pathways towards reform – whether pension privatization efforts in Europe are the result of

domestic factors, the diffusion of pension reform ideas, or the constraints of the Maastricht treaty stipulations. Our findings indicate that all three factors contributed to policy change, but direction and effect size vary across countries and are conditional on the presence or absence of other independent variables.

Being a signatory to the Maastricht treaty increases the likelihood of pension reform, but only for low-debt countries (i.e., Germany, the Netherlands, Finland, Portugal, and Spain). This finding is counterintuitive because the very purpose of the Maastricht treaty was to induce fiscally profligate member states – like Italy and Belgium – to lower their debt and deficit levels, of which unfunded pension liabilities are the largest component. Although Italy and Belgium *did* reform their pension systems in the 1990s, it was not the Maastricht criteria that propelled this propitious development, but domestic policy considerations. Thus, while we can show that the Maastricht treaty is causally related to the observed pension reforms, our estimation procedure helps us avoid bias towards overstating this effect.

Our analysis also shows that the European countries learned from each other, but this process was not characterized by policy imitation. On the contrary, it appears that governments were keen on avoiding the mistakes of others, which may be labeled "negative learning." The implementation of unpopular pension reforms indeed can cause electoral backlash. During the 1990s, coalition governments in Germany, France, and Italy all lost elections because voters resisted their controversial pension reform programs. Peer nations that did not reform evidently learned that pension privatization efforts can be politically toxic and decided to avoid a similar fate. The implication is that future research should pay closer attention to the various channels of policy learning. Observers of limited diffusion dynamics may erroneously conclude that policies fail to "catch on" in other locations when in fact negative learning from other states' failure may better explain policy outcomes.

Finally, we find that demographic aging and concomitant concerns about the cost of social security systems also have an accelerating effect on the likelihood of pension reform,

but not in all countries in our sample. While aging had a particularly strong effect on the hazard of pension reform in the Netherlands and Britain, pension reforms in other countries were driven by the desire to fulfill the Maastricht criteria. However, aging only increased the hazard of pension reform in conjunction with the nature of the pension system in these countries (in our cases, a mature occupational and private pension sector). Each effect taken by itself fails to account for the observed pension regime transitions. While few doubt that the combination of "demographic time bombs" and aging voters resisting pension regime transitions can have explosive societal consequences, it is important to identify how and when these forces are causally linked to policy change.

Our analysis also has implications for democratic accountability and EU integration more generally. In light of the distributional impact of pension policies on the life-course risks of domestic constituencies, political incumbents may not escape electoral backlash, no matter whether pension reforms reflect political preference or a reluctant response to external shocks. Future research may productively investigate under which conditions member states are capable of absorbing EU policy shocks that have distributive consequences. Considering the desperate attempts of political leaders to contain the damage arising from the eurozone's sovereign debt crisis and controversial debates about a two-speed Europe, interest in measures to sustain EU policy coordination is likely to continue in the years to come. A necessary step towards understanding such questions, however, is to first establish the causal pathways of policy change and the effect size of each mechanism.

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