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Social Security and Democracy

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Abstract

Many political economic theories use and emphasize the process of voting in their explanation of the growth of Social Security, government spending, and other public policies. But is there an empirical connection between democracy and Social Security program size or design? Using some new international data sets to produce both country-panel econometric estimates as well as case studies of South American and southern European countries, we find that Social Security policy varies according to economic and demographic factors, but that very different political histories can result in the same Social Security policy. We find little partial effect of democracy on the size of Social Security budgets, on how those budgets are allocated, or how economic and demographic factors affect Social Security. If there is any observed difference, democracies spend a little less of their GDP on Social Security, grow their budgets a bit more slowly, and cap their payroll tax more often, than do economically and demographically similar nondemocracies. Democracies and nondemocracies are equally likely to have benefit formulas inducing retirement and, conditional on GDP per capita, equally likely to induce retirement with a retirement test vs. an earnings test.

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I. Introduction

Assisting the elderly has captured much of the attention, and resources, of the government in recent decades. Public pension expenditures are a big component of that assistance and, for example, have exceeded 10% of GDP in several countries.¹ Opinions can differ as to whether governments have assisted the elderly enough, or in the right forms, or how Social Security should adapt in the future. There is little question, however, that Social Security is a highly political component of government policy and that political careers have been made and lost on the basis of an official's (or a political candidate's) stance on Social Security questions.

The inseparability of Social Security and politics has motivated a number of political-economic theories of the emergence of growth of public pension spending. Although there are differences among the various political economic theories, the institution of voting is at the center of nearly all of them. For example, Browning (1975) models voting cohort-by-cohort, and argues that the political support for the elderly derives from a majority voting coalition of the old and the middle aged.² Tabellini (1992) models a majority voting coalition of the old and poor. Because so many positive economic models of social security (and other aspects of government policy) put the institution of voting at center stage, we believe that it may be helpful for economists to revisit the question, originally posed by sociologists, of whether there is in fact any obvious empirical connection between public pension policies and the institution of voting. The economic theories have implications for not only to the amount spent on Social Security, but the rules for collecting taxes and disbursing benefits. Our second reason to revisit this question is that the

¹International Labour Organization (various issues); data for the 1990's. For example, public pension spending/GDP exceeded 0.1 in Austria, France, and Italy.

²Cooley and Soares (1999) update this argument using modern dynamic game theory.

economic literature has accumulated a number of new and relevant country-panel data sets.³ These new data include the Penn-World Tables, which allow for better cross-country comparisons of standards of living. Mulligan and Sala-i-Martin (1999a) have assembled a large country panel data set of measures and indicators of the *design* of Social Security systems, which permit us to explore not only Social Security spending differences between democracies and nondemocracies, but also differences in the use of payroll taxes, retirement tests, means-tests, etc. Over time, there have also been detailed country case studies of the design of Social Security programs (such as those collected by Gruber and Wise (1999)), which allow for further quantitative comparison of various countries' Social Security systems.

In Section II of the paper we argue that positive theories of social security that are based on voting models predict that the size and the design of social security programs should be different in democratic and non-democratic countries. We also argue that efficiency theories of social security do not make such a prediction. Section III describes the political, economic and demographic data we use. Section IV presents the main results of our regression analysis, showing how democratic and non-democratic regimes do not differ much when it comes to the size and the design of social security programs. Section V presents nine case studies. Section VI concludes.

II. Why Democracy Might Matter, or Not

Many theories of Social Security have been proposed in the literature. In this section we argue that many political economic models built on voting are distinct from positive theories built on economic efficiency, in that the voting-based theories presume that democracy leads to different program design, increases Social Security budgets, and enhances the link between age or income distribution and Social Security spending.

II.A Public Decisions by Voting: Democracies are Different

Some politiceconomic studies of Social Security, and redistribution more generally, have featured

³And time has passed, so the previous data sets have expanded significantly, and improved in quality.

the institution of voting in their explanation of the emergence and growth of Social Security. Many of these studies do not mention whether nondemocratic governments should be expected to have Social Security, but since they use and emphasize voting in their explanation of the emergence and growth of Social Security, they implicitly assume that Social Security would be less likely to emerge and grow without democracy.⁴ An important reason why there can be Social Security, and other redistribution, in the voting models, is that votes do not express intensity of policy preferences, so that large groups can be subsidized at the expense of smaller ones, even if the redistribution has large aggregate net costs. For example, Social Security is politically successful in Browning's (1975), Cooley and Soares' (1999), and Nataraj's (2001) voting models because the old and the middle aged form a majority voting coalition which cannot be defeated by the young regardless of the intensity of costs they bear. As emphasized by Tabellini (1992), the skewness of the distribution of taxable income can be an important determinant of Social Security in a voting model, because it measures the amount that the old can gain by forming a coalition with the poor.⁵ Hence, the models not only suggest that democracies should spend more on Social Security, but that the largest democratic programs should be those in countries with the most skewed income distribution. Furthermore, since obtaining a majority is so critical in a democracy, Social Security spending should be especially sensitive to the size of the elderly population in a democracy.

Olson, McGuire, and Niskanen have a series of theoretical papers⁶ comparing the economies and policies under dictators and democracies. They do not explicitly model the voting process but, when it comes to the democracies, explain how they follow Meltzer and Richard (1981) and other previous authors who explicitly model majority voting over broad-based income taxes to finance transfers to a majority of

⁴After all, why should voting be featured in a model if it were not relevant for the question at hand?

⁵More precisely, the average taxable income in the economy determines the amount of revenue that can be raised from income taxes, and the median income determines the amount of taxes that the median voter would lose by siding with the old in favor of an income redistribution scheme. The ratio of mean to median income is therefore not only a measure of income distribution skewness, but also an indicator of the net gain from redistribution to the coalition of old and poor. See also Meltzer and Richard (1981) or Alesina and Rodrik (1994) for models of the links between redistribution and income distribution skewness.

⁶For example, Olson (1993), Olson and McGuire (1996), and Niskanen (1997).

the population. Nondemocracies, in their view, are different because the transfers go to a relatively small group – namely, the dictator and his friends – and because the taxes are not so broad-based that the dictator and his friends have to pay them. Because the democratic public decision-maker (the median voter) is required to pay a share of the taxes, and the dictator and friends do not, the dictator acts as a leviathan – taxing up to the point where tax base shrinkage is so severe that no additional revenue can be raised – and has a larger budget than a democracy would. Because a democracy’s tax base does not *have* to be broad based, and dictators may not be able to fully escape their own taxes, we doubt that the breadth of taxes is necessarily a fundamental difference between democracies and nondemocracies. Even with the hypothesis that democratic, and not nondemocratic, decision makers are liable for taxes, Olson, McGuire, and Niskanen’s models may support the prediction that *Social Security* budgets would be larger in a democracy, unless nondemocratic Social Security programs were to benefit only a small group of the dictator’s allies. We can investigate this final caveat empirically by studying the design of Social Security in particular countries in more detail and by looking at the likelihood that a democracy vs. nondemocracy means tests Social Security benefits or uses broad based payroll taxes.

In a democratic model like Tabellini’s, Social Security serves in part the purpose of redistributing from rich to poor. Obviously, such a purpose is ill served if the payroll tax is “capped” so that the payroll tax rate applies only on the first x units of a person’s earnings, where x is the “cap”, and a zero rate applies above that. For this reason, we might expect democracies to be less likely to cap their payroll taxes. The Olson, McGuire, and Niskanen studies may also suggest that dictators would be capping their payroll taxes, at least if the very rich were among the dictator’s allies.

II.B Efficiency Theories of Government: No Systematic Democracy Effect

A number of positive theories of public policy ignore politics all together, and suppose that observed public policies are those enhancing economic efficiency. According to this approach, the key explanatory variables are economic and demographic ones, since those are some important variables determining efficiency.⁷ Conversely, political factors – such as the mechanism by which public decisions

⁷There are a variety of reasons to expect that economic variables like the level of GDP, and demographic variables like the age-composition of the population would determine (or be associated

are made – are presumed to be much less important determinants of public policy. One of many examples of this approach is Barro (1979), who builds a positive theory of the public debt by suggesting that it is efficient for tax distortions to be smoothed over time and showing what kinds of public debt policy would achieve that smoothing. Hence, he argues that the timing of government expenditure, and the state of the economy, are the key determinants of the amount and growth of government debt. Because he emphasizes the economic variables, Barro downplays the importance of the institution of voting (or other political institutions) for determining the public debt, unless perhaps those political factors were otherwise determining the key economic variables: the state of the economy and the timing of government spending.

In the field of Social Security, Sala-i-Martin (1996) builds a positive theory based on economic efficiency. He is quite explicit (eg., p. 288) about his claim that efficiency is the reason for the program, so that we expect no Social Security difference between democracies and nondemocracies once we understand what are the economic determinants of efficiency. Furthermore, since Sala-i-Martin emphasizes the life cycle of human capital and the age-composition of the labor force in describing economic efficiencies, his theory suggests that richer and older countries should spend more on Social Security.⁸ Pogue and Sgontz (1977), Laitner (1988), and Becker and Murphy (1988) describe elderly care activities and investments in youth that traditionally occur in a family context, but in more modern economies might be provided as well or better by the government. In other words, they view Social Security as a reaction family activities and, unless family activities themselves depend on the process by which public decisions are made, do not offer a prediction as to how Social Security might be different in democracies and nondemocracies. Diamond and Mirrlees (1978) and Merton (1983) describe Social Security as optimal

with) which public policy is most efficient. See, for example, Mueller's (1989, chapter 17) review of government growth theories. See also below.

Another literature (eg., Wittman 1995) has argued that democratic institutions are efficient. This literature does not always spell out in detail what are the variables that determinant efficiency, or whether nondemocratic political institutions are also efficient, but their arguments do suggest that the better positive theory of public policy is built on efficiency, not on political factors.

⁸See Mulligan and Sala-i-Martin (1999b) for further exposition of efficiency theories of Social Security.

insurance.

The main lesson is that the theories that explain social security programs as an optimal response to economic inefficiencies do not predict size or design differences among programs depending on whether they are in democratic or non-democratic countries once the measures of the relevant inefficiencies are held constant.

II.C Non-Voting Political-Economy Models: No Systematic Democracy Effect

Becker (1983), Becker and Mulligan (1998) and others pay some attention to the political process in their building a positive theory of public policy, but nevertheless emphasize the role of efficiency. Becker and Mulligan argue, for example, that political processes may permit more redistribution when it is economically efficient, that determinants of the amount of efficiency of that redistribution (such as the instruments available for tax collection) are important variables for understanding why some government redistribute more than others, and why government redistribution has grown over time. Mulligan and Sala-i-Martin (1999a) build a positive theory of Social Security, and also pay some attention to the political process. They emphasize economic and demographic determinants of political influence (in particular, the labor force status of the elderly), rather than the mechanism by which public decisions are made or how economic and demographic variables might interact with those mechanisms. Hence, the models of Becker, Becker and Mulligan, and Mulligan and Sala-i-Martin say that, after holding constant the determinants of efficiency and the economic determinants of political influence, Social Security programs should look similar in democratic and nondemocratic countries.

The size of the elderly population is one important determinant of the efficiency of subsidizing the elderly because, among other things, the deadweight costs of Social Security taxes increase with the size of the budget. By itself, this effect suggests that the share of GDP going to Social Security should increase with the fraction of the population above certain age, but less than one-for-one (see also Turner 1984). However, political influence may also increase with group size (in both democratic and nondemocratic regimes), so that the elderly are more powerful when they are more numerous. In either case, these approaches imply that economic and demographic variables will affect Social Security spending, and are ambiguous about the effect of democracy *per se*.

II.D Theories of the Preferences of the Democratic Citizenry

Each person may have a preferred way of running the Social Security program, and these preferences may express a lot more than his personal gains and benefits from the program.⁹ Furthermore, these preferences may vary over time and across countries in a way that is determined by, or correlated with, democracy. It has long been argued that the institution of democracy affects the preferences of its citizens. De Tocqueville (1835) has some of the most well known of those arguments where he suggests, for example, that more patriotism can be expected from citizens when “everyone... takes and active part in the government of society” (Chapter 14). Sen (1999, p. 9) predicts that the discussion and debate associated with democracy “are central to the process of generating informed and considered choices and ... crucial to the formation of values and priorities...” When democracies affect the preferences of citizens in these ways, and the preferences of citizens affect policy, we might expect democracies to have different Social Security programs. However, without a more detailed model of these effects, we cannot say whether democracies would have smaller or larger Social Security budgets, be more or less likely to means-test their program, etc.¹⁰

It may not be the case that democracy causes citizens to have different preferences, but democracy may nonetheless be correlated with citizen preferences. It has, for example, been argued that citizens in democracies find violence more distasteful than citizens of nondemocracies, and this distaste affects the military policies of their governments.¹¹ And it has been found, in fact, that democracies do have different military policies. Perhaps a related argument would apply to Social Security, with citizens in democracies having systematically different preferences for Social Security than citizen of other countries. However, we are not aware of a more detailed model that might tell us whether democracies would have smaller or larger Social Security budgets, be more or less likely to means-test their program, etc. Nor is it clear how the

⁹Some economic models emphasizing preferences as determinants of Social Security include Diamond (1977), Feldstein (1985), Laitner (1988), Mulligan and Philipson (1999) and Profeta (2000).

¹⁰Brennan and Lomansky (1983) and Becker and Mulligan (1999) have argued that people should be expected to act less selfishly when voting than they do in private affairs, so perhaps their arguments could be extended to argue that democracies would spend more on Social Security?

¹¹See de Mesquita and Lalman (1992, Chapter 5) and Elman (1997, Introduction) for surveys of some of these arguments.

size and design of Social Security programs might be correlated with democracy once we hold constant various proxies for the tastes of citizens, such as their age and their incomes.

II.E “Copycat” or “Imitation” Models

It could be argued that non-democratic systems look similar to democratic ones because they have incentives to imitate them. For example, nondemocratic governments could be threatened by, and therefore imitate, similarly situated democratic governments. Perhaps potential revolutionaries are interested in the effect of regime change on public policy, and are more likely to revolt when they suspect that a democratic regime might offer them policies that are significantly different. Nondemocratic governors recognize this, and choose policies similar to those in democracies so that potential revolutionaries among the governed might not expect significant policy changes to result from a regime shift. For example, if growing inequality causes democratic governments to expand Social Security, it might also expand Social Security in nondemocratic countries because the latter countries are trying to look democratic. According to this view, the observed democratic-nondemocratic policy gap is smaller than the true effect of democracy (by which we mean the effect of introducing democracy into a country lead by an unthreatened nondemocratic regime).

It could be argued, however, that democratic governments are more likely to be imitators of neighbor governments. In this view, democratic governors are accountable to the citizenry who are, on average, amateurs when it comes to public policy evaluation. Perhaps an easy way for a citizen to detect a bad policy is one that differs significantly from those used in similar countries. Democratic governors know this, so they try not to deviate too much from their neighbors.^{12,13}

We offer two means of gauging the importance of inter-governmental imitation for interpreting our results. First, we can look at the spatial correlation of our Social Security spending and design measures.

¹²Case, Hines, and Rosen (1993) propose a model like this for state governments within the U.S., but do not discuss whether or not democracies are more likely to be “copycats.”

¹³Neighboring governments may also use similar policies in order to reduce incentives for migration in and out of the country. Democratic governments might be more sensitive to this motive than nondemocratic governments are, if nondemocratic governments are more likely to prohibit migration.

Second, the case studies may reveal some historical analyses of the motivation of nondemocratic governors, including how and whether they imitated democratic neighbors.

II.F Implications for Country-Correlations

In summary, we have argued that many voting models of Social Security are different from positive theories built on preferences or economic efficiency, in that the voting-based theories presume that democracy leads to different Social Security program design, increases Social Security budgets, and enhances the link between age or income distribution and Social Security spending. The economic efficiency approach presumes that voting, and other political institutions are relatively minor determinants of the program size and its design. Preference-based approaches might be consistent with differences between democracies and nondemocracies, but to date do not predict the nature of these differences. “Copycat” or “imitation models” suggest that Social Security spending will be spatially correlated even when we control for economic, demographic, and political variables.

All of these approaches have implications for the amount spent as well as for *how* this money is spent. If, for example, Social Security is intended to alter the operation of the labor market, then we expect revenues to be collected and disbursed in such a way to affect the behavior of employers and employees. Or, if it is intended to redistribute from rich to poor, then taxes and benefits should be administered in a “progressive” way. These administrative considerations are connected to a study of democracy and social security because administrative differences between democratic and nondemocratic governments can tell us whether these two types of governments differ in their motives for having Social Security programs.

III. Our Data

III.A. Measures of Social Security Program Spending and Design

Our study includes three types of variables:¹⁴ those that measure the size and design of SS programs around the world, those that estimate the degree to which a country is democratic, and economic and demographic variables. Our main sample consists of 90 countries with available measures of democracy, the fraction of the population aged 65+, (ppp adjusted) real GDP per capita, and “sufficient”

¹⁴ Summary statistics for the variables are shown in the Appendix.

information on public pension spending.

We have several variables that estimate the size and design of Social Security programs. One such measure is annual public spending on old age pensions, as a share of GDP, and as reported by the International Labor Organization (hereafter, ILO).¹⁵ The ILO reports that public pension spending is distinct from “family allowances,” “unemployment benefits,” “employment injury benefits,” and “sickness-maternity benefits.”¹⁶ We have utilized ILO reports for the years 1960-90, with two exceptions.¹⁷ It is also important for our purpose that the ILO reports are comparable for a pretty broad range of countries, because most of the nondemocracies since 1960 are outside the OECD. Henceforth, we refer to these ILO public pension spending data interchangeably as “public pension spending” and “Social Security spending.”

There are 128 countries reporting some public pension spending in at least one of the years 1960-90, plus 22 additional countries for which we know that no Social Security program existed during at least

¹⁵In its publication *Cost of Social Security*, the ILO reports spending by “Social Security and Assimilated Schemes” and “Family Allowances” in national currency units. It also reports pension spending as a fraction of spending by “Social Security and Assimilated Schemes” and “Family Allowances”. To calculate public pension spending/GDP, we take the product of these two reports, and divide by the GDP reported by the ILO in national currency units.

¹⁶Our research has shown that, for OECD countries where more detailed country-comparable data is available since 1980, ILO reports are very similar to OECD calculations of spending on public old age, disability, and survivor pensions, exclusive of pension schemes for civil servants. We have the impression that the data for nonOECD countries is pretty accurate, although we have not conducted a systematic analysis of this point.

¹⁷The two exceptions are for Italy and Spain, and derive from our case studies. ILO measures of pension spending/GDP for Italy fluctuates wildly from year to year and, in preparing the Italian case study, we obtained a public pension spending series from the Bank of Italy (we thank Alessandro Barbarino for helping us with this) that was very similar in definition to the ILO series, and with a very similar level and trend, but fluctuating much less from year to year. In preparing our Spanish case study, we found González-Catalá and Merino’s (1985) study, which has a Spanish pension spending series that is very similar to ILO’s, but includes the missing years 1967-74, so we use the González-Catalá and Merino (1985) series for those years.

some of those years.¹⁸ Of the 128, most countries have missing data for some of those years; we work with each country's averages for the periods 1960-90, 1960-74, and 1975-90, where the averages are calculated using whatever years are available for the country. 104 of the 128 countries report positive public pension spending in at least 5 of the years 1960-90 and on this criterion are eligible for inclusion in our main 90 country sample.

Spending is only one indicator of the nature and intensity of public support for the elderly. But, regardless of whether one looks at elderly support from an economic or political perspective, it is also relevant *how* Social Security revenue is collected and distributed. Based on reports of the U.S. Social Security Administration, Mulligan and Sala-i-Martin (1999a) have compiled a three year (1958, 1975, and 1995) cross-country panel data set of such indicators of Social Security design. Those indicators include whether there is a Social Security payroll tax how the payroll tax is shared between employer and employee, whether the payroll tax is capped, whether the elderly must exit employment to collect public pension benefits, whether benefits are earnings tested, or means tested, and whether benefits are credited for delayed retirement. Hence, we can address the question of whether democratic and nondemocratic governments administer their programs in similar ways, even when they spend similar amounts on them.

Although our spending and design numbers are of good quality, there are some missing observations and, even with all of the observations, it is difficult to reduce the variety of elderly subsidies to one or two numbers. For this reason, case studies are an important part of our analysis, since those studies do not require numbers that are comparable across a large number of countries – just the few

¹⁸Our Appendix shows which country-years are missing from the ILO pension spending data. Using the Social Security Administration's (1995) report of each country's Social Security program's first year, we have found that much of the missing ILO data derives from the fact that some countries did not have Social Security during each of the years 1960-90. We therefore fill in the ILO data with zeros for each year since 1960 and before the first year of Social Security (these years are also shown in the Appendix, are typically for African and Middle Eastern countries prior to 1975). Doing so has a minuscule effect on our regression results, because countries with young Social Security systems are spending practically zero in the years since their program began.

In general, the Appendix shows how we have nearly all years for European and North American countries, and for some Asian countries. The 1960's, and to some extent the 1970's, are missing for most of the other countries, including many for which we believe a Social Security program existed.

countries in the case study. Our case study analysis utilizes data from a variety of country-specific sources, so we do not have to reduce “Social Security” or “democracy” to one single number.

There are a few measurement issues which are particular to spending data, and may be correlated with democracy. First, democracies may have spending, GDP, and demographic data of higher quality. Better demographic measurement of the latter two variables suggests that we may observe greater sensitivity of spending to GDP and demography in the democratic countries, even when the actual sensitivity is the same under both types of regimes. Second, our spending variable does not include military pensions, and military pensions may be more prevalent (relative to public pensions for civilians) in nondemocratic countries, so we may understate the amount of public pension spending in nondemocratic countries. Third, nondemocratic governments may be more prone to create economic data that favors the government’s image. It is hard to say how this third issue would affect our estimates, since nondemocracies might exaggerate both GDP and the amount of assistance for the elderly and thereby not distort the ratio of the two.

III.B. Indicators of Democracy

We use three different sources of data for our democratic variables. The first source is the POLITY IV project which calculates for 181 countries going back as far as 1800, among other things, a democracy index taking integer values 0-10, an autocracy index taking interger values 0-10 (we divide both of these by 10 to put on a 0-1 scale), and a 0,1,2,3 indicator of the extent to which government executives are chosen through competitive elections (which we divide by 3). The democracy index includes as one component the elections indicator, plus rules for political participation, and the transfer of executive power. The autocracy index has the same components as the democracy index, but weights them differently (and negatively). The POLITY data are available for only 94 of the 104 countries for which we have sufficient Social Security spending data. The POLITY data are missing during years of occupation, political interruption, or political transition (eg., occupation by foreign powers, a collapse of central political authority, or an executive guided process of institutional planning).

In his international studies of economic growth (eg., Barro 1994, 1996), Robert Barro has made

a linear transformation of Gastil's (various issues) classification¹⁹ into a 0-1 scale. A value of 1 identifies the maximum level of democracy, a value of 0 identifies the lowest level of democracy, and values of .17, .33, .5, .67, or .83 identify intermediate cases. We have this measure for 1972, 1975, 1980, 1985, and 1990. For those years, it is highly correlated with the POLITY democracy index. Relatively speaking, the POLITY democracy index does little to distinguish among the least democratic countries; nearly all country-years scored 0-0.33 by (Barro's transformation of) Gastil are scored 0 by POLITY. Another difference between POLITY and GASTIL is that a few Latin countries are scored pretty high by Gastil and very low by POLITY.

The third source is Bollen(1980) and Bollen and Grandjean (1981), widely known as the Bollen democracy measure, which was used by a previous study of Social Security and democracy, Pampel and Williamson (1989). This measure is only available for the years 1960 and 1965 and for different samples of countries. Also, Bollen specifies his criteria to be a mix of the extent of determinate political factors such as press freedom, freedom of group opposition, government sanctions, fairness of election, executive selection and legislative selection. The Bollen measure seems to give a bigger weight to civil rights than does POLITY's.

Although the various democracy indices may be of good quality, it is difficult to reduce democracy to a single number. Hence one reason to extend the empirical analysis to case studies, as in our section V, is that case studies do not require numbers that are comparable across a large number of countries and can thereby rely more on descriptions of political situations from country-specific sources.

III.C. Economic and Demographic Variables

Public pensions are paid to old people, so it may be important to know how many people are old. For this purpose we use the share of population who is 65 years old or older. This variable comes from the ILO (1996) and is available only at ten year intervals. We construct each country's averages for the periods 1960-90, 1960-75 and 1976-90.

¹⁹Gastil and his colleagues classify countries in a range from 1 to 7, being the countries with 1 those where political rights are more developed and protected and those with 7 those where political rights are in the poorest conditions.

We expect some economic variables to affect the program regardless of the exact political model of Social Security, although these effects could be different for democracies and nondemocracies. Furthermore, we expect political institutions to be correlated with economic variables, so it is important to have good measures of the latter in order to better estimate the effect of democracy *per se* on Social Security. Fortunately, there has been significant progress in recent years in the measurement of some key economic variables. The Penn World Tables now report a broad cross-country panel of comparable indicators of standards of living, including real GDP per capita, which we utilize for the years 1960-89. Four of the 94 countries who have sufficient pension spending data and are included in POLITY do not have either real GDP or demographic data, so our main sample has 90 countries.

The shape of the income distribution is important for some of the voting-based theories of social security. We therefore utilize some of the recent improvements in the construction of cross-country comparable indicators of income inequality, and income distribution skewness. In particular we use data elaborated by Deininger and Squire (1996) to obtain multiple income distribution measures of good quality for a broad cross-section of countries.²⁰ Specifically, we use their Gini coefficient and the share of income held by the 40% richest share of population (created by subtracting the third quintile from their data from 1). We also follow Barro (1998) and use the Gini coefficient for educational attainment as another indicator of inequality.

IV. Regression Analysis

We now analyze empirically the relationship between the democracy index, pension spending, and pension program design in a couple of broad cross-sections of countries.

IV.A. Results from our 1960-90 Cross Section

IV.A.1. Evidence on the Amount of Social Security Spending

The correlation between democracy and social spending is displayed in the first column of our

²⁰See Deininger and Squire for some explanation of how their income distribution measures are most often derived from comprehensive coverage of the population, and comparable concepts of income and expenditure. See Atkinson and Brandolini (2001) for a critical view of the Deininger and Squire data set.

Table 1, where we regress time-averaged public pension spending over GDP on the time-averaged democracy index in our main sample of 90 countries. Since our democracy index is on a 0-1 scale and Social Security expenditure measured as a percentage of GDP, the coefficient of 3.71 indicates quite a large difference between a totally democratic country and a totally nondemocratic country²¹ – 3.71 percentage points of GDP.

This result is not new. Jackman (1975) is an early empirical study of social spending policies and political performance for a sixty-country sample in 1960. In several chapters of his book, he examines the effect of democracy and political stability on three different rough measures of social equality: SIPE (Social Insurance Program Experience, which for each country can be interpreted as the number of years since their SS program was created),²² the Schultz coefficient and a Social Welfare Index. Since we are looking at the effect of democracy on the size and design of Social Security programs, Jackman's SIPE estimates are the relevant ones for this study. Measuring democracy following the criteria set forth by Dahl (1956), Downs (1957) and Lenski (1966), he finds a strong positive correlation between SIPE and the democracy index.²³ However, we explain in some detail below why he and others do not interpret this correlation as a democracy effect.

Pampel and Williamson (1989) study a 32 country panel for the years 1950-80, using social spending measures and the Bollen democracy index. They show how democratic governments have larger social budgets, a cross-country correlation which is not surprising given that Jackman found democratic governments to have more SIPE.

²¹We have 17 totally nondemocratic (namely index=0 for all years) countries in our data (Bahrain, C. African Rep., Cameroon, Chad, Ethiopia, Gabon, Indonesia, Iran, Ivory Coast, Kuwait, Liberia, Mali, Niger, Oman, Tanzania, Togo, Tunisia) and 22 totally democratic (namely index=1 for all years) countries (including the U.S., Japan, Papua New Guinea, and several European countries).

²²Jackman and Cutright (see below) are interested in welfare and unemployment programs as well as pensions, and some countries began some program at a different date than the other programs. Therefore, they calculate each country's SIPE as the cross-program average of years since program creation. Our analysis of the SIPE (see "year of first law" below) is only for the old age pension program.

²³Although Flora and Alber (1982) suggest that nondemocratic regimes may introduce social programs earlier.

However, both Jackman (1975) and Pampel and Williamson (1989) point out that, even if democracy had no direct impact on Social Security policy, significant differences between democratic and nondemocratic countries are to be expected given that democratic countries are often economically and demographically unusual.²⁴ Hence, the simple correlation might not indicate an effect of democracy, but instead proxy for economic and demographic variables that, for example, would affect citizens' policy preferences regardless of the political regime. These authors therefore include log gdp per capita and the fraction of the population over age 65²⁵ in the regression, and show how the partial effect of democracy is zero or negative, rather than positive as with the raw correlation.²⁶ We have a very similar finding in our data, as seen in our second and third columns of Table 1, which deviate from the first column by including the gdp and elderly population variables in the regression and report smaller (or even negative) democracy coefficients.

Peter Lindert (1994) is, to our knowledge, the first economist to explore the relation between social security spending and democracy with a formal statistical analysis.²⁷ He has a twenty-one-country panel composed of many of the (now) OECD plus Argentina and Brazil for the period 1880-1930 – a sample evenly split between democracies and nondemocracies – and finds the typical democracy to spend the same fraction of GDP on SS as the typical nondemocracy once GDP per capita, the fraction of the

²⁴For example, economic prosperity may permit a country to become democratic, as suggested by Barro (1998) and many others.

²⁵Jackman uses an economic development indicator rather than log gdp per capita and elderly's population share.

²⁶Cutright (1965) also tries to separate the effects of economic development from those of the "political representativeness" of a nation's institutions. He indicates that there is a weak partial relation between SIPE and political representativeness, although it is hard to say whether his results conform with the other studies, since Cutright uses a cross-tabular analysis (rather than multiple regression) and his political representativeness index is not necessarily an index of democracy.

²⁷Although we infer from a paragraph in Easterly and Rebelo (1993, p. 436), that they looked at a cross-country regression of Social Security spending on GDP, democracy, and other variables, finding no democracy effect.

population elderly, and other variables are held constant.²⁸ Lindert does not explore implications unique to voting-based political-economic theories of social security, except by including the voter turnout rate and a female suffrage dummy in a social spending regression for his democratic observations.²⁹ Not long after Lindert, Sala-i-Martin (1996) created a cross-country data set of Social Security programs for the year 1989, and pointed out (p. 288) that Social Security programs have emerged during nondemocratic regimes such as the USSR under Lenin, Spain under King Alfonso XIII, and Japan under Emperor Ito and during democratic regimes such as the 20th century UK, US, and Sweden.

As explained above, our data suggest that democracies spend more of their GDP on Social Security merely because they are richer and older. Columns (4)-(6) of Table 1 further explore this point, by introducing continent dummies into the regression. By comparing columns (1) and (5), we see how much of the raw democracy-spending correlation is across continents, rather than within. In fact, column (4) shows how most of the correlation is explained by the difference between Europe (democratic and high spending) and the rest of the world – Europe explains 4.08 percentage points of the spending share while democracy explains only 1.56. Column (6) suggests that continent dummies may themselves proxy for GDP per capita and the elderly share, since a comparison of columns (3) and (6) shows that introducing the continent dummies does little to the regression coefficients or fit when the regression already includes GDP per capita and elderly's share.³⁰ The insignificance of the continent dummies suggest that there is little

²⁸Parts of the Lindert (1994) paper (eg., the abstract) suggest that democracy leads to more Social spending, and therefore appear to contract the conclusions of other studies. However, his recent work (2002) explains in more detail how his 1994 findings actually show that more voter turnout is associated with more public pension spending *among democracies*, and that there is not an important spending difference between democracies and nondemocracies. This can be seen in his 1994 Table 2 where, holding constant the economic and demographic variables, the pensions column shows that the average nondemocracy spends 0.33 percentage points of GDP *more* than the average democracy. We calculate this by adding his democracy intercept term (-1.18) to his female suffrage coefficient (0.02) times the mean democratic female suffrage (0.372) plus his turnout coefficient (1.57) times the mean democratic turnout rate (0.534) to get -0.33.

²⁹Notice that our democracy variable also takes some account of different “intensities” of democratic regimes, although not according to population voting rates.

³⁰The F-stat for the hypothesis of all continent coefficients equal zero is 1.42; the p-value of the test (6 coefficients and 80 degrees of freedom) is 22%.

spatial correlation in the residuals in our spending model, and hence no strong evidence that countries tend to follow policies of their neighbors.

One rough indicator of whether nondemocratic governors are following the policies used in otherwise similar democratic countries is the number of years of experience with Social Security. Like Jackman (1975), we find a negative correlation between democracy (1960-90) and the year in which the first Social Security law was passed, but a near zero partial correlation once we control for age and income. In particular, we find in our 90 country sample that the year of the first Social Security law is, on average, 35 years earlier for democratic countries (not reported in the Table; two countries must be dropped because SSA does not report). Regressing year of first law on the democracy index, log GDP per capita and the elderly population share, we estimate a democracy coefficient of -8 (s.e.=5).³¹ Hence, nondemocratic countries do not have systematically less Social Security experience than do similar democracies. Since there have been a number of transitions in the 20th century between democracy and nondemocracy, and we (and Jackman) are using the democracy index later in the century, this result does not necessarily show whether or not democracies are quicker to adopt Social Security.³²

Does democratic Social Security spending have a different relation with the age and income distribution? We address this question for our 90 country sample in Table 1's columns (7)-(10). Column

³¹The point estimate is -3 in our 63 country sample. Cutler and Johnson (2001) apply the econometric duration model to explaining year of first law in a sample of 17 high income countries, and find "nondemocracies" (defined as Flora et al 1983 do: a country with a "powerful non-elected ruler") to adopt Social Security programs slightly *earlier* than other countries.

Other cross-country studies not concerned with the democracy effect (eg., Aaron 1967) have included year of first law as an *independent* variable in Social Security regressions, finding old programs to spend more. A raw correlation like this is obvious in our data (almost one percentage point of GDP for each 10 years of program age), although the partial effect (holding GDP and elderly share constant) is economically insignificant (one percentage point for each 50 years of program age, s.e. = 0.5 percentage points).

³²There are some difficulties in interpreting the "year of first law" data for this purpose, and combined them with historical democracy indices. For example, we suspect that newly independent democracies are likely to "reinvent" Social Security after gaining independence from a colonizer or dictator, and hence report to SSA a year of first law that is after the year in which country residents first enjoyed public pension program participation. A better data set would be like that used by Cutler and Johnson in their duration study.

(7) interacts the elderly population share (minus 6 percentage points) with democracy (minus one). We have subtracted constants from each of the variables in the interaction term, so that the coefficient on the population share by itself can be interpreted as the democratic population share effect, and the coefficient on democracy by itself can be interpreted as the democracy effect in a country with six percent of its population over age 65.³³ The coefficient on the interaction term is negative and practically zero, and we see in comparison with column (3) that the effects of age and democracy by themselves are unchanged when we include the interaction term.

Columns (8)-(10) explore the relation between Social Security spending and income inequality, using the Gini coefficient. We have the Gini coefficient for only 65 of the 90 countries, so we use column (8) to demonstrate that results are not too different in the smaller sample (compare with column (3)). Column (9) introduces the Gini coefficient as a predictor of Social Security spending's GDP share, but its estimated coefficient is negative and practically zero. Perhaps this result is not surprising since several researchers (eg., Benabou 1996, Lindert 1996, and Perotti 1996) have failed to find inequality to be associated with bigger government across countries.³⁴ Column (10) interacts (Gini-35) with (democracy-1), and the estimated coefficient on the interaction term is practically zero.³⁵ We find similar results if we replace the income Gini coefficient with the education Gini coefficient as calculated from the Barro and Lee (1996) data on educational attainment.

Another indicator of the heterogeneity of a country's residents is Easterly and Levine's (1997)

³³6.0 is our 90 country sample average of the percentage of the population over age 65.

³⁴Peltzman (1980) finds inequality to be associated with *smaller* government across countries. Tabellini (1992) is the only study we know that finds a positive relation between government spending (which he measures as Social Security and Welfare spending as a ratio to the elderly population, GDP, or total government spending, averaged for the years 1978-82) in inequality (measured as the pre-tax income going to the top 20%, as a ratio of that going to the bottom 20%). His sample has about 60 countries including some nondemocracies, and his inequality variable has a statistically significant inequality coefficient in 5 out of 12 specifications (see his Tables 3 and 4). His analysis does not include GDP per capita, except as it determines his dummy variable for "industrialized countries."

³⁵Since we use the constants 35 and 1 in the interaction term, the coefficient on democracy by itself can be interpreted as the effect of democracy in a country with Gini-35 (fairly typical, although slightly above average, for Europe) and the coefficient on Gini by itself can be interpreted as the effect of Gini in a democracy.

index of “ethnolinguistic fractionalization.” The index is on a 0-1 scale, and measures the probability that two randomly selected residents speak a different language. Although not shown in our tables, we have included this variable in our spending regressions, and its estimated coefficient is always economically and statistically insignificant.³⁶ We see do not find any evidence of an interaction between ethnolinguistic fractionalization and democracy either. We do not believe that voting theories tell us whether GDP should affect spending more or less in a democracy, but a simple theory of measurement error might. For example, we might expect better quality GDP data in democracies, so that we would observe pension spending to be more sensitive to measured GDP among democracies. Although not shown in Table 1, there is a little evidence in our sample of such an interaction between democracy and log GDP per capita. If we add the interaction term to column (3), that term’s estimated coefficient is statistically insignificant and its magnitude is -0.22, which implies GDP coefficients of 0.17 and 0.40 for democracies and nondemocracies, respectively.

Population age is one of the main predictors of public pension spending. In theory, age can affect pension spending, and pension spending can affect age. Older populations may spend more on old age pensions because, by definition, only the elderly are eligible for old age transfers. The size of the elderly population may also determine political support for social security. Pensions can in theory affect the age of the population by discouraging fertility³⁷ or mortality,³⁸ although these “reverse causal” effects are not contemporaneous, since in these theories current demographics are affected by the anticipation of future pensions. Perhaps our finding (to be shown in detail later) that decade-to-decade growth in pension spending is closely related to the growth of the elderly population over those same decades suggests that

³⁶In their study of 17 countries time until adopting Social Security, Cutler and Johnson (2001) find countries with more ethnolinguistic fractionalization to adopt Social Security somewhat *later*. They do not interact democracy with fractionalization.

³⁷eg., the theory of Becker and Barro (1988, pp. 17-18) although Social Security’s effect on fertility is temporary in their model. Ehrlich and Lui (1998) and Ehrlich and Kim (2001) have models with permanent fertility effects of Social Security.

³⁸eg., the theory of Philipson and Becker (1998).

these reverse causal effects are a minor part of the age-spending correlation shown in Table 1.³⁹

We have “life expectancy” data for all but two of the countries in our 79 country sample (Bahrain and Iceland are missing life expectancy information), and life expectancy is correlated 0.79 with the elderly population share. If we add life expectancy to the pension spending regression, its coefficient is small (0.054, s.e.=0.028), which may suggest that the elderly population share effect may be same regardless of whether the age distribution can be explained by fertility, mortality, or migration.

Standard errors on the democracy coefficient are a bit smaller when POLITY IV’s democracy score is used, as in Table 1, rather than any of the other democracy indices. Our point estimates are not sensitive to the use of democracy index. To see this, consider specification (6) from Table 1. If we replace the POLITY IV democracy score with (one minus) its autocracy score, the coefficient on the score is -0.79 (s.e.=0.52) rather -0.89, the GDP coefficient is 0.31 rather than 0.40, and the other coefficients are quite similar. Using POLITY IV’s election yields a democracy coefficient of -0.80 (s.e.=0.56). Using Gastil’s or Bollen’s index gives (in slightly smaller samples with the available data) coefficients of -1.19 (s.e.=0.65) and -0.06 (s.e.=0.63), respectively.

IV.A.2. Evidence on Retirement and Earnings Tests

Table 2 presents models for explaining the use of Social Security benefit formulas that induce retirement in one way or another. The three dependent variables used in the Table are whether the country’s Social Security program had a retirement test, an earnings test, or one of the two (each a 0-1 variable averaged over the three years 1958, 1975, 1995 for each country). The sample is necessarily smaller than our 90 country sample, because Mulligan and Sala-i-Martin’s sample does not completely overlap ours.⁴⁰

Column (1) is a regression of the fraction of time (for the years 1958, and 1975, 1995 only) a country had “induced retirement” (ie, had a retirement or an earnings test as part of its Social Security

³⁹A reverse causal mechanism which may be contemporaneous is that high social security tax rates encourage young people to emigrate, which raises elderly’s population share.

⁴⁰The spending models shown in Table 1 look similar if we confine our attention to the 65 country sample used in Table 2.

benefit formula) on the same variables used in column (3) of Table 1. We see little partial relation between democracy or the elderly population share and induced retirement. Columns (2) and (3) show that these relations are robust to introducing continent dummies, and that South American countries have less induced retirement than others. Column (4) excludes GDP and the share of elderly from the regression; the coefficient on democracy is still not significant.

The rest of the columns of Table 2 separate the retirement test from the earnings test. Columns (5) and (6) use the retirement test as the dependent variable. The coefficient on democracy in column (5) is negative and significant which suggests that democracies are less likely to induce retirement with a retirement test. Column (6) includes income and the share of elderly and the coefficient on democracy becomes insignificant, which suggests, again, that democracy is correlated with design variables only because it proxies for income per capita as richer countries tend to be more democratic.

Columns (7) and (8) have the earnings test as the dependent variable. The coefficient on democracy is positive which suggests that democracies are more likely to induce it with an earnings test. However, it is again the effect of income and age, and not democracy, that effects the method by which retirement is induced, because the democracy coefficients are practically zero once we control for GDP and the elderly population share (see column (8)).

IV.A.4. Evidence on Payroll Tax

Table 3 present empirical evidence on the design of the payroll tax in a cross-section of countries.⁴¹ The two dependent variables used in the Table are whether the country's Social Security payroll tax was capped (a 0-1 variable for each year) and the employee tax rate as a fraction of the sum of the employee and employer tax rates (each dependent variable is averaged over the three years 1958, 1975, 1995 for each country). The sample is the same as in Table 2, except that Australia is omitted because it does not have payroll taxes during any of the years we study. Columns (1)-(6) model the capping of the payroll tax. Columns (1)-(3) have a democracy index as the only independent variable, and shows how democracies are more likely to have their payroll tax capped although the coefficient is least significant when the

⁴¹We do not model the use of a payroll tax, since Australia and New Zealand are the only countries without a payroll tax sometime during our sample.

POLITY IV democracy index is used. To put it another way, our 64 country sample is evenly divided according to whether the time-average POLITY democracy index is greater or less than 0.4, but only 4 of the 15 countries not capping their payroll tax have index greater than 0.4.

Columns (4)-(6) add GDP per capita, the elderly population share, continent dummies, and a British colony dummy to the regressions, and we see a larger coefficient on the democracy index. Because a country's highest earners enjoy the direct benefit of capped payroll taxes, and we see more democracies with caps, our findings seem at odds with Tabellini's model, where the voting process creates Social Security from a coalition of the old and the poor.

Does the capping of the Social Security payroll tax explain why we see democracies spend less than otherwise similar nondemocracies? Consider two cross-country public pension spending regressions using the sample of 64 countries from Table 3 that suggest this is not a big part of the story. The first has the same independent variables as Table 1's column (3) – democracy, GDP, and the elderly population share – and produces a similar democracy coefficient estimate of -0.77 (s.e.=0.55). The second regression adds the capped payroll tax as an independent variable. The GDP and elderly share coefficients estimates are quite similar in the two regressions, while the democracy coefficient estimate only falls in magnitude from 0.77 to 0.69 (s.e.=0.54), and the payroll tax cap coefficient is -0.54 (s.e.=0.39). Hence, use of the payroll tax cap may explain part, but not all, of democracies' spending less on Social Security.

Columns (7)-(10) look at the (nominal) split of the payroll tax between employers and employees. The U.S. has always split its payroll tax equally between employer and employee, so our employee share variable is 0.5 for the U.S. 0.5 is common internationally, but there still is a lot of international variation in the employee share. Without minimum wages, the usual economic analysis predicts that it does not matter who nominally pays the tax, but the split may matter for political purposes, so we might expect democracies to be different in this regard. The positive democracy coefficients in columns (7)-(10) suggest that democracies put a little bit more of the tax on the employee.

We can investigate the effect of the visibility of Social Security taxes on the size of the budget by considering three cross-country public pension spending regressions using the sample of 64 countries from Table 3. The first has the same independent variables as Table 1's column (3) – democracy, GDP, and the elderly population share. The second adds the split variable to the set of regressors. The split

coefficient of 2.61 (s.e.=1.32) is economically significant, and the democracy coefficient is more negative than in the first regression.⁴² The third regression adds to the second a regressor interacting split and democracy, and its coefficient is found to be economically and statistically insignificant (coef=0.98, s.e.=2.96). These three regressions suggest that tax visibility may have a small effect on the size of the program, but that the effect is not different in democracies vs. nondemocracies.

IV.B. Evidence on the Growth of the Size of Social Security

In order to examine the growth of the SS program, we also partition our sample in two time periods: 1960-74 and 1975-90. We choose this division because it is an equal split chronologically, and point out how it corresponds with the chronological discussion of our European Case Studies (see below). Fourteen of the 90 countries in Table 1's sample do not report social security spending or real GDP for more than one or two of the years 1960-74, so we exclude them from the spending growth analysis.⁴³ Rather than looking at the 1960-90 democracy index average, our spending growth models measure democracy according to the average POLITY index for the years 1960-74.⁴⁴

Table 4's column (1) shows how Social Security spending grew more (by 1.6 GDP percentage points) in democracies than nondemocracies (as classified in 1960-74). But this may derive from the relation between GDP per capita and spending growth, because the partial effect of democracy (holding log GDP per capita constant) on Social Security spending is zero or negative, as shown in column (2). Column (3) shows how Social Security spending growth is related more closely with GDP per capita's log

⁴²This result is probably not very robust, because in previous work (Mulligan and Sala-i-Martin 1999a) using somewhat different samples and spending measures, we found a significantly negative coefficient on the split variable.

⁴³Excluding those fourteen countries (Argentina, Bangladesh, Burkina Faso, Burundi, Honduras, Indonesia, Iran, Ivory Coast, Mali, Peru, and Rwanda eliminated because lack of spending data, and Bahrain, Kuwait, and Bulgaria eliminated because of lack of real GDP data) from Table 1's sample has almost no effect on point estimates, except to increase the democracy coefficient by 0.1 (eg., the democracy coefficient becomes 3.8 in column (1), and -0.79 in column (6)).

⁴⁴As discussed above, there are not many countries in our sample with significant changes in the democracy index, so our comparison of spending changes with democracy *changes* is limited to the case studies.

than with its growth rate.

Older countries and aging countries had more Social Security spending growth, as shown in columns (4) and (5). European countries also had more spending growth, and the inclusion of continent dummies in the spending model makes the democracy coefficient more negative. Columns (8) and (9) add the 1960-74 to 1975-90 change in the democracy index to the models shown in columns (1) and (7). We see in Column (8) that, without an additional controls, it appears that democratic countries, especially those that were democratic at the end of the period, had the most spending growth. However, column (9) shows how this derives from the different demographics⁴⁵ of democratic countries: countries that begin democratic may have a bit less spending growth.

The main lesson from this section is that, once we hold constant per capita income and elderly population share, there appear to be no differences in the size, growth or design of SS programs between democratic and nondemocratic regimes. Although not shown in Table 4, various democracy-interaction terms can be added to the models there, and the point estimates on the interactions terms are economically and statistically insignificant. Hence, democracies and nondemocracies also seem to be similar in terms of the reaction of their pension spending growth to the economic and demographic variables.

V. Case Studies

It is helpful to see whether the regression results for the 90-country sample are confirmed with case studies. One advantage of the case studies is that they can rely more on country-specific data sources that may not be available for a broad cross-section. Another advantage is that the case studies may help us gauge the importance of various causal mechanisms. For example, it may be that Social Security policy affects aging, or GDP, or the form of the political institutions so that coefficients from a regression equation like those displayed in the third column of Table 1 should not be interpreted as the effects of democracy, GDP, and aging on public pension spending. But the time series relationships seen for a case study like Spain's might be more readily interpreted that way, for example, if we think that Franco's death would be the end of dictatorship regardless of when it occurred, and that the year of his death was not affected by

⁴⁵Column (9)'s results would be similar if the continent dummies were excluded.

Spain's pension spending, GDP, or the average age of its population. Unfortunately, case studies are not a full solution to this problem because, for example, the amount of public pension spending during a dictator's regime may affect the likelihood of democracy's emerging after his death and hence our choice of his country as a case study.

Our overall strategy for selecting countries for closer study is to first look for examples of a dramatic change in political regime (either from democracy to dictatorship or vice versa) and then observe whether this change is followed by dramatic alterations in the size or the design of the Social Security program relative to changes occurring elsewhere in the world. More specifically, we begin with two time averages for each country of the POLITY democracy index – 1960-74 and 1975-90 – and take those countries for which the two averages indicate significant secular changes. Portugal and Spain stand out the most in this regard, with democracy index 0 prior to 1975 and 1 in most of the years after. Greece also stands out with democracy index 0.7 1960-66, 0 1967-73, and about 0.9 after 1975. Bangladesh, Chile, and Uruguay are the three countries with largest democracy index reductions over the time period.

We then tried to find economically and demographically similar countries for comparison. This lead us immediately to Italy for comparison to Greece, Portugal and Spain. There are eight South American countries in our data other than Chile and Uruguay, but none of them could be characterized as particularly democratic during the period 1960-90. Argentina is relatively democratic, and the most similar to Uruguay in terms of GDP and age, so we include Argentina for comparison with Uruguay. Based on GDP and age, we include Brazil and Peru for comparison with Chile and each other. We leave the study of Bangladesh, and an appropriate comparison country, for future research.

V.A. Southern Europe

Consider three European countries which have changed to and from democratic regimes during the past couple of decades – Greece, Portugal, and Spain – and compare them with another southern European country which has been continuously democratic since WWII. The top part of Table 5 shows how the countries share not only geography in common, but they also have similar postwar age demographics (percentage of population over age 65 increased 6-7 percentage points 1950-90), similar economic growth (GDP per capita grew 1.6-1.8 percentage points per year 1950-90), and similar income

inequality (Gini coefficients of about 40 in 1974).

Are the different political histories associated with different SS histories? From the mid 1960's through the 1980's (the period during which we have pretty reliable and comparable spending data), Greece, Portugal, and Spain increased public pensions' percentage of GDP by 6.5, 4.9, and 6.2 percentage points, respectively, while Italy's increased 6.2 percentage points. Nearly all of the Greek pension share growth was during its democratic period (since 1975, and before 1967), but Spain had pretty similar spending share growth rates during its nondemocratic and democratic periods (before and after 1975), respectively, as did Portugal. Figure 1 supports this conclusion, using perhaps more reliable country-specific spending data for Italy and Spain.⁴⁶

Italy had the most spending in 1966, although it was not far ahead of Greece.⁴⁷ Italy's spending share grew pretty steadily over time, while the Greek share hardly grew 1966-79, and then grew quite rapidly since. By the late 1980's, the two countries were quite similar. Spain and Portugal both began the period with similar low levels of spending and grew at similar rates. In terms of the age of the population, all four countries age at very similar rates. Greece, Portugal and Spain have very similar fractions of their population over age 65, and are each about one percentage point behind Italy on that metric throughout the period 1960-90.

Mulligan and Sala-i-Martin's SS design indicators show how all four of these countries now (as of 1995) have retirement tested SS benefits, with no delayed retirement credits. Greek and Italian benefits have relatively important means tests, while Spanish and Portuguese do not. Greece, Portugal, and Spain retirement-tested benefits both in their democratic and nondemocratic periods, while Italy moved from earnings tests to its current retirement test. The means testing of benefits has been fairly constant over time in these countries, except Italy where means-testing has become more important over time. All four

⁴⁶We believe that some of the Italian data reported by the ILO may not be comparable over time. However, the Bank of Italy reports that "regular" public pension spending's share of GDP grew by 5.6 percentage points 1966-90 and, as shown in Figure 1, this growth was pretty steady over time. This is in line with the conclusions our Table 5 draws from the ILO data.

⁴⁷Italy's spending is further ahead of Portugal and Spain than we might guess from their age and GDP differences and the pension spending regression coefficient found with our 90 country sample.

countries have relied, and do rely, heavily on payroll taxes for SS program revenue. Greece and Spain capped their payroll tax (i.e., did not levy payroll tax on earnings a person has above some cap amount), and did so throughout the period. Interestingly, all four countries have shared, and do share, a financing difference with most other countries in the world – they all have much higher payroll taxes (nominally) levied on employers than those on employees.

In summary, Social Security policy in Spain and Portugal during their nondemocratic period, as measured by our design and spending indicators, was similar to those of democratic Italy. Nondemocratic Greece had a social security program of similar design to the others, but its share of GDP grew less rapidly. Spanish and Portuguese Social Security spending continued to grow at Italian rates during their democratic period. Democratic Greek spending grew more rapidly, in effect making up for its slower pre-1975 spending growth relative to the other three countries. In other words, of the political regimes represented in the Figure, the unusual one is nondemocratic Greece 1967-74 because it did not increase Social Security's share of GDP. Hence, the Greek case supports, while the Spanish and Portuguese cases contradict, the hypothesis that the introduction of the institution of voting should lead to higher SS spending growth. All three cases are inconsistent with the hypothesis that introducing democracy would significantly affect the design of tax and benefit formulas.

V.B. South America

V.B.1. Democracy in Argentina, Brazil, Chile, Peru, and Uruguay

Uruguay probably has the most experience with democracy since 1960 (POLITY scored it 0.8, 0.9, or 1 in more than half of the years), and is the only one of the five countries getting a perfect score at some point (1989 and 1990). Chile is probably the least democratic (POLITY scored it 0 in terms of elections and overall democracy in half of the years) although, for the purposes of understanding public policy, it may be useful to think of Chile as somewhat more democratic than scored by POLITY because Pinochet planned a transition to democracy several years before the first 1980's election. Chile and Peru probably have the largest changes from democracy to nondemocracy and back, since these two countries are the only ones scored 0 in terms of elections and overall democracy for several consecutive years in the

middle of the period.

For the 1980's alone – 1980's is the period when we have the most social spending data for these countries – the democracy rankings are different. Peru is the most democratic in the 1980's, but had ended a twelve year period of nondemocracy in 1979. By comparison with Peru, perhaps Brazil is less democratic because its 21-year military rule lasted until 1985. Of the five countries, Uruguay is one of the least democratic in the 1980's, since its nondemocratic regime was in power for the first half of the decade, and dated back to the early 1970's. According to the dates of transition, Argentina (1985) looks only slightly more democratic than Uruguay (1983), but we point out that (according to the POLITY codes) Uruguayan executive elections were not fully competitive until 1989. Furthermore, Argentina had the more recent democratic experience prior to 1980: 3-4 years of democracy in the 1970's.⁴⁸

Chile is a complicated case for our analysis because it began the 1980's with a dictator who planned a several year transition to democracy. Thus, it can be persuasively argued that during the transitional years, policies were enacted by a “democratic” regime. Moreover, there were a number of other significant economic reforms coincident with the change in politics and Social Security, and our Social Security spending data is particularly unreliable.

V.B.2 1980's Public Pension Spending in Four Countries

Much of the South American Social Security spending data reported by the ILO is for the 1980's, so most of our South American analysis pertains to that period. We compare Uruguay with Argentina because they are pretty similar in terms of the age of their populations (as compared to Brazil and Peru, both Argentina and Uruguay have about twice the fraction of their population over age 65) and in terms of GDP per capita (just above \$4000 per year). Uruguay spent more on public pensions, but based on population age alone we would have expected a difference.⁴⁹ So the levels of social spending in these two

⁴⁸The democratic years during the 1970's were unstable politically.

⁴⁹Another part of this difference may be attributed to a data error. ILO reports Uruguayan public pension spending of 13.6% of GDP in 1987, as compared with 7.2 and 8.6 % of GDP in 1986 and 1988, respectively. The 1980-89 average Uruguayan public pension spending percentage without the year 1987 is 6.7.

countries are consistent with no democracy effect.

Public pension spending does grow less in Argentina than in Uruguay: pension spending grows in Uruguay, but in Argentina pension spending is pretty constant. Even if political situations were the same, we expect Argentinian pension spending to grow less because it ages less during the decade. Hence, the pension spending growth rates are also consistent with no democracy effect.

Peru is quite similar to Brazil in terms of the age of its population (and in terms of the rate of aging during the 1980's), although it is poorer than Brazil and the other countries we study. If Peru's greater 1980's democracy created more social spending, it is not obvious in our data because it is not large enough to counteract the effect of income.⁵⁰ If anything, comparing all four countries suggests that Brazil's pension spending is the outlier because it spends so much more than Peru and is so similar to Argentina despite being half as old. Since Brazil is arguably less democratic in the 1980's than Peru or Argentina, its data may suggest a negative effect of democracy on pension spending.

ILO provides a relatively long history of Social Security spending for Uruguay and Brazil, which we display in Figure 2 (note that Uruguay data are missing 1967-74, and are suspicious for 1987). The Figure also has vertical lines to show when the two countries changed democracy-nondemocracy status ("D"=democratic). Although the missing data makes it hard to be sure, it does not appear that there was significant SS spending growth during the democratic period prior to 1974, or that the level of spending was unusual during those years. We see some SS spending growth during the nondemocratic years 1974-84 – about at the rate the elderly population share was growing. This growth continued (or perhaps increased slightly) during the recent democratic years. Hence, Uruguay's times series do not show us that democratic governments have significantly more SS spending.

Brazil's public pension spending is quite similar in the first and second half of the 1980's, even though the political regimes were quite different. Brazil's spending seem to grow at a normal rate during its nondemocratic period (prior to 1985), once we consider that its elderly population share grow from .037 to .043 between 1970 and 1990.

⁵⁰Our regression analysis below (and those of previous studies) show how the level of income is an important determinant of the size of the social security program.

IV.B.3. Induced Retirement and Payroll Taxation in the Four Countries

At some time since 1960, all four countries made retirement a necessary condition for receiving the public pension, and did not (from an actuarial point of view) sufficiently credit pensioners for delayed retirement.⁵¹ Brazil, Peru, and Argentina eliminated this requirement in 1966, 1991, and 1993, respectively, and did not replace it with an earnings test. Uruguay still (as of 1999) requires retirement of pensioners. Notice that two of the countries removing the retirement test (Brazil and Peru) did so during nondemocratic regimes, and one during a democratic regime. It is therefore hard to argue from these four cases that democracies have a different likelihood of using retirement or earnings tests.

Brazil reduced the share of the payroll tax levied on employees (by increasing the employer tax rate without increasing the employee rate proportionally) between 1975 and 1995 which, since Brazil became democratic in between those years, by itself suggests that democracies tend to (nominally) tax employers more than employees. However, Brazil was democratic prior to 1963 and (not shown in the Table) had the same employee share (0.5) as in 1975. Furthermore, Peru and Uruguay also changed democratic status between 1975 and 1995, but did not significantly change their employee shares.⁵²

Brazil and Argentina capped their payroll taxes in all three years 1958, 1975, and 1995. Peru removed its cap some time between 1975 and 1995. The SSA reports do not show that Uruguay had a cap at any time since 1958. Peru's recent removal of the cap might suggest that democracies are less likely to have caps, but this tendency does not show itself in the changes over time in the other three countries. We show below that a larger sample of countries shows a pretty strong tendency for democracies to *have* a cap.

V.B.4. Large Budgets Chile Prior to 1981

Chile (not included in Table 6 because we do not have good spending data) shows us how

⁵¹All of the reports in this section about Social Security benefit rules are from Mulligan and Sala-i-Martin's database, or from SSA (various issues) directly.

⁵²Uruguay reduced its employee rate from 15 to 13%, while reducing its employer rate from 15% to 14.5%. We are not sure how this could occur while expenditure was rising significantly, but the SSA (1995) does note that Uruguay's Social Security deficits are financed with general revenue.

nondemocracies have been known to create, or at least maintain, extremely large Social Security budgets. According to the IMF (ILO), Social Security and Welfare spending under General Pinochet exceeded 10 percent (6 percent) of GDP by 1981. As fractions of GDP, 6-10% is as large or larger than the Social Security budgets of European countries, despite the fact that only 6% of Chile's population was over age 65 (compare to 10-15% aged 65+ in most European countries). It is hard to tell from these data alone whether social spending grew to these levels under Pinochet, or under prior governments. But we do have some evidence that Pinochet's government, even though not immediately held accountable by an electoral process, was unprepared to reduce pension and other social spending during the first several years of its regime *even* when it meant increasing already high payroll tax rates. For example, SSA reports that almost 40 percentage points were added to the employer portion of payroll tax rates between 1973 and 1975, and that this increase lasted at least until 1977.⁵³ Hence, the first part of Pinochet's regime shows clearly that an electoral process is unnecessary for the maintenance of large social security budgets.⁵⁴

Foxley et al (1979, p. 129) report that 1969 Chilean social spending was 10-11 percent of GNP (6 of the 10-11 were spent on public pensions). ILO (1961, p. 205) reports that payroll tax revenue and social spending were already pretty high as long ago as 1951 – 10.2 and 8.2 percent of national income, respectively. It is hard to tell how these data might be compared with IMF or later ILO data, but they show a lot of social spending prior to Pinochet, especially when we recognize that only 4.3 and 5.1 percent

⁵³Perhaps one explanation for the tax rate increases 1973-75 is the government's desire to maintain social spending while the tax base was shrinking (for example, real GDP per capita fell by 22% during the period).

The reader should note that changes in the employer tax rate does not accurately measure changes in the tax wedge created by the payroll tax, especially in a place like Chile where employer rates were so high. For this purpose, it is better to look at $(\text{employer rate} + \text{employee rate}) / (1 + \text{employer rate})$ which, according to SSA, increases from about 35% in 1973 to 50% in 1975 (exact percentages depend on whether the contributor is a wage earner or a salaried worker). We thank Salvador Valdés for bringing this point to our attention, and refer readers to Barro and Sahasakul (1986) for further explanation of the formula.

⁵⁴Another interesting observation about Pinochet's public pension programs is that, according to the SSA, he did not change the design of public pensions prior to 1981. In 1958 and 1975 Chile had the same public pension policy regarding earnings and retirement tests (according to SSA, there were none except for salaried employees), and the payroll tax was not capped.

of the Chilean population were over age 65 in 1950 and 1970. The SSA reports pretty high payroll tax rates in, for example, 1958, 1969, 1971, and 1973: about 20% for pensions and another 20% or more for other social programs. These pension payroll tax rates were similar to those in Argentina and Brazil at the time (Uruguay had higher rates, and Peru lower, by about 10 percentage points), and the Chilean rates for other programs were significantly higher. Arellano's (1985) series on payroll tax rates (employer and employee, all programs combined) for wage earners is 8% in 1952 and already 45% by 1960.

If Chilean pension spending growth occurred before Pinochet, was it under a democratic or nondemocratic regime? This is a hard question to answer, for two reasons. First, our data do not clearly indicate when the growth occurred. Arellano shows payroll tax rates quadrupling between 1952 and 1955 (from 8% to 33% – note that there was a major Social Security reform in 1952) and then growing to 50% by 1972, although ILO (1961, p. 205) does not report a dramatic payroll tax revenue increase any time between 1951 and 1957. Second, while the Chilean governments prior to Pinochet were *relatively* more democratic, which of them (if any) should be considered democratic? Consider the period 1952-54, when a major Social Security reform was passed and payroll tax rates quadrupled (according to Arellano). During this time, Chile's president was Ibanez, who was by all accounts a dictator in the 1920's. The POLITY project's democracy score is a mere 0.3 for Chile during these years, in part because Chile's chief executives were not always elected fairly and competitively (ie, elections were "stacked"), executives had a lot of power, and some political parties were outlawed. During *these* years, POLITY ranks Chile as less democratic, and having less competitive elections, than (among our South American case study countries) Brazil, Peru, and Uruguay.

An early ILO report (1961) suggests that social spending (i.e., the sum of pension, health, family, and other social program spending) was already generous in Chile by 1951. A Chilean Social Security system was created in 1925, although this did not resemble the system as of 1952 because the 1925 system was designed to be fully funded (Foxley et al, p. 124). We are not sure of exactly which year the Chilean system was transformed to pay-as-you-go, but we point out that POLITY gives Chile a democracy score of 0.1 for the years 1925-34, in part because dictator Ibanez's regime (1927-31) came about from rigged elections, military support, and from the repression of political activity.⁵⁵ POLITY slightly increases Chile's

⁵⁵<http://www.countryreports.org/>

democracy index to 0.3 in 1935, and notes that its elections were somewhat more competitive. Nevertheless, POLITY clearly characterizes Chile as nondemocratic from 1925 until 1954. Of course, these years also include the Great Depression, so our data do not permit us to determine whether Chile's growing social spending 1925-51 should be attributed to nondemocracy or to the Great Depression.⁵⁶

There may have been substantial increases in pension spending between 1955 and 1972. During this period, Chile may not have been very democratic by world standards, but democratic by Chilean and South American standards. For example, POLITY scores Chile 0.5 (1955-63) and 0.6 (1964-72) – the highest scores in Chile's history prior to 1989.⁵⁷ Among our case studies, Argentina, Brazil, and Peru had lower scores than Chile for the 1960's. Hence, whether we associate any Chilean pension spending growth 1955-72 with democracy depends on some of the details of how we quantify “democracy.”

In summary, Chile's history has a lot to tell us about the connection between pension spending and democracy. Our Chilean data is of limited quality, and sometimes appears contradictory. Nevertheless, all of our data are consistent with two conclusions that might be drawn by focusing on the key years 1925, 1952, and 1973, and the few years immediately following each of them. First of all, the Chilean governments in these years were not democratic by any standard. Sometimes there were not elections, at other times there were elections but they were stacked and led to the appointment of an executive who was very powerful and often suppressed his political competition. Second, of all of the increase over time in pension spending and rates of payroll taxation, much of it occurred in these key years and the few years immediately following. Hence, the Chilean experience suggests that free and fair elections, or even elections of any kind, are not necessary to create, expand, and maintain a large Social Security system.

⁵⁶The 1924 “Ruido de Sables” is an interesting episode suggesting that nondemocracy was a factor. During that episode, there was a conflict between the Parliament and the military – the former wanted to increase congressional salaries and the latter thought social programs needed more attention! (http://icarito.tercera.cl/enc_virtual/historia/parlamento/parla7.html)

⁵⁷Chile's POLITY democracy score is less than 0.5 in all of the years 1818-1954, except 1888-90 (0.6) and 1891-23 (0.5) but even in these years POLITY notes that there were not fair and competitive elections. Also note that Bollen's (1980) democracy index is higher for Chile than for the U.S. in 1960 and 1965.

V.C. European and South American Cases Compared

We chose countries for comparison so that political differences were large, but economic and demographic situations were similar. Might democracy affect Social Security by affecting the response of Social Security spending to economic and demographic shifts? Economics and demographics were (and are) quite different in Europe and South America, so we can offer an answer this question by combining the democratic-nondemocratic spending growth gaps for the European and South American cases. To see the details of the argument, notice first how our European countries aged much more 1960-90 than the South American cases. Elderly population shares almost doubled in Europe (eg., Portugal's grew by a factor of 1.7), from initially high levels (8% in Greece, Portugal and Spain; 9% in Italy). Argentina's elderly population share increased from 5.5% to 8.9% (a factor of 1.6, mainly prior to 1980), but otherwise only Uruguay added more than one percentage point to its elderly population share (8.1% to 11.6%). The European cases also had more per capita real GDP growth in the 1960's, 1970's, and 1980's.

Second, recall how, with the exception of Greece, the democratic-nondemocratic spending growth gap was basically zero in Southern Europe. It seems that democratic Italy's SS spending was reacting to rapid population aging by increasing at a rate similar to those in nondemocratic Spain and Portugal. In South America during the 1980's, Argentina, Brazil and Peru seem to react to slow population aging in the same way – namely, by holding pension spending's share of GDP roughly constant. Uruguay aged somewhat during the 1980's, and its spending growth seems to have reacted in the way we would have expected based on the experience of other countries (and without regard for political institutions) – with slow growth of pension's share of GDP. While it's hard to say which of the South American cases is the most “democratic”, we can say that rates of spending growth are quite similar despite their very different political experiences. Hence, the democratic-nondemocratic spending growth gap seems to be close to zero in South America too. If both Europe and South America have the same democratic-nondemocratic spending growth gap, but different rates of economic and demographic change, then democracy is not interacting with the economic and demographic variables.

Of course, we have the two outlying case studies, Greece and Chile. From the Greek case, it appears the spending growth is more rapid under democracy, while we get the opposite impression from the Chilean case. If we then compare the Greek and Chilean cases, and recognize that the elderly

population was growing more in Greece, we might infer that nondemocracies have more spending growth (as in Chile), but that democracies respond more to population aging. However, this result does not receive support from our other case studies, or from the regression analysis in the larger country cross-sections.

VI. Conclusions

We have three main empirical findings regarding the relation between Social Security and democracy. First, holding constant the fraction of the population over age 65 and GDP per capita, we find no systematic evidence that democratic governments spend a larger share of GDP on Social Security, or differently adjust their spending to economic and demographic trends. Cross-country econometric estimates suggest that the effect of democracy may be to *lower* Social Security spending's share of GDP by 0.9 percentage points. Case studies of seven countries show how countries with very different political histories, but similar economic and demographic histories, can have similar Social Security programs. One country, Chile, shows that Social Security budgets can be quite large without democracy. Greece is the only case, out of nine total, where we see some evidence consistent with a positive effect of democracy on Social Security spending, because Greek Social Security grew significantly in the early 1980's – only a few years after the Greek nondemocratic regime ended.

Second, the relation between pension spending and economic and demographic variables seems to be the same in democracies and nondemocracies. In particular, richer and older countries spend more of their GDP on public pensions, while inequality is uncorrelated with public pension spending. Third, both case studies and cross-country regressions show no effect of democracy on Social Security program design, except perhaps for a somewhat greater tendency for democracies to cap their payroll tax.

Previous empirical studies of other public policies also find that democratic and nondemocratic governments look pretty similar from a public finance perspective. For example, controlling for GDP per capita, Easterly and Rebelo (1993, p. 436) found no relationship between democracy and a number of government tax and expenditure items.⁵⁸ Indeed, the only government budget item in their study that was

⁵⁸We infer from their p. 436 and Table 1 that the budget items they studied in connection to democracy include tax revenue, nontax revenue, current revenue, social security contributions, government consumption, government consumption excluding defense and education, public services expenditure, social security expenditure, and transfers expenditure. Most of their budget data is from

systematically different between democracies and nondemocracies was the amount of aid revenue received by the government from foreign governments!⁵⁹ According to Lott (1999), totalitarianism does not predict the amount of public health spending, or the number of children vaccinated by public health programs, although it does predict (over some range) more public education spending.⁶⁰ Political scientists have long studied the determinants of military policies, and there still is some debate as to whether democracy affects them. Elman (1997) surveys some of the literature, whose findings lean toward some connection between democracy and peaceful foreign policy, at least vis-a-vis other democratic countries. Sen (eg., his 1999 article) has argued that democratic governments are better at preventing famine during a food shortage. There is some evidence that government spending follows an electoral cycle (eg., Alesina, Cohen, and Roubini 1992 on total government spending, or Godoy and Valdes 1993 on pension spending in Chile), and by definition nondemocracies have no electoral cycle.⁶¹ More research is needed to measure the various effects of democracy on the public economy, but our view is that the democracy effects are quite small in comparison with the effects of demography and the private economy.⁶² Dougan and Snyder

Barro and Wolf (1989) and the International Monetary Fund (various issues).

⁵⁹For a detailed study of the determinants of foreign aid, see Alesina and Dollar (2000). All four authors conjecture that the difference does not derive from a difference in the public decision-making processes of democratic and nondemocratic governments, but rather that donor countries prefer the recipient to be democratic.

Budget balance implies that recipient countries – which happen to be disproportionately democratic – would tax less, spend more, or both. Perhaps this effect is small because Easterly and Rebelo report no significant tax or spending difference between democracies and nondemocracies.

⁶⁰Lott does not attribute the public education spending difference directly to the institution of voting, but rather to the greater demand by totalitarian regimes to control information.

⁶¹Nondemocratic governments do not turn over on a regular cycle, but might government spending be different near times of (irregularly spaced) transitions? We are not aware of any studies of this question, so perhaps it is premature to conclude that nondemocracies have no analogue to electoral cycles.

⁶²Persson, Roland, and Tabellini (2000), hereafter PRT, look at a sample of democratic countries, and find quite a significant correlation between “constitutional features” and government spending’s share of GDP. Their results are (partially) reconciled with our findings and the findings of other studies of Social Security and democracy by Mulligan and Gil (2002), who show how PRT’s

(1993) find both authoritarian and democratic regimes to restrict trade, but authoritarian regimes tend to use tariffs while democratic countries tend to use quotas.

Since the institution of voting, and political institutions more generally, are so different in democratic countries, our findings suggest that political institutions are quite minor determinants of the size and design of Social Security programs. Much more important are economic and demographic variables, such as the aging of the population and economic growth. Social Security may still be a highly political issue, because economic and demographic variables may determine the political influence of various groups. For example, an aging population may have more political support for Social Security spending, but we believe that this influence would derive from the size and economic activity of the elderly population itself, and that it does not particularly matter for the size and design of Social Security what are the details of the political institutions in which the various groups interact or even if voting by the citizenry is part of the political process.

A number of positive theories of the public sector in general, and Social Security in particular, are built on models of voting. Are the implications of those models consistent with our findings? We think not, at least for the game theoretic voting models in which the public policy chosen by the voting mechanism is highly sensitive to the rules of the mechanism, because in fact Social Security seems to be so insensitive to quite large changes in political institutions. Perhaps voting models are just a metaphor for a variety of public decision mechanisms, including those that are used by dictatorships. But if this is the reason for building a model of voting, then it seems improper to take seriously any implications that are sensitive to the form of the voting mechanism. One important example is the “one-man, one-vote” property of voting models, which makes it difficult for a citizen to express his intensity of preference for policies considered by the public sector. Because intensity of preference does not matter in such models, we get results like de Tocqueville’s (1835), Meltzer and Richard’s (1981) and Tabellini’s (1992) that income inequality should be associated with larger transfer or Social Security budgets, and that income or earnings taxes have the important purpose of raising revenue from the very rich. An important challenge for political economics

constitutional feature measures are correlated with nonpension Social Spending, but much less so with public pension spending and other forms of government spending. But we are not aware of an explanation of why nonpension spending might be correlated with constitutional features but (according to Easterly and Levine) not with democracy.

is to explain why the elderly have enjoyed political success in nondemocracies as well as democracies.

VII. Data Appendix

The table below shows, for our main 90-country sample, which country-years are missing from the ILO pension spending data. Using the Social Security Administration's (1995) report of each country's Social Security program's first year, we have found that much of the missing ILO data derives from the fact that some countries did not have Social Security during each of the years 1960-90. We therefore fill in the ILO data with zeros for each year since 1960 and before the first year of Social Security (typically for African and Middle Eastern countries prior to 1975). Dark boxes are country-years with no Social Security program, and white boxes are country-years with Social Security but no ILO data.

All of the countries in the Table have at least 5 years of positive ILO data. But if we combine the ILO data with the zeros, there are ten more countries with 5 years of data (including the zero spending years as data points) and with GDP and demographic data. These countries are (with year of first SS law in paren): Chad (1984), Gambia (1981), Ghana (1965), Haiti (1965), South Korea (1973), Liberia (1972), Oman (1975), Papua New Guinea (1980), Thailand (1990), and Zimbabwe (1993). If we average the zero spending years with the positive spending years report by ILO (if any), we get essentially zero spending for all of them: Haiti has the highest estimate, spending 0.02% of GDP. Zero is probably a pretty accurate estimate for most of these countries, except for South Korea, Thailand, and Oman which, if ILO had reported any spending for them, might be significant given their age and level of development. In any case, our regression estimates are similar if we exclude all 10 countries, include all 10, or just include those 7 where we suspect zero to be an accurate spending estimate: democracy coefficient point estimates are -0.89 (see column 3 of Table 1), -0.67, and -0.72, respectively.

Summary Statistics: Continuous Variables							
	years	Countries	avg	std dev	median	min	max
public pension spending/GDP, %	60-90	90	2.19	2.70	0.75	0.00	8.79
years of pension obs.	60-90	90	22	7	23	6	31
democracy index	60-90	90	0.43	0.41	0.59	0	1
real GDP, 1985 \$ per capita	60-89	90	4290	3762	3017	288	1387 3
elderly per capita, %	60, 70, 80, 90	90	6.0	3.9	4.0	1.6	14.9
year of first SS law	na	88	1945	22	1948	1889	1978
Gini coefficient	60-90	65	40	9	39	23	61
ethnolinguistic fract.	60	69	0.26	0.24	0.14	0	0.81
retirement or earnings test	58, 75, 95	65	0.78	0.35	1	0	1
retirement test	58, 75, 95	65	0.62	0.43	0.83	0	1
earnings test	58, 75, 95	65	0.17	0.31	0	0	1
payroll tax is capped	58, 75, 95	64	0.64	0.41	0.67	0	1
employee sh of payroll tax	58, 75, 95	64	0.41	0.12	0.4	0.02	0.90
Summary Statistics: Dummy variables (percent of sample = 1)							
1958 (<i>N</i> =40): retirement test=58, earnings test=25, payroll tax cap=73							
1975 (<i>N</i> =60): retirement test=63, earnings test=18, payroll tax cap=66							
1995 (<i>N</i> =61): retirement test=61, earnings test=16, payroll tax cap=55							

We also point out that, in the 90 country cross-section sample, the democracy index is correlated 0.69 and 0.67 with log real GDP per capita and elderly per capita, respectively.

Table 1: Democracy and Social Security Expenditure in a Cross-Section of Countries

independent variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
democracy index	3.71 (0.58)	0.86 (0.68)	-0.86 (0.42)	1.59 (0.47)	1.03 (0.56)	-0.88 (0.44)	-0.87 (0.43)	-0.80 (0.52)	-0.83 (0.51)	-0.95 (0.54)
avg gdp per capita, log		1.67 (0.27)	0.35 (0.19)			0.40 (0.23)	0.34 (0.19)	0.44 (0.32)	0.61 (0.34)	0.59 (0.35)
% of pop. aged 65+			0.62 (0.05)			0.59 (0.08)	0.60 (0.06)	0.59 (0.07)	0.52 (0.08)	0.54 (0.09)
(%65-6)*(democ-1)							-0.07 (0.10)			
gini									-0.03 (0.02)	-0.00 (0.05)
(gini-35)*(democ-1)										0.03 (0.05)
Europe dummy				4.08 (0.43)	3.30 (0.96)	1.45 (0.73)				
other continent dum's	no	no	no	no	yes	yes	no	no	no	no
adj-R-sq	.31	.51	.83	.66	.67	.84	.83	.81	.82	.81
s.e.	2.25	1.89	1.11	1.59	1.56	1.09	1.12	1.21	1.21	1.21
# of countries	90	90	90	90	90	90	90	65	65	65

Notes: (1) dependent variable is Social Security expenditure, as a percentage of GDP, averaged over the available years 1960-90.

(2) OLS standard errors in parentheses

(3) All regression include a constant term. Coefficients estimates for constants and some dummies are not reported in the Table.

(4) All regressions with continent dummies have North America as the omitted category.

Table 2: Democracy and Induced Retirement in a Cross-Section of 65 Countries

dependent variable:	retirement or earnings test				retirement test		earnings test	
independent variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
democracy index	-0.01 (0.16)	0.07 (0.15)	0.02 (0.16)	0.02 (0.12)	-0.40 (0.14)	0.07 (0.17)	0.42 (0.10)	0.14 (0.12)
avg gdp per capita, log	-0.10 (0.08)	-0.08 (0.08)	-0.10 (0.10)			-0.15 (0.08)		0.08 (0.06)
% of pop. aged 65+	0.01 (0.02)	0.01 (0.03)	0.02 (0.03)			-0.04 (0.03)		0.04 (0.02)
recent British colony		-0.20 (0.11)	-0.22 (0.13)	-0.21 (0.11)	-0.10 (0.13)	-0.13 (0.13)	-0.12 (0.10)	-0.09 (0.09)
Europe dummy		-0.19 (0.16)	-0.20 (0.24)	-0.18 (0.11)	-0.02 (0.13)	0.26 (0.18)	-0.18 (0.09)	-0.45 (0.13)
S. America dummy		-0.46 (0.13)	-0.49 (0.25)	-0.47 (0.13)	-0.39 (0.15)	-0.39 (0.14)	-0.10 (0.11)	-0.09 (0.10)
other continent dum's	no	no	yes	no	no	no	no	no
adj-R-sq	.00	.15	.11	.16	.19	.29	.18	.30
s.e.	0.36	0.33	0.33	0.32	0.39	0.36	0.28	0.26

Notes: (1) Dependent variables are averaged over the years 1958, 1975, and 1995.

(2) OLS standard errors in parentheses

(3) All regression include a constant term. Coefficients estimates for constants and some dummies are not reported in the Table.

(4) All regressions with continent dummies have North America as the omitted category.

(5) Recent British colonies variable = 1 for all countries under British rule for more than 50 years since 1850.

Table 3: Democracy and Payroll Taxation in a Cross-Section of 64 Countries

dependent variable:	payroll tax capped						employee share of payroll tax			
independent variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
democracy index source	POLIT	PELEC	GAST	POLIT	PELEC	GAST	POLIT	POLIT	PELEC	GAST
democracy index	0.16 (0.12)	0.27 (0.15)	0.30 (0.15)	0.25 (0.21)	0.51 (0.26)	0.65 (0.28)	0.13 (0.04)	0.14 (0.06)	0.13 (0.08)	0.10 (0.08)
avg gdp per capita, log				0.12 (0.12)	0.06 (0.12)	0.07 (0.12)		0.02 (0.03)	0.01 (0.04)	0.02 (0.04)
% of pop. aged 65+				-0.01 (0.04)	-0.02 (0.04)	-0.03 (0.04)		0.00 (0.01)	0.01 (0.01)	0.01 (0.01)
recent British colony				0.03 (0.17)	0.00 (0.16)	0.01 (0.16)		0.07 (0.04)	0.08 (0.05)	0.09 (0.05)
Europe dummy				-0.11 (0.23)	-0.29 (0.29)	-0.21 (0.29)	-0.08 (0.04)	-0.05 (0.08)	-0.06 (0.09)	-0.05 (0.09)
other continent dum's	no	no	no	yes	yes	yes	no	yes	yes	yes
adj-R-sq	.01	.03	.04	-.08	-0.03	.00	.13	.10	.04	.02
s.e.	0.40	0.40	0.40	0.42	0.41	0.41	0.12	0.12	0.12	0.12

Notes: (1) OLS standard errors in parentheses

(2) All regression include a constant term. Coefficients estimates for constants and some dummies are not reported in the Table.

(3) All regressions with continent dummies have North America as the omitted category.

(4) Sample is the same as Table 2, except that Australia is excluded.

(5) POLIT = POLITY IV democracy index. PELEC = POLITY IV executive election index. GASTIL = Barro/Gastil index. All indices are on 0-1 scale.

(6) Recent British colonies variable = 1 for all countries under British rule for more than 50 years since 1850.

Table 4: Democracy and Social Security Expenditure Growth in 76 countries,
1960-74 to 1975-90

independent variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
1960-74 democracy	1.59 (0.41)	-0.52 (0.46)	-0.49 (0.46)	-0.55 (0.42)	-0.74 (0.37)	-0.66 (0.37)	-0.83 (0.39)	1.89 (0.40)	-0.95 (0.45)
democracy change								2.53 (0.74)	-0.36 (0.64)
avg gdp per capita, 1960-74, log		1.35 (0.21)	1.29 (0.21)	0.64 (0.25)	0.30 (0.23)	0.36 (0.23)	0.54 (0.28)		0.54 (0.29)
avg gdp per capita, gr			0.01 (0.01)						
% of pop. aged 65+, 1960-74				0.25 (0.06)	0.17 (0.05)	0.09 (0.07)	0.11 (0.07)		0.11 (0.08)
% of pop. aged 65+, chg					0.54 (0.11)	0.48 (0.12)	0.45 (0.12)		0.47 (0.13)
Europe dummy						0.67 (0.45)	1.50 (0.66)		1.52 (0.67)
other continent dum's	no	no	no	no	no	no	yes		yes
adj-R-sq	.16	.46	.46	.56	.66	.67	.66	.26	.66
s.e.	1.49	1.20	1.19	1.08	0.95	0.94	0.94	1.40	0.95

Notes: (1) dependent variable is the percentage point change of 100*Social Security expenditure/GDP, from the period 1960-74 to the period 1975-90, using the available years (see Appendix).

(2) for other variables: "chg" ("gr") = change (log change) from the period 1960-74 to the period 1975-90.

(3) OLS standard errors in parentheses

(4) All regression include a constant term. Coefficients estimates for constants and some dummies are not reported in the Table.

(5) All regressions with continent dummies have North America as the omitted category.

Table 5: Social Security, Economics, and Politics in Southern European Countries, mid 1960's - 1990				
	Greece	Portugal	Spain	Italy
Politics				
nondemocratic years	1967-74	-1974	-1975*	none
democratic years	-1966, 1975-	1975-	1976-	all
Economics and Age-Demographics (1950-90)				
GDP per cap growth (%/year)	1.7	1.8	1.8	1.6
pop share aged 65+ (percentage point change)	6.6	6.1	6.9	6.2
1974 Gini coefficient	41.3	40.6	37.1	41.0
Public Pension Programs				
spending/GDP (percentage point change) [†]	6.5	4.9	6.2	6.2
timing of spending growth	mainly 1979-	even	even	even
uses payroll tax			throughout	
payroll tax is capped	yes	no	yes	no
employee payroll tax rate, as share of employer+employee	0.3	between 0.2 and 0.4	0.3 reduced to 0.2	0.3
retirement test	throughout	throughout	throughout	earnings test, with retirement test later
delayed retirement credit			none	
means test	throughout	little throughout	little throughout	mainly recent
Notes: *Dictator Francisco Franco-Bahamonde died in 1975, democratic Constitution adopted in 1978 [†] Spending data from the ILO.				

Table 6: Social Security, Economics, and Politics in Four South American Countries, 1980's				
	Brazil	Peru	Uruguay	Argentina
Politics				
nondemocratic years	-1985	-1979	-1985**	-1982
democratic years	1986-	1980-	1985-	1983-
Economics and Age-Demographics (1980-89)				
Average GDP per cap (1000s)	4.0	2.7	4.3	4.1
pop share aged 65+ (avg percentage)	4.2	3.8	11	8.5
1981 Gini coefficient*	55	49	49	42
Public Pension Programs				
spending/GDP (avg percentage)	2.8	0.5	7.4	4.3
spending growth	no trend	no trend	some growth	no trend
payroll tax			throughout	
payroll tax is capped	yes	no	no	yes
employee payroll tax rate, as share of employer+employee	0.5 reduced to 0.3	0.3	0.5	0.4
retirement test	none	throughout	throughout	throughout
delayed retirement credit [†]	n/a	none	none	small
<p>Notes: * Gini coefficients are not from the same data set. Year was picked so that coefficients for all four countries were available.</p> <p>[†]“small” credit refers to a credit that is too small to be actuarially fair for a typical retiree.</p> <p>**Prior to the 1980's Uruguay was democratic 1952-70.</p>				

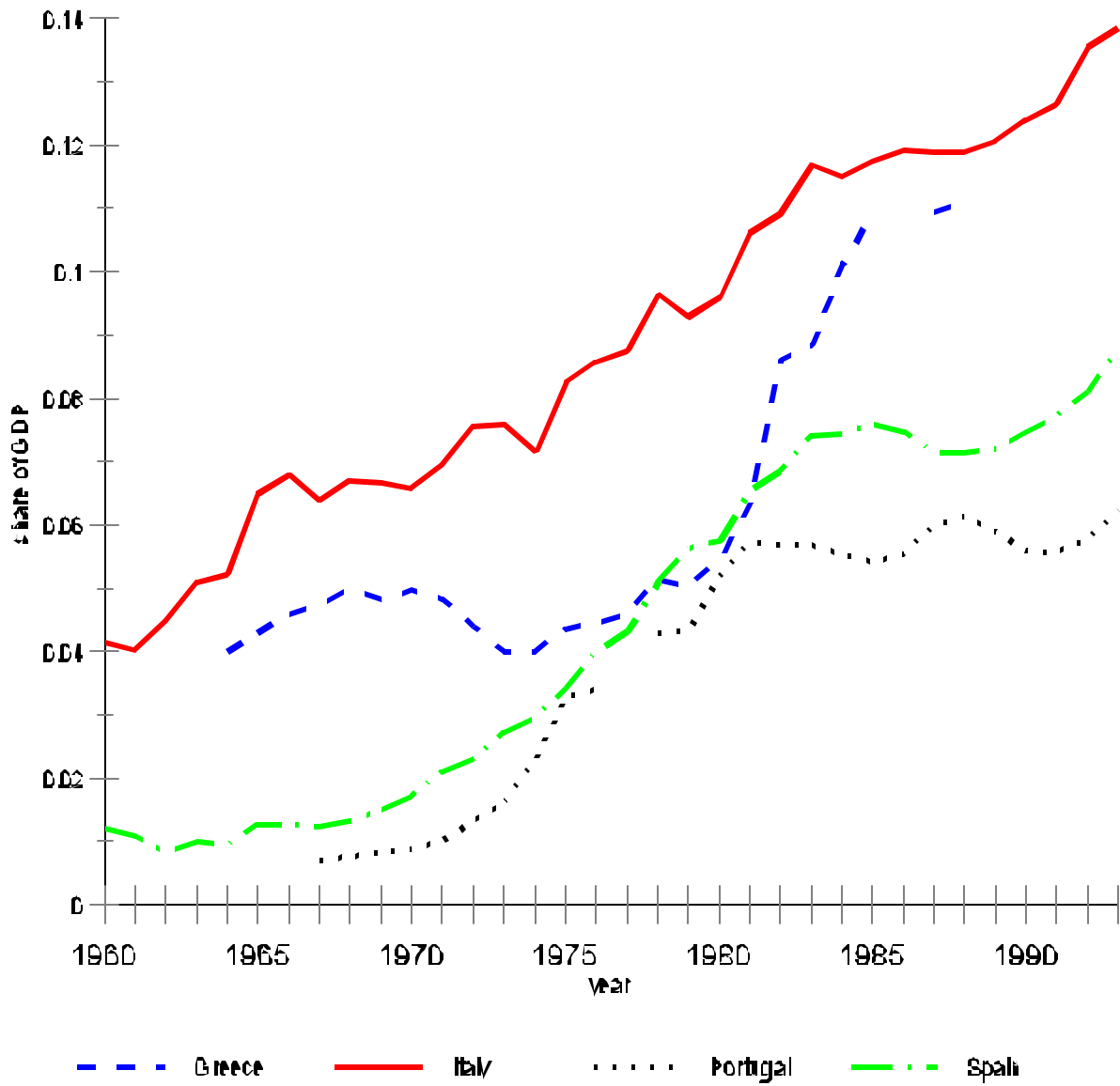


Figure 1 Public Pension Spending in Southern Europe, 1960-93

(Source: Greece – ILO, Italy – Bank of Italy, Portugal – ILO, Spain – ILO & González-Catalá&Merino 1985)

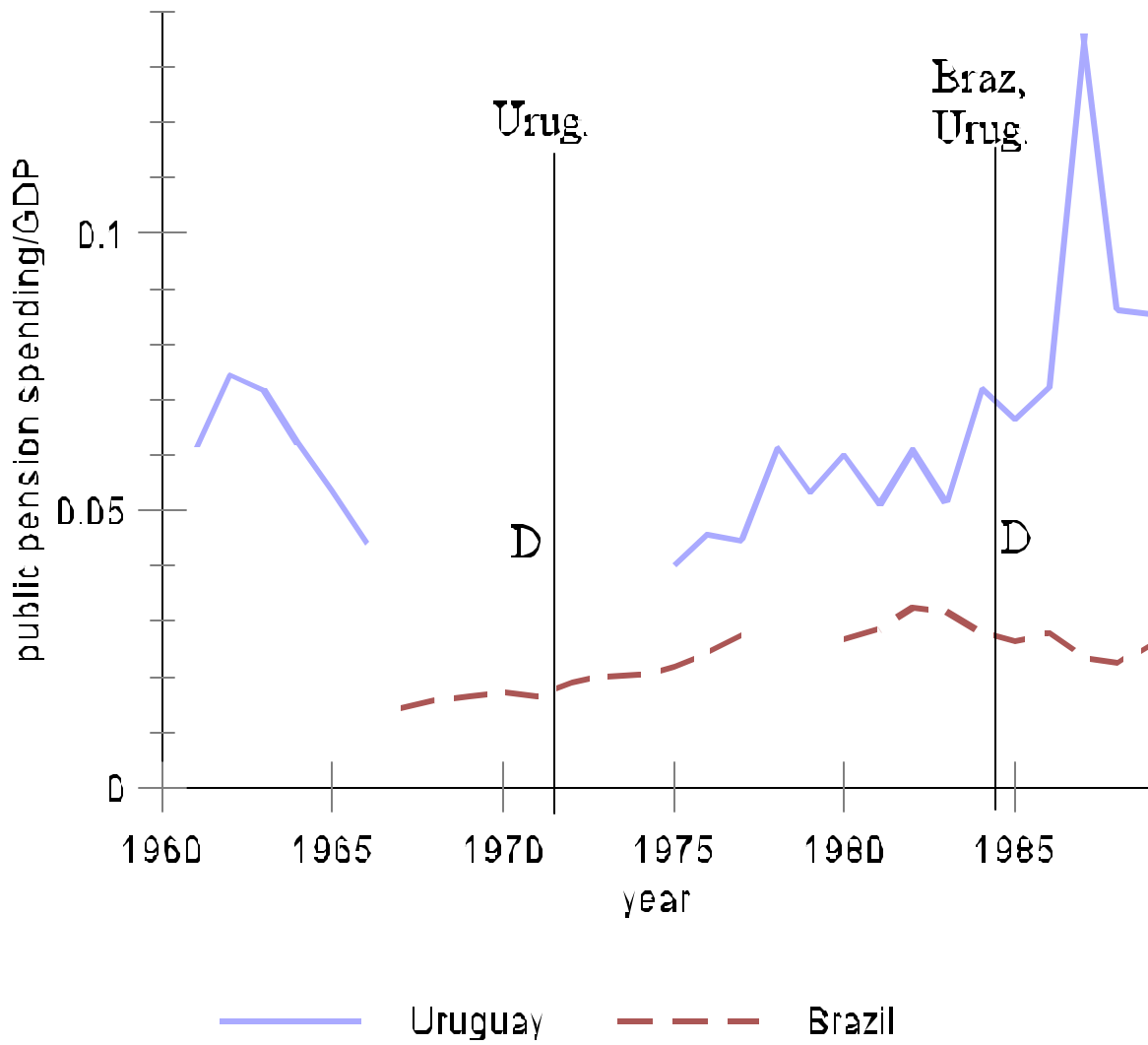


Figure 2 Public Pension Spending in Uruguay and Brazil, 1961-89 (Source: ILO)

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